ABSTRACT

Title of Dissertation: HUMAN CAPITAL INVESTMENTS:
                             PREFERENCES, OPPORTUNITIES, CONSTRAINTS

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Education has long been known as "human capital", the capital involved in producing higher wages and augmenting labor productivity. It has also been associated with better nonmarket outcomes, including better health, lower crime and higher social cohesion. The high private and social benefits of education have motivated extensive research on determinants of schooling choices and skills. My dissertation uses data from a developing country, Indonesia, and studies the role of parents in children's education during primary and secondary school in late 1990s, around the period of the East Asian Financial Crisis.

I study two main questions in order to understand the implications of the crisis for children. First, I examine whether only children with low expected returns from education selected out of schooling during the crisis. I find significant negative effects of school dropout soon after the crisis on mathematics test scores, suggesting that the crisis induced some parents to pull children out of school even though they had the potential to do well. This analysis shows the importance of short-run constraints for school enrollments in Indonesia. Second, I explore how parents allocate education resources between their children at the intensive margin. I find that, on average, parental education

spending is not a function of children's test scores. However, parents are more sensitive to the human capital of younger female children and penalize them for having lower scores compared to their older siblings of either gender. Thus, girls appear to be more vulnerable to resource constraints as parents reduce investments and likely provide them with worse quality of education.

Schooling in Indonesia is associated with high labor market returns. Thus, my research shows that much can be gained by insuring children against short-run shocks that have long-run consequences. As an extension, I also examine whether the crisis had short-run labor market effects as well. I test whether males living in urban areas were less willing to take risks when making occupation choices soon after the crisis. The results of this analysis, however, are ambiguous. Better data should allow answering this question more conclusively in the future.

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by

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DEDICATION

To Austin and to my family, for teaching me about courage, kindness and love.

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Contents

CHAPTER 1: Introduction

1. Introduction

Education has long been known as "human capital", the capital involved in producing higher wages and augmenting labor productivity (Becker, 1975; Mincer, 1974). One pathway of influence, emphasized by the early theoretical literature, has been the effect of education on increasing production efficiency (Becker, 1975). Some have also argued that education determines the choice of inputs and thus increases allocative efficiency (Rosenzweig & Schultz, 1982). The standard approach for estimating the relationship between education and earnings has relied on Mincer's (1974) model, where log earnings are represented as a linear function of years of schooling and a quadratic experience term. Across countries, an extra year of schooling is associated with about 10 percent higher wages on average with generally higher returns for low income countries and lower levels of education (Psacharopoulos & Patrinos, 2004). Dealing with issues of endogeneity due to selection into schooling does not cause large changes in the estimates and, in fact, even yields higher rates of returns (Card, 2001). In addition to better labor market outcomes, education has also been associated with better nonmarket outcomes, including better health, lower crime and higher social cohesion (Grossman, 2006; Lochner, 2011; Wolfe & Haveman, 2002). In developing countries, education has also been important because of its spillover effects on future generations lowering fertility and improving child health (Breierova & Duflo, 2004; Chou et al., 2010; Glewwe, 1999; Osili & Long, 2008).

While traditionally the literature has focused on years of education as the measure of human capital, attention is recently being paid not only on the quantity of education but

also the quality of education. Increasing evidence has shown that measures of cognitive ability and achievement are important predictors of economic outcomes independently of schooling both in developed countries (Cawley, Heckman, & Vytlacil, 2001; Heckman, Stixrud, & Urzua, 2006) and the developing world (Glewwe, 2002; Hanushek & Woessmann, 2008). Even at the macro level, test scores have been shown to explain the variation in economic growth between countries much better than simple measures of the quantity of schooling (Hanushek & Woessmann, 2008).

The high private and social benefits of education, measured by years of schooling or test scores, have motivated extensive research on determinants of schooling choices and skills. Formal education may occur in early childhood development programs, primary and secondary school, college, or on-the-job training. Education may also involve various agents – parents, teachers, children, the government. My dissertation uses data from a developing country, Indonesia, and studies the role of parents in children's education during primary and secondary school in late 1990s, around the period of the East Asian Financial Crisis. The second chapter focuses on parental investment at the extensive margin. I investigate learning outcomes of children who drop out of school. I show that while some children select out of school due to low ability, on average, children would have benefitted from school in terms of achieving higher test scores had they stayed in school longer. The third chapter studies how parents choose to allocate education resources between (two) children while their children go to school. I find that, on average, parents have a neutral investment strategy. However, parents seem to be more sensitive to the skills of the younger child if that child is a girl, choosing to then reinforce skill differences between siblings.

Next, I motivate my research questions by discussing three of the key factors that affect parental investment in the education of their children in developing countries. I review the literature on how resource constraints, parental preferences and the return to education affect investments at both the extensive and intensive margins. Then, I provide some background on the Indonesian context and discuss why Indonesia is a good place to study these questions. Finally, I provide details on the data used for the analysis.

2. Determinants of education investment

In the standard human capital model, children and parents are forward-looking and view schooling as an investment with financial returns. For example, in a seminal contribution to the literature, Willis and Rosen (1979) show for the US that the decision of whether to attend college depends on the expected lifetime earnings under the two options (i.e., college or no college). They find evidence of comparative advantage, showing that college graduates would have been worse off if they had not gone to college compared to people who actually did not go to college, and vice versa. Keane and Wolpin (1997) estimate a structural dynamic model of career choices for young men in the US and similarly find that the data supports the investment view of the human capital model: schooling, work and occupation choices are consistent with maximization of lifetime utility (a function of earnings). People may not always be able to make optimal investment choices, however, if they are resource constrained. In addition, parental preferences, sometimes irrational, may determine outcomes, too. And as in any investment, schooling choices are also largely affected by (perceived) returns.

2.1 Constraints

In developing countries, school fees often represent a significant proportion of family income and schooling may also be associated with high opportunity costs of foregone child labor. As a result, large disparities in educational attainment by household socioeconomic status exist (Filmer, 2000). The positive association between education and income, however, is not necessarily causal since poor parents may lack skills or motivation and their children may have inherited those characteristics, which may in turn affect school performance and school choices. In order to deal with this issue, many studies have used exogenous variation caused by income shocks to identify the impact of household income on schooling. Financial market imperfections make it difficult for households to save or borrow and parents are often unable to insure children against the effects of income shocks. Thus, negative macroeconomic shocks have been shown to reduce enrollment and attendance rates (Ferreira & Schady, 2009). Similarly, Jacoby and Skoufias (1997) find that school attendance in rural India fluctuates during periods of idiosyncratic income shocks. In Brazil, Duryea, Lam, and Levison (2007) study the effect of the male household head becoming unemployed and show significant increases in the probability of dropping out of school permanently and entering the labor force for children between the ages of 10 and 16. Björkman-Nyqvist (2013) uses regional rainfall variation to identify the effect of unexpected decreases in income on child schooling in Uganda and shows significant negative impacts on girls' enrollment (and no impacts on boys). Poor households are unable to smooth consumption not only during unexpected income shocks but also during times when an income shock is anticipated. Edmonds (2006) studies the effect of the social pension income in South Africa and shows large increases in school attendance, especially

for boys, when an elderly household member becomes eligible for the pension. In addition, child labor decreases after receipt of pension income and thus the study suggests that both direct costs such as school fees and indirect costs such as foregone wages may affect schooling decisions.

Studies on reduction or elimination of school fees provide further evidence that the direct cost of schooling is an important factor in school participation. Lucas and Mbiti (2012) find that the free primary education program in Kenya improved access to schooling for disadvantaged populations and increased primary school graduation rates. Similarly, Deininger (2003) shows that primary school enrollment in Uganda increased dramatically after elimination of fees. In addition, once school fees are abolished, Björkman-Nyqvist (2013) finds no significant impact of income shocks on enrollment in Uganda. She does, however, find that average test scores of girls fall during periods of resource constraints and she attributes this finding to parents reducing their school investments at the intensive margin or girls having to work and thus spend less time studying.

The impact of conditional cash transfer (CCT) programs on accumulation of human capital further demonstrates the importance of liquidity constraints (Fiszbein & Schady, 2009). Evidence from around the world, from Mexico (Parker, Rubalcava, & Teruel, 2008) to Malawi (Baird, McIntosh, & Ozler, 2011) and Cambodia (Filmer & Schady, 2009) shows increased enrollment rates and years of education for individuals and communities that benefit from cash transfers. Importantly, these programs condition cash transfers on child enrollment and often on child attendance, as well. While unconditional cash transfers have had positive effects on schooling in some cases (Edmonds, 2006), conditioning transfers on school attendance has been found to be more effective, possibly because of

households' competing financial needs (Baird, McIntosh, & Ozler, 2011; de Brauw & Hoddinott, 2011). CCTs have also been effective at insuring children's education against unexpected income shocks (Cameron, 2009; de Janvry et al., 2006).

One caveat of these findings, however, is that the evidence on the success of CCTs in improving children's test scores has been mixed. Several studies have looked at the effect of the extra education on home-administered test scores (thus avoiding the nonrandom selection into schooling). In Nicaragua, Barham, Macours, and Maluccio (2013) find that boys between the ages of 9 and 12 who lived in communities that benefitted from CCTs stayed in school longer and had higher mathematics and language test scores ten years after the conclusion of the program compared to a control group. Baird, McIntosh, and Ozler (2011) reach similar conclusions for the effects of a CCT program targeted at girls aged 13 to 22 in Malawi, two years after the start of the program. In addition, unlike the Barham, Macours, and Maluccio (2013) study, they also find significant gains in cognition (measured by Raven's Colored Progressive Matrices assessment). On the other hand, Filmer and Schady (2009) study a scholarship program in Cambodia allocated to children in lower secondary school according to an individual dropout risk score. Using a regression discontinuity design, they find no effects of the extra schooling on scores of mathematics and vocabulary tests 18 months after the program start. The scholarship recipients did not do better on the tests even when they attended schools of higher quality. In Mexico, Behrman, Parker, and Todd (2009) use differential exposure to the popular CCT program Progresa/Oportunidades to study its impact on schooling. While children who started receiving the transfers earlier had more years of education compared to those who became beneficiaries later, no significant differences in language and mathematics test

scores between the two groups existed. These studies suggests that if the goal of the program is to improve children's skills, then special school interventions that focus on low-performing students may be needed or investments may need to be aimed at younger children, who are more likely to benefit from them. The lack of effect on test scores may be part of the reason why Progresa (the longest-running CCT program) is found to have no impact on employment, wages or intergenerational occupational mobility despite the extra schooling its beneficiaries received (Rodríguez-Oreggia & Freije, 2012).

2.2 Parental preferences

If parents prefer to have more children, then may have to sacrifice the "quality" of those children when resource-constrained. In a seminal contribution to the literature on household economic behavior, Becker and Tomes (1976) and Becker and Lewis (1973) model household utility as a function of the number of children and their quality. Since quality is increased by investments and households are budget constrained, poor parents may be expected to trade off quantity vs. quality of children. While the empirical evidence has been ambiguous and context-dependent, a few studies provide support for the quantity-quality tradeoff in developing countries. Joshi and Schultz (2007) study a randomized family-planning program in Bangladesh that took place between 1974 and 1996. They find that women in villages that benefitted from the door-to-door outreach program and contraceptive services had fewer children and their children obtained more education. Other studies use twin births as a way to model exogenous fertility changes and study the causal relationship between household size and investment in children. For example, Rosenzweig and Wolpin (1980) show that having twins reduces the average educational attainment of children in Indian households, while Rosenzweig and Zhang (2009) find

negative effects on years of schooling, college enrollment and mathematics and language test scores of Chinese twins.

Becker and Tomes (1976) and Behrman, Pollak, and Taubman (1982) were the first to model the important interactions within households comprised of heterogeneous individuals. In the wealth model developed by Becker and Tomes (1976), parents choose whether to invest in children so as to increase their adult earnings potential or whether to provide them transfers when they are adults to compensate for their low earnings. The model assumes that parents are concerned with total child wealth rather than the sources of wealth. The main conclusion is that parents reinforce endowment differences in children by providing human capital investment for the more endowed child, but equalize wealth by providing more transfers to the less endowed child. An implicit assumption of the wealth model is that parents have enough resources to allocate between children (Behrman, Pollak, & Taubman, 1995). An alternative model was proposed by Behrman, Pollak, and Taubman (1982) who argued that parental preferences are separable in earnings and transfers (SET). In this setting, parents solve a two-stage problem where they allocate total resources between earnings and transfers in the first stage and then, in the second stage, they allocate earnings investments (and transfers) among their children. This assumption allows for analyzing the distribution of human capital investments independent of any possible future transfers, and was widely adopted in the subsequent literature. Behrman, Pollak, and Taubman (1982) show that whether parents follow a compensatory, reinforcing, or a neutral investment strategy depends on parental inequality aversion and on the properties of the child earnings function.

Studying education in Burkina Faso, Akresh et al. (2012) confirm that educational investments reinforce differences between siblings. In particular, they find that having a higher-ability sibling lowers current enrollment by 15 percent and having two higher ability siblings lowers enrollment by 30 percent. Research on Ethiopia also supports the finding that parents invest in children of higher cognitive ability (Ayalew, 2005). In addition, recent work suggests that parents may also consider the multidimensional nature of human capital when making investments, although the empirical evidence is mixed. For example, Conti et al. (2011) use data on twins in China and show that parents provide fewer educational resources to children who experienced an early life health shock, measured restrospectively. On the other hand, Leight (2014) finds that Chinese parents allocate more of the discretionary educational spending to the child with lower health endowment, measured by height-for-age and instrumented by early life climatic shock to nutrition.

In addition to child ability and health, education investments in developing countries have been shown to vary greatly depending on child gender and birth order. For example, Parish and Willis (1993) show that in Taiwan having an older sister is associated with higher educational attainment, especially for older cohorts. Younger children in the Philippines have better educational outcomes compared to their older siblings (Ejrnaes & Portner, 2004). In Indonesia, on the other hand, Pradhan (1998) finds that having younger siblings decreases the probability of delayed enrolment and thus the first-born in the family receive better education. In South and East Asia, as well as in other regions of the world, there is also a strong preferential allocation of resources to males. Gender preferences are manifested in higher mortality and worse health outcomes of girls as well as lower educational investments in girls (Filmer, Friedman, & Schady, 2008). Significant

differences in school enrollment between girls and boys exist in many countries both at the primary and secondary schooling level (Alderman & King, 1998). While gender preferences may often be due to cultural norms, some of the education gap could also be explained by the (perceived) difference in returns to education by gender.

2.3 Returns

Opportunity costs and returns to education are two important factors that affect investment in children's education in developing countries. For example, expansion of manufacturing and higher demand for low-skill labor in Mexico was associated with rises in school dropout rates as the opportunity costs of education rose (Atkin, 2012). Further, Mexican parents in areas with higher demand for low-skilled labor reduced the education investments in their children at the extensive margin as well, spending less time helping children with school and spending less money on school supplies (Majlesi, 2014). Conversely, Oster and Steinberg (2013) use Indian data to show that the creation of a new IT center increases primary school enrollment rates by 5%, as returns to English-language education rise. A recruitment intervention in India, which made job opportunities for women more salient and accessible, increased enrollment in school for younger girls and enrollment in training programs for older girls (Jensen, 2012). Similarly, an increase in garment sector jobs in Bangladesh, targeted at girls, was found to be associated with higher probability that young girls are enrolled in school (Heath & Mobarak, 2011). Jensen (2010) finds that students in the Dominican Republic perceive the labor market returns to secondary school to be very low, despite the high measured returns. Students who were provided with information about the actual returns stayed in school an average of 0.20-0.35 years longer than a control group without the information intervention. Nguyen (2008)

finds similar magnitudes for the effect of provision of labor market return information in Madagaskar.

Even with high labor market returns, education spending may also depend on the time horizon for reaping benefits from these investments. Jayachandran and Lleras-Muney (2009) use data from Sri Lanka to show that a fall in the maternal mortality increases female life expectancy and increases girl literacy and years of education attained. Similarly, Fortson (2011) uses data on geographic and time variation from 15 countries in sub-Saharan Africa to show negative relationship between HIV prevalence and educational attainment. Despite the different possible pathways of influence, the results suggest that the link is likely due to a longevity-related decline in the expected returns to schooling. A recent paper in Malawi shows that availability of AIDS treatment resulted in increased spending on children's human capital (Baranov & Kohler, 2014). Baranov and Kohler (2014) argue that the mechanism of action is through changing perceptions about longevity.

Education investments may also depend on the heterogeneity in the return to years of education. If parents recognize the heterogeneity in labor market returns, they may be less likely to invest in poor-quality education. For example, Lloyd et al. (2003) show that Egyptian girls, in particular, are more likely to drop out when schools have lower quality measures such as multiple shifts, poor physical facilities, more temporary teachers. While choice of school is potentially endogenous, Lloyd et al. (2003) rely on the fact that school choice in Egypt is limited and residential migration is low. Similarly, Hanushek, Lavy, and Hitomi (2008) study dropout behavior in Egypt and show that high-achieving students are more likely to stay in school than low-achieving students. Conditional on their skills, however, students attending schools of lower quality are more likely to drop out of school

and complete fewer grades. Another educational outcome that may be affected by school quality is enrollment. Case and Deaton (1999) use data from South Africa during the apartheid when Black families had limited residential mobility and school resources were controlled centrally. They show that pupil-teacher ratios had significant effects on achievement as well as school enrollment. Studying determinants of enrollment age in Tanzania, Bommier and Lambert (2000) also find that quality influences household educational investments. In particular, they show that higher quality of mathematics teaching is associated with younger enrollment age and higher quality of Swahili teaching is associated with longer school duration. While school quality measures that serve as direct inputs in children's human capital production function could be expected to matter, even physical quality of schools are found to be important determinants of schooling choices. For example, Paxson and Schady (2002) study an increase in government spending on school facilities in Peru and find a plausibly causal relationship between the physical quality of schools and school attendance of children between the ages of 6 and 11. The lack of data on any accompanying improvements in pupil-teacher ratios or time spent in school, however, may mask one channel through which physical infrastructure affects child achievement and future labor market returns.

3. The Indonesian context

3.1 Overview

Indonesia is the fourth most populous country in the world with 250 million people, spread on thousands of islands. A lower-middle-income country, it has GDP per capita of $3,500 in current USD, less than 7% of US GDP per capita. For a period of 20 years starting in the 1970s, Indonesia experienced a rapid economic growth, following a change

in the political leadership and liberalization of the economy. Using foreign aid and oil revenues, the government invested heavily in infrastructure and social programs. The proportion of the population living under poverty fell from 40.1% in 1976 to 11.3% in 1996 (Lanjouw et al., 2001). Between 1973 and 1979, the government constructed more than 61,000 primary schools, raising enrollment rates of primary school students from 69% to 83% (Duflo, 2001). It also created the National Family Planning Coordinating Board which promoted small families and, in particular, a two-child norm. Volunteer and village mid-wife services were used to promote and distribute different contraceptives, and those were made available free of charge during the 70s and 80s (Frankenberg, Sikoki, & Suriastini, 2003). As a result, total fertility rates decreased from 5.6 children per woman in the late 1960s to 3.4 in 1984-1987, and 2.8 in 1995-1997 (Permana & Westoff, 1999).

Unlike many Asian countries, Indonesia shows no male gender bias in parental preferences in birth outcomes or nutrition, and the gender gap in educational attainment has been declining (Kevane & Levine, 2000). In 2012, school enrollment rates of girls and boys were similar: 92.8% of primary school-aged girls and 91.7% of primary school-aged boys were enrolled in primary school; overall enrollment in secondary school was lower but again a larger proportion of secondary school-aged girls (77.5%) than boys (74.8%) were enrolled in secondary school (The World Bank). At the same time, girl education may be a luxury good. For example, Cameron and Worswick (2001) study household capacity to smooth consumption after crop loss and find that when hit by an income shock, households with girls of school age (and not those with boys of school age) reduce education expenditures. Similarly, Thomas et al. (2004) show that during the economic crisis of late 1990s households with more boys experienced lower reductions in the

education budget shares compared to those with more girls. Thomas et al. (2004) also find that households tended to protect the education of the older children at the expense of the younger ones. This is consistent with Pradhan (1998) who finds that having younger siblings decreases the probability of delayed enrolment and thus the first-born in the family receive better education.

The educational system in Indonesia is characterized by three schooling levels – primary school for ages 7 to 13, then three years each for junior high school and senior high school. In 1994, mandatory school going age was increased to 15 years. Yet, while primary school enrollment is almost universal, junior high school enrollment in 1997 was only 72.2%, while senior high school enrollment was less than 50% (Lanjouw et al., 2001). Repetition, especially during primary school, is fairly common with 14.2% of students in grade one and 4.5% of those in grade five of primary school repeating the grade in 1993 (Jones & Hagul, 2001). After each level, students take state exams and their performance determines placement in higher-level schools. School attendance usually requires an annual registration fee, as well as monthly fees. Even in public schools, where annual registration fees have been abolished, parents are expected to pay monthly fees (Suryadarma et al., 2006). In public primary schools, Suryadarma et al. (2006) find a positive relationship between fees and school performance, which they attribute to the use of the money for better school inputs.

Education in Indonesia has been traditionally associated with a high return in the labor market. Earlier studies estimated Mincerian models of log wages as a function of years of schooling using household fixed effects to account for any household-level and community-level unobservable characteristics that affect both schooling choices and wages

(Behrman & Deolalikar, 1993, 1995). Yet, these models ignored individual-level heterogeneity and differences in ability between siblings. A different approach to estimate the causal effect of education on earnings is the use of an instrumental variables model. Duflo (2001) uses the rapid construction of primary schools in the 1970s as a natural experiment that provides exogenous variation in access to schooling by region of birth and date of birth. She finds that an additional school per 1,000 children led to an average of 0.12 to 0.19 additional years of education. In 1995, each additional year of education caused 6.8% to 10.6% higher wages. More recently, Carneiro et al. (2011) examine return to secondary education in Indonesia. They use distance from the community of residence to the nearest secondary school as an instrument for years of education, which they plausibly argue is not correlated with individual ability or motivation in the Indonesian context. They find that in the year 2000, returns to upper secondary schooling are even higher than those estimated by Duflo (2001): a year of secondary schooling yields labor market returns of 27% for the average student.

3.2 The East Asian Financial Crisis

Thailand's financial crisis and currency devaluation in the summer of 1997 rapidly spread to the countries of the region. While Indonesia's currency was volatile in 1997, most Indonesians were only affected in the beginning of 1998 when the rupiah sharply declined after a government budget announcement. In 1998, inflation reached 80% but prices of basic goods increased between 100% and 400%, while real wages fell by 40% (Setiawan, 2000). The timing and magnitude of the economic shock were largely unexpected and households struggled to smooth consumption. Faced with liquidity constraints, households

sold assets (such as gold) and decreased nonfood consumption, including spending on health and education (Frankenberg, Smith, & Thomas, 2003).

The effects of the economic shock were heterogeneous. There was substantial geographic variation. For example, the increase in the price of rice varied between 110% in South Sumatra to 280% in South and Central Kalimantan (Levinsohn, Berry, & Friedman, 1999). Certain sectors of the economy were also affected more than others: while mining and service sectors contracted by 4.5%, the construction and finance sectors contracted by 40% and 27% respectively. In addition, urban unemployment rose but employment in rural areas increased as people (women, in particular) increased labor supply (especially in agriculture) (Setiawan, 2000). School dropout rates in 1998 increased for all income quartiles and for children in both urban and rural areas (Frankenberg, Thomas, & Beegle, 1999). Children in the lowest income quartile, however, seem to have been affected more than those in the other quartiles. Similarly, young children (7 to 12) in rural areas experienced higher increases in dropout rates than young children in urban areas, while older children in urban areas saw higher dropout rates than their counterparts in rural areas.

In July 1998, the government launched several social safety net programs, known as JPS. Those included provision of subsidized rice, creation of public works projects and school scholarships. The scholarship money was in the form of monthly cash transfers, conditional on school attendance. The goal of the program was to reach about 6% of primary school students, 17% of junior high school students, and 10% of senior high school students. Another goal of the program was to allocate at least half of the scholarships to girls. The scholarships were allocated in a decentralized manner to the poorest households

in the poorest districts (Pritchett, 2002; Sparrow, 2007). While targeting of the program was imperfect, the JPS scholarships were found to be effective in mitigating the effects of the economic crisis and the associated income shock, reducing school dropouts (Cameron, 2009; Sparrow, 2007). Yet, the coverage of the program was limited and its rollout delayed.[1]

3.3 Why Indonesia?

Indonesia in the late 1990s is a good setting to study questions of school dropout and resource allocation between children for several reasons. First, Indonesia has very high school enrollment rates and as progress is made on increasing the quantity of schooling, attention is being shifted to improving its quality of education. Teacher absenteeism rates in primary school have been estimated at 19% (Chaudhury et al., 2006). Absenteeism has been linked to low test scores in Indonesia (Suryadarma et al., 2006) as well as other countries (Das et al., 2007). In addition, Indonesian children consistently underperform in international tests such as PISA and TIMSS (Pradhan et al., 2011). Thus, examining whether school dropout is associated with a loss in potential skill accumulation, accounting for selection out of schooling, is an important question, relevant to this context.

Second, there are few gender differences in educational outcomes in Indonesia. At the same time, as discussed above, research by Cameron and Worswick (2001) and Thomas et al. (2004) has shown that boy education may be prioritized over girl education when households are resource constrained. These studies considered total investment, not distinguishing between gender bias in school enrollment and gender bias in school

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[1] A very small proportion of the households interviewed in the Indonesian Family Life Survey data used for the analysis in this dissertation had received JPS scholarship money for one or more of their children before the year 2000. All results of the analysis were found to be robust to controlling for JPS status or excluding JPS households from the sample.

spending. In my study on resource allocation, I only study investments at the intensive margin for children who continue to go to school.

Third, while households in many developing countries may face a tradeoff between the quantity of children and their quality, the extensive family planning programs in Indonesia make it a good setting to study investments in children, independently of household fertility decisions.

Fourth, the Indonesian Family Life Survey (IFLS) dataset used for analysis is one of the very few datasets in developing countries that include cognitive assessments for all children in the household. This allows me to study the effect of school dropout on test scores in chapter 2 and then, in chapter 3, to directly incorporate children's test scores in the parental investment decision problem and show whether parents respond to differences in children's skills.

Finally, two of the waves of this longitudinal dataset, IFLS2 from 1997 and IFLS3 from 2000, span the period of the East Asian Financial crisis – a period of large social and economic turmoil, accompanied by important changes in school enrollments and investments. Studying schooling decisions during a period of resource constraints is important as it provides insight on household coping mechanisms. It also uncovers the vulnerable populations that may need more protection from a short-run income shock that may have long-run negative consequences.

