The Short End of the Deal: Crisis, Dropout, and Sibling Inequality

Tamara J. McGavock*

Department of Economics, Grinnell College

June 2016

Abstract

I examine whether adult siblings are unequally educated as a result of differential exposure to shocks during childhood. Specifically, I consider adult siblings in the 2007 wave of the Indonesian Family Life Survey (IFLS) who were children during the 1998 Indonesian financial crisis. I find evidence, consistent with previous findings by Son (2013), that Indonesian families preserved the schooling of children who were set to finish the final critical year of a schooling level during the financial crisis. Expanding on that evidence, I find that the siblings of those children experienced short-term drop out during the crisis but that after the crisis had passed they are able to catch up to their siblings. Nine years afterward, those siblings are equally educated, but the siblings who experienced short-term drop-out are more likely to still be in school relative to their siblings whose schooling was not interrupted during the crisis nine years prior.

*E-mail: *mcgavock@grinnell.edu*. All errors are my own. Please do not cite without permission. I am grateful for financial support from the Center for the Study of Inequality at Cornell University. I thank Fran Blau, Jim Berry, Kaushik Basu, and Victoria Prowse, as well as seminar participants at Cornell, for helpful comments. Thanks also to Merna Aboulemathy for excellent research assistance.

"Differences among siblings in socioeconomic outcomes are an important element in the structure of overall inequality."

-Dahan & Gaviria (2003)

1 Introduction

Temporary school interruption is a common experience of many students in the developing world. During household, local, or national crises of various kinds, students may withdraw from school because of affordability concerns or changes to their opportunity costs of time. There is a significant literature on the effect of interruption on outcomes for students, but little attention has been paid to the long-term outcomes for siblings relative to one another when dropout may occur only for one sibling. This paper investigates these long-term outcomes using the panel Indonesian Family Life Survey (IFLS). I identify differential investments made during the 1997 financial crisis in children from the same family, which created inequality between siblings that would not otherwise have existed, and track siblings into adulthood seven years later.

Thomas et al. (2004) and Thomas & Frankenberg (2007) find that the effects of the 1998 Indonesian financial crisis were felt strongly by households as it reduced purchasing power by as much as 40% in the short run. They show that households responded to the crisis in the short run by drawing down their assets, by increasing female labor supply (particularly in self-employment and family-run businesses, and by reducing household investments in education, reducing expenditures on education for some children (the intensive margin) and even withdrawing some children from school (the extensive margin). I draw on this work and show that educational attainment among some children was lower relative to that of their siblings as a result of experience of the crisis. The context of the Indonesian financial crisis and the Indonesian Family Life Survey (IFLS), a large panel dataset which follows individuals after they leave households, provides the best existing framework in which to study the question of sibling inequality in the long run: the panel allows me to track siblings even after they leave their parents' home.

In my individual-level empirical analysis, I find evidence, consistent with Son (2013), that children who were scheduled to complete their final year of a schooling level (whether primary, junior high, or secondary school) during the 1998 crisis completed **more** schooling in the short run (as measured 3 years later in 2000) relative to children who were not in a final year of schooling. I expand on that evidence to show that such a decision rule *increases* short-term inequality between siblings in educational attainment. These differences among siblings persist

even when controlling for age, child birth order, and gender. In 2007, ten years after the crisis, the *siblings* of those children whose schooling was preserved in 1998 to finish the final, critical year of a schooling level have *caught up* to them but are also *more likely to still be in school*.

I proceed by describing the context of Indonesia's educational system and the specific context of the 1998 Indonesian Financial Crisis; I then establish the context of this paper in the existing economics literature on unequal investments in siblings. In Section 3 I develop a theoretical model of differential education investments in the face of budget shocks and non-convexities in the returns to education. Section 4 introduces the data I use and lays out my empirical strategy to econometrically identify the impact of the experience of the financial crisis on intra-sibling inequality in educational attainment. I then present my empirical results in Section 5 and conclude.

2 Economic and Institutional Context

2.1 Education in Indonesia

Education in Indonesia consists of three levels before age 18: primary (grades 1 to 6), junior high (grades 7 to 9), and secondary (grades 10 to 12), and students in their final year of secondary school are typically 17 or 18 years old. The Indonesian government made nine years of school compulsory in 1994, although enrollment information from household surveys (including those used in this paper) reveals that the compulsory schooling levels were neither universally enforced nor universally achieved in the mid-2000s.[1] The school year begins in July, with a semester break in December, and the second semester spans January to June. For the purpose of this paper, then, the Indonesian financial crisis struck in the middle of the 1997/8 school year.

School fees and other costs in Indonesia can be a large portion of household expenditures, especially for urban primary students, even in publicly funded schools. Additionally, significant social pressure exists to conform to standards of dress for uniforms, such that being able to afford school fees may still be insufficient motivation for children to stay in school at all types of schools. Transportation costs (and a bump in them associated with the move to junior school or to secondary school from the previous level) may provide further deterrence for school attendance and in particular may pose additional costs to transferring to the next level; indeed, more than 80 percent of schools (of all levels) are in buildings on their own.

[1] The government just made twelve years of schooling compulsory as of 2013. See http://www.thejakartapost.com/news/2013/06/26/ri-kicks-12-year-compulsory-education-program.html.

Students receive certificates of completion upon finishing the final year of each schooling level after taking school-administered examinations. At the end of grades nine (the end of junior high school) and twelve (the end of secondary school), the test results are indicated on the certificates of completion. Certificates are issued by individual schools and additional certificates are issued for individuals who pass the national examinations (based on a minimum score). Advancement to the next level of education requires passing these examinations and they are thus very important landmarks of accreditation.

Despite relatively high enrollment and completion rates relative to those of other developing nations, the quality of schooling in Indonesia is widely regarded as quite low, especially for primary school; UNICEF reported that only 27 percent of teachers were qualified in 2012. Teacher salaries are frequently reported to be insufficient to cover the needs of teachers and their families.[2] Corruption is also commonly cited as a significant problem in Indonesian schooling.

2.2 The 1998 Financial Crisis in Indonesia

The Indonesian financial crisis began in January 1998 with a sudden and unanticipated depreciation in the Rupiah following six months of strain on the financial sector.[3] The macroeconomic effect of this was to reduce output by about 12% in 1998 relative to its 1997 level; it remained there well into 1999. The Rupiah remained unstable and depreciated well into the 2000s. Thomas et al. (2004) note that the collapse of the currency caused prices to rise, such that the CPI increased by about 80%; food prices and particularly rice prices rose by significantly more than that (20% and 50% more, respectively). The result was to temporarily but drastically affect the purchasing power of households that purchase food. In particular, poor non-rice producers (or net consumers) particularly in urban areas saw their real incomes plummet.[4] On the other hand, as has been noted by the literature investigating the short term effects of the crisis, rural rice producing households were relatively sheltered due to the appreciation of rice prices and some even benefited from them. As I move forward with the analysis, then, I focus on non-agricultural households for whom I can be sure that the inflationary effects of the crisis were detrimental since in the data I have available I do not have information that would indicate the extent to which agricultural households might have been sheltered or even benefited.[5]

[2] From IFLS community survey information.

[3] The IMF, the World Bank, and then president of Indonesia Suharto were all surprised by the depth, magnitude, and duration of the crisis, particularly given popular opinion at the time that Indonesia's story was one of miracle.

[4] See Levinsohn et al. (2003).

[5] This also has the benefit of avoiding several other issues that might have proved confounding. First, there was significant drought in some areas of Indonesia during the year following the onset of the crisis, and I would have been unable to identify and separate the effects of this from those of the crisis; by excluding agricultural households,

Thomas et al. (2004) note that the crisis, while unfortunate for those involved, provides an uncommon opportunity to identify the effects of an aggregate shock in a developing context unrelated to agricultural productivity.[6] Households which could not completely smooth consumption by drawing down assets were likely to temporarily adjust their budgets. Browning and Crossley (2001) predict that households reduce spending on durables and other investment goods if a crisis is expected to be short lived, since utility from them derives from the flow of services provided by such goods and is unlikely to suffer greatly from temporary underinvestment in the durables themselves. Thomas et al. (2004) find evidence to support this in the context of Indonesia's financial crisis and suggest that human capital investments in young children on both the intensive (spending on school fees, uniforms, and books) and the extensive (enrollment and attendance) margins qualify as one such example. Temporarily resource-constrained households may have assumed that such short term adjustments to their budgets were unlikely to negatively impact children's lifetime human capital accumulation.

Using a special 25 percent subsample of the IFLS panel, they find that per capita education expenditures decreased substantially in the poorest households and that spending on the youngest children's education suffered the most.[7] They use the household and national school enrollment data to support this finding, confirming that these poor households sought to protect the schooling of their older children[8], and that declines in education spending were greatest among urban households and those that were initially poorest before the crisis.

Son (2013) uses the 1997 and 2000 IFLS data to investigate whether schooling investments can be described by a perceived "sheepskin" effect on the part of parents; she finds that in the short-term experience of shocks related to the financial crisis as well as those more idiosyncratic income or unemployment shocks, families' short term enrollment decisions are consistent with preservation of schooling for those children slated to finish the final critical year of a schooling level. She does not, however, explore the inequality between siblings created by such decisions nor the long-run outcomes between them.

---

the effects of the drought will be small if they are even nonzero at all. Second, a public safety net program was implemented in some rural areas to provide partial scholarships to students who were deemed at risk of dropout from school; by excluding rural areas I exclude this obviously confounding factor. I check to be sure that restricting my analysis to urban households does not affect the results.

[6] An extensive literature in development economics exists documenting the effects of income shocks, particularly those attributable to idiosyncratic changes in weather, on the livelihoods and choices of households.

[7] The authors use the IFLS2+, which was not publicly available at the time of this writing. My own efforts to gain access to these data were unsuccessful.

[8] Thomas et al. (2004) suggest that these decisions were made in order to protect the schooling of the children whose returns would be the greatest.

