UNIVERSITY OF CALIFORNIA
SANTA CRUZ

THREE ESSAYS IN DEVELOPMENT ECONOMICS

A dissertation submitted in partial satisfaction of the
requirements for the degree of

DOCTOR OF PHILOSOPHY

in

ECONOMICS

by

Achmad Maulana

June 2016

The Dissertation of Achmad Maulana
is approved:

Professor Jonathan Robinson, Chair

Professor Carlos Dobkin

Professor Ajay Shenoy

Tyrus Miller
Vice Provost and Dean of Graduate Studies

Table of Contents

List of Figures

List of Tables

Abstract

Three Essays in Development Economics

by

Achmad Maulana

This dissertation consists of three essays on development economics. I use data from the Indonesia Family Life Survey (IFLS) to analyze individual decisions and to investigate the underlying factor determining their decisions.

In chapter one, ”Mistake on Risk Question, Cognitive Ability, and Earning: Evidence from Indonesia”, I study whether individuals' mistake on understanding simple risk task during a survey is associated with cognitive abilities, and whether committing the mistake correlates with individuals' abilities to generate earning. I use data from the fourth wave of IFLS to answer these two questions. I find that people with higher cognitive scores make finer mistake than people with low cognitive score suggesting a human capital channel. I also find that individuals who commit the mistake are more likely to earn lower earning.

In chapter two, ”The Effects of Early Childhood Exposure to Natural Disaster on Mistake on Risk”, I investigate the effect of early childhood exposure to the 1992 Flores Earthquake and Tsunami on the observed mistake on risk attitudes as adult. I use individuals' birth date and place recorded in IFLS and merge with geo-location of the epicenter of the 1992 Flores Earthquake and Tsunami. The evidences in this paper point to no correlation between exposure to the earthquake and making mistake on risk. Furthermore, this study cannot find enough evidence that long-run mistake on risk of early child is sensitive to the environmental conditions they experienced early in life. The null findings may be related to selection bias.

In chapter three, ”The Long Term Effects of the School Construction Program on Education and Non-Farm Business Profits in Indonesia”, I replicate Duflo (2001) in a sample of self-employed workers in the IFLS4. I cannot reject her estimates, though my estimates are very imprecise. Additional research is needed to better understand the effect of that large-scale program, especially on self-employed workers, as it comprises almost 70% of Indonesian’s labor force.

For *Mama* and (*alm*) *Bapak*. Thank you for everything.

Acknowledgments

Completing graduate school would not have been possible without support, encouragement and help from many people in their different ways.

I am forever indebted to my dissertation chair, Jonathan Robinson, for being there for me at each step of the thesis process and being outstanding teacher, mentor, and biggest admirer of my work. I am going to miss our Monday meeting rituals. My thanks also to the rest of dissertation committee members, Carlos Dobkin and Ajay Shenoy for numerous discussions, suggestions and continuous supports. I am also grateful for insightful comments and suggestion from the Applied Microeconomic Workshop group members, Alan Spearot, George Bulman, and Brian Giera.

I am grateful for the Support for Economic Analysis Development in Indonesia (SEADI) project. This project provided very generous financial support for full duration of my doctoral study.

I would like to thank my friends in Santa Cruz. *Pak* Sjamsir Sjarief, who was there from the first day to last day of my stay in Santa Cruz. My UCSC cohort friends, especially Seung Lee, Can Kadirgan, Liam Rose, Mario Gonzales, and Justin Rietz. I would also like to acknowledge Julie Kimball. I learn much about my physical-self in the past year with her than the combination of my previous teachers. Special thanks to Sandra Reebie, who kindly provided me with all the administrative assistance I needed throughout the program, and Alex Chavez for his hospitality during my period 'occupying' space at the Media Center of McHenry Library.

Finally, I am forever indebted to my family. I would like to thank to my mother for her unconditional love, constant prayers, and endless supports for everything that I have been doing and will be doing; my late father, who has always been my source of inspiration. I would like to thank my brother and sisters for their big courage to be my siblings. I know that I deviate from the 'path' and may create some 'burden'. Special thanks go to my lovely wife, Yanuarty Maulina. I am sorry for missing eight months of our marriage due to this 'selfish' act. Importantly, I thank to the God All Mighty for the endurance, health, and strength to finish this study.

Chapter 1

Mistake on Risk, Cognitive Load, and Earning: Evidence from Indonesia

1.1 Introduction

Across a range of context, individuals both in developed and developing countries sometimes make mistake, choosing dominated option, that is hard to reconcile with standard rational model. For example, in Indonesia about 40% of people choose an option that gives certain payoff than a gamble that gives higher expected value with no risk. In Mexico, about 25% of people choose a certain payoff than a gamble that gives them higher expected value with no risk (Hamoudi (2006)). In the US, Andreoni and Sprenger (2011) find that 40% of the subjects who participated in their experiment prefer a dominated option. While behavioral biases can explain some departure from the standard model, it seems unlikely that the same biases can fully explain the finding that people choose dominated option even when the alternate carries no risk or uncertainty.

Why people commit such mistake? One answer is cognition. People who exhibit risk aversion seem to have a psychological barrier that correlates with their cognitive level. Frederick (2005), Benjamin et al. (2013), and Dohmen et al. (2010) report that people with low IQ score are more likely to exhibit risk aversion compare to those who have high IQ score. Unfortunately, these studies use data from developed countries where individual risk aversion is not considered as extreme as in developing countries. Thus it would be interesting if a similar study is carried out in developing country.

This study is an attempt to fill this gap. It seeks to study the correlation between cognitive ability and making mistake on risk and the correlation of this mistake with earnings. I use a response from a task in the risk preference module of the Indonesia Family Life Survey (IFLS) as my measure of mistake on risk and number of recall words as my measure of cognitive capacity. My results are: it suggests robust association between cognitive ability and mistake on risk, and between making mistake on risk and adults' earning. This study contributes to several line of literature. First, it contributes to the literature on the association between cognitive ability and mistake on risk question and between mistake on risk and earning. In addition to contributing to cognitive and risk attitude literature and earnings, this study contributes to other strand of literature. This is the first study using developing country data that try to reveal the link between cognitive capacity, survey-measure risk attitude, and earning.

The findings of this study can be organized into different parts. First, on the link between cognitive ability and risk attitude, I find that in Indonesia, cognitive capacity is negatively associated with mistake on risk attitude. Respondent who could recall more words during a word recall test is more likely not to choose dominated choice. Interestingly, I find that the inclusion of years education control attenuates the correlation between cognitive capacity and mistake on risk attitude suggesting that some of the cognitive effect comes from education. The strength of the associations between the two variable of interest are much stronger for respondents who participate in the wage sector, the coefficient of cognitive capacity survives the 'horse race' test.

Second, I find in Indonesia a negative association between mistake in risk attitude and earnings, wage for those who participate in wage sector and business profit for those who participate in non-wage sector. I find similar results when using another proxies for profit. The relationship between mistake on risk attitude and small firm profit is robust to different specifications, and using an approach to examine potential bias form selection on unobservables, I find that the selection on unobservables needs to be between 1 and 3 times that on observables in order to explain away the results for both earning measures. The findings in this part suggest another channel on where human capital can affect earnings, indirectly by increasing the quality of the mistake.

Noting that in this study I am not making any causal claims about the relationship between making mistake on hypothetical risk aversion question and cognitive ability nor the relationship between making mistake on hypothetical risk aversion question and earnings since I cannot settle all the identification issues, i.e. the correlations that might be result of unobserved factors.

1.2 The Literature Review

1.2.1 Cognition and Risk Preference

Does cognitive ability affect risk aversion? To date, there is no shared beliefs on the exact relationship between cognition and risk aversion. Some studies find that individuals with lower cognitive ability, as measured by IQ test, are more likely to be risk averse (Dohmen et al. (2010), Oechssler et al. (2009), Burks et al. (2009), Beauchamp et al. (2015), and Benjamin et al. (2013)). Memory is also correlated with risk aversion: cognitive reflection test (Frederick (2005)) and memorization test (Deck and Jahedi (2015)).[1] Other studies find no evidence to support the hypothesis that cognition correlates with with risk aversion. Indirect measures of cognitive ability, alcohol consumption and sleep deprivation, have also been used and the results are not always the same. In the lab setting, Corazzini et al. (2014) do not find any evidence that alcohol consumption depletes risk tolerance whereas McKenna et al. (2007) find that sleep deprivation leads to risk neutral behavior.

1.2.2 Risk Preference and Economic well-being

Does risk aversion correlate with economic outcomes? This empirical question has drawn so many attention but no clear evidence has been provided. Early attempt to answer this question in developing countries was carried out by Binswanger (1980). He randomly selects 240 households in rural India, elicits their risk attitudes using real payoffs, and finds that risk preferences are not correlated with households' wealth. Using life insurance data to estimate the Arrow-Pratt coefficient, Halek and Eisenhauer (2001) reveal that self-employed people have higher coefficient of relative risk aversion. Conducting experiment in six Latin-

[1] Studies that find similar findings are Benjamin et al. (2013), Gerardi et al. (2013), Grinblatt et al. (2011), Kremer et al. (2014), and Agarwal and Mazumder (2013).

American countries, Cardenas and Carpenter (2013) find no correlation between risk preference and an aggregate measure of several wealth indicators. Tanaka et al. (2010) directly measure risk preference of household members in nine Vietnamese villages and show that household income does not correlate with risk aversion but find that mean village income is strongly correlated with loss aversion.

Analysis using hypothetical questions provides another evidence for the link between risk aversion and income measure in developing countries. Using a household survey among market person in Nigeria, Lammers et al. (2010) find that propensity to take risk is negatively related to micro and small enterprise (MSEs) profits. This effect is no longer statistically significant once they control for risk perception, risk perception is positively correlated with profit. Using sample from southern African business owners, Krauss et al. (2005) find a weak correlation between risk taking and business growth. They measure growth by a single index which was based on combination of profit, customers, and sales growth. Using a share of financial wealth placed in risky assets and self-reported risk aversion indicator, Shaw (1996) finds that wage growth is positively correlated with individual's preference for risk.

1.3 Context

Indonesia is the fourth most populous nation in the world, of approximately 255 million in 2015, and is a non-secular state, where Muslims account for 87.2% of the population. Population density varies significantly across islands and regions, 60% of Indonesians live in Java Island and 53.7% live in urban area. In 2012, years of education is 13 years, equivalent to the first year of senior high school.

Before the Asian Financial Crisis (AFC), Indonesia was undergoing a period of structural change, transforming from an agricultural based economy to manufacture and service based economy. The average annual country GDP growth between 1976 to 1997 was 7.5%. The AFC had major impact on the Indonesian economy; for example, the country growth between 1998 and 2015 was at 4.07%, major drop from pre-crisis figure. The AFC affected Indonesian households, surge in prices and caused a sharp reduction in household real income. With layoffs and wage cut in the formal sectors, the AFC causes a substantial decline in living standards (Strauss et al. (2004)).

Small firm enterprises play an important role in Indonesia's economy. According to the 2006 firm establishment census conducted by Central Statistical Agency (BPS), small firms account for about 99.03% or 22.52 million establishments. Majority of the medium and big enterprises have their business in the urban area and thus small firms development is synonymous with improving the rural economy. Small firms in Indonesia is the backbone of the informal sector, absorbing 70% of total employment. According to the 2005 Bank Indonesia survey on small and medium enterprises, small firms face major problem to access the credit market, due to collateral and high interest rates.

1.4 Data and Measurement

1.4.1 Data

The data for this study is drawn from the Fourth Wave of the Indonesia Family Life Survey (IFLS4). Due to its longitudinal structure, IFLS4 drew its sample from previous waves. The IFLS1 sampling scheme stratified on province and urban/rural location, covering a sample from 13 provinces that is representative of roughly 83% of the population. Within each province, enumeration areas (EAs) were randomly chosen from the 1993 National Socio Economic Survey (SUSENAS). A total of 321 enumeration areas were selected, which over-sampled urban EAs and EAs in smaller provinces. IFLS4 interviewed 13,535 households with almost 30,000 adults.

IFLS4 contains a rich set of information on socio-economic conditions on all members of each sample household, including a set of questions that measures risk and time preferences, trust, and household economy. For this cross-sectional analysis, I focus on IFLS4's adult sample who attempted to answer the risk and time preference module and have complete educational history. Of 23,347 adults that are selected into the sample, 12,654 adults report positive earnings, either from participating in wage or non-wage sectors.

1.4.2 Measures

Measure of Cognitive Load

My measure of cognitive capacity comes from the health module of IFLS4. The module was asked to household members aged 15 years or above. Respondents were asked to read a list of ten simple nouns and then asked to recite as many nouns as they can remember. After recalling, respondents were asked acute morbidity symptoms that respondent may feel in the past month and in the past six months. Following that module, the respondent were asked to recite the nouns again. This study uses the number of nouns that respondent could recall in the first round as measure of cognitive capacity. McArdle et al. (2002) explain that ability to recall word can be used to examine person's episodic memory, an important aspect of fluid intelligence. Access to memory is basic tenet of cognitive ability.

Measure of Mistake on Risk

IFLS4 asks two sets of choice task that elicit attitudes toward risk. In each set and task, the interviewer presents two choices: (A). a guaranteed income stream, or (B). a high or low income stream with equal probability. An important caveat is that choices are not in real stakes even though the payoff can be considered as huge. The literature is not clear whether this matters. In on side, Holt and Laury (2002) find contrasting findings when using real stakes and hypothetical stakes; subjects are much risk averse with high real-payoff than with comparable hypothetical payoffs. Dohmen et al. (2011) test whether survey question using hypothetical stake in large-scale sample is correlated with risk-taking behavior using real money in the laboratory setting in smaller sample. They find positive association between responses to a survey item that asks individuals about a judgment of their own willingness to take risk and responses to risk attitudes in paid real-stakes lotteries.[2] Recently, Kang et al. (2012) used functional magnetic resonance imaging (fMRI) to show that common areas of the brain are activated when individuals make real and hypothetical choices about the purchase of consumer goods, but they note that the level of this activity differs.