4. Data

This dissertation uses data from the Indonesian Family Life Survey (IFLS): a longitudinal dataset which tracks individuals and households over time. The IFLS was first administered in 1993. The survey sampled households from 13 of Indonesia's 27 provinces

containing 83% of the population: four provinces on Sumatra (North Sumatra, West Sumatra, South Sumatra, and Lampung), all five of the Javanese provinces (DKI Jakarta, West Java, Central Java, DI Yogyakarta, and East Java), and four provinces covering the remaining major island groups (Bali, West Nusa Tenggara, South Kalimantan, and South Sulawesi). The second wave of the survey, IFLS2, took place between July and November 1997, while the third wave, IFLS3, took place between June and December 2000. A total of 6,564 households were interviewed in all three waves of the survey – 90.9% of the original IFLS1 households. In addition, individuals who moved out of their IFLS households were followed with a special attention being paid to children and young adults (Strauss et al., 2004). Thus, while there is attrition, as in any longitudinal survey, the re-contact rate at both the household and individual level is relatively high, ensuring the high quality of the data and minimal attrition bias.

In my analysis, I use information only from IFLS2 and IFLS3 for several reasons. First, both waves collect information on all children in the household unlike IFLS1, where only selected children of the household were interviewed. Second, unlike IFLS1, both IFLS2 and IFLS3 include cognitive assessments for all individuals between the ages of 7 and 24. The survey in 1997 included questions on Indonesian language and mathematics. Children were categorized in four age groups (7 to 9, 10 to 12, 13 to 15, and 16 to 24) and received a test depending on their group. The assessment in 2000 included a general test of cognition (8 or 12 questions on Raven's Colored Progressive Matrices) and a mathematics test (5 questions). Different tests were administered for children under the age of 15 and individuals aged 15 and above. The data from these two survey waves also provide child-specific information on education attainment and parental education spending for the

academic years 1997/1998 and 1999/2000, including annual registration fees, monthly fees, and other expenses for food, travel and extracurricular activities. Third, the 1997 and 2000 data span the period of the East Asian Financial crisis, allowing for the study of changes in enrollments and investments during a period of resource constraints. Finally, as mentioned earlier, the attrition rate between the two survey waves is small. While a fourth wave in 2007 was administered with the same cognitive assessments as in 2000, close to 30% of individuals aged between 15 and 25 in the year 2000 were not tracked in 2007. Thus, had I used information from the later wave, the results would have been subject to significant attrition bias.

CHAPTER 2: Learning outcomes of Indonesian children after dropping out of school

1. Introduction

Traditionally, human capital has been measured by years of schooling. As progress is made around the world on reaching universal primary education and increasing quantity of schooling, more attention is being given to the quality of education and the skills obtained in schools. The evidence on whether more schooling is associated with more learning, however, is inconclusive. For example, Barham, Macours, and Maluccio (2013) find that boys between the ages of 9 and 12 who lived in communities that benefitted from conditional cash transfers (CCTs) in Nicaragua, stayed in school longer and had higher mathematics and language test scores ten years after the conclusion of the program compared to a control group. Baird, McIntosh, and Ozler (2011) reach similar conclusions for the effects of a CCT program targeted at girls aged 13 to 22 in Malawi, two years after the start of the program. In addition, unlike the Barham, Macours, and Maluccio (2013) study, they also find significant gains in cognition (measured by Raven's Colored Progressive Matrices assessment). Importantly, both of these programs were implemented at the community level and the treatment effects being estimated are averages across the affected children.

On the other hand, Filmer and Schady (2009) study a scholarship program in Cambodia allocated to children in lower secondary school according to an individual dropout risk score. Using a regression discontinuity design, they find no effects of extra schooling on scores of similarly home-administered mathematics and vocabulary tests 18 months after the program start. The scholarship recipients did not do better on the tests even when they attended schools of higher quality. Filmer and Schady (2009) attribute their

findings to selection issues as the marginal children who are brought into school are of lower abilities. The authors suggest that special school interventions that focus on low-performing students may be needed if children are to learn more when they stay at school longer.[2]

The studies by Barham, Macours, and Maluccio (2013), Baird, McIntosh, and Ozler (2011) and Filmer and Schady (2009) have all evaluated programs that generally target poorer communities and households and have thus examined the effect of mitigating long-run resource constraints on child schooling and achievement. My analysis provides evidence for how learning is affected by school dropout due to short-run constraints. I use household panel data from the period of the Indonesian economic crisis in late 1990s to estimate the effect of missed schooling on the accumulation of skills learned in school, as well as on general cognition, of girls and boys between the ages of 7 and 15. I identify the effect of dropout on test scores for the population of children who wouldn't have dropped out had the economic crisis not occurred.[3] I find that getting less education because of binding short-run budget constraints is associated with significantly lower mathematics and Raven's scores. The policy implication of these results is that, if Indonesian parents hit by an unexpected income shock had been given cash transfers, they could have afforded to keep their children in school and the additional years of schooling would have allowed their children to obtain valuable skills. However, a government may not be able to distinguish the households who would withdraw their child from school irrespective of

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[2] Alternatively, these findings could offer support for policy interventions at early ages, which in the US have been shown to be more effective in improving learning and reducing dropout (Garces et al., 2002; Heckman & Masterov, 2007).

[3] This effect is conceptually similar to the one identified in Filmer and Schady (2009). They estimate the effect on the marginal children induced to stay in school by the scholarship policy (the decrease in long-run budget constraints). I identify the effect on the marginal children induced to drop out by the crisis (the increase in short-run budget constraints).

income shocks from those who would keep their child in school if not resource constrained. Therefore, I also estimate the policy-relevant effect of treatment on the treated (which is conceptually similar to the effect identified in the CCT studies discussed above). While less schooling is still associated with lower mathematics test scores, the effect on Raven's scores is reduced and loses statistical significance once I control for child work status.

In order to estimate the causal effect of dropping out, the reasons why children stop going to school need to be considered. Factors may include child preferences and ability, school quality and returns to education, as well as resource constraints. In the standard human capital model, children and parents are forward-looking and view schooling as an investment with financial returns. However, Oreopoulos (2007) studies the impact of compulsory schooling laws in the US, Canada and the UK and shows that schooling decisions may not always be governed by investment motives. Rather, students may discount future benefits and non-pecuniary costs such as school distaste may influence their dropout decisions. Eckstein and Wolpin (1999) estimate a structural model of school progression and work choices in the US and find that high-school graduates are inherently different from high-school dropouts in terms of preferences. They also differ in terms of abilities and have comparative advantage at jobs done by nongraduates. Li, Poirier, and Tobias (2004) show that, on average, dropping out of high school in the US has a large negative effect on senior year math scores as students miss in-class learning (Li et al., 2004). However, they find little difference between counterfactual test scores and observed test scores for dropouts, arguing that they would have benefitted little from extra schooling. On the other hand, Cascio and Lewis (2006) and Brinch and Galloway (2012) show that

the increase in education brought about by compulsory schooling laws in the US and Norway was associated with an increase in cognition.

How long children stay in school also depends on the quality of schooling and perceived returns to education. For example, Hanushek, Lavy, and Hitomi (2008) study dropout behavior in Egypt and show that high-achieving students are more likely to stay in school than low-achieving students. Conditional on their skills, however, students attending schools of lower quality are more likely to drop out of school and complete fewer grades. Glewwe and Jacoby (1994) further show that the effect of school quality on child achievement of Ghanaian children is both through improved learning rates and increased school attainment. Case and Deaton (1999) use data from South Africa during the apartheid when Black families had limited residential mobility and school resources were controlled centrally. They show that pupil-teacher ratios had significant effects on school enrollment and achievement. One of the reasons why school quality affects the dropout decision may have to do with the expected returns to schooling. A recruitment intervention in India made job opportunities for women more salient and accessible and increased enrollment in school for younger girls and enrollment in training programs for older girls (Jensen, 2012). Similarly, students in the Dominican republic who were provided with information about the higher than perceived returns to secondary school stayed in school longer (Jensen, 2010).

In developing countries, resource constraints are another important determinant of schooling decisions due to high opportunity costs or households' inability to finance formal education. That is why the school construction program in Indonesia in the 1970s, which decreased distance travelled and thus reduced monetary and time costs of schooling,

increased school attendance (Duflo, 2001). Similarly, abolishing school fees for primary school in Uganda led to dramatic increases in enrollment (Deininger, 2003). Resource constraints are also evident in that parents often fail to insure children against the effects of income shocks. Thus, negative macroeconomic shocks have been shown to reduce enrollment and attendance rates (Ferreira & Schady, 2009). Similarly, Jacoby and Skoufias (1997) find that school attendance in rural India fluctuates during periods of idiosyncratic income shocks because financial market imperfections in rural India prevent households from borrowing against future income. For Brazilian children between the ages of 10 and 16, Duryea, Lam, and Levison (2007) study the effect of the male household head becoming unemployed and show significant increases in the probability of dropping out of school permanently and entering the labor force. While income shocks affect schooling outcomes negatively, their effect on some children may often be disproportionate as parents insure some children better than others. For example, Björkman-Nyqvist (2013) uses regional rainfall variation to identify the effect of decreases in income on child schooling in Uganda, showing negative impacts on girls' enrollment and no impacts on boys.

Given the many factors that affect schooling decisions, I use a variety of empirical approaches in order to account for the possibility of children selecting out of school. If selection is based on pre-treatment heterogeneity where students who drop out differ from those who don't in unobserved factors such as ability or motivation, the ordinary least squares (OLS) estimates of the effect of school interruption on achievement will be biased. Therefore, I use a value-added model, controlling for past mathematics test scores in order to account for any time-constant unobservables that govern both past and present test scores as well as the schooling decision. A more general approach to account for endogeneity in

the dropout decision is to use an instrumental variables model. I take advantage of the fact that the Indonesia Family Life Survey data spans the period before and after the Indonesian economic crisis of late 1990s. I use age and gender-specific differences in predicted and observed non-enrollment rates in the year of the crisis as the source of identifying variation. These serve as exogenous instruments for the individual decision to drop out of school. While the different estimation methods account for different econometric assumptions, they also identify different parameters. The IV model identifies the effect of dropout on test scores for the population of compliers who wouldn't have dropped out had the economic crisis not occurred. From a policy perspective, however, the relevant policy parameter and my preferred estimate is the effect of treatment on the treated - the average effect across all children who dropped out, correcting for selection out of school. It is estimated using an endogenous switching regression model which also allows for treatment effect heterogeneity.

The next section provides some descriptive statistics on the data used for this analysis. In section 3, I present the conceptual framework that governs the empirical specification of section 4. Section 5 contains information on the sample construction and variable definitions, while section 6 discusses the instrument choice. Section 7 presents the findings from the various analyses that quantify the effect of school interruption on learning achievements and cognition. Section 8 includes sensitivity analyses in which I examine the potential effects of changes in school quality at the community level, and test the effectiveness of the government scholarship program instituted during the crisis. Section 9 concludes.

2. Background

Frankenberg, Thomas, and Beegle (1999) present school dropout rates in Indonesia in 1997 and 1998 by age group, urban versus rural area of residence and income quartile, which is included here as figure 1. Figure 1 shows that dropout rates in 1998 increased for all income quartiles and for children in both urban and rural areas. Children in the lowest income quartile, however, seem to have been affected more than those in the other quartiles. Similarly, young children (7 to 12) in rural areas experienced higher increases in dropout rates than young children in urban areas, while older children in urban areas saw higher dropout rates than their counterparts in rural areas. Using the second and third wave of the IFLS data, I present further evidence of the effect of the crisis on school enrollments. Table 1 shows the results from a linear regression of enrollment status on year fixed effects (2000 compared to 1997), age fixed effects and interaction between the year and age dummies. The results suggest that the likelihood of being in school in 2000 is lower than in 1997 at each age level. Figure 2 plots the proportion of children attending school in 1997 and 2000 and shows the same pattern.

If school dropout is to have a negative effect on achievement for a random individual forced to drop out, then more schooling should yield higher test scores. The quality of education in Indonesia, however, has been shown to be very poor. Teacher absenteeism rates in primary school have been estimated at 19%, which is about the average of absenteeism rates in seven developing countries surveyed by Chaudhury et al. (2006). Absenteeism has been linked to low test scores in Indonesia (Suryadarma et al., 2006) as well as other countries (Das et al., 2007). In addition, Indonesian children consistently underperform in international tests such as PISA and TIMSS (Pradhan et al.,

2011). Still, children with more years of education perform better in the IFLS home-administered tests compared to those with fewer years of education. Figure 3 plots the mean number of correct answers in the mathematics and Raven's assessment in 2000 by years of schooling and test group. There is a clear upward trend for both test scores.

The positive correlation between education and test scores is reassuring. The graphs, however, do not imply causality as children may select into schooling. Indeed, this seems to be the case in Indonesia during the study period. For example, figure 4 shows the distribution of mathematics scores in 1997 by future dropout status for children who completed 5 and 8 years of schooling in 1997. The score distribution for those who subsequently drop out is shifted to the left of that of the comparison group (more on the definition of the dropout and comparison groups is given below).[4] Thus, while previous evidence suggests a negative effect on achievement could be expected for a random individual, children who dropped out apparently did not do so randomly. This complicates the estimation of the effect of dropout on learning. The next section presents a simple conceptual framework to illustrate the factors that govern the schooling decision and to provide some insights on the appropriate approach to empirical estimation.

3. The conceptual framework

Suppose parents derive utility from consumption, (C), and the human capital of their child, (H), and parents have a two-period utility function, discounting the second period by a factor δ.[5] In the first period, they choose household consumption and the

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[4] The Kolmogorov-Smirnov test rejects equality of distributions in both cases.
[5] I am assuming a one-child household and ignore any intra-household allocation of resources to simplify discussion of the conceptual model. In the empirical specification, I attempt to account for the potential substitution between the education of different children by controlling for presence of an older sibling in the household, given the strong birth order effects in Indonesia (Pradhan, 1998). In the data, the effect of having a younger sibling is not statistically different from zero.

fraction of time the child spends in school, T, subject to a within-period budget constraint where no saving or borrowing is allowed. While extreme, this assumption serves to illustrate the point that households are budget constrained, and are either myopic or lack access to formal credit markets.[6] Parents pay schooling fees, f, if the child attends school, or receive the child's wage if the child works. Child labor is remunerated based on the average wage of unskilled workers, $\overline{w_u}$. Parental income is denoted by I^p. In the second period, the child is an adult and parents only choose the level of household consumption based on their income level in that period and any transfers their adult child makes to them. Potential transfers are assumed to be a proportion, μ, of the child's adult income, I^c, which is a function of the human capital of the child, other personal characteristics, X, as well as market-level wage determinants, J. Human capital in both periods evolves according to a value-added production function: learning occurs at school as well as at home or at work, represented by an index of other non-school investments, g, and past human capital proxies for missing past inputs, as well as unobserved ability.

$$V = \max_{C_t, T_t, C_{t+1}} U_t(C_t, H_t) + \delta U_{t+1}(C_{t+1}, H_{t+1})$$

$$s.t. \ p_t C_t + f_t T_t = I_t^p + (1 - T_t)\overline{w_u}$$

$$p_{t+1} C_{t+1} = I_{t+1}^p + \mu I_{t+1}^c$$

$$H_t = h(H_{t-1}, T_t, g_t)$$

$$H_{t+1} = h(H_t, g_{t+1})$$

$$I_{t+1}^c = i(H_{t+1}, X, J_{t+1})$$

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[6] While households could potentially have access to informal insurance from friends or neighbors, the aggregate nature of the income shock during the economic crisis would reduce the effectiveness of such insurance mechanisms.

In this setup, parents choose to send their child to school ($T > 0$) if $\partial V/\partial T = 0$. Alternatively, the child drops out of school ($T = 0$) if $\partial V/\partial T < 0$, i.e., if:

$$\frac{\partial U_t}{\partial H_t} \times \frac{\partial H_t}{\partial T_t} + \delta \frac{\partial U_{t+1}}{\partial H_{t+1}} \times \frac{\partial H_{t+1}}{\partial H_t} \times \frac{\partial H_t}{\partial T_t} + \delta \lambda_2 \mu \frac{\partial I^c_{t+1}}{\partial H_{t+1}} \times \frac{\partial H_{t+1}}{\partial H_t} \times \frac{\partial H_t}{\partial T_t} < \lambda_1 \overline{w_u} + \lambda_1 f_t$$

where λ_1 and λ_2 are the Lagrange multipliers or shadow prices associated with the budget constraints in periods 1 and 2, respectively. The expression on the left represents the marginal benefit of schooling. The first two left-hand terms stand for the marginal utility that parents derive in both periods from an increase in the child's human capital, as governed by the child's human capital production function. The last term shows the monetary benefits parents derive in the second period from having invested more in their child's education. The right-hand side represents the marginal cost of schooling in terms of fees and the opportunity cost of schooling in terms of foregone wages. Thus, the schooling first-order condition in this maximization problem suggests that a positive level of schooling is not optimal when the marginal benefit of schooling is less than its marginal cost. Specifically, the model suggests that the schooling decision is determined by factors such as parental preference for education, child abilities and the related effectiveness of human capital investment, future returns to schooling, and parents' insurance capacity and resource constraints. This presents a challenge for empirical estimation of the effect of school dropout on cognitive outcomes because some of these factors also likely affect child cognitive outcomes directly rather than only through schooling investment. The next section presents the empirical approaches used to deal with these issues.

4. The empirical approach

I examine the effect of school interruption by estimating a reduced-form model for cognitive outcomes. If parents are utility maximizers in choosing inputs for the production

of human capital of their child as described above, then the first-order conditions of their optimization problem yield demand functions for the various inputs governed by prices and income. By replacing inputs in the production function with these expressions, reduced-form demand functions for human capital are obtained as a function of prices and income (Rosenzweig & Schultz, 1983). If prices are constant across observations, their effect is absorbed in the intercept term (Todd & Wolpin, 2007). Thus, I estimate test scores in 2000 as a function of household per capita consumption to account for missing inputs, as well as other variables that may affect unobserved parental preferences for inputs and household constraints such as child gender, mother's age and years of education, household composition (i.e., presence of an older sibling in the household), urban/rural area of residence, and province fixed effects. In addition, in order to account for the fact that children in the sample are at different schooling levels, I include fixed effects for years of completed schooling in 1997. By controlling for the fixed effects of education in 1997, I implicitly define the effect of school interruption to be the effect of extra schooling foregone between 1997 and 2000, averaged over estimates for children who were at the same schooling level in 1997.[7] Due to delayed entry into schooling and repetitions of grade levels, considerable differences exist between age and years of education. As a result, I also include child age in the main regression model. Since the survey occurred over a six-month period, I also include dummy variables for the month of interview.

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[7] In other words, I compare, for example, the scores in the year 2000 of two children who were in 5th grade in 1997, but one of them moved on to 8th grade by 2000, while the other dropped out. The effect of dropout is computed as the average effect for children at all schooling levels. This approach identifies the counterfactual outcome based on peer comparison. It is also helpful in dealing with any selection that has already occurred that is not captured by the data. For example, a potential concern could be that the pool of young children includes those of low, average, and high ability, while the pool of older children going to school in 1997 may only include children of high ability. Given the inclusion of grade fixed effects, however, this potential selection concern is eliminated.

The main model I attempt to estimate is: $Y_i = \beta X_i + \alpha D_i + U_i$, where Y_i is the measure of the cognitive outcome of child i in the year 2000, X_i is a function of child and household characteristics as discussed above, and D_i is a dummy variable equal to 1 if the child interrupted schooling between 1997 and 2000, and 0 otherwise (more specifics on the definition of a dropout are provided below). If Y_1 is the potential test score when individuals are "treated" (i.e., drop out of school) and Y_0 is the potential test score when individuals are "not treated" (i.e., don't drop out of school), then the potential outcome can be written as a function of the observable characteristics, X_i, and the unobservable error terms $U_{i,1}$ and $U_{i,0}$, where $Y_{i,1} = \beta_1 X_i + U_{i,1}$ and $Y_{i,0} = \beta_0 X_i + U_{i,0}$. Where the latent propensity to be treated explained by some exogenous shifters Z and an error term V follows $I_i = Z_i \gamma + V_i$, I represent the observed binary outcome of school interruption as $D_i = 1$ if $I_i \geq 0$ and $D_i = 0$ if $I_i < 0$. For each child, I observe either his treated or his untreated outcome and never both. The observed outcome can then be re-written as:

$$Y_i = (1 - D_i) * Y_{i,0} + D_i * Y_{i,1} = \beta_0 X_i + D_i(\beta_1 - \beta_0)X_i + U_{i,0} + D_i(U_{i,1} - U_{i,0}). \quad (1)$$

If the propensity to stay in school is uncorrelated with unobservables $(Cov(V_i, U_i) = 0)$ and unobservables are homogeneous $(U_{i,1} = U_{i,0} = U_i)$, then this model can be estimated using OLS. However, if the error term U_i contains unobservable characteristics that also affect the decision whether to pull the child out of school (i.e., $Cov(V, U_i) \neq 0$), then OLS estimation is biased. In this case, the variable D_i and the error term U_i in equation (1) will be correlated even if $U_{i,1} = U_{i,0} = U_i$ in the empirical specification above. This is the pre-treatment heterogeneity bias where students who drop out differ from those who don't in terms of ability or socioeconomic characteristics, and

the same unobserved factors that determine the difference also affect the decision to drop out. Controlling for covariates is required to reduce or eliminate this bias.

I utilize test scores from 1997 before the dropout decision to estimate a value-added model. Past test scores could be used to proxy for parental preference for education, previously accumulated child skills and the effectiveness of human capital investment (the factors that affect the marginal benefit in the conceptual model presented above). If a child drops out of school because of low marginal cognitive benefit then, under certain conditions, controlling for past scores may help eliminate the bias. This model, however, yields unbiased estimates for past test scores only under restrictive assumptions about the degree of correlation of the error terms in test scores over time (Todd & Wolpin, 2003). If the coefficient on past test scores is biased and the dropout decision is correlated with past test scores, then the dropout coefficient would still be biased. In addition, past test scores may not be a good proxy for unobserved ability and preferences, and may not account for other unobservables that may govern both the dropout decision and skill accumulation, such as the time household members spend in different activities (which is likely to change during the crisis).

As a result, I also employ an instrumental variables approach to account for endogeneity in the schooling decision related to omitted variable bias. I use age and gender-specific difference in predicted and observed non-enrollment rates in 1998 (during the crisis) as the source of identifying variation (more details on instrument choice and validity are given below). If the endogeneity in the schooling decision is based on omitted variable bias related to the marginal benefit from schooling, then the IV estimates of the impact of dropout, as well as the value-added estimates, could be expected to be lower than the OLS

estimates (i.e., IV corrects for a negative bias because omitted ability affects test scores positively but ability and dropout are negatively corrected). If some children drop out because of resource constraints (high marginal cost), however, then the IV estimates may be larger than OLS estimates as has been consistently shown in the literature on labor market returns to schooling (Card, 2001). In this context, higher IV estimates could be expected if poorer families (those affected most by the income shock) have fewer opportunities to provide compensatory time or money investments.

A different type of bias often considered in the human capital literature is treatment effect heterogeneity bias, or sorting on the treatment effect. This bias exists if $U_{i,1} \neq U_{i,0}$. In this case, any instrument that is a good predictor of the propensity to interrupt school will by definition be correlated with D_i and thus with the error term in the observed outcomes equation, $U_{i,0} + D_i(U_{i,1} - U_{i,0})$. The individual error terms could be different under different scenarios if, for example, some children learn better out of school (e.g., at home or at work) because of the quality of schooling they receive or the nature of their human capital production function (note that the model does not constrain the coefficients associated with the various inputs in cognitive achievement to be equal in the different regimes). Thus, when children are heterogeneous in the manifestation of their skills and family effects, and when the decision to stay in school is endogenous and related to these skill and family effects, an endogenous regime-switching regression model can be used to estimate the effect of dropout on child cognitive and achievement scores.

Using the notation above, the endogenous regime-switching regression model is estimated by maximizing the following likelihood function:

$$L = \prod_{i=1}^{n} [f(Y_i|D_i = 1) * P(D_i = 1)]^{D_i} [f(Y_i|D_i = 0) * P(D_i = 0)]^{1-D_i}$$

where $P(D_i = 1) = 1 - P(D_i = 0)$ stands for the probability of dropping out of school, and $f(Y_i|D_i = 1)$ and $f(Y_i|D_i = 0)$ are the conditional distributions of the observed outcomes. As in the previous model, the probability of dropping out of school is modelled as a function of the variables included in the outcome equation as well as an excluded shifter. I use the same age and gender-specific instrument to help with the empirical identification of the model.

Under the assumption of joint normality of the error terms, the first term in the likelihood function can be represented as:

$$f(Y_i|D_i = 1) * P(D_i = 1) = f(Y_{i,1}|I_i \geq 0) * P(I_i \geq 0) = \int_{-\infty}^{-Z_i\gamma} g(U_{i,1}, V_i) dV_i =$$

$$\frac{1}{\sigma_1} * \phi\left(\frac{U_{i,1}}{\sigma_1}\right) * \int_{-\infty}^{-Z_i\gamma} \left(\frac{1}{\sqrt{1-\rho_1^2}} * \phi\left(\frac{V_i - \rho_1\left(\frac{U_{i,1}}{\sigma_1}\right)}{\sqrt{1-\rho_1^2}}\right)\right) dV_i ,$$

where σ_1 is the standard deviation of the error term $U_{i,1}$, ρ_1 is the correlation coefficient between V_i and $U_{i,1}$, and $\phi(.)$ is the standard normal probability distribution function. Similar manipulations are applied to the second part of the likelihood function to yield a final log-likelihood function

$$lnL = \sum_i^n D_i * \left[ln\phi\left(\frac{U_{i,1}}{\sigma_1}\right) - ln\sigma_1 + ln\Phi\left(\frac{\left(Z_i\gamma + \frac{\rho_1 U_{i,1}}{\sigma_1}\right)}{\sqrt{1-\rho_1^2}}\right)\right] +$$

$$+(1 - D_i) * \left[ln\phi\left(\frac{U_{i,0}}{\sigma_0}\right) - ln\sigma_0 + ln\left(1 - \Phi\left(\frac{\left(Z_i\gamma + \frac{\rho_0 U_{i,0}}{\sigma_0}\right)}{\sqrt{1-\rho_0^2}}\right)\right)\right].$$

35

This likelihood function accounts for the fact that the error terms are possibly not independent. In the case when both correlation coefficients, ρ_1 and ρ_0, are zero (i.e., $Cov(U_1, V) = 0$ and $Cov(U_0, V) = 0$), OLS estimation of the effect of dropout is consistent. If at least one of the coefficients is found to be statistically different from zero, then an endogenous switching model is appropriate. If the two coefficients have opposite signs and $\rho_1 > 0$ while $\rho_0 < 0$, then the results provide evidence for comparative advantage. The reason is that individuals who choose to be in regime 1 (i.e., have high values of the unobservable V term) also have high outcomes in regime 1 (high V implies high U_1 when $\rho_1 > 0$). In other words, the unobservable factors that make a child more likely to drop out of school also make the child have higher test scores when not attending school. Similar logic follows for individuals in regime 0.

A model of comparative advantage is plausible when the outcome of dominating interest is future wages of the child and higher wages can be earned in jobs requiring different skills (Roy, 1951). For example, in a seminal contribution to the literature, Willis and Rosen (1979) show for the US that the decision of whether to attend college depends on the expected lifetime earnings under the two options (i.e., college or no college). They also find evidence of comparative advantage, showing that college graduates would have been worse off if they had not gone to college compared to people who actually did not go to college, and vice versa. In the present study, however, the outcome of interest is test scores. Yet, as mentioned above, there could be comparative advantage if some children learn best at school, while other children learn best in other settings such as work. Studies have indeed shown that skills can be accumulated in a variety of settings in addition to

learning at school (Hull & Schultz, 2001). Work, however, is unlikely to completely replace formal training in mathematics.