2.3 Literature: Parental resources and investments in children

Parental resources are widely regarded by the economics literature as being a key determinant of children's human capital, the returns to schooling, and children's adult earnings (Haveman & Wolfe, 1995; Acemoglu & Pischke, 2001; Cameron & Heckman, 2001) (Altonji & Dunn, 1996) (Behrman & Taubman, 1990; Pe- ters, 1992; Solon, 1992; Zimmerman, 1992, Rosenzweig and Wolpin, 1994). Evidence from the development economics literature in particular suggests that adverse shocks to household incomes may affect the nature and level of these investments. Whether adverse shocks to parents' incomes increase or decrease schooling depends on the relationship between the income and substitution effects. This theoretical prediction of ambiguity is described by Basu & Van (1998) and others and has been empirically tested particularly in the context of the literature on child labor: the substitution effect operates by reducing the opportunity cost of school when the child wage rate falls during aggregate shocks, thereby serving to increase schooling hours, whereas the income effect increases the relative importance (the marginal utility) of the child's contributions to earnings, which would decrease schooling hours and increase labor hours.[9]

Whether shocks increase or decrease schooling, then, is an empirical question. Ferreira & Schady (2009) review the existing empirical literature in both developing and developed country contexts and find that macroeconomic shocks which reduce incomes tend to decrease schooling outcomes in Asia but that the opposite is true for the US, the UK, and some Latin American countries. They propose that borrowing constraints and initial wealth levels in part determine these differences.

While the above evidence points to the role of parents in determining the human capital and earnings outcomes of their children, it does not explain the empirical existence of differences among adults who were children in the same household. In fact, previous work regarding long-term sibling differences in outcomes is sparse, in part due to the demands of these questions placed on the data. In the classic child quantity-quality tradeoff posed by Becker (1960), the scarcity of family resources predicts that the investments made in each child are decreasing in the number of children. However, a number of recent empirical studies show that parental resources are not evenly divided, and that the division may be both controllable and intentional on the part of the parents. Therefore, economy wide inequality in educational attainment and

[9]Of course, there is not a one to one tradeoff between time spent in school and time spent working. On the one hand, work and school are not mutually exclusive: children can attend school and work during non-school hours as well. There is evidence also of the transition of children to and from "idleness" although the magnitudes are perhaps tainted by measurement error in the failure of documented "work" to reflect participation in income generating activities in household owned enterprises or in valuable but not income generating household production.

wages may derive not only from the household of origin but also the status of treatment or investment within that household. Francesconi et al. (2015) point to evidence from the United States that inequality in bequests is increasing, and is in many families related to time spent with parents.

Children from the same parents may have different outcomes related to their genetics (which are likely random), from differences related to the order of their birth, or from differential investments on the part of parents, which may also prove to be related to birth order. Genetic differences are outside the scope of the current project, but the initial endowment level of a child in cognitive abilities, in health, and in behavior has been shown to be related to the birth order of the child in the psychology literature and, more recently, in the economics literature. For example, women who give birth too young or too old are more likely to have children with birth defects or of low birthweight, and thus birth order may influence children's outcomes as a mother ages.[10] For child rearing reasons unrelated to investments made intentionally, there may be an advantage to being the first born (more alone time with parents) or the last born (parents have gained expertise in parenting).

Investments on the part of parents, then, are likely not only to be influenced by these initial endowments (a combination of genetics and of non-investment birth order effects) but also to depend on the preferences of parents and their objectives in the investment. There may be various and conflicting reasons that parents invest unequally in their children. On the one hand, parents may prefer equality in outcomes across their children for altruistic reasons, in which case they may invest more heavily in children with lower initial endowments. On the other hand, if parents view their children's outcomes as insurance for their own retirement, they may invest more heavily in the children they think are more likely to succeed; in this case, they may simply choose one child to concentrate their investments, or they may - for cultural and legal reasons - choose the eldest child or the eldest son. Of course, the solution to a standard model of utility maximization on the part of households with preferences for maximizing their expected payoff from their investments in children's education would equate the present day marginal cost of schooling with the present marginal value of expected payoffs from that investment. Social or cultural reasons may dictate that the eldest child is the one whose expected payoff is highest.

These hypothetical reasons are supported by the empirical economics literature. In a seminal work, Behrman and Taubman (1986) show that, in a small sample of US young adults, individuals who are the eldest or near the eldest sibling have higher educational attainment and may

[10] See Rosenzweig & Wolpin (1991) for a list.

have higher wages. They conclude the preference for first borns or children of lower birth order occurs either because these children have higher cognitive abilities related to their birth order or because parents are better able to sense a first born's probability of success relative to the more uncertain success of a (younger) higher order child.[11] Similarly, Horton (1988) uses sibling data from the Philippines to show that birth order has significant effects on both the short- and long-run nutritional status of siblings. Black, Devereux, and Salvanes (2005) find negative family size and birth order effects on educational attainment and wages in a large sample from Norway, and find that the family size effects disappear when birth order is included as a control. Together this evidence suggests that elder children in all families have better educational attainment and wages than higher birth order children in larger families, and these studies provide some evidence against the classic child quantity-quality tradeoff posited by Becker (1960). Child birth order, and not family size in general, appears to determine the outcomes of a particular child in these studies.

On the other hand, Quisumbing, Estuidillo and Otsuka (2003) note that households may exhibit behavior consistent with preferences for equality among their children. In particular, they posit that inequality averse parents may increase educational investment in their lower ability children in order to equalize them with their higher ability siblings. Relevant to the present topic, Quisumbing et al. (2003) also show that households in the Philippines invest more heavily in their daughters' education since they know that the eldest sons will receive inheritance in the form of land. This suggests that households which favor equality of their children may view different types of investments as interchangeable, particularly in the face of institutional or credit constraints which prohibit or restrict their investments.

While these studies do show that parents' preferences and their children's initial endowments explain differences in investment on the parts of parents, they do not address the dynamic nature of the investment decision, an issue which is important in considering the effect of a shock on differences in parental investments in education. Yamauchi (2008) uses panel data from South Africa to examine the dynamic complementarities of investments in human capital and health and their effects on inequality of educational attainment in later life. He notes that investments in early childhood affect the optimal level of investments in later childhood. For example, early nutritional or health investments in young children will affect the optimal level of schooling for high school aged children. Thai and Falaris (2011) find evidence of this behavior in Vietnam, where rainfall shocks during early childhood (age three) are shown to negatively

[11] "Higher order" in this literature refers to the children who are born later, that is, child number 2, 3, 4, etc.

affect children's school entry, progression, and completion later in life, suggesting that perceived returns to schooling, and therefore its attainment, depend on the building up of human capital early in life. In this way, inequalities that present themselves early in a child's life may affect the equity with which they are treated in later childhood.

Dahan and Gaviria (2003) present a simple model of intrahousehold resource allocation to show that parents may generate inequalities among siblings over time in the presence of non convexities in human capital returns and borrowing constraints such that parents cannot borrow to finance all of their children's education.[12] Using data from three Latin American countries, the paper finds behavior consistent with the predictions of the model.

This prediction is the most relevant to the current question of this proposal. In the context of shocks, households may be prompted to remove their children from school temporarily; the question of which child the household keeps in school is likely to depend on the interplay of preferences and objectives described above as well as the relative contributions each child could make to household production or earnings if removed from school. One possible outcome – which would appear consistent with the empirical evidence from Indonesia that younger children were withdrawn while older children remained in school– is that parents might choose to withdraw the younger child, who would have a longer period in which to recover from the shock itself. In this explanation, borrowing constrained parents would choose their younger children to withdraw temporarily from school, hoping that the duration of the shock would be short and the child could catch up later. This would be consistent with altruistic behavior on the part of borrowing or resource constrained households. On the other hand, even non-altruistic utility maximizing parents could be predicted to make the same choice: the expected marginal return to an additional year of school for a child who has completed all but a year of secondary school is likely much larger than that of a child who has completed only some of primary school. Provided that the parents are sufficiently forward looking that they internalize this expected future payoff even as they consider the current opportunity cost of the child's time (an older child is likely to yield higher wages from working during the shock than a younger child), they will likely keep the older child in school at the expense of the younger children. In fact, Son (2013) finds evidence from Indonesia in particular that households invest according to these perceived "sheepskin" effects.[13]

[12] In a separate paper, Gaviria (2002) develops an extension to the Becker-Tomes model of parental investment and uses the PSID to support the prediction that, contrary to the original model, sibling earnings inequality later in life is the same for both rich and poor families.

[13] Sheepskin effects are defined in the literature as additional returns to particular years of education beyond the marginal effects per year in a linear Mincer equation, owing their name to the fact that diplomas used to be written on sheepskin. See Card (1999).

Sadly, evidence on schooling interruption in the US and in the context of Indonesia's financial shock suggests that children do not catch up later and that even if they do, such that their lifetime educational attainment is equal to that of their peers, their wages suffer from these interruptions (Light, 1995 and Mejia-Mantilla, 2012). The question I pose in this study is more nuanced than whether children are adversely affected by a shock in the long-term. I seek to delve into the within-household school removal behaviors to identify whether among siblings the shock generated inequality that would not have otherwise existed and, if so, what the educational differences are between them nine years later.

In the next section, before turning to empirical estimation of inequality created between siblings during the financial crisis, I develop a model of unequal school investment in the presence of irregularities in the returns to schooling that generate perceived sheepskin effects.

3 A Model of Unequal Schooling Investments

Consider a household consisting of a parent (or two parents acting as a unit) and two children.[14] A standard model of human capital investments (such as Becker & Tomes (1976)) would suggest that self-serving parents invest in children's schooling to equalize the marginal return for a particular child and the marginal costs of schooling for that child (which can depend not only on direct schooling costs but also learning ability, heterogeneous opportunity costs, etc.).

If the simple investment model is extended to allow for transfers (either inter-vivos or bequested) made on the part of parents as well as direct human capital investments, parents view financial transfers and direct schooling expenditures as substitutes over the lifetime of a child. Following Behrman et al. (1982), the parent maximizes a two-period household welfare function that takes as arguments lifetime per capita consumption of food and other expenses for the household in each period (C_1 and C_2), schooling of the children during the first period of life (S_1 and S_2), and transfers made to the children in the second period of their life (T_1 and T_2):

$$\max_{C,S_1,S_2,T_1,T_2} W(C, u(S_1, T_1), u_2(S_2, T_2)) \tag{1}$$

[14]The assumption that two parents act as a single unit implies a unitary or collective (and not a competitive bargaining) model of the household, which has been rejected elsewhere in the development literature in a number of contexts. However, Lafave & Thomas (2014) provide a direct test in the Indonesian context (in fact, also using the IFLS data) and they fail to reject that members of households in the Indonesian context operate cooperatively to attain the Pareto efficient level of resources.

The parent has budget constraints for each period as follows:

$$p_1 C_1 + d(S_1 + S_2) = Y \tag{2}$$

$$p_2 C_2 + T_1 + T_2 = Y \tag{3}$$

where schooling costs are equal across children (d does not vary by child), transfers can be made without transaction costs, per-period income is predetermined, and per capita consumption in period t has a price p_t.

