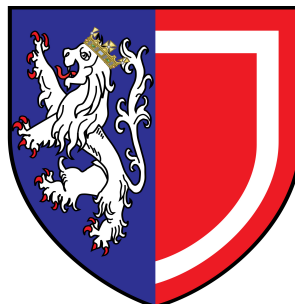


Essays on the allocation of labour and capital in Indonesia

Anisha Sharma

A thesis submitted for the degree of
Doctor of Philosophy



Balliol College
University of Oxford
May 2016

Abstract

Essays on the allocation of labour and capital in Indonesia

Anisha Sharma
Balliol College, University of Oxford
DPhil in Economics
May 2016

This dissertation comprises three essays on the allocation of labour and capital in a large developing country, Indonesia.

In the first essay, I examine the impact of the 1998 East Asian recession on child schooling outcomes in Indonesia. Using panel data on 7-15 year-olds, I exploit the heterogeneous impact of the recession across urban communities as measured by the variation in rice price increases, under the assumption that communities where rice prices increased the most were those where real wages declined the most. I find that for the youngest children (aged 7-12 years) there is a large negative impact of higher rice prices on school attendance and no effect on labour market participation. For older children (aged 13-15 years), schooling enrolment does not respond to rice prices but labour market participation declines sharply in the worst-hit communities. I find no evidence of adverse long-term consequences on human capital formation.

In the second essay, I test the hypothesis that there exists a significant earnings differential between similar workers in the formal and informal sectors. Using panel data on salaried and self-employed individuals, I find that after controlling for firm size and individual-specific heterogeneity, there is no formal sector earnings premium, except in the public sector. The results are robust to the presence of unpaid family workers, measurement error, and non-random attrition in the survey. This questions the commonly held belief that labour markets in developing countries are segmented because of legal institutions that protect high formal sector earnings.

In the third essay, I estimate the effect of a large exchange rate depreciation on the performance of importers. The ability to manage volatility in the cost of imported inputs is likely to depend on a firm's access to external sources of finance as well its ability to hedge against exchange rate movements. Using data from a census on Indonesian firms, I find that while domestic importers face lower value-added due to a rise in their costs of production, foreign-owned importers fare better: they are more likely to sustain higher value-added, hire more labour and use more materials than domestic owned firms. This suggests another channel through which FDI can add value to a firm in a developing country, particularly with the increasing importance of trade in intermediate goods.

Word count: Approximately 50,000 words.

To my grandmother, Sarla.

Acknowledgements

I am very grateful to my supervisors, Stefan Dercon and Patricia Rice, for their extensive support and guidance, both specifically on the thesis, as well on the process of academic research. I must also thank Simon Quinn for his generosity in setting aside time for me, and for his valuable advice.

For very useful suggestions and feedback, I'd like to acknowledge Kathleen Beegle, Francis Teal, Steve Bond, James Fenske, Clement Imbert, Abhijeet Singh, Christopher Roth, Jono Lain, Michael Koelle, Muhammad Meki, Rachel Cassidy, Daniela Scur, Elwyn Davies, and Matej Bajgar.

My graduate studies at Oxford have been funded by the Rhodes Trust, the Eddie Dinshaw Scholarship at Balliol College, and a Doctoral Studentship from the Department of Economics and Brasenose College. The George Webb Medley/OEP Fund supported the purchase of data on Indonesian firms. I am very grateful to all of these remarkable institutions for their support, financial and otherwise.

I am especially grateful to my friends for their loyalty and encouragement; I consider myself very lucky to be able to count on them. My biggest debt is to my parents, Dhruv and Vijaya, for their unconditional love and the many sacrifices they made for me, and to Raghav, for keeping the faith.

Contents

List of Figures	v
List of Tables	vii
Introduction	xvii
1 The impact of a macroeconomic crisis on child schooling outcomes	1
1.1 Introduction	1
1.2 Background and theoretical motivation	4
1.2.1 Background to the Indonesian crisis	4
1.2.2 Theoretical motivation	7
1.3 Data	10
1.3.1 Indonesian Family Life Survey	10
1.3.2 Overview of the data and summary statistics	12
1.4 The impact of rice price increases on schooling outcomes	14
1.4.1 Empirical framework	14
1.4.2 Results	18
1.4.3 Heterogeneity by age	25
1.4.4 Heterogeneity by income	27
1.4.5 Robustness checks	28
1.5 Long-term impact of aggregate shock on schooling	34
1.5.1 Empirical framework	35
1.5.2 Results	37
1.5.3 Robustness checks	42
1.6 Conclusion	43
1.A Appendix	45
1.A.1 Data and construction of sample	45
1.A.2 Robustness check with probit models	45
1.A.3 Robustness check with initial community fixed effects	45
1.A.4 Correcting for attrition	47

CONTENTS

2	Informality, firm size, and labour market segmentation	57
2.1	Introduction	57
2.2	Understanding the key issues	60
2.2.1	Alternative views of the informal sector	60
2.2.2	Informality in Indonesia	62
2.3	Summary of the data	65
2.3.1	The Indonesian Family Life Survey	65
2.3.2	Overview of the data and summary statistics	66
2.4	Explaining intersectoral earnings differentials	71
2.4.1	Empirical framework	71
2.4.2	Results	75
2.4.3	Controlling for unobserved heterogeneity	75
2.4.4	Disaggregating the formal sector	82
2.4.5	Endogenous switching between sectors	84
2.4.6	Robustness checks	90
2.5	Conclusion	90
2.A	Appendix	94
2.A.1	Data and construction of sample	94
2.A.2	Sample selection bias	94
2.A.3	Measurement error	95
2.A.4	Correcting for attrition	100
3	The effect of a currency depreciation on importers: a firm-level analysis	109
3.1	Introduction	109
3.2	Background and theoretical motivation	112
3.2.1	Background to the Indonesian crisis	112
3.2.2	Theoretical motivation	114
3.3	Data	117
3.3.1	Survei Industri	117
3.3.2	Overview of the data and summary statistics	118
3.4	The impact of exchange rate movements on firm performance	120
3.4.1	Empirical framework	120
3.4.2	Results	123
3.4.3	Robustness to added controls	124
3.4.4	Components of value-added	129
3.4.5	The survival of firms	132
3.5	Conclusion	134
3.A	Appendix	136
3.A.1	Data and construction of sample	136
3.A.2	Including interactions by year	136