In order to quantify the effect of school interruption on cognitive outcomes, I estimate the policy-relevant parameter of the effect of treatment on the treated.[8] It is calculated as the difference in potential outcomes for the individuals who had a school interruption obtained by predicting their expected counterfactual cognitive scores (if they had not dropped out of school) based on observed characteristics and the returns to those characteristics in the other regime, given the selection process. It can be expressed as $TT = E[Y_1 - Y_0 | D = 1] = (\beta_1 - \beta_0) * X_1 + (\rho_1\sigma_1 - \rho_0\sigma_0) * \lambda_{1i}$, where the selection term is given by $\lambda_{1i} = \phi(Z_i\gamma)/\Phi(Z_i\gamma)$.[9,10] If children select into schooling based on comparative advantage, so that students who benefit less from formal education drop out while those who benefit more stay in school, then the TT effect should be small. This result was found to be true for high-school seniors in the US (Li, Poirier & Tobias, 2004). In the context of school dropout during the Indonesian crisis, however, resource constraints that govern school decisions appear to be a more plausible explanation than comparative advantage. If so, sorting on the treatment effect would not occur and no difference would be found between the effect of school interruption on dropouts and the effect of interruption on a random individual made to drop out.

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[8] The standard errors for these treatment parameters are estimated by bootstrapping the maximum likelihood estimation with 1000 replications.

[9] Similarly, the effect of treatment on the untreated is the difference in predicted potential outcomes for those individuals who stayed in school given by $TUT = E[Y_1 - Y_0 | D = 0] = (\beta_1 - \beta_0) * X_0 + (\rho_1\sigma_1 - \rho_0\sigma_0) * \lambda_{0i}$, where $\lambda_{0i} = -\phi(Z_i\gamma)/(1 - \Phi(Z_i\gamma))$. The average treatment effect of school interruption on cognitive and achievement outcomes, i.e., the effect on a random person forced to drop out, can be calculated as $ATE = E[Y_1 - Y_0] = (\beta_1 X_1 - \beta_0 X_0)$.

[10] Note that IV estimation assumes no heterogeneity in unobservables. With IV, ATE = TT = TUT.

5. Construction of the sample for estimation

I estimate the effect of school interruption on two outcomes measured at the time of the survey in 2000: Raven's scores and mathematics scores. To avoid the problem of limited dependent variables (since the potential number of correct and wrong answers is predetermined), I standardize both test scores, as is common practice in the literature (e.g., Malamud & Pop-Eleches, 2011; Paxson & Schady, 2007; Todd & Wolpin, 2007).[11] Since children were given different tests according to their age group, scores were standardized by test group (with a mean 0 and a standard deviation 1). All regressions control for gender to account for the potentially different distribution of scores by gender.

I restrict the sample to nuclear households from 1997, excluding extended families or families where any young members (under age 25) are not children of the household head or spouse. The reason for this restriction is to ensure that parents have decision-making ability over child investments, and to avoid issues of resource allocation among cousins.[12] At the individual level, observations are used only for children between 7 and 15 years of age who are attending school and have completed 9 or fewer years of education as of 1997. In addition, individuals who provide inconsistent answers (for example, who report years of schooling in 1997 higher than years of schooling in 2000) or who have missing values for any of the variables used in the analysis, are excluded.[13]

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[11] A related concern regarding the dependent variable is that the mathematics test only contained five questions. A test of robustness of OLS results to model specification by using a count-data model with a Poisson distribution shows that results remain qualitatively similar.

[12] In Chapter 3, I examine parental education investment in this period and find that, on average, parental resource allocation among siblings is not affected by child ability. This may not be the case for extended family members.

[13] No significant differences in demographic characteristics exist between these observations and the rest of the sample.

The resulting sample of children (3,414) is comprised of four different groups. One group is the group of children who are not in school in the year 2000 and have zero or one year of extra education completed by 2000 (258, or 7.56% of the sample). Those are the children most likely to have dropped out because of the crisis due to resource constraints during the large negative income shock. A second group contains the children who are not in school in 2000 but have completed 2 or 3 years of schooling since 1997 (220). These children drop out after the crisis and their school decision is likely due to other considerations such as age or low abilities. The third group of children (1,253) is comprised of children who are still in school in 2000 but are lagging behind in their expected years of education (e.g., only have 1 or 2 years of schooling more in 2000 than in 1997). The reason children lag behind may be because of temporary school interruption due to the crisis or because of grade repetition (which may or may not be associated with absences during the crisis). In addition, the large proportion of children in this group suggests that some parents and children may have misreported the years of schooling and that the definition of this group may suffer from measurement error (see more on this below). Finally, members of the fourth group (1,683) are children who are in school in 2000 and have completed at least three years of education between 1997 and 2000. The fraction of dropouts in the sample may not be representative of the population because of the various sample restrictions imposed on the data. However, my findings are broadly consistent with Thomas et al. (2004) who show that in 1998 the proportion of male (female) children not enrolled in school at age 10 is 4.2% (3.8%), 8.2% (7.4%) at age 12, and 21.5% (22.3%) at age 14.

For analysis of the effects of school dropout on cognitive and achievement outcomes, I compare outcomes of group 1 to the outcomes of the counterfactual sample

comprised of groups 2 and 4. By including group 2 as part of the comparison group, I recognize that some of the children who drop out during the crisis may have stopped going to school later anyway because of age and parental inability to finance further education, or because of low learning abilities, but the crisis induces them to do so earlier. While the sample restrictions on the group of permanent dropouts aims to reduce the endogeneity in the schooling decision and isolate children who drop out because of the crisis rather than because of low abilities or low quality of education, this group may also include children who would have dropped out at the same time during their schooling career in the absence of the income shock. Thus, the effect I identify is an average across these children. If children who would have dropped out anyway select out of school due to low abilities, then my estimates would be biased downward. With a crisis-related instrument for the decision to drop out, the effect on the population of "compliers" can be better identified (more on instrument choice is given in the next section).

It should be noted that I exclude group 3 from the comparison sample because some (but not all) of the temporary school interruptions are likely caused by the crisis. I also do not consider them as part of dropout sample because of the potential measurement error problem.[14,15]

Table 2 lists the number of dropouts by completed years of schooling in 1997 in the sample used for analysis. It shows that a large proportion of children drop out after

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[14] This is of concern because the measurement error in a binary variable representing school interruption is not classical; the direction of the bias is unknown (rather than an expected attenuation bias).

[15] Including the population of students who interrupt schooling temporarily in the analysis would also necessitate the use of a different conceptual and empirical model where some students decide whether to drop out of school in period 1, and then in period 2 those who dropped out decide whether to go back to school. While this would be an important behavior to study, this type of analysis is beyond the scope of this paper. Furthermore, the data used here are not well suited to study such dynamic decision-making.

completing 6 years of primary school or 3 years of junior high school.[16] Table 3 presents descriptive statistics of the characteristics of the dropout group (group 1) compared to groups 2 and 4 combined. Dropouts are on average 2.2 years older and have 2.15 more years of education in 1997, although by the year 2000 they lag behind by 0.34 years in education. Despite their higher education in 1997, dropouts have lower math scores on average (although this may be a function of the different types of tests taken based on age). Dropouts also come from families with lower per capita expenditure in 1997 and are more likely to live in a rural area.

6. Instrument choice

In order to better isolate the effect of school dropout on the population of children who would not have stopped their schooling except for binding budget constraints, a crisis-related instrument is needed for the decision to drop out. One potential instrument is the geographical variation in unemployment rates during the crisis. This is an instrument often used to model schooling decisions in the US especially at the transition between high-school and college (Carneiro, Heckman, & Vytlacil, 2011). In Indonesia, unemployment rates during the crisis did not vary much, but real wages fell substantially. One problem with using inflation rates or average wage rate changes as potential instruments for the size of the crisis is that these instruments are found to be either correlated with pre-crisis outcomes or affect post-crisis outcomes independently of schooling decisions. This is likely to be the case when the variation in inflation and wage rates is measured at the

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[16] The regression results are robust to dropping children with zero years of completed education. Similarly, imposing a restriction for common support of the data does not affect results. Following Carneiro et al. (2011), I define common support as the intersection of the support of the probability of dropout given $D = 1$ and the support of the probability of dropout given $D = 0$. Restricting the empirical estimates to the common support (0 to 0.84) excludes only 9 observations.

provincial level and the quality of education differs between provinces in a similar way, as well as when the crisis has independent effects on outcomes through nutrition, variation in parental and government investments, and other outside factors. In addition, these instruments likely do not have a monotonic effect on school dropout in Indonesia because high inflation increases the direct costs of schooling, while also decreasing the opportunity cost of schooling, as child labor becomes less profitable.

In their paper on the effect of the crisis on school enrollments in Indonesia, Thomas et al. (2004) use a series of national socioeconomic surveys (SUSENAS) and estimate predicted non-enrollment rates in 1998 based on the enrollment trend from previous years. Then, they compare their predictions to the observed non-enrollment rates in 1998 by age and gender and find a large discrepancy between the two rates that could be attributed to the crisis. Similarly, I use the SUSENAS data for my restricted sample of 13 provinces to estimate linear prediction for non-enrollment rates in 1998 for children with ages 7 through 15. I use the age-gender specific differences between the predicted and observed non-enrollment rates as the instrument that provides identifying variation.[17]

Figure 5 presents plots of the observed non-enrollment rates by age and gender from 1993 to 1998, showing the long-term downward trend in non-enrollment and the unexpected increases in 1998. While children of most ages had higher non-enrollment rates than expected, figure 6 shows that the largest deviations from the long-term trend in non-

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[17] For 1998, observed rates for all but 11-year old boys are significantly different from the predicted rates. At the same time, even for 1997, the observed rates (for all but 10-year old boys and 8- and 12-year old girls) deviate significantly from the trend of the previous years. This suggests that the difference between predicted and observed non-enrollment rates in 1998 is comprised of the effect of the negative economic shock, as well as any period-to-period fluctuations. Comparing the deviations from the long-term trend for 1998 and 1997, however, shows that the deviations in 1998 are significantly larger than those of 1997 and usually have a positive sign (i.e., more non-enrollments), while those in 1997 are usually negative. This result supports the hypothesis that identification is not based simply on natural yearly variations in enrollment rates and that the instrument identifies the effect of short-run budget constraints.

enrollment in 1998 are at ages 12, 13 and 14 for both girls and boys. This is consistent with children failing to enroll in secondary school after having completed six years of primary school.[18] Analysis of the 1997 self-reported expenditure data in the IFLS dataset shows that overall education expenses as well as registration fees and monthly fees are significantly higher for children in junior high school than in primary school, including higher registration and monthly fees. Therefore, the observed increased dropout rates after primary school are consistent with household inability to pay fees due to short-term budget constraints. In addition, seven-year old girls also experienced very high non-enrollment rates, which suggests that parents may have postponed girl enrollment in school or may have withdrawn girls who were just starting school. In my sample, however, only one child aged 7 who was in school in the beginning of the 1997/1998 academic year dropped out of school permanently. Thus, the effect is largely identified off of the older children, transitioning out of primary school. I also perform a robustness check excluding seven-year olds from the estimation sample. The regression results remain similar.

The relevance of the instrument could be tested using a linear probability model of dropout status on only a constant term and the instrument. It yields a coefficient of 0.07, significant at the 5% level, clustering standard errors at the level of the instrument. The first stage results, adding various controls, are further discussed below. In terms of validity, the main assumption is that the instrument only affects the outcome through dropout and thus has no direct, independent effect on test scores. Adding the instrument to the main outcome equation confirms that it has not significant impact on test scores and also does not affect the coefficients on the other explanatory variables. Intuitively, if ability is the

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[18] The effect is seen at a range of ages because some children enroll in school later or repeat a grade.

omitted variable that determines both the individual schooling decision and child test scores, then using an indicator of the group-level probability of dropout to predict individual decisions isolates the non-endogenous component of dropout. However, since both group dropout rates and individual test scores generally vary linearly with age, group rates may still be directly correlated with test scores. Using the difference between predicted and observed rates as the instrument accounts for this possibility since the effect of the crisis on group probability of dropout varies nonlinearly with age. In other words, the instrument identifies the effect of dropout on children who were more vulnerable during the economic crisis and who had higher probability of dropout than what would have been expected given their age and gender – largely, the children transitioning between different school levels.

One concern about using this instrument could be that it is a generated variable. However, Wooldridge (2010) shows that typical two-stage least squares estimation yields consistent estimates of the standard errors even with a generated instrument. The first-stage results, however, may be biased. To account for that, I present a robustness analysis for the decision to drop out where I use a two-step bootstrap estimation procedure, following Ashraf and Galor (2013). I draw a random sample with replacement from the SUSENAS data, estimate new values for the generated regressor, and use those to perform the first-stage regression using my IFLS sample. The process is repeated 1,000 times and the standard errors in the first-stage model are calculated as the standard deviation of the point estimates of the estimation coefficients.

Another concern could be that this instrument varies only at the age and gender levels. When grouped variables in OLS are matched to individual-level variables, the

standard errors are biased downward (Moulton, 1990). Similarly, for IV regressions, Shore-Sheppard (1996) has shown that standard errors may be biased when using grouped data for the instrument. Therefore, in order to account for having an aggregate instrument and thus clustered errors, I estimate the second-stage standard errors using the correction for the covariance matrix, presented in Shore-Sheppard (1996).

Having a small number of clusters (18), however, presents additional challenges. Cameron, Gelbach and Miller (2008) show that cluster bootstrap of the t-statistic performs better than the traditional clustered standard errors (as well as better than the cluster bootstrap for the standard errors) when the number of clusters is small. Therefore, I also present robustness analysis for the second-stage bootstrapping the t-statistic. I re-sample clusters with replacement and perform two-stage estimation, clustering standard errors in each stage at the level of the instrument.[19]

The previous literature described important differences in the effect of the crisis by urban/rural area of residence and per capita expenditure quartile in 1997. I perform robustness checks of the main results by interacting the instrument with these variables. This analysis avoids the need to cluster at the instrument level and provides support for the fact that identification is based on variation in resource constraints.

Further, I present evidence that the population of compliers identified by the instrument is the population of children with binding budget constraints by splitting the sample in two based on province inflation levels and studying differences in the effect of dropout.

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[19] The bootstrap t-statistic is computed as $t^* = (\widehat{\beta_j^*} - \widehat{\beta})/se(\widehat{\beta_j^*})$, where $\widehat{\beta_j^*}$ $(se(\widehat{\beta_j^*}))$ is the IV estimate of the coefficient (standard error) from the j-th bootstrap sample, while $\widehat{\beta}$ is the coefficient estimate from the full IV estimation. This procedure yields equal-tail bootstrap p-values, calculated as $p = 2 \times min[\frac{1}{B}\sum_{j=1}^{B} I(t_j^* < \hat{t}), \frac{1}{B}\sum_{j=1}^{B} I(t_j^* > \hat{t})]$(Davidson & MacKinnon, 2010).

7. Effect of school interruption on cognitive and achievement outcomes

7.1 The basic model

The effect of permanent school interruption on test scores is first estimated using OLS (Table 4), clustering standard errors at the household level.[20] In column (1), the effect of dropout on mathematics test scores is found to be about half a standard deviation (-0.50) with a standard error of 0.060. As expected, controlling for past mathematics test scores in column (2) reduces the size of the coefficient associated with school dropout, although it remains large (-0.46) and significant. While school interruption is found to have higher effect on school performance than on general cognition, it also affects Raven's scores. On average, in columns (3) and (4), dropout is associated with a reduction of 0.36 standard deviations in Raven's assessment scores (with a standard error of 0.069), which changes little in the value-added model.[21] Interactions of the dropout indicator by gender, school grade, household per capita expenditure, or 1997 test score show no significant heterogeneity in the effect of dropout by these observable characteristics. The effect of dropout varies significantly by age, where children lose out more if they drop out when they are younger. This finding is consistent with early investments in children being more productive than later investments.

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[20] The main results include multiple children per household and allow dropouts to have siblings in the comparison group. If parents shift resources to the child that stays in school, the skill accumulation of those who stay in school may be overestimated. Excluding siblings of the dropouts from the comparison group (97 observations), however, yields similar results.

[21] These models do not control for the test group in 2000, even though the dependent variable is standardized by the type of test the child took. The reason is that controlling for test group soaks up variation that is not due to the type of test but rather to the age profile of scores. Plotting the age coefficients from a regression of mathematics scores on age and the other explanatory variables reveals no break in the linear relationship between age and test scores (either in terms of change of slope or change in intercept) at the age cutoff (age 15 in 2000) for the two test groups. As a result, I do not include test group in the regressions I report in the main analysis.

The estimates on the effect of school dropout are comparable to findings in the related literature. The CCT program in Nicaragua increased schooling of 9-12 year-old boys by about half a grade and increased test scores on average by 0.2 standard deviations for math scores and 0.13 standard deviations for Raven's scores (Barham, Macours & Maluccio, 2013). Cascio and Lewis (2006) find that an additional year of schooling due to compulsory schooling laws in the US increased AFQT (a general measure of literacy) test scores of black minorities by 0.31 standard deviations. To better relate the estimation results to the particular context of schooling in Indonesia, I fit a linear trend line through the coefficients for each completed grade level. The slope of the trend line is 0.118 for mathematics and 0.129 for Raven's score. This suggests that, on average, an increase of one year of schooling yields an increase of 0.118 and 0.129 standard deviations in mathematics and Raven's scores, respectively. Therefore, a coefficient of -0.36 associated with the dropout variable in the Raven's equation is consistent with an average loss of close to three years of schooling. The dropout coefficient on mathematics scores is somewhat larger than expected. One potential explanation is that mathematics skills obtained only through formal training are more easily eroded than is general cognition that may be a better measure of innate ability or may be developed outside of school as well.

While OLS results are informative, the estimates are biased if dropout is endogenous. If the decision to stay in school is a function of the difference between marginal benefits and marginal costs, a reduction in marginal benefit and an equal increase in marginal cost have the same effect of raising the probability of dropping out. If a child drops out of school because of low ability (low marginal benefit) then under certain conditions, controlling for past scores may help eliminate the bias, as discussed previously.

The value-added models in columns (2) and (4) yield support for this hypothesis as the estimates of the dropout effect are lower than (but close to) the OLS estimates. Similarly, if endogeneity in the schooling decision is based only on omitted ability bias, the IV estimates could be expected to be lower than OLS estimates. If some children drop out because of resource constraints (high marginal cost), however, then the IV estimates may be larger than OLS estimates. The next section presents the associated results.

7.2 The instrumental variables model

Table 5 shows the first-stage linear probability regression model results using the differences in predicted and observed enrollment rates as an instrument for school dropout. Model (1) presents results clustering standard errors at the level of the instrument. The results provide evidence for the relevance of the excluded instrument. The R-squared for the first stage equation is 0.219. The excluded instrument has a high predictive power with an F-statistic (calculated as the square of the t-statistic) of $F(1,17) = 21$ with a p-value less than 0.01. The positive sign associated with the instrument is also as expected. An increase in the group probability of non-enrollment increases dropout. Other factors that increase the propensity to dropout significantly include lower household per capita expenditures, rural area of residence, a less educated mother, and not having an older sibling in the household (who presumably could work and help insure the younger sibling against the effects of the income shock). Grade level is a significant determinant of school dropout as well. Broadly, the probability of dropout increases with the number of completed years of education (although the effect is nonlinear). The probability of dropout is highest for students in junior or senior high school as of 1997. Females are not significantly more likely to drop out than males.

In column (2), I perform a robustness analysis for having a generated regressor in the model. Jointly bootstrapping the "zeroeth-stage" and first-stage estimation yields larger standard errors associated with the coefficient on the instrument but it remains significant at the 5% level.

The second-stage results are presented in Table 6. Columns (1) and (2) use the Shore-Shepard correction of the variance-covariance matrix to account for using a grouped instrument, clustering at the level of the instrument (at the age-gender group). The IV estimates are about 1.6 times larger than the OLS estimates for mathematics scores and about 1.2 times larger for Raven's scores. This finding suggests that the dropout decision is driven by considerations of marginal cost rather than marginal benefit, as argued above. Thus, the population of compliers, induced to drop out of school by the instrument, is likely the population unable to pay school fees or facing high opportunity costs of schooling. These children are shown to have high returns to formal education. The effect on math scores, -0.81, is larger than the effect on Raven's scores, -0.56, and is also more precisely estimated. Accounting for a small number of clusters by using t-percentile bootstrap further increases the size of the standard errors so that only the effect on math scores remains significant at the 10% level.

The next section presents results using an endogenous regime switching model which allows for differences in unobservable characteristics and enables calculation of the effect of treatment on the treated. It also relaxes the constraint imposed by the regression models so far that restricts the coefficients for both the dropout and comparison group to be equal, which may introduce an interaction bias if human capital production functions of children who drop out during the crisis differ from those who stay in school. This may be

the case if production technologies vary by age when children who drop out in the study period are significantly older that children who stay in school, as suggested by Table 3. In addition, the government may not be able to distinguish households who would have withdrawn their child from school irrespective of the income shocks from those who would have kept their child in school if they had not been income constrained (the population of compliers identified in the IV model). From a policy perspective, the effect of treatment on the treated, which is estimated using an endogenous switching regression model, is the relevant policy parameter and thus is my preferred estimate.

7.3 The regime switching model

Table 7 presents the parameter estimates from the maximum likelihood estimation of the regime switching models where "regime 1" denotes outcomes for children who dropped out of school during the crisis and "regime 0" reflects outcomes for the comparison group (the results for the dropout equation are not presented as they are similar to the results on determinants of dropout from the first-stage regression of the IV model). For the comparison group, higher household per capita expenditures in 2000 are associated with better mathematics (column (2)) and Raven's assessment scores (column (4)), and the size of the effect is similar for the two outcomes. Having an older sibling in the household is associated with higher Raven's scores but has no effect on mathematics scores. Conditional on school grade, older children have lower scores on both assessments. Each additional year of mother's education increases mathematics scores by 0.029 standard deviations and Raven's scores by about half as much. More schooling of the child is also associated with higher test scores. Not many variables are statistically significant

determinants of either outcome for the group of dropouts (columns (1) and (3)), although this may be due to the relatively small sample.

As discussed earlier, ρ_0 represents the correlation coefficient between the error term in the test score equation for non-dropouts (U_0) and the error term in the choice model (V), and similarly for the term ρ_1 in the dropout equation (U_1). The correlation terms are not significant for the model on mathematics scores. This suggests that unobserved heterogeneity does not affect the dropout decision and that children who do not drop out during the crisis do no better or worse in math than a random individual would have done by staying in school, and similarly for children who do drop out during the crisis. These results imply that there is no sorting on the gains and that an IV model may be sufficient.

The Raven's model, however, provides some evidence of comparative advantage: children who drop out (i.e., have high values of the unobservable V term that drive the probability of dropping out) have high outcomes in the drop-out regime ($\rho_1 > 0$) and children who stay in school (i.e., have low probability of dropout and low values of the unobservable V term) have high outcomes in the school regime ($\rho_0 < 0$). Only the ρ_1 term is significantly different from zero. This suggests that compared to a random individual, children who drop out during the crisis may lose out less in terms of general cognitive skill accumulation.

One of the main assumptions of the endogenous switching model is that the error terms in the outcome and choice equations are jointly normally distributed. I test this assumption using an approach, suggested by Pagan and Vella (1989). The test is based on the two-step estimation of the endogenous switching model. In the first step, the selection terms for the outcome equations for dropouts ($\lambda_{1,i}$) and non-dropouts ($\lambda_{o,i}$) are calculated

from a probit model for the choice equation. Then the outcome regression model is run separately for dropouts and non-dropouts, controlling for the respective selection terms. The test for joint normality is like a regression specification error test, where the selection terms are additionally multiplied by the linear prediction from the first-step probit model (raised to the power 1, 2 and 3). If the coefficients on these three additional terms are jointly equal to zero, there is little evidence for model misspecification.

Performing this test for joint normality, I cannot reject lack of misspecification for Raven's test scores. For mathematics scores, the null hypothesis of the additional terms being jointly equal to zero cannot be rejected for the outcome equation of the dropouts. However, it is rejected at the 5% level for non-dropouts, suggesting that the endogenous switching model, assuming joint normality, may not be appropriate for studying the effects of dropout on mathematics test scores. This may also explain the lack of significance for the correlation terms in the mathematics outcome model.

Next, I relax the assumption of joint normality for the mathematics model and again test whether there is sorting on gains, this time using a semi-parametric approach as in Carneiro, Heckman, and Vytlacil (2011). I regress mathematics test scores on the vector of explanatory variables, the predicted probability of dropout, the product of the predicted probability of dropout and the explanatory variables, and a polynomial of the predicted probability of dropout. The polynomial terms are not jointly significant for mathematics scores, confirming that there is no sorting on gains in this model. For mathematics scores, the IV model may therefore be sufficient.

Table 8 presents a summary of the results from all analyses thus far, including estimates of the TT effect from the endogenous switching regression model. The TT

parameters are significantly different from zero with an effect size of -0.61 standard deviations for mathematics scores and -0.26 standard deviations for Raven's scores. For mathematics scores, the effect of treatment on the treated is not significantly different from the IV estimates. For Raven's scores, the 95% confidence interval of the effect of TT does not include the IV estimate.

The TT effect is averaged across children who would have dropped out anyway and children who only dropped out because of the crisis-induced variation in the instrument. The fact that the effect of treatment on the treated is smaller than the IV yields support to the hypothesis that children differ in their marginal benefit of schooling and suggests that the treated group may be sorting out of school based on the expected gains in test scores. The fact that the TT effects are significantly different from zero, however, shows that the marginal benefits of schooling for dropouts are not zero. Thus, unlike the case of US dropouts examined by Li, Poirier and Tobias (2004), the negative effect of school dropout for those who drop out is large and significant for both school-acquired skills and general skills.

7.4 Do children learn out of school?

In the analysis presented to this point, school dropout is found to have a large negative effect on cognitive and achievement outcomes. The magnitude of this effect, however, may be an underestimate (in absolute value) if skills can be acquired not only in school but also outside of school, as suggested by Hull and Schultz (2001) and Saxe (1988). About 48.8% of the sample children who dropped out of school soon after the crisis report having worked in the past year when interviewed in the year 2000 (compared to 6.7% in the comparison group). If working increases test scores and dropping out of school is

positively correlated with the probability of working, then working offsets some of the negative effect of dropout and the estimates of dropping out of school are biased downward (i.e., true effect should be larger in absolute terms). Alternatively, if working erodes skills, the negative effect of dropping out would be biased upward (i.e., true effect should be smaller in absolute terms). For example, Gunnarsson et al. (2006) provide evidence that child labor in Latin American countries is associated lower mathematics and language test scores, although their results are for school-going children, rather than children who dropped out of school and may be looking for an alternative source of learning.