As mentioned, IFLS4 includes a new section on risk and time preference. Before eliciting risk attitude the respondents were presented with a baseline task as follows

> *Suppose you are offered two ways to earn some money.*
>
> 1. *With option 1, you are guaranteed IDR 800 thousand per month.*
> 2. *With option 2, you are guaranteed IDR 800 thousand per month, or IDR 1.6 million per month with equal chance.*
>
> *Which option will you choose?*

[2] Other papers who found that some of the lottery measures were correlated with survey-elicited preferences are Hamoudi (2006) and Reynaud and Couture (2010).

Option one is clearly dominated by option two. If a respondent chooses option one, the interviewer then offers the respondent a switch. Those who switch for option 2 then face a sequence of choice task designed to elicit their risk preference, only a handful of respondent switch. Those who stick with choosing the dominated option were then exit the module and I define it as people who commit "mistake on risk".

There are several alternative interpretation that one can use to describe the condition where people stick with the dominated option. One interpretation is that respondent infers option two as gambling and it is immoral to be involved with. Second interpretation is that respondent exhibits high/extreme degree of risk aversion, avoid any option that carries some aspect of uncertainty, either in the outcome or in the probability. Third interpretation is that these respondents may have some cognitive barrier that prevents them choosing the dominant option.

Measure of Self-Employed Business Profit

In every round of the IFLS, a module on household economy was administered. It asked about household housing characteristics, household businesses (farm and non-farm), nonbusiness assets, and non-labor income. IFLS asked a rich set of information about business outcomes: household revenues, expenses, and value of assets of non-farm businesses. IFLS also includes three new questions that can be used to estimate non-farm net income: the value of production used for household consumption, the value of business net income used for household expenditures, and the amount of left over cash. IFLS4 also asks which member of household resumes the business responsibility.

With IFLS4, I could calculate three different measures of non-farm business profit. First is the direct measure of profit: the response to the question "What is the approximate amount in rupiah of net profit generated by this this business in the past 12 months?". Second is the implicit measure of profit, calculated by subtracting self-reported revenue and cost from the business in the past 12 months. Third is the indirect measure of profit, profit is calculated from adding the amount of money that household used, either for consumption or other expenditures, from the business and the amount of saving. This study uses the second approach to get the estimate of profit.

1.4.3 Summary statistics

Table 1.1 presents summary statistics for two subsamples: whole adult and sample with positive profit from non-farm business. Business owners are older, are less educated and are more likely to be married than people who work for a wage. Interestingly, households who have members participate in non-farm business have higher monthly per capita expenditure than whole sample households. Non-farm business owners have slightly lower cognitive capacity, they could recall 4.81 words in the first test while the whole sample able to recall 5.01 words. Interestingly, non-farm business individuals have lower probability of choosing dominated choice compare to the whole sample.

Table 1.2 summarizes main characteristics of self-employed non-farm business in the IFLS. More than 50 percent of the businesses are operated outside home while the rest utilize their home to help them running their businesses. With respect to the line of business, 30 percent of the non-farm business are in trade, selling non-food sales, and almost 35 percent of the firm are restaurant or food catering businesses. Tailor, staff, motorbike messengers, and hairdressing account for about 18 percents.

IFLS4 also asks the amount of start-up capital that non-farm businesses were able to generate. The median start-up capital is about 500,000 rupiah, approximately 55% of the monthly per capita expenditure and mean starting capital is 5,9 million rupiahs. These findings show that starting a business is not free. Household member is going to open or participate non-farm business if expected future benefit exceeds expected cost. Relatively small fractions of the business owners utilize loan from a bank to finance their start-up capital whereas more than 50 percent of the owners use their own saving or ask for family help to finance initial capital.

One striking feature of Indonesia non-farm business is that they tend to maintain the business scale at a small level. This can be seen in the number of paid workers involve in the business. 80 percent of the businesses have zero paid worker while 17 percent has between 1 to 5 paid workers. Almost half of the businesses surveyed even do not have unpaid worker.

1.5 Results

In this section, I argue that making mistake on risk correlates with cognitive capacity and furthermore I find that respondent who is not committing mistake is more likely to earn higher profit when running a non-farm business.

1.5.1 Correlates of making mistake on risk

Table 1.3 presents the estimates of the correlation between cognitive capacity and committing mistake on risk for two samples. In panel A, I use the full respondents while in panel B I pool all respondents who report to have positive earning, either from wage sector or self-employed. To identify for various channels by which cognitive capacity could affect making mistake, I include several regressors. The regressors vary from the most likely to the less likely to be exogenous.

Column 1 in Table 1.3 shows result from univariate regression between cognitive measure and committing mistake on risk. I find that with one standard deviation increase in word recall leads to five percentage points decrease in the likelihood of making mistake for both gropus. This number represents 13 percents decrease over baseline. In column 2 and 3, I add demographic regressors and respondent's years education respectively. I find that some of the effect of cognitive capacity on choosing dominated option seems to work through respondent's education, conditional on years of education the estimated effect decreases in absolute value.[3] The coefficient of interest is still statistically significant different from zero.

Using IFLS, I am able to further investigate for different possible channels on how cognitive capacity correlates with mistake on risk. In column 4, I introduce regional and interviewer fixed effects and find that the correlation between cognitive measure drops even further, the estimated effect is roughly 1 to 2 percentage point decrease over the whole sample and sample with positive earning, respectively. These results indicates that the observed mistake may not entirely indicate respondents' inability to choose a more plausible option heterogeneity of the interviewers and area in which respondents live may also matter.

In panel B, I present results from pooling respondents who report positive earning either from wage sector or non-wage sector. Qualitatively, I find similar results as when using sample from non-wage respondents. The correlation between cognitive capacity and making mistake on risk is robust. The coefficient of cognitive capacity survives when various controls are included.

[3] In Table A.1 I regress the same specifications without including years of education in covariates and find that more number of word that could be recalled is robust and negatively associated with making mistake.

1.5.2 Making Mistake and Earning

In this section I analyze whether the mistake that respondents made during the survey correlates with labor market outcome, earning. If the mistake measure is meaningless then it should not be expected to correlate with respondents earnings either in wage or non-wage sectors. In panel A, I use sample who works for wage whereas in panel B I group sample who participate in non-wage sector.

Table 1.4 presents the correlates of earning. Column 1 presents estimate of the correlation between making mistake on risk and non-farm business profit controlling for respondent's cognitive capacity. Column 2 presents estimate of the correlation when taking into account basic demographics. In column 3 I add education into the equation while for the non-wage sector sample I include various business characteristics on top of education. In column 4 I introduce enumeration area and interviewer fixed effects.

It can be seen that the coefficients on making mistake on risk are negative and statistically significant for both groups. The correlation between making mistake and non-farm business profit when controlling for cognitive capacity is about 16 to 25 percent change, for sample who participate in wage and sample who participate in non-wage sectors, respectively. Without any additional control, the suggested effect is that people who commit mistake on risk is more likely to earn less than people who do not make mistake on risk. Including basic demographics and years of education the same coefficient decreases to 12 to 19 percentage change. For self employed respondent, conditional on years of education the coefficient on word recall becomes not statistically significant different from zero where the coefficient on word recall is still significant different from zero for worker in wage sector. Including enumeration area and interviewer fixed effects, the coefficients of interest vary about 8 to 19 percentage change for the two samples (Using $F-$test on the joint significance of regional and interviewer fixed effects, I find enough evidence to reject null hypothesis of no effect of these two sets of fixed effects variables). The evidence so far points to statistically significant correlation between making mistake on risk question and earnings. How meaningful are these correlations in real terms? To answer that, I compare coefficients of mistake on risk and the coefficients on years of education. Again, in this exercise I am trying to compare correlation coefficients not causal coefficient.

The coefficient of mistake on risk is approximately six times the effect of years education on non-farm business profit (column 4) and is less than the effect of years of education on wage. Interestingly, in panel A the ratio of the magnitude of making mistake and the magnitude of years of education is lower than the same ratio in panel B. These results indicate the possibility of different effect of making mistake for two type of income earners.

1.5.3 Selection

The above estimate of the effect of making mistake on risk on non-farm business profit may still suffer from the standard omitted variable bias, even after controlling a rich set of control variables. Altonji et al. (2005) propose an approach to quantify the magnitude of the omitted variable needed to explain away their entire effect. In the spirit of Altonji et al. (2005), Bellows and Miguel (2009) extend the approach to linear model and calculate the "influence" ratio of omitted variables relative to the observed control variables that would be needed to fully explain the finding.

Table 1.5 presents estimates of the Altonji et al.'s (2005) ratio for two different outcomes. I consider basic (without controlling for years of education and/or business related skills) and extended (controlling for years of education and/or business related skills covariates).[4] Non-farm profit A is extracted from household head answer on the question "how much profit has this business earned in the past year", non-farm profit B is calculated from the self-reported revenue minus the self-reported cost whereas non-farm profit C is calculated from the value of business production used for household consumption, the value of business net revenue used for household expenditure, and the amount of left over cash. I present Altonji et al.'s (2005) ratio for respondents who work for wage in panel A. Panel B presents the ratio for self-employed worker.

In the basic specification of panel A, the magnitude of Altonji et al.'s (2005) ratio is 1.09 while for in the extended specification the ratio drops to 0.45. In the basic specification of panel B, which the coefficients on making mistake on risk are statistically significant, the magnitude of Altonji et al.'s (2005) ratios is between 2.9 and 3.9. Meanwhile for the extended specification the magnitude of these ratio ranges between 0.9 to 1.7. Thus, in most cases, I estimate that the shift of the unobservable variable would have to bigger than the shift in the observables to explain the entire making mistake on risk effect. To illustrate, let focus on the number on column 1 row 2, which is 2.94. This number could be interpreted as the amount of targeting on unobserved variables would have to be over 3 times greater than the amount of targeting on observed variables to explain away the entire choosing dominated choice effect. This seems unlikely, given that I have controlled rich set of observed controls, from individual, household, regional and interviewer fixed effects.[5]

[4]Business related covariates include number of paid and unpaid worker at the initial year of business, the amount of start-up capital, from where they get the start-up capital, location of the business, and sectors of activity.

[5]One last concern is the direction of causality. It is possible that preferences drives profits (via business formation, technology adoption, so on) or profits/outcomes induce preferences. In a cross-sectional study like this, I cannot provide an encouraging answer. Thus, I repeatedly mention that this study can only infer about correlation, not causation.

1.5.4 External Validity

The distribution of people who prefers the sure thing is not unique to IFLS4 sample. 40% of the respondents in the Worker and Iron Supplementation Evaluation (WISE), a longitudinal survey in rural village in Central Java, when presented the similar risk instrument choose dominated option. This number is even higher for respondents in the Study of Tsunami Aftermath and Recovery (STAR), a longitudinal study of households and individuals in Aceh and North Sumatra provinces, 50% in the 2005 survey and 70% in the 2007 survey. A similar finding, significant portion of people choose sure amount even though it is dominated, also found at the Indonesia Access to Finance Survey, a nationally representative household survey conducted by the World Bank. This survey offered respondents a choice between receiving 2,000 IDR for sure or play game that paid 5,000 IDR with probability 0.5 and 0 IDR with probability 0.5 and 36% of households chose the sure amount.

The result from the above findings may reflect the same problem that this study is trying to answer. It must be acknowledge that mistake on risk measure is far from perfect. Respondent low-level education along with their low-level understanding on the probability concept and the possibility of heterogeneity in the interviewer and area where they live may confound the findings.

1.6 Conclusion

Using data from Indonesia, this study provides evidence on the association between cognitive capacity and mistake on hypothetical risk aversion question. People with higher cognitive scores make finer mistake than people with low cognitive score suggesting a human capital channel. These findings illustrate the extent of the association between cognitive ability and mistake on risk attitude and the association between mistake on risk risk aversion question and labor market performance in Indonesia.

The results in this study are related to literature examining the effect of education on risk aversion (i.e. Kremer et al. (2014), Benjamin et al. (2013), Gerardi et al. (2013), Grinblatt et al. (2011), and Agarwal and Mazumder (2013)), This study suggests that human capital may be important to explain the observed mistake on hypothetical risk aversion question. Indirectly, the results in this study point to the inability of respondent to use information effectively during the hypothetical task during survey (thus creating a fear of uncertainty), it can be in some part be minimized through education.

On the other hand, it is interesting to find out that measure mistake on hypothetical risk question correlates with labor market outcome, such as earning. This result connect to literature in labor economics initiated by Shaw (1996) who documented that individual wage growth is higher for individual with greater preference for risk. The robust correlation between mistake on hypothetical risk aversion question, education, and earning points to important role of human capital channel where it would affect productivity not only directly but also indirectly by increasing people awareness to avoid making implausible decisions.

Table 1.1. Summary Statistics

	Whole Sample		Non-Farm Buss. Sample	
	Mean (1)	Std. Dev. (2)	Mean (3)	Std. Dev. (4)
Individual-level variables				
Years of Education	8.59	4.09	8.22	4.12
1st word recall (max is 10)	5.01	1.83	4.81	1.76
2nd word recall (max is 10)	4.02	2.04	3.76	1.94
Age	34.48	12.59	40.10	10.63
Married	0.72	0.45	0.87	0.33
Male	0.50	0.50	0.54	0.50
Live in urban area	0.53	0.50	0.59	0.49
Muslim	0.89	0.30	0.90	0.29
Household-level variables				
HH size (person)	5.67	2.89	5.61	2.59
Monthly HH expenditure per capita (PCE) (in million IDR)	0.86	9.37	0.91	3.64
Log (1+PCE)	12.76	0.92	12.91	0.91
Mistake on Risk				
Choosing dominated option	0.40	2.04	0.38	0.49

Notes: Whole sample is made up from respondents between 15 and 65 years old whereas the non-farm business sample is restricted for household members who are responsible for the non-farm business and able to keep the business until 2007. Number of observations for whole sample is approximately 23,600 and number of observations for non-farm business is 4,500.