Suppose for simplicity that $C_1 = C_2 = \bar{C}$ which is the minimum subsistence level of consumption.[15] This simplifies the parent's problem to one of choosing schooling and transfer amounts for each of two children. If we assume that parents care about the utility of their children, this is altruism in the Beckerian sense, in which the (altruistic) parent's utility function incorporates the utility of her child. I assign each of the children utilities $U_i(S_i, T_i) = V(y_i(s_i) + T_i)$ so that the children have utility from their adult income which is the sum of earnings (with a child-specific earnings function relating schooling levels to earnings) and transfers from the parent. The parent solves for the optimal level of schooling for each child by setting:

$$\frac{\partial W}{\partial V_1}\frac{\partial y_1}{\partial S_1} = \frac{\partial W}{\partial V_2}\frac{\partial y_2}{\partial S_2} \tag{4}$$

If the parent has equal concern for the children (so that the children's utility weights in the parent's welfare function are equivalent), then this simplifies to

$$\frac{\partial y_1}{\partial S_1} = \frac{\partial y_2}{\partial S_2} \tag{5}$$

and suggests the familiar result that parents invest in children to equalize the marginal returns of the investment.[16] For example, if child 1 has earnings function $y_2 = ln(S_1)$ and child 2 has earnings function $y_2 = 2ln(S_2)$ (so that she turns each year of schooling into twice as much as child 1 does), then the solution suggests that the family should provide twice as much schooling for child 2 as for child 1.

Continuing to the second period of the children's lives, when they are grown they receive transfers from their parents to equalize their marginal utilities of their earnings (since their weights in the parent's welfare functions are the same) which means that $T_1 > T_2$ since child

[15] In any case, if the parent's welfare function is separable in C and in child investments, this is an innocuous simplifying assumption.

[16] I note here, as others have, that this occurs even if the parent is operating out of altruism.

2's earnings from schooling are so much larger (because their investment in her schooling was larger and her earnings function has a higher return).

3.1 Irregular Returns to School - "Sheepskin" Effect

Suppose that the earnings functions of children are not continuous. In particular, suppose that there are large and significant returns to finish a particular year of schooling (perhaps the very end of secondary school or the end of junior high school).[17] Suppose in this case that each child's earnings function is as follows (see Figure 1):

$$y(S) = \begin{cases} f(S) & \text{if } S < \bar{S} \\ g(S) + R + f(\bar{S}) & \text{if } S \geq \bar{S} \end{cases} \tag{6}$$

where $f'(S) > 0$, $g'(S) > 0$, $f''(0) < 0$, and $g''(0) < 0$. Such a returns function can be graphically described as in the figure.

A more simple version (a special case) of the above is:

$$y(S) = \begin{cases} ln(S) & \text{if } S < \bar{S} \\ ln(S) + B & \text{if } S \geq \bar{S} \end{cases} \tag{7}$$

where $B > 0$ is a positive shifter (the return to finishing the school year $\bar{S}$).

What are the implications of this return structure for parental investments in education? The non-convexity of the earnings function suggests that we need to consider several cases.

Case 1. Recall that if the household consumes the minimum subsistence consumption level $\bar{C}$ then the household's utility maximization problem is:[18]

$$\max_{S_1,S_2,T_1,T_2} W(u(S_1,T_1),u_2(S_2,T_2)) = W(V(ln(S_1)+\mathbb{1}(S_1>\bar{S})B+T_1), V(ln(S_2)+\mathbb{1}(S_2>\bar{S})B+T_2)) \tag{8}$$

$$d(S_1 + S_2) = Y - p_1\bar{C} \tag{9}$$

$$T_1 + T_2 = Y - p_2\bar{C} \tag{10}$$

Suppose that the family can afford to reach $\bar{S}$ for both children; that is, $2d\bar{S} < Y - p_1C_1$. In this

[17] Here I draw on the results of Son (2013) who found that human capital investments in Indonesia are consistent with a "sheepskin effect."

[18] Or if the household's utility over consumption is additively separable from its utility over its children's consumptions, this result still holds.

case, it can be shown that if parents have equal concern for their children, both children will achieve at least $\bar{S}$ and any additional possible schooling for the children will be divided equally among them.

Case 2. The more interesting and relevant case for the example of Indonesian families experiencing a shock to their incomes occurs when parents *cannot* achieve $\bar{S}$ for both children. In this case, the parents choose the schooling choice combination that maximizes utility. It can be shown that in the event that a family must decide whether to allocate school years among children in this case, the child who is closest to finishing $\bar{S}$ will finish that year and remaining funds for schooling will be divided according to the traditional marginal benefit rule. Depending on how much is left over after finishing $\bar{S}$ for that child, the gap in schooling achieved by children may widen if additional funds are not made available for schooling investments.

4 Empirical Strategy

4.1 The Indonesian Family Life Survey

The Indonesian Family Life Survey (IFLS) is a joint project of the RAND corporation and the Indonesian government to document the socioeconomic circumstances of Indonesia's people at the community, household, and individual levels. Designed as a multi-round panel survey, the project began in 1993 in half of Indonesia's 27 provinces and was designed to be representative of 83% of country in 1993.[19] The original sample size consisted of 7,224 households for a total of more than 30,000 individuals. Followup surveys were collected in 1997, 2000, and 2007 in which all original survey households as well as their offshoots were tracked and interviewed when possible.[20] Extensive cost and effort were made to ensure that attrition was as low as possible in order to maximize the usefulness of the survey in analyzing long term outcomes and socioeconomic patterns. The project boasts an impressively high reinterview rate of 94.5% between 1993 and 1997.[21]

In addition to the advantages of using panel data for controlling for time invariant unobservables within households or individuals, the timing of the 1997 survey collection (IFLS2) being

[19] The sampling frame was designed to minimize cost and maximize representativeness: the 13 provinces were selected and then enumeration areas (EAs) were randomly selected among them.

[20] An offshoot household is established whenever a survey respondent (household member) of the original survey household moves permanently away.

[21] It should be noted, of course, that characteristics of households and individuals lost to attrition are likely to differ systematically from those that are retained in fundamentally unobservable ways which might be correlated with the outcomes of interest. However, the tracking methodology of the IFLS significantly reduces this attrition bias and provides a significant improvement relative to most other surveys in the developing world which do not track, locate, and interview households or their members upon moving.

completed immediately before the financial crisis of 1998 is particularly convenient because pre-crisis conditions can be controlled for with relatively little measurement error. More than 95% of the IFLS2 interviews were completed before December 1997, one month before the inflationary crisis hit Indonesia.[22]

The panel nature of this dataset combined with the large and unanticipated shocks to households experienced during the Indonesian financial crisis make the Indonesian setting and the IFLS data in particular the best existing scenario in which to study creation of sibling inequality in the short and longer run. As I move forward with the analysis, I restrict my sample in he following ways: 1) Non-agricultural households: As described above, previous work has recorded potential gains from the inflationary crisis by rural households who produced food (for which prices rose dramatically during the crisis) and showed that urban households were hardest hit by the crisis. Unfortunately, the IFLS does not have information in the 1997 data (such as land area planted) which would allow me to measure the extent to which agricultural households were insulated or even benefited. Therefore, I restrict my sample to children living in non-agricultural households. 2) School-age children with siblings: I include all children who were under 18 or still enrolled in high school in 1997 and were living with at least one other biological sibling in 1997. and 3) Continuity of sample: I restrict the sample to those children for whom survey responses were available in 1997, 2000, and 2007 for them as well as all the children with whom they resided in 1997.[23] The sample I construct in this way has 2,925 children, who were living in 1,482 unique non-agricultural households.

4.2 Identifying the Impact of the Crisis

Educational outcomes for children who were finishing a schooling level

I begin my empirical work in this paper by estimating the extent to which children who were in their final year of a schooling level were affected differentially by the crisis relative to those who were not. That is, setting aside the question of intra-household or sibling effects, I look for evidence along the lines of Son (2013) that households made school investment decisions in the short term during the crisis that were consistent with a "sheepskin" effect for those children who were close to finishing.[24] To do this, I first identify children who were in their final year of

[22] Frankenberg and Thomas 2001.

[23] I do this in order to avoid the selection bias that might occur from some households having some children available for survey or tracked effectively and others not. Of course, these households may also be selected in non-random ways.

[24] A so-called "sheepskin" effect refers to additional benefits attributable to completion of a particular year of schooling associated with a diploma. The labor economics literature on human capital is divided as to whether the final capstone year is important in itself for the skills and topics mastered or whether the diplomas matter only as a

a schooling level at the time of the crisis. Since the crisis struck in January of 1998, I classify all children who, if they continued their schooling as normal during the crisis, would be in their final year of a schooling level in the school year that started in July 1998, in order to capture differential enrollment. These children were enrolled in their fifth, eighth, or eleventh year of school by the 1997 survey and would continue on to their final year of primary, junior high, or high school (sixth, ninth, or twelfth year of school), respectively, in the middle of the crisis. I call these children "Final" children. I explore the possibility that these children had different school advancement in the short run aftermath of the crisis relative to those children who were not in such a final year (measured when I see them the next time, in 2000) and, possibly, different total years of education attained in the long run (when I observe them again, nine years later in 2007).[25]

Specifically, I estimate the following:

$$\text{Educ Outcome}_{ihp} = \beta_0 + \beta_1 \text{Final}_{ihp} + \text{X}'_{ihp}\gamma + \eta_p + \epsilon_{ihp} \tag{11}$$

where Final_{ihp} is an indicator for whether child i in household h in province p is in the year just prior to the final year of a schooling level in 1997 (meaning that 7 months after the crisis struck she enters the final year of that schooling level if she continues in school and does not withdraw), and X_{ihp} is a set of individual and household level controls: child's age, gender, birth order, and household level controls (educational attainment of the household head, household size, number of siblings, and log per capita expenditures). I also include province fixed effects (η_p) to capture any province-specific time-invariant differences in average educational enrollment or other factors.[26] Thus, a positive and statistically significant estimate of β_1 would indicate that children who were in their final year of a schooling level at the time of the crisis had higher educational outcomes relative to children who were not. I explore the effects on the following educational outcomes: grade progression between 1997 and 2000 (the next time I see them in the data), grades completed between 2000 and 2007, junior high school graduation in 2007, high school graduation in 2007, and total years of education in 2007 (when most children are older than 18).

signal.

