This is to certify that the
dissertation entitled

ECONOMIC CRISES AND DEMOGRAPHIC OUTCOMES:
EVIDENCE FROM INDONESIA

presented by

Pungpond Rukumnuaykit

has been accepted towards fulfillment
of the requirements for the

Ph.D. degree in Economics

Major Professor's Signature

September 15 2003

Date

ECONOMIC CRISES AND DEMOGRAPHIC OUTCOMES:
EVIDENCE FROM INDONESIA

By

Pungpond Rukumnuaykit

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

Department of Economics

2003

ABSTRACT

ECONOMIC CRISES AND DEMOGRAPHIC OUTCOMES:
EVIDENCE FROM INDONESIA

By

Pungpond Rukumnuaykit

This dissertation examines the short-run impacts of the Indonesian economic crises on different demographic outcomes of infants and women. In the first chapter, I examine whether the recent Indonesian financial crisis and the 1997/1998 drought and smoke haze crises had adverse effects on infants' birthweight and mortality. In the second chapter, I examine the effects of the 1998 economic crisis on the ages of female first marriages and first births. This dissertation uses data from three waves of the Indonesia Family Life Survey: IFLS1 (1993), IFLS2 (1997), and IFLS3 (2000).

The methodology used in the first chapter is to compare health conditions of newborns of different birth cohorts. The estimations of both neonatal and post-neonatal mortality risks are carried out using multivariate regressions with socio-economic control variables such as mother's education, place of residence (province/community), and gender of the child. In addition, mortality risks are estimated using hazard models to capture the mortality risks at different ages (in months). The paper uses both nonparametric and parametric hazard models to estimate the hazard rates. The effects of the crises on birthweights are analyzed using multivariate regressions and comparisons of birthweight cumulative distributions.

Estimated results on mortality outcomes show that the financial crisis had adverse impacts on neonatal mortality in both urban and rural areas. The adverse effects of the

financial crisis on post-neonatal mortality risks were larger and more statistically significant for urban infants than for rural infants. The drought/smoke crisis adversely affected post-neonatal mortality risks in rural areas. The estimated results show that rural infants born during the drought/smoke crisis experienced approximately a 4.4 percentage points increase in their infant mortality risks (44 per 1,000 live births). The magnitude of the effects almost doubled after controlling for community fixed effects. None of the crises significantly affected birthweight. I find that the lack of evidence on the adverse effects may be due to selection problems in reported birthweights.

In the second chapter, effects of the crisis on ages of female first marriage and first births are estimated using hazard models. The methods used include both parametric and non-parametric estimations of the marriage and first birth hazards, conditional and unconditional on being married. Estimated results indicate that overall there was an increase in the probability of getting married and a decrease in the probability of having first births among Indonesian women during the crisis in both conditional and unconditional analyses. These findings support the hypothesis that marriages of individuals in a household and delaying first births may have been used as income-smoothing mechanisms in the time of the crisis. Results from this paper are not sufficient to draw any conclusion on why an increase in marriage probability and a delay in having first birth took place. We speculate that women are more likely to get married during the time of the crisis to take advantage of economies of scale and specialization in household production and consumption. The delays of first births might be due to a consumption-smoothing consideration or other supply factors such as the separation of spouses when relocation of individuals occurred during the crisis.

To my parents

ACKNOWLEDGEMENTS

First, I would like to express my profound gratitude to the Royal Thai Government, specifically to the Ministry of Foreign Affairs, for granting me support to pursue my undergraduate and graduate studies. The support has given me not only a future career, but also opportunities to be open to higher education abroad, to work on projects I am interested in, and to prepare myself to better serve my country and society.

I owe a debt of gratitude to many people whose help has been crucial to my success in completing this dissertation. First of all, I have been privileged to have the direction and unfailing guidance of an excellent mentor, my chair, John Strauss. Professor Strauss made invaluable contributions to my thesis. He has always motivated me to do my best work and offered timely feedback and keen insights into the significance of my research. Professor Strauss has been like a father to me for the past three years. His supervision and advice has helped substantially to my study and my stay at MSU. He is the reason I feel very lucky to have come to Michigan State University. I am also grateful to Jeff Biddle, John Goddeeris, Jeff Wooldridge, John Giles, and Andrei Shevchenko, who have contributed insightful and critical comments and suggestions to my research. I am deeply grateful to Albert Park, my other mentor, who introduced me to a fascinating study of development economics. Albert also provided me with his kind encouragement and wise guidance at crucial times. Most of all, he has never failed to believe that I can succeed.

My heartfelt appreciation goes out to Jan Svejnar for his constant support and his kind words of encouragement, David Neumark and Dale Belman for believing in me and giving me an opportunity to teach and learn a great deal from teaching, and Thomas Jeitschko and the department of Economics at MSU for making my academic experience go smoothly throughout the years. Thanks also to Firman, my esteemed colleague, whom I learned a lot from. I have enjoyed the camaraderie of Firman, Nina, Dung, Mu Ren, Lebohang, Elda, Elena, and Paula, whom I have benefited much from their companionship throughout the years. Thanks them for taking me in for a wonderful time at MSU. Many other people have helped me weather the emotional storms and stress throughout my study, some of whom deserve a special mention. Thanks to Laura and Anna for their wonderful friendship and their dependable and enduring support. Foremost, I thank Daeng for being there for the many laughs and tears, for loving me in all good and bad times, and for being my best friend. I could never ask for a better friend.

Finally, I owe much to my family. My sister and brothers have always offered me their support, sympathy, and admiration, my sister and my younger brother especially, who in many ways are my number one fans. I owe to my parents, to whom this dissertation is dedicated, for always believing in me and encouraging me to achieve my goals. My parents have always been there for me. My father has never failed to do what he could to make me feel at home away from home. My mother has been my best listener. She always listened to what I had to say no matter how long and how uninteresting. Her steadfast emotional support has been invaluable in helping me to focus on my academic pursuits. My parents forever inspire me with their infinite capacity for love, joy, and faith.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1

CRISES AND CHILD HEALTH OUTCOMES: THE IMPACTS OF ECONOMIC AND DROUGHT/SMOKE CRISES ON INFANT MORTALITY AND BIRTHWEIGHT IN INDONESIA

Introduction

This paper examines the impacts of the recent Asian financial crisis on infant mortality and birthweight in Indonesia. There have been a number of economic and policy studies focusing on impacts of economic crises on finance and production. Although some studies provide evidence of negative impacts of economic crises on real outcomes, little is known about the impact of economic crises on child health outcomes such as nutrition, and mortality. Often, the association between financial and production disturbances and these outcomes are assumed (e.g. an adverse shock to production is thought to be associated with worse child health outcomes.). This paper utilizes data from the Indonesian Family Life Survey (IFLS) to examine impacts of the crises on child health outcomes directly. Specifically, we study the impacts of the crises on birthweight and infant mortality.

Prior to the Asian financial crisis, Indonesian rising levels of income, education, and public health programs had been successful in reducing infant mortality and improving the overall health of children. The infant mortality rate was reduced by more than half between the 1960s and the 1990s: from 145 deaths per one thousand live births in 1967 to 46 in 1997 (World Bank, 1999).[1] Table 1a shows neonatal mortality and post-neonatal mortality rates[2] prior to the crisis from the 1997 Demographic Household Survey. Data from this table shows that the rates of neonatal mortality and post neonatal mortality noticeably declined over the period of 15 years from 1982 to 1997. Indonesians experienced higher decline in post-neonatal mortality rate (from 31.0 to 23.9 per 1,000 live births) than in neonatal mortality rate (from 28.4 to 21.8 per 1,000 live births).

The Asian financial crisis struck Indonesia in January 1998.[3] Figure 1 shows that the sustained crisis period lasted more than one year with the peak in Rupiah/USD exchange rate in July 1998. As shown in Figure 2, the food prices in both urban and rural areas increased more than 250 percent at the peak of the crisis. This substantial increase in food prices is argued by Alatas (2002)[4] to be a major source of impacts of the crisis felt by Indonesians, except those in the top of the income distribution. Simulation results from Alatas's study indicates that the increase in food prices between February 1999 and February 2000 accounted for approximately a 40 percent of the increase in the poverty rate after the crisis. According to Strauss et al (2002), food expenditures (excluding expenditures on tobacco and alcohol) accounted for approximately 50 percent of a typical

---

[1] Daly, Patricia and Fadia Saadah. "Indonesia: Facing the Challenge to Reduce Maternal Mortality." East Asia and the Pacific Region Watching Brief. World Bank. June 1999. Issue 3.

[2] Neonatal mortality is defined as death before one-month old. 0-28 and 0-30 day periods are both used among researchers. This paper uses 0-30 days. Post-neonatal mortality is defined as death at ages 1 to 11.9 months. Infant mortality is defined as deaths at age 0 to 11.9 months.

[3] Refer to figure 1.

[4] Alatas, Vivi. "What Happen to Indonesia's Poverty? A Micro Simulation Exercise Using Household Surveys. Manuscript. World Bank. Jakarta, Indonesia. March 2002.

Indonesian's household budget in urban areas and 57 percent in rural areas. The food shares of household budget are higher among the poor. [5]

While an increase in food prices could help net food-producers, this large increase in the food prices during the crisis resulted in a sharp reduction of real incomes for most of the Indonesian population, who are net food purchasers. Frankenberg, Thomas, and Beegle in "The Real Costs of Indonesia's Economic Crisis: Preliminary Findings from the IFLS2+," (1999) [6] reported that the proportion of households below the poverty line rose from about 11 percentage points in 1997 to almost 20 percentage points in 1998.[7]

Overall government health expenditures per capita were not sustained at the peak pre-crisis level. Figure 3 shows real government health expenditures from 1980 to 2000. According to Leiberman et al. (2001),[8] the government per capita outlay on heath expenditures fell significantly during the crisis by 2.9 percent and by 6.6 percent in successive years. Then in 1999/2000, the expenditures rebounded. However, during the

---

[5] Between 1997 and 1998 there was a significant increase in the food share in both rural and urban households. According to Frankenberg, Thomas, and Beegle (1999), the increase in food shares was concentrated among households whose per capita expenditure was below the median of the population. A significant portion of the increase in food share can be attributed to an increase in the share of the expenditures on staples (from 13 percent to 21 percent for urban households and from 31 percent to 39 percent in rural households). As a result of a significant increase in the share of staples, some food shares such as that of meat are reported to decline. According to Strauss et al. (2002), nominal wages also increased during the financial crisis, but the increase was considerably less than the increase in food and nonfood prices. Therefore, real wages for those that rely on market wages also declined.

[6] Frankenberg E., Thomas D., and Beegle K. 1999. "The real costs of Indonesia's Economic Crisis: Preliminary Findings from the Indonesia Family Life Surveys." DRU-2064-NIA/NICHD. Santa Monica, CA: RAND.

[7] Allowed for higher inflation in rural than urban areas as indicated by the price data collected in the IFLS communities. (Frankenberg E., Thomas D., and Beegle K. 1999. "The real costs of Indonesia's Economic Crisis: Preliminary Findings from the Indonesia Family Life Surveys." DRU-2064-NIA/NICHD. Santa Monica, CA: RAND.)

[8] Lieberman, S., M. Juwono, and P. Marzoeki. "Government health expenditures in Indonesia through December 2000: An Update." World Bank East Asia and the Pacific Region Watching Brief. October 15, 2001, Issue 6.

crisis, Indonesia received a 278 percent increase in donor assistance, which contributed to the sustainability of health financing and spending. This donor assistance helped dampen the shock to the government budget, resulting in a five percent decrease in overall outlays[9].

According Frankenberg et al. (1999), prices of both public and private healthcare increased from late 1997 to late 1998, but public healthcare prices increased relatively more than the prices of private healthcare. The median price of BCG immunization for children and tetanus toxoid immunization for pregnant women rose significantly in public facilities,[10] but not in private facilities. There was a decrease in the quality of public healthcare such as a reduction of the quantity of drugs given to patients and an increase in the number of referrals to other providers. In terms of supplies, public facilities were found to have been more affected by changes in the availability of drugs and supplies (such as injections and bandages) while private facilities were more affected by the price increase of these inputs. Overall, public and private facilities that provided vitamin A declined in number. Vitamin A is essential for children under three since it reduces their vulnerability to infectious disease. Those above the median of the income distribution were found to shift away from public healthcare. Visits to the *posyandu* (healthcare post) by children under five dropped from 46.7 percent to 27.7 percent.

In addition to the financial crisis, some rural areas in Indonesia were badly affected by the 1997-1998 drought. The drought crisis was a consequence of climatic

---

[9] Lieberman, S., M. Juwono, and P. Marzoeki. "Government health expenditures in Indonesia through December 2000: An Update." World Bank East Asia and the Pacific Region Watching Brief. October 15, 2001, Issue 6.

[10] The prices rose from 500 Rp. in 1997 to 750 Rp. in 1998 for Child immunization and from 500 Rp. in 1997 to 900 Rp. in 1998 for Tenanus Toxoid.

conditions identified with the El Nino Southern Oscillation (ENSO). The impacts of the ENSO on Indonesia included postponement of monsoons, damaging rainless winds, greater rate in infestations, disturbances in fishing patterns, scarcity of drinking water and forest fires. [11] In Kalimantan and Sumatra, the drought, combined with a political decision on land clearing for plantation concessionaires, local populations, and new immigrants, led to fires that lasted for months between late 1997 and early 1998. The fires affected 9.75 million hectares and over 700 million tons of carbon were emitted into the atmosphere causing a major health hazard in Indonesia, Singapore, and Malaysia.

An apparent result of the drought crisis was a sharp drop in food production, especially rice (Indonesia's major food crop). Figure 4 shows that the amount of land harvested for rice in late 1997 was smaller than usual. The harvesting cycle in late 1997 and early 1998 was delayed by as much as two months. It has been reported that some of the worst damage was to Maluku, Nusa Tenggara, and parts of Sulawesi and southwestern Sumatra.[12] Farmers in the driest and poorest provinces in Indonesia were reported at times to survive on one meal a day and to eat food generally reserved for livestock.[13] As a result of the drought crisis, Indonesia became a large food aid recipient in 1998.

---

[11] James J. Fox. "The 1997-1998 Drought in Indonesia." Natural Disasters and Policy Responses in Asia: Implications for Food Security. Harvard University Asia Center. August, 1999.

[12] James J. Fox. "The 1997-1998 Drought in Indonesia." Natural Disasters and Policy Responses in Asia: Implications for Food Security. Harvard University Asia Center. August, 1999.

[13] James J. Fox. "The 1997-1998 Drought in Indonesia." Natural Disasters and Policy Responses in Asia: Implications for Food Security. Harvard University Asia Center. August, 1999.

According to Sastry in his study, "Forest Fires, Air Pollution, and Mortality in Southeast Asia,[14] smoky haze caused by a widespread series forest of fires in Indonesia between April and November 1997 had possible short-term and long-term effects on health. Possible problems that may have resulted in mortality include respiratory infections and chronic conditions. Sastry uses levels of micro-particulate matter with diameter less than 10 microns (PM_{10}) and the mean daily visibility in kilometers as measures of air quality to analyze the effects of smoke haze on mortality (non-traumatic, cardiovascular, respiratory deaths) of population of different age groups (all ages, <1, 65-74,and >74 years) in Kuala Lumpur and Kuching, Malaysia, in 1996-1997. Sastry found significant short-term cumulative effects of smoke haze on mortality in all age groups. Sastry claims that the displacement of deaths from the smoke haze was short-term, however, in one segment of the Malaysian population -- those age 65-74 in Kuala Lumpur--- there was an upward shift in mortality that lasted at least a few weeks. Sastry suggests that an implication of his results on the short-term effects of the smoke haze in Malaysia is that the effects in Indonesia, where the main fires took place, are likely to have been large.

Evidence from IFLS2+ report suggests that the short-run impacts of the financial and the drought/smoke crises on children's health have been small. Results from physical assessments show no deterioration in children's health status. There were only negligible changes in the measurements of children older than six months in their height-for-age and weight-for-height. Very young children were well protected from the effects

[14] Sastry, Narayan. "Forest Fires, Air Pollution, and Mortality in Southeast Asia." Demography. Volume 39 number1. February 2002.

of the crisis although there is a suggestion that weight-for-height of this group of children may have worsened (Frankenberg, Thomas, and Beegle, 1999).

This paper uses IFLS data to examine the effects the crises on neonatal mortality, post-neonatal mortality, and birthweight. Mortality status and birthweight of children of different cohorts (born during pre-crisis, crisis, and post-crisis periods) are examined. In addition, detailed data on births and times of death of children allow this study to examine mortality hazard rates at different specific ages of children.

Estimated results on mortality outcomes show that the financial crisis had adverse impacts on neonatal mortality in both urban and rural areas. The adverse effects of the financial crisis on post-neonatal mortality risks were larger and more statistically significant for urban infants than for rural infants. Overall, the financial crisis increased infant mortality risks by about 3.2 percentage points in both urban and rural areas, a very large effect.

The drought/smoke crisis adversely affected post-neonatal mortality risks in rural areas. The increase in the post-neonatal mortality risk is about 3.1 percentage points. When community-fixed effects are controlled for, the drought/smoke crisis had much larger effects. Overall, the drought/smoke crisis had no statistically significant adverse effects on infant mortality in urban areas, while the effects in rural areas were large. Our estimates show that rural infants born during the drought/smoke crisis experienced approximately 4.4 percentage points increase in their infant mortality risks.

Results from hazard models confirm that the financial crisis and the drought crisis had adverse effects on neonatal and post-neonatal mortality. The financial crisis increased the odds of both neonatal and post-neonatal mortality for urban children more than for rural children. As expected, in rural areas, the crisis-noncrisis differential of the mortality risk at specific age (months) is relatively smaller for financial crisis than that of the drought crisis.

Our findings on differential effects on infants born to mothers with different levels of education show that in urban areas infants born to mothers with different levels of education exhibited significantly different trends in mortality risks over time. We also find that even though some of the crises had adverse effects on infant mortality, infants born to mothers with different education levels did not experienced different adverse crisis effects.

Results from the cumulative distribution comparisons of birthweights suggest that the financial crisis also had adverse impacts on birthweight in urban areas. However, under multivariate analyses, the adverse effect seems to disappear. None of the crises affected birthweights in rural areas. The lack of an evidence on the adverse effects may be due to a selection problem in reported birthweights.

Conceptual Framework and Literature

The financial crises and the drought/smoke haze crisis represent short-term exogenous shocks to Indonesian households. Although specific biological mechanisms by which smoke haze may affect child health outcomes are not directly estimated,[15] both crises are expected to have negative consequences on child health through resource availability, income, prices, and environment.

According to agricultural household models (Singh, Squire, and Strauss (1986); Hill et al. (1993)), in a country with both agriculture and manufacturing sectors, an adverse agricultural shock not only reduces average resource availability but also affects the distribution of resources between the two sectors, largely through its effects on the relative prices of goods and services households consume. The direction of the effect depends on how the price of the agricultural good, expressed in terms of the manufactured good, responds to the shock.

With an increase in the price of agricultural goods after an adverse shock, agricultural households who are net producers may receive a greater amount of manufactured good for each unit of the agricultural good it does produce although each agricultural household produces less. However, if the price of manufactured goods also significantly increased during the crisis, these net sellers of agricultural goods could suffer from an overall price increase.

While an increase in food prices could help net food-producers, households in sectors not benefiting either directly or indirectly from the favorable price movement such as net buyers of rice may face an erosion of their purchasing power as the relative prices of goods change. As noted earlier, a large increase in food prices and an adverse income shock during the financial crisis resulted in a sharp reduction of real incomes for most of the Indonesian population, who are net food purchasers.

An important dimension of a reduction in purchasing power that is relevant to this paper is through changes in the costs of raising children. These costs include both direct and indirect (opportunity) costs. When there is a reduction in purchasing power as a result of adverse income shocks or a rise in the price of agricultural goods, households are likely to reduce their consumption of health inputs and family planning services, provided that these goods and services are normal goods to the households. In addition, the effects of the crises on consumption could be worsened if there is an increase in healthcare costs and a reduction in availability of healthcare services, which are widely provided by the government.

It is, however, worth noting that a drought or a financial crisis in a particular year may have only a limited effect on the household's resources in that year if the household can transfer resources from another place or another time through the formal sector (such as bank loans or crop insurance) or the informal sector (such as loans or transfers from family or friends). However, if the shock occurs at the aggregate level, for instance a widespread financial crisis, the ability to transfer resources could be limited.[16] In this case, we expect to observe short-run negative effects of the adverse shock on health outcomes such as in child mortality.

---

[15] We do not estimate health production function in this paper.

[16] See Bardhan and Udry (1999) for detailed discussions.

A large mainstream body of research on the effects of short-term economic fluctuations on demographic outcomes has used time-series data. The most influential work in this area is the study by Lee (1981) of the impacts of conditions in pre-industrial North and Western Europe using economic indices such as grain prices and weather.[17] The evidence from this study shows that post-infant mortality is positively associated with real prices. In a similar study, Galloway (1988)[18] found that a ten percent increase in grain prices in pre-industrial Europe leads to an increase of approximately 1 percent in mortality.[19]

A number of authors have tried to study the effects of economic fluctuations on demographic outcomes in less developed countries.[20] A major effort is a collective work

---

[17] R. Lee, " Short-term variation: vital rates, prices and weather," in E.A. Wrigley and R.S. Schofield (eds.), The Population History of England, 1541-1871 (Cambridge, Mass: Harvard University Press, 1981), P. Galloway, "Basic Patterns of annual variations in fertility, nuptiality, mortality, and prices in pre-industrial Europe," Population Studies, 42 (1988), pp. 275-302, D. Weir, "Life under pressure: France and England, 1670-1680," Journal of Economic History, 44 (1984), pp. 27-47. T. Richards, "Weather, nutrition, and the economy: the analysis of short-run fluctuation in births, deaths, and marriages, France 1740-1909," in T. Bengtsson et al. (eds.), Pre-Industrial Population Change (Stockholm: Almquist and Wiksell, 1984).

[18] Galloway, P. "Basic pattern in annual variations in fertility, nuptiality, mortality, and prices in pre-industrial Europe." Population Studies 42(2): 275-302. 1988.

[19] Although he observed that the relationship between prices and mortality in countries that are more developed economically is weaker.

[20] J. Bravo, "Economic Crisis and mortality: short and medium-term changes in Latin America." Paper presented at the Conference on the Peopling of the Americas, Veracruz, Mexico (1992). J. Brovo, "Demographic Consequences of structural adjustment in Chile." Paper presented at the Seminar on Demographic Consequences of Structural Adjustment in Latin America, Ouro Preto, Brazil (1992). K. Hill and A. Palloni, " Demographic responses to economic shocks: The Case of Latin America." Paper present at the Conference on the Conference on the Peopling of the Americas, Veracruz, Mexico (1992). A. Palloni and K. Hill, "The Effects of Structural Adjustments on mortality by age and cause in Latin America." Center for Demography and Ecology. Working Paper 92-22, University of Wisconsin, Madison, Wis. (1992). D. Reher and J.A. Ortega, " Short run economic fluctuations and demographic behaviour: some examples from twentieth century South America." Paper presented at the Seminar on Demographic behaviour: some examples from twentieth century South America." Paper presented at the Seminar on Demographic Consequences of Structural Adjustment in Latin America, Ouro Preto, Brazil (1992).

by the National Research Council.[21] In this study, first marriage, timing of first and second birth, and child mortality in seven countries in Sub-Saharan Africa were examined using data from the Demographic Health Surveys (DHS) that were conducted in each of the seven countries some time in the period between 1986 and 1990. In this paper, various economics indicators are used as measures of economic fluctuations. The indicators used are the gross domestic product per capita, the quantity of exports, term of trade, and commodity prices. The authors find strong evidence of adverse effects of economic reversals on time of first marriage and first births.[22] They, however, found no effect of the economic reversals on child mortality net of trend, except in Ghana (in rural areas) and Nigeria.

Palloni, Hill, and Aguirre[23] employed distributed lag models and used average GDP to study the effects of short-term economic fluctuations on marriage, marital fertility and mortality during 1920-1990 in Latin America. They found the directions of the response of the number of marriages are not uniform across countries. The net effects (the sum of all lagged coefficients) are positive in all cases, except Guatemala and Mexico, while the magnitudes of the net effect elasticities vary across countries, from 0.01 (in Mexico) to 0.67 (in Chile). Their results from the estimated responses of births show considerable greater heterogeneity. In five out of eleven countries, the coefficients

---

[21] Hill, K, G. Adansi-pipim, L. Assogba, A. Foster, J. Mukiza-gapere, and C. Paxson. " Demographic Effects of Economic Reversals in Sub-Saharan Africa. National Research Council.National Academy Press. Washington D.C. 1993.

[22] Their results are strongest for the effects on first birth. The authors found a positive relation to economic variation net of trend in all seven countries studied, except Kenya. Evidence of marriage delays as a result of economic reversals were found in Botswana, Senegal, and Togo (especially in urban areas).

[23] Palloni, A, K. Hill, and G.P. Aguirre. "Economic Swings and Demographic Changes in the History of Latin America. Population Studies, 50 (1996), pp. 105-132.

for lags 0 and 1 of the average GDP are positive as expected, but only lag1 in Cuba is significantly different from zero. The estimated net effects are positive in only seven countries, and the magnitude of the net effect elasticity ranges from 0.04 (in Chile) to 0.88 (in Cuba). The effects on infant mortality rate also display considerable heterogeneity in terms of direction of the effects. Results from only five countries are negative, as expected, and the estimated elasticity of the net effects ranges from 0.08 (in El Salvador) to 0.61 (in Panama). The results from pooled sample estimations using pooled sample (except Cuba) give estimated elasticity of the net effects of 0.12, 0.19, and -0.12 on births, marriages, and infant mortality rate respectively. An important finding as suggested by the authors is that the effects of the lag0 and lag1 on infant mortality are significant, but their absolute size are small (-0.10, and 0.00), about one-third that of the effects on marriages and births.

Mckenzie (2002), in his study of how Mexican households coped with aggregate shocks of the Peso crisis of 1994-1996, finds little role for inter-household transfers in consumption smoothing in the presence of aggregate shocks. He finds that the average transfer that households made to non-household members was reduced by 25 percent. Although remittances to Mexican households from friends and family members in the United States increased, on average the households received 19 percents less gifts and donations from other Mexican households.

Another pioneer empirical study in this area is the study of child mortality of children born or conceived during the Dutch Hunger Winter of 1944-45 by Stein *et al.*[24] This study uses vital registration records to compare age-specific mortality rates for different cohorts. The authors found an excess mortality of children born or conceived during the famine crisis. However, due to data limitations, the authors control for only social status (manual/non-manual occupation).

In their study of the effects of the 1974-75 famine in Bangladesh, Razzaque *et al.* (1990)[25] address the above data issues using richer data from Matlab field research in a rural area in Bangladesh. Mortality of children of different cohorts, born during famine, post-famine, and non-famine periods, is examined using various socioeconomic controls such as child's gender and household economic status. Using linear logistic regression models, they found excess mortality of children born or conceived during the famine. However, Razzaque and his colleagues found that the effects of the famine on mortality were not uniform. Better off household experienced little effect.

The incidence of the recent Indonesian financial crisis and the 1997/1998 drought crisis and the availability of IFLS data allow this study to address the affects of larger exogenous short-run shocks on demographic outcomes. This paper uses IFLS data to examine the effects of the crises on neonatal mortality, post-neonatal mortality, and birthweight. By using IFLS3 and earlier IFLS waves (IFLS1 and IFLS2), mortality data on child cohorts that were born during pre-crisis, crisis, and post-crisis periods can be examined. In addition, we can allow for the time trends that are independent of the crises when studying these adverse effects. Since the IFLS covers respondents in both rural and urban areas of Indonesia, the area-specific impacts of the crisis can also be studied. In

---

[24] Stein, Z., M.Susser, G. Saenger and F. Morolla, "Famine and Human Development: The Dutch Hunger Winter of 1944-1945, New York (1975).

[25] Razzaque, A., N. Alam, L. Wai, and A. Foster. "Sustained effects of the 1974-75 famine on infant and child mortality in a rural area of Bangladesh." Population Studies 44: 145-154. 1990.

addition, detailed data on births and times of death of children allow this study to examine mortality hazard rates at different specific ages of children.

A Theoretical Model

The theoretical model in this paper is an adaptation of the work by Foster (1995).[26] Consider a household j. Abstracting from fertility selection, assume parents care about the health of their surviving children, but not their individual consumption. Denote t the beginning of period t. Let h_{it} be the health status of the child i at time t and C_t a vector of the household's consumption of goods and services other than those that are inputs in the production of children's health. Single period household's utility function is assumed to be additively separable between consumption and child's health. The utility function is also assumed to be increasing and concave in household consumption and child's health status ($v'(C_t) > 0, v''(C_t) < 0$ *and* $u'(h_{jit}) > 0, u''(h_{jit}) < 0$).

The expected discounted utility of household j at time s is

$$V_s = E_s \sum_{t=s}^{T} \beta^{t-s} \left[v(C_t) + \sum_{i=1}^{I} u(h_{it}) \cdot M_{it} \right],$$

where E_s the expectation conditional on information at time s, β is the discount factor, and I is the total number of children in the household, and the subscript j is dropped for notational simplicity. $u(h_{it})$ is the utility of the household from the health of the child i, h_i. M_{it} is the child i's mortality index, where $M_{it} = 1$ if the child is alive in period t and $M_{it} = 0$ if the child is not alive in period t. In every period t, the child dies with certainty when his health, h_{it}, is below a heath threshold, h_{it}^*.

---

[26] Foster, Andrew D. "Price, Credit Markets and Child Growth in Low-Income Rural Areas." The Economic Journal, Volume 105, Issue 430 (May, 1995), 551-570.

The production function of the child's health status in the current period is characterized as a function of the child's health status in the last period, augmented by health inputs in the last period. Let h_{it} be the child's health status at time t given characteristics at the individual level (such as age and gender), household level (such as education of the mother), and community level (such as quality of water supply), z_{it}, and unobserved factors that affect health, δ_{it}. Following Foster (1995), the effects of health inputs, N_{it}, in the last period on the current period health status are assumed to be proportional to a time-varying rate at which health inputs are translated into health status gain. This rate is a function of the child's characteristics and unobserved factors δ_{it}. Parents are assumed to know z_{it} with full certainty. The health status of the child at time $t+1$ can be characterized as

$$h_{i\,t+1} = f_{it}(h_{it}; z_{it}, \delta_{it}) + k_{it}(z_{it}, \delta_{it})N_{it},$$

where $\frac{\partial f_{it}(h_{it}; z_{it}, \delta_{it})}{\partial h_{it}} > 0$ and the Mortality index can be characterized as

$$M_{it} = \begin{cases} 1 & if \quad h_{it}(h_{it-1}, N_{it-1}, z_{it-1}, \delta_{i-1t}) > h_{it}^{*}(z_{it}, \varepsilon_{it}) \\ 0 & Otherwise \end{cases},$$

where ε_{it} is unobserved factors, unknown to the household, that affect the mortality threshold at time t.

Assume no inter-temporal borrowing and lending, the household's budget constraints at time t is

$$p_t^C C_t + p_t^N \sum_{i=1}^{I} N_{it} = y_t$$

By the Law Iterated Expectation, the expected discounted utility of household j at time s can be written as

$$V_s = E_s \sum_{t=s}^{T} \beta^{t-s} \left[v(C_t) + \sum_{i=1}^{I} \left(u(h_{it}) . \, prob(M_{it} = 1 \,|\, t-1) \right) \right],$$ [27]

Let function g_{it} characterize mortality risk of the child i in period t;

$$g_{it}(h_{it}, z_{it}, \varepsilon_{it}) = prob(M_{it} = 0) = prob\left(h_{it}(h_{it-1}, N_{it-1}, z_{it-1}, \delta_{i-1t}) < h_{it}^{*}(z_{it}, \varepsilon_{it}) \right),$$
$$where \; g_1' < 0 \; and \; g_1'' > 0$$

Suppose there is a temporary adverse income shock at period 0, either C_0 or N_{i0} or both will be reduced in order to satisfy the budget constraints at time 0. If children's health inputs are considered normal goods, N_{i0} will be reduced as a result of a reduction in income. Supposed N_{i0} is reduced by dN_{i0}, then the health status of the child i in period

---

[27] Proof:

$$V_s = E_s \sum_{t=s}^{T} \beta^{t-s} \left[v(C_t) + \sum_{i=1}^{I} u(h_{it}) \cdot M_{it} \right]$$
$$= E_s \sum_{t=s}^{T} \beta^{t-s} v(C_t) + \sum_{t=s}^{T} \beta^{t-s} \sum_{i=1}^{I} E_s \left[u(h_{it}) \cdot M_{it} \right]$$

By the Law of Iterated Expectation,

$$V_s = E_s \sum_{t=s}^{T} \beta^{t-s} v(C_t) + \sum_{t=s}^{T} \beta^{t-s} \sum_{i=1}^{I} E_s \left[E_{t-1, h_{it}} \left(u(h_{it}) \cdot M_{it} \right) \right]$$
$$= E_s \sum_{t=s}^{T} \beta^{t-s} v(C_t) + \sum_{t=s}^{T} \beta^{t-s} \sum_{i=1}^{I} E_s \left[u(h_{it}) \cdot prob(M_{it} = 1 \,|\, t-1) \right]$$
$$= E_s \sum_{t=s}^{T} \beta^{t-s} \left[v(C_t) + \sum_{i=1}^{I} \left(u(h_{it}) . \, prob(M_{it} = 1 \,|\, t-1) \right) \right].$$

1 will be reduced by $k_{i0}(z_{i0},\delta_{i0})\,dN_{i0}$. Furthermore, this temporary adverse income shock will have spillover effects on future health status of the child in every period after period 0 as the health status at any period *t* depends on the health status in period *t-1* in the health production function. Specifically, a decrease in N_{i0} by dN_{i0} will reduce the health status of the child *i* in period *t* by

$$\frac{\partial h_{it}}{\partial N_{i0}}.dN_{i0} = \left(\prod_{s=1}^{t} \frac{\partial f_{is}(h_{is};z_{is},\delta_{is})}{\partial h_{is}}\right).k_{i0}(z_{i0},\delta_{i0})\,dN_{i0}.$$

The effect of a temporary adverse income shock on mortality risk

An adverse shock to income in period 0 that results in a change of health inputs by dN_{i0} will have adverse effects on the mortality risk through a reduction of the health input by

$$\frac{\partial\, prob(M_{it}=0)}{\partial y_0} = \frac{\partial g_{it}}{\partial h_{it}}\frac{\partial h_{it}}{\partial N_{i0}}\frac{\partial N_{i0}}{\partial y_0} = \frac{\partial g_{it}}{\partial h_{it}}\left(\prod_{s=1}^{t} \frac{\partial f_{is}(h_{is};z_{is},\delta_{is})}{\partial h_{is}}\right).k_{i0}(z_{i0},\delta_{i0})\,dN_{i0}$$

Measuring Mortality Rates

There are two principal estimation methods to calculate mortality rates: direct methods and indirect methods. (Spiegelman, 1955;[28] Pressat, 1978[29]) Direct methods calculate mortality directly using data on the date of birth of children, survival status, and the dates of death or ages at death of deceased children.