Table 1.2. Characteristics of Non-Farm Business

	Mean
Location of Business	
Outside the house	0.52
Partially outside the house	0.24
At the house	0.24
Field of work	
Restaurant and food sales	0.34
Service: staff, tailor, motorbike & hairdressing	0.18
Store: non-food sales	0.30
Industrial: garment & other	0.09
Other	0.07
Start up capital*	
Mean (in million rupiah)	5.9
Median (in million rupiah)	0.5
Source of start up capital	
Saving	0.53
Family	0.43
Other	0.08
Loan from Bank	0.10
Loan from Other	0.08
Number of unpaid worker	
0	0.49
1-5	0.49
> 5	0.02
Number of paid worker	
0	0.80
1-5	0.17
> 5	0.03

Notes: Sample is made up from business where household members own and still in business in 2007.

Table 1.3. Correlates of Making Mistake

	(1)	(2)	(3)	(4)
Panel A. Whole Sample				
Cognitive measure				
Word recall (z-score)	−0.0547	−0.0455	−0.0299	−0.0174
	(0.0034)***	(0.0037)***	(0.0039)***	(0.0036)***
Years of Education			−0.0104	−0.0109
			(0.0009)***	(0.0009)***
R^2	0.01	0.02	0.03	0.28
N	23,347	23,330	23,327	23,327
Panel B. Sample with Positive Earning				
Cognitive measure				
Word recall (z-score)	−0.0567	−0.0482	−0.0308	−0.0176
	(0.0046)***	(0.0050)***	(0.0053)***	(0.0049)***
Years of Education			−0.0104	−0.0118
			(0.0012)***	(0.0012)***
R^2	0.01	0.02	0.02	0.29
N	12,654	12,643	12,640	12,640
Control variables:				
Demographics	No	Yes	Yes	Yes
Enum area & interviewer FEs	No	No	No	Yes

Notes: Sample is individuals aged 15 years and above in 2007 and earn income in 2007. The dependent variable is dummy variable that equal one if individual makes mistake on risk question. Demographic includes age, gender, muslim and household size. Standard errors are in parentheses. *** indicates significance *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 1.4. Correlates of Earning

	(1)	(2)	(3)	(4)
Panel A. Sample who Participate in Wage Sector				
Making mistake				
Dominated Choice	−0.1688	−0.1204	−0.0309	−0.0889
	(0.0304)***	(0.0287)***	(0.0265)	(0.0296)***
Cognitive measure				
Word recall (z-score)	0.3101	0.3159	0.0716	0.0674
	(0.0161)***	(0.0161)***	(0.0160)***	(0.0162)***
Years of Education			0.1402	0.1275
			(0.0035)***	(0.0039)***
R^2	0.05	0.16	0.29	0.41
N	8,575	8,567	8,564	8,564
Panel B. Sample who Participate in Non-Wage Sector				
Making mistake				
Dominated Choice	−0.2473	−0.1922	−0.1282	−0.1944
	(0.0422)***	(0.0409)***	(0.0393)***	(0.0457)***
Cognitive measure				
Word recall (z-score)	0.1295	0.1124	0.0210	−0.0016
	(0.0224)***	(0.0231)***	(0.0234)	(0.0245)
Years of Education			0.0486	0.0382
			(0.0055)***	(0.0062)***
R^2	0.02	0.09	0.19	0.35
N	4,566	4,562	4,501	4,501
Control variables:				
Demographics	No	Yes	Yes	Yes
Enum area & interviewer FEs	No	No	No	Yes

Notes: Sample is individuals aged 15 years and above in 2007 and earn income in 2007. The dependent variable is logarithm of earning, for wage sector is salary while for non-wage sector is non-farm business profit. Demographic includes age, gender, muslim and household size. In column 3 for panel B, I include business characteristics which include number of paid worker at the start of the business, number of unpaid worker at the start of the business, the amount of capital at the start, source of capital, location of the business, and sector of the business. Standard errors are in parentheses. *** indicates significance *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 1.5. Selection on Observable to assess potential bias from unobservable

Outcome	Basic (1)	Extended (2)	Mean dep. variable (3)
Panel A. Sample who Participate in Wage Sector			
Total salary	1.09	0.45	14.9557
(N = 8,564)			
Full-control specification includes:			
Household makes mistake indicator	No	Yes	
Individual level controls	Yes	Yes	
Household level controls	Yes	Yes	
Region and interviewer level controls	Yes	Yes	
Panel B. Sample who participate in non-wage sector			
Non-farm profit A[a]	2.92	1.77	15.3483
(N = 4,414)			
Non-farm profit B[b]	2.94	1.70	15.3478
(N = 4,418)			
Non-farm profit C[c]	3.91	0.95	15.3237
(N = 4,021)			
Full-control specification includes:			
Business related controls	No	Yes	
Individual level controls	Yes	Yes	
Household level controls	Yes	Yes	
Region and interviewer level controls	Yes	Yes	

Notes: Each cell calculates: $\hat{\beta}_C/(\hat{\beta}_{NC} - \hat{\beta}_C)$ where $\hat{\beta}_C$ denotes the estimated choosing dominated choice coefficient in the full-control specification and $\hat{\beta}_{NC}$ denotes the same coefficient in the no-control specification. I estimate using OLS for both full-control and no control specifications.

Appendix A

A.1 Assessing the importance of Omitted Variable Bias

This note briefly describes how much strong the relationship between the unobservable and making mistake on risk question relative to the relationship between the observable and making mistake on risk to explain away their entire effect. It follows closely presentation of Bellows and Miguel (2009). Consider the following model:

$$Y = \alpha D + F\beta + \epsilon \tag{A.1}$$

where F is the index of full control variables, including observables and unobservables, D is binary variable that indicates choosing dominated choice. If I estimate α without F, then the estimates of α will have the standard variable bias:

$$p\text{-lim}\, \hat{\alpha}_{NC} = \alpha + \beta \frac{Cov(D, F)}{Var(D)} \tag{A.2}$$

where NC indicates 'No-control' estimate. Suppose that there is a set of observed controls $\mathbf{X}$ and F is linearly correlated with these controls:

$$F = \mathbf{X}'\gamma + \tilde{F} \tag{A.3}$$

where $\tilde{F}$ denotes the unobserved part of the full control variables. Including (A.3) to the estimating equation (A.1)

$$Y = \alpha D + \mathbf{X}'\gamma\beta + \tilde{F}\beta + \epsilon \tag{A.4}$$

The new OLS estimate of α becomes:

$$p\text{-lim}\,\hat{\alpha}_C = \alpha_0 + \beta\frac{Cov(D, \tilde{F})}{Var(D)} \tag{A.5}$$

where C is 'Control". Given the linear relationship between F and $\mathbf{X}'\gamma$, we have

$$\begin{aligned}\hat{\alpha}_C - \hat{\alpha}_{NC} &= \beta\left(\frac{Cov(D, F)}{Var(D)} - \frac{Cov(D, \tilde{F})}{Var(D)}\right)\\ &= \beta\frac{Cov(D, \mathbf{X}'\gamma)}{Var(D)}\end{aligned} \tag{A.6}$$

This difference is composed of: (i) the effect of full control variables on the outcome (β) and (ii) the correlation between the observed control variables and choosing dominated choice ($\mathbf{X}'\gamma$). Thus, a large reduction in the omitted variable bias after including observed controls can come from a strong relationship between full control control variables and the outcome, or a a stronger correlation between observed control variables and choosing dominated choice.

The question of interest is how strong the covariance between unobserved part of the full control variables and choosing dominated choice must be for the unobserved part to account all the effects. To quantify this, suppose there is no choosing dominated choice effect ($\alpha_0 = 0$). Divide (A.5) with (A.6), we get:

$$\frac{\hat{\alpha}_C}{\hat{\alpha}_{NC} - \hat{\alpha}_C} = \frac{Cov(D, \tilde{F})}{Cov(D, \mathbf{X}'\gamma)} \tag{A.7}$$

The term on the left hand side can be computed from estimating two OLS regression with and without observable control variables. The right hand side shows how strong the covariance between the choosing dominated-choice and the unobserved part of the full control variables, relative to the covariance between the observed part of the full control variables and choosing dominated choice, to fully attenuates the effect of choosing dominated choice.

Table A.1. Correlates of Making Mistake without Controlling for Education

	(1)	(2)	(3)
Panel A. Sample who participate in non-wage sector			
Cognitive measure			
Word recal (z-sc)	−0.0608	−0.0544	−0.0391
	(0.0056)***	(0.0060)***	(0.0057)***
Control variables:			
Demographics	No	Yes	Yes
Enum area & interviewer FEs	No	No	Yes
R^2	0.01	0.02	0.30
Observation	8,751	8,743	8,743
Panel B. Sample who participate in non-wage sector			
Cognitive measure			
Word recal (z-sc)	-0.0510	-0.0367	-0.0203
	(0.0078)***	(0.0083)***	(0.0081)*
Control variables:			
Demographics	No	Yes	Yes
Enum area & interviewer FEs	No	No	Yes
R^2	0.01	0.02	0.35
Observation	4,566	4,562	4,562

Notes: Sample is individuals aged 15 years and above in 2007 and earn income in 2007. The dependent variable is dummy variable that equal one if individual makes mistake on risk question. Demographic includes age, gender, muslim and household size. Standard errors are in parentheses.

Chapter 2

The Effects of Early Childhood Exposure to Natural Disaster on Mistake on Risk

2.1 Introduction

People in developing countries are considered to be extremely risk averse (Haushofer and Fehr (2014)). In Mexico 25% Mexican Family Life Survey (MXFLS) prefer sure amount than a gamble option that yields higher expected value and carries no risk (Hamoudi (2006)). About 36 percent of Indonesians participating in Access to Finance Survey prefer sure amount of 2,000 IDR rather than an equal chance of getting 5,000 IDR or 0 IDR even though it yields higher expected payoff. Similar finding is also found using another data set such the Worker and Iron

Supplementation Evaluation (WISE) and the Study of Tsunami Aftermath and Recovery (STAR). In these last two survey, the number of people choosing sure thing is even higher, 40% and 50%, respectively. Thus it is interesting to examine why people in Indonesia seem to exhibit extreme risk aversion. Is there any reasonable explanation on why significant portion of people choose a sure amount even when the expected payoff is less? One potential answer is experience to traumatic events (see Callen et al. (2014), Cameron and Shah (2015), Cassar et al. (2011), Kim and Lee (2014), Eckel et al. (2009), Ingwersen (2014), and Voors et al. (2012)). It is reasonable to assume that recent experience to traumatic events affects people's risk attitude but it is unknown whether the traumatic events have also a long-run effect on people risk attitude. Why would one expect disaster to have prolonged effect that is potentially stronger than the contemporaneous effect of lost resources? To begin with, most of a person's human-capital and psychological development happens in childhood. On psychological side, a fearful-free environmental conditions in childhood might lead to more robust adult individual. On the human-capital side, living close to area with high probability of disaster might child's weariness and latter could affect their learning process. Another argument is that many of those findings involve small stakes or even hypothetical payoff and then shocks might be correlated with their cognitive development.

While direct effect of natural disaster on risk attitudes have been well studied, little is know about effects that persist from disasters in early life. The study is intended to fill this gap. The is to identify the effect of early childhood exposure to natural disaster on subsequent mistake on risk as an adult. In particular, I examine the effect of Flores earthquake on childhood on the adult mistake on risk of Indonesian men born between 1970 and 1992.

To evaluate the effects of early childhood disaster exposure, I utilize several data sources. First, I use information from the Indonesian Family Life Survey (IFLS) on individual's month-year and region of birth location, and merge everyone in that survey to geo-location of the place of birth to geo-location of the epicenter of the 1992 Flores Earthquake and Tsunami. The exposure of an early childhood to the disaster was determined both by the child age and how far the child region of birth when the disaster was happened. Taking into account for region of birth and cohort birth year fixed effects, interaction between a binary variable indicate the age of an individual in 1992 and the region exposure to the disaster are plausibly exogenous.

Qualitatively, I find that the positive effect of natural disaster on making mistake on risk question, conditional on not committing mistake, but these results are not statistically significant different from zero 15 years after the disaster. The null finding on the effect of disaster on mistake on risk may be related to selection bias since the some fraction of the population migrate after the disaster.

2.2 Related Literature

A first step in understanding the costs of traumatic experience on mistake on risk has been to survey on the research findings that shocks during early childhood has persistent effects on human capital.[1] The literature in developing countries has documented several specific type of early-life shocks that have persistent effect: malnutrition, famine, and war. Maccini and Yang (2009), using rain intensity during year of birth as measure of nutrition, find positive effect of higher early-life rain intensity for adult women; 20 percent increase in local rainfall leads to 0.22 more years of education, and a 0.06-standard deviation increase in an index of house-

[1] For a more comprehensive survey on the empirical findings, the interested reader can consult Currie and Almond (2011) and Currie and Vogl (2013).

hold durable goods ownership. Several research have found that using early-life exposure to famine may have more severe effect on early life nutrition than localized shocks, such as rainfall. Meng and Qian (2009) and Umana-Aponte (2011) find larger effect of famine exposure in utero on subsequent education in China and Uganda, respectively. Cohorts exposed to famine in utero attain approximately 0.58 fewer years in education in China and 0.36 fewer years of education in Uganda. However, these effects may be confounded with selection issue, famine involves high degree of mortality. Like malnutrition and famine, war and exposure to violence have also a short-run effect on health. In Zimbabwe, Alderman et al. (2006), using exposure to civil war and drought as measure of shock, find the negative effect of early child malnutrition on educational attainment; a malnourish child in 1983 is more likely to have less height when becomes young adult and less likely to complete grade in school. Comparing between children exposed to the 1994 Rwanda genocide with older cohort, Akresh and de Walque (2008) find that young cohorts attain 0.42 fewer years of education, the effect is stronger for males and those from non-poor households.