CONTENTS

3.A.3 Estimation of productivity	137
References	139

List of Figures

1.1	CPI and rice prices in Indonesia (1991-2005)	6
1.2	Wholesale price indices for fuel and vehicles (1991-2005)	7
1.3	Rise in coefficient in variation of prices in 1998 (Varela et al., 2012) . .	8
1.4	Variation in rice price increases across communities (1997-2000)	16
1.5	Map of rice price increases (%) across communities	17
2.1	(Log) Real monthly income densities by employment category	69
2.2	(Log) Real monthly income densities by employment category – switchers	80
3.1	Exchange rate movements and consumer prices	113
3.2	Real Exchange Rate movements in East Asia (1994-2001)	114
3.3	Coefficients by year – all firms	125
3.4	Coefficients by year – exporters	126

List of Tables

1.1	Child characteristics and schooling outcomes	13
1.2	Household and community characteristics	15
1.3	Impact of rice price increases on school enrolment	19
1.4	Impact of rice price increases on hours in school	23
1.5	Impact of rice price increases on labour market participation	24
1.6	Heterogeneous impact of rice price increases by age group	26
1.7	Heterogeneous impact of rice price increases by income group	29
1.8	Placebo test: correlation of pre-crisis changes in schooling outcomes with post-crisis changes in prices	30
1.9	Rural-urban migration of panel households	31
1.10	Sector of employment of the heads of panel households	31
1.11	Impact of rice price increases with household effects	32
1.12	Heterogeneous impact of rice price increases by age group with house- hold effects	33
1.13	Long-term impact of rice prices increases – community panel	38
1.14	Long-term impact of rice prices increases – individual panel	39
1.15	Heterogenous long-term impact of rice prices increases by age	40
1.16	Heterogenous long-term impact of rice prices increases by income	41
1.A.1	Probit models for impact of rice prices on school enrolment and labour market participation	46
1.A.2	Heterogeneous impact of rice price increases by age - with and without community fixed effects	48
1.A.3	Heterogeneous impact of rice price increases by household income - with and without community fixed effects	49
1.A.4	Comparing attriters and non-attriters - child characteristics	50
1.A.5	Comparing attriters and non-attriters - household and community characteristics	51
1.A.6	Probit predicting non-attrition by baseline variables	53
1.A.7	Attrition-corrected long-term impact of rice prices increases	54
1.A.8	Attrition-corrected heterogenous long-term impact of rice prices in- creases by age	55

LIST OF TABLES

1.A.9	Attrition-corrected heterogenous long-term impact of rice prices increases by income	56
2.1	Official definition of informality	63
2.2	Share of salaried workers who worked on contracts and received other employee benefits in 2007	65
2.3	Distribution of earners across employment categories over time	68
2.4	Work characteristics by employment category over time (mean)	70
2.5	Demographic characteristics by employment category (mean)	72
2.6	OLS, fixed effects and first-differenced earnings regressions - official definition of formality	76
2.7	Transitions between sectors - 1997 to 2000	77
2.8	Transitions between sectors - 2000 to 2007	78
2.9	Comparing switchers and non-switchers	79
2.10	OLS, fixed effects and first-differenced earnings regressions - broader employment sector definition	83
2.11	Probits explaining switching between informal and formal sectors	87
2.12	First-differenced earnings regressions across different periods	88
2.13	First-differenced earnings regressions testing for sensitivity to direction of transition	89
2.14	System GMM estimates of the effect of formality	91
2.A.1	Fixed effects estimator on full sample and Wooldridge estimator corrected for selection into employment	96
2.A.2	Interviewer assessment of quality of responses	98
2.A.3	OLS, fixed effects and first-differenced earnings regressions - excluding responses assessed to be of poor quality	99
2.A.4	Comparing attriters and non-attriters	101
2.A.5	Non-attrition probit regressions predicting selection in next wave	104
2.A.6	Inverse probability weighted OLS and first-differenced earnings regressions	106
2.A.7	Attrition-corrected fixed effects and first- and second-differenced earnings regressions	107
3.1	Pre-crisis distribution of trading and non-trading firms	119
3.2	Descriptive statistics of sample firms	120
3.3	Effect of exchange rate depreciation on firm value added	124
3.4	Effect of exchange rate depreciation on firm value added with additional controls – all firms	127
3.5	Effect of exchange rate depreciation on firm value added with additional controls – exporters	128

LIST OF TABLES

3.6	Effect of exchange rate depreciation on input use and productivity – all firms	130
3.7	Effect of exchange rate depreciation on input use and productivity – exporters	131
3.8	Effect of exchange rate depreciation on the import decision	132
3.9	Effect of exchange rate depreciation on firm survival	134
3.A.1	Effect of exchange rate depreciation on value added – interactions by year	137

Introduction

Developing countries are characterized by the extensive misallocation of labour and capital. The marginal rate of return to a unit of capital can vary substantially within the same economy, whether it be the wages paid to the same worker by different firms or the interest paid on loans by borrowers. Nor is it clear that individuals and firms are able to take advantage of high marginal rates of return where they exist: the investment in schooling in poor countries, as an example, remains surprisingly low (Banerjee and Duflo, 2005). A recent literature considers this heterogeneity of investment outcomes within the developing world as evidence of misallocation that is at least partly responsible for the failure of its convergence with the developed world. In this dissertation, I build upon that literature by exploring how the income generation process in a large emerging economy is affected by the allocation of labour and capital across temporal, sectoral and firm dimensions.

The reasons for misallocation are diverse and varied. Over the next three chapters, I consider the role played by government interventions that create legal barriers to movement across employment sectors, the absence of insurance markets for households to smooth consumption over time in the face of income shocks, and the presence of credit constraints for firms that do not have access to internal sources of finance. The context of my research is Indonesia: the world's fourth-most populous country and the eighth-largest economy by purchasing power (World Bank, 2014). Despite its size, it has received far less research attention than its Asian neighbours like China and India. A beneficiary of the global oil boom of the 1970s, Indonesia developed rapidly until the East Asian financial crisis of 1997-98, when, in a single year, GDP contracted by 13 per cent. This period of financial turmoil was associated not just with a large drop in output but, potentially, a fall in productivity as well

(Calvo et al., 2006), a phenomenon that has recently been related to the literature on the misallocation of resources (Oberfield, 2013).