---

[28] Spiegelman, M. Introduction to Demography. 1955. The Society of Actuaries. Chicago. Illinois.

[29] Pressat, R. *Statistical Demography.* Translated and Adapted by Damein A. Courtney. 1978. Methuen & Co Ltd.

The direct methods require data that are usually only obtained in specifically designed surveys with birth/pregnancy histories or from vital statistics systems. There are three variants of direct estimation methods: a vital statistics approach, a synthetic life table approach, and a true cohort life table approach.

"A vital statistics approach" is an approach in which the number of deaths to children under age 12 months in a particular period is divided by the number of births in the same period. Under "a synthetic cohort life table approach," mortality probabilities for small age segments based on real cohort mortality experience (e.g. 0, 1-2, 3-5, 6-11 months) are first calculated. Then these component death probabilities are combined into the mortality rates, taking into account exposure to mortality risk of each age cohort. Specifically, component death probability for each small age segment is calculated by dividing the number of deaths to live-born children during specified age range and specified time period by number of surviving children at beginning of specified age range during the specified time period. Births and death incidences within each specific age and specific time ranges are weighted according to exposure to mortality risk, which is indicated by birthdate and mortality risk of interest (e.g. neonatal mortality). This approach allows full use of the most recent data and is also specific for time periods. However, this approach requires intensive computation and the number obtained using this method could be influenced by the arbitrary length of age segments used.[30]

Under "a true cohort life table approach," the number of deaths to children under age 12 months of a specific cohort of births are divided by the number of births in that

---

[30] Studies done during the World Fertility Survey have shown the difference to be negligible when using monthly segments and using 0, 1-2, 3-5, 6-11, 12-23, 24-35, 36-47, 48-59 months segments. (Demographic Health Survey).

cohort. This method gives true probabilities of death but requires that all children in the cohort must have been fully exposed to mortality risk.[31] Although this method does not take into account the most recent experience because of exposure requirement, it is the method we chose in this paper since this method gives true probability of death while the full exposure problem is mitigated by using IFLS3, which includes information on children born in the crisis. Most of these children had had full exposure to neonatal and post-neonatal mortality risk by the time of the interview in 2000. Moreover, the problem is less severe because information of children born in the IFLS survey year (who had less than one year mortality exposure) can be added using data from subsequent IFLS waves so that all of these observations are fully exposed to infant mortality.[32]

An "indirect method" is used to calculate death rates when age-specific death rates for the community are not available, but the total number of deaths is known. By this method, the number of actual age-specific deaths in the community is multiplied by a constant number of the age-specific death rates[33] in a standard population that are usually derived from European experience, which are referred to as the "*standard mortality schedule.*" The result yields the adjusted death rates by the indirect methods. (Spiegelman, 1955; Pressat, 1978).[34]

---

[31] This requirement of full exposure becomes more limiting the higher the age segment of interest. For example, to calculate under-five mortality rates, only information on children born at least five years before the survey can be used.

[32] Please refer to data section for detailed discussions. Later in this paper, monthly hazard rate estimations are performed to avoid the problem of full exposure to infant mortality risk.

[33] The number of deaths between ages x and x+n among residents in a community during a year divided by the average number of persons between ages x and x+n living in that community during the year, multipled by 1,000. (Spiegelman, 1955)

Under indirect methods, mortality rates are calculated using only the number of children ever born, the number of living children to women, and ages of women. The indirect methods can utilize data that are commonly collected in censuses and many general surveys.[35] Although these indirect methods can utilize data that do not contain detailed information on date of birth of children and the dates of death or ages at death of deceased children, these methods are based on an implicit assumption that the births of a cohort of a women represents all children born in a time period.[36] Another problem with indirect methods is that the indirect methods estimate the probability of dying based on experience that can extend over many years, resulting in an average over that period. The methods are subject to error when there are changes in fertility and mortality trends, which occured during 1960s -1990s in Indonesia.

Note that both direct and indirect methods could suffer from reporting error due to the omission of deceased children. Estimation of infant mortality using direct methods also depends on the correct reporting of age at death as under or over one year. The heaping of deaths at age 12 months is also common, and to the extent that it causes a transfer of deaths across the one-year boundary, infant mortality rates may be somewhat underestimated.

---

[35] "Because many late fetal deaths and neonatal deaths may be attributed to the same underlying conditions, it has been proposed to combine the two to compute a "perinatal rate," where the deaths are divided by either live births alone, or the sum of live births and fetal deaths. Since there is no generally accepted convention for computing this rate, the terms entering into it should be defined wherever it is used." (Spiegelman, 1955). One can conclude that although the indirect methods can be used to extract other mortality rates from census data, perinatal rates cannot be computed because census data give only birth history, where only live births are recorded.

[36] Documentation by the Demographic Health Survey reports that recent and on-going work shows that this assumption may not be valid: births to women 20 to 24 (and in some cases to women 25 to 29) have more elements of high risk of mortality than do all children born within the last five years of a survey.

Data

This paper uses data from the Indonesia Family Life Survey (IFLS). IFLS is a continuing longitudinal socioeconomic and health survey that includes more than 30,000 individuals living in 7,200 households. The sample covers 321 communities in 13 provinces in Indonesia and represents about 83 percent of the Indonesia population in 1993[37]. The first wave of IFLS was fielded in 1993 (IFLS1). The same households were revisited in 1997 (IFLS2) and again in 2000 (IFLS3). This paper the data from these three IFLS waves. A 25 percent sub-sample of households was re-interviewed in 1998 (IFLS2+), but the data are not used in this paper.

In IFLS surveys, special attention is paid to the measurement of health, work, migration, marriage, child bearing, life history data on education, and economic status of individuals and households. In each wave of IFLS, the individual and household surveys are complemented by an extremely comprehensive community and facility survey. There is also considerable attention placed on minimizing sample attrition in IFLS. Targeted households and individuals who "split-off" from original households were followed if they moved to new locations within 13 provinces of the survey areas. In each re-survey, about 95 percent of targeted households have been re-contacted. The split-off households added just under 1,000 households to the sample in 1997 and about 2,600 households in 2000.

The survey periods of each IFLS wave is shown in Figure 1. Data on pregnancies, birth outcomes, infant mortality, and age of death are obtained from

---

[37] Frankenberg, E., Hamilton, P., Polich, S., Suriastini, W., and D. Thomas. User's Guide for the Indonesia Family Life Survey. DRU2238/2-NIA-NICHD. March 2000.

retrospective data on pregnancy histories. Complete pregnancy histories were given by women in the household who were between 15 and 49 years of age. These women were asked about information of each pregnancy in detail. This information includes pre-natal care, pregnancy outcome, birth information, post-natal care, and survival status of the child. For prenatal-care, complete information on frequency and type of pre-natal care of each trimester of pregnancy was obtained. For birth information, the women were asked detailed information about their pregnancy outcomes. In the case where there was a miscarriage, the length of time before the pregnancy ended was reported. If the pregnancy resulted in a live birth, information was obtained retrospectively on length of pregnancy, birth date, place of birth, healthcare provider at birth, whether the child was weighed at birth, and birthweight (if the child was weighed). For the child's survival status, mothers were asked if their children were alive at the interview date. If the child died before the interview date, a complete history of the child's death was obtained. This information includes how old the child was when the child died (in days, weeks, months, or years).

Data are obtained from IFLS1, IFLS2, and IFLS3, but are restricted to include only children that were born between 1988 and 2000. This is because we want to restrict the recall to five years for each wave in order to minimize recall error. This is similar to strategies used by demographers.[38]

As IFLS2 and IFLS3 follow the respondents of the original IFLS1, birth data are organized as follows:

---

[38] For instance, such as in the Demographic Health Surveys report infant and child mortality based on events in the previous four or five years before the survey.

If the respondent (mother) was previously interviewed and reported some children in earlier IFLS wave(s), the mother reported only children born since the last reported child in the new IFLS wave.

If the respondent was interviewed in the previous wave, but reported no children then, the mother reported all children born after the last interview in the new interview, which amounts to a complete history

If the respondent was a new respondent, either because she turned 15 or older in the new survey or because she was a new household member, a complete history was taken.

As a result, birth data of children born in any particular year could be from any of the IFLS waves. Information on children born during the financial crisis period (1998-1999) can be obtained only from the IFLS3 wave, except for a few children born in January 1998 when IFLS2 was still taking place.

In addition, observations across IFLS waves for panel respondents with preprinted roster are carefully compared to avoid duplication, which occurred occasionally. According to discussions of the quality of retrospective data on longitudinal surveys,[39] retrospective data are of better quality if the length between the real event and the interview date is minimized. Therefore, in the case of duplication, only the observation from the earlier IFLS wave is included. An exception is made for 1997 and 1998 births. Since these children were not at least one year old by IFLS2 interview date, we cannot

---

[39] Beckett, M., J. DaVanzo, N. Sastry; C.Panis, and C. Peterson. "The quality of retrospective data: An examination of long-term recall in a developing country." Journal of Human Resources. Summer 2001.

This paper on reporting error provides insights into the quality of retrospective reports, particularly as it pertains to short-term recall. Studies were reviewed which analyzed the quality of retrospective reports in extract uncensored mortality information from observations that were reported in IFLS2.[40] In this case, data from IFLS3 are used where duplication occurs. Refer to Appendix II for details.

To identify neonatal mortality and post-neonatal mortality, only live births were included. As previously discussed, this paper uses a "true-cohort-life-table approach" to calculate neonatal and post-neonatal mortality rates. This approach requires that the children in our sample had full exposure to mortality risks, which, in our case, means reaching of age.[41] Instead of dropping observations that did not have one-year full exposure to mortality risks by the interview date in IFLS1 (1993) and IFLS2 (1997), an effort was made to recover these observations. For those who were younger than one-year of age by the interview date in these earlier IFLS waves, information on their survival is obtained from the household roster of the subsequent IFLS using identification numbers that are consistent throughout all waves of IFLS.[42] This household roster provides information on whether the child was still living and the time of death if the child had died. In the case where we could not track a particular child from IFLS1 in IFLS2, we also looked for him/her in IFLS3. With a high re-survey rate of about 95 percent, we were able to recover most of the observations. As a result, we are able to add 487 children born in 1993/1992 and 511 children born in 1996/1997 to our sample.

---

the Malaysian Family Life Surveys (MFLS), fielded in Peninsular Malaysia in 1976 and 1988, and conclude that many of the data quality problems found previously are present in the MFLS.

[40] Some respondents of the IFLS1997 were interviewed in January and February 1998.

[41] This full-exposure restriction is not our concern when studying monthly hazard rates later in the paper.

[42] When the child did not live in the same household in subsequent IFLS survey, survival and date of death (if died) information was obtained from the new household.

In addition to data on pregnancy and survival histories, data from individual, household, and community characteristics surveys are used to allow this study to control for other socioeconomic characteristics such as child's gender, mother's education, urban/rural areas of residence, and province of residence.

The residence of mothers we use in this paper is the residence of mother at the time of the survey. We are aware of a general practice of taking the residence of females at the time when they were 12 years old or residence before the first marriage in feritity and marriage studies, but in this study of the effects on mortality, the residence of mothers after their marriages are thought to affect healthcare provided to their children more than pre-marital residence in terms of type and quality of the healthcare. We take the residence of the mother at the time of the survey to be a proxy for the post-marital residence.

The data are linked using individual and household identifications, which are consistent throughout different surveys of each IFLS and different waves of the IFLS.

Method of Analysis

Testing Different Measures of Mortality Rates

The first goal of this paper is to compare mortality rates from IFLS data to the 1997 Indonesian Demographic Household Survey (DHS) data. We use a "true-cohort life table" approach and compared the rates to mortality rates published in the Indonesian DHS 1997 publication, that uses "a synthetic cohort life table" approach. We then compare mortality rates using DHS and IFLS data, but using the "true-cohort-live table" approach with both datasets.

Table 1a, taken from the Indonesian DHS publication, shows neonatal and post-neonatal mortality rates that are calculated using the "synthetic cohort life table" approach. From the same dataset, table 1b shows computed neonatal mortality and post-neonatal mortality rates from the 1997 DHS using the "true-cohort-life-table". Data are divided into three different periods by birth date (1982-1987, 1987-1992, 1992-1997) to match DHS publication numbers. Mortality rates are calculated with and without individual sampling weights provided in DHS.

Table 1b shows that across different time periods, weighted and unweighted mortality rates are similar, and hence suggesting that the DHS observations give a good representation of the Indonesian population. When comparing results from different estimation approaches, weighted mortality rates are used since the DHS's rates are weighted. The comparison results show that different estimation approaches yield similar rates in all three periods for both neonatal and post-neonatal mortality rates. Regardless of methods used, we can conclude that Indonesia experienced a decrease in both neonatal and post-neonatal mortality rates during the 10 years period prior to the economic and drought/smoke haze crises. The decline is not as sharp for neonatal mortality as it is for post-neonatal mortality.

When comparing the mortality rates of full DHS samples and the mortality rates of DHS observations that are only from the 13 provinces surveyed in the IFLS, both neonatal and post-neonatal mortality rates drop just by a small magnitude for both weighted and unweighted rates. The mortality rates are smaller in the 13 IFLS provinces across all periods. This corresponds to the provinces not surveyed being from the poorer, eastern provinces.

Table 1c presents neonatal and post-neonatal mortality rates from IFLS data using the "true-cohort-life-table" approach. Mortality rates are calculated for the corresponding DHS periods. When using both IFLS1 and IFLS2, IFLS data exhibits higher post-neonatal mortality rates than DHS rates for 1982-1987 and 1987-1992. The IFLS post-neonatal mortality rate is, however, lower for 1992-1997. The differences in the neonatal mortality rates between the two datasets are smaller than the differences in the post-neonatal rates for 1982-1987 and 1987-1992, but larger for 1992-1997.

When comparing mortality rates using only IFLS for consistency check, we find that including data from a more recent wave of IFLS gives higher rates for both neonatal and post-neonatal mortality. The data suggest that when longer retrospective is used, mortality rates are higher.

Birthweight Reporting

When studying the effect of the crises on a health outcome such as birthweight, a crucial concern regarding the data is whether there is any selection problem in reported outcomes. Children born at home are likely to be from rural, poorer households, and less educated mothers. These children are likely to have lower birthweights, which are likely to be unreported. If this selection problem is present in the birthweight distribution, then reported numbers are biased. Table 2 shows that in Indonesia while almost all births that took place in hospitals or community health center/delivery posts have reported birthweight, only 51.6 percent of births that took place at home have reported birthweight. This proportion of reported birthweight is close to the proportion of reported birthweight from office or house of traditional midwives. Note that when a woman gave birth at home or at family members' house, often a traditional midwife was called in to assist in the delivery process. Therefore, some proportion of babies that were delivered at home could be weighed if the midwives were well trained and well-equipped with measuring devices.

Table 3 shows proportion of birth location by different time periods. On average, only approximately 15 percent of deliveries took place in public or private hospitals. Health centers, village delivery posts, clinics of physician, and clinics of formally trained midwives serve as major delivery facilities in Indonesia (26.7 percent). The proportion of births that took place in these formal facilities increased during 1988-2000 from 21.0 percent to 26.7 percent. On average, birth deliveries that took place at home account for more than half of all deliveries in Indonesia. Although the proportion of deliveries that took place at home decreased substantially over time, the proportion remained relatively high in 1998-2000 (49.3 percent). From these results, we expect to encounter problems of reporting birthweight. This is due to a higher proportion of births that took place at home. Unfortunately, we do not have any plausible instrument to correct for this selection problem in reported birthweight. It is difficult to find a factor that affects delivery location (and whether birthweight was reported), but does not affect birthweight itself.

Mortality and Birthweight Statistics

The financial crisis periods are divided into two parts: crisis 1 and crisis 2 (refer to Figure 1). Crisis1 marks the period when the Rupiah rapidly devalued, the exchange rates were extremely volatile, and food prices accelerated (January 1998 to September 1998). Crisis 2 marks the period when the exchange rates began to settle and food prices came down. (October 1998 to June 1999). From earlier discussions, the 1997/1998 drought crisis and the smoke haze crisis covers the period from May 1997 until early 1998 in some areas. According to the timing of these different crises, the sample periods are divided into four sub-periods: *non-crisis* (January 1988 to April 1997 and July 1999 to December 2000), *crisis 1* (January 1998 to September 1998), *crisis 2* (October 1998-June 1999), and *drought 97* (May 1997 to December 1997). By this period grouping, *crisis1* also covers some of the drought/smoke haze crisis in some areas of Indonesia. [43]

In addition, since crisis 1 and crisis 2 each lasted nine months, we can interpret children who were born in crisis 2 as those who were conceived during crisis 1. Those that were born during crisis1 in rural areas that were affected by the drought could also be roughly interpreted as those who were conceived during the drought period.

An overview of mortality rates during our periods of interests is shown in Figure 5. Figure 5 shows mortality rates between 1988-1999. One can observe that the post-neonatal

---

[43] Although harvesting cycle in early 1998 was delayed by as much as two months, most of the effects of the effects of the drought crisis on productions had already been felt in the second half of 1997.

mortality rate gradually declined until 1996. Then the rate went up in 1997, further up in 1998, and started to decline in 1999. Neonatal mortality rates were more volatile with a downward trend until 1996. Neonatal mortality rate slightly increased in 1997, but the rate was still lower than those of the year prior to 1996. The rates heightened in 1998, and, similar to post-neonatal mortality rate, neonatal mortality rate started to decline in 1999.

The mortality status at the end of the first year of children born in crisis and non-crisis periods is shown in Table 5. As we expect from Figure 5, mortality was at a relatively high level up to 1992 and started to decline in 1993. The non-crisis periods are divided into two periods: "1988-1992" and "1993-1997, 1999."[44]

Table 5 shows that both neonatal mortality and post-neonatal mortality rates are higher in rural areas than in urban areas in all periods. Infant mortality significantly increased from the pre-crisis period (32.5 per 1,000 live births) during both the financial crisis (46.2 per 1,000 live birth) and the drought/smoke haze crisis (76.1 per 1,000 live births).

Neonatal mortality and post-neonatal mortality rates of children born during the financial crisis are higher than those of children born during non-crisis period of 1993-1997 and 1999, but lower than those of the children born during 1988-1992. Overall, for those born during the crisis, about 23 infants out of 1000 died within one month of birth as compared to the pre-crisis rate of about 13 per 1000 live births. The percentage increase of neonatal mortality is higher in urban areas (from10.2 to 20.5 per 1000 live births) than in rural areas (from 15.7 to 25.4 per 1000 live births). For overall sample, those born during the financial crisis also exhibit higher post-neonatal mortality than in the pre-crisis period. However, the percentage increase in the post-neonatal mortality is smaller than the percentage increase in neonatal mortality rate. When looking at rural and urban samples separately, the data show that urban children experienced a large increase in the post-neonatal mortality rate (from 10.9 to 16.4 per 1000 live births) while rural children experienced only a small increase in the rate (from 25.8 to 29.0 per 1000 live births).

The drought/smoke haze crisis seems to have had strong negative impacts on both neonatal mortality and post-neonatal mortality in rural areas where the largest negative impacts are expected. Compared to the pre/post-crisis period of 1993-1997 and 1999, Neonatal mortality increased from 15.7 to 29.0 (per 1,000 live births), while post-neonatal mortality increased from 25.8 to 47.1 (per 1,000 live births). One can observe that the post-neonatal mortality rate increased during the drought/smoke haze crisis so much that it surpassed the post-neonatal mortality rate of 1988-1992 period (38.7 per 1,000 live births). This evidence suggests that the drought crisis had stronger adverse effects on mortality than the financial crisis in rural areas. However, it is worthwhile to recognize that the mortality rates shown in Table.4 are calculated from small samples, especially the mortality rates for the crisis periods. As a result, these rates (per 1,000 live births) are sensitive to the number of death incidences among these small samples.

Birthweight statistics are shown in Table 6. Overall, the financial crisis had a small negative effect on birthweight. The proportion of low-birth-weight children (children with birthweight less than 2.5 kilograms) increased from 8.1 percent during non-crisis 1993-1997 to 8.7 percent during the financial crisis. The data also show some

---

[44] Births between July 1999 and December 2000 also belong to the non-crisis period, but they are not

reduction in the mean birthweight with the biggest decline among urban children (from 3.17 to 3.11 kg.). The drought/smoke haze crisis appears to have a negligible adverse effect on the mean birthweight, but the percentage of low-birth-weight children is lower than that of the non-crisis periods in both urban and rural areas. Since we expect to encounter selection problems in reported birthweight as discussed earlier, the estimated effects of the crises on birthweight is subject to errors. For instance, if during a drought/smoke crisis a high proportion of mothers shifted away from public healthcare to in-home care or offices of traditional midwives for delivery, it is more likely that babies were not weighed. Table 4 shows the proportions of delivery locations during pre-crisis, crisis, and post-crisis periods. The data suggest that during both crises, there is a jump in the proportion of deliveries that took place at home or at offices of traditional midwife from a decreasing trend. Babies born during the crises are, therefore, less likely to be weighed. If the unreported babies weighed less than the reported mean birthweight (e.g. mothers who switched to deliver at home because of high costs of public healthcare were the ones endowed with poor health), then the mean reported birthweight during the drought/smoke crisis understimates the adverse crisis impact. On the other hand, if unreported babies weighed more than the reported mean birthweight, (e.g. mothers who were healthy decided to save some money by giving birth at home), then the mean reported birthweight overestimates the adverse crisis impact. The direction of the bias is certainly an empirical question.

---

included because those children were not yet exposed to one year mortality by the interview date.

Testing the Effect of Economic Crises on Birthweight and Infant Mortality

This paper tests the hypothesis of whether crises in Indonesia have any effects on child mortality and birthweight. Children born during the crisis may be affected differently than those conceived during the crisis. Neonatal mortality is influenced by conditions during pregnancy or at birth, while post-neonatal mortality is influenced more by external factors during child rearing after birth[45]. Since conditions during pregnancy and after births are likely to affect neonatal and post-neonatal mortality differently, the analysis is carried out separately for neonatal and post-neonatal mortality.

Birthweight and child mortality may be affected by various socioeconomic characteristics other than conditions affected by economic crises. In this paper, mother's education, urban/rural residence, geographic location (provinces and communities), and child's gender are used as control variables.

According to Schultz,[46] women's education has external benefits to society. Higher mother's education reduces child mortality, improves child nutrition and schooling, and decreases fertility and population growth. Mother's education benefits child health in several ways. First, education helps make learning of the childcare process more efficient, especially when the process is complex (*technical efficiency*).

---

[45] A. Razzaque, N. Alam, L. Wai, and A. Foster. " Sustained Effects of the 1974-5 Famine on Infant and Child Mortality in a Rural Area of Bangladesh." Population Studies. March 1990

M. Rahman, B. Wojtyniak, M. M. Rahaman and K.M.S. Aziz, "Impact of environmental sanitation and crowding on infant mortality in rural Bangladesh." The Lancet (1985), pp. 28-32

[46] T.P. Schultz. "Investments in the schooling and health of women and men: Quantities and returns." Journal of Human Resources. Fall 1993; Vol. 28, Issue. 4; pg. 694, 41.

Second, education helps mothers allocate household resources efficiently to improve child's health (*allocative efficiency*). For example, mother's education increases the willingness to seek medical care and improves nutrition and sanitation practices.[47] Third, mothers with higher education are more likely to earn more income, and therefore can use this additional income to consume more or better-quality child's health inputs (*income effects*). Fourth, higher education may help improve women's bargaining power in household resource allocation. According to Schultz (1993), women may channel more of their income to expenditures on children than their husbands do. Improving women's education, therefore, could result in higher women's bargaining power, which in turn yields an allocation of more of the household income to expenditures on children.

When studying the effects of mother's education on child mortality, one should consider a possibility that education and health services are substitutes. According to a review by Basu and Aaby (1998)[48] that refers to studies by Palloni (1985)[49] and Rosenzweig and Schultz (1982),[50] the dominant theoretical stance on the education-mortality association is that the influence of personal characteristics, such as maternal

---

[47] Schultz, T. Paul. 1989. Benefits of Educating Women. Washington, D.C.: World Bank, Background Papers Series, Education and Employment Division, Population and Human Resources Department."The Benefits of Education for Women HRO Dissemination Notes." Human Resources Development and Operations Policy. Number 2, March 8, 1993.

Mellington and Cameron (1999)_find that mother's primary and secondary schooling significantly decrease the probability of child death in both rural and urban areas in Indonesia. "Female Education and Child Mortality in Indonesia." Melbourne- Department of Economics in its series papers number 693. 1999

[48] Basu, A.M. and Peter Aaby. *The Methods and Uses of Antropological Demography.* 1998. Clarendon Press. Oxford.

[49] Palloni, A. Health Conditions in latin America and policies for mortality change', in J. Vallin and A. Lopez (eds.), *Health Policy, Social Policy and Mortality Prospects*. Liege: Ordina. 1985

education, on child welfare attenuates when good health services are widely available. However, many studies give contrary examples in which access to services appears to make little difference to education differentials (for more detailed discussions, see Cleland and Van Ginneken, 1989[51]). For example, Bicego and Boerma (1991) find a stronger effect of maternal education in urban areas, where health services are assumed to be widely available, than in rural areas.

Table 7 shows the distribution of the mean education in years and percentages of different education levels of mothers over time. Formal education in Indonesia had been successfully improved. From our sample, we observe a significant increase in the mean education of mothers from 5.0 to 8.2 years during the eleven-year period. For example, the proportion of mothers with no formal education decreased from 19.2 percent to 4.6 percent (76 percent decrease). The proportion of mothers with 1-5 years of schooling decreased from 33.9 percent to 14.8 percent (56 percent decrease), while the proportion of mothers with 9-11 years of schooling increased from 7.9 percent to 20.4 percent (158 percent increase).

Table 8 shows that higher mother's education is associated with lower mortality. The effect seems to be stronger for post-neonates than neonates. If mother's education contributes to child mortality as previously argued, we must control for mother's education when studying the effects of crises. Since the children born in the crises

---

[50]Rosenzweig, M and T.P. Shultz. "Child mortality and fetility in Colombia: individual and community effects." *Health Policy and Education*, 2: 305. 1982.

[51] Cleland, J. and J. Van Ginneken. 1989. "Maternal education and child survival in developing countries: the search for pathways of influences." Social Science and Medicine. 27: 1357-60.

periods are from mothers of later cohorts for whom education is higher, not controlling for mother's education, will underestimate the impact of the crises.

The probability of neonatal and post-neonatal mortality is estimated using linear probability and logit regressions. Dependent variables are whether a child born alive had neonatal mortality, whether the child had postneonatal mortality given he/she survived neonatal mortality[52] and whether a child born alive had infant mortality (either neonatal mortality or post-neonatal mortality). Province or communities dummies are used to control for time-invariant community-specific unobserved factors that may influence child mortality (such as local disease patterns and public health infrastructure[53]). Since the focus of this paper is to study the effects of the financial and the drought/smoke crises on child birhtweight and mortality, controls thought to be correlated with the crises such as household income and time-varying public health provision are excluded.

In addition to socioeconomic controls, a time trend is included in our analysis to control for mortality trends that can be observed in Figure 5. According to the data, both neonatal and post-neonatal mortality increased during the crises period, but the level of these mortality rates are still not as high as that of the earlier control period (1988-1992). Since neonatal and post-neonatal mortality had generally declined during the pre-crisis periods from a relatively higher rate in 1988, omitting the time trend would result in an underestimated effect of the crises.

---

[52] Therefore, those born alive that experienced neonatal mortality are dropped from our sample when studying post-neonatal mortality.

[53] Pertersen, W. and R. Petersen with the collaboration of an International Panel of Demographers. 1986. Dictionary of Demography: Terms, Concepts, and Institution. Greenwood Press. New York. Westport, Connecticut. London.

As discussed earlier, the sample used includes children who were at least one year old at the interview date. Both results from the linear probability and the logistic regression are reported. Since urban and rural areas may be affected by different crises in different ways, regressions are performed separately for urban and rural areas.

Due to the nature of the dependent variables, the linear probability model violates one of the Gauss-Markov assumptions. When the dependent variable is a binary variable, its variance, conditional on the explanatory variables, depends on the explanatory variables (unless the probability of success does not depend on any of the explanatory variables):

$$\mathrm{Var}\,(y|x) = p(x)\,[1-p(x)]$$

Where, p(x) is the probability of success, which depends on x.

As a result, there must be heteroskedasticy in a linear probability model. Although heteroskedasticity does not cause any bias in the OLS estimators of the coefficients, homoskedasticity is crucial for justifying the usual t and F statistics.[54] Regressions robust to heteroskedasticity are, therefore, included to correct the conditional variances.

In addition, since our period of analysis includes children born in 1988-2000, children in our sample can be from the same mother. The outcomes of children within a group of mother are likely to be correlated. In this paper, observations are clustered at the mother level to allow for this type of heteroskedasticity problem in the variances of the estimated coefficients.

---

[54] For further discussion, refer to Jeffrey M. Wooldrige. "Introductory Econometrics: A Modern Approach" Copyright 2000. by Sourth-Western College Publishing. USA.

The effects of economic crises on birthweight can be tested nonparametrically by comparing cumulative distribution of birthweight of children born during non-crisis period and that of children born during the crisis period. Special attention, however, should be paid to proportion of children who have "low-birth-weight," which is birthweight that is less than 2.5 kilograms. Note that when testing the differences of the distributions, data are restricted to exclude outliers. The tests include only birthweights between 1.5[55] and 3.0 kilograms. Computing for the difference in the values of the cumulative distribution at each weight point of interest was performed. The comparison follows directly the formulation offered by Davidson and Duclos (2000).[56] We will be able to conclude that the crisis has statistically significant adverse effects on birthweight when the cumulative distribution value of birthweight of those born during the crisis period at all birthweights of interests (that are considered low birthweight) is unambiguously higher than that of those born during non-crisis period. In other words, we are testing whether the cumulative distribution of low birthweights of those born during the crisis period first stochastically dominates that of the cumulative distribution of low birthweights of those born during the non-crisis period.

When the test of first order-stochastic dominance fails, we test for relative riskiness or dispersion of birthweight (second-order stochastic dominance). This is to test whether those born during the crisis period as a group exhibit relatively higher risk of low birthweight than those born during non-crisis period. In other words, we test whether the probability of those born during the crisis period that have birthweight at or below a

[55] There are few birthweights that are less than 1.5 kilograms. These observations are considered outliers.

specific weight is significantly higher than that of those born during the non-crisis period. If this is true for all low birthwieght values tested, we can conclude that the distribution of birthweight of those born during the crisis period second-order dominates that of those born during the crisis, and that those born during the crisis exhibit relatively higher risk of having low birthweight. Computing for the difference in the values of the cumulative distribution at each weight point of interest was performed using a software for Distributative Analysis/Analyse Distributative (DAD).[57] The techniques used in this software follow directly the formulation offered by Davidson and Duclos (2000).[58]

In addition to nonparametric estimation of the effects of the crises on birthweight, we include a simple parametric estimation using Ordinary Least Square (OLS) regressions, using birthweight as a continuous variable. Explanatory variables are the same set used in mortality regressions. Separate regressions are carried out for rural and urban samples.

---

[56] Russel Davidson and Jean-Yves Duclos. "Statistical Inference for Stochastic Dominance and for the Measurement of Poverty and Inequality." Econometrica v86 n6. 2000.

[57] Copyright by Jean-Yves Duclos, Abdelkrim Araar, and Carl Fortin.

[58] Davidson, R. and Jean-Yves Duclos. "Statistical Inference for Stochastic Dominance for the Measurement of Poverty and Inequality." Econometrica, V86. n6. 2000.

Results

Figure 6a-6c present the comparisons of the cumulative distributions of birthweights in the crisis and the non-crisis periods. The results confirm that the reduction in birthweight was only among urban children born in the economic crisis. Table 9 shows the results of testing the difference of cumulative distributions using both first and second order stochastic dominance tests. The points of testing are between 1.5 and 3.0 Kilograms. The results from these tests show that, within this range, none of the comparisons show any significant first and second-order stochastic dominance. However, it is hard to reject that the economic crisis had no impact on urban children as one can observe that there is some evidence suggesting a second order stochastic dominance in the range of 2.1-3.0 Kilograms among urban sample. In rural areas, the results are opposite to what we expect, but none of the comparisons shows first nor second-order dominance.

Table 10 presents the results from the multivariate OLS birthweight regressions. The dependent variable in these regressions is birthweight in kilograms. The estimations are carried out for urban and rural samples separately. Four specifications are presented for each sample. The first specification is our base regression. In this specification, explanatory variables include only the crisis dummies and the time trend. Mother's education dummies are added in the second specification. The last two specifications use province and community dummies to controls for location-specific unobserved factors respectively. Regardless of which geographic location is used, the regressions essentially yield fixed-effect estimators.

The results from these regressions suggest that none of the crises had a statistically significant adverse effects on birthweights. We find no effects even after controlling for other factors that may affect birthweight. We observe a downward trend in birthweight in urban areas and an upward trend in rural areas, but the time trend variable does not appear to have statistically significant effects on birthweight. Male children had significantly higher birthweight than female children in both urban and rural areas. In any case, one should keep in mind that these regression results are based on birthweights that were reported. The observations are subjects to potential selection problems as discussed earlier.

The results from the multivariate analysis of infant mortality are shown in Tables 11 to 14. Tables 11 and 12 present results from the LPM and the Logistic regressions using the urban sample. Tables 13 and 14 present results from the LPM and the Logit regressions using the rural sample.[59] In each set of regressions, four specifications are presented. The first specification is our base regression. In this specification, explanatory variables include only the crisis dummies and the time trend. Mother's education dummies are added in the second specification. The last two specifications use province and community dummies to controls for location-specific unobserved factors. Regardless of which measure of geographic location is used, the regressions essentially yield fixed-effect estimators. In the Logit regressions, the last columns in each group of regressions are estimates of conditional Logit models with fixed province/community effects. To be able to identify the conditional effects, observations used in these

---

[59] None of those born during financial crisis 2 period (122 obs) experienced neonatal mortality. Since we are interested in the estimated coefficients of the crisis dummies (fiancial and drought/smoke crises), the

regressions are only from those provinces/communities that experienced at least one incidence of mortality.[60] To be consistent with the Logit regressions in terms of observations used, the last two columns of the LPM regressions limit the samples to be the same as those in the Logit regressions. Also, when community dummies are included, crisis dummies pick up effects that vary within communities. Then, if there is no variation in the communities, observations belonged to these communities do not help identifying the crisis effects.