[25]This approach assumes that any enrollment effects attributable to the crisis were most salient when it came time to re-enroll in school (or advance to the next grade and/or schooling level) as opposed to during the middle of the school year.

[26]Sample weights are used in all analyses and standard errors are clustered at the *Kecamatan* (discrict) level. There are 35 districts in the sample. Results are forthcoming for using a bootstrap method to correct for the small number of clusters, but results are not substantively different if the standard errors are clustered at the community (enumeration area) level, of which there are 292.

This child-specific identification strategy relies on the assumption that children in their final year of a schooling level *at the time of the crisis* and children who were not in such a year are not systematically different from one another but that the timing of the crisis was such that one child experienced a different "treatment" from others as a result of the crisis; that is, that the timing and experience of the shock was as good as random vis à vis particular children. Of particular concern with this approach is that being in the final year of a schooling level might not only reflect that a child is of a particular age but that a child is also of a particular ability level. That is, if children of different abilities do not all start primary school at the same age or they do not progress through school at the same rate (i.e. do not skip or repeat grades), the "Final" status as described above would confound the effect of being in a final year of a schooling level with the reasons for a particular child's schooling progress. Table 1 shows the ages of children who we would normally expect to be in certain grade levels during the crisis conditional on their starting primary school at age 6 (when most Indonesian children start) and progressing normally through school up to the time of the crisis. However, some children in the sample are not in their age-appropriate grade in school: some of them are ahead and some of them are behind. I therefore identify children who are ahead (have completed more grades than normal given their age) and those who are behind (have completed fewer grades given their age). I define the dummy variables "Behind" and "Ahead" and include them in the estimation:

$$\text{Educ Outcome}_{ihp} = \beta_0 + \beta_1 \text{Final}_{ihp} + \beta_2 \text{Behind}_{ihp} + \beta_3 \text{Ahead}_{ihp} + \text{X}'_{ihp}\gamma + \eta_p + \epsilon_{ihp} \quad (12)$$

Figure 3 graphically shows results from this estimation. Consistent with the findings of Son (2013), children who were in their final year of a schooling level complete about 0.9 additional years of schooling beyond those who were not "Final" children between 1997 and 2000. They are thus more educated in 2000 (column 2), and since they complete no more or fewer grades between 2000 and 2007 (column 3), they are more educated by the time I see them as adults, in 2007 (column 4). These effects are statistically significant and suggest that during the crisis "Final" children were given priority in households' human capital investment decisions. Since I have controlled for whether a child is "Final" because of age or because of differential school starting/progress prior to 1997, these can be seen as the causal effect of the crisis on these children relative to their peers given the unanticipated and large effects to household purchasing power of the financial crisis. In results not shown here, these children are more likely than their peers to have finished junior high and high school by 2007, when most individuals are over 18,

but are no more or less likely to have completed some college or to be college graduates. Figures 4 and 5 suggest that this decision rule – to preserve the schooling of "Final" children in the short term aftermath of the crisis – is more salient than birth order (oldest or younger) in determining longer term educational outcomes among children.

Sibling Inequality Caused by the Crisis

In the previous section, I established that children who were in the final year of a level were given preferential treatment and advanced through school more than their peers during the crisis, resulting in higher levels of education in 2007 when they are adults. While, given the interest of this paper, it may seem natural to wonder what happened to siblings of those children - was their schooling reduced relative to their siblings? - very few papers have ever raised the question, nor have they been able to address it given the limitations of available data. I move forward by exploring this question; specifically, I classify the children who were living with children in their final year of a schooling level at the time of the crisis as "Siblings of Final" children.

A first attempt to address this question would estimate the following:

$$\text{Educ Outcome}_{ihp} = \beta_0 + \beta_1 \text{Final}_{ihp} + \beta_2 \text{Sibling of Final}_{ihp} + X'_{ihp}\gamma + \eta_p + \epsilon_{ihp} \qquad (13)$$

where the controls, fixed effects, and treatments of standard errors are as described above in the previous section. A positive effect on "Final" children relative to their siblings would be reflected by a positive and significant β_1. Given that I have shown "control" children to have been disadvantaged by the crisis, such a coefficient implies that "Sibling of Final" children as well as "control" children are disadvantaged, whereas "Final" children are sheltered by their status at the time the crisis struck.

As above, this child-specific identification strategy relies on the assumption that children in their final year of a schooling level *at the time of the crisis* are not systematically different from other children or *from their siblings* but that the timing of the crisis is such that one child experiences a different "treatment" from the other as a result of the crisis. That is, that the timing and experience of the shock was as good as random vis á vis particular children relative to their siblings as well as to children living in other households. In addition, the strategy requires that those households in which a "Final" child resides are not systematically different from those in which no such child resides in terms of their educational plans for their children and the extent to which they are inclined toward sharing via transfers. If parents were to invest in their best child up to the point where that child completed a level of schooling and only then

do they invest in the second best child, this empirical strategy would be violated; being a "final" child at the time of the crisis would reflect not only the exogenous timing of the crisis but also the previous preferential and intentional investment decisions on the part of parents.In order to address this concern, I again include indicators for whether children are ahead or behind relative to where they would be in school given their age and an assumed primary school starting age of 6. Results on short- and long-term schooling differences are presented in the next section.

5 Empirical Results

5.1 First Stage Results: Estimates of the Effect of Crisis on Education of Siblings

In what follows, I estimate Equation 19 for various outcomes related to educational attainment; that is, I use multivariate regressions and control for various individual and household characteristics to identify the effect of the crisis and its timing on own- and sibling- education levels and adult educational achievement. As described before, children are separated into three categories: 1) children who, if continuing school during the crisis, would have been enrolled in a grade that completed a schooling level, whether primary, junior high, or secondary school (these siblings carry an indicator "Final" in what follows); 2) children who had a sibling who was in a schooling level completion grade ("Sibling of Final"); and 3) "Control" children who were neither in a completion grade nor had a sibling who was (but, by virtue of the sample I have chosen, had co-resident siblings under 18 in 1997). These "control" children are the omitted reference category in the regression results which follow. For each outcome, I explore the role that controlling for the number of siblings, child birth order, and ability (indicated by whether children were ahead or behind in school grades relative to where they would have been if they had started at age six and progressed normally).

Table 3 shows that "Final" children progress through more grades between 1997 (just before the crisis) and 2000 relative to their siblings and relative to the controls. Columns 1 through 3 show that this effect persists even when controlling for the sibship size (number of siblings) and birth order, although birth order also significantly affects grade progression. Higher birth order children (i.e. the second child and younger) progress through more grades in the short-term aftermath of the crisis, but the coefficient on "Final" is twice as large as that on birth order. Recall that child age is controlled for. The positive effect for "Final" children also persists when controlling for whether children were ahead or behind. Notably, children who were behind also

progressed more through school during the crisis, while children who were ahead progressed through fewer grades (column 4). However, the effect of being "Final" does not vary among children who were behind, ahead, or on track since interaction terms for ahead and behind status with "Final" are not significant (column 5).

Turning to longer term schooling progression in Table 4, "Final" children do not progress through more grades during the second period (2000 to 2007) relative to the controls. However, their siblings *do* progress through more grades, indicating a level of catch up relative to their siblings and suggesting that having a "Final" child allowed families to shift resources to ride the crisis. This effect persists, but decreases in magnitude, when controlling for number of siblings, birth order, and ability (including ahead and behind dummies).

Table 5 shows that, as of 2007 (nine years after the crisis), "Final" children are more likely to have completed junior high school relative to control children (by 12 to 18 percentage points).[27] Their siblings are also more likely to have completed junior high relative to the controls, but relative to "Final" children, "Siblings of Final" children are less likely to have completed (by nine percentage points in Column 5). However, the difference in their completion rates is not statistically significant. This perhaps suggests that the decision rule of keeping children in school to finish a final year and then catching their siblings up after the crisis passed allowed families to ride the wave of the crisis better than those who had no such clear decision to make. Table 6 shows a similar outcome for "Final" children and their siblings in the probability of high school completion.

Table 7 shows that total educational attainment is higher for final children relative to control children, by about half a year (column 5). Note also that children who were behind in 1997 have lower educational attainment in 2007 and those who were ahead in 1997 have higher educational attainment (column 5). Educational attainment is also higher for "Sibling of Final" children relative to the controls. Without controlling for ability it looks like "Final" children have an advantage in terms of total years attained relative to their siblings by about a third of a year, but controlling for ability reduces this advantage. It also appears that "Final" children who were behind in 1997 have an advantage of about 0.4 years as well.

To summarize, the results presented above (and shown in Tables 3 to 7) on educational attainment suggest that

1. Children in their final year of a schooling level during the crisis were more likely to remain in school during the crisis (between 1997 and 2000) relative to **both** their siblings and to

[27] Note: marginal effects from a probit regression are shown for tables considering binary outcomes.

children living in other families where no "Final" child was present (recall that these are the omitted reference category of "control" children).

2. After the crisis subsided, between 2000 and 2007 children who were "Siblings of Final" children were back in school and completed more grades than **both** their "Final" siblings who did not experience the temporary dropout and the control children.
3. Combined, these 1997 to 2000 and 2000 to 2007 grade progression patterns result in *higher* rates of junior high and high school completion among **both** "Final" children *and* their siblings relative to control children. Average years of education among "Final" children and their siblings are also higher than the control group. The point estimates suggest that "Final" children are better educated relative to their siblings, but the confidence intervals overlap such that there are no statistically significant differences among these siblings.
4. The effects above persist when controlling for sibship size, birth order, gender, age, and ability (as proxied by whether children were ahead or behind in school when the crisis struck). Together these results suggest that a clear decision rule of preserving schooling mid-crisis of those children who were about to finish the final critical year of a schooling level helped families to adjust their budgets in the medium term in order to maintain higher levels of schooling for *all* of their children in the long run.
5. Finally, information from 2007 on the primary activities of respondents suggests that "Sibling of Final" children are more likely to still be in school, suggesting not only that they have caught up to their siblings who did not experience temporary schooling interruptions during the crisis but that their schooling attainment may indeed surpass that of their siblings. See Tables 8 and 9. The next wave of the panel (set to be released later in 2016) will allow me to explore these even longer-term educational attainment outcomes.