While this present study focuses on mistake on risk question that is more like small stakes risk aversion, it seems unlikely that it represents individual's risk preference. Many papers, however, have empirically estimate the effect of shock on risk preference. The findings from these studies are inconclusive; Some studies find traumatic experience leads to risk aversion (i.e. Callen et al. (2014), Cameron and Shah (2015), Cassar et al. (2011), and Kim and Lee (2014)) while others find it induces risk loving behaviors (i.e. Eckel et al. (2009), Ingwersen (2014), and Voors et al. (2012)).

Callen et al. (2014) conduct an experiment with 816 Afghan people and randomly employ priming mechanism to these people. They find that fearful recollections exacerbate certainty preferences for individuals exposed to violence. The positive effect of traumatic experience on risk aversion is also found when using war as measure for traumatic experience. Kim and Lee (2014) reveal the long-term positive correlation between exposure to Korean War during early child, between age 4 to 8 years old, and certain outcome. They argue that the main channel which traumatic events could affect avoidance to risky outcome is related to the permanent impact to the brain due to traumatic experience, early life traumatic shock may induce abnormalities in the prefrontal cortex regions, regions related with risky decision making, and it may induce a response in abnormal sense.

Using a different measure of traumatic experience, Cameron and Shah (2015) investigate whether experiencing a natural disaster affects risk taking of $1,550$ individuals across 120 rural villages in East Java, Indonesia. Eliciting risk taking behaviors via six gambles with real money stakes they show individuals who have experienced a flood or an earthquake in the past three years are less likely to choose risk tolerant option. Experimenting with 334 subjects in Thailand that live in villages directly hit by the 2004 Asian Tsunami, Cassar et al. (2011) suggest that experiencing traumatic experience drives the effect on risk aversion. Another disastrous events that may have the same effect on risk attitude is exposure to natural disasters. Using this measure, exposure to shock does not always leads to risk averse behaviors. Eckel et al. (2009) find that New Orleans evacuees bussed to Houston, Texas, after hurricane Katrina are more likely to choose risk-loving option compare two groups of people, Houstonians with similar demographics characteristics with the evacuees and the New Orleans evacuees interviewed ten months after moving to Houston. Voors et al. (2012) use violence from conflict as measure for traumatic experience and find that subject with prior exposure or live in the area that have been violently attacked are mo re likely to stay longer in their risky gamble before switching to the safe alternative. Lastly, while Cassar et al. (2011) find that the 2014 Tsunami induce risk aversion among Thai people Ingwersen (2014) reveal that physical exposure to the tsunami, the area which the 2004 Tsunami hit the hardest, leads to temporary decrease in observed risk aversion.[2]

2.3 Flores Earthquake[3]

Indonesia is the world's largest archipelago, with more than 17,000 islands, and sits between the world's most active seismic region, the Pacific Ring of Fire and the Alpide Belt. Sitting between these two means the country could expect to experience some of the strongest earthquakes and powerful volcanic eruption in the world. The U.S. Geological Survey (USGS) estimates that the Pacific Ring of Fire is the world's greatest earthquake belt and source for earthquakes in the world. For example, in the year 2009 Indonesia experience as many as ten earthquakes greater than 6-magnitude. Not only earthquake, Indonesia regularly experiences floods, volcanic eruptions, drought, forest fires, tropical cyclones, and landslide. It is a dangerous country to live.

On December 12, 1992 at 5 h 29 m GMT a large earth-quake with magnitude M_w 7.8 occurred on eastern Flores Island, Indonesia. The earthquake caused major damages for many buildings in Maumere City and its surrounding areas; destroying almost 18,000 homes, 113 schools, 90 religious practices, and about 65 other type of buildings. There were also evidence of liquefaction, sand blows,

[2] This finding however should be viewed with a grain of salt because the survey uses an exactly same instrument to infer about people risk aversion as in this present study. In Ingwersen (2014) study about 50% of the respondents choose dominated option over the lottery despite the lottery carries no risk and yields higher expected value.

[3] I use Tsuji et al. (1995), Yeh et al. (1993), and Beckers and Lay (1995) as main references for this section.

and broken building parts at many places whereas landslide happened in the mountainous area. Several minutes after the shock, tsunamis generated by the earthquake flooded the northeastern part of the island. In Wuring village, 2 km northwest of Maumere, 80% of the wooden house were shattered by the tsunami with total 87 persons of the 1,400 inhabitants died. In Babi island, a diameter of 2.5 km island located 40 km offshore of Maumere City, the tsunami washed away all the houses and took live of 263 persons. The toll of the Flores earthquake is 2080 deaths and 2144, approximately 50% of which are attributed to the tsunamis. 90,000 people were left homeless

2.4 Data and Measurement

2.4.1 Data

This study merges two different level of datasets, the individual-level information where and when an individual was born and region of birth of birth distance to the epicenter of the disaster.

The individual-level data is drawn from the wave four of Indonesia Family Life Survey (IFLS4) which was fielded in 2007. IFLS is a longitudinal, socio-economic household survey. The survey collects rich set of information on adult respondent migration histories, birthplace time and location, location when they were 12 years old, location on the last wave of the IFLS, as well as the number of times the individuals moved since the last wave. For the panel respondent, I extract their migration histories from the wave three of IFLS while for the new respondent the information is extracted from the wave four of IFLS.

The sample is constructed as follows. I focus on male respondents born between 1970 and 1992 since exact birth date and region of birth only available for this group. Next, I limited my sample to respondents who never move from their current residence, in order to avoid distortion due to administrative confusion. I use the information on month of birth and region of birth to precisely identify how old and how far they are when the 1992 Flores Earthquake and Tsunami happen.

2.4.2 Measure of Mistake on Risk

IFLS4 asks two sets of choice task that elicit attitudes toward risk. In each set and task, the interviewer presents two choices: (A). a guaranteed income stream, or (B). a high or low income stream with equal probability. An important caveat is that choices are not in real stakes even though the payoff can be considered as huge. The literature is not clear whether this matters. In on side, Holt and Laury (2002) find contrasting findings when using real stakes and hypothetical stakes; subjects are much risk averse with high real-payoff than with comparable hypothetical payoffs. Dohmen et al. (2011) test whether survey question using hypothetical stake in large-scale sample is correlated with risk-taking behavior using real money in the laboratory setting in smaller sample. They find positive association between responses to a survey item that asks individuals about a judgment of their own willingness to take risk and responses to risk attitudes in paid real-stakes lotteries.[4] Recently, Kang et al. (2012) used functional magnetic resonance imaging (fMRI) to show that common areas of the brain are activated when individuals make real and hypothetical choices about the purchase of consumer goods, but they note that the level of this activity differs.

As mentioned, IFLS4 includes a new section on risk and time preference. Before eliciting risk attitude the respondents were presented with a baseline task as follows

[4]Other papers who found that some of the lottery measures were correlated with survey-elicited preferences are Hamoudi (2006) and Reynaud and Couture (2010).

Suppose you are offered two ways to earn some money.

1. *With option 1, you are guaranteed IDR 800 thousand per month.*
2. *With option 2, you are guaranteed IDR 800 thousand per month, or IDR 1.6 million per month with equal chance.*

Which option will you choose?

Option one is clearly dominated by option two. If a respondent chooses option one, the interviewer then offers the respondent a switch. Those who switch for option 2 then face a sequence of choice task designed to elicit their risk preference, only a handful of respondent switch. Those who stick with choosing the dominated option were then exit the module and I define it as people who commit "mistake on risk".

There are several alternative interpretation that one can use to describe the condition where people stick with the dominated option. One interpretation is that respondent infers option two as gambling and it is immoral to be involved with. Second interpretation is that respondent exhibits high/extreme degree of risk aversion, avoid any option that carries some aspect of uncertainty, either in the outcome or in the probability. Third interpretation is that these respondents may have some cognitive barrier that prevents them choosing the dominant option.

The respondent who passed the filter was asked a sequence of questions where they had to choose between a certain payoff and an uncertain payoff. The sequence where a respondent faced depended on respondent choice in the previous section. For example, if a respondent picked a sure amount in the first question then the next question offered him less risky uncertain amount. Figure 2.1 illustrates the possible sequence of questions that respondent faced in IFLS4. Due to several problems in the module, I decide to use the filter question as my measure for mistake on risk.

2.4.3 Descriptive Statistics

The IFLS wave four is longitudinal data that tracks the original 7,300 households interviewed in 1993 and their splits. Fourteen years after, the households grow to 10,500 households. In this study, I focus on respondents born between 1992 and 1970, which guarantees that the respondents are adult in IFLS 4. Summary statistics for this sample are presented in Table 2.1. There are 6,699 respondents in the sample with average 8.77 years of education. The IFLS elicits risk and time preferences for adult respondents, aged 15 years and above. From this module, 4,062 respondents proceed to risk elicitation process whereas the others exit the process. 39 percent of the respondents choose dominated option and it is higher for respondents live close to the 1992 Flores earthquake.

2.5 Identification Strategy

The month-year of birth and the region of birth jointly determine exposure to the earthquake. Previous have shown that events before the age of five can have long impact on later life outcomes (e.g. Currie and Almond (2011)). All children born in 1987 or after were five years old or younger in 1992, when the Flores' earthquake and tsunami happened. This cohort is most likely to be affected by the disaster. For the control group, studies in psychology find that adolescents/young adults are more likely to take risks than both children and adults (e.g. Arnett (1992) and Mata et al. (2011)). The effect of early child shock for child aged 17 or older in 1992, is expected to be minimal. The effect of the disaster should be economically not meaningful for this cohort.

The exposure to the disaster differs from one region to another and it is related by the distance between the region of birth and the epicenter. Migration after disaster can potentially introduces measurement error, which leads to upward bias in the estimation of the effect of disaster. Endogenous migration might drive the risk averse type individuals to move from high exposure districts after their child were born. Close to epicenter districts are defined as regions where the average of the distance is shorter than the mean distance of all districts from the epicenter.

The identification strategy can be illustrated using the two-by-two tables. Table 2.2 presents means of making mistake on risk for different cohorts and exposure levels. Districts are separated in "close region" and "far region". In panel A, I compare the means of making mistake on risk of respondents who had little exposure to the disaster as child to those respondents who were exposed to the disaster as early childhood. In both cohorts, the number of people making the mistake on risk who live closer to the epicenter are higher than in districts that were further from the epicenter. In both regions, I find no clear pattern for making mistake on risk for young and old respondents. The differences in differences show a child born and raised in the regions close to the 1992 epicenter is no different in making mistake on risk with a child born and raised in the regions far from the epicenter.

In Table 2.2, panel B, I present control experiment. I consider a cohort age 17 to 22 in 1992 and a cohort age 22 to 27 in 1992. The estimated differences in differences are negative and statistically significant. The result in control experiment suggests that a respondent who is old during the disaster and live close to the epicenter on average is less likely to commit mistake on risk by 0.08 percentage point.

2.6 Main Empirical Results

2.6.1 Mistake on Risk Question

In identifying the association between earthquake during early childhood and making mistake on risk question, I estimate the following equation.

$$D_{ijk} = d_1 + \beta_{1j} + \beta_{2k} + (T_i \times P_j)\theta_1 + \varepsilon_{ijk} \tag{2.1}$$

where D_{ijk} is an indicator variable of an individual i, born in region j, in year k, making mistake on risk question. β_{1j} denotes region of birth fixed effect, β_{2k} is the year of birth effect, T_i is a treatment variable that equals one if i's aged in 1992 between 1985 and 1992. The parameter of interest is θ_1. The treatment cohort for this equation is individuals aged 0 to 5 in 1992 whereas the control is individuals aged 17 to 22 in 1992.

The first column of Table 2.3 present estimates for the basic specification of equation (2.1). There is a positive association between earthquake shock during childhood and mistake on risk question during adult, although it is not statistically significant different from zero. The estimates for early childhood shock are not substantially affected by the inclusion of additional control variables. The second column controls for interviewer fixed effects. The third column controls for an indicator variable of whether household head commits a mistake on risk. A disadvantage of estimating equation (2.1) for only two cohorts is that it does not account for pre-existing trends.

In panel B, I estimate equation (2.1) for the control experiment, individuals aged 17 to 22 in 1992 and individual aged 22 to 27 in 1992, to check for pre-existing trends. Column 1 in panel B presents estimate for basic specification (controlling for year of birth fixed and enumeration area fixed effects). Column 2 presents estimates controlling for additional interviewer fixed effects while column 3 presents estimates when indicator variable of whether household head commits mistake on risk question or not. Coefficients in panel B show that he estimated effects are statistically significant different from zero. Interestingly, the estimated effect of early child hood shock on making mistake is positive though magnitude is very small.

The point estimates imply no relationship between exposure to the earthquake to making mistake on risk question. In next sub-section, I proceed to identify alternative hypotheses that may explain this finding.

2.7 Alternative Hypotheses

This study uses a 'cross-sectional' data on individual's response on mistake on risk question collected *after* the natural disasters have occurred. And it may plague the estimates in two ways. First, selective migration of certain type of individuals may confound the estimates. For example, people with finer mistake may be more likely to leave an area after a disaster and they are not be observed in post-disaster cross-sectional data. Second, people who live close to disaster areas might be different than people who live far from disaster areas. The unobserved heterogeneity may produce biased estimates under cross-sectional based studies. I empirically examine the extent of selection bias in the following paragraph.