The estimated coefficients from the linear probability models are the estimated partial effects. For Logit regressions, the odds ratios[61] are reported. As discussed earlier, explanatory variables of interest in the neonatal mortality regressions are whether the child was conceived during the crises, while explanatory variables of interest for post-neonatal and infant mortality regressions are whether the child was born during the crises. The LPM and the Logit regressions give similar results.

In urban areas, the estimated coefficients of the time trend are statistically significant in neonatal, post-neonatal, and infant mortality regressions regardless of whether the regressions control for other fact.ors The estimates suggest that both neonatal and post-neonatal mortality rates in urban areas decreased by approximately 0.2 percentage points per years (by 2 deaths per 1,000 live births per year). Infant mortality

---

"pregnancy in fiancial crisis2" dummy variable was dropped in the Logit regressions instead of dropping these observations. The dummy variable was also dropped in the corresponding LPM regressions.

[60] The data show that none of the infants born in urban areas of Lumpung and Bali provinces experienced post-neonatal mortality. Similarly, none of the infants born in rural Yogyakarta experienced post-neonatal mortality.

[61] The odd ratios, e^{β}, show impacts in terms of Prob (mortality | x) / Prob (survival | x).

declined by approximately 4 deaths per 1,000 live births per year. The estimated effects of the time trend are significantly larger in the community fixed-effects regressions.[62]

In urban areas, children born during the peak of the financial crisis (crisis1) exhibited higher odds of neonatal, post-neonatal, and infant mortality. The effects of the crisis were more precisely estimated in the Logit regressions. Controlling for mother's education and province of residence does not change our point estimates by much. when controlling for the community fixed effects, these point estimates, however, decrease in the Logit regression and increased in the LPM regressions .

Results from the linear probability regressions show that children conceived during the peak of the financial crisis (crisis1) had approximately 1.7 percentage points higher probability of neonatal mortality than those conceived during the non-crisis periods. Similarly, those born during crisis1 exhibited approximately 1.6-1.7 percentage points higher probability of post-neonatal mortality than those born during the non-crisis periods. However, the impacts of the crisis are not statistically significant when community fixed-effects are controlled for.

Urban children conceived during the drought/smoke haze crises exhibited higher neonatal mortality than those conceived during the non-crisis periods. The adverse effects are estimated to be about 2.1 percentage points in the LPM regressions. The adverse effects were statistically significant at 10 percent level even after controlling for the community fixed effects using Logit regression. The 1997 drought/smoke crisis did not have any statistically significant effects on post-neonatal mortality in urban areas.

---

[62] Recall that observations used in the LPM community fixed-effect regressions and the conditional Logit regressions includes only communities with at least one incidence of mortality.

The results from the infant mortality regressions suggest that only the economic crisis had a statistically significant impact on overall infant mortality in urban areas. Similar to results from the neonatal mortality regressions, the effects of the crisis were not statistically significant in the fixed-effect estimation for infant mortality.

In urban areas, male children had higher neonatal, post-neonatal, and infant mortality rates. However, the estimates are not statistically significant. In addition, we found that mother's education did not play a significant role in reducing neonatal mortality.[63] On the other hand, the effects were felt in post-neonatal mortality. Although in some specifications the points estimates are not statistically precise, we do observe that children born to mothers with at least 9 years of education experienced lower probability of post-neonatal mortality by approximately 2.8-3.5 percentage points. When controlling for the community fixed effects, the magnitude of the effects of twelve or more years of education more than doubled the effects in province fixed-effect regressions.[64]

In rural areas, both neonatal and post-neonatal mortality rates decreased over time. The estimated coefficients of the time trend, however, suggest that rural areas experienced a decline in these mortality rates at a slightly slower rate than the urban areas. The community fixed-effect estimations suggest that the reduction in mortality risks was at a faster rate in communities that experienced at least one mortality incidence, controlling for community fixed effects.

---

[63] Except at 12 or more years of education in the Logit regression.

[64] Recall that observations used in the LPM community fixed-effect regressions and the conditional Logit regressions include only communities with at least one incidence of mortality.

In rural areas, the financial crisis 1 period had adverse effects on neonatal and post-neonatal mortality. However, the estimated effects are statistically significant in only neonatal mortality regressions. In these regressions, the estimated effects are statistically significant at the 10-percent level even after controlling for community fixed-effects. Rural infant mortality was affected by financial crisis 2 instead of financial crisis 1. The increase in infant mortality rate is estimated to be approximately 3.2 percentage points. In those communities where we applied community fixed-effect estimation, the effects are higher (4.9 percentage points) and statistically significant at the 5-percent level.

In rural areas, the drought/smoke haze crisis exhibited statistically significant adverse effects on post-neonatal mortality, but not on neonatal mortality. The effects of the crisis on post-neonatal mortality are stronger than the effects of the financial crisis. The LPM estimations show that infants born during the drought/smoke haze crisis had approximately 2.7-3.1 percentage points higher probability of post-neonatal mortality than those born during the non-crisis periods. The estimated effects are higher (6.3 percentage points in the LPM) and still statistically significant when controlling for community fixed-effects in both the LPM and the Logit regressions.

Mother's education played a greater role in reducing neonatal mortality in rural areas than in urban areas. Our estimates from the LPM regressions suggest that mother's primary education is associated with approximately 1.6-1.8 percentage points lower neonatal mortality rate. The effect of education was much stronger (4 percentage points) using community fixed-effect estimation for those communities that experienced neonatal mortality. If a higher level of mother's education increases effectiveness of prenatal care, these results imply that increasing mother's education in rural areas will help reduce neonatal mortality. An explanation of our finding that higher mother's education did not reduce neonatal mortality in urban areas is that urban mothers' ability and effectiveness in providing prenatal care could be substituted by better services and less expensive healthcare that were more readily available in urban areas.

Similar to urban areas, education of rural mothers reduced post-neonatal mortality. Moreover, our estimates suggest that in rural areas mother's education started to have statistically significant effects on post-neonatal mortality at the secondary education level, much earlier than in urban areas.

In rural areas, we found that male infants had a higher chance of both neonatal and post-neonatal mortality than female infants. Recall that in urban areas, a child's gender had no statistically significant effects on neonatal mortality. If male infants are biologically more prone to neonatal mortality than female infants, the result from our regressions suggest that better healthcare and more exposure to prenatal services (such as those available in urban areas) can help overcome higher risks of neonatal mortality among male infants.

How did mothers with different education levels cope with the crises?

An important question we might ask when studying the effects of the crises on infant mortality is how households with different socio-economic backgrounds coped with adverse short-term shocks. For instance, we ask whether poor households responded differently to the financial crisis than rich households did. Nevertheless, since the financial and the drought/smoke haze crises had direct impacts on household's income, distinguishing households by income level may result in biased estimates of the effects of the crises as the crisis itself determines which income group a household belonged to. To avoid this selection problem, we want to use determinant of household' income that are not affected by the crises (in the short-run). In this paper, we experiment with different mother's education levels as such determinants.[65]

Tables 15-17 show LPM mortality regression results of children born to mothers with 0-8 years and 9+ years of education. Table 15 shows the results from infant mortality regressions of infants born to mothers with different levels of education. We observe from the point estimates that in urban areas, infant mortality trends are different between the two groups, even after controlling for community fixed effects. Those born to mothers with lower education levels experienced a decline in infant mortality at a rate that is twice as much as the rate for those born to mothers with higher levels (0.6 vs 0.3 percentage points per year). However, a test of the differential trend effects between these two groups in urban areas indicates that the differential effects are not statistically

[65] Alternatively, one can ask directly how mothers of different education levels cope with the crises.

significant at the 10 percent level.[66] In contrast, the difference in the time trend effects between low and high education groups is small in rural areas. The estimated effect of the time trends for both groups is approximately 0.4 percentage points per year.

In urban areas, the point estimates of the financial crisis effects on infant mortality suggest that the adverse effects are stronger for infants born to mothers with lower education. However, a test of the differential effects cannot reject that the financial crisis effects on the two groups are statistically the same.[67] In rural areas, the effects of the financial crisis on infant mortality were slightly higher for the lower education group. A test of the differential effects indicates no statistically significant difference between the effects on the two groups.[68]

In rural areas, the drought/smoke crisis had statistically significant adverse effects on infant mortality only for infants born to the low education group. The magnitude of the drought/smoke effects are also much larger for those belonging to mothers with lower education than for those belonging to mothers with higher education (6.0 versus 1.3 percentage points). The differential effects are larger when controlling for community fixed effects. However, the effects of the droughts/smoke crisis are not statistically different between these two education groups.[69]

Table 16 shows results from neonatal mortality regressions of infants born to mothers with low and high education. We found that in urban areas the mortality trends

---

[66] The t-statistic for the differential effects is 1.50 (p = 0.13).

[67] The t-statisitics for the differential effects are 1.43 (p = 0.15) for financial crisis 1 and 0.03 (p =0.98) for financial crisis 2.

[68] The t-statisitics for the differential effects are 0.36 (p =0.72) for financial crisis 1 and 0.08 (p =0.94) for financial crisis 2.

of the low and the high education groups are similar. Mothers with higher education levels experience a decline in neonatal mortality of their infants at a rate of approximately 0.28 percentage points per year, slightly higher than that of mothers with lower than six years of education. We observe a larger differential between the two groups after controlling for province and community fixed effects. However, the differential effects of the time trends are still statistically insignificant.[70] In rural areas, neonatal mortality had not been statistically reduced over time regardless of the education of the mother. When community fixed effects are controlled for, the differential effects of the time trend between the two groups is larger, but the point estimates are still statistically insignificant. A test of the differential effects cannot reject that the neonatal mortality trends among the two education groups are similar.[71] In addition, we observe higher and more significant time-trend effects in urban areas than in rural areas for both education groups.

In urban areas, financial crisis 1 had statistically significant adverse effects on only infants from the high education group. Our point estimates suggest that infants born to mothers with lower education were more adversely affected by the drought/smoke crisis in their risks of neonatal mortality than infants born to mothers with low education. However, the differential effects are not statistically significant.[72] In rural areas, infants born to mothers with higher education during the second phase of the financial crisis and the drought/smoke crisis, however, had a lower neonatal mortality rate than those born

---

[69] The t-statisitic for the differential effects is 1.50 (p = 0.13).

[70] The t-statistic for the differential effects is 0.61 (p =0.54).

[71] The t-statistic for the differential effects is 0.42 (p = 0.67).

during the non-crisis periods. A plausible explanation is that even though the neonatal mortality rate for this group of infants increased during these crisis periods, the increase in mortality risk is not so high to surpass the rates of the pre-crisis period. This explanation is supported by the fact that the negative time trends were not statistically significant for this group of infants. When the community-fixed effects are controlled for (for those with 9+ years of education), the crisis dummies still have negative signs while the estimated effect of the time trend is negative and significant. This result is puzzling. However, our test of the differential effects of the crises suggests that there is no statistically significant differential effects between the two education groups.[73]

The neonatal mortality regressions give another interesting result when we look at infants of different genders. In urban areas, we observe a small and statistically insignificant gender difference in the mortality rate regardless of levels of mother's education. In rural areas, among those born to mothers with lower education, male infants had 1.4 percentage points higher neonatal mortality risks than female infants. The differential effect is higher after controlling for community fixed effect. In contrast, male infants had less neonatal mortality risk that female infants among those born to mothers with higher education. Our test indicates that the gender differential in neonatal mortality risk among infants born to mothers with low education is significantly different from that among infants born to mothers with high education.[74]

---

[72] The t-statistics for the differential effects are 0.62 ($p = 0.54$).

[73] The t-statistic for the differential effects is 0.04 ($p = 0.97$), 0.83 ($p = 0.41$), and 1.00($p = 0.32$) for financial crisis 1, financial crisis 2, and drought/smoke crisis.

[74] The t-statistic for the differential effects is 1.79 ($p = 0.07$).

Table 17 shows results from post-neonatal mortality regressions of infants born to mothers with low and high education. We found that in urban areas, the mortality trends are significantly different among the low and the high education groups at the 10% level.[75] In urban areas, infants born to mothers with less than nine years of education exhibited a much larger declining trend than those born to mothers with higher education. The estimated effects of the time trend are estimated to be approximately 0.4-1.2 percentage points per year for lower education group. In contrast to the trend difference in urban areas, the difference between the effects of the time trends between these two education groups are less pronounced in rural areas. Rural infants born to mothers with lower education experienced a faster decline in post-neonatal mortality risk. The estimated effects of the time trend are lower in rural areas than in urban areas (-0.24 versus -0.33 percentage points per year) for lower education group. On the contrary, the time trend effects are higher in rural areas than in urban areas for the higher education group (-0.13 versus -0.04 percentage points per year).

In urban areas, the adverse effects of the financial and the drought/smoke crises on post-neonatal mortality seemed larger for those born to mothers with lower level of education than for those born to mothers with high education. However, none of the estimated coefficients of the crisis dummies are statistically significant in either high or low education groups. The differential effects of the crises between the two samples, in turn, are not statistically significant.

---

[75] The t-statistic for the differential trend effects is 1.67 ($p = 0.09$).

In rural areas, unlike in urban areas, the adverse effects of the financial crisis appear to be similar for both education groups. As expected, the drought/smoke haze crisis adversely affected children born to both groups of mothers. However, the drought/smoke crisis had much worse effects on post-neonatal mortality for infants in low education groups than that of infants in high education group. Our point estimates suggest that infants belonging to mothers with less than 9 years of education tended to suffer from the drought/smoke haze crisis much more than those belonging to mothers with at least 9 years of education (4.3 versus 0.0 percentage points). A test of the differential effects indicates statistically significant differential effects between the two education groups.[76]

In urban areas, our estimates show discrimination in post-neonatal mortality between male and female infants born to mothers with at least nine years of education. The discriminating effect is large when controlling for community fixed effects (4.7 percentage points). A test of the differences in gender discrimination suggests no differential effects between the two education groups.[77] In rural areas, the gender difference in post-neonatal mortality risks is statistically significant for only the low education group. Male infants born to mothers with lower education experienced about 1.4 percentage points higher post-neonatal mortality rate than female infants. The difference between the two education groups are, however, not statistically significant.[78]

---

[76] The t-statistic for the differential effects is 1.84 (p = 0.07).

[77] The t-statistic for the differential effects is 0.93 (p = 0.35).

[78] The t-statistic for the differential effects is 0.71 (p = 0.48).

In sum, our findings indicate that infants born to mothers with different levels of education exhibited no significantly different trends in mortality risks over time in both urban and rural areas. We also find that even though the economic crises had adverse effects on infant mortality, infants born to mothers with different education levels did not experience statistically different adverse financial crisis effects in either urban or rural areas. There were statistically significant differential effects of the drought/smoke crisis on post-neonatal mortality between the low and the high education groups in rural areas. Those born to mothers with lower education were more adversely affected by the drought/smoke crisis than those born to mothers with higher education.

Hazard Models

The findings obtained in the previous section are based on sample observations that include only mortality of children who were exposed to at least one year of life. Many children born in 1999 and all children born in 2000 are excluded by this criterion.

Since we are interested in estimating the probability of dying before one month and within 1-11 months, we can use duration models to directly extract the hazard rates (the probability of dying in the next period given survival up to the current period) without having to exclude those who were not yet one year old at the time of the interview. Using the duration model is also a better way to capture the precise time of death assuming reported dates are accurate. By using duration models, each month's survival status information of each child is used to estimate the mortality hazard. The hazard rate of each month can then be obtained.

Figure 7a-7c show nonparametric estimates of discrete monthly child mortality hazard rates from Nelson-Aalen cumulative hazard function. In urban areas, those born during the financial crisis exhibited higher hazard rates that those born during the non-crisis periods. Similar to results from the previous regressions, in rural areas, the drought crisis had more overall effects on child mortality than the financial crisis. The results from testing the equality of the survivor functions of children born in these periods, however, show that none of crises had statistically significant adverse effects on infant mortality. These insignificant crisis effects are inconsistent with our findings from the OLS and logit regressions. Nevertheless, one should keep in mind that the hazard rates used in this section to calculate the crisis effects are estimated without any controls.

Parametric Estimation of Child Mortality Using Hazard Models

Let $T \geq 0$ denotes the length of time the child lived in months. T has some distribution over the population. Consider time invariant covariates.

Let F(t; x) = conditional cdf of T where x = covariates:

$$F(t; x) = P(T \leq t;\ x),\ t \geq 0$$

The survivor function is defined as

$$S(t;x) = 1 - F(t;x) = P(T > t;x)$$

Then, the probability of leaving the initial state in the time interval (t, t+h) is

$$P(t \leq T < t + h \mid T \geq t;x) \quad \text{for } h > 0$$

Define the hazard function as

$$\lambda(t;x) = P(t \leq T < t + h \mid T \geq t;x) = P(t \leq T < t + h;x) / P(T \geq t;x) = \frac{F(t+h,x) - F(t;x)}{1 - F(t;x)}$$

If the cdf is differentiable, then the hazard function is

$$\lambda(t;x) = \frac{F(t+h,x) - F(t;x)}{1 - F(t;x)} = \frac{f(t;x)}{1 - F(t;x)} = \frac{f(t;x)}{S(t;x)}$$

Then all probabilities can be computed using this hazard function. For example, from time a to time b, a < b is

$$P(a \leq T < b \mid T \geq a;x) = 1 - \exp\left[- \int_a^b \lambda(s;x) ds \right]$$

Since we did not observe the end of the survival period of every child in the sample, the survival data obtained from the survey are considered "flow" data, which are subject to time censoring. In this case, the data are right-censored at the interview date. For example, if the child born on January 1, 2000 was still alive on the interview date of June 30, 2000. We only know that the child's survival time was at least six months. We never observed real survival time of this child. The model can be adjusted to include this time censoring by defining censored flow data in the following way.

Observed duration t*

Define t_i* as the length of time in the initial state that has a continuous conditional density

$$f(t_i \mid x_i;\theta), t \geq 0$$

where θ is the vector of unknown parameters.

The observed length of time, t, in the initial state is

$$t_i = \min(t^*{}_i, c_i)$$

where c_i is censoring time for individual i. In this case, the censoring time is age of the child the interview date.

Table 18 and Table 19 show results of parametric estimations of hazard rates of urban and rural children respectively. Hazard regressions use Weibull distribution to allow for both positive and negative monthly hazard rates. The standard error and the z statistics of the hazard ratios are robust to heteroskedasticity of the variance-covariance matrix at the mother level. In addition, we also present results of these hazard regressions assuming heterogeneity in our observation. In our regressions, the observations are assumed to have inverse-Gaussian heterogeneity (frailty).

The estimated results are similar to those obtained from previous LPM and logit regressions. Those born during the financial and the drought/smoke haze crises appear to have been negatively affected by the crises. The estimated effect of the financial crisis is statistically significant at 10 percent level while the estimated effect of the drought crisis is significant at 5 percent level. Those with mother of at least 12 years of education have lowers estimated hazard rates than those with no education. The estimated coefficient of the male dummy suggests boys had a higher mortality risk than girls.

In rural areas, those born during the drought/smoke haze crisis appear to have been adversely affected. Similar to results found in the LPM and the Logit regressions, the financial crisis did not have statistically significant effects on rural children. Education was also more important in rural areas than in urban areas. In rural areas, having some education, regardless of the education level, helped reduced the risk of mortality. Similar to the results from urban areas, male children exhibited higher risk of mortality.

The results from the hazard regression allowing for heterogeneity adjustment confirms that the observations used are heterogeneous.[79] However, the degree of the statistically significance of the estimated coefficients are similar to those in the regressions that assume homogeneity of the observations.

Figure 8a-8c show estimated child mortality hazard rates using parametric hazard models. Results from hazard models confirm that the financial crisis and the drought

[79] We reject that observation are homogeneous with p-value = 0.000 for both urban and rural observations.

crisis had adverse effects on neonatal and post-neonatal mortality. When comparing rural and urban samples of the same period, rural children exhibited higher probabilities of both neonatal mortality and post-neonatal mortality than urban children. The financial crisis increased the odds of both neonatal and post-neonatal mortality for urban children more than for rural children. As expected, in rural areas, the crisis-noncrisis differential of the mortality risk at specific age (months) is relatively smaller for financial crisis than that of the drought crisis.

Discussions

Several child health outcomes are examined in this paper to assess whether the Indonesian financial and drought/smoke crises negatively impacted young children in Indonesia. Birthweights, neonatal, post-neonatal, and infant mortality are examined in this paper. The results from both the birthweight cumulative distribution comparison and the OLS estimation similarly suggest that none of the crises had negative impacts on birthweight in both urban and rural areas. Nevertheless, we realize that this evidence is drawn from reported birthweights only. An investigation of the probability of reporting birthweight in various delivery locations in Indonesia suggest that the birthweight distribution drawn from reported birthweights may be biased due to unreported birthweights of infants that were born at home or in offices of traditional midwives. Further, biased results could come from selection problems when some mothers switched from hospital to home delivery in the time of crisis. Unfortunately, we do not have any plausible instrument to correct for this selection problem in reported birthweight. It is difficult to find a factor that affects delivery location (and whether birthweight was reported), but does not affect birthweight itself.

Unlike birthweight, our findings indicate that the financial and the drought/smoke crises had significant adverse effects on infant mortality. The overall effects were different in urban and rural areas. Although the financial crisis had adverse effects on neonatal mortality in both urban and rural areas, the effects on post-neonatal mortality were felt by only urban infants. Contrary to the effects of the financial crisis, the effects of the drought/smoke crisis on post-neonatal mortality were felt by only rural infants.[80]

Results from multivariate regressions, nonparametric, and parametric hazard estimations show similar effects on infant mortality. In urban areas, infant mortality was affected by only the financial crisis. In rural areas, infant mortality was affected by both the financial and the drought/smoke crises, but the drought/smoke crisis appeared to have worse effects than the financial crisis.

These differential results are consistent with our expectations. Since a large increase in the food prices and an adverse income shock during the financial crisis resulted in a sharp reduction of real incomes for most of the Indonesian population, who are net food purchasers, we expect to find some adverse effects on child health outcomes among both urban and rural population. Since the smoke haze that resulted from erupted fires during late 1997 and early 1998 affected only parts of urban areas in our sample, we do not expect to find strong effects of the smoke haze on overall urban population. Notice that when community fixed effects are controlled for in our analysis, the effects of the drought/smoke crisis on neonatal mortality in urban areas lost statistical significance. However, instead of combining the drought and the smoke/haze crisis, one could distinguish these two crises by mapping exact locations and levels of the fire smoke haze to each community in our sample in different periods. This task is plausible, but it would involve elaborate work in data collection.

---

[80] We observed that neonatal mortality in urban areas ws affected by the drought/smoke crisis, but the level of statistical significance is at 10%. After controlling for community fixed effects, the crisis appears to have no effects on neonatal mortality.

Since the focus of this paper is to examine whether economic crises have any effects on child mortality, controls correlated with crises such as household income and public health provisions are excluded. Including these correlated variables in the regression, however, may answer different questions.

In addition, regressions in this paper do not control for fertility decisions. Since economic crises may affect fertility decisions, including fertility decisions in the regressions might lead to endogeneity problems. Demographic theory and empirical evidence from different countries suggest that mortality change should call forth some fertility response (Preston, 1975). Major examples of studies that suggest mortality-fertility link are given by Schultz (1969) and Federicksen (1966). According to Schultz, parents try to compensate for the average incidence of death by seeking the number of births that will give them the desired number of surviving children. Federicksen (1966) infers from the regional birth rates and the rates of population growth in Ceylon, Mauritius, and British Guiana that the improvement in health conditions that were responsible for the death rates led to a subsequent reduction in birth rates. On the other hand, some studies such as those by Adelman (1963) and Coale and Hoover (1958) suggest no significant link between mortality and fertility. For instance, Coale and Hoover (1958) in their classic text, *Population Growth and Economic Development in Low Income Countries,* found the absence of major fertility decline in several developing countries that had experienced a prolonged mortality decline. The direction of the change in fertility decision during the Indonesian crises is, of course, an empirical one that needs to be further studied.

Since economic crises may affect people with different socioeconomic backgrounds differently, the study of the effects of economic crises can be extended to focus on these differential effects. Our preliminary findings suggest that even though some of the crises had adverse effects on infant mortality, infants born to mothers with different education levels did not experience different adverse crisis effects.

As for infant mortality, we found that in urban areas, the financial crisis seems to have worse effects on infant mortality for those born to mothers with lower education. In rural areas, the effects of the financial crisis on infant mortality were slightly higher for the lower education group. However, when community-fixed effects are accounted for, the financial crisis effect is higher (and statistically significant) for the higher education group. In rural areas, even though the drought/smoke crisis had adverse effects on infant mortality for both education groups, the magnitude of the effects are larger for those belonged to mothers with lower education. We found the differential effects between the two groups to be larger when controlling for community fixed effects.

Even though the true underlying causes of the differential effects of the crises are not completely explored in this paper, we view our results from the estimation of the differential effects between different mother's education levels as evidence of differential effects of the crises on children with different socio-economic backgrounds. Since male and female children exhibited different mortality rates, studying the differential effects of the crises between male and female children may be of interest. This could be carried out by adding interaction terms of the crisis periods and a sex dummy or by estimating regressions separately for each group of children.

Conclusion

This paper examines the impacts of the recent Asian financial crisis and the 1997/98 drought and smoke haze crises on infant mortality and birthweight in Indonesia. The paper uses data from three waves of the Indonesian Family Life Survey: IFLS1 (1993), IFLS2 (1997), and IFLS3 (2000), utilizing rich data on socio-economic backgrounds as well as detailed information on children's birthdates, birthweights, mortality status at the time of interview, and ages at death if they died.

The methodology used in this paper is to compare health conditions of newborns of different birth cohorts. Specifically, this paper examines whether those conceived/born during the crisis periods exhibited higher risk of neonatal mortality and post-neonatal mortality and whether their birthweights were lower than birthweights of those born during the non-crisis periods.

The estimations of both neonatal and post-neonatal mortality risks are carried out using multivariate regressions with socio-economic control variables such as mother's education, place of residence (province/community), and gender of the child. In addition, mortality risks are estimated using hazard models to capture the mortality risks at different age (in months). The paper uses both nonparametric and parametric hazard models to estimates the hazard rates. The effects of the crises on birthweights are analyzed using multivariate regressions and comparisons of birthweight cumulative distributions. In both mortality and birthweight analyses, urban and rural samples are analyzed separately since the economic crises could have affected mortality and birthweight differently in rural and urban areas.

Estimated results on mortality outcomes show that the financial crisis had adverse impacts on neonatal mortality in both urban and rual areas. Urban infant conceived during the peak of the financial crisis exhibited approximately 1.7 percentage points higher neonatal mortality risk (17 per thousand live births more) than those conceived during non-crisis periods. The increase in neonatal mortality risk was approximately 2.2 percent for rural infants. The adverse effects of the financial crisis on post-neonatal mortality risks were larger and more statistically significant for urban infants than for rural infants. Overall, the financial crisis increased infant mortality risks by about 3.2 percent in both urban and rural areas.

The drought/smoke crisis adversely affected post-neonatal mortality risks in rural areas. The increase in the post-neonatal mortality risk is about 3.1 percent. When community fixed effects are controlled for, the drought/smoke crisis appears to have had much larger effects. Overall, the drought/smoke crisis had no significant advese effects on infant mortality in urban areas, while the effects in rural areas were large. Our estimates show that rural infants born during the drought/smoke crisis experienced approximately 4.4 percent increase in their infant mortality risks (44 per 1,000 live births). The magnitude of the effects almost doubled after controlling for community fixed effects.

Our findings on differential effects on infants born to mothers with different levels of education indicates that infants born to mothers with different levels of education exhibited no significantly different trends in mortality risks over time in both urban and rural areas. We also find that even though the economic crises had adverse effects on infant mortality, infants born to mothers with different education levels did not experience statistically different adverse financial crisis effects in either urban or rural areas. There were statistically significant differencial effects of the drought/smoke crisis on post-neonatal mortality between the low and the high education groups in rural areas. Those born to mothers with lower education were more adversely affected by the drought/smoke crisis than those born to mothers with higher education.

Results from the cumulative distribution comparisons of birthweights suggest that the financial crisis also had adverse impacts on birthweight in urban areas. However, under multivariate analyses, the adverse effect seems to disappear. None of the crises affected birthweights in rural areas. The lack of an evidence on the adverse effects maybe due to a selection problem in reported birthweights. The data show that from 1988 to 2000, 56 percent of women gave birth at home or at a family member's house. For those who were born at home, only 52 percent have reported birthweights, whereas 99 percent of those born in hospitals have reported birthweights.

CHAPTER 2

THE EFFECTS OF THE 1998 ECONOMIC CRISIS ON AGES OF FEMALE FIRST MARRIAGE AND FIRST BIRTHS: EVIDENCE FROM INDONESIA

Introduction

This paper utilizes data from the Indonesian Family Life Surveys (IFLS) to study short-run effects of the 1998 Indonesian financial crisis on the ages at which females first marry and when they have their first child. It is an extension of Rukumnuaykit (2003), which studies the effects of the Indonesian economic crises on infant mortality and birthweight. Previous studies on the effects of economic crisis on demographic outcomes have not found evidence of adverse effects on infant/child mortality, but stronger evidence has been found of the effects on fertility and marriage delays. In this paper, we investigate ages of female first marriages and first births and argue that increasing rates of marriage and fertility delay may have been used as consumption smoothing mechanisms to cope with the Indonesian crisis.

The Asian financial crisis struck Indonesia in January 1998.[1] Figure 1 shows that the sustained crisis period lasted more than one year with the peak in Rupiah/USD exchange rate in July 1998. As shown in Figure 2, food prices in both urban and rural areas increased more than 250 percent at the peak of the crisis. Alatas (2002)[2] argued

---

[1] Refer to figure 1.

[2] Alatas, Vivi. "What Happen to Indonesia's Poverty? "A Micro Simulation Exercise Using Household Surveys." Manuscript. World Bank. Jakarta, Indonesia. March 2002.

that this substantial increase in food prices was a major source of the impact of the crisis felt by Indonesians, except those that belong to the top of the income distribution. Simulation results from Alatas's study indicates that the increase in food prices between February 1999 and February 2000 accounted for approximately 40 percent of the increase in poverty rate after the crisis.

While an increase in food prices could help net food-producers, this large increase in food prices during the crisis resulted in a sharp reduction of real incomes for most of the Indonesian population, who are net food purchasers. The proportion of households below the poverty line rose from about 11 percentage points in 1997 to almost 20 percentage points in 1998 (Frankenberg, Thomas, and Beegle, 1999). The household per capita consumption declined by about 20 percent from 1997 to 1998 (Frankenberg, Thomas, and Beegle, 2003).

Results from physical assessments show no deterioration in children's health status in 1998. There were only negligible changes in the measurements of children older than six months in their height-for-age and weight-for-height. Very young children were well protected from the effects of the crisis although there is a suggestion that weight-for-height of this group of children may have worsened (Frankenberg, Thomas, and Beegle, 1999). The fact that the short-run impacts of the crisis on child health have been small suggests that households may have used various consumption smoothing mechanisms to protect some certain individuals in the households from the crisis effects.

To smooth out the effects of the crisis on consumption, households may have drawn an array of resources available to them. Past empirical evidence showed that a full Pareto-efficient allocation of risk within local communities is rarely achieved. Generally, some idiosyncratic variation still remains uninsured (Bardhan and Udry, 1999). Moreover, when the income shocks are at an aggregate level, cross-sectional risk pooling is not effective. In the case when insurance markets are absent or incomplete, households often rely on alternative income and consumption smoothing mechanisms. These alternative mechanisms are likely to take many forms. Often, the ex post mechanism for consumption smoothing is to smooth consumption over time using saving (assets) and credit transactions (Deaton, 1991; Bardhan and Udry, 1999). Saving and borrowing require a surplus in other periods. The accumulation can take the form of cash, goods, land, or livestock. Since the duration of economic crises are normally unknown, households often face a risk of using up savings and borrowings before adverse conditions improve. Therefore, in many cases, borrowing and saving can only provide short-term relief.

Other consumption-smoothing strategies that households may rely on include changing the allocation of total consumption (e.g. delay consumption of durable and deferrable goods), changing work hours and/or types of work of household members (Murruggarra, 1996), using the entry and exit of household members (Alamgir, 1986) or changing location of residents of household members (Rosenzweig 1988, 1996; Rosenzweig and Stark, 1989), and using children as a substitute for insurance (Portner, 2001; Cain 1981, 1983; Clay and Vander Haar, 1993; De Vos 1985; Nugent 1985; Thomas 1991)

The Indonesian households appeared to have smoothed their consumption by reallocating the household's budget. With a substantial increase in food prices, per capita food consumption was reduced by only 9 percent while expenditures on nonfoods were reduced by about a third. Households substantially reduced per capita expenditures on "deferrable" item including clothing, furniture, and spending on ceremonies, which declined by more than one-third. Investments in human capital (health and education spending) were reduced by around 40 percent.

Households also expanded and relocated during the economic crisis presumably to take advantage of household fixed consumption costs. Household size was expanding in rural areas across the entire distribution of 1993 per capita expenditures while only households above the median 1993 per capita expenditure gained new members in urban areas. Evidence also suggests that some members of the poorest urban households moved to rural areas to take advantage of low costs of living. In addition, households seemed to be adjusting labor supply of their members in an attempt to cope with the crisis. The number of workers and the total number of hours worked by all household members increased in both urban and rural areas. This change reflects an increase number of workers in the wage sector in urban areas and in family business in rural areas. (Frankenberg, Smith, and Thomas, 2003)

Another remarkable channel the Indonesian households used to smooth consumption is through selling previously accumulated jewelry. The ownership of jewelry was reduced by more than 30 percent between 1997 and 1998[3] while the aggregate ownership rates for all other assets have remained stable. The average value of gold sold was approximately equivalent to four months of food consumptions in rural

---

[3] The decline was more than 30 percent in rural areas and slightly less in urban areas. Jewelry was a more common than financial assets in rural areas and most of urban areas. Households stored their wealth using gold rather than financial assets. Those who stored gold were able to sell it for a four-to-fivefold increase in price due to the devaluation of the Indonesian Rupiah.

areas and nine months of food consumption in and urban areas (Frankenberg, Smith, and Thomas, 2003).