5.2 Second Stage Results: Estimates of the Effect of the Crisis on Wages and Coresidency

As we might predict, these educational differences translate into wage advantages. In a reduced form approach in Table 10, final children have a wage advantage that persists when controlling for sibship size, birth order, and ability (as well as per capita expenditures of their families just prior to the crisis). "Siblings of Final" children also have an advantage but this becomes insignificant at standard levels when controlling for ability. In Table 11 I estimate a two-stage regression with years of education instrumented with the siblings category variables (as in Table 7, column 5) and the second stage as a Mincer style log wage equation. The results

suggest a 15% increase in wages per year of education. The F-statistic for a joint significance test of the excluded instruments is 15.34.

Tables 12 through 14 show results for coresidency status in 2007. I look at probability of being married, living in the same household as in 1997, living with one's parents, and having siblings living elsewhere. Table 12 uses the sibship size and birth order controls, Table 13 adds the Behind and Ahead dummies, and Table 14 adds the interactions of Behind and Ahead with "Final" status. The results suggest that "Sibling of Final" children are slightly less likely to be married than controls and "Final" children. Children who were "Behind" in 1997 are more likely to be living in the same household and with their parents in 2007, whereas the opposite is true for children who were Ahead in 1997: they are less likely to live with their parents.

6 Conclusion

I find evidence that families preserved schooling of children during the Indonesian financial crisis if they were set to finish the final, critical year of a schooling level, but that the crisis and this decision rule created short-term inequality in educational attainment among siblings. Years later, when most of these siblings are older than 18, there do not remain any statistically significant differences in educational attainment (years completed) among these siblings. However, those siblings who experienced short-term dropout relative to their siblings are more likely to still be enrolled in school, suggesting that they may be even more educated than their siblings in the longer term. Using the IFLS 5 round of the panel will allow me to provide further evidence on this. Relative to children living in households where *no* child was in the final year of a schooling level at the time of the 1997 financial crisis, children living in households where the decision rule was clear are more educated, whether or not they were in their final year at the time. These preliminary results suggest that preserving the final year of a schooling level allowed families to adjust temporarily in ways that maintained the schooling of *all* of their children over the long run.

References

Basu, K., & Van, P. H. (1998). The Economics of Child Labor. *American Economic Review*.

Becker, G. S., & Tomes, N. (1976). Child Endowments and the Quantity and Quality of Children. *Journal of Political Economy*, *84*(4), S143.

Behrman, J. R., Pollak, R. a., & Taubman, P. (1982). Parental Preferences and Provision for Progeny. *Journal of Political Economy*, *90*(1), 52–73.

Dahan, M., & Gaviria, A. (2003). Parental actions and sibling inequality. *Journal of Development Economics*, *72*(1), 281–297.
URL http://linkinghub.elsevier.com/retrieve/pii/S0304387803000774

Ferreira, F. H. G., & Schady, N. (2009). Aggregate economic shocks, child schooling, and child health. *World Bank Research Observer*, *24*(August), 147–181.

Francesconi, M., Pollak, R. A., Tabasso, D., Francesconi, M., & Pollak, R. A. (2015). Unequal Bequests.

Lafave, D., & Thomas, D. (2014). Extended Families and Child Well-being.

Levinsohn, J., Berry, S., & Friedman, J. (2003). Impacts of the Indonesian Economic Crisis: Price Changes and the Poor. In M. P. Dooley, & J. A. Frankel (Eds.) *Managing currency crises in emerging markets.*, (pp. 393–428). University of Chicago Press.
URL http://www.nber.org/chapters/c9657.pdf

Li, H., Rosenzweig, M., & Zhang, J. (2010). Altruism, Favoritism, and Guilt in the Allocation of Family Resources: Sophie's Choice in Mao's Mass Send-Down Movement. *Journal of Political Economy*, *118*(1), 1–38.

Rosenzweig, M. R., & Wolpin, K. I. (1991). Inequality at birth The scope for policy intervention. *Journal of Econometrics*, *50*(1-2), 205–228.
URL http://linkinghub.elsevier.com/retrieve/pii/030440769190096V

Son, H. (2013). Human Capital Investment When Sheepskin Effects Matter : Evidence from Income Shocks in Indonesia. (pp. 1–48).

Thomas, D., Beegle, K., Frankenberg, E., Sikoki, B., Strauss, J., & Teruel, G. (2004). Education in a crisis. *Journal of Development Economics*, *74*, 53–85.
URL http://linkinghub.elsevier.com/retrieve/pii/S0304387803001809

Thomas, D., & Frankenberg, E. (2007). *Household Responses to the Financial Crisis in Indonesia Longitudinal Evidence on Poverty, Resources, and Well-Being*. March. University of Chicago Press.

URL `http://www.nber.org/books/harr06-1`

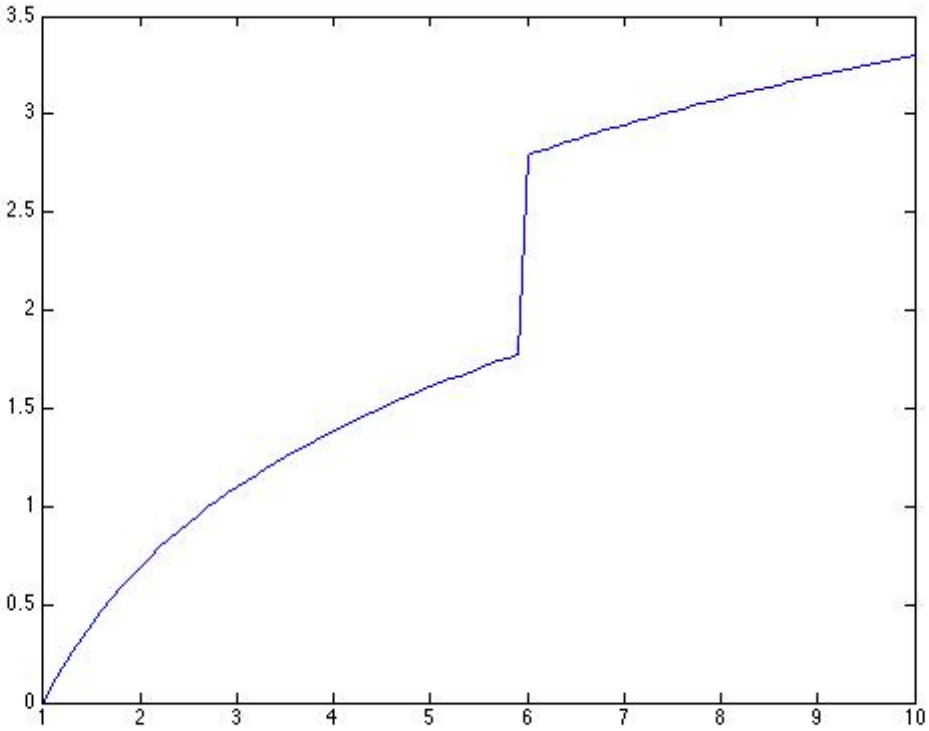

Figure 1: Irregular Returns to Schooling
The figure shows a particular return associated with completion of a school level.

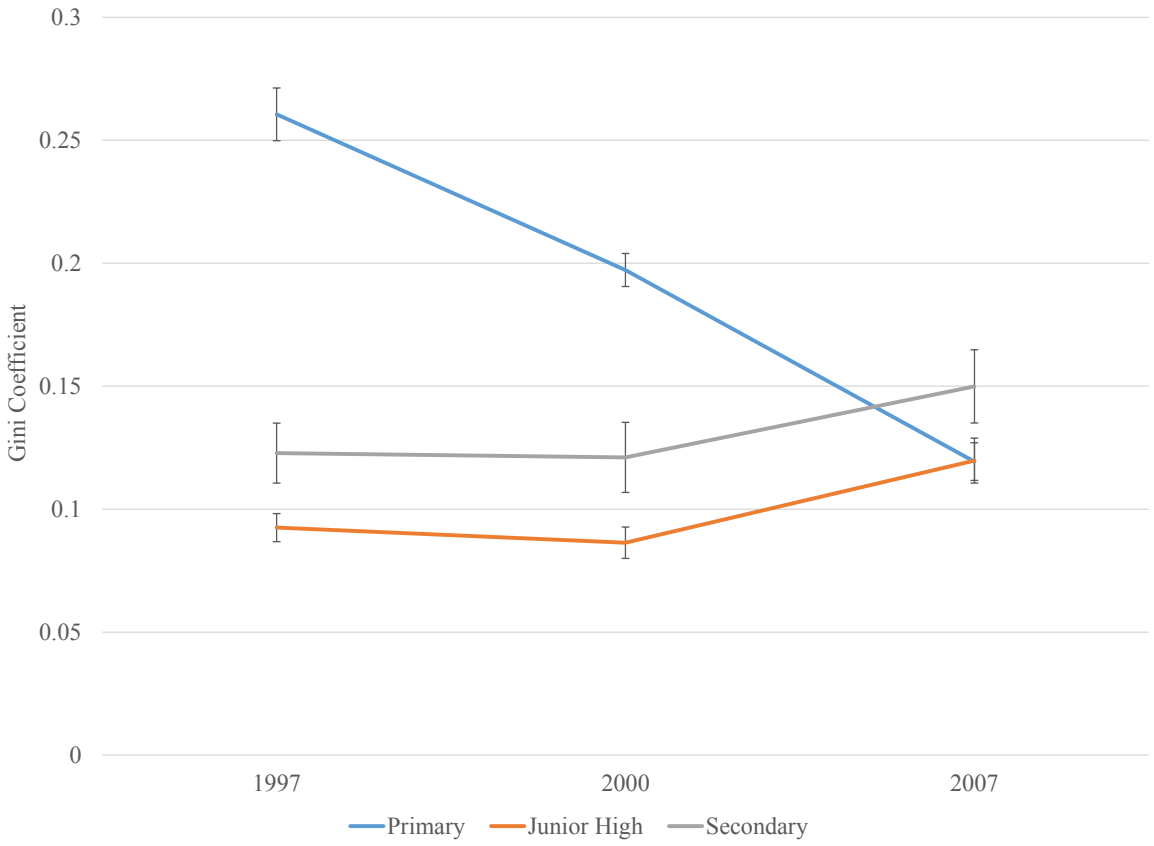

Figure 2: Gini Coefficient of Educational Attainment

Gini coefficients of educational attainment by age groups in 1997. Error bars are 95% confidence intervals using jackknife standard errors.