In Table 2.4, I regress distance to disaster epicenter on the logarithm of household per capita monthly expenditure using IFLS data. The first best solution, I would regress with pre-disaster wealth but I do not have access to that measure. The results from Table 2.4 present the estimate for two different specifications, without and with controlling for interviewer fixed effects, and two different measures for distance to epicenter, the logarithm of distance to epicenter and binary variable that indicates closeness to epicenter. After controlling for interviewer fixed effects, I find no significant correlation between log household expenditure per capita and distance to epicenter. It appears there is no indication of selection effect along the household per capita expenditure. This finding is in line with Cameron and Shah's (2015) who find that post-disaster wealth has no significant effect explaining household behavior to avoid Earthquake and Flood in East Java, Indonesia. They conclude that East Java people who experience natural disasters are not different to those who did not experience natural disasters.

To further analyze the degree to which selection is likely to be a problem, I examine correlation between migration rates and the logarithm of distance to epicenter using the 1995 SUPAS data.[5] The data was collected by Indonesia's statistical agency in 1995, thereby it could inform which household migrate between 1990 and 1995. Approximately 1.1 to 1.7 percent of the sample moved to the current place of living in the past five years. As stated the Flores earthquake happened in December 1992, thus I could test whether distance to Flores island impacted the decision to move. In Table 2.6, I regress household migration decision to migrate on the logarithm of distance to epicenter. Table 2.6 presents the results of migration decision for household who have migrated and live the current place less than five years. All specifications include years of education, gender, age, urban/rural status, district fixed effects, marital status, and duration of stay in the current place. As expected, I find that exposure to Flores earthquake is negatively associated with probability of moving. Interestingly, one year after the disaster the distance to disaster epicenter is not significantly correlated with household decision to move. I find that there is a mild evidence that distance to high risk area correlates with decision to migrate. Using the first and second wave

[5] According to the 1995 SUPAS data, 14.4 percent of HH migrated due to occupation and 30 percent of HH moved because parents and following spouses.

of the IFLS and focusing on rural people migration behavior, Cameron and Shah (2015) find that only 14.4 percent of sample experience a flood or an earthquake between 1990 and 1994 and only handful of people, 16.2 percent of those who experience flood or earthquake, decide to migrate.

2.8 Conclusion

In this study, I aim to identify whether a child exposure to the 1992 Flores Earthquake and Tsunami explain the observed mistake on risk that they commit during early adult. The evidences in this study point to no correlation between exposure to the earthquake and making mistake on risk. Furthermore, this study cannot find enough evidence that long-run mistake on risk of early child is sensitive to the environmental conditions they experienced early in life. There is mild evidence of selection bias, which suggest that individual select out from the disaster area after the disaster.

Figure 2.1: IFLS risk elicitation process

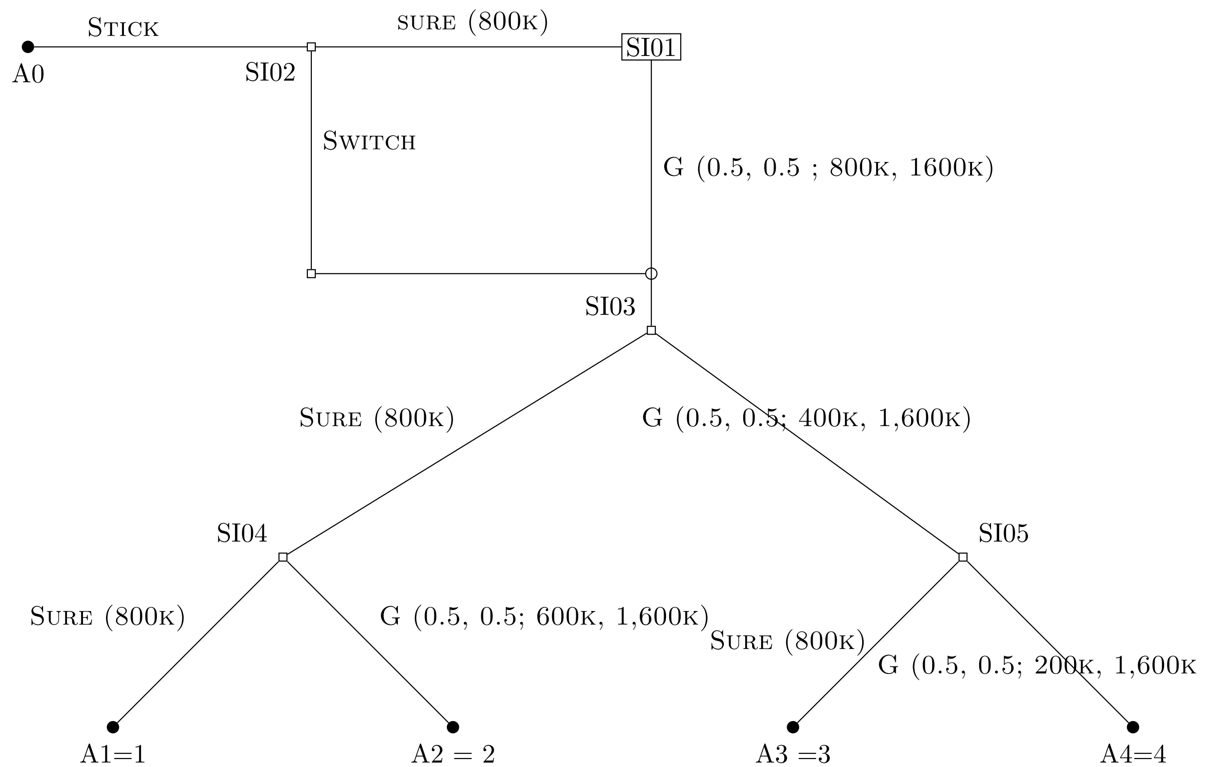

Table 2.1. Descriptive Statistics

	Close		Far	
	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)
Years of Education	8.72	3.52	8.83	3.22
Mistake on Risk	0.42	0.49	0.36	0.48
HH Head Making Mistake	0.39	0.49	0.40	0.49
log(PCE)	26.43	0.83	26.58	0.90

Sources: IFLS4.

Table 2.2. Means of Mistake on Risk by Cohort and Distance

	Close	Far	Difference
	(1)	(2)	(3)
Panel A. Experiment of Interest			
Aged 0 to 5 in 1992	0.41	0.35	0.06
	(0.013)	(0.013)	(0.019)
Aged 17 to 22 in 1992	0.40	0.37	0.03
	(0.014)	(0.015)	(0.021)
Difference	0.01	−0.02	0.03
	(0.019)	(0.020)	(0.028)
Panel B. Control Experiment			
Aged 17 to 22 in 1992	0.40	0.37	0.03
	(0.014)	(0.015)	(0.021)
Aged 22 to 27 in 1992	0.47	0.36	0.11
	(0.015)	(0.017)	(0.023)
Difference	−0.07	0.01	−0.08
	(0.021)	(0.023)	(0.031)

Notes: Standard errors are in parentheses.

Table 2.3. Coefficient of the Interaction between young dummies and the distance to epicenter

	Dominated Choice		
	(1)	(2)	(3)
Panel A. Experiment of Interest			
(Youngest cohort: 0 to 5 in 1992)			
Young $\times$ Distance	0.004	0.002	0.003
	(0.0039)	(0.0037)	(0.0041)
Panel B. Control Experiment			
(Youngest cohort: 17 to 22 in 1992)			
Young $\times$ Distance	-0.006	-0.005	-0.006
	(0.0043)	(0.0038)	(0.0039)
Controls			
Year of birth fixed effects	Yes	Yes	Yes
Enumeration area fixed effects	Yes	Yes	Yes
Interviewer fixed effects	No	Yes	Yes
HH head choose dominated option	No	No	Yes

Table 2.4. Correlates of Distance to Epicenter

	log(Distance to Epicenter)		Close to Epicenter	
	(1)	(2)	(3)	(4)
log(PCE)	0.0011101	-0.0001853	0.0020025	-0.0006066
	(0.0003954)***	(0.0003388)	(0.0011957)*	(0.0008146)
Controls:				
Enumeration area FEs	Yes	Yes	Yes	Yes
Interviewer FEs	No	Yes	No	Yes
Observation	16,370	16,370	5,900	5,900

Notes: Close to epicenter is binary variable that equals one if district distance to epicenter is below national average. All specifications include years of education, gender, age, and urban/rural status. *** indicates significance *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 2.5. Reason for migration

	Frequency (%)
Occupation	15.54
Looking for job	11.54
Education	9.32
Marital Status	4.60
Following family member	48.76
Housing	6.92
Other	3.32

Source: 1995 Inter-census Population Survey (SUPAS), BPS Indonesia.

Table 2.6. Correlate of Migration in the past Five Years

	(1)	(2)	(3)	(4)	(5)
Panel A.					
log(Distance to Epicenter)	-0.00080	-0.00014	0.00039	-0.00111	-0.00081
	(0.00049)	(0.00056)	(0.00054)	(0.00051)*	(0.00045)*
Year moving in current place	1994	1993	1992	1991	1990
R^2	0.81	0.80	0.78	0.78	0.79
Observation	688,839	688,839	688,839	688,839	688,839
Mean Dep. Var	0.014	0.017	0.015	0.013	0.011
Std. Dev. Dep. Var	0.119	0.131	0.122	0.113	0.104
Panel B.					
log(Distance to Epicenter)	-0.00080	-0.00094	-0.00054	-0.00165	-0.00246
	(0.00049)	(0.00074)	(0.00092)	(0.00105)	(0.00114)*
Duration in current place (year)	One	Two	Three	Four	Five
R^2	0.81	0.80	0.79	0.78	0.78
Observation	688,839	688,839	688,839	688,839	688,839
Mean Dep. Var	0.014	0.032	0.047	0.060	0.071
Std. Dev. Dep. Var	0.119	0.175	0.211	0.237	0.256

Notes: Close to epicenter is binary variable that equals one if district distance to epicenter is below national average. All specifications include years of education, gender, age, and urban/rural status. *** indicates significance *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Chapter 3

The Long Term Effects of the School Construction Program on Education and Non-Farm Business Profits in Indonesia[1]

3.1 Introduction

This study extends earlier work by Duflo (2001) by evaluating the long-run effects of the school construction program (INPRES). Duflo (2001) finds an improvement in educational attainment and wage among adult male. These results

[1]I acknowledge BPS for the access to the 1980 PODES data and the 1971 Census.

suggest the program's success in improving education and labor market outcomes. However, there has been no follow-up of the program's effect on the self-employed workers despite finding that 45% of the Indonesia's labor force participate in the wage sector.

The Sekolah Dasar INPRES program was implemented to improve basic education of Indonesia people, especially in areas with low enrolled-children. In addition to school construction, the government also invested heavily to increase the number of teachers and improve the quality of instruction (Prawiro (1998)). Between 1969 and 1984 the number of children enrolled in Indonesia schools rose from 16.8 million to 33.2 million, more than 100 percent increase. The number of books purchased also increased significantly, from 57 million in 1969 to 246 million in 1984. Ministry of Education grew to become the largest employer in the nation, 48% of the Indonesia's civil servants, due to all public school teachers are by law government employee. The schools were expected to lower the cost of acquiring basic education for those who live in remote areas. The schools were built more in the area with low number of children enrolled in the education system in 1972, prior to the start of the program. Thus, new schools in different areas were built at different times and non-randomly. The variations induced by difference in individual year of birth and region of birth are random. Using these two sources of variation, Duflo (2001) estimates the causal effect of the program using Difference-in-Difference (DID) on years of education and hourly wage.

To extend Duflo's (2001) work, this study analyzes whether school construction program improves self-employed education and earning outcomes. I make two modifications. First, I use the Indonesia Family Life Survey (IFLS) sample rather than the 1995 Intercensal Population survey (SUPAS). Second, I use the 1980 Village Potential Census (PODES) data that records the number of school built using INPRES fund in every village. I mimic Duflo's (2001) sample constructions, identification strategy and estimation methods.[2] The instrumental variables are interactions between dummy variables indicating agen in 1974 and the intensity of the program. Qualitatively, I find similar results to Duflo's (2001), positive treatment effect on educational attainment and positive treatment effect on the log annual profit, but these results are not statistically significant. I cannot reject the effect sizes equal to Duflo (2001).

3.2 Data and Measurement

3.2.1 Data

I use three national data for this study. First, The Fourth Wave of the IFLS. Due to longitudinal structure, IFLS4 drew its sample from IFLS 3, IFLS 2, and IFLS 1. The IFLS 1 is representative of about 83% of the Indonesian population in 1993. The sampling scheme stratified on province and urban/rural locations. Within each selected province, enumeration areas (EAs) were randomly chose and total of 321 enumeration areas were selected. IFLS4 interviewed 13,535 household, grew from 7,224 households in IFLS1.

Second, I use the 1980 PODES. PODES is a tri-annual administrative census of village characteristics. In 1980 the data included total number of schools built using the INPRES program and total number public and family toilets built using the INPRES fund. Third, I use the 1971 Population Census. The 1971 population census provided information on the enrollment rate at the district level.

[2] I also carry out the same analysis as Duflo (2001) and the results are available in appendix B.2.

3.2.2 Summary Statistics

I focus on men, mimicking Duflo (2001), who owns or shares responsibility of running non-farm business and is able to keep their business open when the survey took place. Table 3.1 presents summary statistics of selected sample and program exposure at the region of birth. Panel A presents the individual level statistics while panel B shows region level statistics. One noticeable feature is that the number of sample in this study is a small fraction of Duflo's (2001), where sample with valid earning is about 2% of the original study suggesting that the power of this study will be limited. Average level of education for sample with valid profit data is 8.28 years of education, slightly higher than average level of education for the whole sample. The INPRES program aimed to build one school for every 500 children. Using planned number of school, Duflo (2001) find that the program achieved its target, 2.44 schools per 1,000 children. This study finds that the number of school built per 1,000 children is 1.76 schools, less than the number from the original study. The outcome of interest is earning, while Duflo (2001) uses sample for those who participate in wage sector, this study uses net profit earn by self-employed individuals. In panel B, I find the average of school built per district is 195 schools whereas the district mean for number of school constructed per 1,000 children is 1.49 schools.