In this paper, we investigate ages of female first marriages and first births as yet another potential strategy to smooth consumption. We find the effects of the crisis on these demographic outcomes by comparing, at different ages, the odds of becoming married for women who were exposed to the crisis and that for women who were not exposed to the crisis. Similarly, the odds of having a first child conditional on being married were compared. In addition, we compared the odds of having a first child starting from 14 years of age, unconditional on being married. Our method of analysis incorporates both parametric and non-parametric estimations of the marriage and first births hazards. We find evidence of an increase in the probability of marriage and a decrease in both the conditional and unconditional probabilities of a first birth when exposed to the crisis. We argue that marriage and fertility delay may have been used as consumption smoothing mechanisms to cope with the Indonesian crisis. Using marriage of individuals in the households provides additional sources of economies of scales and connection with other households to smooth consumption. However, the decision to postpone or forgo having a child during the crisis may have been due to a dominating income effects caused by a drastic decline in real income and a sharp increase in prices.

Background

Economic crises could affect individuals' decisions on household formation and first birth through changes in prices, employment, and wages received. In terms of changes in employment, Frankenberg, Thomas, and Beegle (1999) found that overall the 1998 crisis had no massive short-run impacts on the unemployment rate among men. However, the crisis had different impacts on men of different age groups. The proportion of men who worked and men who worked for pay increased significantly for men aged 15-24 years old while the proportion of men working for pay declined for an older age group (36-64 years old). The impacts of the crisis on employment among women were distributed across all age ranges. Overall, there was a significant increase in the proportion of women working from 49.2% in 1997 to 56.2% in 1998. The authors found that this increase in proportion of women working was due to more women working as unpaid family labor because the change in the proportion of women working for pay was insignificant (from 36% to 37%).

In terms of wages, Frankenberg et al. found that the 1998 crisis resulted in a very large decline of 20% to 30%[4] in the median real wage of all workers between 1997 and 1998. The declines in real wages were larger for men. Urban residents seemed to be more adversely affected.

The evidence of an increase in the proportion of women working in family businesses and an increase in the proportion of young men working for pay suggests that marriage rates could have increased during the crisis to utilize economies of scale and the comparative advantage of the partner's skills provided by cohabitation, given that men

[4] Depending on the deflation method used.

had comparative advantage working outside home compared to women.[5] Also, income-pooling may have been important for couples to smooth their consumption during the crisis.

No evidence exists on the effects of the Indonesian economic crisis on the age of female at marriage. Marrital age, however, is known to have increased in Indonesia. Hull and Jones (1994) reported a strong upward trend in marital age. They found that the mean age of marriage had risen in all provinces of Indonesia throughout 1960s, 1970s, and 1980s. Overall, the average age at marriage of women in Indonesia was 19.3, 20.0, and 21.1 in 1971, 1980, and 1985 respectively. In Java, the average age increased from 18.1 to 20.7 from 1964 to 1985. Similarly, data from the Demographic Health Surveys showed that the median age at first marriage for women aged 25-49 years rose from 17.2 years in 1987 survey to 18.6 years in 1997 surveys.

It is worthwhile to note that at the same time Indonesia experienced a fertility decline nationwide. Hull and Jones (1994) reported a 40-percent decline in the fertility rate from the 1960s to the 1980s. Overall, total fertility rate was 5.61 in 1967-70 and 3.33 in 1986-89. The authors claimed that the evidence of a major fertility decline in Indonesia was robust to various fertility estimation techniques (see Hull, 1980; Hull and Hull, 1984).

Similar to the crisis effects on the propensity to marry, empirical evidence of the short-run effects of the Indonesian crisis on ages of first births is limited. As long as fertility remains a normal good, theories of the demand for children generally lead to a hypothesis that deterioration in income leads to a delay of having first birth due to income effects. However, an increase in the demand for children could take place due to

[5] Note that cohabitation between non-married couples is still rare in Indonesia.

substitution effects when female wages decline. If fertility can be consciously controlled, contraception use will be adopted to control family size, provided that couple can afford contraception.

Frankenberg et al. (1999) reported that although there was a small change in the type of providers from whom women obtain injections, there was no significant change in prevalence or method mix of contraception during the first year of Indonesia's economic crisis. Contraceptive prevalence was estimated to increase from 56.6% in 1997 to only 57.3% in 1998. The authors concluded that the economic crisis did not result in changes in contraceptive behaviors. They argued that this stability of contraceptive prevalence suggests that contraception is a more appealing option than the risk of having an additional child in the economic crisis environment for the majority of couples in Indonesia.

Theoretical Framework

The Impacts of Economic Crises on Marriage

It is clear from our previous discussions that the Indonesian financial crisis resulted in deterioration in real income and real wages. In this paper, we argue that the combined effect of declines in real income and real wages on women's marriage propensity is *a priori* ambiguous. Specifically, we argue that changes in income and wages induce potential gains from marriage through income pooling and specialization. At the same time, the demand for marriage could decrease due to a decrease in the demand for home-produced goods because of adverse income effects. A drastic decline in wages also decreases the opportunity cost of home-produced goods, resulting in a higher propensity to marry. On the other hand, the crisis could delay marriages through increased in direct costs of marriage (e.g. cost of setting up new a household). Furthermore, if the crisis adversely affects expectation of future income (probability of low future income) and its volatility, households could use marriages of individuals in the households as a tool to smooth future consumption, resulting in an increase in marriage rates.

According to Becker (1973), two persons choose to marry each other if and only if both of them are made better off from marriage than from remaining single. A couple combines their time and market goods and services to produce "household goods," from which their utility is drawn. Examples of these goods include quality and quantity of children, quality of meals, and mutual love and companionship. Since "household goods" are produced partly by marketable commodities, each household member allocates his/her time between marketable and non-marketable activities in the "appropriate proportions."

Single persons could gain from marriage at the time of an economic crisis because marriage provides additional time and good and services from the spouse assuming increasing return to scale. By pooling time and incomes, Becker argues that marriage provides economies of scale in production of both market and household goods when the subsititutability between the times used by each partner is imperfect. The more complementary the inputs (the time of spouse and market goods) provided by each spouse, the more the gain from marriage (see proof by Becker (1973)). In short, the partner with earning capacity outside the home tends to specialize in paid employment, while the other tends to specialize in home work. The gain from this specialization is larger when the disparity between wage rates increases.

When the income of the household decreases, the demand for home produced goods (e.g. quantity and quality of household works and children) decreases, provided that these goods are normal goods. This income effect implies a marriage delay as a result of an economic crisis. At the same time, the deterioration in female wages lowers the shadow price of home-produced goods, resulting in an increase in the demand for marriage. In the case when both male and female wages decrease, this substitution effect implies that the demand for marriage decreases when the decline in male wages is higher than that of the female wages (the female/male relative wages is higher).

Keeley (1977) incorporated search cost into the marriage theory developed by Becker (1973). The gain from marriage depends on the combination of the characteristics of each of the mates. According to Keeley's marriage search model, a single person enters the marriage market only if the expected gain from marriage exceeds the costs. If the benefit exceeds the costs, the searcher decides to enter the marriage market. Then the searcher decides for a minimum accepted offer, which is the share of the total home produced output the searcher would receive when married. After the searcher enters into the marriage market, the searcher accepts any offer of marriage that equal or are higher than her acceptance wage. This theory implies that a decrease in "single" income decreases the duration of marriage search. Therefore, based on this search consideration, a decrease in "single" income (both current and expected future income) as a result of an economic crisis increases the propensity to marry. On the hand, the theory also implies that in the time of the crisis, a decrease in direct costs (monetary and opportunity time cost) of search for partner increases the duration of search, which results in a delay of entering into marriage. In sum, Keeley's marriage search theory implies that the net effects of the Indonesia crisis on the incentive to marry is ambiguous *a priori* since both income and opportunity time cost (wages) declined as a result of the crisis.

Recall that the economic crisis adversely impacted income and wages of both men and women. The gain from marriages, then, depends on the relative income (wages) of men and women. Holding the income of men constant, when the income of a woman (or income of her household) deteriorates, there is a higher probability for her to enter into the marriage market because of expected gain from marriage due to specialization is high. The minimum offer she is willing to accept (compared to her single's income) will be low, resulting in shorter duration of search. At the same time, her low opportunity time cost allows longer duration of search.

Holding income of women constant, when the income of men deteriorates, the women's expected gain from marriage decreases. Women may decide to stop or delay entering into the marriage market. Even if they do enter into the market, the duration of search will be longer given that there is a high proportion of men who are unable to offer the minimum accepted offer the women have set.

Contrary to results in previously discussed theories, the demand theory of marriage also predicts a positive relationship between marriage rates and economic well-being when taking into account changes in costs associated with marriage and resource availability to the household. This view is widely accepted in many studies as an explanation for marriage delay as a consequence of economic crises. According to Hill et al. (1993), marriage costs could be in explicit or implicit terms. Explicit costs include costs incurred with the process of marriage (e.g. payment for a bride price and a new home). Implicit costs, such as a "perceived need" for a certain level of wealth and income security before marriage, may also play and important role in the marriage decision. An economic crisis may stop or delay marriage decisions because it increases financial constraints on the ability to set up a separate household (e.g. purchase a house or other consumer durables) especially when borrowing opportunities against future income are limited. Financial constraints could result in inability of males to accumulate enough resources to facilitate marriage (Hill et al., 1993). Palloni and Hill (1996) find that empirically the typical pattern of nuptiality response of a crisis is an immediate drop in the number of marriages. This drop in marriage is usually followed by a lagged increase above and beyond expected rates in normal times. The authors claim that although the duration of this offsetting response may depend on the severity of the crisis, "the number of marriages will decrease during the first two years after the onset of the crisis, followed by an increase, as its impact receded." See summary results below.

Considerations	Income lower	Wf lower	Wf/Wm higher
Income pooling and cost sharing	Increase	Increase	Increase
HH demand			
Income Effects	Decrease		
Substitution Effects		Increase	Decrease
Search			
Probability of Entering	Increase	Increase	Decrease
Duration of Search	Decrease	Increase	Increase
Direct Cost of Marriage (e.g. housing)	Decrease		

In addition, an economic crisis could result in an increase in the number of marriages due to a household' income-smoothing consideration. Kotlikoff and Spivak (1981) and Rosenzweig and Stark (1989) offer an alternative theory that is in dissonance with standard models of marriage. Their hypothesis is that a marital arrangement plays an important role in a household's ability to smooth consumption when faced with highly variable income streams, especially when access to credit markets is limited. Specifically, they argue that marriage arrangements, or "exchange" of individuals among household, serve to mitigate income risk and facilitate consumption smoothing, under conditions that there are informational costs and spatially covariant risks. Under this consideration, implicit risk sharing arrangement among households can provide strong economic incentives for marriage.

According to Rosenzweig and Stark's review, empirical evidence indicates that inter-household family transfers provide an important source of income insurance in low-income countries. For instance, data from the Malaysian Family Life Survey (MFLS) show that "69 percent of all women who had ever moved from one town to another did so at the time of their marriage, with 32 percent of all moves (town to town) by women accounted for by marriage" (Rosenzweig and Stark, 1989). In India, households that experienced income shortfalls associated with variation in weather patterns relied heavily on nonresident in-laws to provide income transfers (Caldwell, Reddy, and Caldwell 1986; Rosenzweig 1988). Caldwell et al. (1986) found that, in nine villages in South India, 56 percent of the relatives that provided aid during droughts were either relatives of the head's wife or those of the husbands of the head's daughters. Similarly, data from MFLS suggests that 39 percent of the values of all goods and cash transfers are for "emergency" help and for "maintaining" household's expenditures (Stark and Rosenzweig, 1989; Butz and DaVanzo, 1978).

Note that using marriage as an income-smoothing tool is more likely an *ex ante* consumption smoothing consideration to protect households against future income fluctuation. Thus, under this consideration, an economic crisis could result in an increase in marriage rates only when the crisis adversely affects the expectation of future income and its volatility. For instance, if households (individuals) increase the degree of risk aversion after an economic crisis takes place, there is an increase in marriage gain, which in turn induces higher propensity to marry (for proof and simulation results, see Kotlikoff and Spivak (1981)).

The Impacts of Economic Crises on Fertility

Economists hypothesize that fertility decisions are made by rational individuals to maximize their well-being.[6] According to Becker (1960), utility is maximized subject to an exogenously determined budget. The demand for children is then affected by income

[6] Part of this theory review is drawn from a review by Marvellous M. Mloyi (1992).

and prices. As long as fertility stays a normal good, one would expect a contraction in income and an increase in prices to be associated with a fertility decline, or at least by a pause in family expansion (Bertrand et al., 1993). However, demand theory also considers other input goods such as time of the mother needed to raise the quality of children (Mincer 1963; Becker et.al, 1973). When wages decline, the opportunity costs of time to raise the quality of children are lower. This substitution effect induces an increase in fertility.[7]

An economic crisis could also alter expectations regarding future incomes. When fertility within marriage can be consciously controlled, conventional microeconomic theory predicts that "couples will delay births in response to sudden declines in income." (Galloway 1988; Lee 1990; Palloni, Hill, and Aguirre 1996) A shift to small family size desires is a function of both individual and community variables. Once couples desire small family sizes, the adoption of contraception use will facilitate the realization of small family sizes.

The reduction in family size will also result from changes in other supply variables in addition to contraception (Mloyi, 1992). In addition to conscious adjustments in fertility, economic crises can have unintended adverse effects on fertility behavior. Psychological stress and declines in nutritional status associated with

[7] In this paper, we consider only decision on the timing of first birth. Becker and Tomes (1976) argue that when quantity-quality of children are taken into consideration, this increase in the women's contribution to home work (e.g. quality of children) raises the cost of an additional child, which reduces the demand for quantity since higher quality of children would then be more expensive.

economic crisis tend to reduce fecundity[8] and increase abstinence (Bongaarts and Cain 1981; Caldwell and Caldwell 1992; Kidane 1989). Fertility may also be reduced by spousal separation due to labor migration as one or both partners search for means to maintain consumptions, usually through employment (Galloway, 1988; Lindstrom and Berhanu, 1999). Additionally, with the circumstances of economic hardship, nutritional supplementation and/or termination of breastfeeding may be reduced and delayed. As a result, the protective effect of natural contraception from breastfeeding may be retained, even in the case of increased contraception usage. (Mloyi, 1992)

Hill et al. (1993) emphasize the importance of distinguishing short-run and long-run economic effects on fertility and marriage because the relationship that links economic change and these events in the long run are complex. They claim that while the causes of short-term variation in fertility are well-understood, "there is not a clear consensus about the mechanisms underlying the long-run relationship between economic change and fertility." (Hill et al., 1993)

When taking into account long-term considerations, economic crises could induce fertility increase by a "risk insurance approach." Under this theory, high fertility is considered insurance against long-term insecurity. (Cain 1981, 1983; Clay and Vander Haar, 1993; De Vos 1985; Nugent 1985; Thomas 1991). Caldwell (1976, 1978, 1982) proposes a "wealth flow" theory, which suggests that as long as wealth flows from children to parents, fertility could remain high evenn if the costs are high since it is economically rational for a parent to have more children. Cain (1983) offers similar line of reasoning. In his view, when there are limited extra family resources or institutions,

[8] It is argued by Hill et al. that the role of nutrition sub-fecundity may be limited in the case where scarcity is not severe. The evidence of a significant decrease in fecundity is weak. (Menken, Trussell, and

children can offer old-age security to parents, and can offer added insurance against "daily-survival risks," which may range from droughts to illness.

Portner (2001) gives a theoretical framework that explains how children can provide insurance against uncertain future incomes. The main hypothesis of his work is that children can act as a security asset when insurance and credit markets are either absent or poorly functioning. His theory is based on an assumption that children are likely to be more reliable as a means for insurance than more distant family. In Portner's model, children are modeled in a dynamic setting as a general insurance and saving asset. In his model, children are costly to the household in the first period of their life, but they provide a positive net income in subsequent periods. Children can help by working at home or as wage labor, and older children who either have their own households or have migrated can make transfers to their parents. Hence, "parents use children as a means to shift income from a period with certain income to future periods with uncertain income, thereby insuring themselves against the possibility of low income." (Portner, 2001) His model implies a positive relation between income and fertility in a given period holding constant expected future income. When the future expected income is not fixed, the model predicts a negative relation between future expected income and the number of births since an increased probability of low future income leads to high demand for insurance and therefore more births. This theory implies that it is not sufficient to observe present income to determine demand for children. To completely account for the effects of an economic crisis on fertility, one needs to also assess people's own expectation of future income and its variability, which are hardly observed in the data.

Watkins, 1981)

Furthermore, Portner (2001) suggests that there is very little direct evidence on whether children serve as a substitute for insurance.

In addition to effects of economic crises on fertility through changes in income and wages, economic crises could affect changes in fertility through the effects of economic crisis on child mortality. Sah (1991) analyzed the effects of child mortality on fertility decisions assuming that parents derived direct utility from the number of surviving children. This model implied that when child mortality increased as a result of the crisis, parents demanded more children to insure against the risk of child mortality in order to maintain the number of children they desire.

In addition, as implied by Portner (2001) an increase in the probability of child mortality has both substitution and income effects on the demand for children as insurance. First, the substitution effects imply that an increase in the probability of child mortality results in a lower return to births (i.e. more wasted resources). Secondly, The income effects imply that a lower expected number of survivors leads to a lower expected consumption in the future.

While the substitution effect tends to decrease the optimal number of births the income effect tends to increase number of births. If the income effect dominates, the optimal number of birth will increase. Moreover, the more risk averse a household is, the more likely it is that the income effect will at one point dominate the substitution effect when the probability of child mortality is increased. Therefore, the model is able to illustrate the observed increase in fertility following an increase in infant and child mortality, provided households are indeed risk averse.

Previous Studies

According to an empirical review by Galloway (1988), high food prices were significantly associated with a decline in marriages in England, France, Sweden during pre-industrial periods. Likewise, poor harvests resulted in a decline in marriages in Croatia. Historical evidence also suggests a positive relationship between economic well-being and fertility increase. According to the same review by Galloway (1988), historical research showed that when grain prices increased, England, France, and Sweden experienced fertility declines. Similarly, a rise in fertility was linked to an increase in real wages in Sweden and in the harvest in Croatia. In countries outside Europe, Galloway and Lee find that high prices were strongly associated with fertility decrease in pre-World War II Bombay Presidency, slightly associated with a fertility decrease in Taiwan, and not significantly related to changes in fertility in Japan.

Das Gupta (1995) examined fertility decline in Ludhiana District in India. She found evidence that was parallel to that from historical Europe. Total fertility in this area began to decline around 1940, which was before the onset of family planning programmes and the Green Revolution in 1966. According to Das Gupta, this decline in fertility was partly due to the expansion of irrigation system that resulted in an increase in level of yields and a decrease in the yield fluctuation, which in turn results in increased security against mortality risks and food shortages.

In his 1988 study, Galloway investigated the short-run effects of economic crises on fertility, nuptiality, and mortality in Pre-industrial Europe using fluctuation of grain prices and vital statistics. The temporal unit of analysis in his study is the calendar year. Detrended crude birth rates and marriage rates were used as the dependent variables.

Detrended grain prices and detrended non-infant death rates each distributively lagged five years were the independent variables.[9] Galloway found that fertility was highly sensitive to fluctuations of grain prices. As he expected, the largest fertility response occurred one year after the price shock. Both the patterns and the magnitude of elasticities of responses were similar in all areas and all periods. On average, most of the effects of high prices on nuptiality occurred at lag 0, but there was considerable variation between countries.

A similar study by Palloni and Hill (1996) investigated the effects of economic swings on fertility, nuptiality, and mortality in Latin America from 1910 to 1989. In this study, real average GDP was used as an indicator of economic well-being. The authors estimated the effect of this indicator on number of reported births, marriages, and infant mortality rates using local least squares with data from eleven countries. The results showed the response of number of marriages at lag 0 was positive in seven countries (Argentina, Chile, Cuba, Costa Rica, El Salvador, Uruguay, and Venezuela), but the effects were statistically significant only in Chile, Uruguay, and Venezuela. The pattern of responses lag was consistent with expectations. Results from four countries gave negative lag 0 marriage response. The only country that had all negative responses is Guatemala. The results on marital fertility showed greater heterogeneity in the patterns of responses. Data from five countries indicated positive responses at lag 0 and 1 to real

[9] Non-infant death rates are used as proxies for both adult mortality and morbidity in this paper since adult mortality rates are not available.

average GDP,[10] but only in Cuba (lag 1) was the response statistically significant. In six countries, the responses of lag 1 had the unexpected sign.

Hill et al. (1993) used economic conditions such as gross domestic product per capita, the quantity of exports, and terms of trade to study the effects of economic reversals in Sub-Saharan Africa on child mortality, the odds of first marriage, and timing of first and second birth. The authors found that the effects of economic reversals on first births are the strongest among the four demographic outcomes. They found evidence of first-child fertility delays in all seven countries studied, except in Kenya. For first marriage, the study revealed evidence of a positive association between economic conditions and the odds of first marriage in only Botswana, Senegal, and Togo.

Rutenberg and Diamond (1993) examined the rapid fertility decline in Botswana during 1981 and 1989. Based on their examination of fertility rates and employment of traditional agricultural workers in rural areas and their reviews of relief programs during the drought crisis, the authors claimed that "the decline in fertility was linked to a deterioration in social and economic conditions caused by a major drought in the early 1980s and to the increased availability of family planning services in the same period. Fertility began to rebound in the late 1980s in response to improved conditions, which came about as a result of a successful drought relief program. Further declines in fertility depended on the continued success of the family planning program, particularly in the rural areas."

Lindstrom and Berhanu (1999) examined fertility trends between 1973 and 1989 in Ethiopia for evidence of short-term and long-term responses to famine, political

[10] Seven out of eleven countries have positive responses at lag 0. The authors offer an explanation that this positive response would be expected only if there was a possibility of "anticipatory behavior, expressed as

events, and economic decline. Using year dummies as identifying explanatory variables and controlling for characteristics thought to be powerful predictors of fertility (e.g. age at the start of the interval, age squared, and place of residence in the current year), the authors found evidence of significant short-term declines in probability of conception during years of famine and major political and economic crisis. As for long-term effects, the authors found that in both rural and urban areas, fertility declined in the 1980s after increasing moderately in the 1970s.

Mckenzie (2002) studied how Mexican households coped with the 1994-1996 Peso Crisis. His result showed that one strategy households used to cope with the crisis was a reduction in fertility during the crisis, with about one in twenty households decided to postpone or forego having a child during the crisis.

---

reduced number of conceptions or increased numbers of voluntary and spontaneous abortions."

Methodology

We find the effects of the crisis on the ages of female first marriage and the timing of first births by comparing, at same ages, the odds of becoming married for women who were exposed to the crisis and those for women who were not exposed to the crisis. Similarly, the odds of having first child were compared. The dependent variables of interest are the odds of an event occurring.

1) the odds of a women becomes married in year *t* given that she is unmarried at the start of year *t*,

2) the odds of a married woman having the first child after *t* years of marriage given that she has no children at the start of year *t* after marriage. To avoid the complication of a considerable decline in the number of births after first births and other constraints that may affect fertility after first births (e.g. sex preference and condition of the mother after the first birth), this fertility study limits the analysis to first births. Note that in this conditional estimation, we take the age of marriage as exogenous when studying the odd of having first births.

3) the odds of a woman having her first child at *t* years of age given that she had no children at the start of year *t*. In this estimation, *t* starts at age 14 years regardless of whether the women were married. This unconditional hazard estimation takes into account the possibility that the net effects of the crisis, estimated from the conditional fertility hazard estimations, could be biased if the marriage hazard was affected

by the economic crisis. Similar to the conditional analysis, we limit the unconditional analysis to studying only first births.

Nonparametric Estimation of Hazard of Marriage and Fertility

The nonparametric estimates of discrete yearly hazard rates are based on the Nelson-Aalen cumulative hazard function. This cumulative hazard function produces estimated hazard components. It is recorded at all the points at which a failure occurs and computed as d_t/n_t, where d_t is the number of failures occurring at time t and n_t is the population alive at t before the occurrence of the failures. In the marriage analysis, t starts when the woman is 11 years old. In fertility analysis, since age of first pregnancy could be greatly influenced by age of marriage, we use number of years after marriage as the length of time in our survival analysis.

Parametric Estimation of Hazard of Marriage and Fertility

In addition to nonparametric estimations, we estimate the effects of the crisis on marriage and fertility separately using different parametric hazard models. Let $T \geq 0$ denotes the length of time the woman "survived" (remained single) in years. Similar to nonparametric estimations, in marriage analysis, this T starts when the woman is 11 years. In the fertility analysis, we use number of years after marriage as the length of time in our survival analysis.

In this paper, we estimate maximum-likelihood (cox) proportional hazards models and hazard models that assume some distribution of survival time in the population.

Assume T has some distribution over the population. Consider time invariant covariates.

Let F(t; x) = conditional cdf of T where x = covariates; province of residence, rural/urban, village/small town/big city, women's education:

$F(t;\ x) = P(T \leq t;\ x),\ t \geq 0$

The survivor function is defined as

$$S(t;x) = 1 - F(t;x) = P(T > t;x)$$

Then, the probability of leaving the initial state in the time interval (t, t+h) is

$P(t \leq T < t+h \mid T \geq t;x)$ for h > 0

Define the hazard function as

$$\lambda(t;x) = P(t \leq T < t+h \mid T \geq t;x) = \frac{P(t \leq T < t+h;x)}{P(T \geq t;x)} = \frac{F(t+h,x) - F(t;x)}{1 - F(t;x)}.$$

If the c.d.f. is differentiable, then take the limit of the right-hand-side, divide by h, as h approaches zero. The hazard function is

$$\lambda(t;x) = \lim_{h \to 0} \frac{F(t+h,x) - F(t;x)}{h} \circ \frac{1}{1 - F(t;x)} = \frac{f(t;x)}{1 - F(t;x)} = \frac{f(t;x)}{S(t;x)}$$

Then all probabilities can be computed using this hazard function. For example, from time a to time b, a < b is

$$P(a \leq T < b \mid T \geq a;x) = 1 - \exp\left[-\int_a^b \lambda(s;x)ds\right].$$

We estimate the hazard function based on different assumptions of the population distribution of T. In our analyses, we estimate hazards models using Generalized Gamma, Weibull, and Lognormal distributions. The standard error and the z statistics of the estimated coefficients are robust to heteroskedasticity of the variance-covariance matrix and clustering at the individual level. In addition, the estimations assume heterogeneity in our observations. The observations are assumed to have inverse-Gaussian heterogeneity (frailty). The hypothesis being tested is the significance of the crisis dummy variable when the time trend is included.

Data

This paper uses data from the Indonesia Family Life Survey (IFLS). IFLS is a continuing longitudinal socioeconomic and health survey that includes more than 30,000 individuals living in 7,200 households. The sample covers 321 communities in 13 provinces in Indonesia and represents about 83 percent of the Indonesia population in 1993[11]. The first wave of IFLS was fielded in 1993 (IFLS1). The same households were revisited in 1997 (IFLS2) and again in 2000 (IFLS3). This paper uses the data from these three IFLS waves. A 25 percent sub-sample of households was re-interviewed in 1998 (IFLS2+), but the data are not used in this paper.

In IFLS surveys, special attention is paid to the measurement of health, work, migration, marriage, child bearing, life history data on education, and economic status of individuals and households. In each wave of IFLS, the individual and household surveys are complemented by an extremely comprehensive community and facility survey. There is also considerable attention placed on minimizing sample attrition in IFLS. Targeted households and individuals who "split-off" from original households were followed if they moved to new locations within 13 provinces of the survey areas. In each re-survey, about 95 percent of targeted households have been re-contacted. The split-off households added just under 1,000 households to the sample in 1997 and about 2,600 households in 2000. The survey periods of each IFLS wave is shown in Figure 1.

---

[11] Frankenberg, E., Hamilton, P., Polich, S., Suriastini, W., and D. Thomas. User's Guide for the Indonesia Family Life Survey. DRU2238/2-NIA-NICHD. March 2000.

Marriage Data

Marriage data are taken from all three waves of IFLS. We start with IFLS3, which was surveyed in 2000 to obtain complete marriage history of women in our sample. We obtain retrospective marriage data from all women aged 15 and above at the date of the interview. Data of ever-married women aged 15-49 years are taken from "ever-married-women questionnaire." Data of ever-married women aged 50+ years, never-married women aged 50+ years, and never-married women of all ages are taken from "adult questionnaire," which includes all women aged at least 15 years.

From both sources, data on age at the time of the survey, birthdate, and date (or age) of the first marriage (if ever married) are obtained. The marital history is complete for new respondents. When the woman ever married and is a panel respondent, further efforts are taken to track the date of the first marriage from earlier IFLS waves using the personal identification number that is consistent across all IFLS waves. To avoid problems related to outliers, we excluded those that reported the first married before 11 years of age. However, before the observation was taken out of the sample, we investigate whether there was a reporting error by reconciling reported birthdate, the age at the interview date, and the date (or age) of the first marriage.

Fertility Data

Data on pregnancies are obtained from retrospective data on pregnancy histories. Complete pregnancy histories were given by married women whose ages were between 15 and 49 years in "ever-married women questionnaire." These women were asked about information of each pregnancy in detail (if she ever had any). The information we use includes pregnancy order, pregnancy outcome, and the date of the end of pregnancy. Since we investigate fertility decision to have first birth, the date of the end of each pregnancy was taken regardless of whether the pregnancy ended in a birth or a miscarriage (Henceforth, the date of the end of each pregnancy will be called "birth date.").

We start with IFLS1 data to obtain data on number of children and pregnancy information of each ever-married women. For women who ever had any pregnancy, only information of her first pregnancy was extracted. In the case where the birth year of the child is missing, we computed the birth year of the child from the age of mother when pregnancy ended (if reported) and mother's birth date/age at the time of the survey. We encountered a small data problem that birth year of the first child is before the year the women married for 173 cases (out of 4,637 cases). For these observations, we search for marriage information of the women in subsequent IFLS waves, starting with IFLS2 first. As a result, 78 observations were recovered. Further, if a woman had never given birth by the IFLS1 interview date (1993), we track them in IFLS2 and IFLS3 whether they ever gave birth during subsequent IFLS surveys. If they did, information on her first pregnancy was extracted.

After obtaining data from IFLS1 respondents described above, we added women who appear in IFLS2 that did not appear in our IFLS1 sample. These women include those who were at least 15 years old who became married between IFLS1 and IFLS2 surveys and new respondents from split-off households. For these respondents, we carried similar procedure to IFLS1 to obtain the data. Similar tracking procedures were also carried out to track these new observations in IFLS3 survey. We had only 45 observations that birth year of the first child is before the year the women married. 17 of these observations are recovered and corrected using IFLS3 data. Finally, we added observations from IFLS3 that that did not appear in our sample, using similar consistency checks described.

In the case when a woman had multiple marriages, special attention was paid to carefully time the marriages for fertility exposure. For example, suppose a women married at the age of 18 years, ended her marriage at the age of 21 years, married again at the age of 25 years, and had first pregnancy at the age of 27 years, her exposure to fertility would be only between 18-21 and 25-27 years. Note that when the information on the date of the end of marriage was not reported, we compute this date from the reported age of the woman when marriage ended using also her birth date/age at the time of the survey.

Duration Data

In the conditional fertility analysis, the entry of women into the duration analysis was simply the age when they first married. We specify the failure time to be the time when the woman ended her first pregnancy. In other words, the women in our sample contribute each year after their marriages to the duration analysis until the first pregnancy took place (for those who were ever pregnant). For instance, if we take the above example and know that the same woman ended her first pregnancy when she was 20 years old, this woman contributes to our analysis only from 18-20 years (or two years after marriage). The exposure from 20-21 and 25-27 years old will be discarded since the exposures were after her first pregnancy.

Furthermore, since we did not observe the end of the survival period of never-pregnant women in the sample, the survival data obtained from the survey are considered "flow" data, which are subject to time censoring. In this case, the data are right-censored at the interview date.[12] For example, if a woman married when she was 18 years old in the year 1990 and never had any pregnancy by the time of IFLS2 survey (1997), we only know that the woman's survival time was at least seven years. We never observed real survival time of this woman. Thus, this woman only contributes first seven years after marriage to our duration analysis.

In marriage analysis, we specify the entry age to be eleven years old. Women exit our analysis when their first marriages took place. For those that never married, similar procedure for censoring was applied. For instance, if a woman was born in 1973 and was single by IFLS3 (2000) survey, this woman contributes sixteen years of exposure to marriage, starting from when she was 11 and ended when she was 27 years old.

In addition to data on marriage and pregnancy history, data from individual, household, and community characteristics surveys are used to allow this study to control for other socioeconomic characteristics such as education, province of residence, and type residence (urban/rural, village/small town/big city) at the time of exposure to marriage and fertility.

Following a general practice in marriage analysis, the residence of women in our marriage analysis is the residence of women at the time when they were 12 years old. This residence information, whether the woman resided in a village, a small town, or a big city, is assumed to be the residence before the woman married. The residence

[12] For those we tracked in subsequent IFLS waves, the interview date is from the last IFLS wave used in tracking the observation.

information in our fertility analysis is the residence of the woman at the time of survey. We take this to be a proxy for the post-marital residence. The residence of women after their marriages are thought to be correlated to factors that affect fertility decisions and fertility control, such as prices and type of available healthcare, more than pre-marital residence.

Identifying "Crisis" Covariate

In both marriage and fertility analyses, given the time and duration of the Indonesian economic crisis, we identify the year 1998 as the crisis year. If the exposure period is in 1998, the crisis dummy was set to be equal to 1. Since the same woman may be exposed to both crisis and non-crisis periods at different ages, the crisis dummy is a time varying covariate. Hence, data are constructed so that for those that had an exposure to the crisis year may have multiple records for different crisis dummy covariates. For example, in the marriage analysis, a woman born in 1974, whose first marriage year was in 1999, was not exposed to the crisis when she was 11-23 years. She was exposed to the crisis when she was 24. Then at the age of 25 years, she married when she was not exposed to the crisis in 1999.

Results

Results on ages of female first marriage

Table 20 presents summary statistics of female first marriage age by birth cohort from 1921 to 1975. Our data include information from younger females born in 1976-1986, but their marriage-age summary is not reported here because these women had not been fully exposed to almost all of the marriage age groups yet. Although we cannot observe any trend in the age of female first marriage from the median age, a breakdown of the proportions of ages of women when marriages occurred indicates an upward trend in female marriage ages. The proportion of marriage ages between 11 and 14 years old declined from 12.8 percent to 4.2 percent from the oldest cohort to the youngest cohort. During the same period, the proportion of marriage ages between 23 and 25 years old increased from 10.3 percent to 20.6 percent. An interesting trend that is consistent with this increase in the ages of marriage is that the proportion of those who remained single increased over time. This proportion increased from only 0.7 percent for those born in 1921 to 1940 to 7.1 percent for those born in 1966-1970.