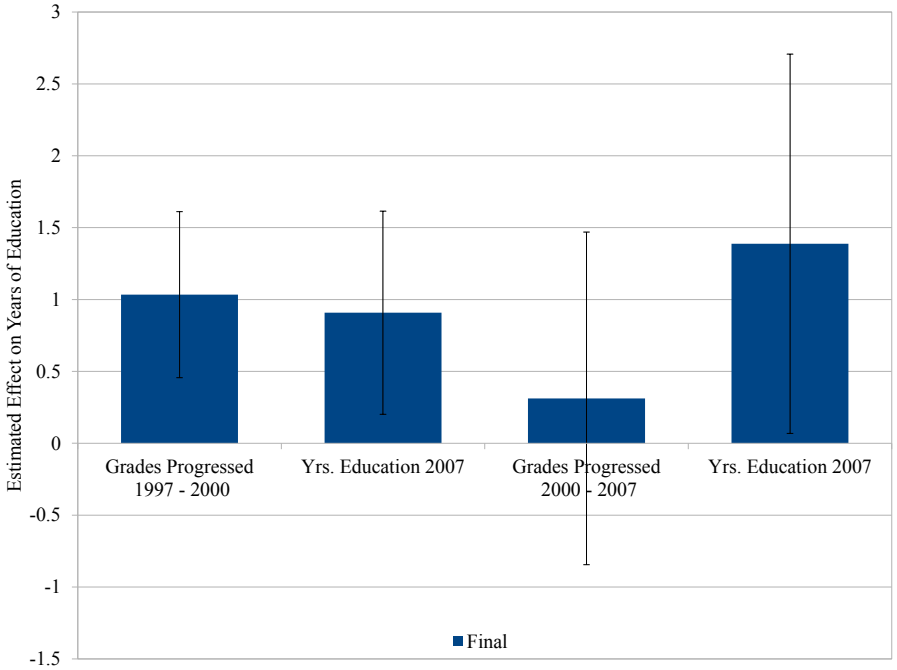

Figure 3: Effect of Crisis on Final Children's Educational Attainment.
Estimated coefficients with 95% confidence intervals. Effects are relative to other children who are not in their final year of a schooling level at the time of the crisis. Controls as described in equation.

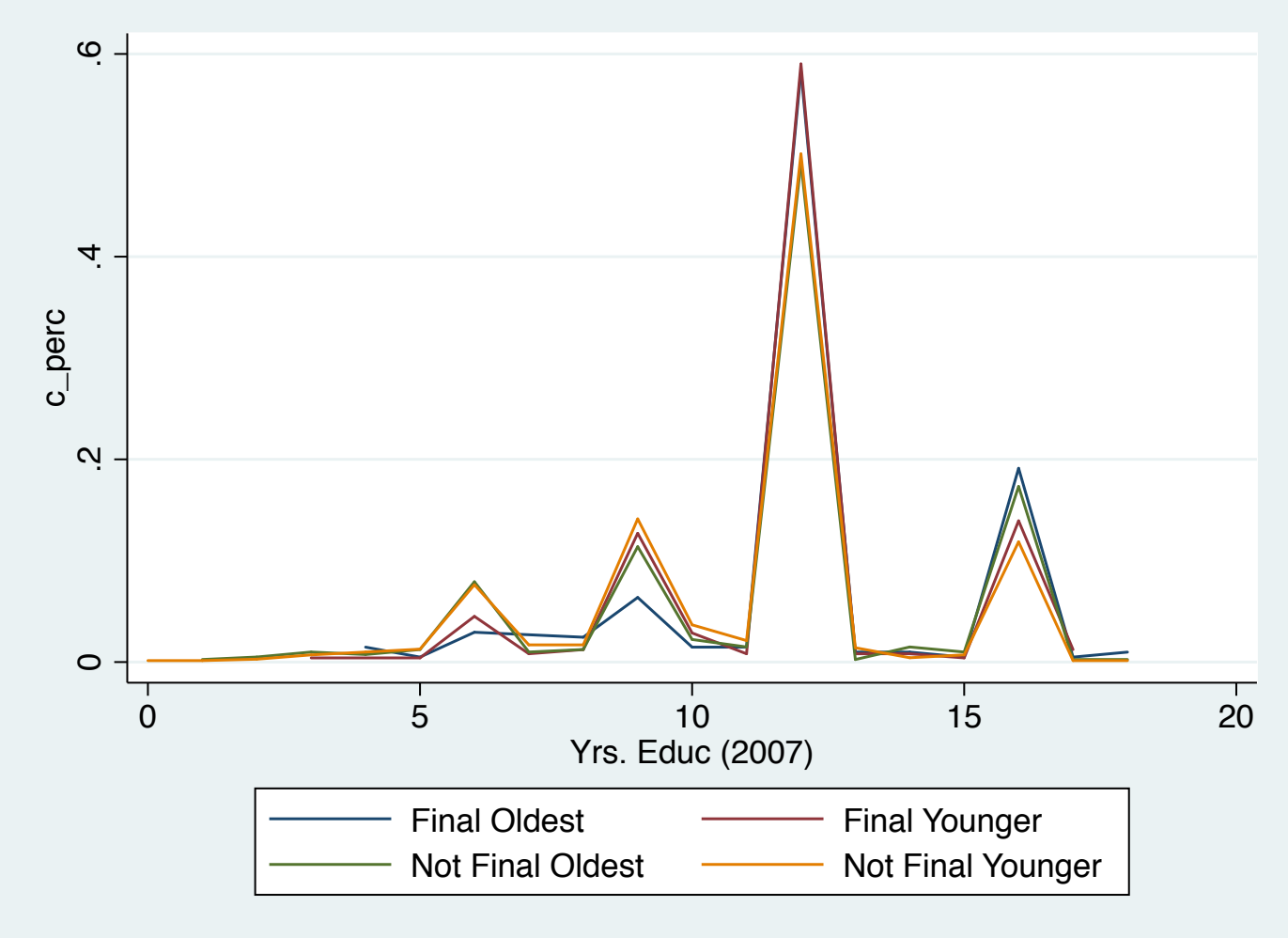

Figure 4: School Completion by Birth Order and Final Status - PDF
Probability density of school completion by birth order and "final" status.

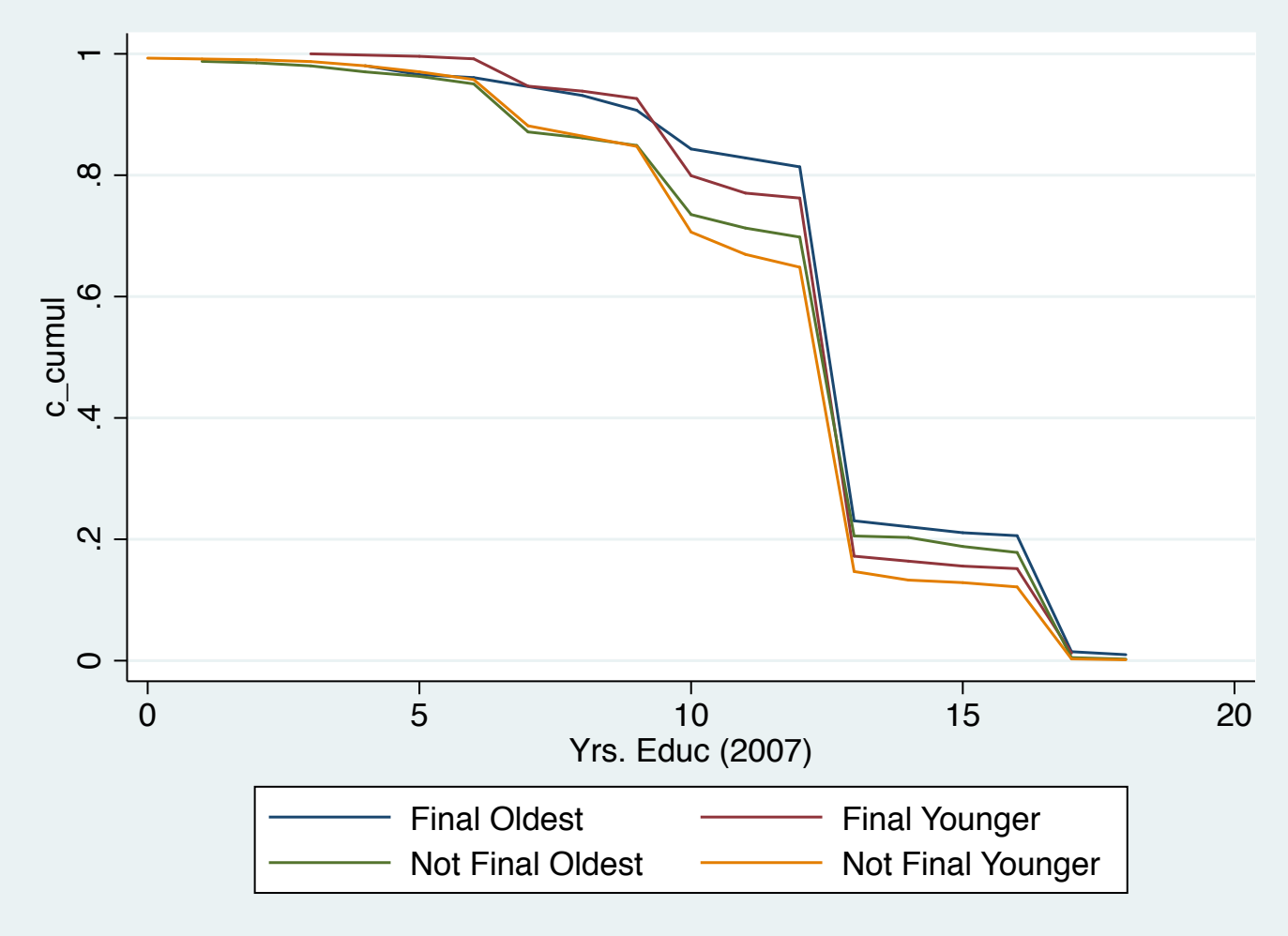

Figure 5: School Completion by Birth Order and Final Status - Cumulative Grade Completion
Cumulative density of school completion by birth order and "final" status.