The crisis severely affected the labour market, as the real median wage declined by 31 per cent in 1998 (Newhouse et al., 2010). In the absence of social safety nets and insurance markets, credit-constrained households reduced per capita spending by 23 per cent, and deferrable investments in health and schooling by 40 per cent in 1998 (Frankenberg et al., 2003). These large average declines led some researchers to caution of a “crisis in education” (Thomas et al., 2004), particularly since disruptions to schooling can have important implications for human capital formation and lifetime earnings.

At the same time, a large-scale transition of workers from the formal to the informal sector took place, reversing the process of structural transformation of the past three decades.¹ Following the recession, Indonesia entered a period of “jobless recovery” (Aswicahyono et al., 2010) – with an average annual decline in non-agricultural employment from 1999-2003, in stark contrast to other crisis-affected East Asian economies such as Thailand, Malaysia, Philippines, Vietnam and China (Newhouse et al., 2010). From 2003-07, non-agricultural job creation rebounded but has remained below pre-crisis levels with an average annual employment growth of 3.5 per cent, compared to 6.3 per cent pre-crisis (1990-1997). Organisations such as the World Bank and the OECD have suggested that increasingly rigid labour regulations introduced by the government since 2000 have created a segmented labour market, with barriers to entry into a high-wage formal sector, leading to a slowdown in the creation of better jobs (Newhouse et al., 2010; Organisation for Economic Co-operation and Development, 2011).

The 1998 crisis was also accompanied by the collapse of the financial sector, which cut off funding to many new projects. 64 of 237 banks were closed down (Claessens et al., 1999) and the value of credit extended by banks to the manufacturing sector fell by 50 per cent from 1996 to 2000 (Blalock et al., 2008). Firms without access to internal sources of finance were unable to take up high-return investments, leading

¹In 1970, the agricultural sector employed two-thirds of the work force and accounted for almost half of GDP; by 1995, agriculture employed 44 per cent of the work force and contributed only 17 per cent to GDP. After 1998, however, the pace of structural transformation has slowed. As of 2010, agriculture accounts for 38 per cent of the work force and 15 per cent of GDP, while manufacturing employs only 19 per cent of the workforce while contributing almost half of GDP.

to a skewed allocation of resources across the manufacturing sector, favouring larger firms with access to foreign funding. The worst affected firms were those reliant on imported intermediate goods, accounting for a quarter of all firms in the country, and an increasingly important part of the global production chain.

This dissertation comprises three chapters, each of which is presented as a distinct research paper, that explore these multiple dimensions of resource allocation during a period of uncertainty. In **Chapter 1**, I consider the effect of an economic crisis on the investment in human capital by credit-constrained households. While the adverse effects of idiosyncratic negative household income shocks on schooling outcomes are well documented, there is less clarity on the effect of an aggregate macroeconomic shock on both short-term and long-term human capital formation in developing countries. I examine this question in the context of an economy-wide recession and declining real wages that affect both household income as well as wage-earning opportunities for children in the labour market. Households face a drop in real income that leads to them spending less on all normal goods, including spending on schooling (the income effect of the shock). At the same time, the fall in real wages reduces the opportunity cost of schooling for children, since they would now earn less in the labour market, thereby encouraging them to stay in school (the substitution effect of the shock). The combined impact of the income effect and substitution effect of lower real earnings on the school and labour market participation of children is theoretically ambiguous. This is particularly the case in a country where children may be enrolled in school and working at the same time.

I estimate the impact of the 1998 East Asian crisis on children aged 7-15 years in Indonesia, by exploiting the heterogeneous impact of the recession across communities. The crisis led to steep declines in real wages across the country but with considerable variation across districts. I use movements in community rice prices, which are plausibly exogenous to individual households, as a measure of the shock to real wages in urban communities, which are net consumers of rice. I find that rice price increases lead to small declines in school enrolment and hours per week spent in school, and large declines in the labour market participation of children. For the youngest children (aged 7-12 years) there is a larger negative impact of higher rice prices on school attendance and a smaller effect on labour market participation. For older children (aged 13-15 years), the impact goes the other way: their schooling

enrolment does not respond to rice prices but their labour market participation declines sharply in the worst-hit communities. This is consistent with the narrative that older children are relatively more sensitive to the decline in the opportunity cost of staying in school, and are more likely to reduce the time they spend away from schoolwork. However, school enrolment does decline for younger children, who are less likely to find paying work.

I am also able to estimate the effects of the recession on long-term human capital formation. I find that children in the communities with largest price increases were more likely to have higher schooling attainment ten years later: they were more likely to have graduated junior secondary school, senior secondary school and college. Nor does the younger cohort exhibit any signs of lagging behind the older cohort, despite the larger short-term impacts of the shock. This supports the narrative that older children spent more time on schoolwork and less time on income-earning activities as a result of the crisis. Moreover, younger children were not adversely affected by the crisis because their lower school enrolment largely reflects delayed entry into the formal education system, rather than a severe disruption to schooling. This suggests that even in developing and emerging economies, sustaining household demand for schooling in the face of an aggregate crisis may be less of a concern than is commonly believed. Of greater policy interest may be the state's role in preserving funding for education, maintaining school quality and promoting vocation-specific training, which can help students find employment in the years that follow a deep recession.

In **Chapter 2**, I turn my attention to the potential misallocation of workers across different sectors of employment. Labour markets in most developing countries are characterised by the presence of a large informal sector. A well-documented stylised fact in these markets is the observation that workers in the informal sector earn significantly less than observably similar workers in the formal sector. Traditionally, this earnings gap has been related to the phenomenon of labour market segmentation, where some exogenous institutional barrier restricts the entry of informal sector workers into a high-wage, high-productivity formal sector that is not large enough to employ the entire workforce. The perceived existence of this barrier to entry has given rise to concerns that informal sector workers who are unable to transition into the formal sector may find themselves trapped in poverty for extended periods of time.

The concern with identifying average earnings differentials as evidence of labour market segmentation, however, is that individuals are heterogeneous in preferences and abilities, and may be selecting into types of employment at which they are relatively more productive. Another concern is that the apparent formal sector premium may be related to the observed strong correlation between firm size and worker earnings: workers in larger firms earn more than workers in small firms. I use a ten-year panel dataset on workers to investigate earnings differentials across sectors, conditional on observed human capital endowments and firm size. I exploit the panel dimension of the data to control for individual-specific time-invariant heterogeneity. My results suggest that it is only the public sector that commands an earnings premium over employment in other sectors. Given the underlying model for earnings, there is no evidence of any significant earnings differential between employment in the private formal sector, employment in the private informal wage sector and informal self-employment. I examine the potential biases arising from the endogenous sorting of individuals into employment sectors over time on the basis of time-varying unobserved characteristics. I address some concerns with respect to the potential for measurement error in the explanatory variable, and also account for the possibility of non-random attrition in the sample, which is a particular concern given the long length of the panel. Finally, I provide evidence on the labour market turmoil during the Asian crisis and its aftermath.