A similar trend can be drawn from observing female marriage ages in different calendar years. Table 21 presents the proportions of marriage ages of women in different years from 1988 to 2000. The proportion of women whose first marriage occurred when they were 11-14 years old declined from 4.2 percent to 1.4 percent from 1988 to 2000 while the proportion of women whose first marriage occurred when they were 31-35 years old increased from 1.4 percent to 4.1 percent. Figure 9 shows results from nonparametric estimates of first-marriage-age hazard by three birth cohorts, 1900-1940, 1941-1960, and 1971-1980. From these estimates, we observe that the hazard rates of first marriage at younger ages declined over time. For instance, the probability of getting married at the age of 15 years (if single at the beginning of the year) is 0.12 for those born during 1900-1940, 0.09 for those born during 1941-1960, and only 0.01 for those born during 1971-1980. The trend is reverse for older ages, indicating an upward trend in the ages of first marriage.

Figure 10 and 11 show nonparametric estimates of first-marriage-age hazard by education. Figure 10 presents estimates of the hazard rates of women who had no education, 1-5 years, and 6-8 years of education. Compared to those who had no education, Indonesian women who had higher education had a lower likelihood of getting married at younger ages (11-15 years old), but they appeared to be more likely to get married after 20 years of age. Even though the delay in marriage for the higher-educated groups may be due longer time spent in school, it appears that once they finished schooling, the probability of getting married quickly started to surpass that of uneducated women. More apparent evidence is shown in Figure 11. Uneducated women had a much lower probability of getting married than those with 9-11 or 12+ years of education in their 20s. Those with 9-11 years of education had the highest probability of getting married among the three groups in their 20s, with an exception of 26 to 28 years old where the marriage hazard was highest among those with 12+ years of education. Interestingly, the probability of getting married for 12+ years group remains higher than other groups in their 30s.

The differences in the marriage hazard rates of women with different education levels may be explained by theories related to human capital investment, opportunity search costs, and expected income gain from marriage. However, explaining these differences in the marriage trends is not our objective in this paper. The purpose of this exercise is to demonstrate that education levels influence female marriage hazard rates. Thus, ignoring education level could cause a bias in our estimations of marriage hazard rates when studying the effects of the economic crisis on ages of female first marriage.

Figure 12 presents estimated first-marriage hazard rates at different ages by women' residence when whey were 12 years old. The IFLS survey stratifies these residence data into village, small town, and big city. A clear picture emerges from these estimated hazard rates. Among these three groups, those who lived in a village had the highest probability of getting married at every age from 11 years up to approximately 24 years old. Urban residents such as those who lived in small town or big city tended to have a higher probability to marry later in their lives. This differential trend could be due to different characteristics of urban and rural residence such as consumption prices, marriage market conditions, or different characteristics of those who live in urban and rural areas (e.g. education).

Results from parametric marriage hazard regressions using Cox-proportional hazard assumption are presented in Table 22. In this table, we show results from six different specifications, all with standard errors of the estimated coefficients that are robust to heteroskedasticity and clustering at the individual level. In these regressions, only women born between 1941 and 1986 are included to minimize recall errors. Moreover, including only those born in a more recent periods forces the control groups (those who were not exposed to the crisis) to be more comparable to the targeted group (those who were exposed to the crisis) in the case when birth cohort dummies do not pick up all of the time trend effects.[13] The univariate regression (1) shows a positive, but insignificant estimated coefficient of the exposure to the crisis risk. The estimated coefficients of the crisis exposure are larger and statistically significant in all multivariate regressions, suggesting an increase in the hazard of female marriage during the economic crisis. Over time, women were less likely to get married. The estimated trend is significant assuming either linear (birthyear) or non-linear (birthyear cohort dummies) time trend. Overall, more years of education lower the probability of getting married, but the estimated coefficients from regressions using education level dummies (regression 5 and 6) show that those with 1-8 years of education exhibited higher probability of getting married than those with no education while those with education higher than 8 years had lower probability of getting married. Interestingly, the effects of the time trend are much smaller when we include education in the regressions. The estimated coefficient of the linear time trend decreases from –0.016 to –0.001 when years of education is included in the regression and to –0.005 when education level dummies are included. These results suggest that the time trend captures also the increasing trend towards higher education of women over time. Finally, women who resided in villages and small towns are more likely get married than those resided in big cities.

Table 23 presents results from the parametric regressions of marriage hazard using Generalized Gamma distribution of the survival time. For robustness check, we chose to report also the results from Lognormal distribution.[14] The reported standard errors of the estimated coefficients are robust to heteroskedasticity and clustering at the

[13] We have estimates from all women (born in 1900-1986), but these estimates are not reported here. The estimated coefficients and their significance are similar to those from women born in 1941-1856.
[14] The estimated parameters in these Generalized Gamma regressions suggest that we accept the Lognormal distribution over the Weibull distribution.

individual level. In addition, in all of the specifications, we assume an Inverse-Gaussian distribution (heterogeneity) of the observations instead of assuming homogeneity of the samples. The estimated coefficients presented here are the coefficients of the survival risk. All results are consistent with results from regressions using the Cox-proportional hazard assumption except results from the univariate regressions. We find that when the time trend and other covariates are not controlled for, women who were exposed to the crisis risk appeared to have had lower probability to marry compared to those who were not exposed to the crisis. When other factors are controlled for, the estimates change sign, indicating a significantly higher probability to marry. The estimated coefficients of the crisis-exposure dummy are statistically significant at 1 percent.

Results on first marital pregnancies

Figure 13-15 and Table 13-15 shows results from the analysis of females' first births after marriage, conditional on being married. Figures 13 presents nonparametric estimates of fertility hazard rates at different time duration (years) after marriage. Our data give similar results to data from the United States that age at first marriage had a causal effect on the occurrence of a short first births. (Marini and Hodsdon, 1981) These trends are also found in developing countries such as China. Feng and Quanhe (1996) found rising age at first marriage and shortening of the interval between marriage and first births to be two prominent features of China's demographic transition during the past two decades. Our results indicate that Indonesian women who married in 1986-2000 had higher probability of having first births in the first three years after marriage compared to those who married in the earlier period. Recall our results on ages at first marriage that marriage ages increased during the same period. Notice that the hazards of first birth of women who married later in life (the younger cohort) declined faster than that of those that married sooner. These results are consistent with results that suggest substantial female fertility decline in their late 20s. We observe that those young women who married later in life tended to have lower probability of having birth than those married sooner after three years of childlessness.

When looking at the effects of education on the timing of first births in Figure 13, we observe a significant difference in the probability of having first births among women with 0-5, 6-11, and 12+ years of education. Women with higher education levels exhibited a higher probability of having birth in the first three years after marriage. Evidence of the shortening of first birth intervals among higher-educated women is consistent with our earlier findings that women with higher education tended to marry later in life. We also observe that the probability of having first births declined substantially after two years of childlessness among women with the highest education (12+ years).

Figures 15 shows nonparametric fertility hazard estimates by residence. Our results indicate that urban residents had a higher probability of having first births in the first two years intervals than rural residents. The probability of having first births among urban residents declined substantially after marriage, resulting in a lower probability of having births if they stayed childlessness for at least three years after marriage. This result could be due to delays in marriage among urban residents or other unobserved conditions that are different between urban and rural settings such as quality of health facilities.

Results from parametric marriage hazard regressions using Cox-proportional hazard assumption are presented in Table 25. In these regressions, the standard errors of the estimated coefficients are robust to heteroskedasticity and clustering at the individual level. We find that married women who were exposed to the crisis exhibited lower hazard of having first birth. The estimated coefficients are significant at 1 percent in all specifications. The stimated coefficients of the time trend indicates that fertility declined over time. The estimated coefficients of years of education and education-level dummies indicate similar effects to the effects found in nonparametric estimations. Overall, women with higher education tended to have a higher probability to give births at different intervals after marriage than women with lower education, provided that higher-educated women married later in life.

Similar results are found in parametric hazard regressions assuming Lognormal and Weibull distributions of survival times. The estimated coefficients of survival risk using Lognormal distribution are presented in Table 26. The estimated coefficients of first birth hazard using Weibull distribution are present in Table 27. We find that having exposed to the crisis risk is associated with a lower probability of having first births. The estimates are significant at 1 percent in all specifications.

Unconditional Fertility Hazard Estimation

One should keep in mind that the analysis of first births in the previous section takes ages of marriage as exogenous. The estimation of the hazard rates of first birth is conditional on the women being married at the beginning of the time interval. Results from these conditional fertility hazard estimations could be biased when the marriage hazard was affected by the economic crisis. Our results show that the marriage hazard increased and the first birth hazard, conditional on being married, decreased as a result of the economic crisis. From these two sets of results, we cannot tell what the net crisis effect on first birth was. In principal, we could estimate the correlated hazards by defining a complicated likelihood function that estimate marriage and first birth hazards simultaneously. We did not carry out this estimation in this paper.

Instead, we estimated the unconditional first birth hazard rates of women starting at age 14 years regardless of whether the women were married. In this estimation, the dependent variable is the odds of a woman having her first child in year *t* given that she had no children at the start of year *t*. A similar procedure used in the marriage analysis is applied to constructing duration data and censoring process. The hypothesis being tested is the significance of the crisis dummy when the time trend is included.

Figure 16 compares the nonparametric unconditional fertility hazard rates, at same ages, of women born in 1941-1960 and 1961-1986. From the estimated results, we can see that the younger-cohort women were less likely to have their first births at younger ages and more likely to have their first births after age 25 years compared to the older-cohort women. Table 30 and 31 show estimates from the unconditional fertility hazard regressions using a Cox proportional hazard model and a Gamma distribution of survival time respectively. The estimated coefficients of the crisis dummy are negative[15] and statistically significant, indicating that being exposed to the economic crisis lowered the likelihood of having a first birth. This finding and our earlier finding from the conditional fertility hazard estimates suggest that the net effect of the economic crisis resulted in a delay of first births, even though there was a movement towards earlier marriages.

[15] positive in the Gamma distribution regressions because coefficients of the "survival" time are estimated.

Robustness

In our analyses, we identify the crisis effects through the estimated coefficients of the 1998 crisis exposure. In the case when birth years and marriage years do not fully capture the time-trend effects, the significance of the crisis exposure could be due to unobserved trend effects rather than the crisis effects. We ran a robustness check using year 1996 dummy instead of using the 1998 crisis dummy in the marriage analysis. Results of the parametric estimates using the Cox proportional hazard models are shown in Table 28. The estimated coefficients of the 1996 dummy are negative and not statistically significant, indicating that our earlier estimates are robust to this potential problem.

In addition, since the increase in the marriage rates and the delay in first births may not be only a short-run effect. We tested the effects of the economic crisis by extending the crisis period to the year 1999 to include a longer-run effect. Results of the parametric estimates using the Cox proportional hazard models are shown in Table 29. Similar to previous results using the 1998 crisis dummy, the 1998/1999-crisis dummy is positive and statistically significant in all specifications when the time trend is included, suggesting that the crisis effect may not be only short-run.

Conclusion

This paper investigates whether there was any short-run change in the ages of female first marriages and the timing of female first marital births as a result of the 1998 Indonesian crisis. The paper discusses channels to which an economic crisis could affect these demographic outcomes. The discussions of various theories lead to a conclusion that the effects of an economic crisis of female first marriage and ages of first births are ambiguous *a priori* due to opposing income and substitution effects of a decline in income and wages.

We found evidence that overall there was an increase in the probability of getting married and a decrease in the probability of having first births among Indonesian women during the crisis in both conditional and unconditional analyses. We argue that these findings support the hypothesis that marriages of individuals in the household and delaying having births may have been used as income-smoothing mechanisms in the time of the crisis. Results from this paper are not sufficient to draw any conclusion on why an increase in marriage probability and a delay in having first birth took place. We speculate that women are more likely to get married during the time of the crisis to take advantage of economies of scale and specialization in household consumption. The delays of first births might be due to consumption-smoothing consideration or other supply factors such as the separation of spouses when relocation of individuals occurred during the crisis.

In the case when insurance markets are absent or incomplete, household appeared to rely on alternative income and consumption smoothing mechanisms other than saving.

It is important to realize that even though changing behaviors of individuals in the household (such as delaying births and decreasing educational investment) is a reliable way to smooth consumption in the short run, the longer-term effects on welfare of such actions need to be considered in order to fully assess the effects of the economic crisis.

APPENDIX I

Table 1: Neonatal and Post-neonatal Mortality Rates Comparisons (per 1000 live births)
(Data from 1997 Demographic Household Survey of Indonesia and Indonesian Family Life Surveys)

Table 1a: DHS 1997 Publication

	1992-1997		1987-1992		1982-1987	
Years preceding	0-4		5-9		10-14	
	Neonatal	Post-neonatal	Neonatal	Post-neonatal	Neonatal	Post-neonatal
DHS1997	21.8	23.9	28.2	30.3	28.4	37.0

Table 1b: DHS Data (Replicated Numbers)

	1992-1997		1987-1992		1982-1987	
Years preceding 1997	0-4		5-9		10-14	
	Neonatal	Post-neonatal	Neonatal	Post-neonatal	Neonatal	Post-neonatal
Full sample (unweighted)	23.9	23.9	28.9	32.2	29	36.8
Full sample (weighted)	22.4	24.1	28.1	28.2	28.3	35.4
IFLS provinces (unweighted)	23.5	23.2	28.7	30.6	29.3	34.3
IFLS provinces (weighted)	21.9	23.6	27.5	27.1	28.0	34.7

Table 1c: IFLS Data

	1992-1997		1987-1992		1982-1987		1995-2000	
Years preceding 1997	0-4		5-9		10-14		***	
	Neonatal	Post-neonatal	Neonatal	Post-neonatal	Neonatal	Post-neonatal	Neonatal	Post-neonatal
IFLS1	---	---	25.3	28.2	29.7	36.4	---	---
IFLS1 and IFLS2	15.5	17.9	28.0	31.1	30.6	38.5	---	---
IFLS2	16.1	20.1	---	---	---	---	---	---
IFLS2 and IFLS3	---	---	---	---	---	---	16.3	20.1
IFLS3	---	---	---	---	---	---	20.0	23.9

Table 2: Proportion of births that have reported birthweight by birth location (%)

Birth Location	Year Born			
	1988-1992	1993-1997	1998-2000	Average
Own house/family members house	38.4	55.7	64.6	51.6
Office/house of traditional midwife	41.7	45.8	54.6	46.2
Clinic of Physician/ Clinic or office of midwife	97.8	97.3	99.4	98.1
Community health center/village delivery post	98.1	98.6	97.0	98.0
Public/private/delivery hospital	99.7	99.1	99.5	99.4
Others	47.8	71.4	87.5	55.1

Source: IFLS1, IFLS2, and IFLS3

Table 3: Birth Location (%)

Birth Location	Year Born			
	1988-1992	1993-1997	1998-2000	Average
Own house/family members house	63.2	55.3	49.3	56.2
Office/house of traditional midwife	1.3	2.0	1.0	1.5
Clinic of Physician/ Clinic or office of midwife	17.1	23.0	28.6	22.6
Community health center/village delivery post	3.9	4.0	4.4	4.1
Public/private/delivery hospital	11.9	15.3	16.5	14.5
Others	2.5	0.4	0.4	1.1
Number of obs. with reported birth location	2682	3535	2275	8492

Source: IFLS1, IFLS2, and IFLS3

Table 4. Birth Location Comparison (%)

Birth Location	Year Born						
	Noncrisis			Financial Crisis	Drought/smoke Crisis	Non-crisis	
	1995	1996	1997			1999	2000
Own house/family members house	55.4	52.5	48.4	50.6	50.4	48.8	47.7
Office/house of traditional midwife	1.8	2.2	1.2	1.0	2.2	1.6	0.6
Clinic of Physician/ Clinic or office of midwife	22.8	24.0	27.6	27.5	27.1	28.4	30.2
Community health center/village delivery post	3.6	4.0	4.0	3.9	3.2	5.1	4.5
Public/private/delivery hospital	16.1	17.2	17.2	16.8	16.9	15.6	16.6
Others	0.3	0.2	1.6	0.2	0.2	0.6	0.4
Number of Obs.	781	834	250	1033	498	514	728

Source: IFLS1, IFLS2, and IFLS3

Table 5: Mortality Rate (per 1000 live births)

	Neonatal mortality	Post-neonatal mortality	Infant mortality	Obs
Born during financial crisis	**23.1**	**23.1**	**46.2**	**1040**
rural	25.4	29.0	54.4	552
urban	20.5	16.4	36.9	488
Born during 97 drought period (rural)	**29.0**	**47.1**	**76.1**	**276**
Born during non-crisis period				
1988-1992	**28.2**	**33.0**	**61.2**	**4117**
rural	30.6	38.7	69.3	2324
urban	25.1	25.7	50.8	1793
1993-1997, 1999	**13.3**	**19.2**	**32.5**	**3542**
rural	15.7	25.8	41.5	1976
urban	10.2	10.9	21.1	1566

financial crisis = January 1998 to June 1999
drought97 = May 1997 to December 1997
non-crisis = January 1988 to April 1997 and July 1999 to December 2000
Obs include children that had a chance to live >=356 days by the interview date.

Table 6: Birthweight Statistics

	Mean Birth Weight (Kg.)	% of Low Birthweight(<2.5 kg)	Total Obs
Born during financial crisis	**3.13**	**8.7**	**841**
rural	3.17	6.9	390
urban	3.11	10.2	451
Born during 97 drought period	**3.16**	**6.7**	**386**
rural	3.15	8.1	186
urban	3.17	5.5	200
Born during non-crisis period			
1988-1992	**3.15**	**9.4**	**1568**
rural	3.11	11.1	624
urban	3.17	8.3	944
1993-1997, 1999	**3.18**	**8.1**	**2635**
rural	3.19	8.4	1245
urban	3.17	7.7	1390

financial crisis = January 1998 to June 1999
drought97 = May 1997 to December 1997
non-crisis = January 1988 to April 1997 and July 1999 to December 2000

Table 7: Mother's Education

Year	Mean (years)	No edu (%)	1-5 years (%)	6-8 years (%)	9-11 years (%)	12+ years (%)	Obs
1988	5.0	19.2	33.9	26.2	7.9	12.9	776
1989	5.5	15.2	32.8	26.9	9.6	15.6	845
1990	5.6	15.9	31.4	26.5	10.7	15.5	867
1991	5.8	14.2	27.2	30.1	12.1	16.5	832
1992	5.9	14.5	26.6	29.6	12.3	17.1	791
1993	6.1	11.7	27.8	30.0	13.4	17.2	746
1994	6.6	11.0	23.4	30.6	14.5	20.5	689
1995	7.2	9.5	28.0	32.1	14.0	26.5	794
1996	7.3	9.1	19.0	30.5	15.3	26.1	836
1997	7.6	8.1	15.9	30.2	17.7	28.1	718
1998	7.7	5.2	18.1	31.6	17.5	27.7	653
1999	8.2	4.9	14.8	28.2	20.0	32.1	614

Obs include mothers of children that had a chance to live >=356 days by the interview date.

Table 8: Child Mortality Rates (per 1,000 live births) by Mother's Education

	Neonatal Mortality	Post-neonatal Mortality	Infant Mortality	Obs
No edu	34.1	40.6	74.7	1084
1-5 years	19.6	48.2	67.8	2241
6-8 years	23.5	20.9	44.4	2685
9-11 years	22.7	17.8	40.5	1236
12+ years	12.5	6.3	18.8	1915

Obs include children that had a chance to live >=356 days by the interview date.
Sample: 1988-1999

Table 9: Birthweight First and Second Order Stochastic Dominance

	First crossing point and difference between curves (first and second order stochastic dominance)											
	Urban				Rural							
	Financial Crisis - Noncrisis				Financial Crisis - Noncrisis				Drought Crisis - Noncrisis			
	s = 1	(sd)	s = 2	(sd)	s = 1	(sd)	s = 2	(sd)	s = 1	(sd)	s = 2	(sd)
First crossing point	4.499	(0.106)	-	-	3.310	(0.143)	-	-	2.100	0.683	-	-
Points of testing (Kg.)												
1.5	4.01	(5.08)	0.00	(0.00)	-6.13	(1.69)	0.00	(0.00)	-6.13	(1.69)	0.00	(0.00)
1.6	9.92	(6.36)	0.56	(0.53)	-8.48	(1.99)	-0.61	(0.17)	-8.48	(1.99)	-0.61	(0.17)
1.7	8.40	(6.40)	1.55	(1.12)	-7.78	(3.39)	-1.46	(0.35)	-10.37	(2.19)	-1.46	(0.35)
1.8	17.23	(8.65)	2.39	(1.75)	-11.08	(3.67)	-2.32	(0.61)	-2.80	(8.08)	-2.31	(0.62)
1.9	17.18	(8.94)	4.11	(2.45)	-12.02	(3.72)	-3.43	(0.94)	-3.74	(8.10)	-2.59	(1.25)
2.0	29.09	(11.50)	5.92	(3.23)	-24.99	(8.57)	-4.63	(1.29)	-13.10	(15.75)	-2.96	(2.02)
2.1	22.79	(12.42)	8.63	(4.11)	-29.94	(10.00)	-7.25	(1.85)	1.35	(19.56)	-3.87	(3.13)
2.2	29.24	(13.95)	10.95	(5.12)	-33.00	(11.12)	-10.27	(2.65)	-9.49	(19.68)	-3.76	(4.72)
2.3	25.87	(15.23)	13.68	(6.24)	-32.30	(12.06)	-13.62	(3.58)	-11.13	(20.41)	-4.75	(6.51)
2.4	24.45	(15.98)	16.26	(7.49)	-19.59	(13.54)	-16.85	(4.60)	-8.52	(21.06)	-5.86	(8.39)
2.5	22.52	(18.55)	18.67	(8.83)	-17.74	(19.45)	-18.83	(5.72)	-19.17	(27.31)	-6.74	(10.34)
2.6	20.50	(19.38)	20.91	(10.22)	-23.87	(20.01)	-20.62	(7.03)	-24.80	(28.17)	-8.67	(12.36)
2.7	39.61	(21.31)	22.97	(11.72)	-21.06	(21.54)	-23.00	(8.58)	-20.46	(30.32)	-11.15	(14.61)
2.8	41.09	(23.05)	27.29	(13.25)	-14.95	(23.41)	-25.51	(10.26)	-6.64	(33.08)	-13.33	(16.98)
2.9	65.61	(24.17)	31.26	(14.87)	-18.26	(24.06)	-27.03	(12.06)	-8.94	(34.12)	-13.74	(19.45)
3.0	48.33	(25.32)	37.77	(16.52)	-23.77	(27.16)	-28.94	(13.97)	35.56	(38.20)	-14.50	(22.10)
# observations	Financial Crisis:		450		Financial Crisis:		386		Financial Crisis:		184	
	Noncrisis:		2634		Noncrisis:		2122		Noncrisis:		2122	

Source: IFLS I, IFLS2, and IFLS3.
Note:
Standard errors reflect clustering at the mother level.
S=1 is the difference in cummulative distribution for the first order stochastice dominance (grams).
S=2 is the difference in the cummulative distribution for the second order stochastice dominance (grams).
Observations include birthweight betweeen 1.5 kilograms and 5.0 kilograms.
Financial Crisis = January 1998 - June 1999
Drougt Crisis = May 1997 - December 1997
Noncrisis = January 1988 - April 1997 and July 1999 - December 2000
Dash (-) indicates that the curves do not cross.
Formulation for the standard deviation is from Russel Davidson and Jean-Yves Duclos (2000), Statistical Inference for Stochastic Dominance and for the Measurement of Poverty and Inequality," Economometrica v86 n6. Computing for the table above was performed using "DAD: A software for Distributative Analysis/Analyse Distributative." copyright by Jean-Yves Duclos, Abdelkrim Araar, and Carl Fortin.

Table 10: Birthweight Regressions

Birthweight (Kg.)	Urban				Rural			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
OLS	weight	weight	weight	weight	weight	weight	weight	weight
t	-0.002	-0.001	-0.001	-0.001	0.005	0.005	0.006	0.005
	(0.44)	(0.33)	(0.25)	(0.20)	(1.11)	(1.14)	(1.29)	(1.06)
preg crisis1	-0.043	-0.043	-0.049	-0.025	0.000	-0.003	-0.002	0.009
	(0.97)	(0.98)	(1.11)	(0.52)	(0.01)	(0.06)	(0.04)	(0.17)
preg crisis2	0.032	0.031	0.028	0.046	-0.005	-0.012	-0.015	-0.013
	(0.85)	(0.80)	(0.74)	(1.05)	(0.13)	(0.30)	(0.37)	(0.27)
preg_cs97	-0.064	-0.062	-0.061	-0.073	-0.011	-0.016	-0.023	0.011
	(1.46)	(1.41)	(1.40)	(1.46)	(0.27)	(0.38)	(0.57)	(0.22)
male	0.073	0.074	0.069	0.070	0.067	0.066	0.064	0.055
	(3.53)***	(3.57)***	(3.37)***	(3.04)***	(2.76)***	(2.76)***	(2.67)***	(2.10)**
Mother's edu								
1-5 yrs		-0.094	-0.082	-0.088		0.103	0.064	0.017
		(1.26)	(1.14)	(1.12)		(1.64)	(1.00)	(0.24)
6-8 yrs		-0.129	-0.107	-0.117		0.049	0.019	-0.037
		(1.74)*	(1.49)	(1.53)		(0.84)	(0.33)	(0.55)
9-11 yrs		-0.106	-0.085	-0.128		0.118	0.088	0.022
		(1.41)	(1.19)	(1.65)		(1.89)*	(1.39)	(0.30)
12+ yrs		-0.103	-0.076	-0.102		0.056	0.012	-0.051
		(1.42)	(1.10)	(1.34)		(0.93)	(0.20)	(0.71)
Constant	6.333	5.712	5.213	4.947	-6.818	-7.159	-7.936	-7.729
	(0.86)	(0.76)	(0.70)	(0.59)	(0.76)	(0.80)	(0.90)	(0.75)
Province dummies	No	No	Yes	No	No	No	Yes	No
Community dummies	No	No	No	Yes	No	No	No	Yes
Observations	3293	3293	3293	3293	2701	2701	2701	2701
R-squared	0.010	0.010	0.030	0.200	0.000	0.010	0.030	0.220
p-value(crises)	0.196	0.222	0.207	0.192	0.994	0.981	0.945	0.973
p-value(mother's edu)		0.510	0.629	0.517		0.192	0.256	0.450

Absolute values of t-statistics robust to heteroskedasticity and clustering at mother level in parentheses.
* significant at 10%;** significant at 5%; *** significant at 1%
Observations include all livebirths born 1998-2000.
preg_cs1 = Pregnant in January 1998 - September 1998
preg_cs2 = Pregnant in October 1998 - June 1999
preg_cs97 = Pregnant in May 1997 - December 1997
"No-education" and "North Sumatra" are omitted categories.

Table 11: Mortality Linear Probability Regressions (Urban)

LPM	(1) Neonatal	(2) Neonatal	(3) Neonatal	(4) Neonatal	(1) Post-Neonatal	(2) Post-Neonatal	(3) Post-Neonatal	(4) Post-Neonatal	(1) Infant	(2) Infant	(3) Infant	(4) Infant
t (x100)	-0.261	-0.235	-0.228	-0.756	-0.252	-0.174	-0.174	-0.751	-0.482	-0.379	-0.360	-0.855
	(3.63)***	(3.25)***	(3.20)***	(2.56)**	(2.98)***	(2.15)**	(2.01)**	(2.06)**	(4.40)***	(3.55)***	(3.37)***	(3.08)***
preg crisis1 (x100)	1.769	1.757	1.640	5.011								
	(1.91)*	(1.90)*	(1.76)*	(1.50)								
preg crisis2 (x100)	0.828	0.802	0.913	3.488								
	(0.85)	(0.82)	(0.94)	(0.63)								
preg_cs97 (x100)	2.119	2.135	2.050	7.071								
	(1.87)*	(1.88)*	(1.86)*	(1.28)								
born crisis1 (x100)					1.781	1.759	1.757	3.385	3.502	3.492	3.240	5.356
					(1.61)	(1.60)	(1.47)	(0.68)	(2.33)**	(2.33)**	(2.18)**	(1.34)
born crisis2 (x100)					0.817	0.815	0.575	0.231	2.366	2.350	1.976	3.419
					(1.00)	(1.00)	(0.64)	(0.06)	(1.93)*	(1.92)*	(1.59)	(1.04)
born 97 crisis (x100)					-0.241	-0.269	-0.249	-0.962	-0.430	-0.468	-0.526	-0.351
					(0.39)	(0.43)	(0.37)	(0.26)	(0.40)	(0.43)	(0.47)	(0.10)
male (x100)	0.435	0.434	0.463	1.119	0.512	0.512	0.563	1.285	0.940	0.939	0.986	1.462
	(1.05)	(1.05)	(1.12)	(0.69)	(1.15)	(1.16)	(1.18)	(0.68)	(1.56)	(1.57)	(1.63)	(0.99)
Mother's edu (x100)												
1-5 yrs		-0.946	-0.498	-2.709		-0.283	0.150	3.328		-1.173	-0.186	0.177
		(0.68)	(0.39)	(0.75)		(0.15)	(0.08)	(0.80)		(0.54)	(0.09)	(0.05)
6-8 yrs		-1.666	-1.139	-3.830		-2.461	-2.118	-3.036		-4.022	-2.919	-4.552
		(1.26)	(0.94)	(1.10)		(1.48)	(1.25)	(0.83)		(1.99)**	(1.60)	(1.50)
9-11 yrs		-0.999	-0.622	-1.862		-2.824	-2.426	-3.889		-3.715	-2.691	-3.690
		(0.72)	(0.49)	(0.48)		(1.67)*	(1.42)	(0.95)		(1.80)*	(1.45)	(1.08)
12+ yrs		-1.936	-1.605	-6.404		-3.537	-3.167	-8.194		-5.340	-4.330	-8.314
		(1.48)	(1.37)	(1.75)*		(2.16)**	(1.91)*	(2.11)**		(2.71)***	(2.46)**	(2.59)***
Constant	5.224	4.717	4.557	15.163	5.043	3.499	3.512	15.062	9.633	7.621	7.213	17.154
	(3.63)***	(3.27)***	(3.21)***	(2.58)**	(2.99)***	(2.18)**	(2.03)**	(2.07)**	(4.41)***	(3.58)***	(3.38)***	(3.10)***
Province dummies	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Community dummies	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Observations	4032	4032	4032	997	3961	3961	3731	890	4032	4032	4032	1586
R-squared	0.000	0.010	0.010	0.070	0.000	0.010	0.020	0.070	0.010	0.010	0.020	0.080
p-value(crises)	0.091	0.093	0.104	0.310	0.253	0.249	0.382	0.895	0.031	0.031	0.062	0.468
p-value(mother's edu)		0.253	0.262	0.247		0.000	0.001	0.003		0.000	0.000	0.001

Absolute values of t-statistics robust to heteroskedasticity and clustering at mother level in parentheses.
* significant at 10%;** significant at 5%; *** significant at 1%
Observations include all individuals that were born 1988 - 2000 who were exposed to at least 365 days of life.
crisis1 = Jan 98 - Sept 98, crisis2 = Oct 98 - June 99, crisis97 = May 97 - Dec 97
"No-education" and "North Sumatra" are omitted categories.

Table 12: Mortality Logistic Regressions (Urban)

Logit	(1) Neonatal	(2) Neonatal	(3) Neonatal	(4) Neonatal	(1) Post-Neonatal	(2) Post-Neonatal	(3) Post-Neonatal	(4) Post-Neonatal	(1) Infant	(2) Infant	(3) Infant	(4) Infant
t	0.851	0.863	0.867	0.875	0.866	0.903	0.907	0.894	0.864	0.889	0.893	0.889
	(3.81)***	(3.38)***	(3.31)***	(2.44)**	(2.99)***	(2.06)**	(1.93)*	(2.08)**	(4.41)***	(3.47)***	(3.29)***	(3.06)***
preg crisis1	3.262	3.271	3.006	2.542								
	(2.09)**	(2.09)**	(1.91)*	(1.36)								
preg crisis2	1.586	1.578	1.648	1.994								
	(0.43)	(0.43)	(0.47)	(0.60)								
preg_cs97	3.761	3.809	3.649	3.142								
	(2.40)**	(2.41)**	(2.37)**	(1.65)*								
born crisis1					2.901	2.931	2.706	1.891	3.018	3.048	2.823	2.119
					(1.86)*	(1.86)*	(1.66)*	(0.85)	(2.73)***	(2.73)***	(2.52)**	(1.49)
born crisis2					1.596	1.654	1.396	1.030	2.211	2.246	1.971	1.659
					(0.67)	(0.72)	(0.46)	(0.04)	(1.78)*	(1.81)*	(1.47)	(0.97)
born 97 crisis					0.540	0.538	0.547	0.740	0.559	0.557	0.531	0.802
					(0.59)	(0.59)	(0.57)	(0.28)	(0.57)	(0.57)	(0.61)	(0.29)
male	1.289	1.283	1.312	1.198	1.333	1.321	1.337	1.130	1.317	1.309	1.336	1.182
	(1.04)	(1.02)	(1.11)	(0.68)	(1.12)	(1.08)	(1.10)	(0.47)	(1.53)	(1.49)	(1.57)	(0.89)
Mother's edu												
1-5 yrs		0.708	0.898	0.652		0.928	1.153	1.435		0.831	1.050	1.039
		(0.74)	(0.25)	(0.80)		(0.16)	(0.33)	(0.77)		(0.56)	(0.15)	(0.11)
6-8 yrs		0.484	0.635	0.520		0.413	0.532	0.672		0.437	0.572	0.599
		(1.54)	(0.99)	(1.20)		(1.92)*	(1.41)	(0.81)		(2.46)**	(1.72)*	(1.38)
9-11 yrs		0.709	0.848	0.731		0.322	0.428	0.582		0.482	0.613	0.680
		(0.70)	(0.36)	(0.55)		(2.06)**	(1.60)	(0.96)		(2.01)**	(1.41)	(0.96)
12+ yrs		0.384	0.447	0.324		0.135	0.181	0.252		0.238	0.298	0.308
		(1.97)**	(1.73)*	(1.84)*		(3.38)***	(2.90)***	(2.01)**		(3.90)***	(3.39)***	(2.60)***
Province dummies	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Conditional Logit	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Observations	4032	4032	4032	997	3961	3961	3731	890	4032	4032	4032	1586
Log-Likelihood	-350.07	-346.91	-332.57	-172.35	-352.15	-336.47	-324.11	-167.50	-603.55	-588.41	-574.21	-326.27
Pseudo R-Squared	0.020	0.029	0.069	0.039	0.022	0.065	0.088	0.067	0.023	0.048	0.071	0.048
p-value(crises)	0.054	0.054	0.070	0.333	0.227	0.224	0.310	0.824	0.028	0.027	0.053	0.415
p-value(mother's edu)		0.206	0.281	0.311		0.000	0.001	0.021		0.000	0.000	0.011

Reported coefficients are partial odd ratios.
Absolute values of t-statistics robust to heteroskedasticity and clustering at mother level in parentheses.
* significant at 10%;** significant at 5%; *** significant at 1%
Observations include all individuals that were born 1988 - 2000 who were exposed to at least 365 days of life.
crisis1 = Jan 98 - Sept 98, crisis2 = Oct 98 - June 99, crisis97 = May 97 - Dec 97
"No-education" and "North Sumatra" are omitted categories.