In order to test for these hypotheses, I include an additional dummy variable reflecting working status as a control in all regression models. The findings are presented in Table 8. Generally, including an indicator for work status reduces the absolute size of the negative coefficient on dropout, suggesting that working is associated with lower scores. This suggests that working is not a good substitute for formal schooling and potentially reflects the fact that children who work have less time to engage in other informal learning activities.

In the preferred estimation for Raven's scores, the TT effect of dropout on Raven's scores is reduced by 40% (from -0.261 to -0.156) and loses statistical significance when controlling for work status. This finding implies a strong negative correlation between working and Raven's scores. The results therefore suggest that children of lower cognition are more likely to drop out and start working. This would be consistent with the previous finding that children who drop out of school are less able to improve their cognition at school than are children in the comparison group.

Controlling for work status also reduces the effect of dropout on mathematics scores in both the endogenous switching regression model and the IV model, although the change is much smaller (1% and 5%, respectively).

8. Robustness checks

8.1 Using a different set of instruments

While the main IV results are based on age-gender specific variation in the effect of the crisis, figure 1 suggests important differences by urban/rural area of residence and by income quartile. In order to better identify the effect on the affected population of children, as well as to allow more variation in the instrument, I include three additional variables as instruments: an interaction of the main instrument with a dummy for urban area, an interaction of the main instrument with a dummy for the households in the lowest per capita expenditure quartile, as well as a triple interaction term between the main instrument, area of residence and per capita quartile. I then adjust the main model to also control for the lowest per capita expenditure quartile and for its interaction with area of residence. I cluster standard errors at the household level.

The first stage yields results that are consistent with findings by Thomas et al. (2004): poor children and children in urban areas are those affected most by the crisis. In addition, the sign of the triple interaction term is negative, suggesting that children in rural areas and in the bottom quartile of the income distribution are affected the most (again confirming previous findings). The IV estimate of the effect of dropout in this case is broadly consistent with the previous results, yielding larger negative results for mathematics scores (-0.75, standard error=0.504), and smaller negative results for Raven's scores (-0.16, standard error=0.480). The results, however, are subject to a weak instrument

problem ($F(4, 1596) = 5.90$), which could explain the large standard errors and may also result in a bias of the coefficient estimates.

Next, I present further suggestive evidence that the population of compliers identified by the instrument is the population of children with resource constraints that bind during the crisis. I split the sample in two based on province inflation levels. Inflation levels in 1998 ranged between 67% and 95% in the thirteen provinces studied. I perform separate analysis for the provinces with inflation levels under 80% (6 provinces) and above 80% (the remaining 7). This results in a roughly equal split in the sample, with about 12% of children being dropouts in each of the two subsamples. The effect of dropout on mathematics scores is about twice as large for the subsample with higher inflation (-1.04 SD vs. -0.54 SD). This provides some suggestive evidence that supports that hypothesis that school dropout identified by the instrument is driven by resource constraints rather than low ability.

8.2 Using the panel nature of the data

The main analysis of this chapter identifies the effect of school dropout based on differences between observations in cross-sectional data. As a robustness check, I have also utilized the panel nature and household-level structure of the data. If the unobserved factors that determine both schooling decisions and schooling outcomes are common within a household so that the individual error term is comprised only of a random error and a family error component (i.e., $U_i = \epsilon_t + U_{hh}$), then a siblings fixed effects estimation would yield unbiased results on the effect of school dropout. This assumption is likely to be violated when parents selectively allocate resources among children and when siblings learn from each other. In addition, this method assumes away any individual-level error component.

Even if the siblings fixed effects estimation was appropriate, however, it would identify the effect of schooling based on variation among siblings, which may not be large. This method also reduces the sample substantially as only households with two or more school-going children with ages 7 through 15 in 1997 are used for the analysis (477 households).

Alternatively, child fixed effects could be used to allow both a family and an individual error component. The data used in the analysis, however, is not well suited for applying a child fixed effects approach because of the difference in the mathematics assessment tests in 1997 and 2000 in both the type and number of questions (in addition, no Raven's assessment was administered in 1997). Child fixed effects would also assume away any time-varying unobservables.

Given these caveats of the fixed effects analysis, I briefly discuss the robustness checks using the siblings and child fixed effects estimation. Overall, the fixed effects estimation confirms that school dropout during the crisis had a negative impact on mathematics test scores. In the sibling fixed effects model, the effect of school dropout on mathematics scores in 2000 is found to be a quarter of a standard deviation (-0.25) with a standard error of 0.108. A similar effect is also obtained with a child fixed effects estimation, where dropping out is associated with a reduction of 0.28 standard deviations in mathematics scores (with a standard error of 0.073).

8.3 Testing for changes in school quality over time

The preceding analysis has provided evidence that more schooling is associated with more learning and that children who dropped out during the crisis were likely to benefit from more schooling in terms of improving their mathematics as well as Raven's scores. While this estimated effect has internal validity with respect to the time period

studied, it may not have external validity when other time periods are considered if any changes in school quality took place between 1997 and 2000 because of the nature of the aggregate income shock. For example, if in a given community many children dropped out of school, then class sizes would have been reduced, which may lead to more effective teaching and higher learning than what would have occurred otherwise. This would lead to an upward bias in the estimated learning loss from dropout. At the same time, if parents are unable to pay school fees, schools may have reduced the use of productive inputs, such as books, which may have a negative effect on learning. By estimating the effect of missing schooling based on children who remain in this worse learning environment, I might be biasing the coefficient of dropout downwards.

In order to test whether any external validity concerns are justified, I take advantage of the fact that the IFLS also collected data at the community level, surveying schools and healthcare facilities. For the panel of schools interviewed in both 1997 and 2000, I compare the means of various measures of school quality and inputs across the two years by performing a t-test. I find that schools had smaller class sizes (down from about 40 to 35) and reduced book availability in 2000 compared to 1997. In addition, schools in 2000 were open for one week less than in 1997. On the other hand, teachers were less likely to hold another job, and worked fewer hours out of school and more hours in school. These simple descriptive statistics suggest that the crisis may have worsened school quality on some dimensions but improved it on others. In order to understand better the importance of these changes, I merged the community data with the child-level data and ran a linear regression of mathematics test scores on the community-level school quality measures, while also controlling for various child and household characteristics. I find that none of these

measures of school quality are significantly correlated with mathematics test scores for children who attended school in the year 2000. The only exception is the number of hours teachers worked outside of school which has a significantly negative effect on mathematics skills in 2000. Results remain unclear with respect to whether the lack of significance for the other measures is due to the fact that the sample of students in school in 2000 is a selected sample, excluding children who dropped out.

8.4 Testing the effectiveness of government scholarship grants

While the government in Indonesia was not able to respond immediately to the rising education costs in the middle of the 1997/1998 academic year, the government budget from July 1998 allocated scholarship money for the 1998/1999 academic year under the Indonesian Social Safety Net (JPS) program. The scholarship money was in the form of monthly cash transfers, conditional on school attendance. Several studies have shown that the JPS government program was effective in reducing the impact of the economic crisis on child education. For example, using census data and geographic variation in targeting, Sparrow (2007) performs district-level analysis and shows that JPS was effective in reducing school dropout. Using household-level data from the 100 Villages project and controlling for selection on observables, Cameron (2009) also finds that the JPS program was successful. No one, however, has examined learning outcomes associated with the extra schooling obtained. The 2000 wave of the IFLS survey included questions on receipt of JPS funds. This provides a unique opportunity to test the hypothesis that students helped to stay in school during an income shock would have good schooling outcomes in terms of both years of education and learning.

One of the challenges of estimating the effect of the JPS program is the non-random allocation of the scholarships. The goal of the program was to reach about 6% of primary school students, 17% of junior high school students, and 10% of senior high school students, as well as distribute at least half of the scholarships to girls. The scholarships were allocated in a decentralized manner. First, money was sent to the poorest districts. Then, committees were formed to select the poorest schools. Finally, school committees were formed to select the eligible students based on poverty and various socio-economic indicators (Pritchett, 2002; Sparrow, 2007). Availability of data on the exact socio-economic index used to distribute the grants would allow me to perform a regression-discontinuity analysis, while data on the geographic targeting would provide exogenous variation in the probability of receipt of the grants. Lacking such information, however, I identify the effect of JPS from variation in the receipt of scholarships between children in the same household.

Variation within the household exists because of JPS quotas by schooling level and gender. The main assumption of this analysis is that the allocation of funds within the household was exogenous and was not determined by the parents based on certain characteristics of the child (e.g., was not given to the smarter child or the child that was more likely to be kept in school otherwise). In order to test this assumption, I check whether receipt of JPS funds in the academic year 1999/2000 (the year for which households report scholarship availability in the third wave of the IFLS survey) is a significant predictor of years of schooling and parental expenditures on education in 1997. It is not. Mathematics scores in 1997 are not a significant predictor of JPS receipt, either. In addition, in order to interpret better the estimate of the effect of JPS receipt, it is

important to examine how parents responded to the government support. For budget-constrained households, if parental and government inputs are complements in the production of child human capital, receipt of JPS funds may be associated with higher parental investments in the targeted child at the expense of his or her siblings, which would bias the coefficient associated with JPS receipt upwards. Alternatively, if parental and government inputs are substitutes, overall investments in JPS and non-JPS children should not be different. The results suggest that the latter hypothesis is more plausible. There is no significant impact of JPS receipt on the probability that a child works, on number of hours spent in school, or on total education expenses.

The sibling fixed-effects regressions, which investigates the effects of the government program are based on 220 children in 87 households with at least two children, one or more of which received JPS funds. The results show that receiving JPS in 1999/2000 increased the probability of staying in school in the fall of 2000 by 12 percentage points, controlling for age, gender, and years of education in 1997. The effect of a JPS scholarship on years of schooling in 2000 is also both statistically and economically significant. For every two children who received the JPS funds, overall schooling increased by approximately one year compared to children who did not receive support but were potentially eligible for it (their siblings). Looking at the effect of a JPS scholarship on test scores, I find positive but not statistically significant effects on mathematics scores and positive and marginally significant effects at the 10% level on Raven's assessment scores (controlling for mathematics test scores in 1997, age, gender, and years of education in 2000). These results suggest that keeping marginal children in school, i.e., those who were most likely to drop out during the crisis, does not necessarily lead to more learning. This

result is contrary to previous findings of a large impact of dropping out. However, these estimates of the impact of JPS on test scores are based on a very small sample size (170 children in 67 households) because of missing test score information for some children. In addition, they are identified from within-household variation, which is not ideal because effects of one child receiving more schooling may have important spillover effects on siblings' skill accumulation.

9. Conclusion

Children may drop out of school for a variety of reasons, including low ability or motivation, low expected returns from education, or budget constraints. In this chapter, I use a variety of estimation approaches to identify the causal effect of missed schooling during the Indonesian crisis in late 1990s on test scores. All approaches yield significant negative effects of school dropout on mathematics test scores. The OLS estimates suggest that missing an average of three years of schooling is associated with 0.5 standard deviations lower mathematics scores. Some of this effect, however, could be explained by the fact that some dropouts have lower learning ability and select out of school. Indeed, controlling for past test scores in a value-added model reduces the effect of lack of schooling.

Similarly, IV estimates could be expected to be lower if unobserved ability is the main factor driving the endogeneity in the dropout decision. Using differences between predicted and observed dropout rates at the age-gender level as the source of crisis-induced identifying variation in propensity to dropout, I find IV estimates that are larger than OLS estimates (1.6 times larger). These results are consistent with children primarily dropping out because of short-run binding resource constraints during the Indonesian crisis.

Estimating an endogenous switching regression model, I also find evidence that children sort out of school based on their general cognitive skills (but not their mathematics scores). In addition, the effect of treatment on the treated for general cognition, measured by Raven's Colored Progressive Matrices assessment, is not significantly different from zero once I control for child work status. This result suggests that children of lower cognition and lower potential to improve their cognition at school are more likely to drop out of school and start working, whether they have lower mathematics learning abilities or not. This finding may be due to parents making decisions about the schooling of their children based on observation of children's general cognition, rather than their school performance.

Schooling in Indonesia has significant returns. One year of extra education is associated with an increase in hourly wages of 6.5% to 10.8% (Duflo, 2001). In addition, the literature on labor markets in the US as well as in developing countries (Glewwe, 2002; Hanushek & Woessmann, 2008) suggests that school-acquired skills have independent effects on labor outcomes. Therefore, much can be gained by insuring children against income volatility in order to prevent school interruptions when children who would have stayed in school in the absence of an income shock might otherwise be forced to drop out. Keeping these children in school would be an effective way to improve the long-term labor market outcomes of future generations.

Figures

Figure 1: School Dropout rates in 1997 and 1998
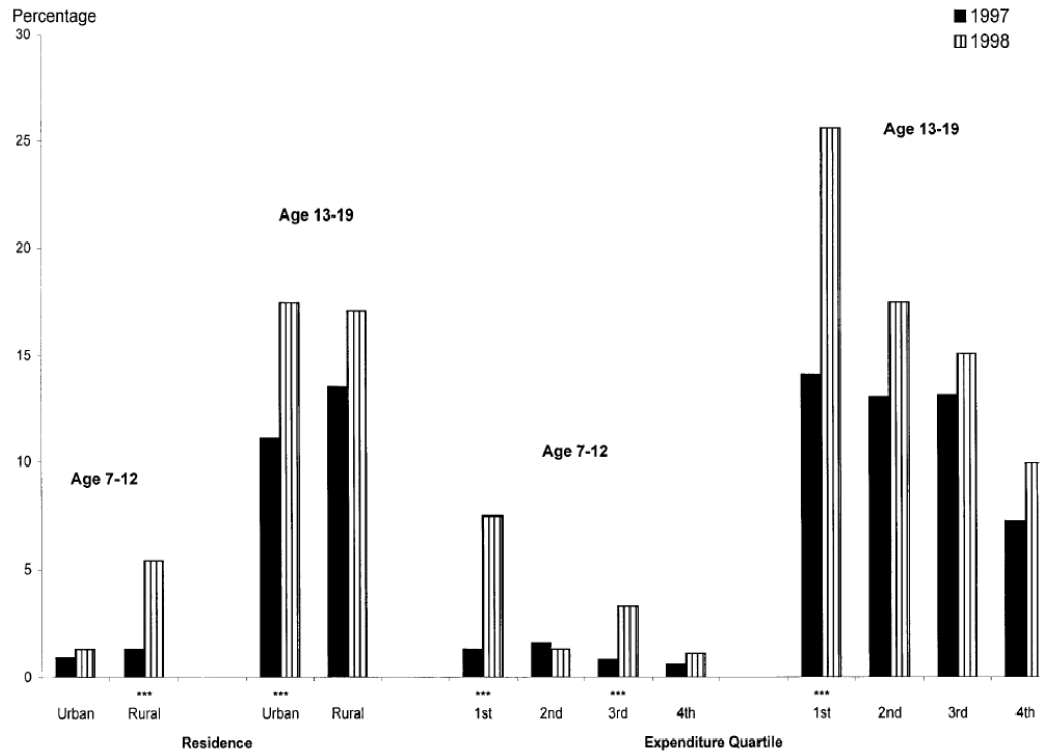
Figure 2: Proportion of children in school by age in 1997 and 2000
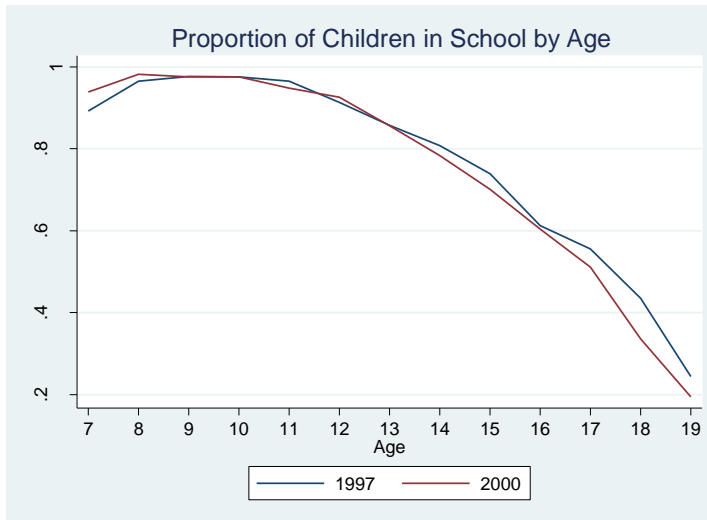
Figure 3: Mean number of correct test responses in 2000 by years of schooling for the comparison group

Mean number of correct answers in 2000
by years of schooling and separately by test group

Notes:
[1] Raven's test included 12 questions for ages 7 to 14 and 8 questions for 15 and above.
Mathematics test included 5 questions for both age groups.
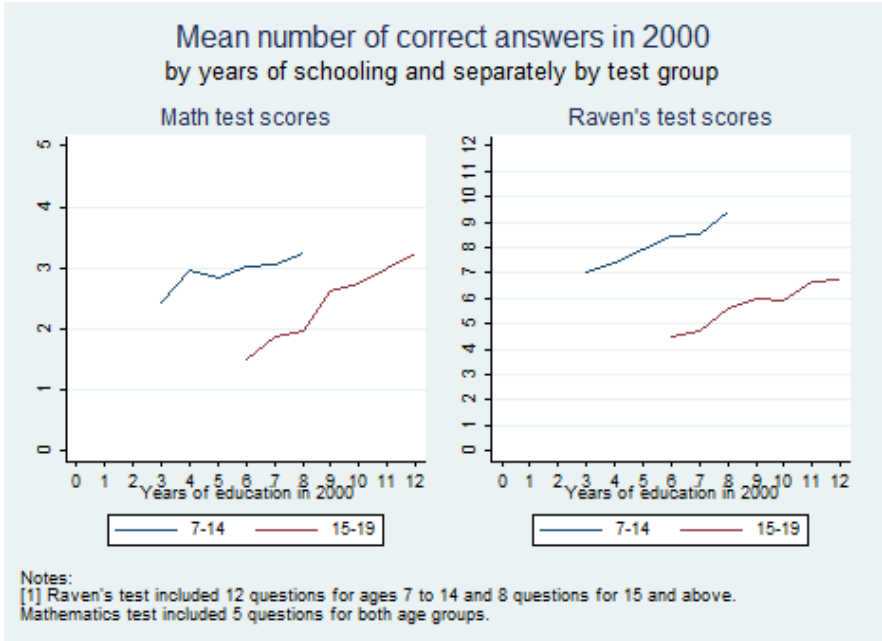
Figure 4: Distrbution of math scores in 1997 by future dropout status

Initial score distribution by dropout status
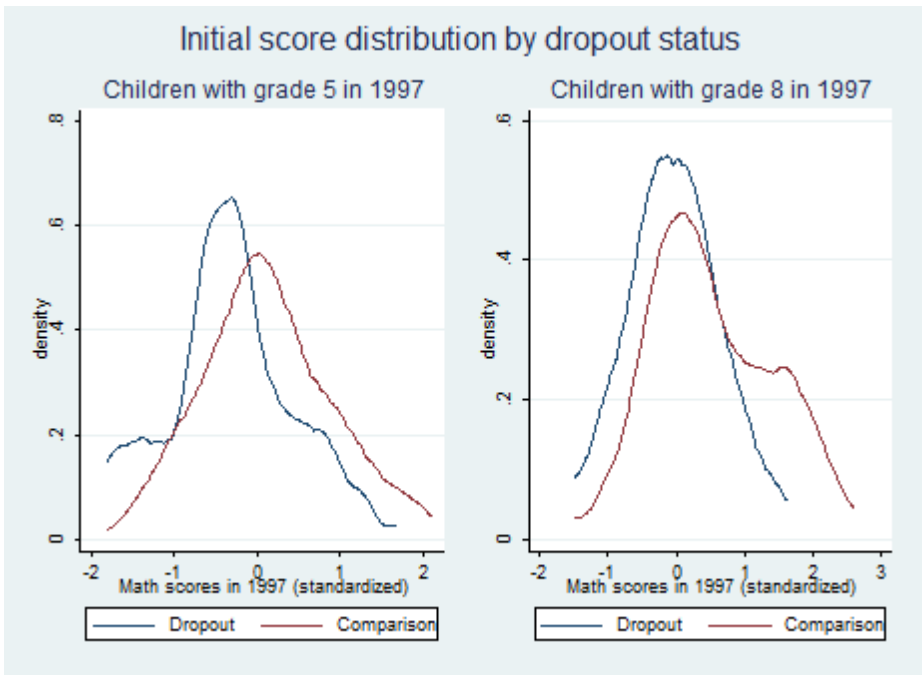
Figure 5: Observed non-enrollment rates, 1993-1998, SUSENAS data
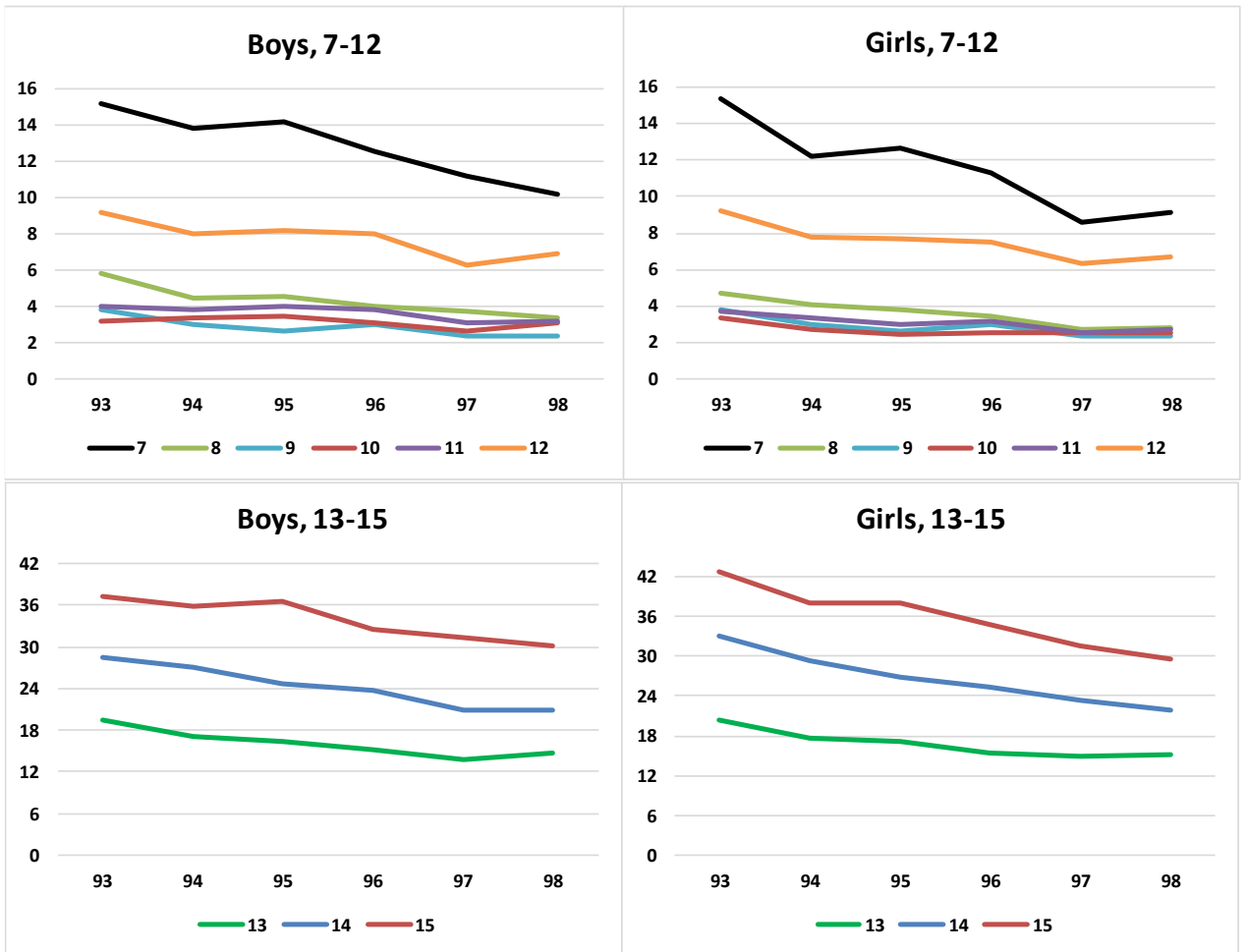
Figure 6: Difference between observed and predicted non-enrollment rates in 1998
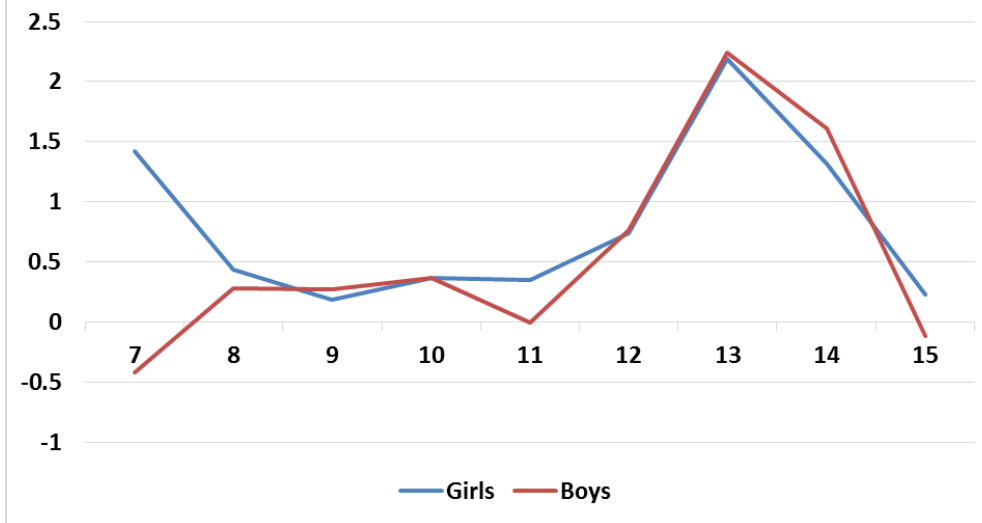
Table 1: School attendance by age

Variable	In school
Year 2000* Age=8	-0.0281
	(0.0264)
Year 2000* Age=9	-0.0464*
	(0.0265)
Year 2000* Age=10	-0.0460*
	(0.0263)
Year 2000* Age=11	-0.0627**
	(0.0263)
Year 2000* Age=12	-0.0332
	(0.0261)
Year 2000* Age=13	-0.0463*
	(0.0259)
Year 2000* Age=14	-0.0700**
	(0.0256)
Year 2000* Age=15	-0.0840**
	(0.0262)
Year 2000* Age=16	-0.0551**
	(0.0261)
Year 2000* Age=17	-0.0899**
	(0.0260)
Year 2000* Age=18	-0.1446**
	(0.0264)
Year 2000* Age=19	-0.0951**
	(0.0277)
Year fixed effects	Yes
Age fixed effects	Yes
Constant	0.8926**
	(0.0135)
N	19479

Notes:
[1] Results based on a linear regression for all children
between the ages of 7 and 19 in the two waves of the
IFLS survey in 1997 and 2000.