Similar developing country research on informality and firm size has focused primarily on Latin American and African countries; given the importance of the informal sector and self-employment in Asia, this is a critical gap in our understanding of developing country labour markets. This paper fills part of that gap in the context of Indonesia and argues against a sharp dichotomy between formal and informal employment. Instead, observed earnings differences across sectors can be explained by individual-specific heterogeneity and the firm size-productivity correlation.

Chapter 3 considers the allocation of capital across firms differentiated by their access to finance. The 1998 crisis was associated with a sharp depreciation in the rupiah as well as the collapse of the financial sector. Firms that relied on the import of intermediate inputs faced a large, unanticipated increase in their costs of production. The firms best placed to weather this period were foreign owned firms,

with continued access to credit lines, low external funding needs, and the ability to hedge against exchange rate movements.

I estimate the effect of the large exchange rate depreciation on the performance of importers. The characteristics and behaviour of importing firms has received little research attention even though over 40 per cent of the global trade in non-fuel goods is in the import of intermediate inputs used in manufacturing. The ability to manage volatility in the cost of imported inputs is likely to depend on a firm's access to external sources of finance, particularly trade credit, as well its ability to hedge against exchange rate movements. I examine this hypothesis using data from a census on Indonesian firms that spans the crisis period. I find that while domestic importers face lower value-added due to a rise in their costs of production, foreign-owned importers in Indonesia fare relatively better. Foreign-owned firms are more likely to continue importing, to use more materials, hire more labour and sustain higher value-added than domestic owned firms. These differences cannot be explained by differences in the probability that a firm survives the crisis. I conclude that foreign firms are better able to mitigate the effects of large cost shocks due to their access to finance as well as strategies to manage exchange rate risk. This finding is important because it identifies another channel through which FDI can benefit a developing country and sustain volumes of international trade during a financial crisis.

The analysis in all three chapters of the dissertation relies substantially on the use of relatively long-term panel datasets on individuals, households and firms. For Chapters 1 and 2, I use data from a unique ongoing longitudinal household survey. The Indonesian Family Life Survey (IFLS) collects information at the level of the individual, the family, the household, and the community on multiple dimensions of economic and social wellbeing. Starting with just over 7,000 households in 1993, it grew to cover over 13,500 households by 2007. The IFLS is unusual among developing countries in that it has enabled the creation of a 14-year old panel dataset with low rates of survey attrition.

For Chapter 3, I use data from the *Survei Tahunan Perusahaan Industri Pengolahan* (SI), which is an annual survey of all manufacturing plants in Indonesia that employ at least 20 workers. The data has been collected since 1975 by the *Badan Pusat Statistik* (BPS), the Central Bureau of Statistics of the Republic of Indonesia,

and is designed to be a census, or a complete enumeration of all manufacturing units in the country. This rich dataset collects detailed firm-level information on revenues, costs, capital, and investment, and enables the generation of an unbalanced panel dataset on over 22,000 manufacturing establishments with little sampling error.

The availability of detailed panel datasets on the individuals, households and firms of developing countries is a recent phenomenon, but one which has already had a significant impact on the quality of empirical microeconomic research. In particular, “[t]hese data have opened the eyes of the profession to the diversity and heterogeneity of economic life” (Heckman, 2001, p. 734), and have challenged many standard assumptions about microeconomic relationships along the way. The ability to control for unobserved heterogeneity in economic agents is vital, whether in evaluating public policy, assessing the effects of unanticipated external shocks, or modelling individual choice within a dynamic setting. In order to be able to answer questions significant to economic activity, more panel data needs to be collected, particularly from developing countries. In Indonesia, a fifth wave of the IFLS was fielded earlier this year, which will increase the length of the household panel to 22 years. The country’s central statistical agency, meanwhile, is currently upgrading its data collection infrastructure through an \$80 million project funded by the World Bank. These are welcome signs for economic researchers, and will hopefully offer opportunities to build upon the findings presented in this thesis.

Chapter 1

The impact of a macroeconomic crisis on child schooling outcomes

1.1 Introduction

Human capital is a critical determinant of lifetime earnings; consequently, a disruption to schooling in childhood can have important consequences for an individual's well-being. One important reason for schooling disruptions is household income volatility. Families that are unable to smooth consumption in the face of income shocks may reduce their expenditure on the schooling of their children, or reallocate the time spent by their children towards income-earning activities, rather than education. A second reason to drop out of school relates to available job opportunities for children, as well as the prevailing child wage rate, which measures the opportunity cost of staying in school. If the wage rate falls, children ought to have a greater incentive to stay in school.

I examine the short-run and long-run impact of a negative aggregate income shock on child schooling outcomes in a developing country. One would expect that a large economy-wide collapse in real wages would invariably have a negative effect on human capital formation, but the theoretical predictions of such shocks are ambiguous and the empirical evidence is also mixed. A recession is typically characterised by a large drop in real wages, causing household real incomes to decline, but also leading to a drop in the prevailing wage rate in available jobs for adults as well as children. The net impact of the economy-wide fall in real wages on schooling and labour decisions is ambiguous, and the long-run consequences of aggregate crises

might not necessarily lead to reduced levels of human capital attainment. In this paper, I exploit the heterogeneous impact of an aggregate shock across communities to quantify the impact of the crisis on the school and labour market participation of children across a medium-term horizon of three years. I consider children's schooling outcomes both at the extensive margin (school enrolment) and the intensive margin (hours spent in school), and I also consider their labour market participation. I focus on heterogeneous impacts on children by age in order to identify whether the decision to stay in school varies by the relative ability of the child to find employment. I also consider whether the effects of the crisis persist into adulthood.

