# Consequences of Interactions between Resident and Nonresident Kin

## Evidence from Human Capital Accumulation during the 1997-8 Indonesian Financial Crisis

Paul Karner*

**Job Market Paper**

November 2011

**Abstract**

The terms "family" and "household" are used interchangeably in the literature. But do interactions between family members residing in separate households affect individual outcomes? I address this question by testing whether children's human capital accumulation was affected by the earnings shocks of their nonresident kin during the 1997-8 financial crisis in Indonesia. The crisis produced sudden, heterogeneous changes in the real earnings of Indonesian workers, providing a natural experiment for conducting this test. Earnings shocks to children's nonresident kin – including extended family and relatives living in other districts – significantly affected their human capital accumulation between 1997 and 2000. These effects were persistent. Results from 2007, nearly a decade after the crisis hit, indicate that the shocks affected children's ultimate educational attainment. I explore several possible channels of causation and find evidence that intra-family transfers, underpinned by *ex-post* altruism as opposed to *ex-ante* insurance contracts, were important. Additional evidence highlights the role of family networks as a source of vulnerability to households reliant upon transfers to finance human capital investments. The results have implications for the design of surveys, as well as the targeting and evaluation of policies aimed at improving educational outcomes and protecting individuals from the long-term consequences of economic shocks.

Keywords: Family, social networks, remittances, economic crisis, Indonesia
JEL codes: O1, O53, D1, F22, I25, L14

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*PhD Candidate, Department of Economics, Boston University. 270 Bay State Road, Boston, MA 02215 USA. pkarner@bu.edu. http://people.bu.edu/pkarner. I am indebted to my dissertation advisor Dilip Mookherjee. I also wish to thank Robert E. B. Lucas, M. Daniele Paserman, John R. Harris, Kehinde Ajayi, Kevin Lang, Rizki Nauli Siregar, and Wesley Yin. Financial support from the Department of Economics and the Institute for Economic Development at Boston University, and assistance with the data from Fera Hermawati and Dewi Sri Wahyuni of the BPS in Indonesia, Christine Peterson at RAND, and Yahya Jammal, are gratefully acknowledged. Xiaying Gao provided excellent research assistance.

# 1 Introduction

The family has long been recognized as central to the determination of individual outcomes. Yet much of the economics literature has focused on models of household-level decision making (e.g. Bourguignon, et al., 2009), and empirical studies typically employ household-level data and have households as their unit of analysis.[1] The United Nations defines a household "...based on the arrangements made by persons, individually or in groups, for providing themselves with food and other essentials for living..."[2] By contrast, the family defies categorization by physical boundaries. Most individuals have family members who reside in different households – i.e. "nonresident" family members.[3]

Of what relevance are nonresident family members to individual wellbeing and the design of economic policy? This paper sheds light on this question by testing the hypothesis that the earnings changes of a child's nonresident kin affect her human capital accumulation. The 1997-8 financial crisis in Indonesia produced relatively sudden and extremely heterogeneous changes in the earnings of workers throughout the country. The wide dispersion of these changes, combined with detailed panel data on school-aged children, their households, and their nonresident kin, enables me to estimate the impact of the nonresident family's earnings shocks on children's human capital accumulation.

The crisis is central to my ability interpret the association between nonresident family members' earnings shocks and children's human capital accumulation as causal. Adequate treatment of the relevant endogeneity concerns is, in other circumstances, likely to leave little remaining variation with which to identify this effect. The crisis produced wide dispersion in workers' earnings changes that was correlated with their pre-crisis characteristics. This enables me to construct a proxy for the earnings changes of children's nonresident kin.

The main part of my analysis provides evidence on the medium-run effects of nonresident family members' earnings shocks on children's human capital accumulation. I regress changes in children's enrollment status and number of grades completed between 1997 and 2000 on proxies for the earnings shocks of their relatives living elsewhere within Indonesia. Conditional upon the earnings changes of *co-resident* family members, larger earnings declines of a child's nonresident grandparents, uncles and aunts, and older sib-

[1] Bergstrom (1997) surveys this literature.

[2] United Nations, 2008. Beaman and Dillon (2009) provide fascinating evidence from Mali on the hazards of differing household definitions across data sources. The definition of a household in the Indonesia Family Life Survey (the main data source for this study) is "...a group of people whose members reside in the same dwelling and share food from the same cooking pot." (Strauss, et al. (2009), p. 4).

[3] "Family" is conceptually neither broader nor narrower than "household." In the US Census, for example, households often contain multiple families since "family" means immediate kin. See IPUMS USA (2011).

lings increased her probability of dropping out of school and decreased the number of grades she completed. The effects seem to have been larger for children who were close to or had recently completed the transition from primary to junior secondary school when the crisis hit. Supplementary results demonstrate that these effects arise from the earnings shocks of family members living in other districts, in addition to those living within the child's own district. Conditional on the earnings changes of other nonresident kin, shocks to nonresident uncles and aunts significantly affected children's human capital accumulation, suggesting that the earnings of both nuclear and extended family members are important. Finally, nonresident family members were a source of vulnerability: the human capital accumulation of some children actually appears to have been disrupted by below-average shocks to nonresident family members' earnings.

These effects were persistent. Results from 2007, nearly a decade after the crisis hit, indicate that shocks to nonresident kin produced differences in children's ultimate educational attainment. A one standard deviation increase in my measure of nonresident family members' earnings changes caused a child to complete about 0.14 more grades by 2007. This effect is large in light of existing estimates of the return to additional schooling in Indonesia.

What mechanisms produced these results? The evidence points to a link between nonresident family members' earnings and parental resources. Changes in nonresident family members' earnings are positively associated with changes in household expenditures. Financial transfers exchanged between resident and nonresident kin appear to have been important. Changes in nonresident family earnings are significantly associated with changes in transfers between resident and nonresident kin whose incomes were relatively unequal *ex-ante*. For children in this same group of households, nonresident kin's earnings shocks generated gaps in school achievement. This form of heterogeneity in transfer functions is consistent with a simple model of transfers between altruistically-linked family members, as opposed to transfers underpinned by *ex-ante* insurance contracts.[4] I also investigate the extent to which changes in patterns of family co-residence and peer effects between family members may have played a role, but find no robust evidence that these channels were important.

My results contribute to our understanding of the role that family networks play in determining individual outcomes. Udry and Conley (2005) argue that the boundaries of African households are often "mobile and permeable," emphasizing the importance of social networks in which individuals are embedded. My results suggest that household boundaries are permeable in non-African contexts as well. Complementing evidence from

[4]Previous studies that examine altruism as a possible motive for private transfers ignore the possibility that such transfers may be inoperative at low levels of inequality. See, e.g., Altonji, et al. (1997) and Park (2003).

the emerging literature on social networks within one's village, my findings highlight the importance of *nonlocal* network members.[5] Indeed, more than sixty percent of the nonresident family members examined in this study live in a different village than the sample household. Moreover, the economic significance of nonresident family members seems likely to increase over time as countries develop and households split apart, often across distant locations. Ignoring links to these family members misses an economically significant and increasingly important determinant of individual wellbeing.

Recent studies of migration and remittances also suggest that nonlocal network members are important.[6] However, such studies are incapable of fully capturing the importance of family networks for three reasons. First, the "migrants" that are typically the subject of such studies consist of any individuals who previously lived in the household under study.[7] Hence, looking across households, "migrants" can be comprised of a variety of different kinds of family and non-family members. Second, since international migrants are typically positively selected from the population (see Grogger and Hanson, 2011), households with such migrants are not representative. Finally, since migrants (as typically defined) are current or former household members, studies of these migrants do not capture the role of family members who are not, and perhaps never were, part of the household roster.[8]

The results of this study, by contrast, are primarily based upon a fixed set of relationships: those between children and their nonresident grandparents, uncles/aunts, and siblings. Studying these relationships does not require me to impose particularly restrictive selection criteria since most children in my sample had at least one employed nonresident family member. And the significance of nonresident, *extended* family members' earnings shocks demonstrates the importance of family members who are not migrants.[9]

My results also shed light on the role of "traditional" institutions in the development process, particularly during periods of rapid structural change. Theoretical research highlights the potential for networks to give rise to community effects that accentuate restrictions on income mobility.[10] Recent empirical studies provide some support for this pessimistic view.[11] Such restrictions on mobility can reduce economic growth.[12] By

[5]See, for example, Conley and Udry (2010) and Angelucci, et al. (2010).

[6]Hanson and Woodruff (2003), Amuedo-Dorantes and Pozo (2006), and Ashraf, et al. (2009). See Rapoport and Docquier (2005) for a recent survey.

[7]See, for example, Adams, et al. (2008), Amuedo-Dorantes (2010), and Yang (2008).

[8]The focus on migrants rather than family networks may explain why a large fraction of households apparently receive remittances from non-migrants. See, e.g., Amuedo-Dorantes and Pozo (2010).

[9]This study also sheds light on rural-rural migration by demonstrating the economic importance of cross-village linkages between individuals. Rural-rural migration, which is neglected by the literature, is more common in low-income countries than rural-urban migration (Lucas, 2007).

[10]Greif (1994), Kranton (1996)

[11]Munshi and Rosenzweig (2006), Munshi and Wilson (2011)

[12]Mookherjee and Ray (2003)

contrast, other studies view social networks as an asset, highlighting their role in learning and resource-sharing that can lead to greater efficiency in the use of technology, sharing of risk, and financing of investments in the face of incomplete markets and indivisibilities.[13] Finally, there is much speculation on the implications of the development process for traditional institutions.[14]

My results show that the family, irrespective of physical boundaries, was an important institution during the Indonesian financial crisis despite three preceding decades of rapid industrialization. Consistent with both optimistic and pessimistic views of social networks, the nonresident family in Indonesia seems to be both an asset and a liability. Before the crisis, some households may have been reliant on transfers from nonresident family members to help finance the education of young children: households' transfer receipts in 1997 are positively associated with the presence of school-aged children.[15] However, adverse earnings shocks to nonresident family members negatively and persistently affected children's human capital accumulation.

This study also contributes to the literature on risk-sharing.[16] Altruistically-motivated transfers may be an important *ex-post* risk coping mechanism for households, much like assets or buffer stocks.[17] The evidence presented here suggests that such transfers have important consequences for individual wellbeing. In addition, the importance of both local and nonlocal family members documented in this study may help explain the wide variety of responses to risk and income shocks across households observed by Townsend (1995).

Finally, this paper contributes to the growing literature documenting the longer-term effects of individuals' early-life environments.[18] A number of these studies measure variation in early-life conditions that may have also had direct health consequences, in addition to economic consequences, or measure effects on outcomes such as wages and mortality. This study connects economic disruptions to changes in medium- and long-term human capital accumulation, providing direct evidence on human capital as a possible mechanism through which other outcomes may have been affected. It also provides insight into

[13]Conley and Udry (2010) and Angelucci, et al. (2010), Munshi (2011).

[14]Polanyi (1944), Putnam (1993). Miguel, et al. (2006) provides evidence of interactions between migration patterns associated with the development process in Indonesia before the 1997 financial crisis, and measures of "social capital" such as co-residence patterns and divorce rates.

[15]Angelucci, et al. (2010) argue that some households used their PROGRESA cash transfers to help finance the secondary schooling of children of family members residing in the same village.

[16]See, e.g., Rosenzweig and Stark (1989), Townsend (1994), Paulson (2000), Ligon, et al. (2002), and Bourguignon, et al. (2009). Fafchamps and Lund (2003) and Witoelar (2005) reject the hypothesis of full risk-sharing between family members. The latter is based on the same survey data used in this study.

[17]See Kazianga and Udry (2006). Yang and Choi (2007) provide evidence that international remittances may provide households with insurance.

[18]See, e.g., Gould, Lavy, and Paserman (2004 and 2011), Almond (2006), Van den Berg, et al. (2006), and Oreopolous, et al. (2008), Maccini and Yang (2009).

how the long-term effects of shocks disrupting human capital accumulation may vary by the age of exposure to the shock.

There are several policy implications. First, just as connections between financial institutions can increase the hazard of default and give rise to systemic risk, connections between family members – coupled with spatial and income mobility – can be a source of vulnerability and may impede the development process, especially during a crisis. Policies designed to guard against this vulnerability may be particularly helpful in promoting development.

More generally, the results of this study suggest that policies designed to promote financial inclusion and enhance social safety nets can have spillovers to locations far away from targeted households.[19] Failure to account for these spillovers will cause the estimated benefits of such policies to be understated. Accounting for links to nonresident kin may enhance policymakers' ability to target and evaluate policies designed to smooth fluctuations in individuals' wellbeing.

## 2 The 1997-8 financial crisis

In July 1997, the devaluation of Thailand's currency marked the beginning of the East Asian Financial Crisis. In Indonesia, the second half of 1997 saw increases in exchange and interest rate volatility and major strain on the financial sector in connection with the pullout of foreign capital. Yet most Indonesians were relatively unaffected by the crisis until the beginning of 1998.[20] In January, President Suharto released a budget that appeared to be incompatible with terms previously agreed upon with the IMF, and the crisis became much more severe. The Indonesian rupiah lost more than half of its value relative to the major world currencies within a few days. The government made drastic cuts in subsidies and in large public projects. The price of food staples overall increased by more than 80 percent, and in a span of less than two years, rice prices more

[19] Angelucci and De Giorgi (2009) present evidence of consumption spillovers to family members within the same village.

[20] Setiawan (2000), p. 43, Frankenberg, et al. 2003, p. 284. There were some public protests in Indonesia during the second half of 1997 (see Sanger, 1997). The "pre-crisis" data used for this study comes either directly from 1996 or from survey questions administered during 1997 but in reference to the previous year. Perhaps more troublesome on a day-to-day basis for many was the effects of the forest fires on Sumatra and Kalimantan (Borneo) between August and November 1997, and again in Southeastern Kalimantan between March and July 1998. The fires, which burned out of control due to the extreme drought brought on by the El Niño weather phenomenon, spread haze throughout large parts of Indonesia and disrupted agriculture and tourism (Jayachandran (2009) and Smith, et al. (2002)). Southern Sumatra and Kalimantan, where the fires were concentrated, were most affected by the haze. Northern Sumatra was also heavily affected, while Java was relatively unaffected. Jayachandran (2009) and references therein analyze the health consequences of the fires.

than doubled in 10 of Indonesia's 27 provinces.[21] That the crisis hit during the worst drought Indonesia had experienced in 50 years added insult to injury.[22] The interaction between the drought, which lowered crop yields from the 1997 harvest, cuts in food and fuel subsidies, and the massive exchange rate depreciation in January, which made food imports prohibitively expensive, produced widespread shortages. These shortages, coupled with increasing urban unemployment and the government's perceived ineffectiveness, resulted in rioting. Ultimately, the political regime itself was compromised. In the two years following the onset of the crisis, the Indonesian economy fared much worse than most others in the region. Large changes in prices, interest rates, and real wages were associated with a severe decline in economic growth and increases in poverty.[23] Figure 1 presents a timeline of the crisis.[24]

*Heterogeneous effects*

The effects of the crisis throughout Indonesia were extremely heterogeneous. There was a spectacular collapse in the spatial arbitrage of prices and wages, attributable in part to the country's geographic fragmentation. Levinsohn, et al. (2003) document tremendous geographic and product-level dispersion in the price changes of consumer goods. There was also substantial heterogeneity across regions and industries in real wages and employment changes. Figure 2 depicts substantial dispersion in aggregate earnings changes both across industries and across provinces, within a given industry, between 1996 and 1998.[25]

Tremendous heterogeneity existed along other dimensions as well. Fallon and Lucas (2002), Smith, et al. (2002), and Frankenberg, et al. (2003) document increases in self- and informal-employment along with an increase in the number of unpaid family workers. Naturally, this was associated with gender differences. While overall employment rates fell between 1997 and 1998, they actually increased among rural females.[26] Wage and employment changes also varied by education and pre-crisis wage levels.