3.3 Identification Strategy

The identification assumption assumes no omitted time-varying and region specific effects correlated with the program, it is violated if the expected benefit of building school is correlated with the initial level. And also, if the allocation of other INPRES programs was correlated with the allocation of fund for building schools. To take into account the possible correlation between the school construction and the other INPRES program, Duflo (2001) estimates her model by including the interaction of individual cohort and other INPRES program, water and sanitation, and interaction between the individual cohort and the preprogram enrollment rate. The possible of general equilibrium effects in the education and labor market is another issue that may arise given the size of the program. In the education sector, more public schools may crowd-out private or community funded schools, diminish the benefits from the building schools. In the labor market, more schools makes skilled workers more abundant thus lower earnings received while it may at the same time attracts more capital and technology companies which offer higher earning for workers.

3.4 Results

3.4.1 School Allocation

The allocation of INPRES schools stated that the schools should be built proportional to the number of unenrolled students. I first check whether the use of PODES to generate school information on the INPRES schools points to the same finding as Duflo (2001): the stated allocation rule is not strictly followed. Table 3.2 presents the results of district-level regression of the number of schools built using INPRES fund on number of children aged $5-14$ and rate of un-enrolled children in the primary school. Following the allocation rule would imply that both coefficients on the independent variables are close to one. Column (1) presents the estimates from Duflo's (2001) where column (2) presents the estimates from this study. Both study find positive correlations between the number of schools and the number of children and between the number of schools and the one minus enrollment rate. This study finds the coefficient of the nonenrollment rate higher than Duflo (2001), suggesting differences between (i). the 1971 Census data, that this study uses to calculate the number of enrollment rate in pre-program year, and the 1973 Ministry of Education data on enrollment rate, and (ii). the 1980 PODES data, that this study uses to calculate the number of INPRES schools, and the planned number of INPRES schools used by the original study.

3.4.2 Sources of Variation

Duflo (2001) uses two sources of variation to estimate the INPRES program effect on education and earning in wage sector: variation on the number schools built in each districts, and cohort exposure. The program was meant to build primary school buildings and it benefited kids aged 7 and 12 in 1974. The government first allocated funds for building schools on the fiscal year (FY) 1973/1974. The INPRES school was first opened in 1974, a child who was older than 12 years old in 1974 would not benefit from the program.

The identification strategy can be illustrated by simple two-by-two table. Table 3.3 presents means of education and logarithm of annual profit for various cohorts and program intensity. Panel A is the main experiment of interest, comparing cohort who were exposed to the program to cohort who were not exposed at all to the program. The average educational attainment and non-business profits in regions that received less schools are higher than in regions that received more schools since the program targeted regions with low enrollment rate at the primary school. In line with Duflo (2001), I find that average educational attainment increased over time and it is greater in the regions that received more schools. The difference in these differences is the causal effect of the program, assuming the increased is not statistically different between the low and the high intensity regions in the absence of the program. A young individual, born in a high intensity region, received on average 0.91 more years of education and the logarithm of profit was 0.104 higher. However, these differences in differences are much higher than Duflo's (2001) estimate of 0.12 gain in education and a 0.026 gain in the logarithm of wages. I reach to the same conclusion that these numbers are not statistically different from zero.

Duflo's (2001) identification assumption can be tested. Individuals aged 12 or older in 1974 were not exposed and thus the increase in education should not differ systematically between regions for these cohorts. Panel B presents the control experiment, comparing aged 12 to 17 in 1974 and aged 18 to 24 in 1974. In absolute value, the estimated difference in differences are closer to 0, both for education and logarithm of annual profit than the estimated difference in difference for the main experiment cohorts. Despite the calculations in both panels are imprecisely estimated the signs of the difference in differences for the main experiment cohorts in this study are same as the original study, which is a reassuring. This lends some support for this study to use Duflo's (2001) approach.

One side note, pre-treatment education level using IFLS data is very much different than the pre-treatment education level in the original study. This study finds that the pre-treatment number in high intensity regions is 6.31 years where the pre-treatment number in low intensity regions is 8.31 years. The original study finds that the pre-treatment level in high intensity regions is 8 years whereas the pre-treatment level in low intensity regions is 9.4 years. One possible explanation is the differences in the individual-level data. This study uses IFLS that covers 13 provinces that represented by 321 enumeration areas (EAs), over sampling urban EAs and EAs in smaller provinces (Strauss et al. (2009)). Meanwhile the original study uses 1995 SUPAS covers all provinces and districts. The SUPAS EAs are randomly selected thus the number from it could be inferred as representative of Indonesia's population.

3.4.3 Program Effect on Education

Basic Results

Following Duflo (2001), I estimate the effect of the program on education (S) with the following specification:

$$S_{ijk} = c_1 + \alpha_{1j} + \beta_{1k} + \Big(P_j \times T_i\Big)\gamma_1 + \Big(\mathbf{C_j} \times \mathbf{T_i}\Big)\delta_1 + \epsilon_{ijk} \tag{3.1}$$

where T_i is a treatment indicator which equals one if individual's aged is between 2 to 6 in 1974 and zero otherwise, c_1 is a constant, β_k is a year of birth fixed effect, α_j is region of birth fixed effect, P_j denotes the number of schools constructed per 1,000 children in the region of birth.

Table 3.4 presents the estimates of equation (3.1) for two subsamples. Panel A presents the estimate program effect for the main experiment, comparing children aged 2 to 6 in 1974 with children aged 12 to 17 in 1974. Additional school built per 1,000 children increased the educational attainment for children aged 2 to 6 in 1974 by 0.05 years for the whole sample, and 0.87 years for the sample with valid profit. This last number is comparable to the figure from the 2x2 table before, that is 0.91 years. These results are not statistically significant different from zero. A major issue here is that due to low sample sizes, the standard errors from this study is about 10 times bigger than Duflo's (2001). So these are very imprecise findings. The validity of positive effect of the program depends whether equation (3.1) has take into account all the time-varying and region specific effects correlated with the program. Noting that the schools were built in reference to regions' enrollment rate in primary education. Column (2) presents the estimate of the program effect when controlling for enrollment rate in pre-program period. One school per 1,000 children decreased the educational attainment for treatment group by 0.03 years for the whole sample in contrast to an increased education of treatment group by 0.8 years for the sample with valid profit data. Column (3) presents the estimate of building one school per 1,000 children when controlling for other programs financed by the same fund. The results remained steady when controlling for other programs financed by the INPRES fund and statistically not significant from zero.

In panel B I presents the estimate of the program effect for the control experiment, comparing children aged 12 to 17 in 1974 with children aged 18 to 24 in 1974. Like in panel A, I find the impact of one school built per 1,000 children is very minimal and not statistically significant.

Restricted Estimation

As stated in previous sections children aged 12 or older in 1974 did not benefit from the program. The treatment effect for these cohort should be zero. Imposing this restriction on equation (3.1) and modifying the treatment status on equation (3.1), I estimate the following reduced-form specification.

$$S_{ijk} = c_1^1 + \alpha_{1j}^1 + \beta_{1k}^1 + \sum_{l=2}^{12} \Big(P_j \times d_{il}\Big)\gamma_{1l}^1 + \sum_{l=2}^{12} \Big(\mathbf{C_j} \times d_{il}\Big)\delta_{1l}^1 + \epsilon_{ijk} \tag{3.2}$$

where d_{il} is an indicator variable that takes value one if individual i is age l in 1974 and zero otherwise. The omitted group (the control group) is children aged 13 to 24 in 1974. Each coefficient γ_{1l}^1 represents the effect of building one more school per 1,000 children on given cohort.

Columns 1 to 6 in table 3.5 provide estimates of coefficient γ_1^1 for each cohort l

for whole sample and sample with valid profit data. In columns 1 and 4, I estimate model (3.2) without take into account pre-program enrollment rate and other INPRES programs. Columns 2 and 5 present the effect of one school per 1,000 children controlling for pre-program enrollment rate. Column 3 and 6 present the effect of one school per 1,000 children controlling for pre-program enrollment rate and other INPRES programs.

The expected pattern of coefficients should be decreasing with respect to age, younger cohort should get higher benefit than the old cohort. I found not enough evidence to reject the hypothesis of no effect of the program on cohort's educational attainment for almost all cohorts in the full sample and sample with valid profit data. For some cohort, I find negative treatment effects though the effects are not statistically significant different from zero.

3.4.4 Program Effect on Non-Farm Business Profit

Basic Results

I estimate the effect of the program on self-employed earning as measured by the logarithm of non-farm business profit (Y).

$$Y_{ijk} = c_1^0 + \alpha_{1j}^0 + \beta_{1k}^0 + \Big(P_j \times T_i\Big)\gamma_1^0 + \Big(\mathbf{C_j} \times \mathbf{T_i}\Big)\delta_1^0 + \epsilon_{ijk}^0 \qquad (3.3)$$

where c_1^0 is a constant, β_k^0 is year of birth fixed effect, α_j^0 is region of birth fixed effect, P_j denotes the number of schools constructed per 1,000 children in the region of birth. Columns 4 to 6 of Table (3.4) present the effect of building one more school per 1,000 children on profit for the main experiment cohorts while panel B present the building one more school per 1,000 children for the control experiment cohort. Same as when presenting the program effect on education, I start by estimating specification (3.3) without controlling for pre-program enrollment rate and other INPRES programs, adding pre-program enrollment rate and full controls (with pre-program enrollment rate and other INPRES programs).

In table 3.4, panel A, the estimates range between 0.1 percent to 10 percent, depending on the control variables. The estimate increases when pre-program enrollment rate are included while it is slightly decreases from previous column when other INPRES programs are controlled. These effects are imprecisely estimated both for the main and for the control experiments. All the coefficients of the program effect on the logarithm of annual profit are not statistically significant, they are positive, which is consistent with the original study. I find that the joint set of instruments are not different from zero, statistically.

Restricted Estimation

I present estimates of the equation in columns 7 to 9 of Table (3.5)

$$Y_{ijk} = c_1^2 + \alpha_{1j}^2 + \beta_{1k}^2 + \sum_{l=2}^{12} \Big(P_j \times d_{il}\Big)\gamma_{1l}^2 + \sum_{l=2}^{12} \Big(\mathbf{C_j} \times d_{il}\Big)\delta_{1l}^2 + \epsilon_{ijk}^2 \tag{3.4}$$

Like in education, it is difficult to get a precise estimate the effect of the INPRES program on annual profit because profits vary across people and the size of the size of the sample is small No surprise that I find all the coefficients are statistically insignificant and the joint set of instruments are close to zero. Qualitatively, the results echo the estimated effects on educational attainment. Most coefficients, however, are positive but no discernible patterns could be inferred from the results. The estimates are higher for some cohorts when I control for district pre-program enrollment and other INPRES programs.

3.5 Conclusion

I replicate Duflo (2001) in a sample of self-employed workers in the IFLS4. The downside is that the sample is very small compared to the original study. I cannot reject her estimates, though my estimates are very imprecise. Additional research is needed to better understand the effect of that large-scale program, especially on self-employed workers, as it comprises almost 70% of Indonesian's labor force.

Table 3.1. Descriptive Statistics

	Duflo (2001)		IFLS (2007)	
	Obs	Mean	Obs	Mean
	(1)	(2)	(3)	(4)
Panel A: Individual Level				
Education (whole sample)	152,989	7.98	4,610	8.18
Education (with valid earning)	60,663	9.00	1,368	8.28
INPRES schools built per 1,000 children		1.98		1.35
INPRES schools built per 1,000 children (with valid earning)		1.89		1.31
INPRES schools built per 1,000 children (High program regions)		2.44		1.76
INPRES schools built per 1,000 children (Low program regions)		1.54		0.89
Monthly earnings, thousands Rupiah		205		1,191
Panel B: District Level				
INPRES schools constructed	293	222	191	195
INPRES schools constructed per 1,000 children		2.34		1.49
Fraction of the population attending school in 1971 (Census)		0.174		
Enrollment rate in primary school		0.68		0.47

Sources: Duflo (2001), IFLS4, Census 1971, PODES 1980

Table 3.2. The Allocation of schools

	Log(INPRES schools)[a]	
	Duflo (2001)	IFLS (2007)
	(1)	(2)
Log of number of children aged $5-14$ in the region	0.78	0.74
	(0.027)***	(0.043)***
Log(1−enrollment rate in primary school)[b]	0.12	0.47
	(0.038)***	(0.138)***
Number of observations	255	177
R^2	0.78	0.67

Notes: Standard errors are in parentheses. *** indicates significance *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

[a] The dependent variable is the log of the number of INPRES schools built between 1973 and 1978

[b] The enrollment rate in primary school is the number of children enrolled in primary school (preprogram) divided by the number of children aged $5-14$ in the region (preprogram)

Table 3.3. MEANS OF EDUCATION AND LOG(ANNUAL PROFIT) BY COHORT AND LEVEL OF PROGRAM CELLS

	Years of Education			Log(Annual Profit)		
	High (1)	Low (2)	Difference (3)	High (4)	Low (5)	Difference (6)
Panel A: Experiment of Interest						
Aged 2 to 6 in 1974	9.109	10.194	−1.085	15.628	15.799	−0.172
	(0.279)	(0.275)	(0.396)	(0.089)	(0.093)	(0.168)
Aged 12 to 17 in 1974	6.309	8.307	−1.999	15.4889	15.765	−0.276
	(0.292)	(0.318)	(0.434)	(0.095)	(0.096)	(0.281)
Difference	2.800	1.886	0.914	0.139	0.035	0.104
	(0.407)	(0.419)	(0.585)	(0.131)	(0.131)	(0.188)
N			738			738
Panel B: Control Experiment						
Aged 12 to 17 in 1974	6.309	8.307	−1.999	15.4889	15.765	−0.276
	(0.292)	(0.318)	(0.434)	(0.095)	(0.096)	(0.281)
Aged 18 to 24 in 1974	6.506	8.133	−0.997	15.489	15.783	−0.294
	(0.289)	(0.305)	(0.421)	(0.093)	(0.104)	(0.292)
Difference	−0.197	0.174	−0.372	0.000	−0.018	0.018
	(0.413)	(0.442)	(0.606)	(0.134)	(0.141)	(0.195)
N			656			656

Notes: The sample is made of the individuals who owns non-farm business and generate valid profit data. Standard errors are in parentheses.