I analyse these questions in the context of Indonesia, a fast-growing developing economy, which suffered a severe setback in the form of the East Asian financial crisis of 1998. The crisis was characterised by soaring inflation and an extraordinary drop in real wages of over 40 per cent over the course of a year (Frankenberg et al., 2003). High inflation was sparked off by the collapse of the Indonesian rupiah in January 1998 and rapid increases in the price of rice, the single most important expenditure item in the budget of the average Indonesian family. Net consumers of rice – a group which includes urban households – were particularly affected by declining real wages. However, there was considerable variation in rice price increases across the country, with urban households potentially facing the sharpest declines in real earnings in those communities where rice price increases were greatest. Using data from three waves of the Indonesian Family Life Survey (IFLS), I exploit the cross-community variation in the price of rice before and after the crisis to identify the within-community change in child schooling and labour outcomes in urban households as a result of the crisis. I also use data from a follow-up survey taken ten years after the shock to examine its long-term consequences on human capital formation.

This paper contributes to several strands of the literature on schooling investments. First, while there is a large literature on the impact of economic distress on investments in human capital in developing countries, much of the research relates to the impact of idiosyncratic household income volatility on child schooling and labour outcomes.¹ Many of these papers study household-level shocks such as crop loss or

¹See, for example, Jacoby (1994); Jacoby and Skoufias (1997); Jensen (2000); Beegle et al. (2006); Guarcello et al. (2010).

the household head losing a job, which are arguably not exogenous to household characteristics that affect decisions on human capital investment. By using regional variation in the effect of an unanticipated aggregate economic shock, I identify the within-community impact of income shocks using an instrument that is exogenous to the households living within these communities. Other papers with similar strategies include Edmonds and Pavcnik (2006), who use community variation in rice prices in Vietnam following a period of trade liberalisation to identify the effect of this policy change on child labour. Yang (2006) exploits the heterogeneous impact of the East Asian financial crisis on international immigrants from the Philippines, who were differently affected by the changes in the value of the peso depending on which country they were living in. Chen (2010) uses the differential impact of the East Asian financial crisis on public sector employees (who were particularly hurt by the erosion in real wages) and wetland rice farmers in Indonesia (who benefited from the rise in rice prices) to estimate the effect of a household income shock on religious intensity.

Second, the methodological strategy I use allows me to quantify the impact of an economy-wide aggregate shock, the theoretical effects of which are ambiguous, as opposed to a household-specific shock. In the the context of Indonesia, I find that while there is a small decline in schooling enrolment in response to the economic recession of 1998-99, there is an even larger decline in labour market participation in the worst-hit communities.

Third, I identify heterogeneity in the impact of economic distress across different ages and stages of schooling and find that for younger, primary school-aged children, schooling enrolment declined, while for older children, labour market participation declined in response to the crisis. This agrees with previous studies on Indonesia: Thomas et al. (2004) compare enrolment rates and changes in per capita consumption expenditure before and after the crisis and conclude that between 1997 and 1998, real household expenditure on education declined particularly in poor, urban households with more young children (10-14 years old), suggesting that older children were relatively better protected from the crisis at the expense of the young. In Brazil, Duryea et al. (2007) find that older children of about 16 are more likely to enter the workforce in response to the household head losing his or her job than younger children.

Finally, the long length of the dataset allows me to examine the long-term effects of the recession on schooling attainment over a ten-year horizon, which is unusual among developing country studies.² Given that children might increase the time spent on schoolwork due to the inability to find wage-employment, there may well be no adverse long-term consequences for human capital formation. I observe children ten years after the crisis and find no evidence of lower schooling attainment in Indonesia.

The rest of the paper is organised as follows: Section 1.2 provides the background to the crisis and the theoretical motivation for this paper. Section 1.3 describes the data. Section 1.4 presents the empirical framework and estimation results on the impact of the economic shock on children's schooling outcomes. Section 1.5 considers the long-term impact of the economic shock on schooling. Section 1.6 concludes.

1.2 Background and theoretical motivation

1.2.1 Background to the Indonesian crisis

In the second half of 1997, the East Asian currency crisis erupted as international investors launched a speculative attack on the Thai baht. The contraction of credit markets put the currencies of neighbouring countries under pressure as well, and in January 1998, the Indonesian rupiah fell from Rp 4,800/\$ to above 16,000/\$, plunging Indonesia into a deep and long-drawn economic crisis. While the real exchange rate gradually appreciated over the next two years, in mid-2000 it was still 40 per cent lower than its mid-1997 level. GDP contracted by 13.1 per cent in 1998 and marginally recovered the next year to 0.8 per cent (World Economic Outlook Database, 2015). Real wages were badly hit; Frankenberg et al. (2003) estimate that real wages fell by upto 40 per cent in the market sector in 1998, and, consequently, household per capita spending was reduced by 23 per cent. They find that much of this reduced spending was at the expense of deferrable investments in human

²Two other papers that use long-term panel data from developing countries are Beegle et al. (2009), who estimate the effect of child labour on schooling outcomes in Vietnam over a five-year horizon, and Krutikova (2010), who identifies the impact of household income shocks on the schooling distribution in Tanzania over a 10-13 year horizon.

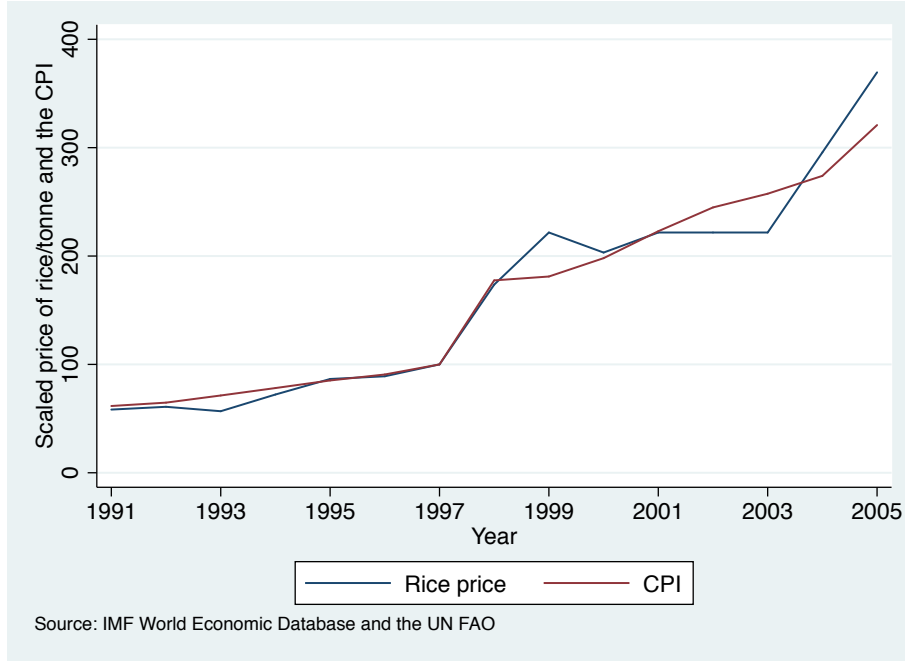
capital: household spending on both health and education declined by close to 40 per cent in 1998.