These patterns of heterogeneity marked, to a large extent, a reversal in an ongoing trend before the crisis in which employment was shifting from the agricultural to the mod-

[21] PBS (2011), Levinsohn, et al. (2003)

[22] Much of Indonesia was abnormally dry during 1997 and 1998, but it was the eastern province of Irian Jaya that bore the brunt of El Niño. The drought there lasted past the 1998 monsoon season, causing widespread hunger and fatalities due to dehydration. See ReliefWeb (1998). Irian Jaya was not covered by the household survey used for this study.

[23] World Bank (2011)

[24] The recovery of Indonesia's economy was relatively protracted. While the per capita incomes of most other countries in the region had recovered by 2000, Indonesia's did not reach its pre-crisis high until 2004 (World Bank, 2011).

[25] Data tabulated from Indonesia's National Labor Force Survey (SAKERNAS) and the Indonesia Family Life Survey (IFLS), the two main data sources used in this study.

[26] Smith, et al. (2002), Table 2

ern, higher-wage sector. As one study of the crisis notes, "...there was little connection between the regional distribution of the impact of the shock and the regional distribution of pre-shock poverty."[27] There was good reason for this. The modern, import-intensive industries such as textiles and electronics manufacturing, and industries dependent upon short-term debt such as construction and real estate, were hardest-hit by the currency devaluation, investment flight, and credit constraints due to bank closures and high lending interest rates. The close linkages of these industries to (mostly non-tradeable) financial services and transportation resulted in declines in those industries as well. Meanwhile, the export and agricultural sectors benefited to some extent from terms-of-trade improvements.[28]

*Household responses*

Households responded to the crisis by selling assets (primarily gold jewelery) and by cutting expenditures on durable and semi-durable goods and investment in education. There were big cuts in educational expenditures among most households, and the poorest households may have cut expenditures on education proportionally more than other types of expenditures. The differential effects for poor households were probably due in part to a lack of assets with which to smooth expenditures, combined with a tightening of liquidity constraints for all households. To the extent that credit and insurance markets were ever functioning, the crisis rendered them much less effective. Credit became unaffordable as interest rates skyrocketed, reflecting high risk premia, inflation, and tight monetary policy.[29] Insurance, borrowing, and intra-community redistribution were likely to function less well as smoothing mechanisms, forcing households to resort to more costly ones and to make deeper budget cuts. These cuts are reflected in existing evidence of declines in school enrollments, which are particularly striking in light of the rapid increase in enrollment rates in the years before the crisis. National data reveal outright declines in enrollment rates among children of almost all ages between 7 and 15 years.[30]

The employment effects of the crisis induced massive net migration back to rural areas (although there were high absolute levels in the opposite direction as well), as well as return migration from Malaysia.[31] Complementing this migration were changes in household structure and co-residence patterns.[32]

[27]Pritchett, et al. (2002), p. 6.

[28]Setiawan (2000), Fallon and Lucas (2002), Levinsohn, et al. (2003)

[29]Fallon and Lucas (2002). Frankenberg, et al. (2003) also present survey evidence, fielded during the second half of 1998 from a 25 percent sub-sample of IFLS households, that there were steep declines relative to 1997 in both the incidence of borrowing and amounts borrowed by households from the formal credit sector.

[30]Thomas, et al. 2004, Table 2a

[31]Fallon and Lucas (2002).

[32]Frankenberg, et al. (2003)

*Government response*

The Indonesian government responded to the crisis by making large cuts in overall expenditures (including public education) and launching a new social safety net known as the JPS.[33] Most programs within the JPS had not previously existed in Indonesia. Although launched in 1998, regional budget politics prevented many of the programs from becoming active until the second half of the year. The JPS programs provided rice subsidies, managed public works and other employment-creating projects, subsidized education and health, and gave block grants to villages. The subsidized rice (OPK) program was by far the largest in terms of the fraction of the population covered.[34] The "scholarships" (SBG) program targeted individual students and gave block grants to schools. The block grants were targeted at the "poorest" schools (i.e. those serving poorest students), and school committees allocated the grants to individual students on the basis of numerous criteria related to their poverty status and subjective likelihood of dropping out. The scholarships, which ranged between Rp10,000 and Rp30,000 per month depending upon the student's schooling level, were intended to exceed recipients' school fees.[35] Yet coverage of the eligible population was relatively low.[36] Although expenditures on the JPS programs constituted a relatively large share of the government's (declining) budget, the heterogeneous impact of the crisis made targeting very difficult,[37] and there is evidence that the programs were fraught with corruption.[38]

# 3 Empirical Strategy and Data

## 3.1 Conceptual framework

*Human capital investment*

A model of optimal human capital investment (e.g. that of Baland and Robinson, 2000) is a natural starting point for thinking about the expected effects of nonresident family members' earnings on children's human capital accumulation. In such models, changes in parents' current resources can give rise to changes in their utility-maximizing levels of investment in their children's human capital in a number of ways. For example, parents

[33]Pritchett, et al. (2002), and Sumarto and Suryahadi (2001) summarize and evaluate the various JPS programs.

[34]Sumarto and Suryahadi (2001) estimates that the coverage ratio for eligible recipients with below-median income was 40 percent.

[35]Pritchett, et al. (2002)

[36]Sumarto and Suryahadi (2001) estimates that the scholarships reached only 5.4 percent of poor students and 3.3 percent of non-poor students, although coverage among poor students in junior secondary school may have been somewhat higher.

[37]Pritchett, et al. (2002)

[38]Olken (2006) documents a substantial share of "missing rice" in the OPK program.

may face binding liquidity constraints which prevent them from internalizing the consequences of under-investing in their children's human capital by breaking the link between current parental resources and children's future income. It seems likely that many households experienced a tightening of such constraints during the crisis. Education may also be viewed by parents as a consumption good. In both cases, a decrease in their resources reduces parents' expenditures on their children's human capital.

*Linkages to nonresident kin*

There is some evidence suggesting that intra-family transfers may have been important to households' ability to cope with the crisis.[39] Changes in transfers exchanged with nonresident kin are linked directly to parental resources.[40] Among households initially dependent upon transfers, a decline in transfer receipts that is not replaced by increased income from other sources will reduce available parental resources.

Other possible links between nonresident kin's earnings and children's human capital include changing cohabitation patterns and peer effects between family members. Changes in household size and composition were common during the crisis, and constituted an important coping mechanisms for households.[41] In addition, peer effects between family members could arise from interdependencies between households' human capital investment functions or imperfect information about the possible effects of the crisis. I discuss possible forms and implications of these links in greater detail in Section 5.

### 3.2 Description of data

The database used for this study combines child- and household-level information from successive waves of the Indonesia Family Life Survey (IFLS), a nationally-representative longitudinal household survey, with data on changes in the earnings of workers obtained from the 1996 and 1998 rounds of Indonesia's national labor force survey (SAKERNAS). The IFLS was first fielded in 1993 in 13 of the 27 provinces Indonesia had at the time. The population of these provinces constituted over 80 percent of Indonesia's total population in 1993. Three subsequent waves were fielded in 1997, 2000, and 2007.[42] The data used in this study come primarily from the latter 3 waves, although I use the 1993 wave to a limited extent to fill in missing data as discussed below.

[39] Frankenberg, et al. (1999)

[40] Parental resources, in turn, may be linked to the shadow value of a child's time via parental labor supply.

[41] Thomas, et al. (2004).

[42] A follow-up survey based on a 25 percent sub-sample of the 1993 survey frame was administered in the initial wake of the crisis in 1998. The data from that survey were not publicly available as of this writing.

The focus of my main analysis is on changes in children's human capital accumulation between 1997 and 2000, for which I primarily rely on the second and third main waves of the IFLS. In Section 6, I study the longer-run impact on children's educational attainment by analyzing outcomes in 2007, for which I rely on the fourth wave of the IFLS. The timing of the 1997 IFLS survey wave is important to this and other studies that use the IFLS to analyze the effects of the Indonesian crisis. Ninety-five percent of the household interviews were completed by the end of December 1997, before the crisis intensified (see Figure 1).[43]

This study also uses data from the 1996 and 1998 rounds of the SAKERNAS, administered to more than 65,000 Indonesian households in August of each year. The data is used to construct proxies for the initial wages and changes in earnings of households' nonresident kin, as discussed in greater detail below.

### 3.3 Estimation samples

The child-level sample used for my main analysis tracks children's progress through school from 1997, immediately before the crisis, to 2000. The sample consists of children ages 4–19 years in 1997. The main household-level sample is based on the households in which these children were living in 1997. A distinguishing feature of the IFLS is that it attempted to track individuals who moved between survey rounds. This enables me to follow households across survey rounds as they move or split apart. Household-level information from 1997 and subsequent waves, including information on nonresident kin, was obtained by matching data to the identifiers of the heads of these households. I adopted this strategy, as opposed to matching data to the same household identifiers across rounds, to ensure that household-level data were pertinent to the household in which the parents of children in my sample were living in subsequent waves, even when their household identifier changes due to a move or household split.[44] Tables 1 and 2a present basic descriptive statistics of the households and children in my sample. The Appendix contains more details on sample construction.

Attrition from my sample is relatively low, primarily because data on individual children continues to be available in the IFLS as these children move through school and into the labor force. Were I not able to follow these children, my sample would have been smaller and more selective. In the Appendix, I test whether attrition is a significant source of bias in my results and find no evidence that it is.

The IFLS contains detailed data on nonresident grandparents, uncles/aunts, and older

[43] Frankenberg and Thomas (2000), p. 19.

[44] About 8 percent of children in my sample lived in households that moved locations between 1997 and 2000.

siblings of the sample children, who I henceforth refer to as the child's nonresident family members or kin.[45] Table 3 presents characteristics of the nonresident kin of households in my sample. Each household has an average of about 10 nonresident family members, and all households had at least one nonresident family member. The vast majority of nonresident kin were located within Indonesia. The focus of this paper is on households' interactions with these nonresident family members. Most commonly, the household head and/or spouse indicated that they had siblings who lived elsewhere. On average, the head and spouse had about 7 nonresident siblings between them. Just over forty percent of children in my sample have at least one nonresident sibling, and about three-quarters of children in my sample have at least one nonresident grandparent.

Nonresident family members are roughly evenly divided in their locations between the same *kecamaten* (i.e. district, the administrative division two levels below the province and akin to a county in the United States) as the household and a different *kecamaten* within Indonesia.[46] Nonresident siblings are on average somewhat more spread out geographically than nonresident uncles/aunts and grandparents.

Nonresident kin were distributed across a variety of industries in 1997. Among the roughly half of nonresident grandparents who were working, a majority (64 percent) were employed in agriculture. Trade and tourism was their next most popular industry. By contrast, the roughly two-thirds of nonresident uncles/aunts and siblings who were employed in 1997 were somewhat less concentrated in agriculture. Working nonresident uncles/aunts were divided between social and community services, trade and tourism, and manufacturing, while working nonresident siblings were employed in sizeable shares in all industries.

### 3.4 Empirical model and variables

The goal of the empirical analysis is to measure the effect of nonresident family members' earnings shocks on changes in children's human capital. Working with changes in, as opposed to levels of, earnings and human capital has the advantage of eliminating many of the confounding effects of time-invariant child characteristics. Ideally, one would want earnings shocks randomly distributed across households' nonresident family members.

[45]The data in the IFLS technically refer to the nonresident parents, siblings, and children of the household head and spouse. These correspond to nonresident grandparents, uncles or aunts, and siblings for the vast majority of children in my sample. Roughly 8 percent of children in my sample are grandchildren of the household head and spouse (see Table 2a). My main results are not sensitive to the presence of these children in the sample.

[46]The treatment of nonresident family members for whom there were missing data is discussed in the Appendix. The locations of nonresident grandparents were unavailable in the data for the 1997 survey wave, so I report (and make use of) their location relative to the household in 1993, which I obtained from the first IFLS survey wave.

The effect of the earnings shocks on children's human capital accumulation could then be measured by estimating the following specification:

$$\Delta y_{iht} = \beta_1 \Delta e_{ht}^{NRF} + \varepsilon_{iht} \tag{3.1}$$

The variable $\Delta y_{iht}$ is the change between times $t-1$ and $t$ in a measure of the human capital outcomes (such as enrollment status or the number of grades completed) of child $i$ living in household $h$. The variable $\Delta e_{ht}^{NRF}$ measures the change in nonresident family members' earnings over the same period. If nonresident family members' earnings directly affected available parental resources as suggested above, then $\beta_1$ should be positive.

In order to obtain an unbiased estimate of $\beta_1$, it must be the case that, in the absence of a change in their nonresident family members' earnings between 1997 and 2000, children's levels of human capital would have continued to follow their pre-crisis trends. This seems unlikely since the earnings changes of *resident* family members are correlated with both children's human capital accumulation and the earnings shocks of their nonresident family members. I address this issue by controlling for a measure of the resident family's earnings changes, $\Delta e_{ht}^{RF}$.

The relationship of family members' earnings changes to ongoing trends in human capital accumulation is also important to identifying $\beta_1$. The estimated effect of earnings on human capital accumulation would be biased upwards if the crisis produced earnings changes that were positively correlated with ongoing trends in human capital accumulation. However, there is good reason to believe that there was no systematic positive relationship between the earnings changes produced by the crisis and ongoing trends in human capital accumulation. The reason is that the crisis had a disproportionately adverse impact on higher-wage industries. Absent the crisis, children whose family members were employed in these industries would have likely obtained a greater-than-average amount of human capital between 1997 and 2000. The earnings changes generated by the crisis were, if anything, *negatively* related to ongoing trends in human capital accumulation, potentially biasing the estimate of $\beta_1$ (and $\beta_2$) *downwards*.

I do not observe the actual earnings of nonresident kin for the vast majority of children in my sample. Instead, I construct proxies for $\Delta e_{ht}^{NRF}$ based on the experience of workers with characteristics similar to those of children's nonresident kin. I did this by combining information on the pre-crisis characteristics (industry of employment, province, educational attainment, and gender) of nonresident kin, reported in the IFLS data, with estimates of the average earnings and aggregate employment changes of all Indonesian workers with those characteristics. I derived these estimates from two rounds of Indonesia's National Labor Force Survey (SAKERNAS), which straddle the onset of the crisis,

by dividing surveyed workers into cells defined by their gender and one of eight industries, twenty-seven provinces, two education levels (primary school or less, more than primary school), and by computing the average earnings and employment levels within those cells.[47]

The earnings shocks actually experienced by nonresident kin likely consisted of disruptions to their employment, as well as earnings changes arising from wage and price changes conditional upon employment.[48] Hence, employment and earnings changes of Indonesian workers with similar characteristics may convey non-overlapping information about the earnings shocks experienced by nonresident kin. I therefore construct three proxies for nonresident kin's earnings shocks: one based on the change in average earnings of Indonesian workers ("Proxy 1"), a second based on the change in aggregate employment of Indonesian workers ("Proxy 2"), and a third that combines changes in earnings and employment of Indonesian workers with characteristics similar to each nonresident family member into a composite proxy ("Proxy 3"). This composite proxy summarizes the change in aggregate earnings of all Indonesian workers with characteristics similar to a child's nonresident kin.