Table 2: Number of children who drop out by completed years of school and age

Completed years of school in 1997	Stayers	Dropouts	Total
0	139	0	139
1	127	2	129
2	258	6	264
3	295	12	307
4	185	11	196
5	247	55	302
6	340	64	404
7	133	9	142
8	116	71	187
9	63	28	91
Total	**1,903**	**258**	**2,161**

Age in 1997	Stayers	Dropouts	Total
7	249	1	250
8	246	4	250
9	222	3	225
10	232	11	243
11	256	31	287
12	182	53	235
13	184	50	234
14	154	64	218
15	178	41	219
Total	**1,903**	**258**	**2,161**

Table 3: Characteristics of children who drop out vs. the comparison group

Variable	Comparison, mean (sd)	Dropouts, mean (sd)	P-value
Age in 1997	10.64	12.84	<0.001
	(2.54)	(1.63)	
Female	0.50	0.49	0.834
	(0.5)	(0.5)	
Years of education in 1997	4.19	6.34	<0.001
	(2.39)	(1.84)	
Years of education in 2000	7.27	6.93	0.020
	(2.29)	(1.88)	
Math score in 1997	0.12	-0.18	<0.001
	(0.92)	(0.76)	
Math score in 2000	0.29	-0.27	<0.001
	(0.91)	(0.82)	
Raven's score in 2000	0.26	-0.08	<0.001
	(0.88)	(0.97)	
Urban area of residence	0.41	0.30	<0.001
	(0.49)	(0.46)	
Real per capita expenditure in 1997, '000 Rps	275.17	193.91	<0.001
	(378.63)	(142.5)	
Real per capita expenditure in 2000, '000 Rps	266.24	216.45	0.008
	(287.44)	(235.24)	
Has an older sibling living in the household	0.54	0.28	<0.001
	(0.5)	(0.45)	
Number of Children	1903	258	

Notes:

[1] P-values based on a t-test.

Table 4: The effect of school dropout on cognitive outcomes

	Math score		Raven's score	
Variable	OLS	OLS-VA	OLS	OLS-VA
	(1)	(2)	(1)	(2)
Dropout	-0.5001**	-0.4604**	-0.3628**	-0.3511**
	(0.0600)	(0.0635)	(0.0690)	(0.0754)
Math score in 1997		0.1664**		0.1234**
		(0.0263)		(0.0252)
Age in 1997	-0.0884**	-0.0777**	-0.1008**	-0.0879**
	(0.0188)	(0.0197)	(0.0206)	(0.0225)
Female	0.0844**	0.0889**	-0.1729**	-0.1836**
	(0.0371)	(0.0393)	(0.0351)	(0.0379)
Having a completed 2nd grade in 1997	0.1328*	0.0786	0.3245**	0.2406**
	(0.0737)	(0.0799)	(0.0777)	(0.0854)
Having a completed 3rd grade in 1997	0.3320**	0.3019**	0.6096**	0.5479**
	(0.0782)	(0.0839)	(0.0787)	(0.0855)
Having a completed 4th grade in 1997	0.4361**	0.4094**	0.6485**	0.5955**
	(0.0972)	(0.1065)	(0.1014)	(0.1113)
Having a completed 5th grade in 1997	0.5101**	0.4149**	0.8858**	0.7813**
	(0.0979)	(0.1061)	(0.1050)	(0.1147)
Having a completed 6th grade in 1997	0.6786**	0.5374**	0.9428**	0.8064**
	(0.1158)	(0.1243)	(0.1275)	(0.1407)
Having a completed 7th grade in 1997	0.7743**	0.6199**	0.9168**	0.7891**
	(0.1463)	(0.1566)	(0.1632)	(0.1781)
Having a completed 8th grade in 1997	0.8880**	0.7335**	1.2476**	1.1057**
	(0.1535)	(0.1642)	(0.1728)	(0.1904)
Having a completed 9th grade in 1997	0.9810**	0.7075**	1.2886**	1.1739**
	(0.1802)	(0.1904)	(0.1850)	(0.2024)
Mother years of education	0.0305**	0.0226**	0.0162**	0.0145**
	(0.0060)	(0.0064)	(0.0057)	(0.0061)
Mother age	0.0029	0.0038	0.001	0.0003
	(0.0033)	(0.0033)	(0.0031)	(0.0033)
Has older sibling who lives in household	0.0622	0.0301	0.1407**	0.1341**
	(0.0397)	(0.0425)	(0.0378)	(0.0409)
Log of per capita expenditure in 2000	0.0773**	0.0693*	0.0733**	0.0475
	(0.0359)	(0.0364)	(0.0368)	(0.0379)
Log of per capita expenditure in 1997	0.0446	0.0522	0.0221	0.0198
	(0.0342)	(0.0358)	(0.0334)	(0.0349)
Urban area of residence	0.1844**	0.1495**	0.1450**	0.0945**
	(0.0432)	(0.0456)	(0.0407)	(0.0439)
Month of interview fixed effects	YES	YES	YES	YES
Province fixed effects	YES	YES	YES	YES
Constant	-1.0672**	-1.4872**	-0.7049	-0.4032
	(0.4965)	(0.5128)	(0.4920)	(0.5013)
Observations	2161	1864	2161	1864
Number of clusters	1597	1409	1597	1409

Notes:

[1] Standard errors in parentheses, clustered at the household level.

[2] * significant at the 10% level, ** significant at the 5% level

Table 5: Determinants of school dropout (first-stage regression)

Variable	Standard error sensitivity models	
	Clustered at instrument level	Two-step bootstrap estimation of first stage
	(1)	(2)
Effect of crisis	0.0549**	0.0549**
	(0.0114)	(0.0206)
Age in 1997	-0.0091	-0.0091
	(0.0081)	(0.0108)
Female	-0.0298	-0.0298
	(0.0174)	(0.0285)
Having a completed 2nd grade in 1997	0.0329**	0.0329
	(0.0147)	(0.0221)
Having a completed 3rd grade in 1997	0.0633**	0.0633**
	(0.0191)	(0.0294)
Having a completed 4th grade in 1997	0.0681**	0.0681*
	(0.0284)	(0.0410)
Having a completed 5th grade in 1997	0.2094**	0.2094**
	(0.0347)	(0.0625)
Having a completed 6th grade in 1997	0.1616**	0.1616**
	(0.0477)	(0.0656)
Having a completed 7th grade in 1997	0.0711	0.0711
	(0.0564)	(0.0511)
Having a completed 8th grade in 1997	0.4342**	0.4342**
	(0.0647)	(0.0675)
Having a completed 9th grade in 1997	0.4199**	0.4199**
	(0.0758)	(0.1377)
Mother years of education	-0.0117**	-0.0117**
	(0.0024)	(0.0023)
Mother age	0.0002	0.0002
	(0.0010)	(0.0010)
Has older sibling who lives in household	-0.0556**	-0.0556**
	(0.0128)	(0.0126)
Log of per capita expenditure in 2000	-0.0295**	-0.0295**
	(0.0133)	(0.0122)
Log of per capita expenditure in 1997	-0.0336**	-0.0336**
	(0.0131)	(0.0133)
Urban area of residence	-0.0438**	-0.0438**
	(0.0194)	(0.0190)
Month of interview fixed effects	YES	YES
Province fixed effects	YES	YES
Constant	0.9169**	1.0253**
	(0.2623)	(0.2724)
Observations	2161	
Number of clusters	18	
R-squared	0.2193	

Notes:

[1] Analyses performed using a linear regression model.

[2] Model 2 accounts for the fact that the instrument is a generated variable. It is performed usign a two-step bootstrap procedure of the "zeroth" and first stage. Standard errros are clustered at the instrument level in the first-stage regression, which is being bootstrapped. Number of bootstrap replications=1000.

[3] * significant at the 10% level, ** significant at the 5% level

Table 6: The effect of school dropout on cognitive outcomes

Variable	Clustered standard errors in parentheses		p-values from t-percentile bootstrap in parentheses	
	Math score Second Stage	Raven's score Second Stage	Math score Second Stage	Raven's score Second Stage
	(1)	(2)	(3)	(4)
Dropout	-0.8126**	-0.5588*	-0.8126*	-0.5588
	(0.4004)	(0.3165)	(0.072)	(0.126)
Age in 1997	-0.0899**	-0.1018**	-0.0899**	-0.1018**
	(0.0163)	(0.0149)	(<0.001)	(<0.001)
Female	0.0791**	-0.1762**	0.0791**	-0.1762**
	(0.0340)	(0.0279)	(0.042)	(<0.001)
Having a completed 2nd grade in 1997	0.1412**	0.3298**	0.1412**	0.3298**
	(0.0439)	(0.0605)	(0.002)	(0.006)
Having a completed 3rd grade in 1997	0.3476**	0.6194**	0.3476**	0.6194**
	(0.0821)	(0.0658)	(<0.001)	(<0.001)
Having a completed 4th grade in 1997	0.4539**	0.6597**	0.4539**	0.6597**
	(0.0600)	(0.0987)	(<0.001)	(<0.001)
Having a completed 5th grade in 1997	0.5732**	0.9253**	0.5732**	0.9253**
	(0.0988)	(0.1021)	(<0.001)	(<0.001)
Having a completed 6th grade in 1997	0.7353**	0.9783**	0.7353**	0.9783**
	(0.0913)	(0.1246)	(<0.001)	(<0.001)
Having a completed 7th grade in 1997	0.8021**	0.9342**	0.8021**	0.9342**
	(0.1281)	(0.1534)	(<0.001)	(<0.001)
Having a completed 8th grade in 1997	1.0214**	1.3313**	1.0214**	1.3313**
	(0.2279)	(0.2060)	(0.002)	(<0.001)
Having a completed 9th grade in 1997	1.1051**	1.3665**	1.1051**	1.3665**
	(0.1669)	(0.2086)	(0.008)	(0.008)
Mother years of education	0.0268**	0.0139*	0.0268**	0.0139**
	(0.0052)	(0.0078)	(<0.001)	(0.022)
Mother age	0.0029	0.001	0.0029	0.001
	(0.0032)	(0.0025)	(0.258)	(0.571)
Has older sibling who lives in household	0.0444	0.1295**	0.0444	0.1295**
	(0.0551)	(0.0419)	(0.368)	(<0.001)
Log of per capita expenditure in 2000	0.0685**	0.0678*	0.0685*	0.0678**
	(0.0336)	(0.0361)	(0.072)	(0.024)
Log of per capita expenditure in 1997	0.0335	0.0152	0.0335	0.0152
	(0.0273)	(0.0306)	(0.104)	(0.527)
Urban area of residence	0.1700**	0.1359**	0.1700**	0.1359**
	(0.0524)	(0.0550)	(0.008)	(0.006)
Month of interview fixed effects	YES	YES	YES	YES
Province fixed effects	YES	YES	YES	YES
Constant	-1.0914*	-0.5768		
	(0.5713)	(0.6271)		
Observations	2161	2161		
Number of clusters	18	18		

Notes:

[1] Standard errors in models (1) and (2) are estimated using the Shore-Shepard (1996) correction of the second-stage errors, accounting for having a grouped instrument.

[2] Equal tail p-values in models (3) and (4) are estimated using a t-percentile bootstrap model.

[3] * significant at the 10% level, ** significant at the 5% level

Table 7 - Switching regression for school dropout

Variable	Permanent dropout			
	Math scores		Raven's scores	
	Regime 1	Regime 0	Regime 1	Regime 0
	(1)	(2)	(3)	(4)
Age in 1997	-0.023	-0.099**	-0.024	-0.104**
	(0.058)	(0.021)	(0.068)	(0.015)
Female	0.133	0.081	-0.327**	-0.159**
	(0.130)	(0.043)	(0.160)	(0.030)
Having a completed 2nd grade in 1997	0.166	0.153**	-0.394	0.342**
	(1.057)	(0.050)	(1.466)	(0.068)
Having a completed 3rd grade in 1997	0.906	0.343**	0.231	0.612**
	(0.953)	(0.085)	(1.355)	(0.071)
Having a completed 4th grade in 1997	0.560	0.484**	-0.214	0.692**
	(0.999)	(0.063)	(1.450)	(0.099)
Having a completed 5th grade in 1997	1.297	0.529**	0.581	0.897**
	(1.310)	(0.085)	(1.381)	(0.089)
Having a completed 6th grade in 1997	1.139	0.762**	0.323	0.999**
	(1.192)	(0.105)	(1.359)	(0.110)
Having a completed 7th grade in 1997	0.797	0.859**	0.563	0.932**
	(0.994)	(0.153)	(1.604)	(0.122)
Having a completed 8th grade in 1997	1.651	0.929**	0.988	1.184**
	(1.643)	(0.170)	(1.308)	(0.175)
Having a completed 9th grade in 1997	1.559	1.142**	1.064	1.297**
	(1.649)	(0.205)	(1.383)	(0.244)
Mother years of education	0.000	0.029**	0.004	0.016**
	(0.058)	(0.004)	(0.035)	(0.005)
Mother age	0.000	0.003	0.011	-0.001
	(0.007)	(0.003)	(0.013)	(0.002)
Has older sibling who lives in household	-0.082	0.056	-0.007	0.171**
	(0.206)	(0.043)	(0.105)	(0.040)
Log of per capita expenditure in 2000	0.010	0.078**	-0.051	0.080**
	(0.102)	(0.038)	(0.102)	(0.038)
Log of per capita expenditure in 1997	-0.015	0.040	-0.016	0.025
	(0.208)	(0.022)	(0.103)	(0.032)
Urban area of residence	0.332	0.163**	0.067	0.145**
	(0.224)	(0.048)	(0.149)	(0.055)
Month of interview fixed effects	YES	YES	YES	YES
Province fixed effects	YES	YES	YES	YES
Constant	-1.926	-1.128**	-0.908	-1.104**
	(1.451)	(0.506)	(1.700)	(0.538)
ρ_1	0.493		0.334**	
	(0.901)		(0.141)	
ρ_0	0.097		-0.060	
	(0.076)		(0.133)	

Notes:

[1] Standard errors in parentheses, clustered at the instrument level.

[2] * significant at the 10% level, ** significant at the 5% level

Table 8: Summary of treatment effects of permanent school dropout

	OLS	OLS - Value-added	IV	IV - percentile-t bootstrap	Endogenous Switching (TT)
			Not controlling for work		
Math scores	-0.5001**	-0.4604**	-0.8126**	-0.8126*	-0.6136**
	(0.0600)	(0.0635)	(0.4004)	pvalue=0.0721	(0.1143)
Raven's scores	-0.3628**	-0.3511**	-0.5588*	-0.5588	-0.2609**
	(0.0690)	(0.0754)	(0.3165)	pvalue=0.1261	(0.1106)
			Controlling for work		
	OLS	OLS - Value-added	IV	IV - percentile-t bootstrap	Endogenous Switching (TT)
Math scores	-0.4255**	-0.4000**	-0.7719*	-0.7719	-0.6065**
	(0.0662)	(0.0701)	(0.4683)	pvalue=0.1261	(0.1842)
Raven's scores	-0.3449**	-0.3554**	-0.5717	-0.5717	-0.1561
	(0.0757)	(0.0819)	(0.3779)	pvalue=0.1902	(0.1209)

Notes:

[1] The OLS and OLS-Value added models have standard errors clustered at the household level.

[2] The basic IV model is estimated using the Shore-Shepard correction for standard errors clustered at the level of the instrument (a total of 18 clusters).

[3] The percentile t-bootstrap IV model is performed using block random sampling (clustered at the level of the instrument) and estimating a two-step regression model with clustered OLS standard errrors in each step.

[4] The endogenous switching model is estimated with robust standard errors, clustered at the level of the instrument. The standard errors of the Treatment on the Treated (TT) effect are obtained by cluster-bootstrapping the parameter estimate (with 1000 replications).

[5] ** denotes significance at the 5% confidence level, * denotes significance at the 10% level.

CHAPTER 3: Parental preference for equality in education outcomes

1. Introduction

   While investment in children's education depends on the returns and costs of schooling, it may also depend on parental preferences for resource allocation among their children. For example, Parish and Willis (1993) show that in Taiwan having an older sister is associated with higher educational attainment, especially for older cohorts. Younger children in the Philippines have better educational outcomes compared to their older siblings (Ejrnaes & Portner, 2004). In Indonesia, on the other hand, Pradhan (1998) finds that having younger siblings decreases the probability of delayed enrolment and thus the first-born in the family receive better education. In addition to gender and birth order preferences, parents may also exhibit inequality preferences. Within a resource-constrained household, parents may have equity motives and choose to invest more in the child with lower endowments in order to compensate for the initial differences between siblings, may choose to act based on efficiency considerations and thus reinforce those differences, or may follow a neutral investment strategy (Behrman, Pollak, and Taubman, 1982). Understanding how parents make investment decisions could help identify groups of children that may be at a disadvantage if their parents are income-constrained and may be more vulnerable to income shocks. For example, Behrman (1988) studies the distribution of nutrients in rural India and shows that in surplus seasons, parents favor the child with lower health outcomes. When food is scarce, however, parents favor the male child because of higher marginal returns to nutrient investment in males and because of parental preference for males. Pitt, Rosenzweig, and Hassan (1990) provide similar evidence for Bangladesh, where allocation of calories serves to reinforce health endowments because of

the strong linkage between health and productivity. On the other hand, Ayalew (2005) studies household investment decisions in Ethiopia and finds that investments in health are compensating. He also extends the evidence to education and finds that investments in education are reinforcing. Similarly, Akresh et al. (2012) show that schooling investments are reinforcing in Burkina Faso, and that children of higher cognitive ability are more likely to be enrolled in school and receive more discretionary expenditures.

In this chapter, I use data for Indonesia and provide further evidence for whether parents have efficiency or equity preferences when they invest in the education of their children. I contribute to the literature in three main ways. First, the Indonesian Family Life Survey contains information on mathematics test scores – a direct measure of school performance. School performance is affected by ability as well as noncognitive skills such as motivation and self-efficacy (Rosen et al., 2010). Heckman, Stixrud, and Urzua (2006) show that in the US both cognitive and noncognitive skills have significant positive effects on wages. Several studies in developing countries similarly have shown that test scores are significant determinants of wages while measures of innate intelligence are not (reviewed in Glewwe, 2002). Thus, if parents invest in order to maximize children's future earning potential, then mathematics scores may be more relevant for parental investment decisions than the ability measures used by Akresh et al. (2012) and Ayalew (2005). In addition, the data also contains detailed information on education expenses. Thus, I can study separately spending on school fees and other spending (e.g., on extracurricular activities) and thus better understand parental investment strategies. While Akresh et al. (2012) provide some evidence on investments at the intensive margin, the literature has generally used outcomes such as school enrollment or years of education as proxies for investment (Ayalew, 2005;

Behrman, Rosenzweig, & Taubman, 1994). A couple of recent papers study how educational investments are allocated according to child health endowments (likely correlated with cognitive endowments). The evidence is mixed. For example, Conti et al. (2011) use data on twins in China and show that parents provide fewer educational resources to children who experienced an early life health shock. On the other hand, Leight (2014) finds that Chinese parents allocate more of the discretionary educational spending to the child with lower health endowment, measured by height-for-age and instrumented by early life climatic shock to nutrition.

Second, I provide estimates of the parental preference parameter from estimation of a structural model. One possible approach to estimation would be the use of an instrumental variables model, as in Leight (2014). The lack of a suitable instrument for child school achievement that differs between children within the household renders this approach infeasible. Thus, following the methodology of Behrman (1988) and Rosenzweig and Schultz (1982), I estimate a system of equations comprised of production functions for children's test scores and an investment equation, representing the first-order condition, derived from a utility-maximization problem for a household with two children. More recently, this methodology of joint estimation was used by Bernal and Keane (2010) who apply it to the problem of child achievement and a mother's childcare choices. This approach is more general than a sibling fixed effects estimation, as used in Akresh et al. (2012), which assumes that test scores are not a function of any child-specific unobservables that may be correlated with child-specific shocks to investment. While I provide results from a sibling fixed effects model as a robustness check, I recognize that a

sibling fixed effects model is only valid if any unobserved heterogeneity is at the household level only.

Finally, one of the advantages of using the Indonesian data to study how parents invest in their children is that information on spending and test scores is available in 1997, before the East Asian Financial Crisis hit Indonesia, and in 2000, after the crisis. The effects of both aggregate and idiosyncratic income shocks on child school attendance and enrollment are well documented (Ferreira & Schady, 2009; Jacoby & Skoufias, 1997). Less is known, however, about changes in investments at the intensive margin. In a recent paper on Uganda, Björkman-Nyqvist (2013) uses district-level data to show that income shocks are associated with lower average test scores for girls (but not for boys) when school fees are abolished. Bjorkman-Nyqvist attributes this finding to parents providing fewer resources to girls or requiring more time spent on domestic labor while going to school. In this chapter, I use household-level data and provide some suggestive evidence on how parents may have responded to the economic crisis in Indonesia by changing education investments at the intensive margin.

I find that contrary to previous studies, on average, parental resource allocation between children is not a function of the difference in the test scores of their children. Looking at the effects of child birth order and gender, however, I find that when the younger child has lower test scores, then that child receives lower investment if it is a female. This is consistent with research by Cameron and Worswick (2001) and Thomas et al. (2004) which shows that girl education in Indonesia may be a luxury good. Households with more girls of school age reduce education expenditures when hit by an income shock due to crop loss or a macroeconomic crisis. Thomas et al. (2004) also find that households

tended to protect the education of the older children at the expense of the younger ones. One of the shortcomings of these studies, however, is that their measure of expenditures includes investment at the extensive margin as well, not distinguishing between gender bias in school enrollment and gender bias in school spending. I focus on expenditures on the intensive margin only. Decomposing school spending into monthly fees and other school expenditures, I show that investment differences between siblings are driven by fees. This implies that younger female children may be attending schools of lower quality. Importantly, such differences in investment allocation did not exist prior to the crisis in 1997 which may suggest that one of the mechanisms parents used to cope with the negative income shock was to move young girls to schools of lower quality.

2. Conceptual model

Becker and Tomes (1976) and Behrman, Pollak, and Taubman (1982) were the first to model the important interactions within households comprised of heterogeneous individuals. In the wealth model developed by Becker and Tomes (1976), parents choose whether to invest in children so as to increase their adult earnings potential or whether to provide them transfers when they are adults to compensate for their low earnings. The model assumes that parents are concerned with total child wealth, rather than the sources of wealth. The main conclusion is that parents reinforce endowment differences in children by providing human capital investment for the more endowed child, but equalize wealth by providing more transfers to the less endowed child. An implicit assumption of the wealth model is that parents have enough resources to allocate between children (Behrman, Pollak, & Taubman, 1995). An alternative model was proposed by Behrman, Pollak, and Taubman (1982) who argued that parental preferences are separable in earnings and transfers (SET).

In this setting, parents solve a two-stage problem where they allocate total resources between earnings and transfers in the first stage and then, in the second stage, they allocate earnings investments (and transfers) among their children. This assumption allows for analyzing the distribution of human capital investments independent of any possible future transfers, and has been widely adopted in the subsequent literature.

Following Behrman, Pollak, and Taubman (1982), I assume that the parental utility function is additively separable in consumption (S) and human capital (C). It can therefore be represented as: $V = U^*(S) + U(C)$. As in Behrman, Pollak, and Taubman (1982), I also assume the human capital subutility function $U(C)$ to be of the CES form: $U(C) = \left(\sum_i^n \pi_i C_i^\rho\right)^{1/\rho}$, where parents trade off the human capital of n different children. Parental preferences for one child over another are given by the π_i weights where $\sum_i^n \pi_i = 1$. The elasticity of substitution is a function of the parameter ρ and is defined by $1/(1 - \rho)$. In each time period t, parents choose levels of consumption and investment in the human capital of each child i subject to a budget constraint, that is binding in each period, and a cumulative human capital production technology given initial child endowments (η_i) at time $t = 0$:

$$\max_{S_t, I_{i,t} \dots I_{n,t}} U^*(S_t) + \left(\sum_i^n \pi_i C_{i,t}^\rho\right)^{\frac{1}{\rho}}$$

$$s.t. \sum_i^n I_{i,t} + pS_t = Y_t \ \ and \ \ C_{i,t} = f(I_{i,t}, I_{i,t-1}, \dots I_{i,0}, \eta_i)$$

where Y_t is household income at time t, p is the price of the consumption good, and $I_{i,t}$ represents education investment in child i at time t. If the shadow price of income is

denoted by λ, then the remaining first-order conditions of this problem for a household with two children i and j can be represented as

$$\frac{\partial U(C)}{\partial C_{i,t}} \frac{\partial C_{i,t}}{\partial I_{i,t}} - \lambda = 0, \qquad \frac{\partial U(C)}{\partial C_{j,t}} \frac{\partial C_{j,t}}{\partial I_{j,t}} - \lambda = 0.$$

This implies that parental allocation of resources between child i and child j will be governed by the following relationship[22]:

$$\frac{\partial C_{i,t}}{\partial I_{i,t}} \Big/ \frac{\partial C_{j,t}}{\partial I_{j,t}} = \pi_i C_{i,t}^{\rho-1} \Big/ \pi_j C_{j,t}^{\rho-1}. \tag{1}$$

This model rests on three assumptions. First, parents are assumed to maximize a one-period model. This assumption is made to reflect the fact that formal and informal insurance opportunities during this aggregate shock are limited and budget constraints bind in each period. Second, parents do not trade off investments across time periods. In other words, if parents reduced their investment in the first time period, they do not compensate for it by increasing investment in the next time period. Again, this is plausible in the case when parents are budget-constrained and cannot save or borrow. This seems particularly appropriate because my data (from 2000) are from soon after the Asian Financial Crisis. Third, parents' utility is a function of the human capital accumulated in their children, rather than the value of that human capital (i.e., the potential earnings). This is valid if parents invest in the human capital of their children for altruistic reasons, or if returns to human capital do not vary between children. Either way, accounting for potential gender differences in returns, r_i, to human capital across children (i.e., modelling $r_i * C_i$ as opposed to C_i) does not affect the conclusions of the model. The reason is that under the

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[22] This relationship holds for any two children in a given household. For the main case analysis, however, the sample is restricted to households with only two children of school age who attend school in both 1997 and 2000.

log-linearization used in the estimation, the ratio of returns is absorbed in the intercept of the equation.

For the empirical estimation, I use households with two children (more on sample construction below). A Cobb-Douglas form of the production function is used where current stocks of human capital, measured by mathematics test scores, depend on past stocks (i.e., past test scores) and current investments, measured by parental education spending. This value-added specification relies on past test scores to proxy for past, unobserved, investments and has been widely used in the previous literature (e.g., Todd and Wolpin 2003) (for derivation of the value-added specification, see the Appendix). It also fits the data well. I perform two specification tests to determine whether the value-added functional form of the production functions for child i and child j is supported by the data. First, I test whether past investment belongs in the production function. The F-statistics for the older child ($F(1,439)=5.18$) and the younger child ($F(1,439)=3.99$) imply that I reject the hypothesis that the restricted model fits the data better. As a result, I use the "value-added plus" specification for both production functions. This specification has been found to fit US data better as well (Todd & Wolpin, 2007). Second, using the modified version of the model, I test whether the log-linear specification is appropriate by estimating a translog production function and testing whether the interaction terms are jointly significant. While for the younger child the null hypothesis is rejected (p-value=0.01), for the older child I cannot reject the null hypothesis that the production function is of the Cobb-Douglas form (p-value=0.93). For ease of estimation, I use the log-linearized Cobb-Douglas specification of the production function for both children.