Table 3.4. Effect of the program on education & Non-Farm Business Profit: Coefficients of the Interactions between cohort dummies and the number of schools constructed per 1,000 children in the region of birth

		Dependent variable					
		Years of education			Log(Annual Profit)		
	Obs	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest:							
Whole sample	2,411	0.0454	−0.0371	−0.0116			
		(0.2393)	(0.2445)	(0.2475)			
Sample with valid profit	705	0.8678	0.8583	0.7853	0.0068	0.1024	0.0941
		(0.6008)	(0.6324)	(0.6391)	(0.2090)	(0.2208)	(0.2201)
Panel B: Control Experiment							
Whole sample	2,034	−0.0888	−0.0564	0.0112			
		(0.3265)	(0.3372)	(0.3439)			
Sample with valid profit	629	−0.3065	0.0177	0.2441	0.1970	0.1343	0.2139
		(0.5895)	(0.6273)	(0.6469)	(0.2158)	(0.2158)	(0.2206)
Control variables:							
Year of birth*enrollment rate in 1971		No	Yes	Yes	No	Yes	Yes
Year of birth*water and sanitation program		No	No	Yes	No	Yes	Yes

Notes: All specifications include region of birth dummies, year of birth dummies, and interactions between year of birth dummies and the number of children in the region of birth. Standard errors are in parentheses.

Table 3.5. Effect of the Program on Education and Wages: Coefficients of the Interactions between Dummies Indicating Age in 1974 and the Number of Schools Constructed per 1,000 children in Region of Birth

	Dependent var: years of education						Dependent var:		
	Whole sample			Valid Profit Data			log(Annual Profit)		
Age in 1974	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2	-0.313	-0.531	-0.367	-0.814	-0.975	-0.677	-0.260	-0.331	-0.308
	(0.321)	(0.362)	(0.369)	(0.527)	(0.632)	(0.617)	(0.163)	(0.189)	(0.192)
3	0.115	-0.020	0.037	0.012	-0.113	0.442	0.271	0.231	0.336
	(0.295)	(0.355)	(0.356)	(0.836)	(0.996)	(0.949)	(0.156)	(0.186)	(0.194)
4	0.300	0.340	0.380	-0.019	0.049	0.085	-0.061	-0.033	-0.078
	(0.452)	(0.482)	(0.494)	(0.874)	(0.968)	(0.999)	(0.176)	(0.179)	(0.192)
5	0.125	0.044	0.050	-0.655	-0.733	-0.790	-0.071	-0.011	-0.023
	(0.309)	(0.348)	(0.344)	(0.752)	(0.915)	(0.963)	(0.195)	(0.206)	(0.210)
6	-0.328	-0.262	-0.311	-0.282	-0.228	-0.135	-0.038	0.007	-0.024
	(0.359)	(0.376)	(0.383)	(0.468)	(0.456)	(0.469)	(0.213)	(0.212)	(0.228)

Table 3.5 – *Continued on next page*

Table 3.5 – *Continued from previous page*

Age in 1974	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
7	-1.024	-0.927	-0.930	-1.268	-1.287	-1.278	0.186	0.194	0.120
	(0.312)**	(0.338)**	(0.364)*	(0.401)**	(0.390)**	(0.454)**	(0.173)	(0.157)	(0.185)
8	-0.332	-0.345	-0.365	0.475	0.732	0.533	0.228	0.301	0.273
	(0.386)	(0.379)	(0.384)	(0.956)	(0.967)	(0.930)	(0.198)	(0.243)	(0.245)
9	0.157	0.161	0.035	1.658	1.697	1.574	0.089	0.136	0.139
	(0.447)	(0.454)	(0.481)	(0.868)	(0.888)	(0.938)	(0.280)	(0.249)	(0.259)
10	0.823	1.018	0.987	0.887	1.255	1.087	-0.150	-0.141	-0.179
	(0.487)	(0.503)*	(0.500)*	(0.919)	(1.077)	(1.097)	(0.328)	(0.369)	(0.369)
11	0.177	0.321	0.313	-0.443	0.169	0.182	-0.293	-0.163	-0.155
	(0.579)	(0.624)	(0.625)	(0.829)	(0.898)	(0.898)	(0.281)	(0.270)	(0.272)
12	-0.217	-0.136	-0.141	-0.942	-0.908	-0.936	-0.161	-0.052	-0.068
	(0.404)	(0.337)	(0.356)	(0.763)	(0.874)	(0.894)	(0.224)	(0.220)	(0.226)
Control vars:[a]									
Birth year*enrl	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Table 3.5 – *Continued on next page*

Table 3.5 – *Continued from previous page*

Age in 1974	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
rate in 1971									
Birth year*pub & Family Program	No	No	Yes	No	No	Yes	No	No	Yes
R^2	0.25	0.25	0.25	0.24	0.25	0.25	0.19	0.19	0.20
N	4,589	4,589	4,589	2,174	2,174	2,174	2,175	2,175	2,175
F-Stat	0.057	0.023	0.018	0.060	0.003	0.000	0.028	0.007	0.000

Notes: All specifications include region of birth dummies, year of birth dummies, and interactions between the year of birth dummies and the number children in the region of birth (in 1971). Standard errors are in parentheses.

[a] The control group is comprised of individuals aged 13-24 in 1974.

[b] The F-stat test the hypothesis that the coefficients of the interaction between the year of birth dummies and the program intensity in the region of birth are jointly zero.

Appendix B

B.1 IFLS1 Sampling Design[1]

The IFLS sampling scheme stratified on provinces, then randomly sampled within provinces. Provinces were selected with respect to two considerations: population representation and cost. The far eastern provinces, three provinces on Sumatra Island, three provinces on Kalimantan, and three provinces on Sulawesi were left out due to high cost while Aceh was deleted out of concern for the area's political violence and the potential risk to interviewers.

The IFLS randomly selected enumeration areas (EAs) within each of the 13 provinces. The EAs were chosen from a nationally representative sample frame used in the 1993 National Socioeconomic Household Survey (SUSENAS). The SUSENAS frame is based on the 1990 Population census. The SUSENAS EAs each contain between 200 and 300 households, only on a small area EAs contain 60 to 70 households. Using the SUSENAS frame, the IFLS randomly choose 321 enumeration areas in the 13 provinces. Using a direct proportional sample most enumeration areas will be dominated by EAs in the Island of Java, more than 50 percent of the population live in Java. Thus IFLS put higher weight for EAs in urban and for EAs in smaller provinces to facilitate urban-rural and Java non-Java

[1] I use Strauss et al. (2009) as main reference for this part.

comparisons.

Within a selected EAs, households were randomly selected from the 1993 SUSENAS household listings from regional offices of the Central Statistical Agency (BPS). Twenty households were selected from each urban EA while thirty households were selected from each EA. IFLS uses BPS's definition of household, a group of people whose members reside in the same dwelling and share food from the same cooking pot. A total of 7,730 households were sampled to obtain a final sample size of 7,000 completed households. Of the 7,730 household sampled, a complete interview was obtained for 91.1 percent of households. The final sample of 7,224 partially or fully completed households consists of 3,436 households in urban areas (90.7 percent partial/full completion rate), and 3,788 households in rural areas (95.9 percent partial/full completion rate).

B.2 Pure Replication

This study adapts Duflo's (2001) strategy to find the effect of school construction program on education and worker's earning. To check whether the adopted approach is correct it is necessary to recover the original results when same information is used. This appendix presents pure replication of Duflo's (2001) study. First, I utilize the 1995 Intercensal Population Survey (SUPAS) to extract individual's wage and educational history, same as original study. Second, I attain the number of schools financed by the INPRES program in each district from the presidential decrees (*instruksi presiden*), same as the original study. Third, I use the 1971 Indonesia's Census to extract district-level number of children, same as the original study. For district-level preprogram enrollment rate, I aggregate individual-level education status from the 1971 Indonesia's Census while Duflo (2001) uses the 1973 enrollment data from the Ministry of National Education and Culture (MoNE). Thus with all that information, I expect the replication exercises in this section produce similar results.

Table B.1 compares the simple two-by-two tables of the replication and the original exercises. Panel I shows means of education and hourly wages for various cohorts, experiment and control, and program levels from the replication exercises. I present results from the original study in panel II. From the replication exercises, I find that the average educational attainment and hourly wages in regions that received more schools are lower than in regions that received less schools. The causal estimate of the program on education is 0.16 years, slightly higher than Duflo's (2001) estimate of 0.12 years. The causal estimate of the program on the logarithm of hourly wages is 0.006, lower than Duflo's (2001) estimate. The Wald estimate of returns to education is 26 percent, significantly larger than Duflo's (2001) of 5 percent.

Despite using the same information with the original study, I can not fully produce the same magnitudes of the INPRES program effect on educational attainment and the logarithm of hourly wage. Why there are still some discrepancies between the replication and the original study? First, construction of years of education. In the replication analysis years of education is calculated based on the variables of the current or achieved grade and the attainment of education level in the 1995 Intercensal Population Survey (SUPAS) questionnaire (UNDP et al. (2004, p. 198)) while Duflo (2001) provides no information on the conversion from SUPAS questionnaire education to years education. Second, merging between individual-level and regional-level information. The original study uses regional information on the number of schools and the individual's education and wage in 1995. This create a complication, as some district changed their name and boundaries between 1979 and 1995. To resolve the possible ambiguities the original study uses maps of Indonesia to match individual and regional level data while this study uses both district code and names published by the Indonesia's Central Statistical Agency as means for matching, its a match if district code and name agree.

Table B.1. Means of Education and Log(Hourly Wage) by Cohort and Level of Program Cells

	Years of Education			Log(Hourly Wage)		
	High	Low	Difference	High	Low	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel I. Replication Exercises						
Panel I.A: Experiment of Interest						
Aged 2 to 6 in 1974	8.93	10.19	−1.27	6.59	6.76	−0.17
	(0.040)	(0.040)	(0.057)	(0.0076)	(0.0081)	(0.011)
Aged 12 to 17 in 1974	8.44	9.86	−1.42	6.84	7.02	−0.18
	(0.050)	(0.047)	(0.069)	(0.0082)	(0.0084)	(0.012)
Difference	0.49	0.33	0.16	−0.26	−0.26	0.006
	(0.064)	(0.063)	(0.090)	(0.011)	(0.012)	(0.016)
Panel I.B: Control Experiment						
Aged 12 to 17 in 1974	8.44	9.86	−1.42	6.84	7.02	−0.18
	(0.050)	(0.047)	(0.069)	(0.0082)	(0.0084)	(0.012)

Table B.1 – *Continued on next page*

Table B.1 – *Continued from previous page*

	High	Low	Difference	High	Low	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Aged 18 to 24 in 1974	8.22	9.67	−1.45	6.93	7.10	−0.18
	(0.054)	(0.050)	(0.074)	(0.0925)	(0.0092)	(0.013)
Difference	0.22	0.19	0.03	−0.081	−0.083	0.002
	(0.074)	(0.069)	(0.101)	(0.012)	(0.012)	(0.018)
Panel II. Original Study						
Panel II.A: Experiment of Interest						
Aged 2 to 6 in 1974	8.49	9.76	−1.27	6.61	6.73	−0.12
	(0.043)	(0.037)	(0.057)	(0.0078)	(0.0064)	(0.010)
Aged 12 to 17 in 1974	8.02	9.40	−1.39	6.87	7.02	−0.15
	(0.053)	(0.042)	(0.067)	(0.0085)	(0.0069)	(0.011)
Difference	0.47	0.36	0.12	−0.26	−0.29	0.026
	(0.070)	(0.038)	(0.089)	(0.011)	(0.0096)	(0.015)
Panel II.B: Control Experiment						

Table B.1 – *Continued on next page*

Table B.1 – *Continued from previous page*

	High	Low	Difference	High	Low	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Aged 12 to 17 in 1974	8.02	9.40	−1.39	6.87	7.02	−0.15
	(0.053)	(0.042)	(0.067)	(0.0085)	(0.0069)	(0.011)
Aged 18 to 24 in 1974	7.70	9.12	−1.42	6.92	7.08	−0.16
	(0.059)	(0.044)	(0.072)	(0.0097)	(0.0076)	(0.012)
Difference	0.32	0.28	0.034	0.056	0.063	0.0070
	(0.080)	(0.061)	(0.098)	(0.013)	(0.010)	(0.016)

Notes: The sample is made of male and have valid wage data. Standard errors are in parentheses.

Bibliography

Agarwal, S. and Mazumder, B. (2013), 'Cognitive abilities and household financial decision making', *American Economic Journal: Applied Economics* **5**(1), 193–207.

Akresh, R. and de Walque, D. (2008), Armed Conflict and Schooling: Evidence from the 1994 Rwandan Genocide, HiCN Working Papers 47, Households in Conflict Network.

Alderman, H., Hoddinott, J. and Kinsey, B. (2006), 'Long term consequences of early childhood malnutrition', *Oxford Economic Papers* **58**(3), 450–474.

Almond, D. (2006), 'Is the 1918 influenza pandemic over? long term effects of in utero influenza exposure in the post?1940 u.s. population', *Journal of Political Economy* **114**(4), pp. 672–712.

Almond, D., Chay, K. Y. and Lee, D. S. (2005), 'The costs of low birth weight', *The Quarterly Journal of Economics* **120**(3), 1031–1083.

Altonji, J. G., Elder, T. E. and Taber, C. R. (2005), 'Selection on observed and unobserved variables: Assessing the effectiveness of catholic schools', *Journal of Political Economy* **113**(1), 151–184.