In the empirical analysis, I first establish that the earnings- and employment-based proxies both summarize relevant information about nonresident family members' earnings shocks (and that my main results are qualitatively unchanged when using proxies constructed by different methods). I then turn to the composite Proxy 3 and use it throughout the rest of the paper. Proxy 1 is equal to the change, between 1996 and 1998, in the log total earnings of all 1997 nonresident kin, where each nonresident kin's earnings in a given year equals the average earnings of all workers with similar characteristics in that year. Proxy 2 is equal to the simple average (across all nonresident kin) of the percentage change in aggregate employment of all workers with similar characteristics to each 1997 nonresident family member. The construction of these proxies is described in greater detail in the Appendix. Proxy 3, the composite proxy used throughout most of the paper, is equal to the simple average of the percentage change in aggregate earnings of Indonesian workers with similar characteristics to each nonresident family member. It

[47] In cases where these cells contained fewer than 100 workers, I estimated earnings and employment levels from more broadly-defined cells defined by industry and province only. The Appendix contains more details on this procedure. The average number of observations underlying the estimates I use is 293, and the minimum is 91.

[48] Employment and real wage changes during this period are negatively correlated in the SAKERNAS data. On average, real wages declined by about 7 percent while aggregate employment increased by about 13 percent.

is defined as follows:

$$\widetilde{\Delta e_{ht}^{NRF}} \equiv \frac{1}{N_{h,t-1}^{\text{NRF}}} \sum_{geil} [N_{h,geil,t-1}^{\text{NRF}} \; AggEarnCh_{geil}] \tag{3.2}$$

$AggEarnCh_{geil}$ denotes the percentage change, between 1996 and 1998, in the aggregate earnings of Indonesian workers (including the self-employed) of gender $g$ and education level $e$, employed in industry $i$ and province $l$. I construct the proxy from the period 1996-1998 rather than 1997-2000 in order to limit the scope for bias arising from reverse-causality that might be induced by anticipation of the crisis, and to measure the initial impact of the crisis rather than the subsequent recovery underway by 2000.[49] Table 3 contains summary statistics of the proxy variables. The Appendix contains further details on their construction.

In contrast to nonresident kin, I do observe the (self-reported) earnings of the household head and other employed household members. I use this measure of the head's earnings in my benchmark specification to ensure that my results are not driven by a failure to capture the effects of the resident family's earnings changes. A drawback of this approach is that introducing an endogenous regressor could bias the coefficient estimates of the other regressors. Moreover, there is an argument to be made for treating the resident and nonresident family earnings symmetrically by using proxies for both. I address this issue by gauging the robustness of my results to replacing the household head's actual earnings change with a proxy, constructed in the same way as the proxy for nonresident kin's earnings changes. But it is worth emphasizing that the interpretation of the coefficient on the actual change in the head's earnings in my main specifications is not intended to be causal.

My estimating equation takes the following form:

$$\Delta y_{iht} = \beta_1 \widetilde{\Delta e_{ht}^{NRF}} + \beta_2 \Delta e_{ht}^{RF} + \beta_3 \mathbf{X}_{iht} + \varepsilon_{iht} \tag{3.3}$$

The dependent variable used in my main analysis is the change in the child's enrollment status (defined as a dummy variable equal to 1 if the child is enrolled in school at time $t$), between 1997 and 2000. Supplementary specifications adopt an alternative measure of human capital based on achievement: the change in the highest number of grades competed by the child between 1997 and 2000. In order to limit the extent of downward bias in $\beta_1$ and $\beta_2$ that may arise from the tendency for the crisis to have a disproportion-

[49] An added advantage of using the proxies is that they are uncorrelated with individual workers' labor supply responses, so that measured earnings changes reflect only the initial impact of the crisis. Hence, the proxy can be thought of as capturing prospective changes – i.e. what would have happened to an individual's earnings if there had been no labor supply adjustment.

ately adverse impact upon industries that paid higher wages *ex-ante*, I introduce controls for the average pre-crisis earnings level of nonresident family members and the pre-crisis earnings level of the household head. I also introduce several additional controls: (1) indicators for the presence of nonresident family members who were living outside of Indonesia or were employed in "other" industries for which an earnings proxy could not be computed, or whose industry in 1997 is unknown (I exclude such individuals in the calculation of the proxy variables for nonresident family members' initial wages and earnings changes); (2) a full set of indicators for the ages of the child and of the household head in 1997; (3) the employment status of the household head in 1997 and 2000 (the head's initial wage and earnings changes are set to zero for heads who were unemployed); and (4) the child's school enrollment status in 1997. The vector $\mathbf{X}_{iht}$ denotes these controls in (3.3). Tables 2b and 2c present descriptive statistics of the dependent variables by age cohort.

# 4 Results, 1997–2000

*Overall effects on enrollment*

Table 4 presents my main estimates of the medium-run relationship between the earnings shocks of nonresident kin and children's human capital accumulation. The dependent variable is the change in an indicator of the child's enrollment status between 1997 and 2000. Standard errors permit heteroscedasticity of unknown form and are clustered at the community level.[50]

In Column 1, the change in enrollment status is regressed on the change in the head of household's earnings between 1997 and 2000. The point estimate for the change in head's earnings is positive but very small and insignificantly different from zero. Column 2 adds the head's initial earnings as a control. Children whose father is employed in higher-wage industries in 1997 are more likely to be enrolled in school in 2000, pointing to a pattern of divergence in human capital accumulation among children with parents of different incomes: children from initially higher-income households are, at the end of the 3 year period, more likely to be enrolled in school. This is consistent with existing evidence from Indonesia and Mexico that children with higher socioeconomic status at baseline have lower chances of dropping out of school in a subsequent period (see Gertler, et al., 2004).

As anticipated, the addition of the head's initial earnings as a control causes the coefficient on the head's earnings change to become much larger in magnitude and statistically

[50]Communities, which contain approximately 20-30 households, are the enumeration area of the IFLS survey.

significant. The point estimate indicates that a ten percentage point larger earnings change for the household head between 1997 and 2000 is associated with a 0.2 percentage point higher probability of enrollment in 2000 (i.e. lower probability of dropout) for a child enrolled in school in 1997.[51]

Column 3 adds the proxy for initial earnings of nonresident kin, along with the earnings-only proxy (Proxy 1) for the change in the nonresident family's earnings between 1996 and 1998. The coefficient estimates on these variables display a similar pattern to those of the resident family. Children whose nonresident family members were employed in higher-wage industries before the crisis are, *ceteris paribus*, more likely to be enrolled in school in 2000. In addition, children whose nonresident kin experienced a more positive earnings change overall are more likely to be enrolled in school in 2000, conditional upon the earnings of their parents. The point estimate suggests that a ten percentage point increase in the nonresident family's earnings results in approximately a one percentage point increase in the probability that the child is enrolled in school in 2000 (about 4 percent of the mean probability of disenrollment for a child enrolled in 1997).

Why is the estimated effect of nonresident family earnings on enrollment much larger than the association between head's earnings and enrollment? With a standard deviation of 86 percentage points, the measured change in head's earnings is much more disperse than that of the proxy for nonresident family earnings, which has a standard deviation of 12 percentage points (see Tables 1 and 3). This is exactly what one would expect from a proxy derived from an average of many workers in the National Labor Force Survey, versus the head's self-reported earnings. This difference in dispersion implies that the change in enrollment probability resulting from a one standard deviation increase in the proxy for nonresident kin's earnings is about 65 percent of the change in enrollment probability associated with a one standard deviation increase in the change in head's earnings.[52]

Column 4 adds the employment-based proxy (Proxy 2) for nonresident kin's earnings change. Its coefficient is positive and significant, and the coefficient on the earnings-only proxy is only slightly reduced. It seems that the proxy for nonresident kin's earnings shocks based on employment changes adds independent explanatory power to the enrollment regression. This is reflected in Column 5, where the earnings- and employment-

[51]This result does not have a causal interpretation. However, replacing the head's actual earnings change with a proxy constructed analogously to Proxy 1 also yields a positive and significant coefficient. These positive effects are consistent with studies from the developed country literature on the effect of parental income on child educational outcomes (e.g. Sacerdote (2002) and Akee, et al. (2010)) as well as with existing evidence of disruptions to children's human capital accumulation among households hit hardest by the Indonesian crisis (e.g. Thomas, et al., 2004).

[52]In addition, the change in head's earnings is measured over the period 1997-2000, while the proxy for the change in nonresident kin's earnings corresponds to the period 1996-1998.

based proxies are replaced by the proxy for nonresident kin's earnings changes that combines earnings and employment. The coefficient estimate on Proxy 3 is positive and highly significant. The point estimate implies that a one standard deviation increase in the proxy for nonresident kin's earnings changes results in a 1.8 percentage point increase in enrollment probability (nearly 8 percent of the mean probability of disenrollment by 2000 for children enrolled in 1997). Since the composite earnings-employment proxy captures the effect of nonresident family earnings changes more parsimoniously than the two separate proxies, I use the specification in Column 5 as my benchmark.

The estimated effect of nonresident kin's earnings changes on enrollment in Column 5 of Table 4 is robust to alternative specifications. In particular, the point estimate is virtually unchanged in magnitude and significance across regressions in which the head's actual earnings change is replaced by a proxy constructed analogously to the nonresident kin proxy and the initial earnings level and change in total earnings of all employed household members *other than* the head are included as additional regressors. When district-by-rural/urban fixed effects are added to the specification in Column 5 of Table 4, the effect of nonresident kin's earnings changes on enrollment becomes smaller in magnitude, but remains significant. See the Appendix Table. In addition, the coefficient on the proxy for nonresident kin's earnings shocks remains positive and highly significant when I restrict the sample to children whose nonresident kin experienced (proxy) earnings changes below the median (about -6 percent). This suggests that particularly adverse earnings shocks to nonresident kin actually caused some children to disenroll from school.[53]

Finally, the results in Column 5 of Table 4 do not appear to be driven by bias arising from measurement error in the proxy variable for nonresident kin's earnings. The results are nearly unchanged when I include as a regressor a variable measuring, for each household, the average number of observations from the SAKERNAS data that were involved in constructing the proxies for nonresident kin's earnings changes, and this variable itself was statistically insignificant. In addition, the results are not driven by children in households for which the head is not their own parent.[54] When I introduce dummies for the relationship of the child to the household head into the specification in Column 5 of

[53]The samples differ slightly across specifications in the Appendix Table due to varying data availability for the different regressors. Restricting the sample of specification in Column 5 of Table 4 to each of the smaller samples yields nearly identical estimates, and hence (the lack of) differences in the coefficient estimates on nonresident kin's earnings in the Appendix Table can be attributed to differences in specification rather than sample.

[54]In particular, the school enrollment status of children whose own *parents* are nonresident might be particularly sensitive to these parents' earnings shocks. As discussed above, about 8 percent of children in my sample live with their grandparents. Since nonresident kin in the IFLS data are classified on the basis of their relationship to the *head* of the household in which the child lives, these children's parents (if nonresident) would be included in my proxy for nonresident kin's earnings shocks since they are the nonresident children of the household head and spouse.

Table 4, and when I restrict the sample to children whose parents are the household head and spouse, the magnitude and significance of the proxy for nonresident kin's earnings shocks is virtually unchanged.

*Effects by age cohort*

I now explore how the effects of nonresident kin's earnings shocks on school enrollments vary by the age of the child at the time of the crisis. Doing so has the potential to shed light on the process that gives rise to these effects, as well as on their welfare implications. Figure 3 plots the coefficient estimates (and 95 percent confidence intervals) of the vector of interactions between the proxy for nonresident kin's earnings shocks and the child's age in 1997.

Caution must be used in drawing inferences from the coefficient estimates in Figure 3 since most are not significantly different from one another. Yet the figure points to some potentially interesting patterns. First, note that while only two of the coefficients are significant at the 5 percent level, all but two of the point estimates are positive. Next, the effects of nonresident kin's earnings shocks on enrollment seem to have been largest for children who were age 11 in 1997 (the coefficient on the 11 year-old interaction term is significantly different from both the 10 and 12 year-old interaction terms at the 10 and 5 percent levels, respectively).

What could account for this phenomenon? The standard school curriculum in Indonesia consists of 6 years of primary school, followed by three years each of junior and senior secondary school. A relatively large fraction of 11 year olds were on track to complete primary school during 1998, just as the crisis in Indonesia reached its peak intensity. Roughly one-fifth of 11 year olds who were enrolled in school in 1997 were already attending their last year of primary school *before* the most severe impact of the crisis hit Indonesia. The same was true for less than 5 percent of 10 year olds. Meanwhile, nearly 30 percent of *enrolled* 12 year olds had completed primary school and were already attending junior secondary school when the crisis hit. It seems that the most severe effects of nonresident kin's earnings shocks may have been concentrated in the age cohort with a relatively large fraction of children making the transition from primary to secondary school during the height of the crisis, and with relatively few children who had already completed that transition.

The pattern in Figure 3 raises the possibility that households may have sought to shield children who were near completion of primary school, while holding back children from enrolling in secondary school. Before the crisis hit, primary school was nearly universal, but there still existed significant gaps in secondary school enrollment. As can be seen in Table 2b, the dropout rate is large for children in this age range, relative to younger

children. *A priori* it seems possible that children who disenrolled from school during the crisis due to their nonresident kin's adverse earnings shocks may not have continued onto secondary school anyway. If this were the case, however, then the effects in Figure 3 would be zero. Rather, children whose nonresident kin experienced more adverse earnings shocks disenrolled from school at a higher rate than their peers whose nonresident kin fared better during the crisis, suggesting that nonresident kin's earnings shocks may have substantially impeded these children's progress through school.

*Effects by distance from and relation to child*

Another important question is whether the effects of nonresident kin's earnings shocks varied by their distance from the household, and by their relationship to the child. Table 5 provides evidence on these questions.[55] Just over half of overall nonresident family members reside outside of the household's district (see Table 3). Therefore, I break out the effect by constructing separate proxies for nonresident family members living within and outside of the child's district in 1997. Column 1 reports the results of this specification. Both coefficient estimates are positive and significant. Although the point estimate for the effect of earnings shocks to nonresident kin living in the same district as the household is slightly larger than that for nonresident kin living in another district, the two effects are statistically indistinguishable. It seems that the effect of nonresident kin's earnings shocks on children's human capital accumulation is not an entirely local phenomenon.