Age in 1997	Grade in 1997 Survey	Grade Mid-crisis	Would be Final?
6	1	2	
7	2	3	
8	3	4	
9	4	5	
10	5	6	Y
11	6	7	
12	7	8	
13	8	9	Y
14	9	10	
15	10	11	
16	11	12	Y

Table 1: Would be Final Status

Age and Schooling Levels			
	Age 9	Age 10	Age 11
Grade 4	On Track	Behind	Behind
Grade 5	Ahead	On Track	Behind
Grade 6	Ahead	Ahead	On Track

Table 2: On Track, Ahead, and Behind Status

Dependent variable: Grades Progressed 1997 to 2000

VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.233***	0.231***	0.216***	0.304***	0.327***
	(0.0681)	(0.0705)	(0.0702)	(0.0692)	(0.118)
Behind				0.166***	0.176**
				(0.0466)	(0.0659)
Ahead				-0.515***	-0.509***
				(0.0513)	(0.0601)
Final*Behind					-0.0414
					(0.136)
Final*Ahead					-0.0251
					(0.182)
Final Sib	0.0941	0.0885	0.0660	0.0983	0.0986
	(0.0689)	(0.0758)	(0.0744)	(0.0721)	(0.0718)
Log Expenditures (percap)	0.0886	0.0886	0.0891	0.111	0.111
	(0.0819)	(0.0817)	(0.0815)	(0.0786)	(0.0785)
Female	0.00909	0.00925	0.0115	0.0419	0.0422
	(0.0564)	(0.0564)	(0.0567)	(0.0572)	(0.0573)
Number of Siblings		0.0145	-0.0187	-0.0189	-0.0190
		(0.0377)	(0.0406)	(0.0387)	(0.0391)
Birth Order			0.129***	0.101**	0.101**
			(0.0436)	(0.0409)	(0.0407)
Age (1997)	-0.160***	-0.161***	-0.143***	-0.174***	-0.174***
	(0.00710)	(0.00669)	(0.00995)	(0.0103)	(0.0102)
Constant	3.208***	3.221***	2.821***	2.992***	2.991***
	(0.857)	(0.837)	(0.843)	(0.822)	(0.822)
Observations	2,960	2,960	2,960	2,960	2,960
R-squared	0.159	0.159	0.162	0.190	0.190
Province FE?	Y	Y	Y	Y	Y

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

Table 3: Effect of Crisis on Grade Progression (by 2000) for Final Year Children and Siblings

Dependent variable: Grades Progressed 2000 to 2007

VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.0288	0.0250	0.0248	0.0125	-0.107
	(0.0916)	(0.0903)	(0.0949)	(0.101)	(0.128)
Behind				-0.0674	-0.0823
				(0.131)	(0.143)
Ahead				0.0162	-0.0648
				(0.117)	(0.144)
Final*Behind					0.0598
					(0.213)
Final*Ahead					0.330
					(0.231)
Final Sib	0.373***	0.361***	0.361***	0.357***	0.359***
	(0.116)	(0.112)	(0.118)	(0.118)	(0.117)
Log Expenditures (percap)	0.320***	0.320***	0.320***	0.318***	0.319***
	(0.0532)	(0.0528)	(0.0529)	(0.0528)	(0.0531)
Female	0.00221	0.00279	0.00282	-0.00176	0.000951
	(0.0808)	(0.0811)	(0.0814)	(0.0812)	(0.0807)
Number of Siblings		0.0317	0.0312	0.0317	0.0335
		(0.0413)	(0.0467)	(0.0466)	(0.0468)
Birth Order			0.00164	0.00364	0.00457
			(0.0887)	(0.0889)	(0.0891)
Age (1997)	-0.507***	-0.508***	-0.508***	-0.503***	-0.503***
	(0.0104)	(0.0104)	(0.0157)	(0.0191)	(0.0191)
Constant	5.253***	5.276***	5.271***	5.275***	5.275***
	(0.547)	(0.541)	(0.632)	(0.631)	(0.632)
Observations	2,925	2,925	2,925	2,925	2,925
R-squared	0.475	0.475	0.475	0.475	0.476
Province FE?	Y	Y	Y	Y	Y

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

Table 4: Effect of Crisis on Grade Progression (2000 to 2007) for Final Year Children and Siblings

Dependent variable: Junior High Graduate

VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.189***	0.190***	0.186***	0.124***	0.131***
	(0.0152)	(0.0156)	(0.0165)	(0.0170)	(0.0270)
Behind				-0.211***	-0.209***
				(0.0145)	(0.0179)
Ahead				0.0835***	0.0864***
				(0.0211)	(0.0252)
Final*Behind					-0.00875
					(0.0405)
Final*Ahead					-0.0123
					(0.0404)
Final Sib	0.0639***	0.0671***	0.0598***	0.0443***	0.0443***
	(0.0139)	(0.0158)	(0.0162)	(0.0157)	(0.0157)
Log Expenditures (percap)	0.0245**	0.0247**	0.0250**	0.0114	0.0114
	(0.0109)	(0.0110)	(0.0110)	(0.00817)	(0.00813)
Female	0.0488**	0.0487**	0.0493**	0.0271	0.0270
	(0.0198)	(0.0197)	(0.0200)	(0.0174)	(0.0175)
Number of Siblings		-0.00903	-0.0207	-0.0173	-0.0173
		(0.0136)	(0.0128)	(0.0123)	(0.0122)
Birth Order			0.0449***	0.0489***	0.0489***
			(0.0149)	(0.0117)	(0.0117)
Age (1997)	0.0424***	0.0427***	0.0489***	0.0613***	0.0613***
	(0.00285)	(0.00267)	(0.00368)	(0.00327)	(0.00326)
Constant				-0.264**	-0.265**
				(0.119)	(0.119)
Observations	3,085	3,085	3,085	3,085	3,085
R-squared				0.248	0.248
Province FE?	Y	Y	Y	Y	Y

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

Table 5: Effect of Crisis on Junior High Graduation (by 2007) for Final Year Children and Siblings

Dependent variable: High School Graduate

VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.188***	0.188***	0.184***	0.109***	0.0978***
	(0.0225)	(0.0224)	(0.0232)	(0.0203)	(0.0246)
Behind				-0.250***	-0.251***
				(0.0168)	(0.0184)
Ahead				0.113***	0.105***
				(0.0145)	(0.0168)
Final*Behind					0.00519
					(0.0417)
Final*Ahead					0.0316
					(0.0344)
Final Sib	0.0945***	0.0932***	0.0860***	0.0606***	0.0608***
	(0.0224)	(0.0240)	(0.0237)	(0.0193)	(0.0192)
Log Expenditures (percap)	0.0508***	0.0508***	0.0513***	0.0310**	0.0311**
	(0.0165)	(0.0165)	(0.0164)	(0.0128)	(0.0128)
Female	0.0187	0.0188	0.0191	-0.00280	-0.00263
	(0.0229)	(0.0229)	(0.0231)	(0.0189)	(0.0191)
Number of Siblings		0.00346	-0.00822	-0.00533	-0.00511
		(0.0140)	(0.0167)	(0.0126)	(0.0126)
Birth Order			0.0425	0.0474**	0.0475**
			(0.0260)	(0.0185)	(0.0185)
Age (1997)	0.0433***	0.0431***	0.0490***	0.0618***	0.0618***
	(0.00277)	(0.00256)	(0.00446)	(0.00286)	(0.00286)
Constant				-0.517***	-0.517***
				(0.169)	(0.169)
Observations	3,058	3,058	3,058	3,058	3,058
R-squared				0.272	0.272
Province FE?	Y	Y	Y	Y	Y

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

Table 6: Effect of Crisis on High School Graduation (by 2007) for Final Year Children and Siblings

Dependent variable: Years of Education in 2007

VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.991***	0.990***	0.980***	0.667***	0.460***
	(0.135)	(0.136)	(0.139)	(0.130)	(0.161)
Behind				-1.573***	-1.667***
				(0.149)	(0.163)
Ahead				0.558***	0.517***
				(0.118)	(0.131)
Final*Behind					0.411*
					(0.224)
Final*Ahead					0.187
					(0.221)
Final Sib	0.648***	0.647***	0.629***	0.519***	0.515***
	(0.132)	(0.135)	(0.138)	(0.126)	(0.125)
Log Expenditures (percap)	0.504***	0.504***	0.504***	0.446***	0.448***
	(0.0904)	(0.0903)	(0.0902)	(0.0856)	(0.0862)
Female	0.160	0.160*	0.161*	0.0507	0.0474
	(0.0948)	(0.0945)	(0.0944)	(0.0866)	(0.0873)
Number of Siblings		0.00281	-0.0263	-0.0120	-0.0119
		(0.0464)	(0.0580)	(0.0570)	(0.0570)
Birth Order			0.107	0.164	0.163
			(0.125)	(0.105)	(0.105)
Age (1997)	0.104***	0.103***	0.118***	0.229***	0.230***
	(0.0139)	(0.0139)	(0.0199)	(0.0185)	(0.0186)
Constant	3.057***	3.059***	2.740**	2.751**	2.772**
	(1.119)	(1.121)	(1.247)	(1.107)	(1.115)
Observations	3,058	3,058	3,058	3,058	3,058
R-squared	0.225	0.225	0.226	0.296	0.297
Province FE?	Y	Y	Y	Y	Y

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

Table 7: Effect of Crisis on Education Attained (by 2007) for Final Year Children and Siblings

	Primary Activity in 2007			
	(1)	(2)	(3)	(4)
VARIABLES	Working	In School	House Work	Looking for Work
Final	0.0206	-0.0234*	-0.0126	0.00610
	(0.0304)	(0.0125)	(0.0243)	(0.0175)
Behind	-0.0415	0.0200	-0.0447*	0.00192
	(0.0330)	(0.0146)	(0.0268)	(0.0166)
Ahead	0.0176	0.0102	-0.0275	0.00446
	(0.0286)	(0.0130)	(0.0237)	(0.0191)
Final Sib	-0.0342	0.0288**	-0.0223	-0.0203
	(0.0209)	(0.0121)	(0.0317)	(0.0137)
Log Expenditures (percap)	-0.0323**	0.0395***	-0.00982	-0.0112
	(0.0134)	(0.00788)	(0.0135)	(0.00902)
Female	-0.206***	0.00325	0.514***	0.350***
	(0.0269)	(0.0114)	(0.0163)	(0.0176)
Number of Siblings	-7.21e-06	-0.00626	0.0235*	-0.00518
	(0.00942)	(0.00646)	(0.0124)	(0.00832)
Birth Order	0.0121	-0.00792	-0.0276**	-0.00633
	(0.0160)	(0.0108)	(0.0126)	(0.00991)
Age (1997)	0.0518***	-0.0479***	0.0205***	0.0127***
	(0.00358)	(0.00214)	(0.00468)	(0.00248)
Observations	3,086	3,086	3,086	3,087
Province FE?	Y	Y	Y	Y

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

Table 8: Effect of Crisis on 2007 Primary Activity for Final Year Children and Siblings