Table 13: Mortality Linear Probability Regressions (Rural)

LPM	(1) Neonatal	(2) Neonatal	(3) Neonatal	(4) Neonatal	(1) Post-Neonatal	(2) Post-Neonatal	(3) Post-Neonatal	(4) Post-Neonatal	(1) Infant	(2) Infant	(3) Infant	(4) Infant
t (x100)	-0.172	-0.174	-0.176	-0.425	-0.265	-0.166	-0.176	-0.329	-0.468	-0.371	-0.376	-0.581
	(2.33)**	(2.37)**	(2.40)**	(2.54)**	(2.78)***	(1.72)*	(1.79)*	(1.92)*	(4.01)***	(3.15)***	(3.22)***	(3.57)***
preg crisis1 (x100)	2.187	2.183	2.184	5.448								
	(1.76)*	(1.75)*	(1.76)*	(1.83)*								
preg_cs97 (x100)	0.226	0.232	0.317	-1.210								
	(0.25)	(0.25)	(0.35)	(0.68)								
born crisis1 (x100)					1.676	1.555	1.848	2.836	2.059	1.941	2.224	1.974
					(1.34)	(1.24)	(1.43)	(1.29)	(1.39)	(1.31)	(1.51)	(0.97)
born crisis2 (x100)					0.906	0.857	0.788	0.942	3.352	3.307	3.223	4.953
					(0.82)	(0.78)	(0.70)	(0.41)	(2.06)**	(2.03)**	(2.00)**	(1.96)**
born 97 crisis (x100)					2.747	2.848	3.109	6.345	4.206	4.277	4.434	7.707
					(1.99)**	(2.07)**	(2.22)**	(2.39)**	(2.39)**	(2.43)**	(2.53)**	(2.92)***
male (x100)	1.038	1.035	1.006	2.055	1.419	1.470	1.531	2.218	2.389	2.431	2.407	2.930
	(2.38)**	(2.36)**	(2.30)**	(2.20)**	(2.64)***	(2.73)***	(2.77)***	(2.33)**	(3.54)***	(3.60)***	(3.57)***	(3.20)***
Mother's edu (x100)												
1-5 yrs		-1.607	-1.838	-4.375		1.057	1.332	2.357		-0.486	-0.424	-0.572
		(1.73)*	(1.91)*	(2.29)**		(1.11)	(1.36)	(1.54)		(0.37)	(0.32)	(0.35)
6-8 yrs		-0.380	-0.477	-1.129		-1.737	-1.352	-2.215		-2.048	-1.754	-2.208
		(0.39)	(0.49)	(0.54)		(2.06)**	(1.53)	(1.51)		(1.64)	(1.38)	(1.30)
9-11 yrs		-0.680	-0.804	-0.372		-1.630	-1.111	-1.256		-2.233	-1.822	-0.852
		(0.62)	(0.72)	(0.15)		(1.58)	(1.01)	(0.59)		(1.48)	(1.19)	(0.38)
12+ yrs		-1.585	-1.726	-2.305		-3.242	-2.719	-4.660		-4.709	-4.207	-4.434
		(1.53)	(1.58)	(0.97)		(3.89)***	(3.03)***	(2.19)**		(3.60)***	(3.08)***	(2.08)**
Constant	3.448	3.485	3.529	8.537	5.314	3.333	3.517	6.598	9.364	7.440	7.546	11.640
	(2.35)**	(2.38)**	(2.42)**	(2.56)**	(2.79)***	(1.74)*	(1.80)*	(1.94)*	(4.03)***	(3.17)***	(3.24)***	(3.60)***
Province dummies	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Community dummies	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Observations	5100	5100	5100	2241	4978	4978	4844	2738	5100	5100	5100	3619
R-squared	0.000	0.000	0.010	0.070	0.000	0.010	0.020	0.070	0.010	0.010	0.020	0.070
p-value(crises)	0.213	0.215	0.212	0.128	0.152	0.146	0.096	0.085	0.025	0.026	0.019	0.009
p-value(mother's edu)		0.139	0.102	0.022		0.000	0.000	0.001		0.000	0.003	0.167

Absolute values of t-statistics robust to heteroskedasticity and clustering at mother level in parentheses.
* significant at 10%;** significant at 5%; *** significant at 1%
Observations include all individuals that were born 1988 - 2000 who were exposed to at least 365 days of life.
crisis1 = Jan 98 - Sept 98, crisis2 = Oct 98 - June 99, crisis97 = May 97 - Dec 97
"No-education" and "North Sumatra" are omitted categories.

Table 14: Mortality Logistic Regressions (Rural)

Logit	(1) Neonatal	(2) Neonatal	(3) Neonatal	(4) Neonatal	(1) Post-Neonatal	(2) Post-Neonatal	(3) Post-Neonatal	(4) Post-Neonatal	(1) Infant	(2) Infant	(3) Infant	(4) Infant
t	0.926	0.926	0.925	0.912	0.918	0.945	0.942	0.937	0.911	0.927	0.926	0.913
	(2.37)**	(2.37)**	(2.40)**	(2.46)**	(2.77)***	(1.80)*	(1.89)*	(1.92)*	(3.99)***	(3.18)***	(3.26)***	(3.43)***
preg crisis1	2.457	2.460	2.462	2.654								
	(2.18)**	(2.18)**	(2.19)**	(2.13)**								
preg_cs97	1.082	1.078	1.125	0.580								
	(0.14)	(0.14)	(0.21)	(0.71)								
born crisis1					1.767	1.732	1.872	1.613	1.548	1.522	1.608	1.274
					(1.43)	(1.37)	(1.55)	(1.04)	(1.34)	(1.28)	(1.45)	(0.63)
born crisis2					1.349	1.351	1.287	1.176	1.996	1.997	1.968	2.132
					(0.67)	(0.67)	(0.56)	(0.33)	(2.21)**	(2.20)**	(2.15)**	(2.21)**
born 97 crisis					2.269	2.372	2.567	2.886	2.218	2.261	2.353	2.924
					(2.44)**	(2.55)**	(2.78)***	(2.89)***	(2.87)***	(2.91)***	(3.04)***	(3.69)***
male	1.580	1.577	1.565	1.534	1.563	1.593	1.605	1.511	1.581	1.597	1.597	1.540
	(2.41)**	(2.38)**	(2.34)**	(2.09)**	(2.61)***	(2.71)***	(2.73)***	(2.41)**	(3.54)***	(3.60)***	(3.58)***	(3.25)***
Mother's edu												
1-5 yrs		0.510	0.459	0.404		1.275	1.393	1.498		0.934	0.945	0.953
		(1.95)*	(2.18)**	(2.93)***		(1.11)	(1.45)	(1.66)*		(0.35)	(0.28)	(0.25)
6-8 yrs		0.893	0.853	0.872		0.572	0.647	0.625		0.705	0.748	0.750
		(0.36)	(0.50)	(0.48)		(2.19)**	(1.65)*	(1.61)		(1.72)*	(1.39)	(1.39)
9-11 yrs		0.796	0.749	1.013		0.595	0.705	0.803		0.674	0.728	0.930
		(0.58)	(0.71)	(0.03)		(1.49)	(0.98)	(0.57)		(1.43)	(1.14)	(0.26)
12+ yrs		0.494	0.457	0.660		0.204	0.258	0.348		0.321	0.354	0.449
		(1.48)	(1.54)	(0.80)		(3.26)***	(2.71)***	(1.99)**		(3.31)***	(2.92)***	(2.15)**
Province dummies	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Conditional Logit	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Observations	5100	5100	5100	2241	4978	4978	4844	2738	5100	5100	5100	3619
Log-Likelihood	-569.16	-564.43	-558.80	-333.74	-725.42	-708.04	-689.04	-453.80	-1097.35	-1086.87	-1068.60	-742.54
Pseudo R-Squared	0.012	0.020	0.030	0.039	0.012	0.036	0.056	0.039	0.014	0.024	0.040	0.025
p-value(crises)	0.088	0.088	0.088	0.057	0.085	0.072	0.036	0.034	0.015	0.014	0.011	0.002
p-value(mother's edu)		0.171	0.118	0.022		0.000	0.000	0.001		0.007	0.028	0.166

Reported coefficients are partial odd ratios.
Absolute values of t-statistics robust to heteroskedasticity and clustering at mother level in parentheses.
* significant at 10%;** significant at 5%; *** significant at 1%
Observations include all individuals that were born 1988 - 2000 who were exposed to at least 365 days of life.
crisis1 = Jan 98 - Sept 98, crisis2 = Oct 98 - June 99, crisis97 = May 97 - Dec 97
"No-education" and "North Sumatra" are omitted categories.

Table 15: Infant Mortality : Low Education and High Education

Urban	Mother's education = 0 - 5 years			Mother's education >= 6 years		
LPM	(1)	(2)	(3)	(1)	(2)	(3)
t (x100)	-0.887	-0.735	-1.693	-0.246	-0.234	-0.558
	(3.02)***	(2.51)**	(2.59)***	(2.29)**	(2.19)**	(1.80)*
born crisis1 (x100)	6.062	3.844	8.318	2.826	2.874	5.067
	(1.17)	(0.78)	(0.60)	(1.84)*	(1.86)*	(1.19)
born crisis2 (x100)	5.417	2.635	-0.881	1.472	1.332	3.329
	(1.17)	(0.50)	(0.12)	(1.22)	(1.10)	(0.91)
born cs97 (x100)	-2.142	-1.010	2.532	-0.277	-0.370	0.507
	(1.44)	(0.58)	(0.57)	(0.22)	(0.29)	(0.12)
male (x100)	-0.756	-0.557	-3.042	1.418	1.440	3.063
	(0.46)	(0.33)	(0.93)	(2.42)**	(2.46)**	(1.91)*
Constant	17.731	14.689	33.864	4.921	4.669	11.173
	(3.03)***	(2.51)**	(2.60)***	(2.29)**	(2.19)**	(1.81)*
Province dummies	No	Yes	No	No	Yes	No
Community dummies	No	No	Yes	No	No	Yes
Observations	955	955	489	3077	3077	1102
R-squared	0.010	0.040	0.214	0.000	0.010	0.119
p-value (crises)	0.024	0.493	0.872	0.207	0.145	0.600

Rural	Mother's education = 0 - 5 years			Mother's education >= 6 years		
LPM	(1)	(2)	(3)	(1)	(2)	(3)
t (x100)	-0.413	-0.401	-0.490	-0.377	-0.387	-0.756
	(2.12)**	(2.08)**	(1.98)**	(2.77)***	(2.88)***	(3.60)***
born crisis1 (x100)	2.798	2.586	1.747	1.579	2.043	2.248
	(0.92)	(0.85)	(0.46)	(0.98)	(1.26)	(0.96)
born crisis2 (x100)	3.418	3.045	1.452	3.238	3.025	7.262
	(1.10)	(1.00)	(0.43)	(1.70)*	(1.60)	(2.07)**
born cs97 (x100)	6.129	6.747	11.173	3.447	3.428	6.683
	(1.55)	(1.72)*	(1.87)*	(1.86)*	(1.86)*	(2.42)**
male (x100)	3.029	2.832	3.581	1.908	1.957	2.074
	(2.65)***	(2.49)**	(2.55)**	(2.44)**	(2.50)**	(1.77)*
Constant	8.280	8.038	9.819	7.554	7.732	15.127
	(2.13)**	(2.09)**	(2.00)**	(2.78)***	(2.88)***	(3.61)***
Province dummies	No	Yes	No	No	Yes	No
Community dummies	No	No	Yes	No	No	Yes
Observations	2352	2352	1861	2748	2748	1766
R-squared	0.010	0.020	0.110	0.010	0.020	0.145
p-value (crises)	0.272	0.367	0.290	0.140	0.128	0.027

Absolute values of t-statistics robust to heteroskedasticity and clustering at mother level in parentheses.
* significant at 10%;** significant at 5%; *** significant at 1%
Observations include all individuals that were born 1988 - 2000 who were exposed to at least 365 days of life.
crisis1 = Jan 98 - Sept 98, crisis2 = Oct 98 - June 99, crisis97 = May 97 - Dec 97
"North Sumatra" is omitted.

Table 16: Neonatal Mortality : Low Education and High Education

Urban	Mother's education = 0 - 5 years			Mother's education >= 6 years		
LPM	(1)	(2)	(3)	(1)	(2)	(3)
t (x100)	-0.224	-0.135	-0.536	-0.243	-0.243	-0.904
	(1.18)	(0.71)	(0.69)	(3.29)***	(3.35)***	(3.03)***
preg crisis1 (x100)	1.945	0.829	-7.923	1.732	1.712	7.192
	(0.59)	(0.24)	(1.16)	(1.86)*	(1.84)*	(1.79)*
preg crisis2 (x100)	-0.970	-1.076	-11.483	1.087	1.281	5.205
	(0.73)	(0.75)	(0.98)	(0.98)	(1.16)	(0.76)
preg_cs97 (x100)	2.438	2.012	-2.699	2.068	2.103	9.298
	(0.64)	(0.54)	(0.21)	(1.78)*	(1.85)*	(1.54)
male (x100)	0.430	0.602	2.889	0.432	0.429	1.062
	(0.44)	(0.62)	(0.85)	(0.98)	(0.97)	(0.55)
Constant	4.494	2.690	10.743	4.862	4.847	18.067
	(1.19)	(0.71)	(0.69)	(3.29)***	(3.35)***	(3.04)***
Province dummies	No	Yes	No	No	Yes	No
Community dummies	No	No	Yes	No	No	Yes
Observations	955	955	296	3077	3077	703
R-squared	0.000	0.020	0.207	0.000	0.010	0.107
p-value (crises)	0.509	0.671	0.655	0.095	0.066	0.149

Rural	Mother's education = 0 - 5 years			Mother's education >= 6 years		
LPM	(1)	(2)	(3)	(1)	(2)	(3)
t (x100)	-0.231	-0.237	-0.495	-0.095	-0.098	-0.444
	(1.94)*	(2.00)**	(1.96)*	(0.86)	(0.90)	(1.77)*
preg crisis1 (x100)	1.728	1.780	2.091	1.892	1.730	6.338
	(0.97)	(1.01)	(0.66)	(1.15)	(1.06)	(1.47)
preg crisis2 (x100)	-0.675	-0.522	0.427	-1.975	-2.109	-5.649
	(0.83)	(0.61)	(0.19)	(2.74)***	(2.74)***	(2.17)**
preg_cs97 (x100)	1.808	1.718	4.151	-0.862	-0.584	-3.543
	(0.91)	(0.85)	(1.02)	(0.82)	(0.56)	(1.97)**
male (x100)	1.258	1.261	2.425	0.873	0.878	1.659
	(1.91)*	(1.91)*	(1.86)*	(1.49)	(1.49)	(1.25)
Constant	4.611	4.738	9.886	1.917	1.956	8.908
	(1.95)*	(2.01)**	(1.96)**	(0.87)	(0.91)	(1.78)*
Province dummies	No	Yes	No	No	Yes	No
Community dummies	No	No	Yes	No	No	Yes
Observations	2352	2352	1119	2748	2748	1127
R-squared	0.000	0.010	0.104	0.000	0.010	0.159
p-value (crises)	0.298	0.400	0.678	0.002	0.004	0.018

Absolute values of t-statistics robust to heteroskedasticity and clustering at mother level in parentheses.
* significant at 10%;** significant at 5%; *** significant at 1%
Observations include all individuals that were born 1988 - 2000 who were exposed to at least 365 days of life.
crisis1 = Jan 98 - Sept 98, crisis2 = Oct 98 - June 99, crisis97 = May 97 - Dec 97
"North Sumatra" is omitted.

Table 17: Post-neonatal Mortality : Low Education and High Education

Urban	Mother's education = 0 - 5 years			Mother's education >= 6 years		
LPM	(1)	(2)	(3)	(1)	(2)	(3)
t (x100)	-0.709	-0.635	-2.113	-0.030	-0.023	-0.017
	(3.00)***	(2.69)***	(2.58)**	(0.38)	(0.29)	(0.04)
born crisis1 (x100)	4.076	2.414	17.717	1.130	1.154	-0.191
	(1.05)	(0.63)	(0.92)	(1.00)	(1.00)	(0.04)
born crisis2 (x100)	3.801	1.747	7.489	-0.063	-0.139	-2.357
	(1.10)	(0.43)	(0.79)	(0.08)	(0.18)	(0.55)
born cs97 (x100)	-0.596	0.400	1.598	-0.342	-0.342	-0.844
	(0.54)	(0.29)	(0.28)	(0.49)	(0.49)	(0.21)
male (x100)	-1.189	-1.165	-7.473	0.993	1.013	4.738
	(0.85)	(0.80)	(1.90)*	(2.64)***	(2.70)***	(2.55)**
Constant	14.169	12.709	42.254	0.596	0.456	0.365
	(3.00)***	(2.70)***	(2.59)**	(0.39)	(0.29)	(0.04)
Province dummies	No	Yes	No	No	Yes	No
Community dummies	No	No	Yes	No	No	Yes
Observations	930	930	304	3031	3031	589
R-squared	0.010	0.040	0.202	0.000	0.010	0.142
p-value (crises)	0.231	0.887	0.715	0.675	0.571	0.952

Rural	Mother's education = 0 - 5 years			Mother's education >= 6 years		
LPM	(1)	(2)	(3)	(1)	(2)	(3)
t (x100)	-0.157	-0.147	-0.208	-0.195	-0.200	-0.382
	(0.92)	(0.87)	(0.77)	(1.93)*	(2.00)**	(1.85)*
born crisis1 (x100)	1.090	1.102	-0.803	1.882	2.078	4.548
	(0.42)	(0.43)	(0.22)	(1.40)	(1.53)	(1.63)
born crisis2 (x100)	1.481	1.096	-0.171	0.693	0.637	1.969
	(0.53)	(0.40)	(0.04)	(0.67)	(0.62)	(0.71)
born cs97 (x100)	4.736	5.307	11.160	2.026	2.044	4.044
	(1.44)	(1.63)	(1.79)*	(1.50)	(1.51)	(1.57)
male (x100)	1.870	1.676	2.643	1.094	1.141	1.343
	(1.92)*	(1.74)*	(1.74)*	(1.98)**	(2.08)**	(1.18)
Constant	3.159	2.952	4.198	3.894	3.993	7.642
	(0.93)	(0.87)	(0.78)	(1.94)*	(1.99)**	(1.86)*
Province dummies	No	Yes	No	No	Yes	No
Community dummies	No	No	Yes	No	No	Yes
Observations	2298	2298	1462	2680	2680	1279
R-squared	0.000	0.020	0.093	0.000	0.020	0.134
p-value (crises)	0.513	0.626	0.326	0.283	0.212	0.189

Absolute values of t-statistics robust to heteroskedasticity and clustering at mother level in parentheses.
* significant at 10%;** significant at 5%; *** significant at 1%
Observations include all individuals that were born 1988 - 2000 who were exposed to at least 365 days of life.
crisis1 = Jan 98 - Sept 98, crisis2 = Oct 98 - June 99, crisis97 = May 97 - Dec 97
"North Sumatra" is omitted.

Table 18: Parametric Estimations of Mortality Hazard Rates (Urban)

Weibull Regressions	Log Relative-hazard Form	Log Relative-hazard Form with Inverse Gaussian Heterogeneity
	Hazard Ratio (Robust Standard Error)	Hazard Ratio (Robust Standard Error)
t	0.970	0.945
	(0.029)	(0.040)
born financial crisis	1.721	2.232
	(0.503)*	(0.958)*
born drought/smoke crisis	2.746	4.735
	(1.000)***	(2.674)***
mother's edu		
1-5 years	1.057	0.993
	(0.352)	(0.526)
6-8 years	0.719	0.549
	(0.250)	(0.289)
9-11 years	0.836	0.718
	(0.300)	(0.395)
12+ years	0.409	0.265
	(0.145)**	(0.140)**
male	1.399	1.834
	(0.252)*	(0.484)**
/ ln p	-0.852	-0.400
	(0.038)***	(0.042)***
Log Likelihood	-843.03	-833.57
Observations	3956	3956
Times at risk	117276	117276

Standard errors are robust to heteroskedasticity clustering at the mother level.

Province dummies are included in the estimations, but not reported.

Table 19: Parametric Estimations of Mortality Hazard Rates (Rural)

Weibull Regressions	Log Relative-hazard Form	Log Relative-hazard Form with Inverse Gaussian Heterogeneity
	Hazard Ratio (Robust Standard Error)	Hazard Ratio (Robust Standard Error)
t	1.017	1.013
	(0.020)	(0.029)
born financial crisis	1.105	1.159
	(0.225)	(0.363)
born drought/smoke crisis	1.621	2.144
	(0.411)*	(0.823)**
mother's edu		
1-5 years	0.744	0.596
	(0.123)*	(0.154)*
6-8 years	0.537	0.388
	(0.094)***	(0.104)***
9-11 years	0.539	0.395
	(0.130)**	(0.143)***
12+ years	0.275	0.152
	(0.082)***	(0.063)***
male	1.402	1.652
	(0.162)***	(0.287)***
/ ln p	-0.806	-0.355
	(0.027)***	(0.030)***
Log Likelihood	-1788.26	-1767.35
Observations	5033	5033
Times at risk	144635	144635

Standard errors are robust to heteroskedasticity clustering at the mother level.

Province dummies are included in the estimations, but not reported.

Table 20: Summary Statistics of Marriage Age by Birth Cohort

Birth Cohort	Median Marriage Age	Age Married (%)									
		11-14	15-18	19-22	23-25	26-30	31-35	36-45	46+	single	obs.
1900-1920	19	17.0	30.0	22.0	9.0	10.0	3.0	6.0	3.0	0.0	100
1921-1940	20	12.8	32.5	23.4	10.3	10.0	3.5	4.7	2.1	0.7	1365
1941-1960	19	11.6	35.0	26.5	11.7	7.6	3.3	1.9	0.3	2.2	3350
1961-1965	19	10.4	30.4	28.2	14.1	9.1	2.8	0.5	0.0	4.5	1380
1966-1970	20	6.9	26.1	28.3	15.5	14.7	1.5	---	---	7.1	1489
1971-1975	20	4.2	23.7	27.7	20.6	7.7	---	---	---	16.1	1632

Source: IFLS1, IFLS2, and IFLS3

Table 21: Marriage Age by Year of Marriage

Year of Marriage	Median Marriage	Marriage Age (%)								
		11-14	15-18	19-22	23-25	26-30	31-35	36-45	46+	obs.
1988	20	4.2	30.5	35.2	15.5	12.2	1.4	0.0	0.9	213
1992	21	3.6	28.3	31.1	19.5	11.6	2.4	2.4	1.2	251
1996	20	3.2	29.7	35.0	17.0	11.7	1.9	1.6	0.0	317
1997	21	1.9	27.2	32.3	20.7	15.0	1.6	0.8	0.5	368
1998	20	2.1	29.2	37.9	15.6	11.6	3.1	0.5	0.0	422
1999	21	0.7	28.6	37.0	19.5	12.3	1.2	0.5	0.2	416
2000	21	1.4	24.6	36.9	18.4	13.3	4.1	1.4	0.0	293

Table 22: Parametric Marriage Hazard Regressions (Cox Proportional Hazard)

	(1)	(2)	(3)	(4)	(5)	(6)
exposed to crisis risk	0.056	0.267	0.341	0.251	0.262	0.260
	(1.20)	(5.48)***	(6.79)***	(5.17)***	(5.39)***	(5.36)***
time trend (birth year)		-0.016		-0.001	-0.005	-0.005
		(15.75)***		(1.08)	(4.21)***	(4.59)***
born 1961-1965			-0.082			
			(2.52)**			
born 1966 -1970			-0.225			
			(7.48)***			
born 1971-1975			-0.353			
			(11.68)***			
born 1976 - 1980			-0.429			
			(12.65)***			
born 1981 -1986			-0.756			
			(13.40)***			
years of educ				-0.076		
				(31.11)***		
1-5 years					0.131	0.146
					(2.91)***	(3.14)***
6-8 years					0.053	0.086
					(1.25)	(1.95)*
9-11 years					-0.270	-0.187
					(5.84)***	(3.89)***
12+ years					-0.857	-0.744
					(20.65)***	(16.76)***
village						0.215
						(5.47)***
small town						0.010
						(0.24)
Province Dummies	No	No	No	No	No	Yes
Observations	11600	11600	11600	11465	11465	11465
Times at risk	105539	105539	105539	104321	104321	104321
Wald Chi (2)	1.4	249.2	322.5	1133.5	1897.6	2074.2
Prob > Chi (2)	0.231	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that were born between 1941 - 1986.

Entry begins at age 11 years old

"No Education", "Big city", and "North Sumatra" are omitted categories.

Table 23: Parametric Marriage Hazard Regressions (Gamma Distribution)
(survival coefficients)

	(1)	(2)	(3)	(4)	(5)	(6)
exposed to crisis risk	0.030	-0.037	-0.032	-0.024	-0.028	-0.032
	(2.81)***	(3.24)***	(2.78)***	(2.98)***	(3.50)***	(3.80)***
time trend (birth year)		0.005		0.001	0.002	0.002
		(21.50)***		(4.10)***	(8.59)***	(9.44)***
born 1961-1965			0.027			
			(3.17)***			
born 1966 -1970			0.077			
			(9.51)***			
born 1971-1975			0.116			
			(15.13)***			
born 1976 - 1980			0.129			
			(18.82)***			
born 1981 -1986			0.133			
			(16.10)***			
years of educ				0.028		
				(55.11)***		
1-5 years					-0.005	-0.006
					(0.61)	(0.69)
6-8 years					0.048	0.045
					(5.84)***	(5.59)***
9-11 years					0.137	0.122
					(16.04)***	(14.17)***
12+ years					0.311	0.286
					(38.10)***	(33.72)***
village						-0.043
						(5.69)***
small town						-0.015
						(1.92)*
constant	2.97	(6.06)	2.90	1.15	(0.51)	(0.78)
	(829.89)***	(14.43)***	(569.41)***	(2.94)***	(1.31)	(1.99)**
Province Dummies	No	No	No	No	No	Yes
Observations	11600	11600	11600	11465	11465	11465
Times at risk	105539	105539	105539	104321	104321	104321
Wald Chi (2)	7.9	468.7	542.0	3365.9	4107.1	4321.9
Prob > Chi (2)	0.005	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that were born between 1941 - 1986.

Entry begins at age 11 years old

"No Education", "Big city", and "North Sumatra" are omitted categories.

Table 24: Parametric Marriage Hazard Regressions (Lognormal Distribution)
Change sign

	(1)	(2)	(3)	(4)	(5)	(6)
exposed to crisis risk	0.029	-0.038	-0.032	-0.018	-0.021	-0.025
	(2.73)***	(3.28)***	(2.81)***	(2.06)**	(2.57)**	(2.90)***
time trend (birth year)		0.005		0.001	0.002	0.002
		(21.43)***		(4.22)***	(8.53)***	(9.57)***
born 1961-1965			0.026			
			(3.12)***			
born 1966 -1970			0.077			
			(9.40)***			
born 1971-1975			0.116			
			(15.11)***			
born 1976 - 1980			0.129			
			(18.69)***			
born 1981 -1986			0.132			
			(15.97)***			
years of educ				0.028		
				(54.05)***		
1-5 years					-0.004	-0.004
					(0.45)	(0.44)
6-8 years					0.050	0.049
					(6.02)***	(5.92)***
9-11 years					0.136	0.121
					(15.69)***	(13.96)***
12+ years					0.307	0.283
					(37.07)***	(33.22)***
village						-0.041
						(5.47)***
small town						-0.016
						(2.00)**
constant	2.97	(6.03)	2.90	1.11	(0.50)	(0.80)
	(762.24)***	(14.36)***	(522.52)***	(2.84)***	(1.29)	(2.07)**
Province Dummies	No	No	No	No	No	Yes
Observations	11600	11600	11600	11465	11465	11465
Times at risk	105539	105539	105539	104321	104321	104321
Wald Chi (2)	7.5	466.1	533.1	3277.9	4014.6	4254.4
Prob > Chi (2)	0.006	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that were born between 1941 - 1986.

Entry begins at age 11 years old

"No Education", "Big city", and "North Sumatra" are omitted categories.

Table 25: Parametric Fertility Hazard Regressions (Cox Proportional Hazard)

	(1)	(2)	(3)	(4)	(5)
exposed to crisis risk	-0.584	-0.578	-0.585	-0.584	-0.587
	(11.22)***	(10.75)***	(10.67)***	(10.66)***	(10.73)***
time trend (marriage year)		-0.001	-0.009	-0.010	-0.009
		(0.40)	(5.21)***	(5.34)***	(4.85)***
years of educ			0.04		
			(14.09)***		
1-5 years				0.219	0.205
				(4.51)***	(4.22)***
6-8 years				0.332	0.323
				(7.15)***	(6.92)***
9-11 years				0.447	0.415
				(9.10)***	(8.35)***
12+ years				0.533	0.480
				(11.31)***	(9.86)***
urban					0.080
					(3.58)***
Province Dummies	No	No	No	No	Yes
Observations	6555	6555	6383	6383	6383
Times at risk	10306	10306	10014	10014	10014
Wald Chi (2)	125.8	125.9	330.3	326.4	420.0
Prob > Chi (2)	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that married after 1979.

"No Education" and "North Sumatra" are omitted categories.

Table 26: Parametric Fertility Hazard Regressions (Lognormal Distribution)
(survival coefficients)

	(1)	(2)	(3)	(4)	(5)
exposed to crisis risk	0.105	0.140	0.141	0.143	0.146
	(7.39)***	(9.57)***	(7.95)***	(0.81)	(8.96)***
time trend (marriage year)		-0.005	-0.001	-0.001	-0.002
		(6.67)***	(2.17)**	(0.32)	(2.66)***
years of educ			(0.01)		
			(9.73)***		
1-5 years				-0.088	-0.082
				(0.95)	(3.63)***
6-8 years				-0.122	-0.119
				(0.84)	(5.24)***
9-11 years				-0.170	-0.158
				(0.83)	(6.74)***
12+ years				-0.203	-0.181
				(0.87)	(7.85)***
urban					-0.042
					(4.85)***
constant	0.177	9.215	3.182	3.102	4.015
	(28.23)***	(6.79)***	(2.37)**	(0.34)	(2.89)***
Province Dummies	No	No	No	No	Yes
Observations	6555	6555	6383	6383	6383
Times at risk	10306	10306	10014	10014	10014
Wald Chi (2)	54.7	110.1	113.4	13.1	232.0
Prob > Chi (2)	0.000	0.000	0.000	0.041	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that married after 1979.

"No Education" and "North Sumatra" are omitted categories.

Table 27: Parametric Fertility Hazard Regressions (Weibull Distribution)

	(1)	(2)	(3)	(4)	(5)
exposed to crisis risk	-0.616	-0.937	-0.943	-0.939	-0.943
	(8.71)***	(13.23)***	(12.97)***	(12.97)***	(13.01)***
time trend (marriage year)		0.042	0.021	0.020	0.021
		(10.23)***	(4.74)***	(4.35)***	(4.73)***
years of educ			0.09		
			(12.74)***		
1-5 years				0.607	0.575
				(4.26)***	(4.06)***
6-8 years				0.880	0.850
				(6.55)***	(6.27)***
9-11 years				1.167	1.087
				(8.52)***	(7.87)***
12+ years				1.332	1.203
				(9.82)***	(8.66)***
urban					0.183
					(3.03)***
constant	-0.832	-84.290	-42.704	-40.750	-44.215
	(68.95)***	(10.34)***	(4.92)***	(4.56)***	(4.94)***
Province Dummies	No	No	No	No	Yes
Observations	6555	6555	6383	6383	6383
Times at risk	10306	10306	10014	10014	10014
Wald Chi (2)	75.9	232.2	364.8	367.1	432.4
Prob > Chi (2)	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that married after 1979.

"No Education" and "North Sumatra" are omitted categories.

Table 28: Unconditional Fertility Hazard Regressions (Cox Proportional Hazard)

	(1)	(2)	(3)	(4)	(5)	(6)
exposed to crisis risk	-0.470	-0.443	-0.459	-0.430	-0.428	-0.423
	(18.97)***	(20.47)***	(21.36)***	(18.76)***	(18.68)***	(18.75)***
time trend (birth year)		-0.004		0.006	0.002	0.000
		(2.69)***		(3.88)***	(1.29)	(0.08)
born 1961-1965			-0.119			
			(2.69)***			
born 1966 -1970			-0.256			
			(6.52)***			
born 1971-1975			-0.341			
			(8.83)***			
born 1976 - 1980			-0.005			
			(0.11)			
born 1981 -1986			0.232			
			(2.81)***			
years of educ				-0.059		
				(18.47)***		
1-5 years					0.309	0.353
					(5.02)***	(5.70)***
6-8 years					0.280	0.334
					(4.75)***	(5.58)***
9-11 years					0.072	0.160
					(1.13)	(2.45)**
12+ years					-0.483	-0.369
					(8.46)***	(6.16)***
urban						-0.193
						(6.05)***
Province Dummies	No	No	No	No	No	Yes
Observations	16042	16042	16042	15717	15717	15717
Times at risk	113309	113309	113309	111347	111347	111347
Wald Chi (2)	359.9	419.1	791.6	864.6	1105.2	1201.5
Prob > Chi (2)	0.000	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that were born between 1941 - 1986.

Entry begins at age 14 years old

"No Education" and and "North Sumatra" are omitted categories.