This deep contraction was accompanied by high rates of inflation. Starting in January 1998, the prices of commodities began to soar, with inflation reaching 80 per cent by the end of 1998 (International Financial Statistics, 2015). Soaring inflation was driven by the sharp fall in the rupiah as well as the removal of subsidies on essential goods such as rice and fuel. The price of food, of which rice comprises the largest part, increased by 120 per cent over the next two years (Food and Agricultural Organization of the United Nations, 2015). Figure 1.1 depicts the sharp and sustained increase in rice prices and the CPI between 1997 and 2007. Frankenberg et al. point to the doubling of the price of rice in the year following the crisis as the main reason for the increase in CPI, since rice expenditure is a large part of the total budget of the average Indonesian family. Thomas and Frankenberg (2007) provide evidence of the rising share of food expenditure, driven by increased expenditure on rice, in the total household budget; this leads, in turn, to reduced spending on non-food items, including health and education. The rise in rice prices, therefore, is an important measure of the extent of the crisis for Indonesian consumers that were net consumers of rice – a rise in the real price of rice is a terms of trade movement against these households.

However, there was significant variation in the increase in rice prices across communities that took place after the start of the 1998 crisis. Levinsohn et al. (2003) point out that geography was the most important determinant of rice price increases between the start of the crisis and the end of 1998, and document large inter-provincial variation in the increase in rice prices in this period (120 to 280 per cent). They discuss some of the reasons for the variation in inflation and commodity prices at this time and conclude that regional inequalities in infrastructure and transport were particularly critical in driving regional disparities in inflation. A similar phenomenon is documented by Frankenberg et al., who point out that labour and product markets are unlikely to clear across all regions of a 13,000 island archipelago, where transport costs are likely to be high.

Identifying the causes of the breakdown of price integration in Indonesia is beyond the scope of this paper. However, the observed rise in commodity prices after 1998 is consistent with a seminal model of spatial market integration – the

Figure 1.1: CPI and rice prices in Indonesia (1991-2005)



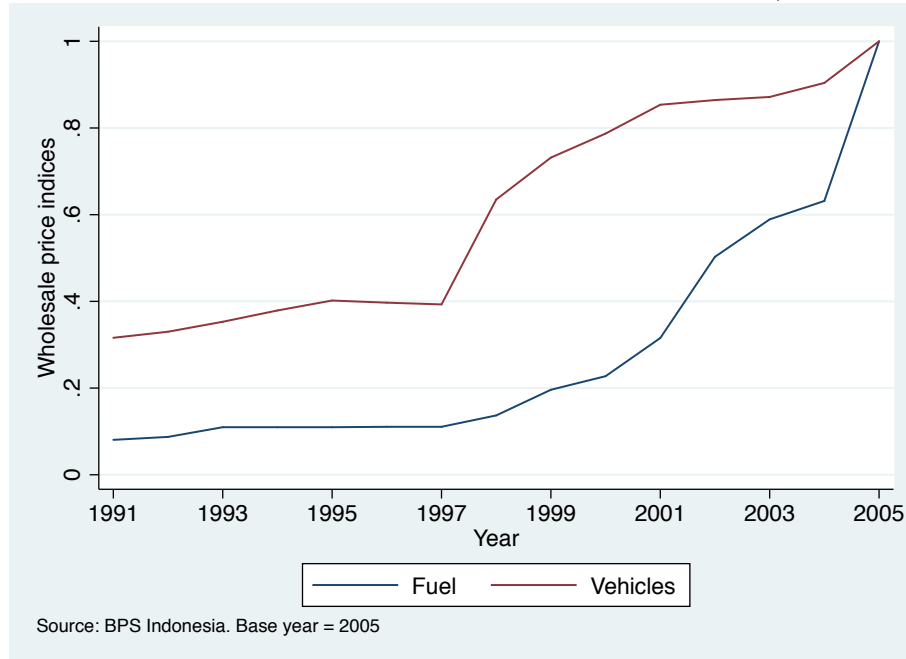
Enke-Samuelson-Takayama-Judge spatial equilibrium model – which establishes the bounds for the dispersion of prices of identical goods across spaces. Following the notation in Barrett (2001), this model can be represented by the following equations:

$$\begin{aligned}
 p^i &= p^j + t & \text{if } q &\in (0, q^*); \\
 p^i &\leq p^j + t & \text{if } q &= 0; \\
 p^i &\geq p^j + t & \text{if } q &= q^*
 \end{aligned} \tag{1.1}$$

where p^i and p^j index prices in locations i and j , q is the quantity traded between the two locations, with q^* being the maximum value that the traded quantity can take, and t is the transactions cost of trading the good between the two locations. At the height of the Indonesian crisis, fuel subsidies were eliminated and the price of fuel rose by 70 per cent. Figure 1.2 shows a sharp rise in the wholesale price index for fuels and for motor vehicles in 1998. These increases would have had the effect of a rise in the transactions costs of trade, t , widening the bands of price dispersion, and increasing the variation in prices across regions.

Varela et al. (2012) find that there was, in fact, an increase in the regional variation of prices during the crisis period (Figure 1.3). They also find that higher

Figure 1.2: Wholesale price indices for fuel and vehicles (1991-2005)

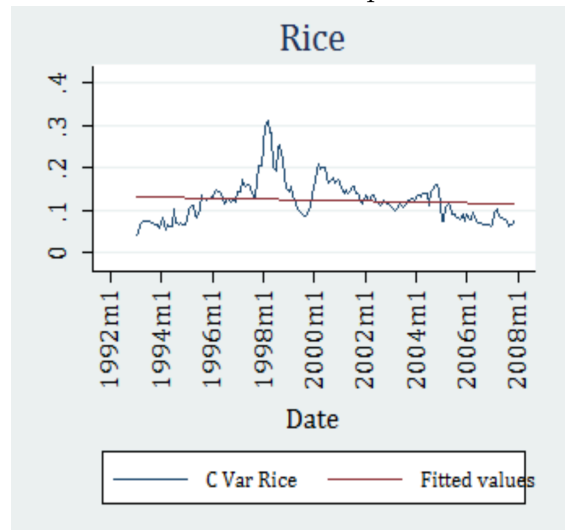


prices are associated with availability of infrastructure and accessibility. It is this community-level variation in rice price increases that I exploit to identify the within-community impact of the economic shock on child schooling and labour outcomes, with added controls for changes in infrastructure and accessibility.