Primary Activity in 2007, Ability Interacted

VARIABLES	(1) Working	(2) In School	(3) House Work	(4) Looking for Work
Final	-0.00339	-0.0597***	-0.0348	0.000322
	(0.0470)	(0.0222)	(0.0348)	(0.0230)
Behind	-0.0509	0.0169	-0.0512	-0.000771
	(0.0367)	(0.0151)	(0.0315)	(0.0193)
Ahead	0.0108	-0.00353	-0.0378	0.00331
	(0.0311)	(0.0137)	(0.0287)	(0.0221)
Final*Behind	0.0418	0.0145	0.0275	0.0115
	(0.0538)	(0.0526)	(0.0635)	(0.0291)
Final*Ahead	0.0294	0.108**	0.0435	0.00506
	(0.0440)	(0.0535)	(0.0706)	(0.0378)
Final Sib	-0.0345	0.0283**	-0.0221	-0.0204
	(0.0213)	(0.0118)	(0.0317)	(0.0138)
Log Expenditures (percap)	-0.0321**	0.0394***	-0.00966	-0.0111
	(0.0134)	(0.00788)	(0.0135)	(0.00896)
Female	-0.206***	0.00437	0.514***	0.350***
	(0.0268)	(0.0114)	(0.0163)	(0.0175)
Number of Siblings	0.000107	-0.00569	0.0237*	-0.00517
	(0.00938)	(0.00644)	(0.0124)	(0.00843)
Birth Order	0.0119	-0.00739	-0.0275**	-0.00635
	(0.0161)	(0.0104)	(0.0126)	(0.01000)
Age (1997)	0.0519***	-0.0466***	0.0206***	0.0127***
	(0.00355)	(0.00216)	(0.00465)	(0.00249)
Observations	3,086	3,086	3,086	3,087
Province FE?	Y	Y	Y	Y

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

Table 9: Effect of Crisis on 2007 Primary Activity for Final Year Children and Siblings, Ability Interacted

Dependent variable: Log monthly earnings in 2007					
	(1)	(2)	(3)	(4)	(5)
VARIABLES		With Num Sibs Control	Birth Order Control	Controls for Ability	Controls for Ability and Interact
Final	0.133**	0.129**	0.126**	0.0930*	0.137
	(0.0535)	(0.0529)	(0.0543)	(0.0522)	(0.0970)
Behind				-0.285***	-0.263**
				(0.0808)	(0.105)
Ahead				0.0780	0.0902
				(0.0608)	(0.0773)
Final*Behind					-0.0799
					(0.160)
Final*Ahead					-0.0454
					(0.134)
Final Sib	0.0911*	0.0837*	0.0800*	0.0701	0.0702
	(0.0498)	(0.0449)	(0.0464)	(0.0489)	(0.0489)
Log Expenditures (percap)	0.156***	0.157***	0.156***	0.141***	0.142***
	(0.0414)	(0.0419)	(0.0420)	(0.0424)	(0.0426)
Female	-0.121*	-0.122*	-0.122*	-0.150**	-0.149**
	(0.0623)	(0.0624)	(0.0624)	(0.0628)	(0.0626)
Number of Siblings		0.0172	0.0114	0.0111	0.0112
		(0.0258)	(0.0290)	(0.0288)	(0.0288)
Birth Order			0.0204	0.0283	0.0276
			(0.0439)	(0.0406)	(0.0409)
Age (1997)	0.0616***	0.0609***	0.0639***	0.0850***	0.0848***
	(0.00855)	(0.00864)	(0.0133)	(0.0135)	(0.0137)
Constant	10.91***	10.92***	10.85***	10.80***	10.79***
	(0.394)	(0.387)	(0.387)	(0.351)	(0.358)
Observations	1,309	1,309	1,309	1,309	1,309
R-squared	0.157	0.157	0.157	0.180	0.181
Province FE?	Y	Y	Y	Y	Y

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

Table 10: Effect of Crisis on 2007 Earnings for Final Year Children and Siblings

Dependent variable: Log monthly earnings in 2007

VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Yrs. Educ (2007)	0.154**	0.154**	0.155**	0.165***	0.159***
	(0.0748)	(0.0743)	(0.0776)	(0.0320)	(0.0325)
Log Expenditures (percap)	0.0702	0.0705	0.0702	0.0639	0.0676
	(0.0530)	(0.0547)	(0.0549)	(0.0580)	(0.0587)
Female	-0.229***	-0.229***	-0.229***	-0.236***	-0.232***
	(0.0758)	(0.0740)	(0.0745)	(0.0631)	(0.0624)
Number of Siblings		0.000682	0.00172	0.00120	0.00150
		(0.0251)	(0.0291)	(0.0314)	(0.0312)
Birth Order			-0.00376	-0.00667	-0.00499
			(0.0454)	(0.0409)	(0.0408)
Age (1997)	0.0379**	0.0380**	0.0373*	0.0351***	0.0364***
	(0.0176)	(0.0174)	(0.0221)	(0.0130)	(0.0126)
Constant	10.14***	10.14***	10.15***	10.14***	10.14***
	(0.506)	(0.501)	(0.468)	(0.428)	(0.421)
Observations	1,298	1,298	1,298	1,298	1,298
R-squared	0.060	0.061	0.060	0.037	0.050
Province FE?	Y	Y	Y	Y	Y
IV?	Y	Y	Y		

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

Table 11: Returns to Education in 2007 (Instrumenting with Final Year and Sibling Status)

Coresidency Outcomes in 2007

VARIABLES	(1) Ever Married	(2) Still in Same HH	(3) Lives with Parents	(4) Has Siblings Elsewhere
Final	-0.0120	-0.0193	-0.00299	-0.0137
	(0.0240)	(0.0237)	(0.0329)	(0.0252)
Final Sib	-0.0483*	0.0321	0.0314	-0.0308
	(0.0262)	(0.0261)	(0.0349)	(0.0206)
Log Expenditures (percap)	-0.0208	-0.00562	-0.00544	0.00292
	(0.0132)	(0.0175)	(0.0158)	(0.0130)
Female	0.311***	-0.0768***	-0.0865***	0.0245**
	(0.0188)	(0.0163)	(0.0185)	(0.0113)
Number of Siblings	-0.01000	0.00822	0.00442	0.0372***
	(0.0149)	(0.0131)	(0.0139)	(0.0135)
Birth Order	-0.0216	0.0180	0.0198	0.0888***
	(0.0181)	(0.0148)	(0.0143)	(0.0163)
Age (1997)	0.0743***	-0.0317***	-0.0367***	0.0289***
	(0.00506)	(0.00365)	(0.00390)	(0.00408)
Observations	3,086	3,087	3,087	3,085
Province FE?	Y	Y	Y	Y

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

Table 12: Effect of Crisis on Coresidency with Parents and Siblings (no ability controls)

Coresidency Outcomes in 2007 (Ability Controls)

VARIABLES	(1) Ever Married	(2) Still in Same HH	(3) Lives with Parents	(4) Has Siblings Elsewhere
Final	-0.0194	-0.00250	0.0113	-0.0135
	(0.0241)	(0.0235)	(0.0325)	(0.0244)
Behind	-0.0279	0.0601***	0.0547**	0.00397
	(0.0274)	(0.0188)	(0.0246)	(0.0189)
Ahead	0.0354	-0.0709***	-0.0562*	0.00348
	(0.0318)	(0.0272)	(0.0287)	(0.0190)
Final Sib	-0.0522**	0.0390	0.0369	-0.0308
	(0.0263)	(0.0270)	(0.0353)	(0.0204)
Log Expenditures (percap)	-0.0228*	-0.00187	-0.00251	0.00288
	(0.0133)	(0.0179)	(0.0155)	(0.0131)
Female	0.309***	-0.0713***	-0.0818***	0.0245**
	(0.0192)	(0.0171)	(0.0191)	(0.0111)
Number of Siblings	-0.00973	0.00785	0.00400	0.0371***
	(0.0146)	(0.0132)	(0.0140)	(0.0135)
Birth Order	-0.0194	0.0139	0.0164	0.0890***
	(0.0187)	(0.0147)	(0.0145)	(0.0167)
Age (1997)	0.0774***	-0.0382***	-0.0423***	0.0289***
	(0.00577)	(0.00430)	(0.00446)	(0.00431)
Observations	3,086	3,087	3,087	3,085
Province FE?	Y	Y	Y	Y

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

Table 13: Effect of Crisis on Coresidency with Parents and Siblings (with ability controls)

Coresidency Outcomes in 2007 (Ability Controls and Interacted)

VARIABLES	(1) Ever Married	(2) Still in Same HH	(3) Lives with Parents	(4) Has Siblings Elsewhere
Final	-0.0196	0.0366	0.0237	0.00201
	(0.0354)	(0.0402)	(0.0584)	(0.0349)
Behind	-0.0335	0.0824***	0.0588*	0.00469
	(0.0323)	(0.0235)	(0.0313)	(0.0205)
Ahead	0.0455	-0.0684*	-0.0507	0.0138
	(0.0416)	(0.0364)	(0.0327)	(0.0227)
Final*Behind	0.0231	-0.0968**	-0.0169	-0.00223
	(0.0490)	(0.0469)	(0.0688)	(0.0383)
Final*Ahead	-0.0321	-0.0140	-0.0213	-0.0467
	(0.0488)	(0.0537)	(0.0554)	(0.0456)
Final Sib	-0.0527**	0.0403	0.0369	-0.0310
	(0.0259)	(0.0269)	(0.0353)	(0.0204)
Log Expenditures (percap)	-0.0227*	-0.00216	-0.00261	0.00264
	(0.0133)	(0.0181)	(0.0156)	(0.0130)
Female	0.308***	-0.0705***	-0.0819***	0.0242**
	(0.0190)	(0.0171)	(0.0191)	(0.0112)
Number of Siblings	-0.0100	0.00797	0.00390	0.0368***
	(0.0147)	(0.0134)	(0.0141)	(0.0135)
Birth Order	-0.0197	0.0143	0.0164	0.0889***
	(0.0188)	(0.0151)	(0.0146)	(0.0166)
Age (1997)	0.0774***	-0.0384***	-0.0423***	0.0288***
	(0.00581)	(0.00427)	(0.00444)	(0.00429)
Observations	3,086	3,087	3,087	3,085
Province FE?	Y	Y	Y	Y
Lincom Final - Sibling	0.101	-0.0102	-0.0352	0.126
SE Final - Sibling	0.114	0.113	0.125	0.125

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

Table 14: Effect of Crisis on Coresidency with Parents and Siblings (with ability controls interacted with Final)