Given the Cobb-Douglas value-added specification of the production function, the log-linearized first-order condition (1) for household h takes the form:

$$logI_{i,t,h} - logI_{j,t,h} = \rho\left(logC_{i,t,h} - logC_{j,t,h}\right) + \alpha_t,$$

where the constant term, α_t, is a function of the parental preference weights π. The coefficient ρ represents substitution between the cognitive stocks of the two children in the parental utility function of human capital. If $\rho \to -\infty$, parents' valuation of child human capital is described by infinite inequality aversion, so that they will value improved cognitive stocks of one child, only if his or her cognitive stocks are lower than the cognitive stocks of the other child. If $\rho \cong 1$, then parental valuation of human capital is governed by efficiency motives and investment reinforces differences in human capital between children. If $\rho = 0$, then equity and efficiency motives are balanced and investments are allocated between children independently of the child's relative test scores.

3. Estimation

One of the challenges in estimating how investment responds to score differences is that education investments and test scores are jointly determined (further details in the Appendix). The simultaneity in investments and skill production may bias the coefficient associated with the differences in test scores. Econometrically, the simultaneity bias can be illustrated in the following stylized version of the model developed above. Scores of child i of household h in period t, $C_{i,t,h}$, are a function of current education investment, $I_{i,t,h}$, child and household characteristics, $X_{i,t,h}$ (such as child age and gender, past test scores, past investment, and mother's education), and an error term, $u_{i,t,h}$ (keeping the intercept implicit). As shown in the conceptual model, current investment levels are a function of the expected test scores, child and household characteristics, $Z_{i,t,h}$ (such as

child gender and years of education, area of residence, household per capita consumption, household size, and mother's education), and an error term, $\epsilon_{i,t,h}$ (again, keeping the intercept implicit):

$$C_{i,t,h} = aI_{i,t,h} + bX_{i,t,h} + u_{i,t,h} \text{ and}$$

$$I_{i,t,h} = gC_{i,t,h} + dZ_{i,t,h} + \epsilon_{i,t,h}.$$

This system of two equations can be re-written in reduced form as:

$$C_{i,t,h} = a\big(gC_{i,t,h} + dZ_{i,t,h} + \epsilon_{i,t,h}\big) + bX_{i,t,h} + u_{i,t,h}$$

$$= \frac{ad}{1-ag}Z_{i,t,h} + \frac{b}{1-ag}X_{i,t,h} + \frac{a}{1-ag}\epsilon_{i,t,h} + \frac{1}{1-ag}u_{i,t,h}.$$

The bias of the coefficient g when estimating the investment equation alone will depend on the correlation between the endogenous variable $C_{i,t,h}$ in that equation and the error term $\epsilon_{i,t,h}$. From the reduced form, it follows that:

$$Cov\big(C_{i,t,h}, \epsilon_{i,t,h}\big) = (a/(1-ag))Var\big(\epsilon_{i,t,h}\big) + (1/(1-ag))Cov\big(\epsilon_{i,t,h}, u_{i,t,h}\big).$$

In the context of this research, the sign of a can be expected to be positive (and less than one given the Cobb-Douglas specification of the production functions). Then, if $g < 0$ or $g > 0$ but $g < 1$, the bias on the estimated coefficient g would be positive (when the covariance between the two error terms is non-negative, as could be expected when there are omitted variables that affect both investment and human capital levels).

Estimating the differenced equation suggested by the first-order condition derived from the conceptual model alleviates the problem of simultaneous determination of investment and stocks. The reason is that while individual test scores are endogenous in a child-specific investment equation, the difference in test scores of child i and child j in household h may not be endogenous in a model for the difference in investments as long

as the error terms $\epsilon_{i,t,h}$ and $\epsilon_{j,t,h}$ contain a household-level error component only (e.g., if any shocks to investment are common to all children in the household). In that case, estimating a sibling fixed effects model of the relationship between investments and stocks would be appropriate, too. In the case when the investment error term is child-specific, however, the difference in test scores remains correlated with the error term for two reasons: 1) the simultaneous production of skills and investment, and 2) non-zero cross-equation correlation of the error terms in the production and investment equations.

The first problem is hard to deal with. In the human capital literature, the use of instrumental variables (IV) or fixed effects (FE) have been the preferred approaches to deal with endogenous inputs. As discussed, FE models rely on strong assumptions about the unobserved factors affecting test scores. A more general method would be the use of an IV model. Rosenzweig and Schultz (1983) are the first to try to account for endogeneity of human capital investments. They estimate a two-stage least squares regression with health input demand as the first stage and health production as the second stage. They use price, income and parental education as the instruments in the first stage, which are assumed to be unrelated to child endowments. While this approach would be suitable in studying differences among households, differences within households could only be identified using child-specific exogenous instruments. In his 1986 paper, Rosenzweig considers both within-household and across-household heterogeneity (Rosenzweig, 1986). He studies the effect of birth spacing on birth outcomes, given the unobserved parental characteristics and the unobserved endowments of children already born. To account for both types of unobserved variables, he proposes using an IV-FE procedure. Since parental characteristics are shared among children, these can be differenced out by sibling FE estimation. The

difference in input levels between two siblings, however, is affected by parental differential response to individual child endowments. As a result, Rosenzweig proposes using the lagged input from the older sibling (before the younger sibling was born) as an instrument for the input difference. Finding an instrument for differences in test scores for school-going children, however, is difficult.

In the absence of a suitable instrument for the difference in test scores between children, I attempt to minimize the potential simultaneity bias by controlling for a variety of covariates. For example, the choice of current education investments may be affected by the gender of the children if parents have higher preference for one child over another. In addition, heterogeneity across households in their budget constraints will matter, too. Therefore, in the equation I estimate, I control for child gender, urban versus rural area of residence, household per capita consumption, pce, household size, $hhsize$, and mother's education, $meduc$. Those should reduce the variance in the error term associated with the empirical approximation of the first-order condition ($\epsilon_i - \epsilon_j$). Similarly, Akresh et al. (2012) lack a good instrument for skills and impose various sample restrictions (such as looking at younger kids only) in order to minimize the potential bias resulting from reverse causality. Any remaining bias on the coefficient associated with the differences in test scores, however, is likely to be positive, as explained above. In his paper on nutrient allocations in India, Behrman (1988) also notes that it is difficult to find a plausible instrument and expects a positive bias in his estimation of parental preference parameters.

Joint estimation of the first-order condition and the production functions in a three-stage least squares framework accounts for the second problem of potential correlation between the equation-specific error terms. The use of the log-linearized Cobb-Douglas

specification of the value-added production functions suggests the estimation of the following system of equations for each household with two children of school age (derivation in the Appendix):

$$logI_{i,1999/2000,h} - logI_{j,1999/2000,h} = \rho \times \left(logC_{i,2000,h} - logC_{j,2000,h}\right) + \mu_{i,2000,h} +$$

$$\mu_{j,2000,h} + d_1 female_{i,h} + d_2 female_{j,h} + d_3 urban_{2000,h} + d_4 pce_{2000,h} +$$

$$d_5 hhsize_{2000,h} + d_6 meduc_{2000,h} + \alpha_{1999/2000,h} + \varsigma_{1999/2000,h} ,$$

$$logC_{i,2000,h} = c_1 logC_{i,1997,h} + a_{11} logI_{i,1999/2000,h} + a_{12} logI_{i,1997/1998,h} + \gamma_{i,2000,h} +$$

$$b_{11} female_{i,h} + b_{12} meduc_{2000,h} + b_{10} + u_{i,2000,h} ,$$

$$logC_{j,2000,h} = c_2 logC_{i,1997,h} + a_{21} logI_{j,1999/2000,h} + a_{22} logI_{j,1997/1998,h} + \gamma_{j,2000,h} +$$

$$b_{21} female_{i,h} + b_{22} meduc_{2000,h} + b_{20} + u_{j,2000,h} ,$$

where μ denotes fixed effects for the level of schooling of each child, included in the investment equation, and γ denotes the age fixed effects, included in the production functions. Test scores are standardized by age, as is common practice in the literature (Paxson and Schady 2007; Todd and Wolpin 2007). Similar to Behrman (1988), I also standardize the investments in order to make both outcomes and inputs comparable in scale and readily derive the parental inequality aversion parameter from the equation representing the first-order condition. Investments are standardized at the school level (variation occurs between school levels rather than between different ages). Since these variables are then logged, as suggested by the conceptual model, they are all standardized around a mean of 100 (with a standard deviation of 1).

4. Sample construction

I restrict the sample used for analysis to households from 1997 who were interviewed in school year 2000/2001 and had at least 2 children of school age (between 7

and 19 years of age) in 1997. I drop households with missing schooling information on any one of their children and households who had no children attending school in both 1997/1998 and 1999/2000. This yields 1,837 households. Households who have at least one child who attended school in 1997 but dropped out by 2000 before completing his or her education are excluded from the main analysis. This exclusion is imposed because of the confounding effect on investment allocation from any selection at the extensive margin. Of the remaining households, I consider households with exactly two children of school age who attend school in both years and have complete information on investments and scores. This results in 440 households. I perform two types of robustness checks to analyze the effect of the sample construction on the results on resource allocation. First, I present findings from analysis that does not restrict the sample to two-children households only (resulting in an expanded sample of 542 households). Second, in the sample of households with two children attending school, I also include households who had other children who dropped out (for a total of 500 households). Then, I provide results on allocation of resources using a selection model.

5. Results and discussion

5.1 Main model

Table 1 presents the results from the estimation of the main model. In column (1), the empirical approximation of the first-order condition derived from the conceptual model is estimated using ordinary least squares regression. The estimate of the parameter of interest, ρ, governing parental substitution between the stocks of different children is positive but close to zero (0.0376). Overall, differences in tests scores between siblings do not have a significant effect on differences in investments (a standard error of 0.0467).

None of the other household-specific factors (per capita expenditure, household size, urban area of residence, mother's education) are significant determinants of investment differences, either. Next, in column (2), I estimate the investment equation jointly with the production function of each child. The results from the 3SLS model yield a coefficient on test scores that is two times larger than the one estimated in a single-equation model (0.0743) although it is still not statistically different from zero (0.0460).

These findings suggest that, on average, parental resource allocation between children is not a function of the difference in the stocks of their children. In other words, parents invest in children by maximizing the total returns of their investment, independent of their distribution between children. This implies that both children would be equally well (or equally poorly) insured against the impact of an income shock; it would not be the case that children with poorer initial outcomes would, on average, suffer more from the crisis than their better performing siblings. This result is in contrast to previous studies in developing countries, which have shown that education spending is determined based on a reinforcing, rather than neutral, investment strategy (Akresh et al., 2012; Ayalew, 2005).

One reason why parents may respond to children's skills in a reinforcing way is if children serve as an insurance mechanism for old age and higher education investments imply higher transfers by children later on (e.g., because of higher income, or because of reciprocity motives). In Indonesia, social norms obligate all children to provide old-age support to parents and even if parents live with one child, the other children should provide monetary assistance (Frankenberg, Lillard, & Willis, 2002). Frankenberg, Lillard, and Willis (2002) show that the educational attainment of children is not a significant predictor of whether parents receive transfers from their adult children. On the other hand, the study

finds that the amount received increases in the children's education. A study by Park (2003), however, finds no systematic relationship between education and transfer amounts, while Raut and Tran (2005) show that the result on the effect of education on transfers is sensitive to the empirical specification. Overall, for the case of Indonesia, no conclusive evidence exists that higher education investments are likely to elicit higher transfers to parents later on. This may be one possible reason why, unlike studies in low-income countries, I find no reinforcing investment motives in Indonesia.

Another potential explanation of my findings is that fertility is endogenous and parents only have as many children as they can afford to educate well, irrespective of their ability. In Indonesia, the National Family Planning Coordinating Board, created in the 70s, promoted small families and, in particular, a two-child norm. Volunteer and village mid-wife services were used to promote and distribute different contraceptives, and those were made available free of charge during the 70s and 80s (Frankenberg, Sikoki, & Suriastini, 2003). As a result, total fertility rates decreased from 5.6 children per woman in late 1960s to 3.4 in 1984-1987, and 2.8 in 1995-1997 (Permana & Westoff, 1999). This suggests that parents had control over their fertility decisions and that smaller families were preferred. Maralani (2008) uses the IFLS data to test the relationship between family size and children's educational attainment for three cohorts: individuals born in 1948–1957, 1958–1967, and 1968–1977. She doesn't find household size to be significantly associated with school attainment in rural areas for any of the cohorts, although she finds a negative relationship for the most recent cohort in urban areas. This suggests that if parents of the children in my sample (born between 1982 and 1990) preferred smaller households, they may not have faced a trade-off between quantity and quality, investing equally in all

children. Any differences in investment allocation patterns should only be visible in urban areas or more resource-constrained households. Below, I test for possible heterogeneity in the parameter of interest.

5.2 Heterogeneity in the parameter of interest

In order to test whether there is heterogeneity in the substitution parameter by various socio-economic characteristics of the household, I include interaction terms between the difference in test scores of the two children and household per capita expenditure, as well as the dummy variable denoting urban area of residence. Poorer households, i.e., those with lower per capita expenditure, could be expected to be more likely to exhibit efficiency investment motives if they are budget constrained and they invest in the smarter child for whom investment may be more productive. Similarly, parents in urban areas are more likely to exhibit efficiency motives in investment if there are different preferences in urban areas or differences in infrastructure, schooling opportunities and labor market returns. For example, parents in urban areas may have more choices for the type of school their child attends and the types of extracurricular activities for the child, and thus may have more opportunities to discriminate between children. The results from the analysis support these two hypotheses. The coefficient on the interaction between test scores with per capita expenditure is negative, suggesting that a decrease in income is associated with an increase in the substitution parameter. In addition, as expected, the coefficient on the interaction with the indicator for urban area of residence is positive. However, these coefficients are statistically insignificant, which may be due to the small sample size.

Next, I examine the role of child gender in parental investment decisions. Unlike many Asian countries, Indonesia shows no male gender bias in parental preferences in birth outcomes or nutrition, and the gender gap in educational attainment has been declining (Kevane & Levine, 2000). Behrman and Deolalikar (1995) show that while wage rates for females are lower than for males, there is no evidence that females face lower rates of return to education. In addition, both females and males are expected to provide old-age transfers to parents and thus parental private expected benefits from investment in one child or another should not vary (Park, 2003). A potential implication of these findings is that there should be no differences in education spending by gender in households where all children attend school. Despite equal returns, however, parents may be more responsive to the level of endowments of one child than another. Further, gender differences may exist in the opportunity cost of time. Pitt and Rosenzweig (1990) show that, at least in 1980, older sisters were more likely to stay home from school and care for sick siblings compared to their teenage brothers.

The results in column (3) of Table 1 show that the gender of the young child, but not the older child, is a significant factor in how parents choose to allocate education resources ($\rho = 0.1598$, significant at the 10% level). Households where the younger child is a female have significantly higher elasticity of substitution between the two children compared to households where the younger child is a male. This could suggest that during an income shock, young female children are more likely to see a reduction in education investments than their older siblings of either gender.

In order to better understand the implication of this finding, it is useful to examine the results for the production function estimation, as well. Past mathematics scores are

significant determinants of current mathematics scores. Consistent with previous studies on dynamic production functions in the US (e.g., Cunha, Heckman, & Schennach, 2010), the skill production function of older children in Indonesia is characterized by a greater degree of state dependence. For older children, a 10% increase in the standardized mathematics score in 1997 is associated with 2.4% higher mathematics score in 2000 compared to a 1.8% increase for younger children. The effect of current education expenditures in older children as well as younger children is not precisely estimated. Lagged investment matters for both. When tested jointly, given the high degree of correlation between the two variables, the two investment variables are significantly different from zero for both children. While lagged investment has a stronger effect for younger children, I cannot reject equality of parameters between the two children. Yet, the results from the production function estimation provide some suggestive evidence that early investments are potentially more productive than later investments, as has been argued in prior literature (Cunha & Heckman, 2006). If that is the case, then the long-term effect of lower investments in the younger girls will be large as they may not be able to make up for the lower investments in their early childhood.

5.3 Reduced-form analysis

In line with prior literature, I also use a sibling fixed-effects regression framework to estimate a reduced-form version of the model for the investment problem only. I test whether the interaction between child gender and child order, as well as child gender, order and math scores is significant. These regressions yield results for how parents respond to a given child's test scores, thus modelling "absolute" differences between children. In addition, as suggested by the structural model, I test whether "relative" differences between

children matter, that is, whether parents punish the younger child for having lower scores than her sibling (as opposed to having low scores in absolute).

The difference in interpretation could be seen in the model specification. When testing "absolute" differences, the equations for each child are formulated as follows:

$$I_{k,h} = gC_{k,h} + dZ_{k,h} + g_1 F_{k,h} \times B_{k,h} \times C_{k,h} + \epsilon_{k,h} \text{ for } k = i, j.$$

where $F_{k,h}$ is an indicator variable for child k in household h being a female, and $B_{k,h}$ is an indicator for child k's birth order such that the interaction term $F_{k,h} \times B_{k,h}$ is always equal to 0 for child k when child k is the older sibling. The first-differenced model would then yield:

$$I_{i,h} - I_{j,h} = g(C_{i,h} - C_{j,h}) + d(Z_{i,h} - Z_{j,h}) + g_1 F_{j,h} \times B_{j,h} \times C_{j,h} + \epsilon_{i,h} - \epsilon_{j,h}.$$

On the other hand, when testing "relative differences", the equations for each child are specified as:

$$I_{k,h} = gC_{k,h} + dZ_{k,h} + g_2 D_{j,h} \times C_{k,h} + \epsilon_{k,h} \text{ for } k = i, j ,$$

where $D_{j,h}$ is an indicator variable, defined at the household level, for the younger sibling, sibling j, being a female. The first-differenced model would then yield:

$$I_{i,h} - I_{j,h} = g(C_{i,h} - C_{j,h}) + d(Z_{i,h} - Z_{j,h}) + g_2 D_{j,h} \times (C_{i,h} - C_{j,h}) + \epsilon_{i,h} - \epsilon_{j,h},$$

which corresponds to the specification of the structural model estimated earlier.

Table 2 presents the results of this analysis. First, using total expenditures, I show that the results from this analysis are consistent with the previous findings and that the results are explained by relative differences between siblings, rather than absolute skill levels. Being either a female or the younger child in the household is not a significant determinant of differences in investments (column (1)). Younger children who are females do not receive significantly lower levels of investments either, whether their school

performance is taken into consideration or not (column (2)). However, younger females who have lower age-adjusted test scores than their older siblings of either gender experience lower levels of total education investment (column (3)). The coefficient on the interaction term between the younger child in the household being a female and test scores in column (3) is 0.1813, significant at the 5% level. This reduced-form analysis confirms the previous findings that if younger female children have lower test scores, then they would receive lower education investment compared to their older sibling of either gender.

5.4 Differences in type of investment

In order to better understand the source of the differences in investments, I decompose the investment variable into fees (comprised of monthly fees, registration fees, and exam fees) and other education expenses. Using the sibling fixed-effects model, I find no significant differences in expenses for school supplies, special courses, transportation and pocket money (grouped under the heading "other") (Table 2, columns (7), (8), (9)). The main source for the differences in education spending between siblings appears to be monthly fees (Table 2, columns (4), (5) and (6)). This suggests that young female children with lower skills compared to their older siblings are likely to attend schools with lower fees. If higher fees are an indication of better school inputs and are associated with better school performance (as suggested by Suryadarma et al., 2006), this may imply that parents enroll young female children in schools of worse quality.[23]

In order to test whether school choice was potentially affected by the crisis I repeat the analysis presented in Table 2 using 1997 data on school spending and mathematics scores. Unlike the results for 2000, I find no significant differences in education spending

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[23] The data does not allow testing this directly as I cannot identify the specific school attended by the child.

in 1997 between siblings. This finding is consistent with parents being forced by the income shock to move some children (in particular, lower-performing younger girls) to schools of lower quality. Another possible explanation may have to do with schools reducing their fees shortly after the crisis if lower-grade schools (attended by the younger children) reduced fees more than upper-grade schools. While this is plausible, the finding that the gender of the younger child matters cannot be explained away by this alternative explanation. This study cannot separately identify whether the crisis changed preferences, or existing preferences became more important under limited resources, but it suggests that not all children were equally well insured from the crisis since after the crisis, parents were more sensitive to the human capital of younger girls.

6. Robustness Checks

6.1 Households with more than two children and no dropouts

In order to examine the sensitivity of the results to the sample construction, I perform a robustness analysis using all households who had two or more children attending school in both 1997 and 2000 and no children who dropped out during that time period. This adds 102 more households to the sample, 90 of which have 3 children, $11 - 4$ children, and $1 - 5$ children. The results from the sibling fixed effects estimation confirm the previous findings: while gender, sibling order and absolute skill levels do not matter, the relative performance of the youngest child matters when that child is a female. The results are presented in Table 3. The estimate of the parameter of interest is statistically significant, although lower than the main case estimate – 0.1349 vs. 0.1598. Having a lower estimate when including multiple-children households is expected. When there are more opportunities to spread resources between children (or more ways to decrease overall

resources), fewer resources need to be diverted from one specific child (e.g., the youngest one).

6.2 Selection model for households who have two children attending school

Next, I perform a robustness analysis that uses all households who have two children currently attending school, including households where one or more children dropped out between the two survey waves in 1997 and 2000. This robustness check accounts for the fact that households with no dropouts are a selected sample. For this analysis, I need an instrument that exogenously affects the probability that a household will have a child who drops out during the study period. I create a measure of this household-level probability based on the 1998 non-enrollment rates by child age and quartile of household per capita expenditure as reported in Thomas et al. (2004). Thus, this measure depends on both household size (number of children) and household socio-economic well-being. Using this instrument, I perform a Heckman two-step estimation. The first-stage selection equation is estimated using all households who have two children currently attending school. It is a function of all the variables included in the main model in addition to the measure of household-level propensity to pull a child out of school. Based on the estimates from this model, the inverse mills ratio can be calculated. This selection term is then included in the second-stage equation, which is estimated for the original set of households with two children who have had no children dropping out during the crisis. Table 4 presents the results of this robustness analysis.

The exogenous instrument is a good predictor of whether the household has a child who drops out: the higher the household-level probability of dropout, the lower the likelihood of being selected into the main sample for analysis on households with no

dropouts. Controlling for the selection effects, the results on resource allocation within the household confirm the previous findings: differences in test scores between children are not associated with differences in investments unless the younger child is a female. The estimate of the parameter of parental preference for equality is 0.1973 for the case when the younger child is a female, which is even larger than the size of the parameter in the main analysis – 0.1598.

The finding from this robustness check is consistent with the case when some households who may struggle with schooling costs substitute between children at the extensive margin and then have equal preference for those who remain in school. Other households who struggle but choose to keep their children in school (the main sample used in this chapter) may need to substitute between children at the intensive margin, reducing investments in some children more than in others. That is why when controlling for selection into keeping all children in school, the inequality aversion parameter is larger than before.

7. Conclusion

This chapter examines the role of parental preferences in human capital accumulation. Using data from Indonesia soon after the economic crisis in late 1990s, I examine whether parents allocate education resources within the household based on efficiency or equity motives. I show that, on average, parental investment in children does not reinforce differences in skills. However, parents are more sensitive to the human capital of younger female children, penalizing them for having lower skills compared to their older siblings of either gender. Decomposing investment into monthly fees and other school expenditures shows that investment differences between siblings are driven by fees and

implies that younger female children may be attending schools of lower quality after the crisis. No differences in investment by child gender or order exist before the crisis, which suggests that maybe young female children were less fully insured from the negative effects of this income shock compared to their older siblings of either gender. Thus, while I cannot identify the causal effect of the crisis on changes in household resource allocation, by examining behavior prior to the crisis and after the crisis, I provide some suggestive evidence on household coping strategies with the income shock.

Tables

Table 1: Resource allocation between children - Main analysis

	Estimated Model		
	(1)	(2)	(3)
(Expenses$_{1,2000}$-Expenses$_{2,2000)}$			
Math$_{1,2000}$ -Math$_{2,2000}$	0.0376	0.0743	-0.0521
	(0.0467)	(0.0460)	(0.0809)
Older child is a female	0.0005	0.0004	0.0004
	(0.0009)	(0.0009)	(0.0009)
Younger child is a female	-0.0006	-0.0002	-0.0003
	(0.0009)	(0.0009)	(0.0009)
Older child is female*(Math$_{1,2000}$ -Math$_{2,2000}$)			0.1107
			(0.0928)
Younger child is female*(Math$_{1,2000}$ -Math$_{2,2000}$)			0.1598*
			(0.0928)
Urban area of residence	-0.0015	-0.0015	-0.0014
	(0.0010)	(0.0010)	(0.0010)
Log per capita expenditures in 2000	-0.0002	-0.0002	-0.0002
	(0.0009)	(0.0008)	(0.0008)
Household size	-0.0004	-0.0004	-0.0004
	(0.0003)	(0.0003)	(0.0003)
Mother years of education	0.0002	0.0002	0.0002
	(0.0001)	(0.0001)	(0.0001)
Older child math scores in 2000, Math$_{1,2000}$			
Math$_{1,1997}$		0.2368**	0.2371**
		(0.0441)	(0.0441)
Expenses$_{1,2000}$		0.0767	0.0758
		(0.0467)	(0.0467)
Expenses$_{1,1997}$		0.0798*	0.0800*
		(0.0468)	(0.0468)
Female		0.0014*	0.0014*
		(0.0008)	(0.0008)
Mother years of education		0.0005**	0.0005**
		(0.0001)	(0.0001)
Younger child math scores in 2000, Math$_{2,2000}$			
Math$_{2,1997}$		0.1788**	0.1788**
		(0.0425)	(0.0425)
Expenses$_{2,2000}$		0.0318	0.0326
		(0.0429)	(0.0429)
Expenses$_{2,1997}$		0.0890**	0.0888**
		(0.0404)	(0.0404)
Female		0.0001	0.0001
		(0.0008)	(0.0008)
Mother years of education		0.0005**	0.0005**
		(0.0001)	(0.0001)
Households	440	440	440

Notes:

[1] The investment equation includes school level fixed effects. The production functions include age fixed effects. All equations also include a constant term.

[2] All variables are standardized around mean of 100 with a standard deviation of 1, and then logged. Math scores are standardized by age, investment is standardized by school level.