1.2.2 Theoretical motivation

In a simple model of educational choice, households invest in child education in expectation of higher future earnings. The opportunity cost of investing in schooling is measured both by the explicit costs of schooling and the wages a child without schooling would earn by entering the labour market instead. In the context of recession marked by declining real wages, we would expect that households face both a shock to real income, as a result of which they consume less of all normal goods, including expenditure on schooling. At the same time, children face lower real wages, reducing the opportunity cost of staying in school. In the presence of complete credit markets, households would theoretically be able to smooth spending on education and only the substitution effect or the price effect of a drop in real wages would

Figure 1.3: Rise in coefficient in variation of prices in 1998 (Varela et al., 2012)



operate. However, with incomplete credit markets, as in most developing economies, both the income effect and the substitution effect of falling real wages would affect the schooling decision. The net impact of an aggregate shock on households on school attendance would, therefore, be theoretically ambiguous.

Empirical research in developed countries suggests that the impact of an aggregate shock leads to an *increase* in school enrolment, due to fewer available jobs and lower real wages. For example, Goldin (1999) points out that the “one positive effect” of the Great Depression in the United States was that it induced young people to stay in school; US states that saw the highest increase in unemployment, particularly due to the loss of jobs in the manufacturing sector, were those that saw the greatest increase in school enrolment over the 1930s. Using more recent data from the United States, Betts and McFarland (1995) find that a one per cent increase in the unemployment rate is associated with a four per cent increase in enrolment in community colleges. In developing countries, the evidence is less clear. Binder (1999) models the relationship between changes in school participation and changes in GDP to estimate the income-elasticity of schooling, using data across two recessions in Mexico in the 1980s. She finds that the positive effect of schooling enrolment of lower wages cancelled out the negative effect of schooling enrolment of lower incomes, leading to largely stagnant school enrolment rates during this period.

Duryea and Arends-Kuenning (2003) exploit regional variation in the decline in real wages in Brazil between 1977 and 1998 to identify the effect of local labour market conditions on schooling enrolment and find that the income effect of lower wages is largely offset by the substitution effect of lower wages. McKenzie (2003) uses a differences-in-differences approach to compare school enrolment increases before, during and after the 1994-95 Mexican recession to find that school enrolment increased during the recession year, due to the falling opportunity costs of schooling. Schady (2004) considers human capital investment during the deep 1988-92 recession in Peru. He identifies the determinants of school enrolment and child labour by using within-district variation across school-going and non school-going cohorts to find no evidence of a drop in school enrolment and some evidence of a drop in child labour. As a result, children exposed to the crisis were more likely to have higher schooling enrolment, four years later.

While I cannot disentangle the income and substitution effects of a fall in real wages with the Indonesian data, I do consider the impact of the rice price shock on both schooling participation as well as labour market participation. I also compare outcomes between children in primary school (aged between 7 and 12) and in junior secondary school (aged between 13 and 15). The distinction is important because 13-15 year-olds face higher opportunity costs of schooling, due to their ability to find employment, and are likely to be more sensitive to the price effect of a decline in real wages, compared to the younger children.

Another factor that affects the schooling decision is a change in school quality. As has been pointed out by Ferreira and Schady (2009), if the aggregate shock causes the state to reduce funding to education, resulting in lower-quality services, the household demand for schooling might decline. I address this concern in the empirical framework in Section 1.4. Further, if the returns to schooling change permanently as a result of the crisis, the demand for schooling would also be affected. This might be a concern in the Indonesian context, where the recession was associated with the loss of jobs in urban areas, particularly in relatively skilled work such as in the financial sector. A permanent fall in the return to schooling may be accompanied by a decline in the household demand for schooling. In this paper, I do not account for this effect.

The theoretical predictions would potentially vary across different types of consumers and producers; in particular, the increase in rice prices could be expected to benefit net rice producers. Thomas and Frankenberg (2007) provide some evidence that rice producers in Indonesia were better insulated against the crisis due to favourable movements in relative prices in 1998.³ Were both the relative price of rice and net production of rice to increase, net producers of rice would experience an income effect leading to higher spending on normal goods such as human capital investment. Since rice farming is a labour-intensive activity, there is also a predicted substitution effect leading to a reallocation of children's time from schooling to labour, due to the increase in the value of rice production. The effect on child school participation would be theoretically ambiguous but the operating effects are very different compared to net consumers of rice. In order to identify more precisely the effect of a terms of trade movement against households and the adverse effects of the prolonged recession, I focus on urban households only, which do not produce rice and can only be adversely affected by a sudden increase in the price of a commodity of which they are net consumers.

Finally, since the data analysis compares outcomes in 1997 and 2000, I cannot examine short-run household responses to the recession; instead, I focus on the medium-term impact of a sustained period of low real earnings on household investment in schooling and labour market participation.

1.3 Data

1.3.1 Indonesian Family Life Survey

The data I use is from the Indonesian Family Life Survey (IFLS), an ongoing longitudinal socio-economic and health survey that collects information at the level of the individual, the family, the household, and the community on a wide range of topics, including those relevant to this paper – economic well-being, educational attainment and community-level rice prices and other characteristics. The survey is based on an initial sample of 7,224 households (approximately 22,000 individuals)

³However, this benefit was somewhat mitigated by the occurrence of El Niño-related droughts and severe forest fires that led to the destruction of crops in late 1997 and 1998.

living in 13 of Indonesia's 26 provinces in 1993, and representing 83 per cent of the Indonesian population in that year. These IFLS-1 households were subsequently re-contacted in 1997, 2000 and 2007.⁴ Since the IFLS tracks individuals who split off from original survey households, the number of surveyed households has increased with every wave, with 13,535 households (approximately 44,100 individuals) interviewed in 2007.