Table 29: Unconditional Fertility Hazard Regressions (Gamma Distribution)
(survival coefficients)

	(1)	(2)	(3)	(4)	(5)	(6)
exposed to crisis risk	0.087	0.062	0.075	0.040	0.036	0.036
	(17.15)***	(14.09)***	(19.20)***	(11.20)***	(10.43)***	(10.37)***
time trend (birth year)		0.002		0.000	0.000	0.001
		(10.71)***		(2.00)**	(2.63)***	(4.08)***
born 1961-1965			0.032			
			(4.62)***			
born 1966 -1970			0.069			
			(9.07)***			
born 1971-1975			0.111			
			(15.89)***			
born 1976 - 1980			0.083			
			(15.16)***			
born 1981 -1986			0.023			
			(3.47)***			
years of educ				0.023		
				(39.65)***		
1-5 years					-0.010	-0.013
					(1.37)	(1.74)*
6-8 years					0.024	0.019
					(3.28)***	(2.44)**
9-11 years					0.079	0.068
					(10.04)***	(8.17)***
12+ years					0.222	0.206
					(28.75)***	(25.10)***
urban						0.02
						(5.78)***
constant	3.025	-1.103	2.969	3.605	1.988	1.427
	(847.87)***	(2.87)***	(640.41)***	(9.51)***	(5.54)***	(3.81)***
Province Dummies	No	No	No	No	No	Yes
Observations	16042	16042	16042	15717	15717	15717
Times at risk	113309	113309	113309	111347	111347	111347
Wald Chi (2)	294.1	284.0	986.0	1778.5	2258.4	2356.3
Prob > Chi (2)	0.000	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that were born between 1941 - 1986.

Entry begins at age 14 years old

"No Education" and and "North Sumatra" are omitted categories.

Table 30: Parametric Marriage Hazard Regressions (Cox Proportional Hazard)

	(1)	(2)	(3)	(4)	(5)	(6)
Exposed to 1996 Risk	-0.204	-0.040	-0.014	-0.047	-0.035	-0.041
	(3.76)***	(0.73)	(0.26)	(0.85)	(0.63)	(0.74)
time trend (birth year)		-0.014		0.000	-0.003	-0.004
		(14.57)***		(0.12)	(3.04)***	(3.45)***
born 1961-1965			-0.080			
			(2.45)**			
born 1966 -1970			-0.222			
			(7.35)***			
born 1971-1975			-0.330			
			(10.97)***			
born 1976 - 1980			-0.372			
			(11.09)***			
born 1981 -1986			-0.672			
			(12.34)***			
years of educ				-0.076		
				(31.15)***		
1-5 years					0.127	0.142
					(2.83)***	(3.05)***
6-8 years					0.048	0.081
					(1.13)	(1.83)*
9-11 years					-0.271	-0.188
					(5.85)***	(3.91)***
12+ years					-0.861	-0.748
					(20.75)***	(16.86)***
village						0.215
						(5.47)***
small town						0.010
						(0.24)
Province Dummies	No	No	No	No	No	Yes
Observations	11600	11600	11600	11465	11465	11465
Times at risk	105552	105552	105552	104334	104334	104334
Wald Chi (2)	14.2	228.4	292.4	1112.0	1860.0	2033.9
Prob > Chi (2)	0.000	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that were born between 1941 - 1946.

Entry begins at age 11 years old

"No Education", "Big city", and "North Sumatra" are omitted categories.

Table 31: Parametric Marriage Hazard Regressions (Cox Proportional Hazard)

	(1)	(2)	(3)	(4)	(5)	(6)
98/99 crisis risk	0.065	0.321	0.457	0.289	0.299	0.299
	(1.94)*	(8.69)***	(11.42)***	(7.77)***	(8.03)***	(8.05)***
time trend (birth year)		-0.017		-0.003	-0.006	-0.007
		(16.79)***		(2.39)**	(5.45)***	(5.80)***
born 1961-1965			-0.085			
			(2.63)***			
born 1966 -1970			-0.232			
			(7.72)***			
born 1971-1975			-0.386			
			(12.51)***			
born 1976 - 1980			-0.514			
			(14.35)***			
born 1981 -1986			-0.922			
			(15.40)***			
years of educ				-0.076		
				(30.98)***		
1-5 years					0.136	0.151
					(3.02)***	(3.25)***
6-8 years					0.059	0.093
					(1.39)	(2.10)**
9-11 years					-0.271	-0.187
					(5.86)***	(3.90)***
12+ years					(0.85)	(0.74)
					(20.47)***	(16.57)***
village						0.21
						(5.44)***
small town						0.010
						(0.23)
Province Dummies	No	No	No	No	No	Yes
Observations	11600	11600	11600	11465	11465	11465
Times at risk	105539	105539	105539	104321	104321	104321
Wald Chi (2)	3.8	285.5	388.3	1165.2	1933.5	2099.2
Prob > Chi (2)	0.052	0.000	0.000	0.000	0.000	0.000

Absolute value of z statistics in parentheses

Standard errors are robust to heteroskedasticity and clustering at individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

Observations include all individuals that were born between 1941 - 1986.

Entry begins at age 11 years old

"No Education", "Big city", and "North Sumatra" are omitted categories.

APPENDIX II

Appendix II: Mortality Crosswalk from the Indonesian Family Life Surveys (IFLS1-IFLS3)

ch00=1	panel respondent with no preprinted child roster
ch00=2	panel respondent with preprinted child roster
ch00=3	new respondent

	Number of Obs		
FILS2 and 3 (1995-2000)	Neonatal	Post-neonatal	Total live births
All obs	59	69	3093

	Number of Obs		
FILS3 (1995-2000)	Neonatal	Post-neonatal	Total live births
All obs	41	49	2034
ch00==2 or 3	29	36	1443
ch00==1	12	13	591

	Rates (per 1000 live births)	
FILS3 (1995-2000)	Neonatal	Post-neonatal
All obs	20.2	24.1
ch00==2 or 3	20.1	24.9
ch00==1	20.3	22.0

Number of Obs by Year

	IFLS3 (1995)			IFLS2 (1995)		
	Neonatal	Post-neonatal	Total live births	Neonatal	Post-neonatal	Total live births
All obs	3	5	214	12	10	582
ch00==2 or 3	3	4	200	8	5	296
ch00==1	0	1	14	4	5	286
	IFLS3 (1996)			IFLS2 (1996)		
	Neonatal	Post-neonatal	Total live births	Neonatal	Post-neonatal	Total live births
All obs	4	4	271	6	10	472
ch00==2 or 3	3	1	239	0	7	253
ch00==1	1	3	32	0	3	219
	IFLS3 (1997)					
	Neonatal	Post-neonatal	Total live births	Neonatal	Post-neonatal	Total live births
All obs	9	11	297	0	0	5
ch00==2 or 3	6	9	245	0	0	5
ch00==1	3	2	52	0	0	0
	IFLS3 (1998)			IFLS2 (1998)		
	Neonatal	Post-neonatal	Total live births	Neonatal	Post-neonatal	Total live births
All obs	15	16	656	NA	NA	NA
ch00==2 or 3	10	14	390	NA	NA	NA
ch00==1	5	2	266	NA	NA	NA
	IFLS3 (1999)			IFLS2 (1999)		
	Neonatal	Post-neonatal	Total live births	Neonatal	Post-neonatal	Total live births
All obs	10	13	596	NA	NA	NA
ch00==2 or 3	7	8	369	NA	NA	NA
ch00==1	3	5	227	NA	NA	NA

NOTE:
Too few obs to calculate and compare mortality rates for each year
No obs in born in 2000 that had lived 365 days by the interview date of IFLS3
Similar reason for IFLS2 data with those born in 1997
(5 obs of 1997-borns were interviewed in 1998. These have age>=365 day by the interview)

APPENDIX III

Figure 1: Timing of the IFLS and the Rp/USD Exchange Rate

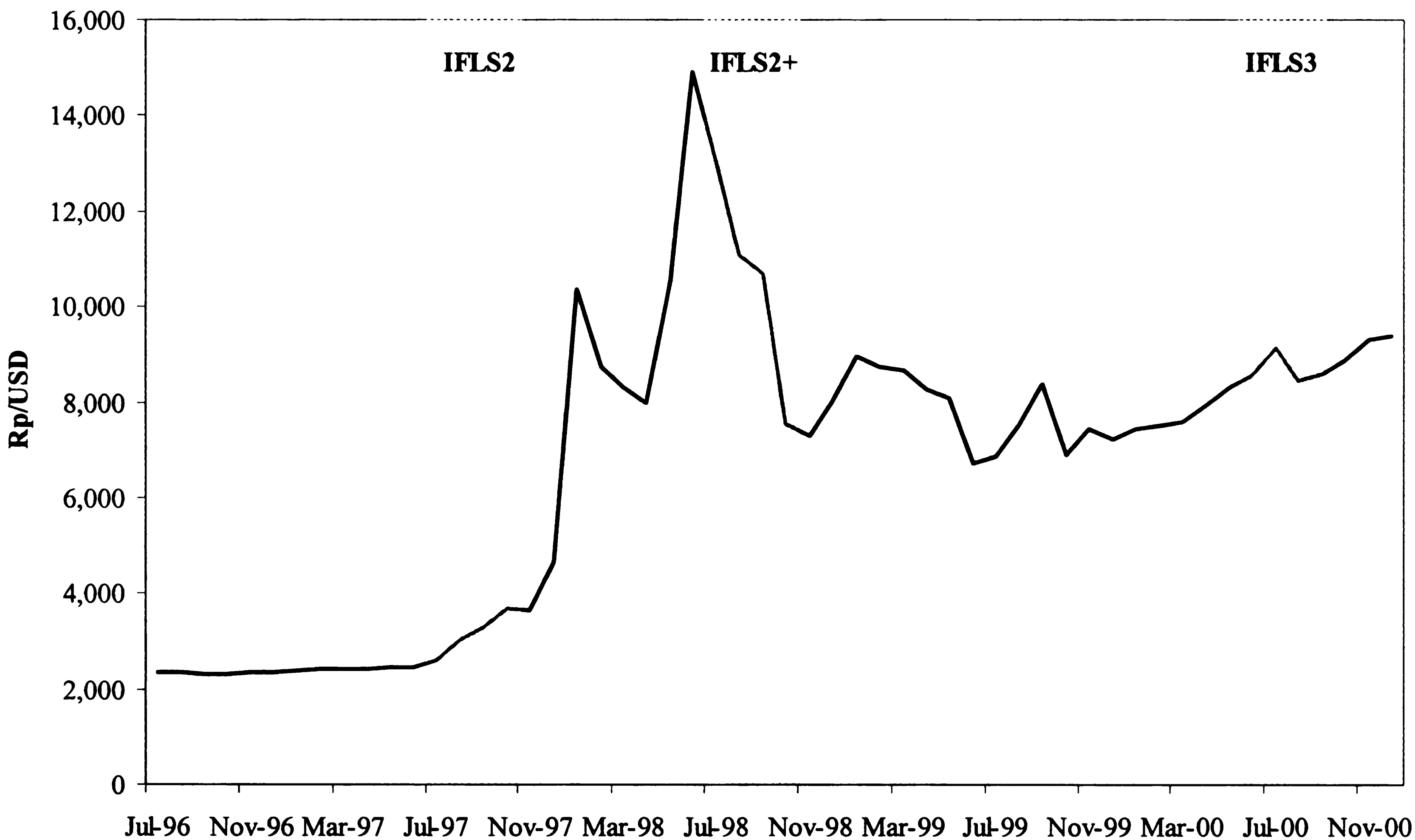


Figure 2: Food Price Index (January 1997 = 100)

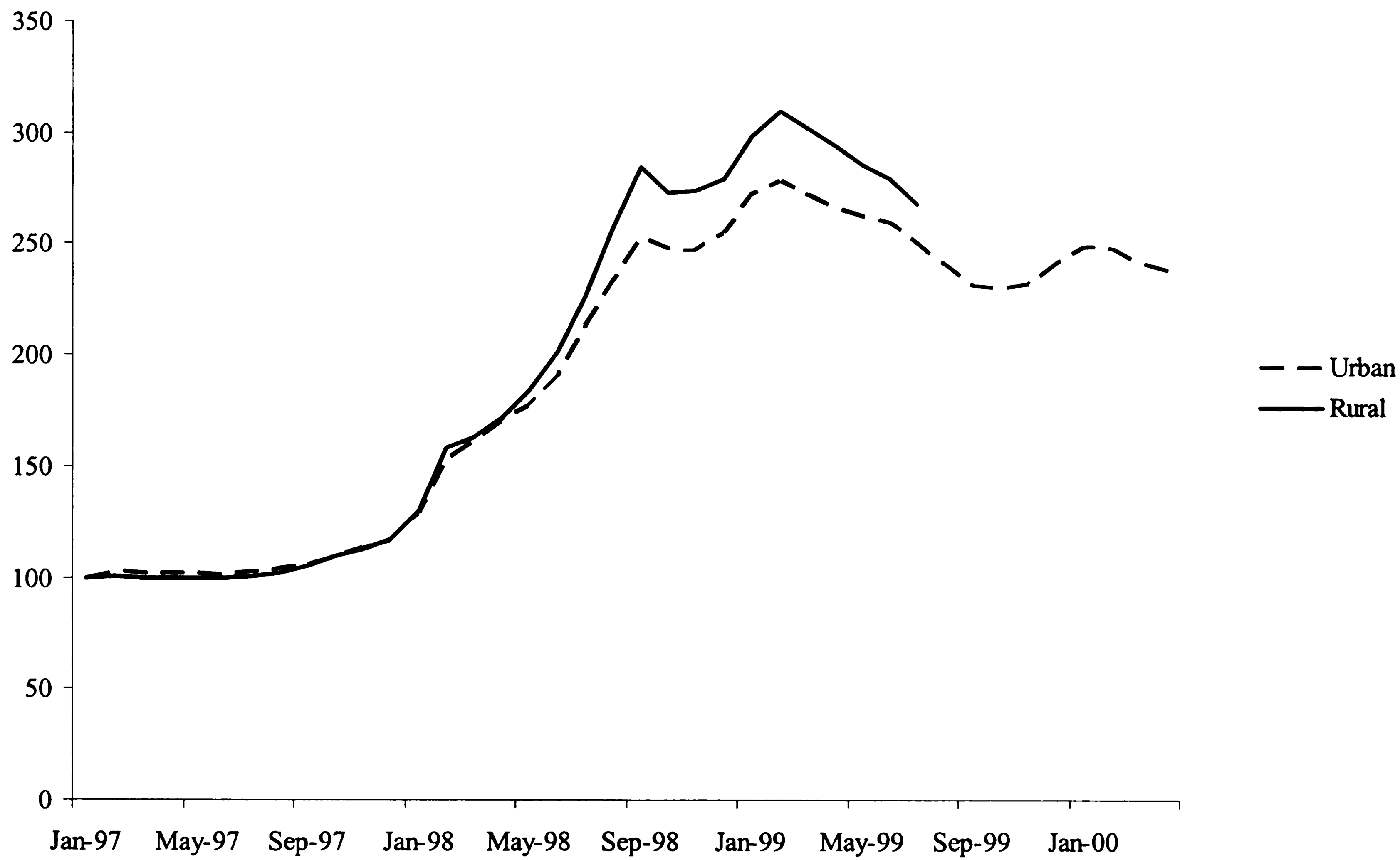
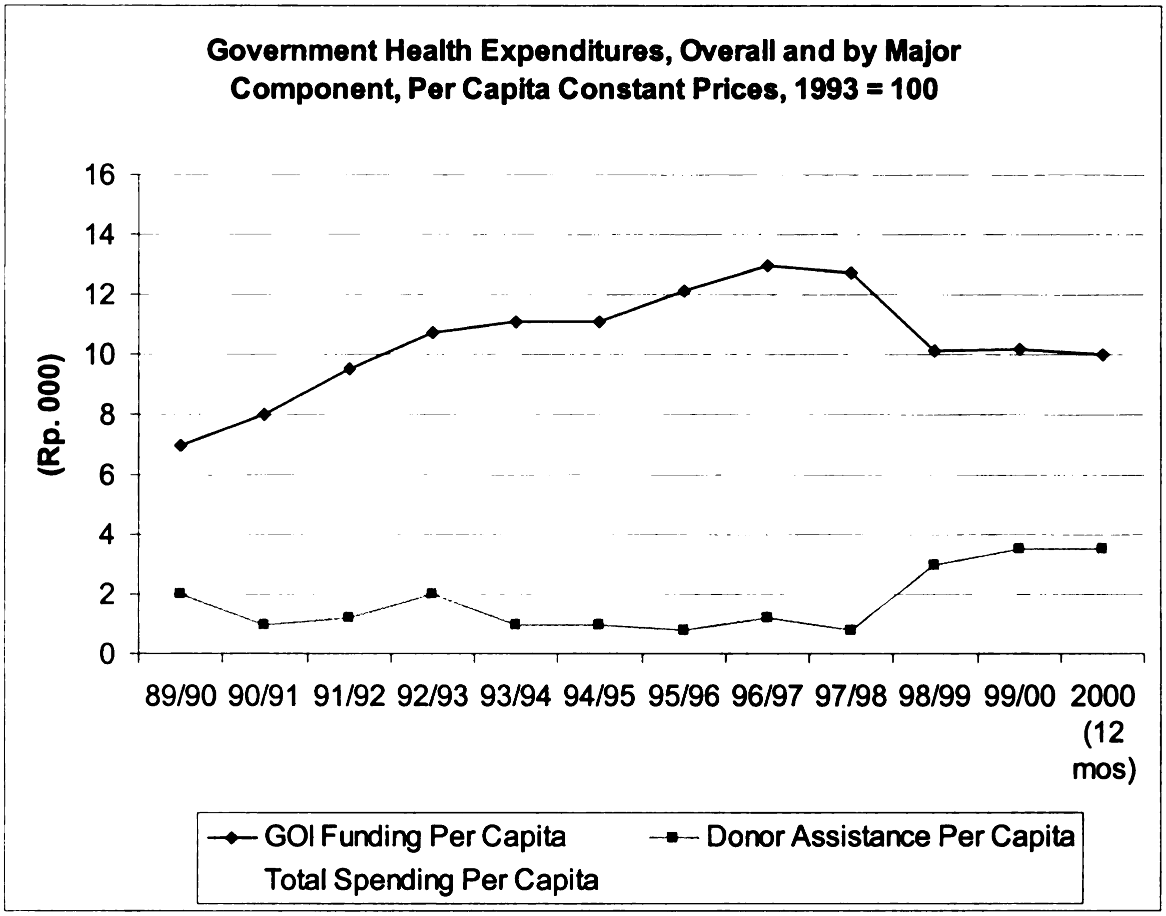
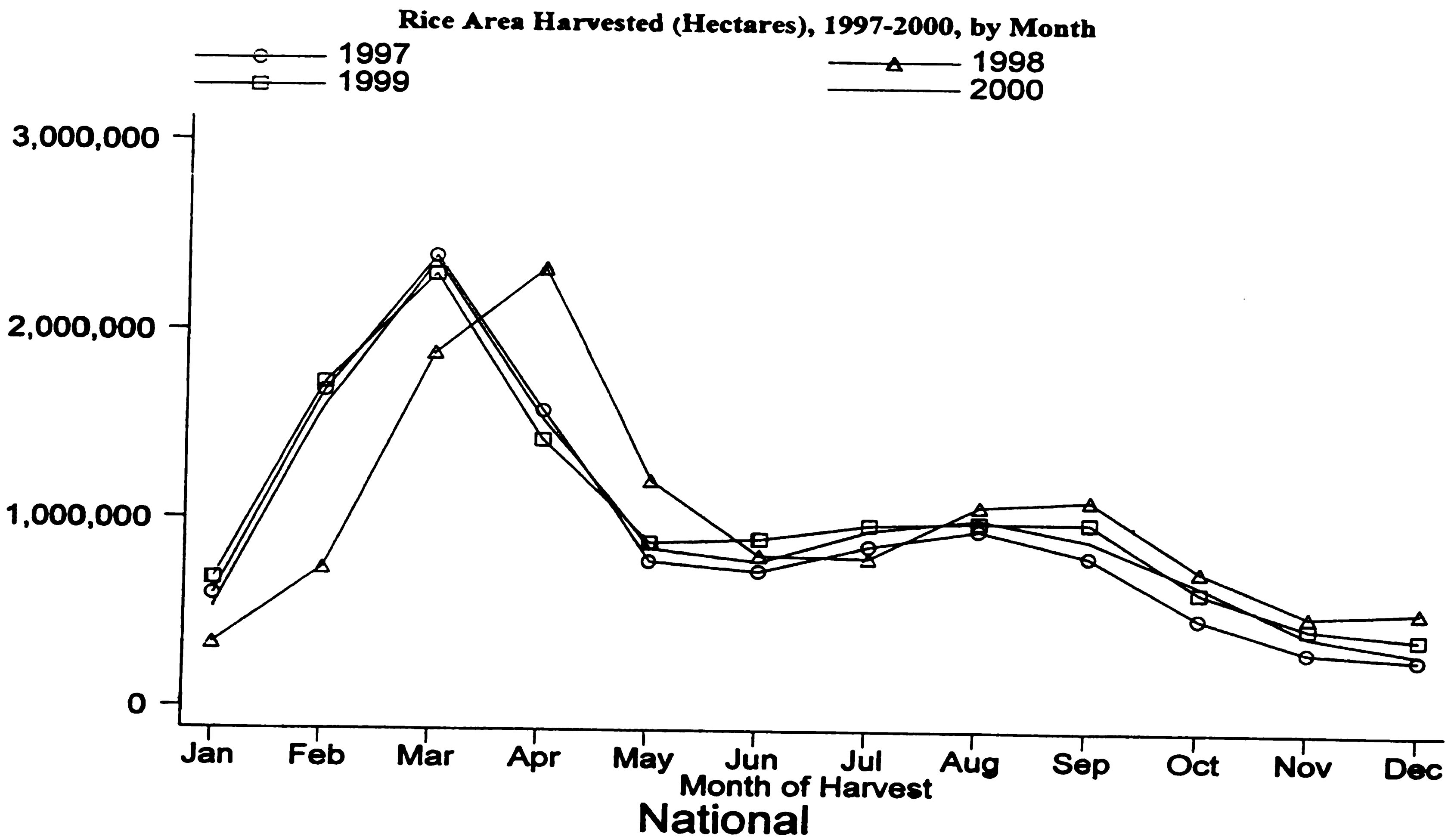


Figure 3: Government Health Expenditures (1993 = 100)



Source: Figure 1: Lieberman S., M. Juwono, and P. Marzoeki. "Government Health Expenditures in Indonesia through December 2000: An Update." World Bank East Asia and the Pacific Region Brief.

Figure 4: Rice Area Harvested (1997-2000)



Source: Courtesy of Jack Molyneaux

Figure 5: Mortality Rates (per 1,000 live births)

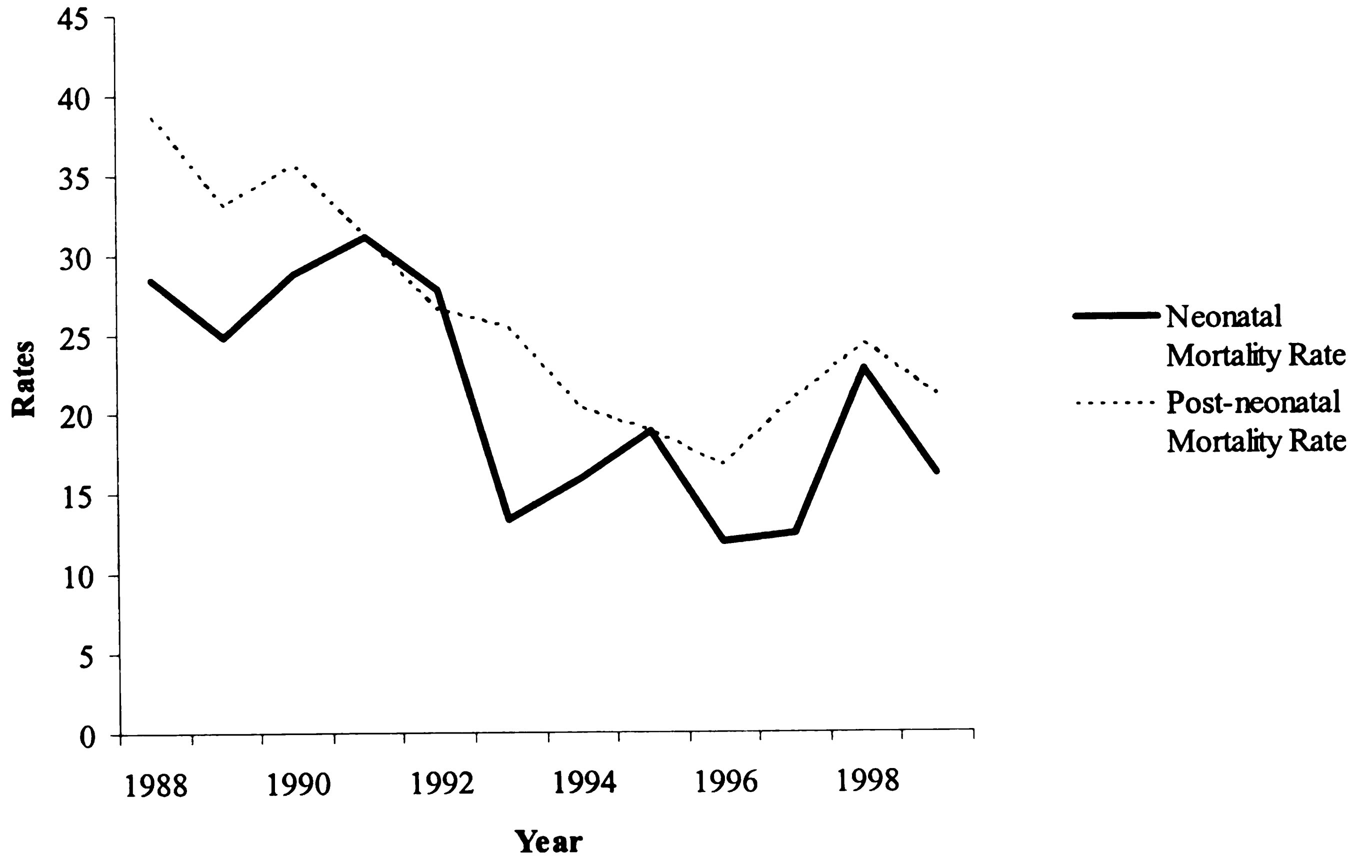


Figure 6a-6c: Cummulative Distribution of Birthweight (Kg.)

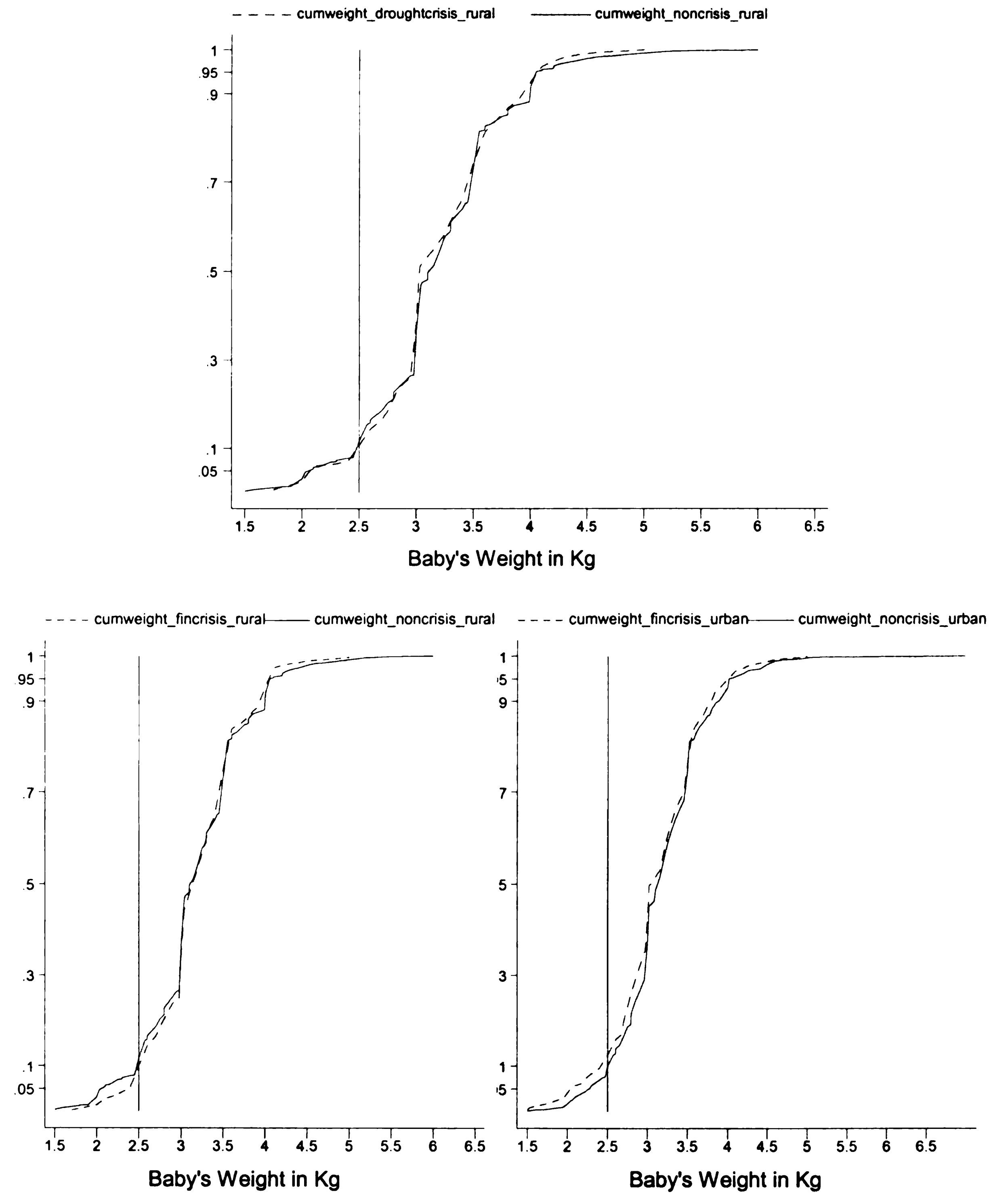
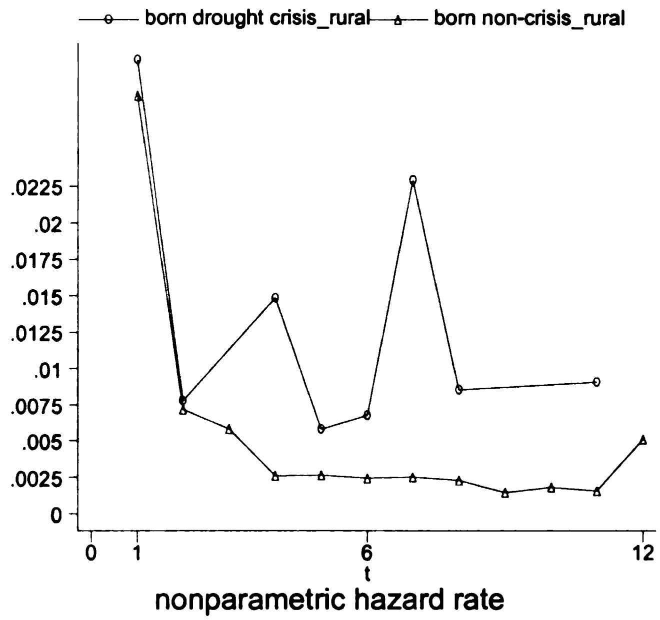
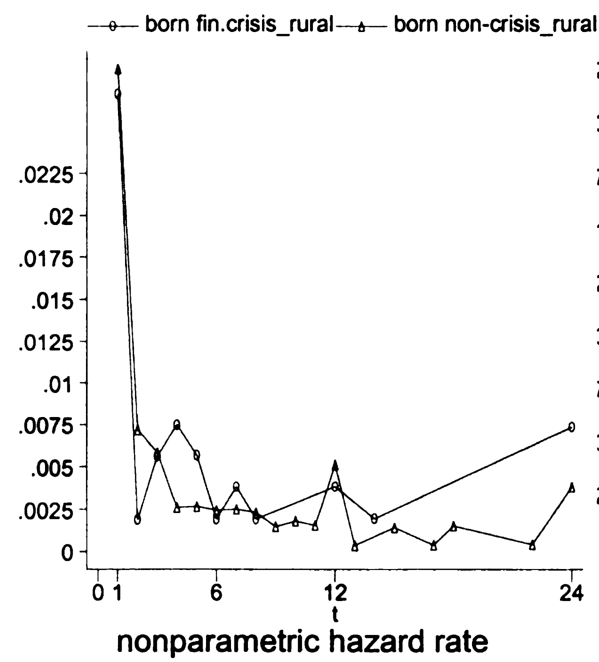


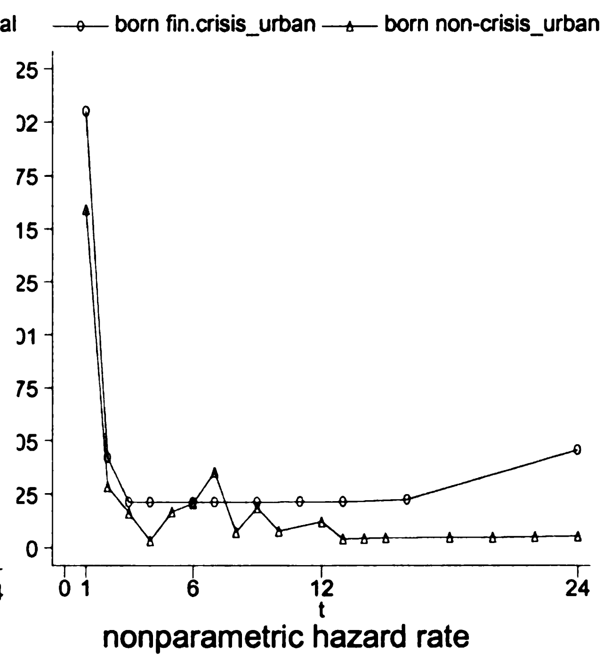
Figure 7a-7c: Non-parametric Hazard Rates (by Month)