Andreoni, J. and Sprenger, C. (2011), Uncertainty equivalents: Testing the limits of the independence axiom, Working Paper 17342, National Bureau of Economic Research.

Arnett, J. (1992), 'Reckless behavior in adolescence: A developmental perspective', *Developmental Review* pp. 339–373.

Badan Pusat Statistik (2006), *Sensus Ekonomi*, Badan Pusat Statistik.

Bank Indonesia (2005), Results of study of micro, small and medium businesses profil, Technical report.

Beauchamp, J., Cesarini, D. and Johannesson, M. (2015), 'The psychometric and empirical properties of measures of risk preferences', *SSRN No. 2614973* .

Beckers, J. and Lay, T. (1995), 'Very broadband seismic analysis of the 1992 flores, indonesia, earthquake (mw = 7.9)', *Journal of Geophysical Research: Solid Earth* **100**(B9), 18179–18193.

Bellows, J. and Miguel, E. (2009), 'War and local collective action in sierra leone', *Journal of Public Economics* **93**(11-12), 1144 – 1157.

Benjamin, D. J., Brown, S. A. and Shapiro, J. M. (2013), 'Who is behavioral? cognitive ability and anomalous preferences', *Journal of the European Economic Association* **11**(6), 1231–1255.

Binswanger, H. (1980), 'Attitudes toward risk: Experimental measurement in rural india', *American Journal of Agricultural Economics* **62**, 395–407.

Black, S. E., Devereux, P. J. and Salvanes, K. G. (2007), 'From the cradle to the labor market? the effect of birth weight on adult outcomes', *The Quarterly Journal of Economics* **122**(1), 409–439.

Bound, J., Jaeger, D. A. and Baker, R. M. (1995), 'Problems with instrumental variables estimation when the correlation between the instruments and the endogeneous explanatory variable is weak', *Journal of the American Statistical Association* **90**(430), 443–450.

Burks, S. V., Carpenter, J. P., Goette, L. and Rustichini, A. (2009), 'Cognitive skills affect economic preferences, strategic behavior, and job attachment', *Proceedings of the National Academy of Sciences* **106**(19), 7745–7750.

Callen, M., Isaqzadeh, M., Long, J. D. and Sprenger, C. (2014), 'Violence and risk preference: Experimental evidence from afghanistan', *American Economic Review* **104**(1), 123–48.

Cameron, L. and Shah, M. (2015), 'Risk-taking behavior in the wake of natural disasters', *Journal of Human Resources* **50**(2), 484–515.

Cardenas, J. C. and Carpenter, J. (2013), 'Risk attitudes and economic well-being in latin america', *Journal of Development Economics* **103**, 52 – 61.

Cassar, A., Healy, A. and Von Kessler, C. (2011), 'Trust, risk, and time preferences after a natural disaster: experimental evidence from thailand', *Unpublished manuscript* .

Cattell, R. B. (1963), 'Theory of fluid and crystallized intelligence: A critical experiment', *Journal of educational psychology* **54**(1), 1–22.

Conti, G., Hansman, C., Heckman, J. J., Novak, M. F. X., Ruggiero, A. and Suomi, S. J. (2012), 'Primate evidence on the late health effects of early-life adversity', *Proceedings of the National Academy of Sciences* **109**(23), 8866–8871.

Corazzini, L., Filippin, A. and Vanin, P. (2014), Economic Behavior under Alcohol Influence: An Experiment on Time, Risk, and Social Preferences, IZA Discussion Papers 8170, Institute for the Study of Labor (IZA).

Currie, J. and Almond, D. (2011), Chapter 15 - human capital development before age five, Vol. 4, Part B of *Handbook of Labor Economics*, Elsevier, pp. 1315 – 1486.

Currie, J. and Vogl, T. (2013), 'Early-Life Health and Adult Circumstance in Developing Countries', *Annual Review of Economics* **5**(1), 1–36.

Davis, L. (2008), *Natural Disasters*, Facts On File, Inc.

de Mel, S., McKenzie, D. and Woodruff, C. (2008), 'Returns to capital in microenterprises: Evidence from a field experiment', *The Quarterly Journal of Economics* **123**(4), 1329–1372.

Deck, C. and Jahedi, S. (2015), 'The effect of cognitive load on economic decision making: A survey and new experiments', *European Economic Review* **78**, 97 – 119.

Dohmen, T., Falk, A., Huffman, D. and Sunde, U. (2010), 'Are risk aversion and impatience related to cognitive ability?', *American Economic Review* **100**(3), 1238–60.

Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J. and Wagner, G. G. (2011), 'Individual Risk Attitudes: Measurement, Determinants, And Behavioral Consequences', *Journal of the European Economic Association* **9**(3), 522–550.

Duflo, E. (2001), 'Schooling and labor market consequences of school construction in indonesia: Evidence from an unusual policy experiment', *American Economic Review* **91**(4), 795–813.

Eckel, C. C., El-Gamal, M. A. and Wilson, R. K. (2009), 'Risk loving after the storm: A Bayesian-Network study of Hurricane Katrina evacuees', *Journal of Economic Behavior & Organization* **69**(2), 110–124.

Eckel, C. C. and Grossman, P. J. (2002), 'Sex differences and statistical stereotyping in attitudes toward financial risk', *Evolution and Human Behavior* **23**(4), 281 – 295.

Frankenberg, E., Sikoki, B., Sumantri, C., Suriastini, W. and Thomas, D. (2013), 'Education, vulnerability, and resilience after a natural disasters', *Ecology and Society* **2**.

Frankenberg, E., Smith, J. P. and Thomas, D. (2003), 'Economic Shocks, Wealth, and Welfare', *Journal of Human Resources* **38**(2).

Frederick, S. (2005), 'Cognitive reflection and decision making', *Journal of Economic Perspectives* **19**(4), 25–42.

Gerardi, K., Goette, L. and Meier, S. (2013), 'Numerical ability predicts mortgage default', *Proceedings of the National Academy of Sciences of the United States of America* **110**(28), 11267–11271.

Grinblatt, M., Keloharju, M. and Linnainmaa, J. (2011), 'Iq and stock market participation', *The Journal of Finance* **66**(6), 2121–2164.

Halek, M. and Eisenhauer, J. G. (2001), 'Demography of risk aversion', *The Journal of Risk and Insurance* **68**(1), 1–24.

Hamoudi, A. (2006), Risk preferences in household and families, Technical report, UCLA.

Haushofer, J. and Fehr, E. (2014), 'On the psychology of poverty', *Science* **344**(6186), 862–867.

Holt, C. A. and Laury, S. K. (2002), 'Risk aversion and incentive effects', *The American Economic Review* **92**(5), 1644–1655.

Hryshko, D., Luengo-Prado, M. J. and SÃÿrensen, B. E. (2011), 'Childhood determinants of risk aversion: The long shadow of compulsory education', *Quantitative Economics* **2**(1), 37–72.

Ingwersen, N. (2014), Impact of a natural disaster on observed risk aversion. Job Market Paper.

Kang, M. J., Rangel, A., Camus, M. and Camerer, C. F. (2012), 'Hypothetical and real choice differentially activate common valuation areas', *The Journal of Neuroscience* **31**(2), 461–468.

Kim, Y.-I. and Lee, J. (2014), 'The long-run impact of a traumatic experience on risk aversion', *Journal of Economic Behavior & Organization* **108**(0), 174 – 186.

Krauss, S. I., Frese, M., Friedrich, C. and Unger, J. M. (2005), 'Entrepreneurial orientation: A psychological model of success among southern african small business owners', *European Journal of Work and Organizational Psychology* **14**(3), 315–344.

Kremer, M., Robinson, J., Rostasphova, O. and Lee, J. (2014), Rates of return, optimization failures, and loss aversion: Evidence from kenyan retail shops.

LaFave, D. and Thomas, D. (2014), Extended families and child well-being, Working Paper 20702, National Bureau of Economic Research.

Lammers, J., Willebrands, D. and Hartog, J. (2010), Risk attitude and profits among small enterprises in nigeria, Technical report, Tinbergen Institute.

Li, J.-Z., Li, S., Wang, W.-Z., Rao, L.-L. and Liu, H. (2011), 'Are people always more risk averse after disasters? surveys after a heavy snow-hit and a major earthquake in china in 2008', *Applied Cognitive Psychology* **25**(1), 104–111.

Maccini, S. and Yang, D. (2009), 'Under the weather: Health, schooling, and economic consequences of early-life rainfall', *American Economic Review* **99**(3), 1006–26.

Malmendier, U. and Nagel, S. (2011), 'Depression babies: Do macroeconomic experiences affect risk taking?', *The Quarterly Journal of Economics* **126**(1), 373–416.

Mata, R., Josef, A. K., Samanez-Larkin, G. R. and Hertwig, R. (2011), 'Age differences in risky choice: a meta-analysis', *Annals of the New York Academy of S* **1235**, 18–29.

McArdle, J. J., Ferrer-Caja, E., Hamagami, F. and Woodcock, R. W. (2002), 'Comparative longitudinal structural analyses of the growth and decline of multiple intellectual abilities over the life span', *Developmental psychology* **38**(1), 115–142.

McKenna, B. S., Dickinson, D. L., Orff, H. J. and Drummond, S. P. A. (2007), 'The effects of one night of sleep deprivation on known-risk and ambiguous-risk decisions', *Journal of Sleep Research* **16**(3), 245–252.

Meng, X. and Qian, N. (2009), The long term consequences of famine on survivors: Evidence from a unique natural experiment using china's great famine, Working Paper 14917, National Bureau of Economic Research.

Nichter, S. and Goldmark, L. (2009), 'Small firm growth in developing countries', *World Development* **37**(9), 1453 – 1464.

Oechssler, J., Roider, A. and Schmitz, P. W. (2009), 'Cognitive abilities and behavioral biases', *Journal of Economic Behavior & Organization* **72**(1), 147 – 152.

Prawiro, R. (1998), *Indonesia's Struggle for Economic Development: Pragmatism in Action*, Oxford University Press.

Rabin, M. (2000), 'Risk Aversion and Expected-Utility Theory: A Calibration Theorem', *Econometrica* **68**(5), 1281–1292.

Reynaud, A. and Couture, S. (2010), Stability of risk preferences measures: Results from a field experiment on french farmers, Technical report, TSE Working Paper.

Rosenzweig, M. R. and Stark, O. (1989), 'Consumption smoothing, migration, and marriage: Evidence from rural india', *Journal of Political Economy* **97**(4), pp. 905–926.

Rosenzweig, M. R. and Wolpin, K. I. (1993), 'Credit market constraints, consumption smoothing, and the accumulation of durable production assets in low-income countries: Investments in bullocks in india', *Journal of Political Economy* **101**(2), 223–244.

Sacco, K., Galletto, V. and Blanzieri, E. (2003), 'How has the 9/11 terrorist attack influenced decision making?', *Applied Cognitive Psychology* **17**(9), 1113–1127.

Schultz, T. P. (1993), Demand for children in low income countries, *in* M. R. Rosenzweig and O. Stark, eds, 'Handbook of Population and Family Economics', Vol. 1 of *Handbook of Population and Family Economics*, Elsevier, chapter 8, pp. 349–430.

Schurer, S. (2015), 'Lifecycle patterns in the socioeconomic gradient of risk preferences', *IZA Discussion Paper* (8821).

Shaw, K. L. (1996), 'An empirical analysis of risk aversion and income growth', *Journal of Labor Economics* **14**(4), 626–653.

Strauss, J., Beegle, K., Dwiyanto, A., Herawati, Y., Pattinasarany, D., Satriawan, E., Sikoki, B., Sukamdi and Witoelar, F. (2004), *Indonesian Living Standards: Before and After the Financial Crisis*, Rand Corporation.

Strauss, J. and Thomas, D. (1995), Human resources: Empirical modeling of household and family decisions, *in* H. Chenery and T. Srinivasan, eds, 'Handbook of Development Economics', Vol. 3 of *Handbook of Development Economics*, Elsevier, chapter 34, pp. 1883–2023.

Strauss, J., Witoelar, F., Sikoki, B. and Wattie, A. M. (2009), The fourth wave of the indonesian family life survey (ifls4): Overview and field report, Technical report. WR-675/1-NIA/NICHD.

Tanaka, T., Camerer, C. F. and Nguyen, Q. (2010), 'Risk and time preferences: Linking experimental and household survey data from vietnam', *American Economic Review* **100**(1), 557–71.

Tella, R. D., Galiani, S. and Schargrodsky, E. (2007), 'The Formation of Beliefs: Evidence from the Allocation of Land Titles to Squatters', *The Quarterly Journal of Economics* **122**(1), 209–241.

Tsuji, Y., Matsutomi, H., Imamura, F., Takeo, M., Kawata, Y., Matsuyama, M., Takahashi, T., Harjadi, P. et al. (1995), 'Damage to coastal villages due to the 1992 flores island earthquake tsunami', *Pure and Applied Geophysics* **144**(3-4), 481–524.

Umana-Aponte, M. (2011), Long-term effects of a nutritional shock: the 1980 famine of karamoja, uganda, The centre for market and public organisation, Department of Economics, University of Bristol, UK.

UNDP, BPS and Bappenas (2004), *Indonesia Human Development Report 2004*, UNDP Indonesia.

Voors, M. J., Nillesen, E. E. M., Verwimp, P., Bulte, E. H., Lensink, R. and Soest, D. P. V. (2012), 'Violent conflict and behavior: A field experiment in burundi', *The American Economic Review* **102**(2), pp. 941–964.

World Bank (2010), 'Improving access to financial service in indonesia'.

Yeh, H., Imamura, F., Synolakis, C., Tsuji, Y., Liu, P. and Shi, S. (1993), 'The flores island tsunamis', *Eos, Transactions American Geophysical Union* **74**(33), 369–373.