Do nonresident, *extended* family members play a role in determining children's outcomes? Column 2 of Table 5 reports separate estimates of the effect of earnings shocks to uncles and aunts versus the other types of nonresident kin, categories that correspond roughly to extended and nuclear family members. Perhaps surprisingly, the point estimate of the effect of earnings shocks to extended family members is larger in magnitude than that of nuclear family members, although the two are not statistically different from one another. Earnings shocks to nonresident uncles and aunts appear to be just as important as earnings shocks to nonresident grandparents and older children as drivers of changes in children's school enrollment status.

This result complements those of previous studies in the migration literature. The operative definition of a migrant in this literature typically captures only individuals who at some point were members of the household under study, and often focuses on individuals who are still listed in the household roster but are away on a temporary basis. The fact

[55]Sample sizes in Table 5 are slightly smaller due to the need to trim the 1 percent tails of two proxy variables rather than just one. Restricting the sample in Column 5 of Table 4 to be each of these samples does not materially affect the magnitude or significance of the estimated effect of nonresident family members' earnings.

that earnings shocks to children's uncles and aunts – i.e. the siblings of the household head and spouse – play a role in determining household outcomes demonstrates that this definition does not fully capture the influence of nonresident family members.

*Achievement effects*

To what extent did the enrollment effects of nonresident kin's earnings shocks translate into gaps in achievement? In Column 1 of Table 6, I report the same specification as in Column 5 of Table 4, but where the dependent variable is the change in the number of grades the child has completed between 1997 and 2000. The average number of grades completed during this period is 2.0. The coefficients on all variables display a very similar pattern to their counterparts in Table 4. The effect of nonresident kin's earnings changes on achievement is positive, though only marginally significant. The point estimate implies that a one standard deviation larger earnings change of nonresident kin caused children to complete approximately 0.03 more grades between 1997 and 2000, or about 1.4 percent of the mean.

It seems that nonresident kin's earnings shocks resulted in variation in children's achievement levels that remained after the worst of the crisis had subsided. While these medium-run achievement gaps raise the possibility that nonresident kin's earnings shocks had long-term effects, they were not necessarily permanent. These achievement gaps can be made up for over time, possibly with affected children re-enrolling and remaining in school until a later age than they would have otherwise. Of course the welfare effects of disruptions to human capital accumulation hinge critically upon the persistence of any effects. Before analyzing their persistence, I first attempt to shed additional light on the mechanism and channels through which nonresident kin's earnings shocks may have disrupted children's human capital accumulation.

# 5 Links between the resident and nonresident family

## 5.1 Transfers

*Transfers in theory*

Financial transfers constitute one possible link between households and their nonresident kin that could have mediated the enrollment and achievement effects reported above. The motivations underpinning transfers are crucial to detecting their presence in the data and assessing their importance as a channel of causation. To the extent that financial transfers are underpinned by incentive-compatible contracts, as envisioned by Stark and Lucas (1988) among others, binding commitment constraints may limit the extent to which household members are willing to provide support to each other during times crisis. On

the other hand, transfers may be underpinned by altruism. I show below that such transfers in effect provide partial insurance. However, they may be operative in circumstances when binding commitment constraints would preclude or limit transfers underpinned by *ex-ante* insurance arrangements. In addition, unlike *ex-ante* insurance-based transfers, altruistic transfers are not operative unless the amount of autarky inequality between would-be senders and recipients exceeds a certain threshold.

I now sketch a simple model of transfers between two isolated family members based on altruism. Consider a parent $P$ and a child $C$ who have interdependent utility functions given as follows:

$$W_i = u(c_i) + \gamma u(c_{-i}) \quad i = \{P, C\}$$

The parameter $\gamma \in (0, 1)$ governs the level of altruism (which I assume to be symmetric) between parent and child. Suppose that the two parties engage in a game, the timing of which is as follows. Parent and child receive endowments $y_P$ and $y_C$, respectively, which are common knowledge. Upon observing the endowments, each player $i$ chooses an amount of her endowment $t_i \in [0, y_i]$ to transfer to the other player, and then consumes what is left over: $c_i = y_i - t_i \quad i = \{P, C\}$.

I now describe some features of a Nash equilibrium of this game. The level of the transfer $t_i$ is chosen optimally by $i$ taking as given the transfers made by the other player. The optimal value of $t_i$ is characterized by the following inequality:

$$\frac{u'(c_i)}{u'(c_{-i})} \geq \gamma \tag{5.1}$$

If the non-negativity constraint on $t_i$ is slack, then (5.1) holds with equality, and with strict inequality otherwise. Hence, for any ($y_P$, $y_C$) such that $\frac{u'(y_P)}{u'(y_C)} < \gamma$, the parent makes a transfer to her child. Similarly, $C$ would like to choose $t_C$ such that $\frac{u'(c_C)}{u'(c_P)} = \gamma$, and hence for any ($y_P$, $y_C$) such that $\frac{u'(y_P)}{u'(y_C)} > \frac{1}{\gamma}$, the child makes a transfer to the parent. Assume that $y_P > y_C$. If relative endowments are such that $\gamma \leq \frac{u'(y_P)}{u'(y_C)} \leq \frac{1}{\gamma}$, then $P$ prefers to receive a transfer from $C$ while $C$ prefers to receive a transfer from $P$, but the non-negativity constraint on transfers binds for both players. Hence, in equilibrium, strictly positive transfers between players occur when the level of *ex-ante* inequality is sufficiently large relative to the level of altruism that the richer player has a strict incentive to send a transfer to the poorer player. Since the players care about each other less than they care about themselves ($\gamma < 1$), both would prefer to receive transfers from the other (and hence there are no transfers between them) for relatively small amounts of inequality. When the inequality between two players is sufficiently large to give rise to a transfer

from rich to poor, the size of the transfer is just sufficient to equate the marginal utility that the sender derives from her own consumption to the marginal utility that she derives from the consumption of the receiver.

This model dictates that an individual's aggregate net transfers are a convex function of their wealth level relative to the average wealth level of their family. The function is horizontal at levels of low inequality, and then increasing at high levels of inequality. In contrast to the altruistic model, theories of *ex-ante* risk sharing between among family members (such as the one tested by Townsend, 1994), dictate that parties exchange transfers in order to maintain a ratio of marginal utilities agreed upon *ex-ante*. In models of insurance with limited commitment such as that of Coate and Ravallion (1993), transfers are subject to an additional incentive-compatibility constraint which may bind, and consequently cause parties to deviate from this ratio, in instances of relatively extreme inequality. The equilibrium transfer functions stemming from such models are thus *concave* – increasing in an individual's income at low levels of inequality, and horizontal at high levels of inequality where commitment constraints bind.

*Empirical results on transfers*

Using data from the 1997 and 2000 IFLS survey waves on the transfers that households exchanged with their nonresident family members over the 12 months preceding the survey, I explore the effect of nonresident kin's earnings shocks on net transfers that households receive from their nonresident kin. Table 7 provides descriptive statistics on these transfers. One interesting feature of transfers during the crisis is that on average, gross remittance flows declined in magnitude. This can be seen in a decline in net remittances received by households that were net receivers of transfers from nonresident kin in 1997, and in a decline in net remittances sent by households that were net senders of transfers to nonresident kin in 1997.

Table 8 presents the results of my analysis. The dependent variable in Columns 1 and 2 of Table 8 is the increase between 1997 and 2000 in the sum of net transfers received by the household from nonresident grandparents, uncles/aunts, and older siblings of the sample children, normalized by 1997 household expenditures.[56] All regressions include the same household-level controls used in the previous child-level specifications.

In Column 1 of Table 8, I test the effect of nonresident kin's earnings shocks on transfers using a specification dictated by a model of complete *ex-ante* insurance in which transfers increase in the relative inequality between sender and receiver. In Column

[56]Normalizing transfers by household expenditures is common in the literature on remittances (see, e.g., Yang (2008)), functioning like the log transformation in its smoothing the data while still allowing for non-positive values. Results without the normalization are qualitatively similar although less precisely estimated.

2, I specify a transfer function that is non-monotonic in the initial inequality between sender and receiver. Using measures of each party's income (the household's 1997 per capita expenditures and the proxy for the average wage of the nonresident kin in 1997), I categorize households into one of two groups – "low" or "high" inequality – based on the amount of inequality between the household and its nonresident kin. I construct the inequality groups by breaking the income measures into quartiles. "High" inequality households are those in either the first or fourth quartile of 1997 per capita expenditures and whose nonresident family has an average proxy wage in either the fourth or first quartile, respectively. All other households are categorized as "low" inequality.[57] I then interact the shock variables with these inequality categories.[58]

The results in Column 1 of Table 8 do not reveal any effect of nonresident family earnings on net transfers. In Column 2, nonresident family earnings declines significantly decrease net transfers received from the nonresident family for high-inequality households, but have no significant effect on net transfers among low-inequality households. A one standard deviation decrease in the change in the proxy for nonresident kin's earnings results in an approximately 1.1 percentage point decrease in the change in net remittance receipts as a share of 1997 household expenditures. Table 7 shows that net remittance receipts from nonresident kin declined between 1997 and 2000 by an average of about 3 percent of pre-crisis household expenditures among households that were net receivers in 1997. Hence, this effect amounts to nearly 40 percent of the mean for initial receivers of transfers.[59]

Results from the corresponding child-level regressions are reported in Columns 3 and 4. Recall that nonresident family earnings changes affected both enrollment and achievement. Column 4 of Table 8 shows that nonresident family earnings changes affected the number of grades completed by children in high-inequality households. It seems that differential changes in children's school achievement between 1997 and 2000, caused by nonresident family members' earnings shocks, occurred in the same set of households for which those earnings shocks were significantly associated with changes in transfer receipts. Moreover, the effect is in the direction that one would expect if decreases in

[57]The results are similar if households are instead categorized as "high" inequality if they are only two or more quartiles apart from their nonresident kin.

[58]Column 2 also includes controls for the log of household per capita expenditures (PCE), the main effects of the inequality categories, and interactions between the two.

[59]I attempted explore whether the effect of nonresident kin's earnings shocks on transfer receipts depends upon whether the household is sending or receiving remittances. I did this by restricting the sample in Column 2 of Table 8 to households that were, in turn, net receivers or net senders of transfers in 1997. In both regressions, the coefficient on nonresident kin's earnings shocks for households in the high-inequality group was positive but imprecisely estimated. Hence, I do not find evidence that households' initial sender/receiver status is a significant source of heterogeneity in the effect of nonresident kin's earnings shocks on net transfers among households in the high-inequality group.

transfer receipts were straining household budgets, forcing cuts in expenditures on children's education. Curiously, there is no evidence among high-inequality households of declines in school enrollments.

Column 2 of Table 8 shows that it was children in low-inequality households whose enrollment status was affected by shocks to their nonresident family members' earnings. The corresponding achievement effect is statistically insignificant. Since the enrollment effects seem to have been concentrated in low-inequality households, while the association between nonresident family members' earnings shocks and changes in transfers was concentrated in high-inequality households, it seems unlikely that changes in transfers were the channel through which these enrollment effects operated.

*Additional results and discussion*

The results above suggest that transfers may have been an important channel through which the effects of nonresident kin's earnings shocks on children's human capital accumulation were mediated. This suggests that household budgets may have been strained by reduced transfer receipts. I investigated this hypothesis by estimating specifications with measures of household expenditures as the dependent variable. The result of this investigation is reported in Columns 1 and 2 of Table 9. In Column 1, the change in the log of total household expenditures is regressed on the change in the household head's earnings, the proxy for the change in nonresident kin's earnings, and the additional controls listed in Table 4.[60] Unsurprisingly, there is a significant positive association between the change in household expenditures and the change in the head of household's earnings. More interestingly, the change in household expenditures is also positively related to the change in nonresident kin's earnings, although the coefficient estimate is only significant at the 10 percent level.

In Column 2, I use as the dependent variable the change in the share of schooling expenditures in total current household expenditures. The IFLS contains a detailed module on household expenditures. Schooling expenditures asked about in the survey include costs for tuition, fees, uniforms and other supplies, transportation, and room and board.[61] If households were cutting all expenditures uniformly in response to their nonresident kin's earnings declines, then the share of schooling expenditures in total expenditures would not be affected by nonresident kin's earnings shocks. This seems to

[60]The sample size is slightly smaller than in Table 8 after trimming the 1 percent tails of the change in total household expenditures.

[61]I use the budget share on schooling as the dependent variable rather than the change in log schooling expenditures because some households report zero schooling expenditures, and because this specification facilitates a direct comparison of the rates of change in total expenditures versus schooling expenditures. The sample size is smaller in Column 2 due to missing data on school expenditures. The results from Column 1 are essentially unchanged when the sample is restricted to that of Column 2.

have been the case, as the coefficient estimate on the proxy for nonresident kin's earnings change in Column 2 is small in magnitude and statistically insignificant.

The positive relationship between nonresident kin's earnings changes and total household expenditures in Column 1, paired with evidence in Column 2 of Table 9 that schooling and total expenditures are equally sensitive to nonresident kin's earnings shocks, is consistent with the hypothesis that a reduction in nonresident kin's earnings decreased transfer receipts and consequently the size of households' budgets, forcing cuts in human capital investment. One interesting possibility this raises is that disruptions to children's human capital accumulation from nonresident kin's earnings shocks may, to an extent, have been concentrated among children whose schooling was being partly financed by transfers from their nonresident kin *before* the crisis hit. I investigated this possibility by first testing whether there is any evidence in the data that remittance receipts are associated with the presence of school-aged children. It seems likely that if households rely on remittance receipts to help finance schooling expenditures, then households with more school-aged children would *ceteris paribus* tend to receive more remittances.[62]

In Column 1 of Table 10, I report the results of a regression of net transfers received in 1997 on the number of co-resident children ages 4–19, controlling for household size, using the 1997 cross-section of the IFLS data for households in my sample. The estimates indicate that one additional co-resident child was positively and significantly associated with net remittance receipts in 1997 (the magnitude of the effect is about 7 percent of mean remittance receipts for net remittance receivers in 1997).[63] While this result itself does not have a causal interpretation, it is consistent with the possibility that households with more children in 1997 relied to a greater extent on remittances from nonresident kin to finance the schooling of these children.

Next, I tested whether the sensitivity of changes in household expenditures to changes in nonresident kin's earnings, reported in the first two columns of Table 9, depends upon the number of co-resident children. In Column 3 of Table 9, I interact the proxy for nonresident kin's earnings shocks with the number of co-resident children ages 4-19 in 1997. By construction, all households in my sample have at least one such child. The results indicate that expenditure changes between 1997 and 2000 of households with more co-resident children were more sensitive to nonresident kin's earnings shocks.[64] The

[62]One could imagine exploring the association between schooling expenditures and transfer receipts directly. The drawback of this approach is that the costs of foregone earnings and family labor are likely to be at least as important as direct schooling costs.

[63]The specification also includes dummies for the head of household's age and community fixed effects. Remittance receipts are normalized by total household expenditures. Results were qualitatively similar but less precisely estimated with remittance levels as the dependent variable.

[64]The median number of children per household is 2. The sum of the main effect and the interaction term for nonresident kin's earnings shocks is not significantly different from zero for households with either one or two children, indicating that the expenditures of these households are not sensitive to

results in Column 4 indicate that the sensitivity of the change in schooling expenditures relative to total expenditures does not depend upon the number of co-resident children in 1997.