[3] Standard errors in parentheses; * significant at the 10% level, ** significant at the 5% level

Table 2: Fixed effects model for resource allocation between children in 2000 - by type of investment

	Estimated Model								
	total educational expenses 2000			monthly fees 2000			other educational expenses 2000		
	Basic model	Absolute	Relative	Basic model	Absolute	Relative	Basic mode	Absolute	Relative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age	0.00001	-0.00002	-0.00003	0.00008	0.00003	0.00002	-0.00041	-0.0004	-0.00042
	(0.00042)	(0.00042)	(0.00042)	(0.00045)	(0.00045)	(0.00045)	(0.00041)	(0.00041)	(0.00041)
Female	0.00009	0.00034	0.00033	-0.00043	-0.00095	-0.00095	0.00002	0	0.00003
	(0.00068)	(0.00097)	(0.00096)	(0.00072)	(0.00102)	(0.00101)	(0.00066)	(0.00093)	(0.00093)
Sibling order	-0.00131	-0.00114	-0.00121	-0.0008	-0.00138	-0.00146	-0.00159	-0.0016	-0.00162
	(0.00108)	(0.00126)	(0.00126)	(0.00114)	(0.00133)	(0.00133)	(0.00104)	(0.00121)	(0.00121)
Math score in 2000	0.03755	0.02123	-0.04623	0.08128	0.06298	-0.02385	0.04178	0.04477	0.01909
	(0.04845)	(0.05120)	(0.06562)	(0.05125)	(0.05407)	(0.06889)	(0.04680)	(0.04956)	(0.06391)
Female*Sibling order		-0.35977	-0.00023		-0.42851	0.00135		0.06599	0.00004
		(0.36709)	(0.00134)		(0.38802)	(0.00141)		(0.35386)	(0.00130)
Female*Sibling order*Math score		0.078			0.09325			-0.01432	
		(0.07966)			(0.08420)			(0.07679)	
Younger child in the household is a female*Math score			0.18128*			0.23279**			0.04886
			(0.09628)			(0.10178)			(0.09339)
Years of education in 2000	-0.00067	-0.00065	-0.00063	-0.00064	-0.00058	-0.00056	-0.0002	-0.00021	-0.00019
	(0.00041)	(0.00042)	(0.00041)	(0.00044)	(0.00044)	(0.00044)	(0.00040)	(0.00040)	(0.00040)
Constant	4.43695**	4.51223**	4.41872**	4.23451**	4.31937**	4.20219**	4.41979**	4.40595**	4.41516**
	(0.22314)	(0.23587)	(0.22302)	(0.23607)	(0.24909)	(0.23536)	(0.21556)	(0.22836)	(0.21627)
N of children			880			875			878
N of households			440			440			440

Notes:

[1] All variables are standardized around mean of 100 with a standard deviation of 1, and then logged. Math scores are standardized by age, investment is standardized by school level (primary, junior high, senior high).

[2] Standard errors in parentheses.

[3] * significant at the 10% level, ** significant at the 5% level

Table 3: Robustness check - Fixed effects model for resource allocation between two or more children in 2000

	Estimated Model	
	total educational expenses 2000	
	Absolute	Relative
Age	0.00008	0.00007
	(0.00033)	(0.00033)
Female	-0.00024	-0.00025
	(0.00075)	(0.00075)
Youngest child	-0.00045	-0.00047
	(0.00098)	(0.00098)
Math score in 2000	0.0172	-0.0303
	(0.03982)	(0.05153)
Female*Youngest child	-0.38573	0.00084
	(0.30996)	(0.00111)
Female*Youngest child*Math score	0.08389	
	(0.06728)	
Youngest child in the household is a female*Math score		0.13494*
		(0.07517)
Years of education in 2000	-0.00034	-0.00033
	(0.00033)	(0.00033)
Constant	4.52671**	4.45153**
	(0.18363)	(0.17446)
N of children	1199	1199
N of households	542	542

Notes:

[1] All variables are standardized around mean of 100 with a standard deviation of 1, and then logged. Math scores are standardized by age, investment is standardized by school level (primary, junior high, senior high).

[2] Standard errors in parentheses.

[3] * significant at the 10% level, ** significant at the 5% level

Table 4: Robustness check - Selection model for resource allocation between two children in 2000

	(1)		(2)	
	Selection Model	Main model	Selection Model	Main model
Probability of having a child dropout	-6.4836**		-6.5752**	
(at the household level)	(0.8085)		(0.8276)	
$Math_{1,2000} - Math_{2,2000}$	-8.2337	0.0511	4.5954	-0.1022
	(8.8462)	(0.0475)	(15.5332)	(0.0840)
Older child is female*$(Math_{1,2000} - Math_{2,2000})$			-8.6768	0.129
			(18.1310)	(0.0960)
Younger child is female*$(Math_{1,2000} - Math_{2,2000})$			-24.4313	0.1973**
			(18.9286)	(0.0968)
Older child is female	-0.0711	0.0004	-0.0861	0.0003
	(0.1908)	(0.0009)	(0.1938)	(0.0009)
Younger child is female	0.2698	-0.0007	0.2966	-0.0009
	(0.1941)	(0.0010)	(0.2000)	(0.0010)
Urban area of residence	-0.2635	-0.0016	-0.2469	-0.0015
	(0.2110)	(0.0010)	(0.2123)	(0.0010)
Log per capita expenditures in 2000	0.0953	-0.0004	0.1196	-0.0004
	(0.1743)	(0.0009)	(0.1774)	(0.0009)
Household size	-0.2408**	0	-0.2510**	0
	(0.0562)	(0.0004)	(0.0577)	(0.0004)
Mother years of education	0.0306	0.0002	0.0269	0.0002
	(0.0303)	(0.0001)	(0.0307)	(0.0001)
Older child is in junior high school	0.4701*	-0.0037**	0.4667*	-0.0037**
	(0.2790)	(0.0017)	(0.2813)	(0.0017)
Older child is in senior high school	1.2735**	-0.0080**	1.2777**	-0.0080**
	(0.3417)	(0.0019)	(0.3433)	(0.0019)
Younger child is in junior high school	0.2731	0.0057**	0.3026	0.0056**
	(0.2409)	(0.0011)	(0.2445)	(0.0011)
Younger child is in senior high school	0.1499	0.0096**	0.1828	0.0096**
	(0.4431)	(0.0025)	(0.4479)	(0.0025)
Constant	1.9747	0.0084	1.7768	0.0085
	(2.1386)	(0.0108)	(2.1681)	(0.0108)
Inverse mills ratio		-0.0082**		-0.0084**
		(0.0030)		(0.0029)
Number of households	500		500	

Notes:

[1] Of the 500 households, 440 have no children who dropped out, while 60 have at least one child who dropped out.

[2] All variables are standardized around mean of 100 with a standard deviation of 1, and then logged. Math scores are standardized by age, investment is standardized by school level.

[3] Standard errors in parentheses; * significant at the 10% level, ** significant at the 5% level

Appendix

A quasi-Cobb-Douglas specification of the production function is assumed:

$$C_{i,t} = E_t * \eta_i \prod_{k=1}^{t} I_{i,k}^{\alpha_{t-k}} v_{i,k}.$$

Where logarithms are implicit, the linearly additive specification is:

$$C_{i,t} = \alpha_0 I_{i,t} + \phi_0 v_{i,t} + \alpha_1 I_{i,t-1} + \phi_1 v_{i,t-1} + \cdots + \alpha_{t-1} I_{i,1} + \phi_{t-1} v_{i,1} + \beta_t \eta_i + \kappa_t E_t$$

$$+ \epsilon_{i,t}, (A1)$$

where $\epsilon_{i,t}$ is the measurement error associated with the cognitive outcome at time t. In this model, the expected impact of investment at the beginning of time t on an outcome at the end of time t is given by the parameter α_0, and the impact of an investment at the beginning of time $t-1$ is given by the parameter α_1. As shown in Todd and Wolpin (2003), the function for the outcome in time $t-1$ can be written similarly as:

$$C_{i,t-1} = \alpha_0 I_{i,t-1} + \phi_0 v_{i,t-1} + \alpha_1 I_{i,t-2} + \phi_1 v_{i,t-2} + \cdots + \alpha_{t-2} I_{i,1} + \phi_{t-2} v_{i,1} + \beta_{t-1} \eta_i$$

$$+ \kappa_{t-1} E_{t-1} + \epsilon_{i,t-1}.$$

Therefore, subtracting $\delta C_{i,t-1}$ from both sides of equation (A1) for some constant δ obtains:

$$C_{i,t} - \delta C_{i,t-1} = \alpha_0 I_{i,t} + \phi_0 v_{i,t} + (\alpha_1 - \delta\alpha_0)I_{i,t-1} + (\phi_1 - \delta\phi_0)v_{i,t-1} + \cdots + (\alpha_{t-1}$$

$$- \delta\alpha_{t-2})I_{i,1} + (\phi_{t-1} - \delta\phi_{t-2})v_{i,1} + (\beta_t - \delta\beta_{t-1})\eta_i + (\kappa_t E_t$$

$$- \delta\kappa_{t-1}E_{t-1}) + (\epsilon_{i,t} - \delta\epsilon_{i,t-1}) . \qquad (A2)$$

Under the assumption that the impacts of each input, shock, and the initial endowment deteriorate over time at the geometric rate of δ so that $\alpha_k = \delta\alpha_{k-1}$, $\phi_k = \delta\phi_{k-1}$, and $\beta_k = \delta\beta_{k-1}$, the linearized production function, where logarithms are implicit, can be presented in a value-added form as follows:

$$C_{i,t} = \delta C_{i,t-1} + \alpha_0 I_{i,t} + \phi_0 v_{i,t} + (\kappa_t E_t - \delta \kappa_{t-1} E_{t-1}) + (\epsilon_{i,t} - \delta \epsilon_{i,t-1}), \qquad (A3)$$

where the term $(\kappa_t E_t - \delta \kappa_{t-1} E_{t-1})$ is a constant.

If the $v_{i,t}$ shocks, which are assumed to be identically and independently distributed, are realized after the investment has been made so that the investment decision is not affected by the shock realization, then $Cov(I_{i,t}, v_{i,t}) = 0$. The unbiased estimation of equation (A3) also requires that the measurement errors in the two time periods are correlated with correlation equal to δ so that $Cov(C_{i,t-1}, \epsilon_{i,t} - \delta \epsilon_{i,t-1}) = 0$ (Todd & Wolpin 2003, 2007). While harder to justify, this assumption is common in the human capital literature. Since the coefficient on past test scores is not the main parameter of interest, I will take this assumption as given.

In the empirical model, I use data on test scores in 1997 and 2000. The value-added specification of the production function then takes the form:

$$C_{i,2000} = \delta^2 C_{i,1997} + \alpha_0 I_{i,1999/2000} + \delta \alpha_0 I_{i,1998/1999}$$

$$+ (\phi_0 v_{i,1999/2000} + \delta \phi_0 v_{i,1998/1999}) + constant_{2000} + (\epsilon_{i,1999/2000}$$

$$- \delta^2 \epsilon_{i,1997/1998}).$$

Thus, in addition to the assumptions described earlier that allow unbiased estimation of the model parameters, I also need to account for the fact that households are not surveyed every year. Assuming investments are lumpy and made annually, the above formulation suggests that investments in academic year 1998/1999 belong in the model. I consider estimation of this equation when data for $I_{i,1998/1999}$ are missing. First, if the investments in academic years 1999/2000 and 1998/1999 are equal, then estimation does not suffer from omitted variable bias. The coefficient associated with current investment will represent the sum of current and lagged investment. At the other extreme, if

$Cov(I_{i,1998/1999}, I_{i,1999/2000}) = 0$, omitted variable bias is also not present. While this assumption may seem implausible, it is not unlikely because the economic crisis of 1998 affected both the cost of schooling and parental ability to pay. Assuming that the correlation between investments in 1999/2000 and 1998/1999 is small, the omitted variable bias due to missing information on 1998/1999 investments will be small. Alternatively, investments in the academic year 1997/1998 can be used to proxy for investments in 1998/1999 since both academic years were affected by the crisis of 1998 (even though investment information reported in the 1997 survey is likely affected much less by the crisis or by expectations of the crisis). I test whether past investment belongs in the production function. The F-statistics for the older child (F(1,439)=5.18) and the younger child (F(1,439)=3.99) imply that I cannot reject the hypothesis that the unrestricted model fits the data better. Therefore, the final model also includes past investment in the production functions. In addition, I include mother's education to account for other unobserved investments at the household level. An additional concern is that while $(I_{i,1999/2000}, v_{i,1999/2000}) = 0$, $Cov(I_{i,1998/1999}, v_{i,1998/1999}) = 0$ and $Cov(I_{i,1998/1999}, v_{i,1999/2000}) = 0$, parents may respond to past shocks to human capital so that $Cov(I_{i,1999/2000}, v_{i,1998/1999}) \neq 0$. In other words, current investment is likely to be endogenous in a model of human capital production.

While the main goal of the chapter is not to estimate a production function, but rather provide insights on investment allocation, this model is useful in that it explicitly shows how investment and human capital are interlinked. Investments affect human capital and expectations of the cumulative human capital affect investments.

CHAPTER 4: Occupation choice after the crisis

1. Introduction

Intergenerational transmission of income in developing countries is often high. Even as educational attainment has been increasing over time, breaking the cycle of poverty has been difficult. One of the reasons for that could be that poor households are often vulnerable and exposed to various shocks. Shocks may be associated with reduced willingness to take risks when choosing an occupation. In this chapter, I examine determinants of occupation choice of Indonesian males after the East Asian Financial Crisis and provide some indirect tests of whether the economy-wide shock increased people's preference for an occupation with lower earnings volatility. This research question is related to three interconnected strands of research: occupation choice, household consumption and income smoothing, and social learning and expectations about labor market returns.

Labor markets in developing countries are often characterized with strong intergenerational transmission of occupations. Emran and Shilpi (2011) study occupation choice in rural Nepal and Vietnam, identifying the separate effects of intergenerational transmission of ability, of tangible assets such as education and wealth and of intangible assets such as social networks. They find that the intergenerational occupation linkages between fathers and sons could largely be explained by genetic endowments in both Nepal and Vietnam. In Nepal, mother-daughter linkages are also affected by shared intangible assets. Similarly, Magruder (2010) shows that in South Africa, when a father's industry grows by 10 percent, the son is 3-4 percent more likely to work in that industry. In this case, he attributes the intergenerational transmission of occupations not to accumulated skills or inherited human capital but to the importance of parents for social network

connections. The lack of occupation and social mobility across generations makes breaking the cycle of poverty hard. For example, even with globalization, increased labor demand and higher education spending, Krishna (2013) shows that in urban India, sons tend to follow their fathers and uncles in informal and low-skilled occupations.

Intergenerational transmission of income and labor market outcomes is also affected by the inherent riskiness of the environment. There is a large literature on consumption smoothing during periods of negative income shocks in developing countries. Households may sell assets, reduce investments, or borrow in response to an income shock (Frankenberg, Smith, & Thomas, 2003; Rosenzweig & Wolpin, 1993; Townsend, 1994). When households lack access to credit and insurance or have lumpy assets they may also take actions to smooth income variability prior to the realization of any shocks (Morduch, 1995). For example, Rosenzweig and Binswanger (1993) study farmers' agricultural investments and find that farmers in riskier environments choose investment portfolios that mitigate risk, which results in lower incomes because of the observed trade-off between profit variability and average profits. In addition, households may choose to diversify their income sources. Shenoy (2014) studies how Thai farmers respond to volatility in international rice prices and shows that higher volatility is associated with less specialization. At the same time, he finds that diversification is inefficient and decreases household revenue. Similarly negative effects of diversification on household welfare are found in Bangladesh. There, Bandyopadhyay and Skoufias (2013) show that if the household head is in agriculture, then household members are less likely to also be employed in the agricultural sector if they live in areas with more rainfall uncertainty compared to people in areas with less variability in rainfall.

Given the uncertainty in livelihoods, people keep updating their expectations and behavior based on their experiences and newly acquired information. In Indonesia, Cameron and Shah (2012) study the effect of natural disasters on risk perceptions and risk taking and show that people who experienced a recent shock were more risk averse and less likely to take risks in terms of technology adoption, starting up a business or changing jobs. Malmendier and Nagel (2011) show that individuals who experienced low stock market returns during their lives had lower willingness to take financial risk, especially if the negative stock market shocks were more recent and the people affected were younger. In addition, Tokuoka (2013) shows that people learn from the experience of their family. Thus, a household's saving rate increases when a sibling becomes unemployed. Similarly, Diagne (2008) studies whether people's expectations are mostly explained by their own characteristics and the objective average labor market conditions, or by the labor market experience of their siblings. She finds that young people have limited labor market information and place disproportionate weight on social learning. On the other hand, Osman (2014) experimentally provides occupation information to high school students in Egypt, correcting prior held beliefs about average earnings and volatility. He finds that they respond to objective information and that risk-averse students choose occupations with lower earnings variability once they graduate from high school.

My research examines the connection between occupation choice and earnings volatility in a developing country setting. Unlike most studies in developing countries, I focus on males living in urban areas where the occupational choice set is different/larger. In addition, I expand on previous research on the effects of long-run risk by studying occupation response to volatility associated with a short-run aggregate shock. Since young

people have little prior information about earnings, I expect that they will be most sensitive to earnings volatility. In particular, I test whether young people whose fathers engaged in occupations with a high volatility in income after the crisis were less willing to take a riskier occupation.

I find ambiguous results. If anything, the analysis shows that willingness to take risk in occupation choice increased after the crisis, which is contrary to the theoretical predictions. I also find little evidence of a trade-off between volatility and average earnings. This would suggest that the extra risk taken would not be necessarily associated with higher earnings and thus may have low (if any) welfare benefits. Overall, the Indonesian data does not appear to be suitable for this analysis and the results presented below should be interpreted with caution. Nevertheless, this study serves to describe the issues related to occupation choice and willingness to take risk and outline a research plan for future work. Next, I present a brief conceptual model that guides the estimation, a description of the sample and volatility definition, and finally, a discussion of the results and a conclusion.

2. Conceptual framework

I develop a simple conceptual model in order to illustrate the factors that affect occupational choices. Child i in household h chooses an occupation to maximize his utility function, $U[Y_{ij}|\Omega]$. Utility is defined over earnings in job j and is conditional on the child's information and opportunity set, Ω. Earnings are a function of the average income in a given job ($\overline{Y}_j$), child ability (α_i^o), and unexpected job-specific shocks (η_j). Child ability is distributed normally with a mean of 0 and a variance of σ_0^2. Shocks to income have a normal distribution with a mean of 0 and a variance of σ_j^2. The child's total income, $Y_{i,j} =$

$\overline{Y}_j + \alpha_i^o - \eta_j$, is distributed normally with a mean $\overline{Y}_j$ and a variance $s_j^2 = \sigma_0^2 + \sigma_j^2$. A

second-order Taylor-series approximation around the expected level of income, $\overline{Y}_j$, yields:

$$U[Y_{ij}|\Omega] = U[\overline{Y}_j|\Omega] + U'[\overline{Y}_j|\Omega](\alpha_i^o - \eta_j) + \frac{1}{2}U''[\overline{Y}_j|\Omega\,](\alpha_i^o - \eta_j)^2$$

Taking expectations:

$$EU[Y_{ij}|\Omega] = EU[\overline{Y}_j|\Omega] + \frac{1}{2}EU''[\overline{Y}_j|\Omega]s_j^2$$

$$= EU[\overline{Y}_j|\Omega] - \frac{1}{2}EU'[\overline{Y}_j|\Omega] \times \left(-\frac{EU''[\overline{Y}_j|\Omega]}{EU'[\overline{Y}_j|\Omega]}\right) \times s_j^2$$

$$= EU[\overline{Y}_j|\Omega] - \frac{1}{2}\frac{EU'[\overline{Y}_j|\Omega]}{\overline{Y}_j} \times \left(-\overline{Y}_j\frac{EU''[\overline{Y}_j|\Omega]}{EU'[\overline{Y}_j|\Omega]}\right) \times s_j^2$$

$$= EU[\overline{Y}_j|\Omega] - \frac{1}{2}\frac{EU'[\overline{Y}_j|\Omega]}{\overline{Y}_j} \times \gamma \times s_j^2$$

where γ is the coefficient of relative risk aversion. This expression shows that, for a risk-averse agent, expected utility is decreasing in the variance of the labor market outcomes and increasing in expected returns.

Empirically, I assume an additive random utility model of occupation choice and use a conditional logit regression model, where individuals choose between six occupations to maximize their utility:

$$Occupation_{i,j} = \beta_1 \overline{Y}_j + \beta_2 \sigma_j + Z_i \delta_1 + \epsilon_{i,j} \ . \tag{1}$$

The error term has a type 1 extreme value distribution. The vector Z is a vector of variables which determine the information and opportunity set of the child, Ω, such as child age and education. If the utility function is concave, then mean earnings and volatility should enter

the equation in log form.[24] I expect to find a significant and positive sign for the coefficient associated with average earnings, β_1. If people are unable to ensure perfectly against income volatility, then variance, σ_j, should also be a significant determinant of occupation choice. I expect a negative sign since, for a given income level, risk-averse agents would prefer lower volatility.

A modified model also includes an interaction term between the job variance for a given occupation and the variance in the parent's occupation in order to test whether parental income volatility after the crisis affected willingness to take risks when making occupation choices:

$$Occupation_{i,j} = b_1 \overline{Y_j} + b_2 \sigma_j + b_3 (\sigma_j \times \sigma_{i,p}) + Z_i d_1 + d_2 \sigma_{i,p} + e_{i,j} \ , \qquad (2)$$

If individuals react only according to objective labor market conditions, earnings volatility in the father's occupation should not have an additional effect on the probability of choosing a given occupation. If, however, risk-averse individuals learn socially and respond to personal experiences, then an increase in earnings volatility in the father's occupation should be associated with higher likelihood of choosing a safer occupation (either because of the information shock and changing expectations or because of diversification motives).

This conceptual framework suggests that the error terms contain unobserved individual ability. Yet average income and volatility in different occupations are plausibly exogenous and the estimates of their effects should not be subject to omitted variable bias. The model also shows the need to account for risk preferences. Unfortunately, the data does not contain information on risk preferences for the year 2000. Thus, individual risk

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[24] The results are qualitatively similar if both variables are entered linearly and the signs of the effects don't change.

preferences are also a part of the error term. Again, they should not be correlated with the aggregate measures of earnings and volatility. Thus, the effects identified in the empirical approximation of the model should be consistent estimates of the average effect across people with heterogeneous risk preferences.

The main concern in the empirical identification of the model is that occupation choice also depends on other job characteristics that may determine job satisfaction and that are correlated with volatility and earnings. If volatility during the crisis is a short-run and unexpected characteristic of a given job, then it will likely not be correlated with other factors in a systematic way. Similarly, earnings volatility in the father's occupation would be endogenous in a model of sons' occupation choice because of the likely intergenerational correlation of risk preferences and ability. Again, the randomness of the crisis, where some of the previously safest occupations experienced the largest earnings shocks, alleviate this problem.

Still, considering these caveats of the analysis, this chapter does not claim to show causal estimates but merely associations. In order to identify the causal effects of the crisis, a difference-in-difference analysis could be performed using regional variation in changes in labor demand to estimate the impact of rising volatility on willingness to take risk. This type of analysis, however, is not well suited to an occupation choice model. Instead, in this chapter, I estimate the effect of volatility on occupation choice prior to the crisis and then again after the crisis. Thus, even if showing associations only, the comparison between the two models provides some suggestive evidence about the possible effects of the crisis.

3. Sample construction

For the main analysis, I use information on males only because the vast majority of age-eligible males are employed in all waves of the survey. Thus, my sample is less likely

114

to be subject to bias related to selection into working (especially if less risk-averse individuals were more likely to work). I also restrict the sample to males between the ages of 25 and 45 in the year 2000 in order to include only individuals who have completed their education and whose fathers are likely to be of working age prior to the crisis (information used in the second part of the analysis). I use information on individuals interviewed as part of the household survey as well as members of the household who live away. The survey collects information on the household head's children living away, including their gender, age, years of education, employment status and occupation. Similarly, it also collects data on parents of the household head living away. This information reduces any potential sample selection bias which may occur if living away from your parents was associated with more willingness to take risks. Excluding those individuals from the analysis could have resulted in biased estimates of the importance of earnings volatility. Finally, I restrict the sample to males living in urban areas in 1997 in order to ensure the same/large choice set since choice of occupations in rural areas may be limited.[25] Defining this restriction based on 1997 rather than 2000 residence accounts for the fact that there may be selective migration after the crisis and thus the sample does not exclude people who moved away from urban areas by 2000 (and does not include those who only moved to an urban area in 2000).

4. Occupation categories and definition of volatility

I calculate earnings volatility using annual data on individual monthly earnings from 1995 to 2000 (provided retrospectively in the different survey waves) for all

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[25] In addition, there is strong intergenerational transmission of occupations in Indonesia, especially for rural households. Appendix Table 1 presents the proportion of individuals who have the same occupation in 2000 as their father did prior to the crisis in 1997. The descriptive statistics show that in rural areas, more than 43% of males whose fathers were agricultural workers in 1997 also engaged in agriculture in 2000.

employed males of working age (19 to 65) living in urban areas. I adjust nominal earnings reported in the survey using province-specific consumer price indices from 1995 to 2000. For the main analysis, I use a nonparametric measure of total earnings variability based on Cappellari and Jenkins (2014).

First, I calculate the person-specific, occupation-specific volatility in earnings:

$$I_i = \sqrt{Variance\left[100 * \frac{E_{it} - E_{it-1}}{\frac{E_{it} + E_{it-1}}{2}}\right]}$$

where E_{it} are the real occupation-specific earnings for person i in year t.

Next, I calculate pre-crisis occupation-specific, province-specific income volatility as the volatility, averaged across all individuals and across all time periods from 1993 to 1997. Similarly, post-crisis occupation-specific, province-specific income volatility is calculated as the volatility, averaged across all individuals and across all time periods from 1998 to 2000.

The IFLS distinguishes between 10 different occupation categories: two categories for professional/technical workers, a category each for administrative and managerial workers, clerical workers, sales workers, service workers, and agricultural laborers, and three separate categories for production workers. I find no significant differences in mean earnings or volatility between the two categories for professional/technical workers and combine them into one category. Similarly, no significant differences exist between the three categories for production workers. Of the resulting seven categories, administrative/managerial workers comprise less than one percent and are excluded from the main model estimation (Table 1).

Table 2 presents summary statistics for earnings volatility measures averaged across 12 different provinces. It shows that occupation-specific volatility increased after the crisis. Nationwide, sales workers had the highest earnings volatility after the crisis while agricultural workers had the lowest pre-crisis earnings volatility. The correlation between occupation-specific, province-specific earnings volatility before and after the crisis is low (0.08), which supports the hypothesis that the income shock was largely unpredictable.

Figure 1 shows a weak positive relationship between average earnings and volatility in earnings for both the pre-crisis period and post-crisis period. There is some evidence for a trade-off between higher earnings and higher variance in earnings: a one percent increase in average earnings was associated with a 0.016 percent higher variance prior to the crisis and 0.027 percent higher variance after the crisis but the effects are not significantly different from zero.

5. Results and Discussion

5.1 Descriptive statistics

Table 1 presents population-level descriptive statistics. About 90 percent of all males aged 25 to 45 are employed in both 1997 and 2000 and the difference in employment rates for this population is not statistically significantly different from zero at the 5% level.[26] There are, however, important differences in the proportion of people engaged in different occupations in 1997 and 2000. For example, the overall proportion of people engaged in service work increased from 7.2 to 13.7%, while the proportion of sales workers decreased from 15.5 to 12.3%. While these population numbers may be partly due to cohort

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[26] The numbers are even higher if looking at the population of individuals present in both survey waves and reporting a non-missing working status: about 93% both nationwide and in urban areas only.

differences between 1997 and 2000 (e.g., if young people who enter the job market in 2000 are significantly different from those who enter the job market in 1997), some of the differences are likely due to behavior that responds to changing conditions during the crisis. For example, among the people who report an occupation in both 1997 and 2000 (88% or 2,882 out of 3,260 present in both waves), 41% report switching jobs.