Log-rank test for equality of survival function : p = 0.358

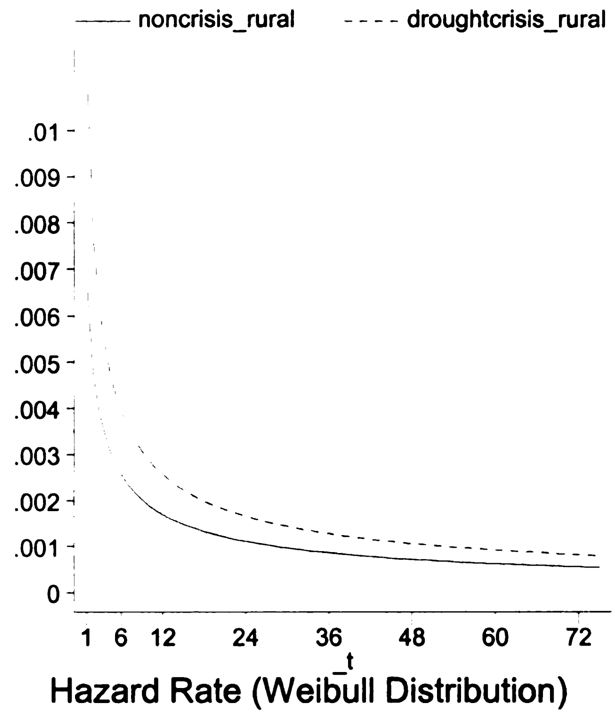


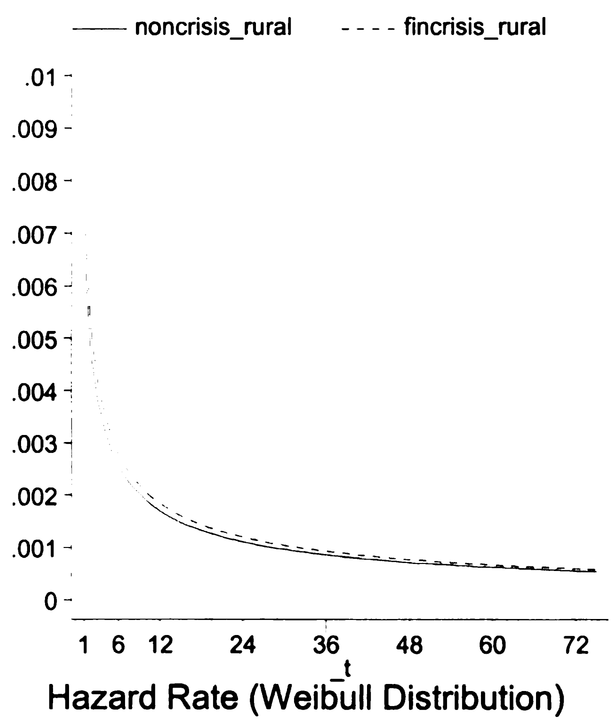
Log-rank test for equality of survival function : p = 0.823



Log-rank test for equality of survival function : p = 0.187

Figure 8a-8c: Parametric Estimated Hazard Rate (Weibull Distribution)





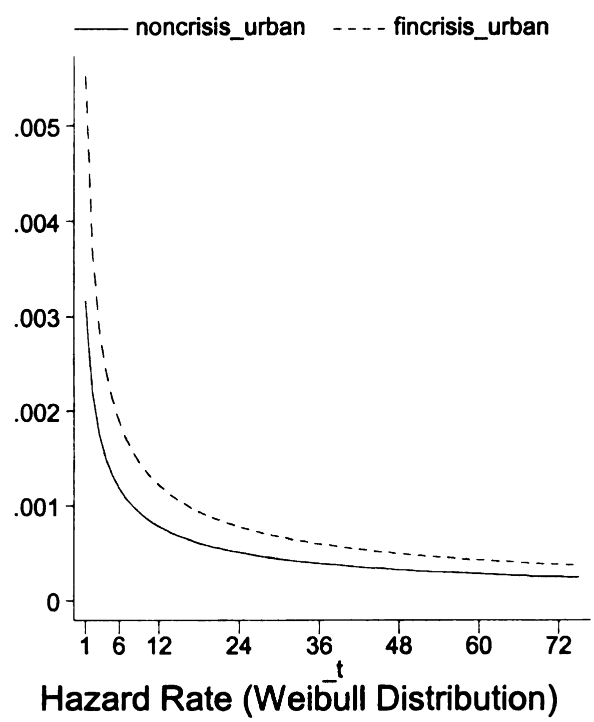


Figure 9: Nonparametric First-Marriage-Age Hazard by Birth Cohort

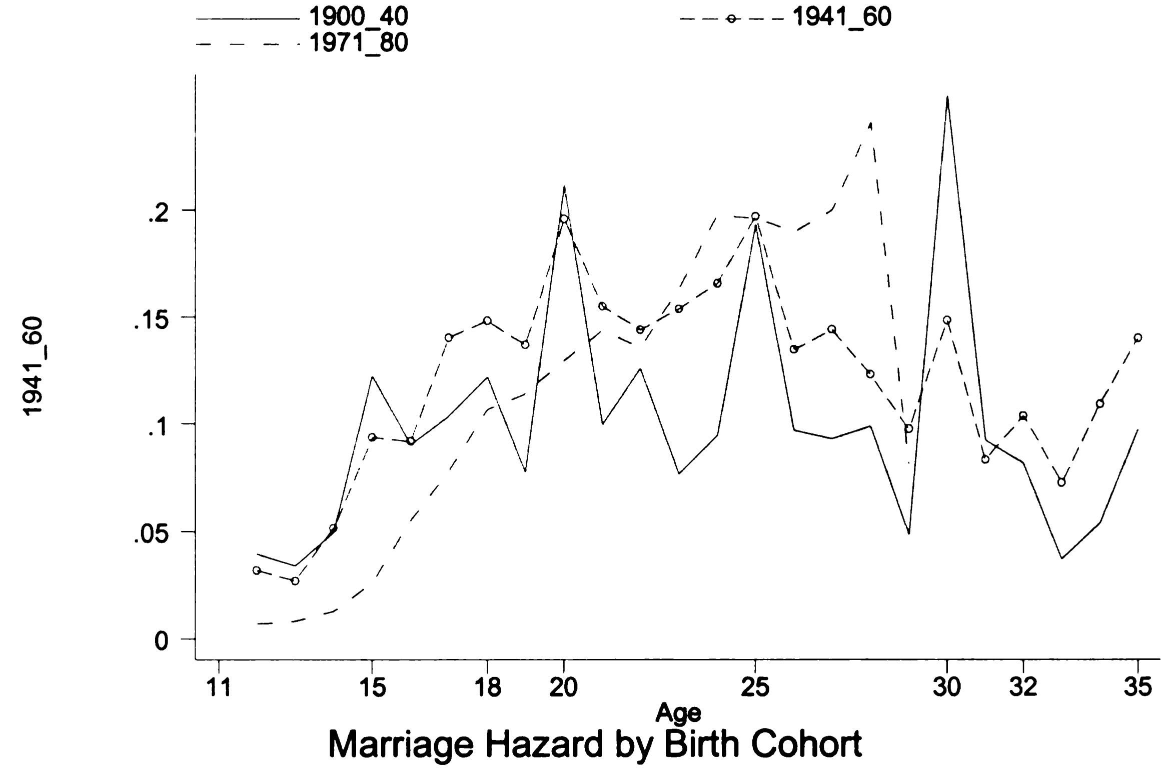


Figure 10: Nonparametric First-Marriage-Age Hazard by Education

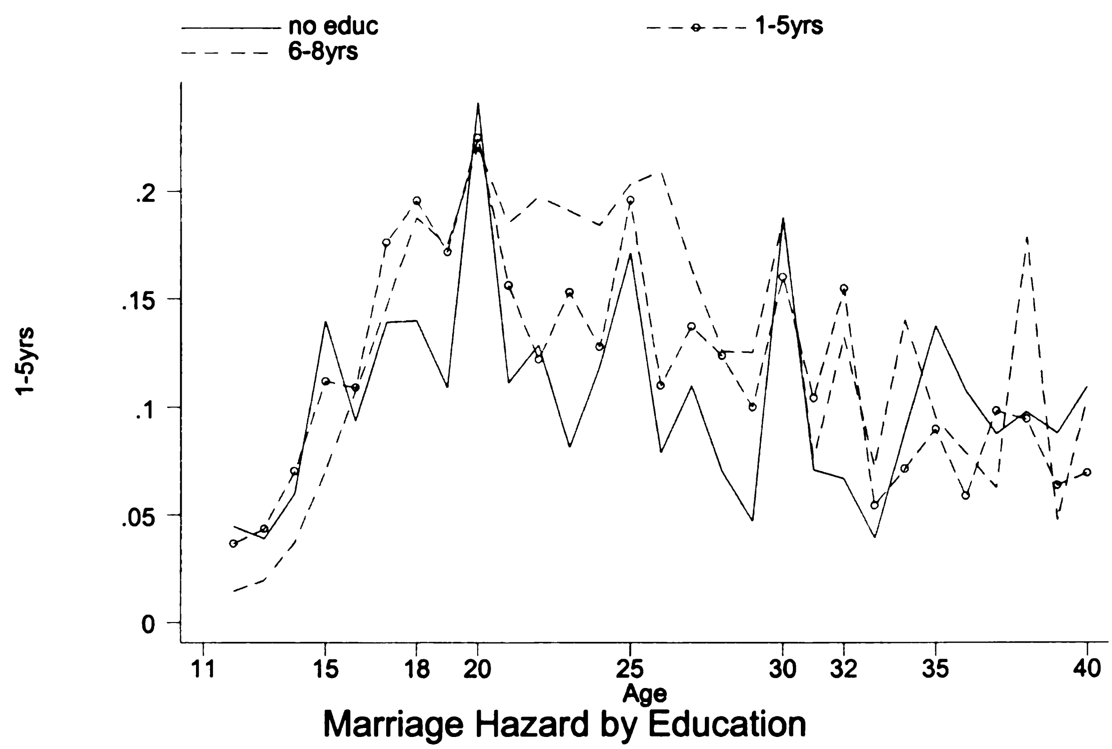


Figure 11: Nonparametric First-Marriage-Age Hazard by Education

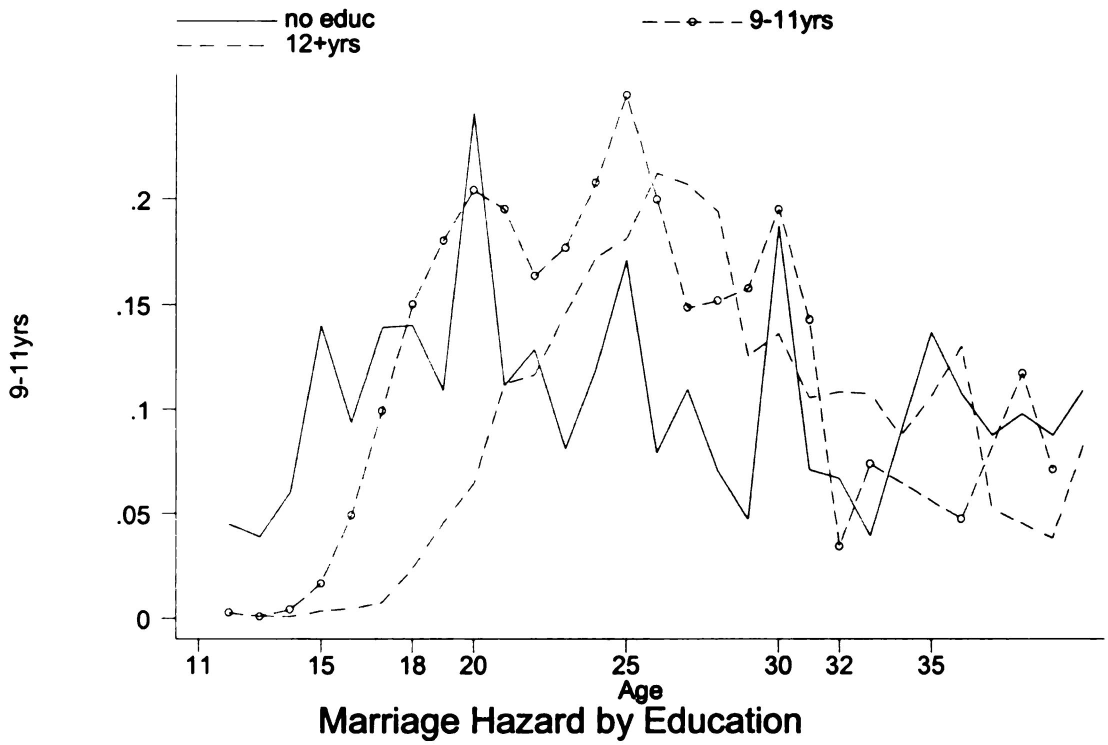


Figure 12: Nonparametric First-Marriage-Age Hazard by Residence at Age 12

Figure 13: Nonparametric Fertility Hazard by Marriage Cohort

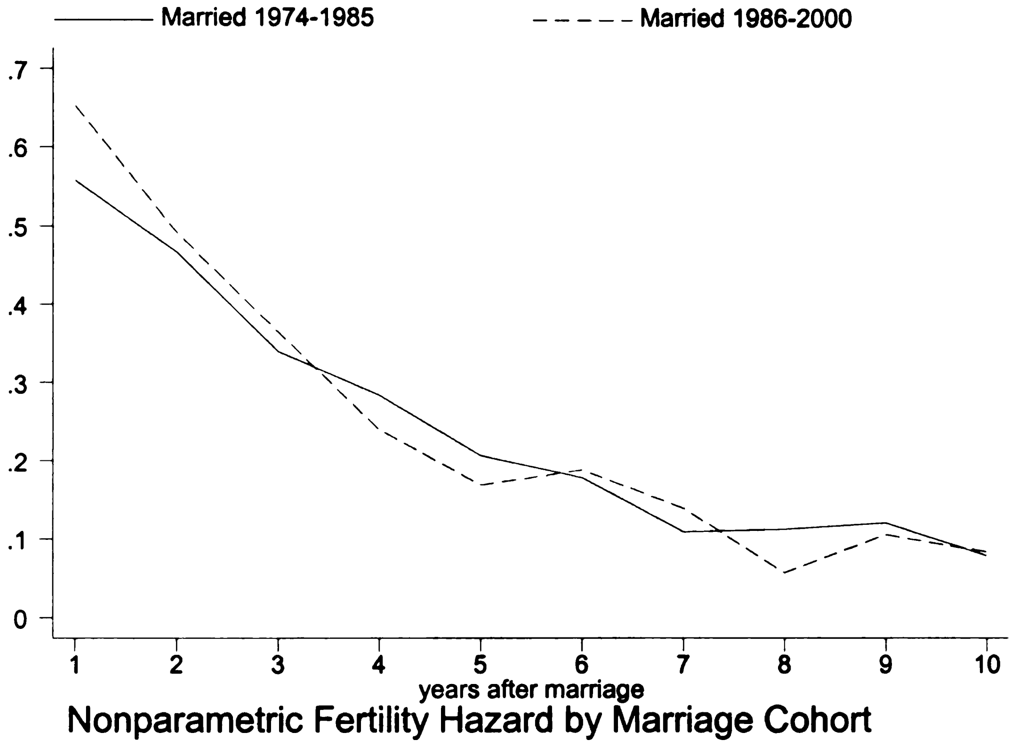


Figure 14: Nonparametric Fertility Hazard by Education

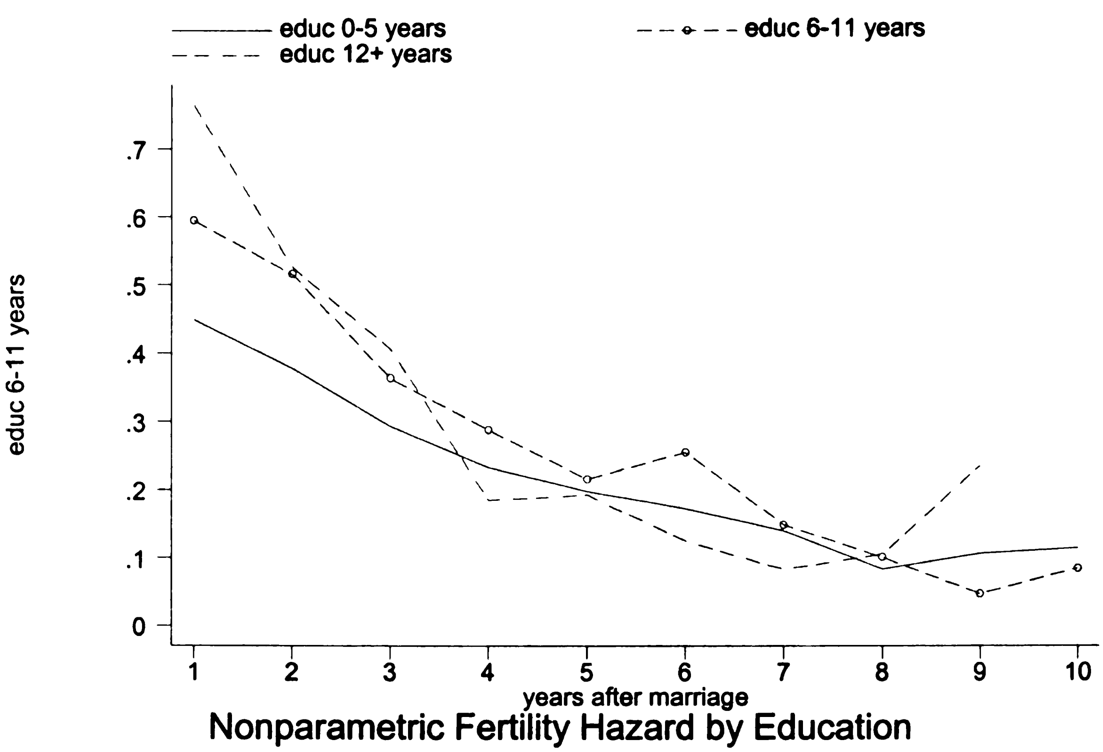


Figure 15: Nonparametric Fertility Hazard by Residence

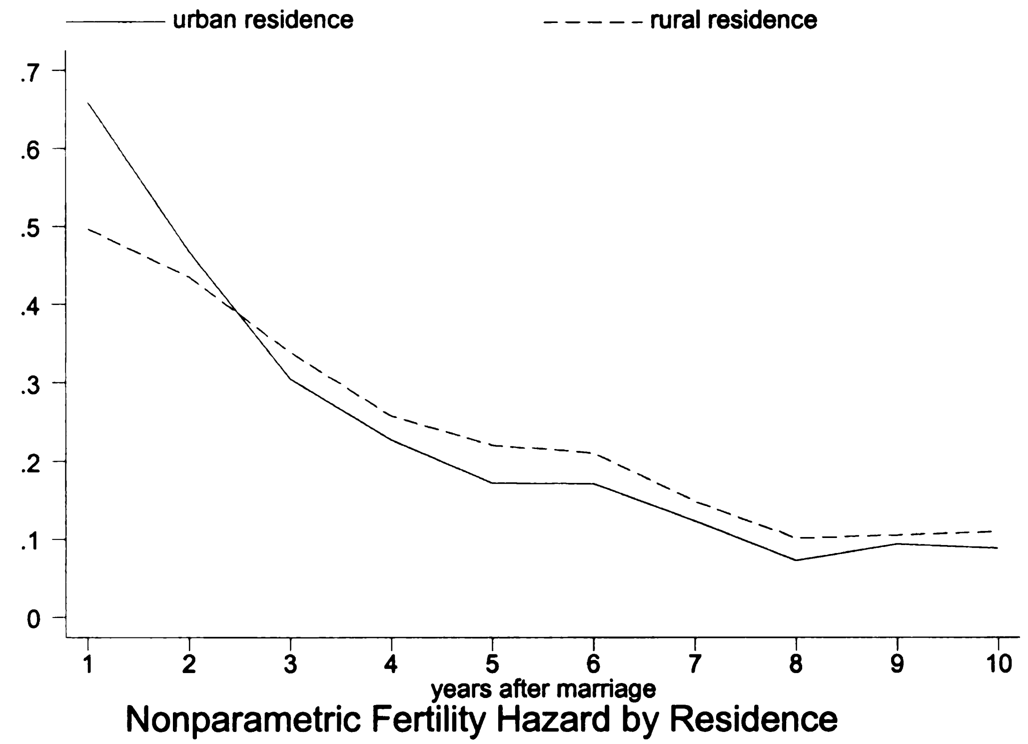
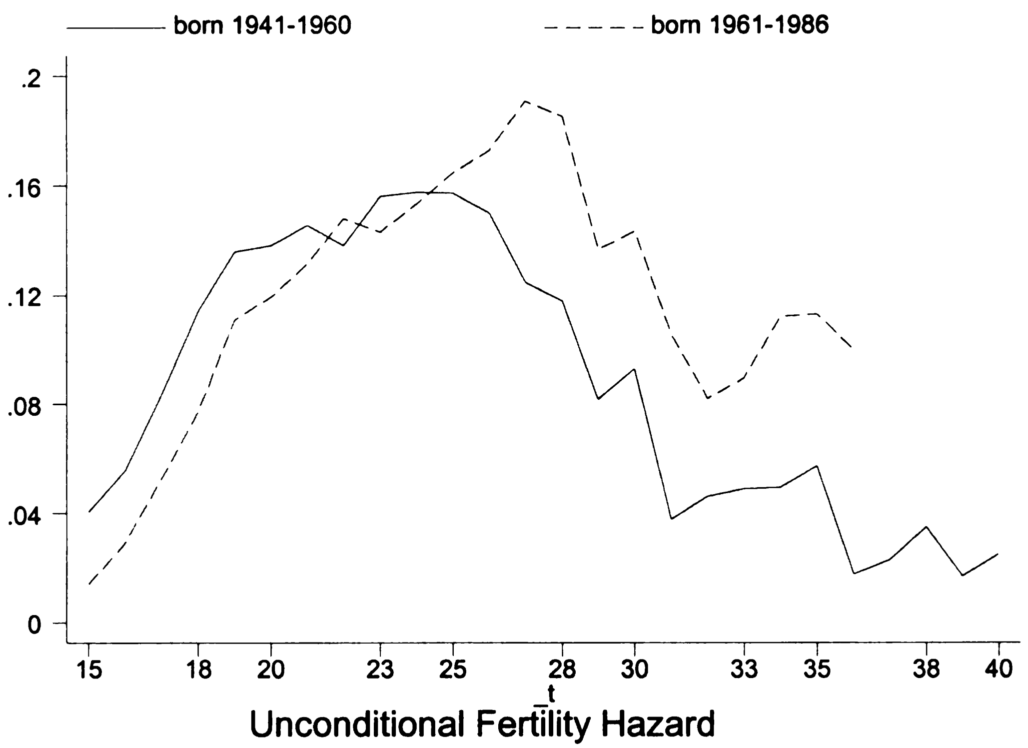


Figure 16: Unconditional Nonparametric Fertility Hazard by Birth Cohort



BIBLIOGRAPHY

BIBLIOGRAPHY

Adelman, Irman. "An econometric analysis of population growth." American Economic Review. 53, no. 3 (June 1963): 314-339.

Alatas, Vivi. "What Happen to Indonesia's Poverty? A Micro Simulation Exercise Using Household Surveys. Manuscript. World Bank. Jakarta, Indonesia. March 2002.

Bardhan, P. and C. Udry. Development Microeconomics. 1999. Oxford University Press.

Basu, A.M. and Peter Aaby. *The Methods and Uses of Antropological Demography.* 1998. Clarendon Press. Oxford.

Becker, Gary. "A Theory of Marriage: Part I." Journal of Political Economy, Volume 81, Issue 4, Jul.-Aug., 1973, 813-846.

Beckett, M., J. DaVanzo, N. Sastry; C.Panis, and C. Peterson. "The quality of retrospective data: An examination of long-term recall in a developing country." Journal of Human Resources. Summer 2001.

Boulier, Brian and Mark Rosenzweig. "Schooling, Search, and Spouse Selection: Testing Economic Theories of Marriage and Household Behavior." Journal of Political Economy, Volume 92, Issue 4, Aug., 1984, 712-732.

Bravo, J. "Demographic Consequences of structural adjustment in Chile." Paper presented at the Seminar on Demographic Consequences of Structural Adjustment in Latin America, Ouro Preto, Brazil (1992).

Bravo, J. "Economic Crisis and mortality: short and medium-term changes in Latin America." Paper presented at the Conference on the Peopling of the Americas, Veracruz, Mexico (1992).

Butz, William P., and DaVanzo, Julie. The Malaysian Family Life Survey: Summary Report. RAND report no. R.2351-AID. Santa Monica, Calif: RAND Corp., March 1978.

Caldwell, J., Reddy, P. and P. Caldwell. "Periodic High Risk as Cause of Fertility Decline in a Changing Rural Environment: Survival Strategies in the 1980-1983 South Indian Drought." Economic Development and Cultural Change. 34 (July 1986): 677-701.

Cleland, J. and J. Van Ginneken. 1989. "Maternal education and child survival in developing countries: the search for pathways of influences." Social Science and Medicine. 27: 1357-60.

Coale, Ansley and Edgar Hoover. "Population Growth and Economic Development in Low Income Counties. Princeton, NJ: Princeton Univerity Press, 1958.

Daly, Patricia and Fadia Saadah. "Indonesia: Facing the Challenge to Reduce Maternal Mortality." East Asia and the Pacific Region Watching Brief. World Bank. June 1999. Issue 3.

Das Gupta, M. "Fertility Decline in Panjab, India: Parallels with Historical Europe. Population Studies 49(3): 481-500.

Davidson, R. and Jean-Yves Duclos. "Statistical Inference for Stochastic Dominance and for the Measurement of Poverty and Inequality." Econometrica v86 n6. 2000.

East Asia and Pacific Region. Watching Brief. "Indonesia: Undernutritiion in Young Children." World Bank. January 1999. Issue 1.

Ermisch, J. "Economic Opportunities, Marriage Squeezes, and Propensity to Marry: An Economic Analysis of Period Marriage Rates in England and Wales." Population Studies, Volume 35, Issue 3, Nov., 1981, 347-356.

Federicksen, Harald. " Determinants and consequences of mortality and fertility trends." Public Health Reports. 81, no. 8 (August 1966): 715-727.

Foster, Andrew D. "Price, Credit Markets and Child Growth in Low-Income Rural Areas." The Economic Journal, Volume 105, Issue 430 (May, 1995), 551-570.

Frankenberg E., Thomas D., and Beegle K. 1999. "The real costs of Indonesia's Economic Crisis: Preliminary Findings from the Indonesia Family Life Surveys." DRU-2064-NIA/NICHD. Santa Monica, CA: RAND.

Frankenberg, E., Hamilton, P., Polich, S., Suriastini, W., and D. Thomas. *User's Guide for the Indonesia Family Life Survey*. DRU2238/2-NIA-NICHD. March 2000.

Frankenberg, E., Smith, J. and D. Thomas. "Economic Shocks, Wealth, and Welfare." Journal of Human Resources, Volume 38 (Spring 2003).

Galloway, P. "Basic Patterns in Annual Variations in Fertility, Nuptiality, Mortality, and Prices in Pre-Industrial Europe." Population Studies, Volume 42, Issue 2 (Jul., 1988), 275-302.

Hill, K, G. Adansi-pipim, L. Assogba, A. Foster, J. Mukiza-gapere, and C. Paxson. "Demographic Effects of Economic Reversals in Sub-Saharan Africa." National Research Council.National Academy Press. Washington D.C. 1993.

Hill, K. and A. Palloni, " Demographic responses to economic shocks: The Case of Latin America." Paper present at the Conference on the Conference on the Peopling of the Americas, Veracruz, Mexico (1992).

Hull, T and G. Jones. "Demographic Perspectives." in Indonesia's New Order: The Dynamics of Socio-economic Transformation. Edited by H. Hill. Allen & Unwin Pty Ltd. SRM Production Services Sdn Bhd, Malaysia. 1994.

Hull, T. "Fertility Decline in Indonesia: a review of recent evidence." 1980. BIES 16(2), pp. 104-12.

Hull, T.H. and V.J. Hull. "Population Change in Indonesia: findings of the 1980 census." 1984. BIES. 20(3), pp. 95-119.

James J. Fox. "The 1997-1998 Drought in Indonesia." Natural Disasters and Policy Responses in Asia: Implications for Food Security. Harvard University Asia Center. August, 1999.

Keeley, Michael. "Age Analysis of Age Pattern of First Marriage." International Economic Review, Volume 20, Issue2, Jun., 1979, 527-544.

Kotlikoff, L. and A. Spivak. "The Family as an Incomplete Annuities Market." Journal of Political Economy, Volume 89, Issue 2 (Apr., 1981), 372-391.

Lee, R. " Short-term variation: vital rates, prices and weather," in E.A. Wrigley and R.S. Schofield (eds.). The Population History of England. 1541-1871. Cambridge, Mass: Harvard University Press. 1981.

Lieberman, S., M. Juwono, and P. Marzoeki. "Government health expenditures in Indonesia through December 2000: An Update." World Bank East Asia and the Pacific Region Watching Brief. October 15, 2001, Issue 6.

Lindstrom, David and Betemariam Berhanu. "The Impact of War, Famine, and Economic Decline on Marital Fertility in Ethiopia." Demography, Volume 36, Issue 2 (May, 1999), 247-261.

Mckenzie, D. "How Do Households Cope with Aggregate Shocks? Evidence from the Mexican Peso Crisis." Manuscript. 2002.

Mellington and Cameron. "Female Education and Child Mortality in Indonesia." Melbourne- Department of Economics in its series papers number 693. 1999

Menken, J., J. Trussell, and S. Watkins. 1981. "The nutrition fertility link: An evaluation of the evidence." Journal of Interdisciplinary History XI(3): 425-441.

Mloyi, M. "Crisis-Driven Fertility Decline." Background Paper. UN Economic and Social Council Third African Population Conference, Meeting of Experts, Dakar, Senegal 7-10 December, 1992.

Mooney, M. and P. Hodsdon. "Effects of the Timing of Marriage and First Birth of the Spacing of Subsequent Births." Demography, Vol. 18, No. 4. (Nov., 1981). Pp. 529-548.

Palloni, A, K. Hill, and G.P. Aguirre. "Economic Swings and Demographic Changes in the History of Latin America." Population Studies, Volume 50, Issue 1 (Mar., 1996), 105-132.

Palloni, A. "Health Conditions in latin America and policies for mortality change", in J. Vallin and A. Lopez (eds.), Health Policy, Social Policy and Mortality Prospects. Liege: Ordina. 1985

Palloni, A. and K. Hill, "The Effects of Structural Adjustments on mortality by age and cause in Latin America." Center for Demography and Ecology. Working Paper 92-22, University of Wisconsin, Madison, Wis. 1992.

Pertersen, W. and R. Petersen with the collaboration of an International Panel of Demographers. *Dictionary of Demography: Terms, Concepts, and Institution.* Greenwood Press. New York. Westport, Connecticut. London. 1986

Portner, Claus. "Children as Insurance." Journal of Population Economics (2001) 14: 119-136.

Pressat, R. *Statistical Demography.* Translated and Adapted by Damein A. Courtney. 1978. Methuen & Co Ltd.

Preston, Samuel H. "Health Programs and Population Growth." Population and Development Review. Volume 1, Issue 2 (Dec. 1975), 189-199.

Rahman, M., B. Wojtyniak, M. M. Rahaman and K.M.S. Aziz, "Impact of environmental sanitation and crowding on infant mortality in rural Bangladesh." The Lancet (1985), pp. 28-32.

Razzaque, A., N. Alam, L. Wai, and A. Foster. "Sustained effects of the 1974-75 famine on infant and child mortality in a rural area of Bangladesh." Population Studies 44: 145-154. 1990.

Reher, D. and J.A. Ortega, "Short run economic fluctuations and demographic behaviour: some examples from twentieth century South America." Paper presented at the Seminar on Demographic behaviour: some examples from twentieth century South America." Paper presented at the Seminar on Demographic Consequences of Structural Adjustment in Latin America, Ouro Preto, Brazil (1992).

Richards, A. and. S. Preston. "The Influence of Women's Work Opportunities and Marriage Rates." Demography, Volume 12, Issue 2, May 1975, 209-222.

Richards, T. "Weather, nutrition, and the economy: the analysis of short-run fluctuation in births, deaths, and marriages, France 1740-1909," in T. Bengtsson et al. (eds.), Pre-Industrial Population Change. Stockholm: Almquist and Wiksell. 1984.

Rosenzweig, M and T.P. Shultz. "Child mortality and fetility in Colombia: individual and community effects." Health Policy and Education, 2: 305. 1982.

Rosenzweig, M. "Risk, Implicit Contracts and the Family in Rural Areas of Low-Income Countries." Economic Journal. 98 (December 1988): 1148-70.

Rosenzweig, M. and Oded Stark. "Consumption Smoothing, Migration, and Marriage: Evidence from Rural India." Journal of Political Economy, Volume 97, Issue 4 (Aug., 1989), 905-926.

Rutenberg, N. and Ian Diamond. "Fertility in Botswana: The Recent Decline and Future Prospects." Demography, Vol. 30, No. 2, May 1993.

Sah, RK. "The Effects of Child Mortality Changes on Fertility Choice and Parental Welfare." Journal of Political Economy 99(3):582-606.

Sastry, Narayan. "Forest Fires, Air Pollution, and Mortality in Southeast Asia." Demography. Volume 39 number1. February 2002.

Schultz, T. P. 1989. "Benefits of Educating Women." Washington, D.C.: World Bank, Background Papers Series, Education and Employment Division, Population and Human Resources Department. The Benefits of Education for Women HRO Dissemination Notes. Human Resources Development and Operations Policy. Number 2, March 8, 1993.

Schultz, T.P. "An economic model of family planning and fertility." Journal of Political Economy. 77, no.2 (March/April 1969): 153-180.

Schultz, T.P. "Investments in the schooling and health of women and men: Quantities and returns." Journal of Human Resources. Fall 1993; Vol. 28, Iss. 4; pg. 694, 41.

Singh, I., L. Squire and J. Strauss (eds.). 1986. *Agricultural Household Models: Extensions, Applications and Policy*. Baltimore: John Hopkins University.

Spiegelman, M. *Introduction to Demography.* 1955. The Society of Actuaries. Chicago. Illinois.

Stein, Z., M.Susser, G. Saenger and F. Morolla. *Famine and Human Development: The Dutch Hunger Winter of 1944-1945*. New York. 1975.

Strauss, J., Beegle, K., Dwiyanto, A., Herawati, Y., Pattinasarany, D., Satriawan, E., Sikoki, B., Sukumdi and F. Witoelar. "Indonesian Living Standards Three Years after the Crisis: Evidence from the Indonesia Family Life Survey." 2003. RAND, Santa Monica, CA.

Wang, F. and Yang Quanhe. "Age at Marriage and the First Birth Interval: The Emerging Change in Sexual Behavior Among Young Couples in China." Population and Development Review, Vol. 22, No. 2. (Jun, 1996). Pp 299-320.

Weir, D. "Life under pressure: France and England, 1670-1680," Journal of Economic History, 44 (1984), pp. 27-47.

Wooldridge, J.M. *Introductory Econometrics: A Modern Approach*. Copyright 2000 by Sourth-Western College Publishing. USA.