Finally, I test whether the enrollment effect of nonresident kin's earnings shocks depends upon the number of co-resident children. In Column 2 of Table 10, I regress the change in a child's enrollment status between 1997 and 2000 on the proxy for nonresident kin's earnings shocks and the interaction between that variable and the total number of children co-resident in the child's household in 1997. The coefficient on the interaction term is positive and statistically significant. The sum of the main effect and the interaction term of nonresident kin's earnings shocks is not statistically different from zero, indicating that nonresident kin's earnings shocks have no enrollment effects on children in households with only one child in 1997. The hypothesis that the sum of the main effect and the interaction term equals zero for children in households with two children is rejected at the 5 percent significance level. It seems that enrollment effects of nonresident kin's earnings shocks were concentrated in households with at least two children, and were larger for children in households with a larger number of children co-resident in 1997.

Collectively, this evidence suggests that households with more children may have relied disproportionately on transfers from nonresident kin in order to finance schooling expenditures, that the earnings shocks of nonresident kin reduced transfers sent to these households, and that their expenditures consequently decreased, resulting in disruptions to the human capital accumulation of their children.[65]

To the extent that transfers help households finance investments in their children's human capital, they could constitute an important (and possibly efficiency-enhancing) mechanism promoting intergenerational mobility, particularly in the presence of lumpy schooling costs and incomplete credit markets.[66] However, reliance on transfers from nonresident kin to finance such investments may be a double-edged sword, since continued investment could be particularly fragile to adverse earnings shocks to nonresident kin.

nonresident kin's earnings shocks. The hypothesis that the sum of the main effect and interaction term equals zero for households with three co-resident children is rejected at the 5 percent level of significance.

[65]The results of Table 8 demonstrate that changes in transfer receipts among households in the high-inequality group are sensitive to nonresident kin's earnings shocks. I found no evidence of heterogeneity in this effect by the number of co-resident children in the household in 1997. If transfers received from nonresident kin declined for all households regardless of the number of co-resident children, then why do such declines disproportionately impact expenditures and enrollment in households with more co-resident children? One explanation could be that households with more co-resident children devote a larger *share* of total expenditures to schooling. Indeed, there is a strong association between the schooling budget share and the number of co-resident children in the 1997 cross-section of the IFLS data.

[66]Angelucci, et al. (2010) report evidence from Mexico suggesting that family members in the same village helped one another finance the education of their children.

### 5.2 Other links

*Co-residence patterns*

Another way in which the effects reported above may have been channeled from nonresident to resident family members is through changing patterns of family cohabitation. Existing evidence suggests that such changes may have been quite frequent during the crisis.[67] Possible motives for doing so included the desire to exploit scale economies in consumption, locate unemployed family members and dependents in relatively low-cost rural areas, and take advantage of the shift in relative employment opportunities toward the rural sector. Changes in household size and composition that occurred as family members responded to the crisis could conceivably have disrupted or enhanced children's human capital accumulation by reducing or increasing per capita resources, changing the supply of inputs into human capital accumulation (e.g. privacy), or changing the shadow value of children's time.

To investigate whether changes in co-residence patterns caused nonresident kin's earnings shocks to disrupt children's human capital accumulation, I tested the significance of nonresident family earnings shocks in a variety of specifications involving changes in household size and composition, along with the additional controls listed in Table 4. The results of three of these specifications are reported in the first three columns of Table 11. In Column 1, the change in household size between 1997 and 2000 is regressed on the earnings shocks of the household head and nonresident kin. In Column 2, the dependent variable is the change in the number of co-resident children. The proxy for nonresident kin's earnings shocks is not significant in either regression. Introducing additional controls for initial household size and the number of co-resident children does not alter this result. In Column 3, I test whether the number of *co-resident* uncles, aunts, and cousins of sample children was affected by earnings shocks to uncles and aunts who were *nonresident* in 1997.[68] Again, the proxy for the earnings shocks of these nonresident kin is insignificant, and remain so even after including controls for initial household size and composition.

I found no robust patterns of statistical significance in the proxy for nonresident family earnings across numerous specifications, including ones classified households into the low- and high-inequality groups used in Table 8 and nonresident kin by their proximity to the sample household as in Table 5. This is puzzling given the large migration flows that occurred within Indonesia during the crisis and existing evidence that changes in household structure constituted an important coping mechanism during the crisis. It

[67] Thomas, et al. (2004), Fallon and Lucas (2002)

[68] The sample size is slightly smaller in this specification after trimming the one percent tails of the proxy for both nonresident uncles/aunts' and grandparents/older siblings' earnings shocks.

seems unlikely that changing co-residence patterns played no role in mediating these effects. One possibility is that measurement error in the proxy variables for nonresident kin's earnings shocks precludes the detection of the true relationship between family co-residence patterns and nonresident kin's earnings shocks.

*Peer effects*

Finally, I explore the possibility that the results reported above may be caused by peer effects between family members. Suppose, for example, that a nonresident uncle of one of my sample children received an adverse earnings shock during the crisis, and responded by removing his own daughter (i.e. the cousin of the child in my sample) from school. It is conceivable that the parents of the child in my sample, upon observing this, respond by pulling their own child (i.e. the child in my sample) out of school. A less extreme response by parents could involve reducing their investment levels more moderately. Alternatively, children in my sample could experience a reduction in their motivation to learn after observing their cousins being pulled out of school. Such interdependencies could arise from non-standard preferences (e.g. of the "keeping-up-with-the-Jones" form). Alternatively, observations by one family member of unexpectedly severe effects of the crisis on another family member – such as the disenrollment of a child from school – could induce a response intended to mitigate expected consequences of the crisis.

The IFLS data do not contain information on whether nonresident uncles and aunts of my sample children have children of their own. They do, however, contain the marital status of these nonresident kin. Nonresident uncles and aunts who are single should be much less likely to have children of their own. This assumption forms the basis of a possible test for the role of peer effects in explaining my main findings.

Perhaps unsurprisingly, all of the nonresident uncles and aunts for nearly sixty percent of children in my sample are married, and the share of nonresident uncles and aunts who are married is over 50 percent for less than five percent of sample children. I implement the test for peer effects by restricting the sample used in Column 2 of Table 5 to children with at least one unmarried nonresident uncles or aunt. The results of this regression are reported in Column 4 of Table 11.[69] If the effect of nonresident uncles and aunts' earnings shocks were much smaller than the corresponding estimates in Column 2 of Table 5, then peer effects of the form described above could be an important link between resident and nonresident family members. However, the effect of nonresident uncles and aunts' earnings shocks on enrollment is only slightly smaller in magnitude (0.094 versus 0.116) and, as one would expect with the smaller sample, less precisely estimated. The hypothesis that the two are equal cannot be rejected. I conclude that this test

[69]This specification includes a full set of dummies for the head of household's age in 1997.

reveals no significant evidence that peer effects are an important link between resident and nonresident kin that could explain the enrollment effects in my main results.

## 6 Long-term evidence

Important to the welfare implications of the results reported above is the question of whether any disruptions to children's human capital had persistent effects. The fourth main wave of the IFLS enables me to assess whether any variation in children's human capital accumulation caused by their nonresident family members' earnings shocks was still present in 2007, nearly ten years after the crisis hit.[70]

In Column 2 of Table 6, I verify that my findings on the medium-run effects of nonresident kin's earnings shocks on achievement remain when the sample is restricted to the set of children for whom 2007 data is available. In the last four columns of Table 6, I regress the change in the number of grades completed between 1997 and 2007 on the same set of covariates as Column 1. Column 3 reports the results for all cohorts of children ages 4–19 in 1997 (i.e. ages 14–29 in 2007). In Columns 4, 5, and 6, I split the sample into 3 different age groups: children ages 4–6 years, 7–11 years, and 12–19 years in 1997, respectively. Most 4–6 year-olds in 1997 had not yet begun school when the crisis intensified in early 1998 (see Table 2b). By contrast, 7–11 year-olds were generally in or just completing primary school when the crisis hit. Enrollment rates in the next levels of schooling – junior and senior secondary – were lower than primary school even before the crisis hit, suggesting that any long-term effects on 12–19 year-olds may be different.

In Column 3, the sample of all age cohorts, the estimated effect of nonresident family members' earnings shocks on the change in the number of grades completed over the 10-year period is statistically significant. The point estimate implies that a one standard deviation increase in the composite proxy for nonresident family members' earnings shocks caused children to complete about 0.14 additional grades of schooling by 2007 (about 2.4 percent of the mean). Turning to Columns 4–6, children who were very young when the crisis hit seem to have been less affected in the long term by the earnings of their nonresident kin, although the effect is impreisely estimated. By contrast, the effects on children who were older when the crisis hit were relatively worse. A one standard deviation decline in the proxy for nonresident kin's earnings shock reduced the ultimate educational attainment of children ages 12-19 in 1997 by about 4 percent on average.

The fact that approximately 65 percent of these children were over age 18 by 2007 suggests that, for most children in the sample, this achievement gap reflects a permanent

[70] As with the main part of my analysis, attrition between 1997 and 2000 was relatively low. In an analysis described in the Appendix, I find no evidence that attrition is a significant source of bias in the results reported in this section.

difference in human capital. It seems that the achievement effects detected in 2000, which were somewhat smaller by comparison, propagated into larger differences among children in their ultimate educational attainment. These effects are sizeable in terms of wages foregone by those whose nonresident family members did not fare well during the crisis. Using Duflo's (2001) midpoint estimate of an 8.7 percent increase in wages for each additional year of schooling in Indonesia, the estimate in Column 3 of Table 6 implies that children whose nonresident family members experienced an earnings increase one standard deviation larger than their peers will see an increase in their annual wages of about 1.1 percent.

The inverted U-shaped pattern in the size of the effect with children's age at the time that the crisis hit is noteworthy. A possible explanation is that because many of the youngest group of children were likely in school for the entire 10-year period between 1997 and 2007, they had more school-aged years in which to catch up than did children in the older groups. Because those years likely saw better economic conditions as Indonesia's economy continued to recover from the crisis, their parents may have been better-positioned to invest in their education. Alternatively, they may have stayed in school longer than they would otherwise have (although it is not clear why this explanation would apply differentially to young children). I attempt to distinguish between these two explanations by regressing a measure of the child's "grade-for-age" – the number of grades completed by the child, less her age in 2007 – on the same set of covariates listed in Table 9. Nonresident family members' earnings shocks were statistically insignificant, suggesting that there were no significant differences between the ages, given grades completed, among very young children whose nonresident family members experienced different earnings shocks. A more plausible explanation seems to be that the human capital accumulation of these young children was, on average, less affected by the crisis to begin with, and that they were better-situated than older children to catch up during the years they were in school.

Finally, children who were older when the crisis hit may have experienced relatively less disruption to their human capital accumulation, either because their parents took (possibly costly) steps to shield them, or because many of them had finished school before the crisis hit.[71] Analysis from earlier IFLS survey waves suggests that households may have sought to shield older children's human capital accumulation of from disruption at the expense of younger children,[72] possibly because there may be high returns to secondary relative to primary schooling in Indonesia.[73] The pattern of effects reported

[71]Nearly a third of children ages 12 and older, and nearly half of children ages 16 and older in 1997, were not enrolled in school in 1997 – i.e. before the crisis hit. See Table 4a.

[72]Thomas, et al. (2004)

[73]Behrman and Deolalikar (1995)

in Table 6 suggest that part of the impact from which parents sought to shield their older children, possibly at the expense of their younger children, may have come from nonresident family members hit hard by the crisis.

## 7 Conclusion

The results of this study indicate that shocks to the earnings of their nonresident kin had a significant and persistent impact upon children's human capital accumulation during the 1997-8 Indonesian crisis. Shocks to both local and nonlocal nonresident kin were important, as were shocks to both nuclear and extended nonresident family. The effects seem to have been larger for children near the transition point between primary and secondary school. Altruistically-motivated transfers may have been important in channeling these effects. Since households with a larger number of children may have relied disproportionately on such transfers to finance schooling investments, children in these households may have been particularly vulnerable.

The sensitivity of household investment levels to changes in the economic fortunes of nonresident kin suggests that the *family* – regardless of household membership status – deserves greater attention in future research. The economic importance of nonresident family members is poised to increase over time as countries develop and households become more fragmented. Future efforts to collect information on family networks, in even greater detail than was done in the IFLS, may prove cost-effective.

Finally, cost-benefit evaluations of social safety nets and interventions to promote financial inclusion should account for welfare effects on the nonresident kin of targeted individuals.[74] It may be possible to enhance the targeting of these programs by utilizing information on individuals' nonresident kin. Conventional wisdom suggests that market-based targeting schemes should strive to maximize participation among the poor.[75] But how do the poor participate? Accounting in the design of such programs for indirect benefits, which accrue to individuals connected to program participants, constitutes a challenging mechanism design problem and a fruitful path for future research.

[74]This point relates to existing evidence suggesting that policies targeted at the elderly can benefit future generations. See Edmonds, et al. (2004).

[75]See, e.g., van de Walle (1998)

# Appendix

### Sample construction and attrition

The two individual-level samples used in this study are used to analyze children's human capital accumulation over the periods 1997-2000 and 1997-2007. The 1997-2000 sample was constructed by starting with a list of all individuals ages 4-19 years in 1997 who were household members in 1997. These individuals were matched to the household-level data corresponding to the households in which they resided in 1997. A small number of these individuals who were heads or spouses of their 1997 households, or who lived in households whose head and spouse did not report having any nonresident kin, were then excluded. This left a potential sample of 9,018 individuals whose enrollment status and educational attainment I attempted to locate in the data from the IFLS3 (i.e. 2000) survey wave. In order to facilitate the household-level analysis on transfers and cohabitation patterns, I also attempted to locate household-level data in the IFLS3 survey data for the Y households (regardless of whether the child continued to live in that household in 2000). After matching these individuals to the IFLS3 data, a small number of individuals who reportedly experienced a decrease in years of schooling completed or skipped an implausibly large number of grades between 1997 and 2000 were excluded. The first- and 99th-percentile tails of the proxies for the earnings changes of the household head and overall nonresident family, and of the changes in overall intra-family transfers and household size, were also trimmed. This resulted in a benchmark sample, after trimming of the shock data, of 8,191 individuals and 3,908 households.

The 2007 sample was constructed by attempting to locate educational attainment data for all individuals in the 1997-2000 sample. Most of these individuals had been re-interviewed by the IFLS4 survey teams in 2007 and hence provided their educational attainment directly, but in roughly 15 percent of cases the highest grade completed by an individual in 2007 was calculated from data contained in the household roster of the individual's last known place of residence. After excluding a small number of individuals who reportedly experienced a decrease in years of schooling completed between 1997 and 2007, this yielded a final sample of 7,779 individuals.

Attrition from the sample raises potential concerns about bias if the patterns of attrition are affected by nonresident family members' earnings shocks. Fortunately, the IFLS design explicitly tracked individuals who left households between survey rounds, resulting in re-contact rates that are astoundingly high for a longitudinal survey in a developing country. The IFLS data are packaged with sample weights adjusted for attrition, based on flexible logit models of observable individual- and household-level determinants of attrition.[76] There are at least two reasons why these pre-packaged attrition adjustments may not adequately address concerns about bias. First, the analytical samples used in this study are based on individuals who, at ages 4-19 in 1997, may be particularly mobile relative to the average individual in the full IFLS survey frame. Indeed the coefficients on individuals in this age range are often negative and significant in the logit regressions for the probability of being found in the next survey wave.[77] Furthermore, to the extent that nonresident family members' earnings shocks influence attrition patterns conditional

[76] Strauss, et al. (2009) contains further details.