These descriptive results suggest that important changes in occupation choices occurred between 1997 and 2000. In order to test if they are potentially due to lower willingness to take risk after the crisis, I next examine whether (a change in) volatility was a determinant of occupation choices in the year 2000. However, some of the behavior observed in 2000 could also be explained by demand-side factors as the economy was undergoing structural changes. As a robustness check, I control for a measure of labor demand – the widely used Bartik index – calculated as the product of the national occupation growth rate between 1997 and 2000 and the 1997 province-specific employment rate in a given occupation (Bartik, 1991; Blanchard & Katz, 1992; Schaller, 2012).

5.2 Determinants of occupation choice

5.2.1 Results using a conditional logit model

Table 3 presents the results for the empirical approximation of the main model in equation (1). The model is estimated using a conditional logit regression and table 3 presents the coefficient estimates for the alternative-specific regressors and the associated p-values. Since the coefficients don't have an easy interpretation (other than showing the direction of the effects), I also calculate odds ratios (OR). The interpretation of an odds ratio greater than 1 is that an increase in the variance of a given occupation is associated

with larger probability of choosing that occupation and a smaller probability of choosing another occupation (and vice versa).

The base case model (2) studies the full sample of working male individuals in 2000 and shows that volatility but not earnings are significant determinants of occupation choice. Yet, the sign associated with volatility is opposite to what theory predicts for risk-averse individuals: conditional on average earnings, if volatility increases for one occupation, then this occupation is chosen more.

Interestingly, the model for 1997 doesn't show the same patterns. In 1997, as expected, higher volatility in a job is associated with lower likelihood of choosing that job and higher earnings – with a higher likelihood. Both of these factors, however, are not statistically significant determinants of occupation choice in 1997. One explanation for these results is that prior to the crisis individuals did not make occupation decisions based on the long-run volatility in earnings and they were perfectly insured against this risk. Yet, after the crisis, they preferred riskier, not less risky occupations.

If occupation choices are made prior to the crisis, then volatility between 1998 and 2000 may not be a good predictor of occupation choice in 2000. Restricting the sample to individuals who started their current jobs recently (i.e., job tenure of less than 3 years) in model (3), however, shows that neither average earnings nor volatility are significant factors in occupation choice. Controlling for changes in labor demand during the post-crisis period in model (4) does not change these conclusions. However, the coefficient on the Bartik index is not significant, which may suggest that it doesn't capture labor demand well, and so the rest of the analysis proceeds without this labor demand control and with

the caveat that changes in the underlying structure of the economy may be affecting the results.

Overall, I cannot find strong evidence that sons of households engaged in occupations that experienced large shocks to income have a decreased willingness to take risks when choosing an occupation after the crisis. This finding could be explained in several ways. First, people may only respond to long-run volatility trends. With only 3 years of post-crisis data, the volatility measure may be subject to substantial measurement error, leading to biased estimates of the effect of current volatility on current occupation choice. Yet, occupation choice of people who started their current job after the start of the crisis is not a function of prior occupation volatility and income and neither is occupation choice of people prior to the crisis affected by these long-term trends.

Second, job choice during the period may not be governed by the utility maximization process presented in the conceptual model and the empirical modelling may not be correct. Indeed, using the Hausman test to test the independence of irrelevant alternatives assumption (IIA) embedded in the conditional logit regression model, I reject the null hypothesis of no systematic differences in the coefficients of the full choice model and the coefficients of a model excluding any one occupation. Next, I present the results from a nested logit model, which relaxes the IIA assumption and models the choice between agricultural and non-agricultural occupations separately.

5.2.2 Results using a nested logit model

As shown in the descriptive statistics, a large proportion of individuals with fathers who work in the agricultural sector also work in agriculture. Agriculture employment may depend on factors such as land and legacy. A nested logit model may thus better model the decision-making process, where the first branch of individual's decision tree can be

120

represented as a choice between agricultural and non-agricultural employment. Non-agricultural employment further branches out to professional/technical, clerical, sales, service, and production work. Table 4 presents the results from the nested logit model. In all model specifications, volatility is again associated with a large positive coefficient, significantly different from zero. Average earnings are not significant determinants of occupation choice.

Next, I test whether earnings volatility in the parental occupation affected willingness to take risks when making occupation choices by including an interaction term between volatility and volatility in the parental occupation, as in equation (2), using a nested logit model.

5.2.3 Heterogeneity in occupation choice

This analysis is performed for the subset of individuals with working fathers and non-missing information on their father's 1997 occupation. Unfortunately, more than half of all individuals in the sample lack information on parental work status and about half of those who report that their father is employed do not provide information on occupation type. It is unclear if there is any reporting bias (and if so, the direction which it would take) or if the variables are missing at random. Either way, the results should be taken with caution due to the small sample size.

Table 5 presents the results of this analysis. Since I estimate a nonlinear model, the interaction term is difficult to interpret and neither the size nor the sign of the coefficient are meaningful. Instead, I calculate the average marginal effect of a change in earnings volatility in the father's occupation manually using the calculus method (Cameron & Trivedi, 2010). To do so, I estimate the model with the interaction term as an additional alternative-specific regressor (and father's occupation volatility as an additional case-

specific regressor) and calculate the predicted probabilities of occupation choice. Then, I increase the value of the volatility in the father's occupation by one one-hundredth of its standard deviation, re-compute the interaction term and re-estimate the model. I define the marginal effect as the difference in predicted probabilities in both cases, divided by the change in the volatility variable.

The results suggest that prior to the crisis, an increase in earnings volatility in the father's occupation did not have a significant effect on individual probability of choosing a given occupation. The only exception is the decreased probability of choosing work as a production worker. Indeed, prior to the crisis, production work was characterized with high volatility (ranked number 2, following agriculture work, as shown in Table 2). In the year 2000, higher volatility in earnings in the father's occupation was associated with higher probability of choosing agricultural work and lower probability of production work. The results are hard to interpret. After the crisis hit, production work remained one of the occupations with highest volatility, which would provide support to the hypothesis that males whose fathers were engaged in occupations that experienced large volatility in the post-crisis period were less willing to take risk. However, the fact that the probability of choosing to work in agriculture, the very volatile occupation after the crisis, increased contradicts this main hypothesis. The results are equally ambiguous if the sample is restricted only to sons who started their current job after the crisis hit (model (3)).

In order to better understand the results on parental occupation volatility, I perform several sensitivity checks. An individual could be more sensitive to earnings volatility because of the volatility in his father's occupation if he has a diversification motive or if the father's experience serves as an information shock. Diversification motives should be

stronger for individuals who still live with their parents as the household may be pooling their resources. Alternatively, individuals who live at home may be more likely to start helping in the family business if the father faces high earnings volatility. On the other hand, those who don't live with their parents likely act in response to the information shock only. Thus, in Table 5, model (4), I restrict the data to individuals who don't live with their parents anymore and I find that the results are similarly ambiguous as the main case in model (2).

Next, if information shocks matter, then the son's age during the economic crisis should matter, too. Younger individuals should have accumulated less outside information and thus be more affected by an information shock from the father's experience during the crisis. Thus, I expect that if information matters, parental occupation volatility would be a stronger predictor of their occupation choice. The analysis in models (5) and (6) don't provide clear evidence for the direction of the effects on willingness to take risk.

6. Conclusion

Occupation choices in developing countries have been shown to be affected by household ability to deal with shocks and information about labor market outcomes, among other factors. In this chapter, I attempt to provide evidence for the association between occupation choice and short-run earnings volatility for urban males in Indonesia between the ages of 25 and 45. The results, however, are ambiguous. If anything, the analysis shows that willingness to take risk in occupation choice increased after the crisis, which is contrary to the theoretical predictions. Even so, the lack of strong trade-off between volatility and average earnings would suggest that the extra risk taken would not be necessarily associated with higher earnings and thus may have low (if any) welfare benefits.

The results may be explained by people not having perfect information about mean earnings and volatility or by making job choices based on a different utility maximization model. In addition, there could be other job-specific variables that affect occupation choice and are systematically correlated with average earnings and volatility after the crisis. While the short-run volatility after the crisis is different from the long-run volatility trend prior to the crisis, this issue could still be a potential concern, especially if I fail to account for changes in labor demand appropriately. Finally, the sample construction aimed to minimize any selection and attrition bias. One of the downsides of including individuals who don't live in the household as part of the analysis, however, is that there is no information on type of employment (e.g., private vs. self-employed vs. government), hours worked, or having a secondary job. It may be the case that changes in volatility do not affect occupation choice but rather affect these three other dimensions of labor market participation.

Thus, better data would allow answering the question of whether occupation choice and willingness to take risk is affected by short-term shocks to income more conclusively. In addition, the research could be extended to test whose labor market experience actually matters – the father's or maybe an older brother's. Responding to shocks to a father's occupation is more likely to involve diversification motives. Importantly, the role of insurance capacity could be directly studied as people who are better able to insure against shocks (e.g., because they receive transfers from others or have more assets to sell) should be less sensitive to volatility in earnings and thus more willing to take risks. In addition, identifying the extent to which individuals have good information about different occupations is important. Studying differences in how people respond to general shocks to

occupations vs. individual-specific shock that a household member experiences would help design effective policies. If an individual is more likely to respond to personal experience, then an information intervention would be useful. If, however, an individual responds to aggregate occupation changes, other measures, such as increasing insurance capacity, would be more effective in overcoming risk avoidance.

Figures

Figure 1: Relationship between province an occupation-specific volatility and average earnings in the pre-crisis and post-crisis period
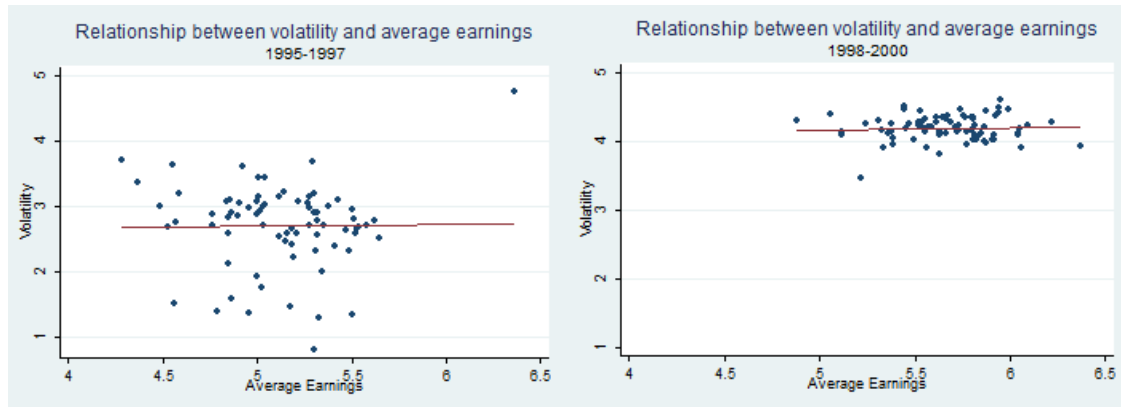
Tables

Table 1: Proportion of males between the ages of 25 and 45 engaged in each of 10 occupations by year

	Overall			Urban areas		
	1997	2000	p-value	1997	2000	p-value
Employed	92.2	91.3	0.092	91.5	90.4	0.087
Among the employed:						
Occupation: Professional/Technical workers	6.7	6.8	0.831	8.1	8.0	0.878
Occupation: Administrative/managerial workers	0.5	1.3	<0.001	0.7	1.5	0.019
Occupation: Clerical and related workers	7.5	6.6	0.082	10.6	8.2	0.008
Occupation: Sales workers	15.5	12.3	<0.001	19.9	15.7	<0.001
Occupation: Service workers	7.2	13.7	<0.001	7.5	16.8	<0.001
Occupation: Agricultural workers	28.1	27.3	0.395	10.0	11.0	0.298
Occupation: Production workers	34.5	32.1	0.012	43.1	38.8	0.004

Notes:

[1] Occupations are defined based on the IFLS data.

Table 2: Average occupation-specific variance in earnings

	1995 - 1997			1998 - 2000		
	Variance Rank	Average log variance	Standard deviation in log variance	Variance Rank	Average log variance	Standard deviation in log variance
Occupation: Professional/Technical workers	4	2.52	0.63	5	4.07	0.13
Occupation: Clerical and related workers	5	2.47	0.39	2	4.25	0.15
Occupation: Sales workers	3	2.87	0.35	1	4.26	0.16
Occupation: Service workers	6	2.21	0.79	4	4.11	0.14
Occupation: Agricultural workers	1	3.19	0.79	2	4.25	0.28
Occupation: Production workers	2	2.89	0.25	3	4.23	0.10

Table 3: Occupation choice and volatility: conditional logit model

	Full sample, 1997			Full sample, 2000			Sample with job tenure <3 years, 2000			Sample with job tenure <3 years, 2000		
	(1)			(2)			(3)			(4)		
	beta	OR	p-value	beta	OR	p-value	beta	OR	p-value	beta	OR	p-value
Log variance	-0.048	0.953	0.566	0.587	1.799	0.011	0.350	1.419	0.313	0.332	1.394	0.345
Log mean earnings	0.051	1.052	0.754	0.188	1.207	0.305	-0.024	0.976	0.931	-0.022	0.978	0.937
Bartik index										1.072	2.921	0.737
Individual-specific controls	Yes			Yes			Yes			Yes		
Number of individuals	1723			2298			1072			1072		

Notes:

[1] The results are based on an alternative-specific conditional logit model. The individual-specific controls included in the model are age and years of education.

[2] Variance and mean earnings are defined at the occupation-province level. The regression model for 1997 uses mean real earnings from 1995 to 1997 and volatility estimates are based on data from 1995 to 1997. The regression models for 2000 use mean real earnings for the period from 1998 to 2000 and volatility estimates are based on data from 1998 to 2000.

[3] The odds ratios (OR) are calculated as the exponentiated coefficients (beta). An odds ratio greater than 1 is interpreted to mean that increase in the variance of a given occupation is associated with larger probability of choosing that occupation, and vice versa.

Table 4: Occupation choice and volatility: nested logit model

	Full sample, 1997			Full sample, 2000			Sample with job tenure <3 years, 2000			Sample with job tenure <3 years, 2000		
	(1)			(2)			(3)			(4)		
	beta	OR	p-value	beta	OR	p-value	beta	OR	p-value	beta	OR	p-value
Log variance	0.554	1.740	0.001	0.933	2.542	0.013	1.480	4.393	0.054	1.452	4.272	0.033
Log mean earnings	-0.155	0.856	0.488	0.361	1.435	0.185	-0.259	0.772	0.602	-0.760	0.468	0.164
Bartik index										-29.403	0.000	0.028
Individual-specific controls	Yes			Yes			Yes			Yes		
Number of individuals	1723			2298			1072			1072		

Notes:

[1] The results are based on an alternative-specific nested logit model, where the first nest represents choice between agricultural and non-agricultural employment. The individual-specific controls included in the model are age and years of education.

[2] Variance and mean earnings are defined at the occupation-province level. The regression model for 1997 uses mean real earnings from 1995 to 1997 and volatility estimates are based on data from 1995 to 1997. The regression models for 2000 use mean real earnings for the period from 1998 to 2000 and volatility estimates are based on data from 1998 to 2000.

[3] The odds ratios (OR) are calculated as the exponentiated coefficients (beta). An odds ratio greater than 1 is interpreted to mean that increase in the variance of a given occupation is associated with larger probability of choosing that occupation, and vice versa.

Table 5: Model with interaction between own and father's volatility: nested logit model

	Full sample, 1997			Full sample, 2000			Sample with job tenure <3 years, 2000			Sample of nonresident sons, 2000			Sample of sons aged 25-34 , 2000			Sample of sons aged 35-45, 2000		
	(1)			(2)			(3)			(4)			(5)			(6)		
	Mean	Sd	t-stat	Mean	Sd	t-stat	Mean	Sd	t-stat	Mean	Sd	t-stat	Mean	Sd	t-stat	Mean	Sd	t-stat
Occupation: Professional/Technical workers	0.011	0.014	0.786	-0.095	0.097	-0.979	0.112	0.092	1.217	-0.037	0.080	-0.463	0.162	0.111	1.459	-0.298	0.304	-0.980
Occupation: Clerical and related workers	0.023	0.014	1.643	0.202	0.135	1.496	-0.071	0.041	-1.732	0.201	0.118	1.703	0.278	0.167	1.665	0.145	0.170	0.853
Occupation: Sales workers	-0.010	0.019	-0.526	-0.095	0.056	-1.696	0.016	0.042	0.381	-0.057	0.072	-0.792	-0.030	0.061	-0.492	-0.151	0.119	-1.269
Occupation: Service workers	0.053	0.038	1.395	-0.032	0.059	-0.542	-0.158	0.071	-2.225	0.068	0.067	1.015	-0.372	0.201	-1.851	0.218	0.148	1.473
Occupation: Agricultural workers	0.018	0.031	0.581	0.221	0.101	2.188	0.319	0.140	2.279	0.154	0.058	2.655	0.335	0.198	1.692	0.196	0.090	2.178
Occupation: Production workers	-0.094	0.036	-2.611	-0.201	0.095	-2.116	-0.218	0.105	-2.076	-0.329	0.098	-3.357	-0.374	0.178	-2.101	-0.111	0.232	-0.478
Number of individuals		411			423			208			353			200			223	

Notes:

[1] The results are based on an alternative-specific nested logit model, where the first nest represents choice between agricultural and non-agricultural employment. The individual-specific controls included in the model are age, years of education, and post-crisis volatility of father's 1997 occupation.

[2] Variance and mean earnings are defined at the occupation-province level. The regression model for 1997 uses mean real earnings from 1995 to 1997 and volatility estimates are based on data from 1995 to 1997. The regression models for 2000 use mean real earnings for the period from 1998 to 2000 and volatility estimates are based on data from 1998 to 2000.

Appendix Tables

Appendix Table 1: Intergenerational transmission of occupations

Urban households

Child Occupation in 2000	Father Occupation in 1997						
	1	2	3	4	5	6	7
Occupation 1 : Professional/Technical workers	11.4	2.3	9.1	22.7	22.7	13.6	18.2
Occupation 2: Clerical and related workers	0	0	0	55.6	0	11.1	33.3
Occupation 3: Administrative/managerial worker	15.6	2.2	11.1	24.4	13.3	11.1	22.2
Occupation 4: Sales workers	1.8	1.8	18.2	21.8	30.9	18.2	7.3
Occupation 5: Service workers	7.5	0	17	20.8	17	9.4	28.3
Occupation 6: Agricultural workers	3.8	0	5.7	9.4	39.6	24.5	17
Occupation 7: Production workers	7.3	1.7	12.9	13.5	27.5	13.5	23.6

Rural households

Child Occupation in 2000	Father Occupation in 1997						
	1	2	3	4	5	6	7
Occupation 1 : Professional/Technical workers	8.8	0	2.9	14.7	29.4	38.2	5.9
Occupation 2: Clerical and related workers	16.7	0	0	0	0	83.3	0
Occupation 3: Administrative/managerial worker	4.9	2.4	7.3	9.8	12.2	46.3	17.1
Occupation 4: Sales workers	0	2	16	8	26	36	12
Occupation 5: Service workers	3.3	1.1	0	5.5	11	71.4	7.7
Occupation 6: Agricultural workers	2	0	3.5	3	42.8	43.3	5.5
Occupation 7: Production workers	2.7	1.4	6.8	6.2	26.7	42.5	13.7

CHAPTER 5: Lessons learned

Indonesia has made significant progress in improving access to schooling for its children, reaching nearly universal primary school enrollment for both boys and girls and steadily increasing secondary school enrollment rates over time. The East Asian Financial Crisis of late 1990s briefly halted this progress as parents were unable to pay the school fees or demanded from their children more time spent working inside or outside the house. While school enrollments recovered soon after the crisis, the children who were affected by the short-run budget constraints likely faced long-term consequences. In this dissertation, I study two main questions in order to understand the crisis implications for children. First, I examine whether only children with low expected returns from education selected out of schooling during the crisis or whether children who had the potential to do well in school dropped out as well. Second, I explore how parents allocate education resources between their children at the intensive margin and thus study whether parents are likely to invest more in the better-performing child.

My research on school dropout supports the previous literature that has described the importance of short-run constraints for school enrollments in Indonesia. For example, Frankenberg, Thomas, and Beegle (1999) show that school dropout rates in 1998 increased significantly, especially for children in the lowest income quartile, young children (7 to 12) in rural areas and older children in urban areas. My research expands on this literature by exploring to what extent low skills were a determinant of school dropout and identifying the impact of school dropout on mathematics and general cognition test scores about three years after the crisis began.

I find that children who drop out of school during the crisis come from families with lower per capita expenditures. They drop out at every age and school level, although the highest proportion of dropouts leave school as they transition between primary and junior high school or junior high school and senior high school. This is consistent with binding budget constraints as school fees are substantially higher in high school than primary school. It could also be evidence for lower ability as children need to pass exams in order to go to the next level of schooling. That dropouts select out of school based on their ability is shown by the fact that the 1997 (i.e., prior to dropping out) distribution of mathematics scores for dropouts is shifted to the left of that of the comparison group.

I use a value-added model, controlling for mathematics test scores in 1997, to estimate the effect of school dropout on test outcomes in 2000. I find significant negative effects of missing schooling on both school-acquired and general cognitive skills, as measured by mathematics test scores and Raven's Progressive matrices assessment scores, respectively. The OLS estimates suggest that missing an average of three years of schooling is associated with 0.46 standard deviations lower mathematics scores and 0.35 standard deviations lower Raven's scores.

Past test scores, however, may not fully account for all unobservables that drive selection into schooling. A more general approach to estimating the effect of dropout while dealing with endogeneity concerns is to use an instrumental variables (IV) model. Prior to the crisis, there was a long-run declining trend in non-enrollment rates at each age for both girls and boys. During the crisis this trend was reversed. Using data from the national socio-economic survey (Susenas) for 1993 to 1998, I calculate linear predictions of 1998 non-enrollment rates at the age-gender level. Then, I use differences between the predicted and

observed non-enrollment rates as the source of crisis-induced identifying variation. This group-level instrument varies at the age-gender level and is a good predictor of individual decision to drop out, which is not correlated with ability or other individual unobservables. For mathematics scores, I find IV estimates that are 1.6 times larger. These results are consistent with children primarily dropping out because of short-run binding resource constraints during the Indonesian crisis and suggest that parents pulled children out of school even if they had the potential to do well in school.

Interestingly, children do seem to sort out of school based on expected gains in their general cognitive skills. Estimating an endogenous switching regression model, I find evidence for comparative advantage where children who drop out during the crisis expect to lose less in terms of general cognitive skill accumulation compared to a random individual made to drop out. The effect of treatment on the treated for Raven's scores is thus lower than the OLS results (-0.26 standard deviations) but still significantly different from zero. Yet, it is reduced by about 40% and loses statistical significance after controlling for child work status. This result suggests that children of lower cognition and lower potential to improve their cognition at school are more likely to drop out of school and start working, whether they have lower mathematics learning abilities or not. This finding may be due to parents making decisions about the schooling of their children based on observation of children's general cognition, rather than their school performance.

Next, I study how parents invest in their children when their children stay in school. Parents may reinforce skill differences between siblings, investing in the child that is more likely to benefit from further resources, compensate for initial differences, investing in the less capable child so he can catch up, or have a neutral investment strategy, not governed

by skill differences. Using a structural model, I estimate the parental preference for equality parameter implied by the constant elasticity of substitution utility function. I estimate a system of equations comprised of production functions for children's test scores and an investment equation, representing the first-order condition, derived from a utility-maximization problem for a household with two children.

I find that, on average, parental education spending is not a function of children's test scores. However, parents seem to be more sensitive to the human capital of younger female children and would penalize them for having lower scores compared to their older siblings of either gender. This result holds using a sibling fixed effects estimation as well. The fixed effects model shows no differences in investment by child gender and order before the crisis. This study cannot separately identify whether the crisis changed preferences or existing preferences became more important under limited resources, but it suggests that not all children were equally well insured from the crisis since after the crisis, parents were more sensitive to the human capital of younger girls.

In order to better understand the source of the differences in investments, I decompose the investment variable into fees and other education expenses. Using the sibling fixed-effects model, I find that the main source for the differences in education spending between siblings appears to be monthly fees. There is evidence that higher fees are an indication of better school inputs. My results would therefore be consistent with young female children attending schools of worse quality.

My findings on resource allocation between children support previous work by Cameron and Worswick (2001) and Thomas et al. (2004) which has shown that girl education in Indonesia may be a luxury good. Cameron and Worswick (2001) find that

after crop loss, households with girls (but not boys) of school age reduce education expenditures. Thomas et al. (2004) study household expenditures before and after the Asian Financial crisis and find that households with more boys experienced lower reductions in the education budget shares compared to those with more girls. They also find that households tended to protect the education of the older children at the expense of the younger ones. Previous studies considered investment at the intensive and extensive margin, not distinguishing between gender bias in school enrollment and gender bias in school spending. I separate the two effects by studying only investments at the intensive margin for children who continue to go to school. In addition, I directly incorporate children's test scores in the parental investment decision problem and show whether parents respond to differences in children's school performance in a reinforcing, compensatory or neutral manner.

Schooling in Indonesia has significant returns. One year of extra education is associated with an increase in hourly wages of 6.5% to 10.8% (Duflo, 2001). In addition, the literature on labor markets in the US as well as in developing countries (Glewwe, 2002; Hanushek & Woessmann, 2008) suggests that skills, especially school-acquired skills, have independent effects on labor outcomes. Therefore, much can be gained by insuring children against income volatility in order to prevent school interruptions when children who would have stayed in school in the absence of an income shock might otherwise be forced to drop out. Girls, in particular, may be more vulnerable to resource constraints as parents reduce their investments in them and potentially provide them with worse quality of education. Thus, targeted government programs may be effective in protecting them against these short-run shocks that have long-run consequences.

Chapters 2 and 3 studied schooling decisions during a period of resource constraints. These chapters argued that underinvestment in human capital may lead to suboptimal labor market outcomes in the long run. However, the crisis may also affect labor market choices and outcomes directly in the short-run. In chapter 4, I examine determinants of occupation choice of Indonesian males after the East Asian Financial Crisis and provide some indirect tests of whether the economy-wide shock increased people's preference for an occupation with lower earnings volatility. If shocks are associated with reduced willingness to take risks when choosing an occupation, then this may help explain the high intergenerational transmission of income in developing countries, even as education rises, and the difficulties of breaking the cycle of poverty.

Yet, the results show that willingness to take risk in occupation choice increased after the crisis, which is contrary to the theoretical predictions. I also find little evidence of a trade-off between volatility and average earnings. This would suggest that the extra risk taken would not be necessarily associated with higher earnings and thus may have low (if any) welfare benefits. Overall, however, the Indonesian data does not appear to be suitable for this analysis. Better data would allow answering the question of whether occupation choice and willingness to take risk is affected by short-term shocks to income more conclusively. This remains an area of future research.

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