[77] Strauss, et al. (2009), Table 3.2.

upon other observables, the attrition adjustments of the pre-packaged sample weights will not adequately address concerns about bias.

In order to assess the extent to which the results of this study may be biased by attrition, I two indicator variables for attrition: one for attrition between 1997 and 2000, and another for attrition between 2000 and 2007. I regressed the attrition indicators on the same set of 1997 individual- and household- level characteristics used as controls in Table 4, along with the composite proxy for nonresident kin's earnings shocks. The coefficient on nonresident kin's earnings shocks was insignificant in both regressions, suggesting that the potential for bias in the results of this study arising from correlation between attrition and nonresident kin's earnings shocks is limited.

**Construction of variables**

*Proxy variables*

The proxy variables for initial earnings levels and earnings changes used in this paper were constructed by combining information on the gender, education, industry of employment, and province of residence of the head of household and each nonresident parent, sibling, and child of the household head and spouse, with data on earnings changes experienced by workers in the SAKERNAS data, in the "cells" defined by those characteristics. Cells were constructed by defining 2 levels of education (completed 6 grades or less vs. completed more than 6 grades), and combining these categories with data on the 9 industries and 27 provinces in the SAKERNAS survey, for a total of 972 cells. Let $\tilde{x}$ index these cells. I also constructed a set of more broadly-defined cells defined by industry and location only. Let $\hat{x}$ index these cells. In cases where either round of the SAKERNAS contained fewer than 100 workers in cell $\tilde{x}$, I used information from cell $\hat{x}$ to construct the proxy variables. Let $x$ index elements in the composite set of cells in which each cell $\tilde{x}$ that contains fewer than the minimum number of observations in either SAKERNAS round is replaced by the corresponding cell $\hat{x}$. Let $n$ index the nonresident kin of the children in household $h$ (notation suppressed), $x_n$ denote the cell defined by the characteristics of $n$, and $N_{it}$ denote the total number of nonresident kin of child $i$. The value of Proxy 1 (earnings-only) for the nonresident kin of children in household $h$ is defined as follows:

$$Proxy1_h \equiv \Delta_{(t)} \ln \left( \sum_{n=1}^{N_{i96}} \overline{Earn_t^{x_n}} \right)$$

where $\overline{Earn_t^{x_n}} = \frac{1}{N_{x_n t}} \sum_{j=1}^{N_{x_n t}} \text{Earn}_j^{x_n t}$, $\text{Earn}_j^{x_n t}$ is the earnings of worker $j$ in cell $x_n$ reported in the time $t$ round of the SAKERNAS, and $N_{x_n t}$ is total the number of such workers.

The value of Proxy 2 (employment-only) for the nonresident kin of children in household $h$ is defined as follows:

$$Proxy2_h \equiv \frac{1}{N_{i96}} \sum_{n=1}^{N_{i96}} \text{EmpCh}^{x_n}$$

where $\text{EmpCh}^{x_n}$ equals the percentage change in aggregate employment, 1996-8, of all

workers in cell $x_n$.

Proxy 3 (the composite employment-earnings proxy), defined in the main text, is based on the variable $AggEarnCh^{x_n}$ which equals the percentage change in aggregate earnings, 1996-8, of all workers in cell $x_n$. Aggregate earnings are defined as the product of real wages and employment, where wages are total monthly earnings divided by hours worked over the same period.

Real wages and earnings were calculated by applying province-specific price deflators to the nominal values reported in the SAKERNAS. Price deflators were tabulated from price indices in capital cities obtained from BPS (1998, 1999, and 2000). Heads of IFLS households were matched to cells based on their characteristics in 1997. The one-digit industry classification for household heads reported in the IFLS data matched straightforwardly with the two-digit classification in the SAKERNAS. However, only (one-digit) occupational codes were available for nonresident family members. I was able to develop a concordance matching occupations to industries since the occupations of household heads were also reported in the IFLS. The overlap between industries and occupations was not perfect. Using data on the occupations and industries of household heads in IFLS2, I matched occupations to industries using the criterion that the given assignment ought to maximize the fraction of workers whose industry and occupation reported in the IFLS match exactly. The employment-weighted average match share across occupations was 86 percent, and the occupation with the minimum match share (40 percent) contained fewer than 13 percent of workers.[78]

Nonresident family members' actual provinces of residence were not reported in the IFLS2 data release as of this writing. Instead, their locations *relative to the household* are reported, allowing one to observe only whether or not the nonresident family member lived in the same province as the household (the data do contain the actual province of IFLS households) in 1997. The IFLS data from 2000 contain nonresident family members' actual locations in 2000, but use of a proxy based on this information would risk introducing endogeneity caused by the selective movement of nonresident family members across provinces during the crisis. Instead, for nonresident kin whose 1997 location I do not observe, I adopted a strategy of estimating the required information the from Indonesian workers with similar characteristics to these nonresident kin, but who lived in 1996 in any province *other than* that of the household. Specifically, let $l_h$ denote the 1997 province of household $h$. When constructing the proxies, I derive average earnings, changes in aggregate employment, and changes in aggregate earnings from the single cell $x_n$ corresponding to province $l_h$, for nonresident family members in the same province as the household in 1997. For nonresident family members not living in the same province as the household in 1997, I calculated these variables for each element in the union of cells $x$ defined by the characteristics of $n$ but with locations *other than* $l_h$, and took the simple average.

*Other variables*

[78] This 1-digit occupation consisted mostly of "bricklayers, carpenters and other construction workers" and "transport equipment operators". All were assigned to the construction industry. Pre-crisis wage levels and the wage change during the crisis were relatively similar for these two occupations.

Household-specific price deflators were calculated by the staff at RAND for use with the IFLS survey data. These deflators, which are based on households' locations (province and rural/urban) and month of interview, were used to convert IFLS data on household expenditures and transfers into real terms. Consumption shares for the price deflators were derived from the 1996 and 1999 waves of Indonesia's National Socioeconomic Survey (SUSENAS). Witoelar (1999) describes the calculations in greater detail.

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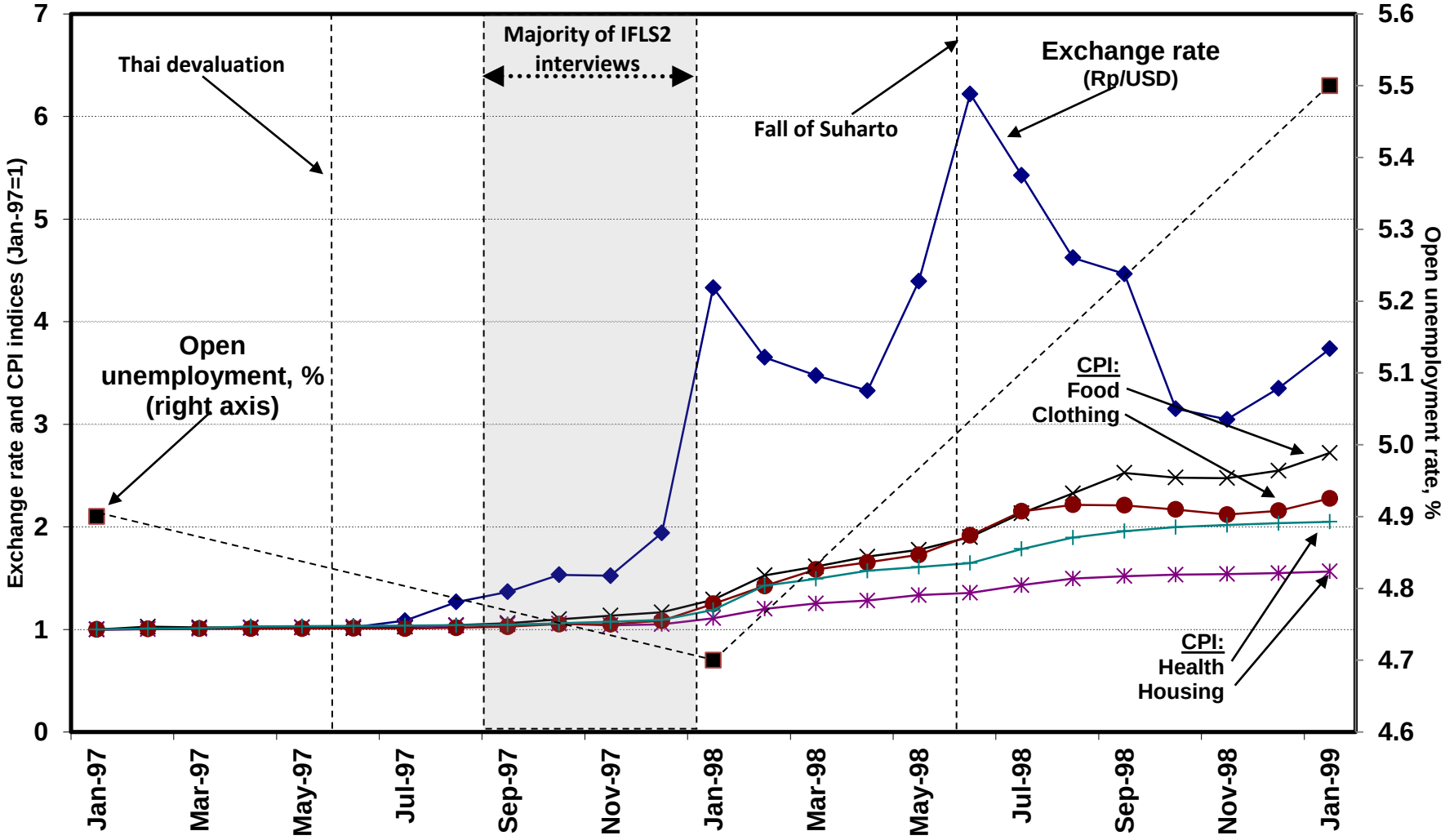
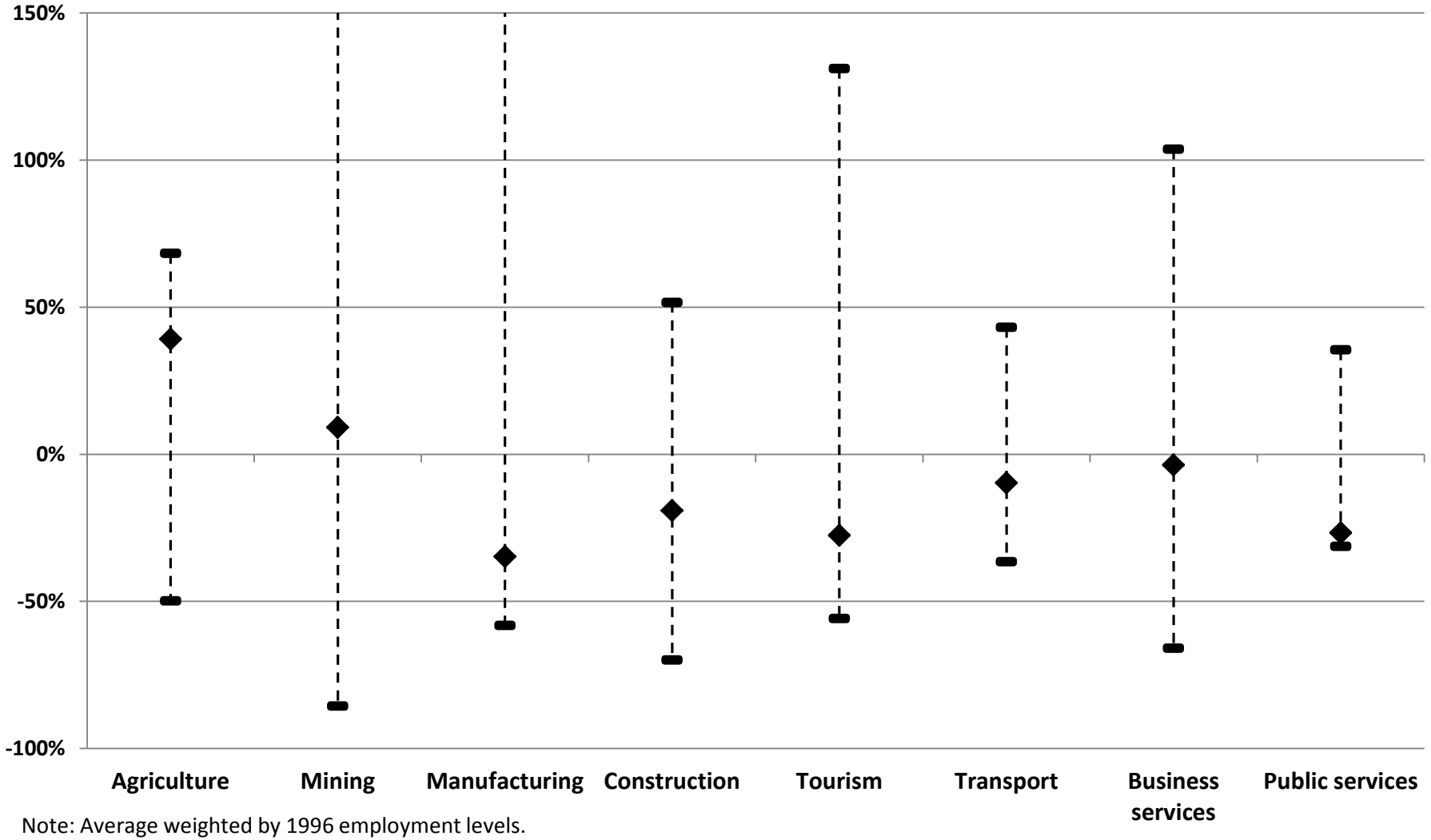
# Figure 1
# Timeline of the crisis and IFLS survey work



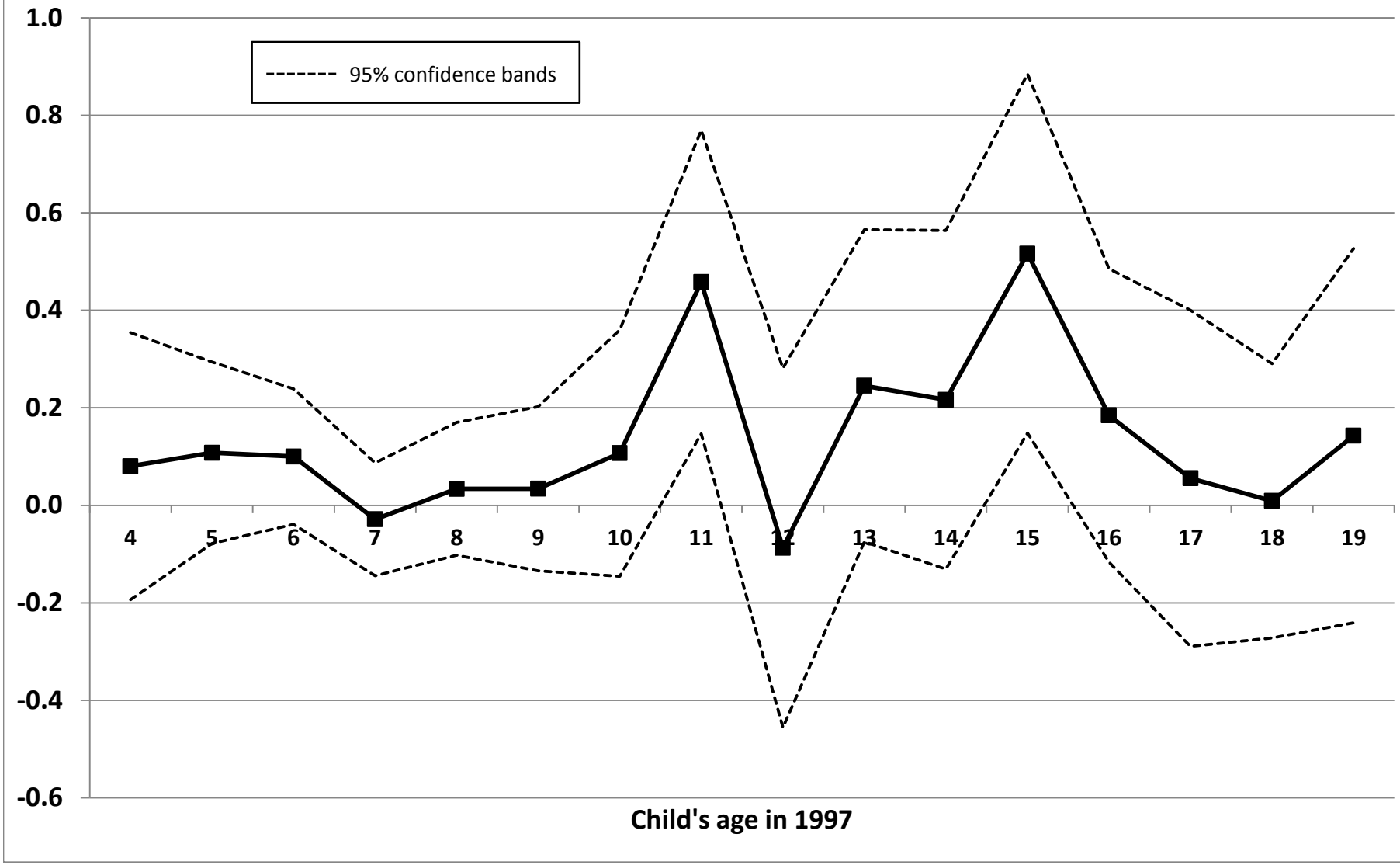

Source: Price and exchange rate data from Levinsohn, et al. (2003), Table 12.1. Unemployment data from Suryadarma, et al. (2007), Figure 2.

## Figure 2
## Change in aggregate earnings, 1996-8
### Min, max, and average across provinces, by industry




Note: Average weighted by 1996 employment levels.
Source: National Labor Force Survey (SAKERNAS)

**Figure 3**
**Enrollment effects of nonresident family earnings shock, 1997-2000 by age cohort**



## Table 1
**Characteristics of sample households**

| | 1997 | | 2000 | |
|---|---|---|---|---|
| | **Mean** | **sd** | **Mean** | **sd** |
| **N** | 3,908 | - | - | - |
| **Urban** *(share)* | 0.46 | - | 0.48 | - |
| **Moved between survey rounds** *(share)* | - | - | 0.09 | - |
| **Household size** | 5.8 | 2.0 | 5.5 | 2.0 |
| **Number of children** | | | | |
| Ages 4-19 | 2.8 | 1.4 | 2.5 | 1.4 |
| Ages 4-11 | 1.4 | 1.1 | 1.1 | 1.0 |
| Ages 12-19 | 1.4 | 1.1 | 1.4 | 1.0 |
| **Household expenditures** | | | | |
| Per capita *(Tens of thousands of 2000 rupiah)* | 325 | 282 | 309 | 252 |
| Share on food | 0.57 | 0.17 | 0.59 | 0.16 |
| Share on schooling expenses | 0.08 | 0.09 | 0.09 | 0.10 |
| **Characteristics of head** | | | | |
| Age | 44.9 | 11.7 | - | - |
| Years of schooling | 5.7 | 4.5 | - | - |
| Married *(share)* | 0.91 | - | 0.90 | - |
| Spouse co-resident | 0.97 | - | 0.97 | - |
| Industry of employment *(shares)* | | | | |
| Agriculture | 0.31 | - | 0.33 | - |
| Construction | 0.07 | - | 0.05 | - |
| Manufacturing | 0.11 | - | 0.08 | - |
| Other | 0.37 | - | 0.36 | - |
| Not working | 0.14 | - | 0.18 | - |
| Change in earnings | | | | |
| Actual, 1997-2000 | -0.15 | 0.86 | - | - |
| Proxy, 1996-8 | -0.16 | 0.15 | - | - |

Note: Statistics weighted by number of sample children in each household.
Source: IFLS and SAKERNAS

## Table 2a

**Basic characteristics of sample children**

| | |
|---|---|
| **N** | 8,191 |
| | **Mean** |
| **Female** *(share)* | 0.49 |
| **Age in 1997 [sd]** | 11.0 [4.4] |
| **Enrolled in school *(shares)*** | |
| 1997 | 0.71 |
| 2000 | 0.70 |
| **Relationship to household head in 1997** | |
| Biological child | 0.87 |
| Adopted child | 0.01 |
| Grandchild | 0.09 |
| Niece or nephew | 0.01 |
| Other | 0.02 |

Source: IFLS

## Table 2b

**Enrollment transitions of sample children, by 1997 age cohorts**

| Age in 1997 | Enrollment status in 1997 | Enrollment status in 2000 | | Change, 1997-2000 | |
|---|---|---|---|---|---|
| | | Un-enrolled | Enrolled | By initial enrollment status | Overall |
| **4-6** | Unenrolled | 71 | 1,181 | 0.94 | - |
| | Enrolled | 4 | 397 | -0.01 | 0.14 |
| **7-9** | Unenrolled | 22 | 53 | 0.71 | - |
| | Enrolled | 49 | 1,535 | -0.03 | 0.00 |
| **10-12** | Unenrolled | 57 | 9 | 0.14 | - |
| | Enrolled | 261 | 1,336 | -0.16 | -0.03 |
| **13-15** | Unenrolled | 256 | 6 | 0.02 | - |
| | Enrolled | 490 | 937 | -0.34 | -0.06 |
| **16-19** | Unenrolled | 726 | 24 | 0.03 | - |
| | Enrolled | 518 | 259 | -0.67 | -0.06 |
| **4-19** | Unenrolled | 1,132 | 1,273 | 0.53 | - |
| | Enrolled | 1,322 | 4,464 | -0.23 | -0.01 |

Source: IFLS

## Table 2c

**Grades completed by sample children, by 1997 age cohorts**

| Age in 1997 | 1997 | | 2000 | | 2007 | | Change, 1997-2000 | | Change, 1997-2007 | |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Mean** | ***sd*** | **Mean** | ***sd*** | **Mean** | ***sd*** | **Mean** | ***sd*** | **Mean** | ***sd*** |
| 4 | 0.0 | *0.0* | 0.4 | *0.6* | 7.1 | *1.4* | 0.4 | *0.6* | 7.1 | *1.4* |
| 5 | 0.0 | *0.0* | 1.1 | *0.9* | 7.9 | *1.6* | 1.1 | *0.9* | 7.9 | *1.6* |
| 6 | 0.1 | *0.3* | 2.0 | *1.0* | 8.5 | *2.0* | 1.9 | *0.9* | 8.4 | *1.9* |
| 7 | 0.5 | *0.6* | 2.8 | *1.2* | 8.8 | *2.7* | 2.4 | *1.0* | 8.3 | *2.5* |
| 8 | 1.2 | *0.9* | 3.8 | *1.3* | 9.6 | *2.8* | 2.6 | *0.9* | 8.4 | *2.5* |
| 9 | 2.0 | *1.0* | 4.7 | *1.4* | 9.8 | *3.0* | 2.7 | *0.9* | 7.8 | *2.6* |
| 10 | 2.9 | *1.2* | 5.7 | *1.3* | 10.4 | *3.0* | 2.8 | *0.8* | 7.5 | *2.6* |
| 11 | 3.6 | *1.2* | 6.3 | *1.6* | 10.0 | *3.4* | 2.6 | *0.9* | 6.3 | *2.9* |
| 12 | 4.7 | *1.4* | 7.4 | *1.7* | 10.4 | *3.3* | 2.6 | *1.0* | 5.7 | *2.9* |
| 13 | 5.6 | *1.4* | 8.2 | *2.1* | 10.4 | *3.5* | 2.6 | *1.1* | 4.8 | *2.9* |
| 14 | 6.5 | *1.4* | 9.1 | *2.0* | 10.8 | *3.2* | 2.6 | *1.2* | 4.3 | *2.6* |
| 15 | 7.6 | *1.7* | 9.6 | *2.3* | 10.8 | *3.2* | 1.9 | *1.2* | 3.2 | *2.4* |
| 16 | 8.1 | *2.3* | 9.8 | *2.9* | 10.6 | *3.6* | 1.6 | *1.3* | 2.5 | *2.2* |
| 17 | 8.9 | *2.4* | 10.3 | *3.0* | 10.9 | *3.6* | 1.3 | *1.2* | 2.0 | *2.0* |
| 18 | 9.1 | *2.9* | 10.0 | *3.2* | 10.4 | *3.6* | 0.9 | *1.2* | 1.3 | *1.7* |
| 19 | 9.2 | *3.4* | 9.9 | *3.5* | 10.1 | *3.9* | 0.7 | *1.1* | 0.9 | *1.4* |
| Overall | 3.9 | *3.6* | 5.9 | *3.8* | 9.7 | *3.2* | 2.0 | *1.3* | 5.8 | *3.4* |

Source: IFLS

## Table 3

**Characteristics of nonresident kin of sample children, 1997**

| | Grand-parents | Uncles/ aunts | Older sibling | Overall |
|---|---|---|---|---|
| Number | | | | |
| Mean | 1.6 | 7.4 | 1.1 | 10.2 |
| Share with zero | 0.24 | 0.01 | 0.57 | 0.00 |
| Location *(shares)* | | | | |
| Same village | 0.40 | 0.33 | 0.28 | 0.34 |
| Another village, same district | 0.19 | 0.12 | 0.09 | 0.13 |
| Another district, same province | 0.10 | 0.32 | 0.33 | 0.29 |
| Another province | 0.11 | 0.20 | 0.25 | 0.20 |
| International | 0.00 | 0.01 | 0.04 | 0.02 |
| Unknown | 0.19 | 0.01 | 0.01 | 0.03 |
| Industry *(shares)* | | | | |
| Not working | 0.53 | 0.30 | 0.35 | 0.35 |
| Agriculture | 0.30 | 0.24 | 0.15 | 0.24 |
| Construction | 0.02 | 0.02 | 0.10 | 0.03 |
| Manufacturing | 0.01 | 0.10 | 0.07 | 0.08 |
| Trade, tourism, business svcs | 0.08 | 0.12 | 0.10 | 0.11 |
| Public services | 0.04 | 0.16 | 0.20 | 0.15 |
| Other/unknown | 0.01 | 0.05 | 0.03 | 0.04 |
| Proxies for earnings changes, 1996-8 | | | | |
| Proxy 1 (Earnings only, dlog) | -0.12 | -0.17 | -0.17 | -0.17 |
| [sd] | [0.14] | [0.14] | [0.10] | [0.12] |
| Proxy 2 (Employment only, %) | 0.06 | 0.06 | 0.07 | 0.06 |
| [sd] | [0.11] | [0.09] | [0.15] | [0.08] |
| Proxy 3 (Earnings and employment, %) | -0.04 | -0.06 | -0.05 | -0.06 |
| [sd] | [0.16] | [0.13] | [0.13] | [0.13] |

Note: Statistics weighted by number children in each sample household. Location is relative to sample household. Nonresident parents' locations based on 1993 data.

Source: Author's calculations from IFLS and SAKERNAS

## Table 4

**Main results on effect of nonresident kin's earnings shocks on human capital accumulation**

| | 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|---|
| | Change in enrollment status, 1997-2000 | | | | |
| **Change in head's earnings** | | | | | |
| (dlog) | 0.004 | 0.024*** | 0.022*** | 0.021*** | 0.021*** |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) |
| **Change in nonresident kin's earnings** | | | | | |
| Proxy 1 (Earnings only, dlog) | | | 0.100** | 0.095** | |
| | | | (0.047) | (0.047) | |
| Proxy 2 (Employment only, %) | | | | 0.135** | |
| | | | | (0.058) | |
| Proxy 3 (Earnings and employment, %) | | | | | 0.135*** |
| | | | | | (0.039) |
| **Other controls** | | | | | |
| Head's initial earnings (log) | | 0.050*** | 0.047*** | 0.046*** | 0.047*** |
| | | (0.005) | (0.005) | (0.005) | (0.005) |
| Proxy for nonresident kin's initial earnings (log) | | | 0.031*** | 0.030*** | 0.031*** |
| | | | (0.010) | (0.010) | (0.010) |
| Initial enrollment status (=1 if enrolled in 1997) | -0.669*** | -0.688*** | -0.693*** | -0.693*** | -0.694*** |
| | (0.014) | (0.013) | (0.013) | (0.013) | (0.013) |
| Head employed in 1997 | 0.015 | -0.738*** | -0.694*** | -0.680*** | -0.689*** |
| | (0.014) | (0.079) | (0.081) | (0.081) | (0.081) |
| Head employed in 2000 | 0.023** | 0.022** | 0.018* | 0.018* | 0.019* |
| | (0.011) | (0.010) | (0.010) | (0.010) | (0.010) |
| Have international nonresident family | | | 0.001 | 0.001 | -0.000 |
| | | | (0.008) | (0.008) | (0.008) |
| Have nonresident family employed in "other" industry | | | 0.006 | 0.007 | 0.004 |
| | | | (0.012) | (0.011) | (0.011) |
| Constant | 0.901*** | 0.875*** | 0.472*** | 0.476*** | 0.464*** |
| | (0.030) | (0.029) | (0.135) | (0.134) | (0.136) |
| Observations | 8,191 | 8,191 | 8,191 | 8,191 | 8,191 |
| $R^2$ | 0.68 | 0.68 | 0.68 | 0.68 | 0.68 |
| Dependent variable mean: | | | | | |
| Overall | -0.006 | | | | |
| Enrolled in 1997 (n=5,786) | -0.228 | | | | |
| Unenrolled in 1997 (n=2,405) | 0.529 | | | | |

Notes:

1. Sample consists of cohorts of children ages 4-19 years in 1997.
2. Huber-White standard errors in parentheses, clustered at the community level.
3. All regressions include dummies for the ages of the child and the head of household in 1997.

## Table 5

**Effects by nonresident kin's location and relationship to child**

| | 1 | 2 |
|---|---|---|
| | Change in enrollment status, 1997-2000 | |
| **Shock variables** | | |
| Change in head's earnings (%) | 0.019*** | 0.023*** |
| | (0.007) | (0.007) |
| Change in nonresident kin's earnings (Proxy 3, %) | | |
| Same district as household | 0.095** | |
| | (0.038) | |
| Different district than household | 0.074** | |
| | (0.032) | |
| Nuclear family (grandparents and older siblings) | | 0.071* |
| | | (0.039) |
| Extended family (uncles and aunts) | | 0.116*** |
| | | (0.038) |
| **Other controls** | | |
| Head's initial earnings (log) | 0.042*** | 0.047*** |
| | (0.006) | (0.005) |
| Proxy for nonresident kin's initial earnings (log) | | |
| Same district as household | 0.017*** | |
| | (0.006) | |
| Different district than household | 0.033*** | |
| | (0.006) | |
| Nuclear family (grandparents and older siblings) | | 0.021*** |
| | | (0.007) |
| Extended family (uncles and aunts) | | 0.022*** |
| | | (0.004) |
| Constant | 0.823*** | 0.773*** |
| | (0.032) | (0.055) |
| Observations | 7,993 | 7,976 |
| $R^2$ | 0.68 | 0.68 |

Notes:

1. Sample consists of cohorts of children ages 4-19 years in 1997.
2. Huber-White standard errors in parentheses, clustered at the community level.
3. Column 1 includes dummies for the presence of employed nonresident kin in the same and different districts than the household. Column 2 includes dummies for the presence of employed nuclear and extended family members. Both regressions include additional controls listed in Table 4.

## Table 6

**Medium- and long-run effects on achievement**

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| | Change in grades completed | | | | | |
| | 1997-2000 | | 1997-2007 | | | |
| | All ages | All ages | All ages | Ages 4-6 | Ages 7-11 | Ages 12-19 |
| Change in head's earnings (dlog) | 0.055*** | 0.053*** | 0.297*** | 0.225*** | 0.333*** | 0.285*** |
| | (0.019) | (0.019) | (0.053) | (0.062) | (0.084) | (0.069) |
| Change in nonresident kin's earnings (Proxy 3, %) | 0.215* | 0.210* | 1.094*** | 0.424 | 1.280** | 1.083*** |
| | (0.118) | (0.122) | (0.380) | (0.456) | (0.584) | (0.378) |
| **Other controls** | | | | | | |
| Head's initial earnings (log) | 0.075*** | 0.071*** | 0.555*** | 0.418*** | 0.635*** | 0.526*** |
| | (0.015) | (0.015) | (0.044) | (0.061) | (0.069) | (0.051) |
| Proxy for nonresident kin's initial earnings (log) | 0.079*** | 0.073*** | 0.247*** | 0.162** | 0.338** | 0.215*** |
| | (0.022) | (0.021) | (0.082) | (0.071) | (0.134) | (0.077) |
| Initial enrollment status (=1 if enrolled in 1997) | 1.536*** | 1.552*** | 2.394*** | 1.414*** | 4.216*** | 2.463*** |
| | (0.043) | (0.044) | (0.104) | (0.171) | (0.367) | (0.107) |
| Head employed in 1997 | -1.111*** | -1.044*** | -8.234*** | -6.120*** | -9.405*** | -7.830*** |
| | (0.227) | (0.227) | (0.654) | (0.927) | (1.051) | (0.751) |
| Head employed in 2000 | 0.036 | 0.033 | 0.198** | 0.041 | 0.482*** | 0.019 |
| | (0.034) | (0.033) | (0.087) | (0.136) | (0.151) | (0.096) |
| Have international nonresident family | -0.010 | -0.009 | 0.021 | 0.138 | -0.048 | 0.019 |
| | (0.026) | (0.026) | (0.072) | (0.094) | (0.105) | (0.089) |
| Have nonresident family employed in "other" industry | -0.028 | -0.028 | 0.060 | 0.023 | 0.111 | 0.045 |
| | (0.031) | (0.032) | (0.069) | (0.064) | (0.091) | (0.098) |
| Constant | -0.661** | -0.600** | 3.515*** | 4.815*** | -1.597 | -2.927*** |
| | (0.312) | (0.302) | (1.151) | (1.006) | (1.874) | (1.089) |
| Observations | 8,191 | 7,779 | 7,779 | 1,596 | 2,667 | 3,516 |
| $R^2$ | 0.54 | 0.55 | 0.60 | 0.26 | 0.29 | 0.48 |
| Dependent variable mean: | 2.0 | 2.0 | 5.8 | 7.8 | 7.7 | 3.5 |

Notes:

1. Sample in columns 1-3 consists of cohorts of children ages 4-19 years in 1997; samples in columns 4, 5, and 6 consist of cohorts of children ages 4-6, 7-11, and 12-19 years in 1997, respectively.
2. Huber-White standard errors in parentheses, clustered at the community level.
3. All regressions include additional controls listed in Table 4.

## Table 7

**Transfers exchanged by sample households with nonresident kin, by initial sender/receiver status of household**

(tens of thousands of 2000 rupiah)

| | 1997 | 2000 | Change |
|---|---|---|---|
| **Net receivers in 1997** | | | |
| Share of all households | 0.36 | - | - |
| Net received | | | |
| Mean | 77.0 | 25.6 | -51.5 |
| *sd* | *156.7* | *142.7* | *176.8* |
| % of 1997 household expenditures | | | |
| Mean | 5.6 | 2.3 | -3.3 |
| *sd* | *9.3* | *10.9* | *11.8* |
| **Net senders in 1997** | | | |
| Share of all households | 0.51 | - | - |
| Net sent | | | |
| Mean | 83.5 | 60.1 | -23.4 |
| *sd* | *155.7* | *211.6* | *193.2* |
| % of 1997 household expenditures | | | |
| Mean | 5.0 | 3.2 | -1.8 |
| *sd* | *8.7* | *10.9* | *11.7* |
| **All households** | | | |
| Share with zero net sent | 0.13 | 0.10 | 0.04 |
| Net sent | | | |
| Mean | 15.3 | 23.1 | 7.9 |
| *sd* | *163.6* | *184.0* | *183.2* |
| % of 1997 household expenditures | | | |
| Mean | 0.6 | 0.9 | 0.3 |
| *sd* | *9.7* | *10.9* | *11.6* |

Note: Sum of transfers exchanged over the past 12 months with child's nonresident grandparents, older siblings, and uncles/aunts.

Source: IFLS

## Table 8

**Intra-family transfers, 1997-2000**

Changes in net intra-family transfers received (Columns 1 and 2)

Changes in schooling outcomes (Columns 3 and 4)

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| | Household-level | | Child-level | |
| | Change in net transfers received | | Change in enrollment status | Change in grades completed |
| Change in head's earnings (%) | -0.007** | | | |
| | (0.003) | | | |
| *Low inequality | | -0.006** | 0.017** | 0.049** |
| | | (0.003) | (0.007) | (0.019) |
| *High inequality | | -0.014* | 0.014 | 0.015 |
| | | (0.008) | (0.019) | (0.060) |
| Change in nonresident kin's earnings (Proxy 3, %) | -0.001 | | | |
| | (0.012) | | | |
| *Low inequality | | -0.011 | 0.142*** | 0.133 |
| | | (0.013) | (0.040) | (0.119) |
| *High inequality | | 0.085** | -0.024 | 0.843** |
| | | (0.037) | (0.104) | (0.370) |
| Constant | -0.049 | -0.088* | 0.025 | -1.342*** |
| | (0.036) | (0.053) | (0.171) | (0.442) |
| Observations | 3,908 | 3,908 | 8,191 | 8,191 |
| $R^2$ | 0.03 | 0.04 | 0.69 | 0.54 |

Notes:

1. Sample in Columns 1 and 2 consists of households with children ages 4-19 years in 1997; sample in Columns 3 and 4 consists of children ages 4-19 years in 1997.

2. Huber-White standard errors in parentheses, clustered at the community level.

3. Dependent variable in Columns 1 and 2 is the change in net transfers received from nonresident parents, siblings, and children of the household head and spouse, normalized by 1997 household expenditures.

4. High inequality refers to households in the lowest (highest) quartile of 1997 per capita expenditures with nonresident kin in the highest (lowest) quartile of the proxy for nonresident kin's 1997 average earnings. All other households classified as low inequality.

5. All regressions include controls for 1997 household per capita expenditures (PCE), indicators for high and low inequality categories and their interactions with 1997 PCE, and the additional controls listed in Table 4.

## Table 9

**Household expenditures, 1997-2000**

Tests of changes in total expenditures and schooling expenditues

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| | **Change in total expenditures (dlog)** | **Change in schooling expenditures share** | **Change in total expenditures (dlog)** | **Change in schooling expenditures share** |
| Change in head's earnings (%) | 0.069*** | 0.002 | 0.089** | 0.008 |
| | (0.018) | (0.003) | (0.035) | (0.007) |
| * number of co-resident children in 1997 (ages 4-19) | | | -0.007 | -0.002 |
| | | | (0.011) | (0.002) |
| Change in nonresident kin's earnings (Proxy 3, %) | 0.181* | -0.014 | -0.242 | -0.039 |
| | (0.109) | (0.013) | (0.201) | (0.025) |
| * number of co-resident children in 1997 (ages 4-19) | | | 0.173*** | 0.011 |
| | | | (0.066) | (0.009) |
| Constant | 0.113 | -0.086** | 0.256 | -0.072* |
| | (0.295) | (0.037) | (0.292) | (0.037) |
| Observations | 3,886 | 3,787 | 3,886 | 3,787 |
| $R^2$ | 0.03 | 0.04 | 0.04 | 0.05 |

Notes:

1. Sample consists of households with children ages 4-19 years in 1997.

2. Huber-White standard errors in parentheses, clustered at the community level.

3. Dependent variable in Columns 1 and 3 is the change in the log of total household expenditures; in Columns 2 and 4 is the change in schooling expenditures divided by total current household expenditures. All changes refer to the period 1997-2000.

4. Columns 3 and 4 include controls for initial household size and the number of co-resident children in 1997 ages 4-11 and 12-19 years. All regressions include the additional controls listed in Table 4.

## Table 10

**Households' reliance on transfers from nonresident kin**

Tests of sensitivity of initial transfer receipts and changes in school enrollment status, 1997-2000, to initial household composition

| | 1<br>Net transfers received in 1997 (Household-level) | 2<br>Change in enrollment status (Child-level) |
|---|---|---|
| Number of co-resident children in 1997 (ages 4-19) | 0.004**<br>(0.002) | -0.011**<br>(0.006) |
| Household size in 1997 | -0.003**<br>(0.001) | 0.001<br>(0.004) |
| **Shock variables** | | |
| Change in head's earnings (%) | | 0.031**<br>(0.013) |
| * number of co-resident children in 1997 (ages 4-19) | | -0.003<br>(0.004) |
| Change in nonresident kin's earnings (Proxy 3, %) | | 0.010<br>(0.067) |
| * number of co-resident children in 1997 (ages 4-19) | | 0.041*<br>(0.022) |
| Constant | -0.036***<br>(0.010) | 0.494***<br>(0.140) |
| Observations | 3,908 | 8,191 |
| $R^2$ | 0.08 | 0.68 |

Notes:

1. Sample in Column 1 consists of households with children ages 4-19 years in 1997; sample in Column 2 consists of children ages 4-19 years in 1997.

2. Huber-White standard errors in parentheses, clustered at the community level.

3. Dependent variable in Column 1 is net transfers received from nonresident parents, siblings, and children of the household head and spouse in 1997, divided by household expenditures; in Column 2 is the change in the child's enrollment status between 1997 and 2000.

4. Column 1 includes a full set of dummies for the head of household's age in 1997 and community fixed effects. Column 2 includes the additional controls listed in Table 4.

## Table 11

**Other channels**

Tests of changes in co-residence patterns and peer effects

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| | Tests for changing co-residence patterns (Household-level) | | | Test for peer effects (Child-level) |
| | Change in household size | Change in number of co-resident children | Change in number of co-resident uncles/aunts and cousins | Change in enrollment status |
| **Shock variables** | | | | |
| Change in head's earnings (%) | -0.001 | -0.012 | 0.024 | 0.036*** |
| | (0.045) | (0.025) | (0.015) | (0.009) |
| Change in nonresident kin's earnings (Proxy 3, %) | 0.110 | 0.111 | | |
| | (0.269) | (0.143) | | |
| Uncles and aunts | | | -0.034 | 0.094* |
| | | | (0.087) | (0.050) |
| Constant | -0.646 | -0.751** | -0.091 | 0.905*** |
| | (0.634) | (0.343) | (0.166) | (0.104) |
| Observations | 3,908 | 3,908 | 3,802 | 3,364 |
| $R^2$ | 0.05 | 0.08 | 0.04 | 0.74 |

Notes:

1. Sample in Columns 1-3 consists of households with children ages 4-19 years in 1997; sample in Column 4 consists of children ages 4-19 years in 1997 with at least one unmarried nonresident uncle or aunt.

2. Huber-White standard errors in parentheses, clustered at the community level.

3. Dependent variable in Column 1 is the change in household size; in Column 2 is the change in the number of co-resident children of the household head and spouse; in Column 3 is the change in the number of co-resident uncles, aunts, and cousins of sample children. All changes refer to the period 1997-2000.

4. Columns 3 and 4 include the controls listed in Column 2 of Table 5. All regressions include the additional controls listed in Table 4.

## Appendix Table
**Alternative specifications**

| | 1 | 2 | 3 |
|---|---|---|---|
| | Change in enrollment status, 1997-2000 | | |
| Change in head's earnings (dlog) | | 0.020*** | 0.020*** |
| | | (0.007) | (0.007) |
| Proxy for change in head's earnings (%) | 0.049 | | |
| | (0.032) | | |
| Change in earnings of other householders (dlog) | | 0.010 | |
| | | (0.009) | |
| Change in nonresident kin's earnings (Proxy 3, %) | 0.117*** | 0.132*** | 0.097** |
| | (0.040) | (0.040) | (0.044) |
| **Other controls** | | | |
| Head's initial earnings (log) | 0.042*** | 0.043*** | 0.042*** |
| | (0.005) | (0.005) | (0.006) |
| Other householders' initial earnings (log) | | 0.017*** | |
| | | (0.006) | |
| Proxy for nonresident kin's initial earnings (log) | 0.034*** | 0.029*** | 0.032*** |
| | (0.010) | (0.010) | (0.009) |
| Initial enrollment status (=1 if enrolled in 1997) | -0.693*** | -0.696*** | -0.712*** |
| | (0.014) | (0.014) | (0.014) |
| Head employed in 1997 | -0.615*** | -0.623*** | -0.620*** |
| | (0.074) | (0.082) | (0.086) |
| Head employed in 2000 | 0.014 | 0.020* | 0.021* |
| | (0.011) | (0.010) | (0.011) |
| Constant | 0.427*** | 0.474*** | 0.441*** |
| | (0.143) | (0.136) | (0.115) |
| Observations | 8,078 | 8,163 | 8,191 |
| $R^2$ | 0.68 | 0.68 | 0.67 |
| **District x rural/urban fixed effects?** | **N** | **N** | **Y** |

Notes:
1. Sample consists of cohorts of children ages 4-19 years in 1997.
2. Huber-White standard errors in parentheses, clustered at the community level.
3. All regressions include additional controls listed in Table 4.