The identifying variation in the empirical analysis is community-level variation in the price of rice between 1997 and 2000. The level at which the community is defined is the district, or *kabupaten*: this unit is small enough to allow for sufficient variation in rice prices across communities but large enough that the price of rice is exogenous to individual households living within the community. The sample contains 124 such communities that were surveyed in 1997 and 2000. The IFLS collects community-level data at the level of the enumeration areas (*kelurahan* or *desas*); I aggregate the information collected at each enumeration area – each roughly the size of a census block – up to the level of the district.⁵

The first part of this paper confines itself to the study of 3,156 children between the ages of 7 and 15 (inclusive) in 1997 and 3,317 children in 2000, living in the 124 urban panel communities.⁶ The second part of this paper examines 2,942 children (of the original 3,156 children surveyed in 1997) who lived in panel households that were subsequently surveyed in 2007, to examine the impact of the recession on the long-term educational attainment of children. The Indonesian schooling system is organised into basic, secondary, and higher education. Basic education is a nine-year programme of free, compulsory schooling for all children between the ages of 7 and 15, consisting of elementary school (six years, between the ages of 7 and 12) followed by junior secondary school (three years between the ages of 13 and 15). At the end of basic education, students who have successfully completed a national examination may enter senior secondary school (three years) through either an academic or a

⁴The data for the first wave of the panel was collected between August and November 1993, and between September 1993 and January 1994. The data for the second wave was collected between August and December 1997. The third wave of data was collected between June and October 2000. The fourth wave of data was collected between November 2007 and April 2008. Information on all four waves can be found in Frankenberg and Karoly (1995); Frankenberg and Thomas (2000); Strauss et al. (2004, 2009).

⁵Each district contains 6 enumeration areas, on average.

⁶An average of 113 children are surveyed in each community in a given survey round.

vocational track. After completing the senior secondary national exam, students may enter higher education at universities, academies or colleges. The students selected for this analysis are of the age where they ought to be enrolled in basic education.

Surveyed families are asked a number of questions on socio-economic and demographic characteristics of the household as well as on the economic activities of the household head. Information is collected on the schooling outcomes for children – whether they are enrolled in school in the present year, how many hours they spent in school in a typical week, and the level and grade in which they are currently enrolled. Information is also collected on whether a child has been employed in the past year.

At the community level, rice prices are collected from multiple sources in the survey: households are asked how much they pay for a kilogramme of rice, the head of the village or community women’s group is asked about the prevailing price of rice, and rice prices were collected directly by the surveyor from three sales outlets in each community. I use the rice prices calculated from the average of the prices at the three outlets and the head of the community women’s group (an average of between 1 and 4 data points). I ignore the household reported prices, as these are not exogenous to the household. The price of rice is then deflated by the national consumer price index to reflect prices in Indonesian rupiahs in December 1997.⁷ The head of the community women’s group is also asked a number of questions about the infrastructure available in the community, as well as the implementation of social security programmes in the area. Community schools are directly surveyed about the block grants they received from the government between 1997 and 2000.

1.3.2 Overview of the data and summary statistics

Summary statistics for the outcome variables and other child characteristics are presented in Table 1.1. The children surveyed in each year have an average age of 11 and approximately half the surveyed children are girls. School enrolment rates are reasonably high for developing countries, at 95 per cent for the younger age group (7-12 years old) and 83 per cent for the older age group (13-15 years).

⁷Data on consumer price indices are from the BPS, Indonesia.

School enrolment declines marginally on average between survey years but a high standard deviation suggests considerable variation in school-enrolment rates across communities. Average hours spent in school every week declines to a greater extent from 31 hours per week to 25 between 1997 and 2000. Younger children see a larger decline in school hours per week (29 hours to 22 hours); they also spend less time in school compared to older children (for whom hours per week spent in school decline from 35 hours to 32 hours). The labour market participation of school-aged children climbs sharply between the years, particularly for the older age group. In 1997, only 2 per cent of children were reported as being employed in either family businesses or in a wage-paying job; this increased to 10 per cent in 2000. 18 per cent of children aged 13-15 years reported themselves as being employed in 2000. However, it is important to note that is just an average estimate, with considerable variation around the mean across different communities.

Table 1.1: Child characteristics and schooling outcomes

	All children		7-12 yr-olds		13-15 yr-olds	
	1997	2000	1997	2000	1997	2000
Age	11.41 (2.62)	11.16 (2.66)	9.60 (1.76)	9.46 (1.69)	14.04 (0.80)	14.10 (0.81)
Male	0.50 (0.50)	0.50 (0.50)	0.52 (0.50)	0.50 (0.50)	0.48 (0.50)	0.50 (0.50)
Enrolled in school	0.90 (0.30)	0.88 (0.32)	0.95 (0.21)	0.94 (0.24)	0.83 (0.38)	0.79 (0.41)
School hours/week	31.73 (6.81)	25.70 (12.71)	29.71 (6.92)	22.76 (12.08)	34.96 (5.19)	31.27 (12.01)
In labour market	0.02 (0.16)	0.10 (0.30)	0.01 (0.09)	0.05 (0.22)	0.05 (0.22)	0.18 (0.39)
Observations	3,156	3,317	1,865	2,100	1,291	1,217

Notes: Sample includes children aged between 7 and 15 surveyed in 124 panel communities in 1997 and 2000. Values reported are sample means with standard deviations in parentheses.

Socio-economic and demographic characteristics of households and communities are presented in Table 1.2 on page 15. More than 70 per cent of the households in the survey live in owned homes in families of an average size of six. Household heads are usually male (90 per cent), employed (90 per cent) and almost always

married (99 per cent). The level of schooling of the household head is not very high with 60 per cent not having progressed beyond primary school. More importantly, household characteristics do not change substantially between the two survey years. The average schooling of the household head is lower in 2000, compared to 1997; with a few families with a college-educated household head dropping out of the sample.

In terms of community characteristics, some of the communities have seen infrastructure improvements over the three-year period, particularly with an increase in the availability of local transport services, markets and public telephones. However, there was a sharp drop in the number of local banks, due to the adverse impact of the crisis on the domestic financial sector. In response to the crisis in 1998, a number of social programmes targeted at the poorest 10 per cent of the population were rolled out in some communities: these are explained in detail in Section 1.4. About 51 per cent of the communities also reported suffering El Niño-related forest fires or drought.

The key source of variation that I exploit is in the price of rice. Figure 1.4 on page 16 shows the extent of variation in the real price of rice across communities. On average, rice prices increased by 92 per cent in this period; however, there was considerable variation across communities, with a standard deviation of 33 per cent. Figure 1.5 on page 17 depicts the location of the urban IFLS communities as well as the extent of rice price increase in each community. While some provinces such as South Sumatra and West Java are associated with particularly high price increases, there is price variation even within provinces and island groups.

1.4 The impact of rice price increases on schooling outcomes

1.4.1 Empirical framework

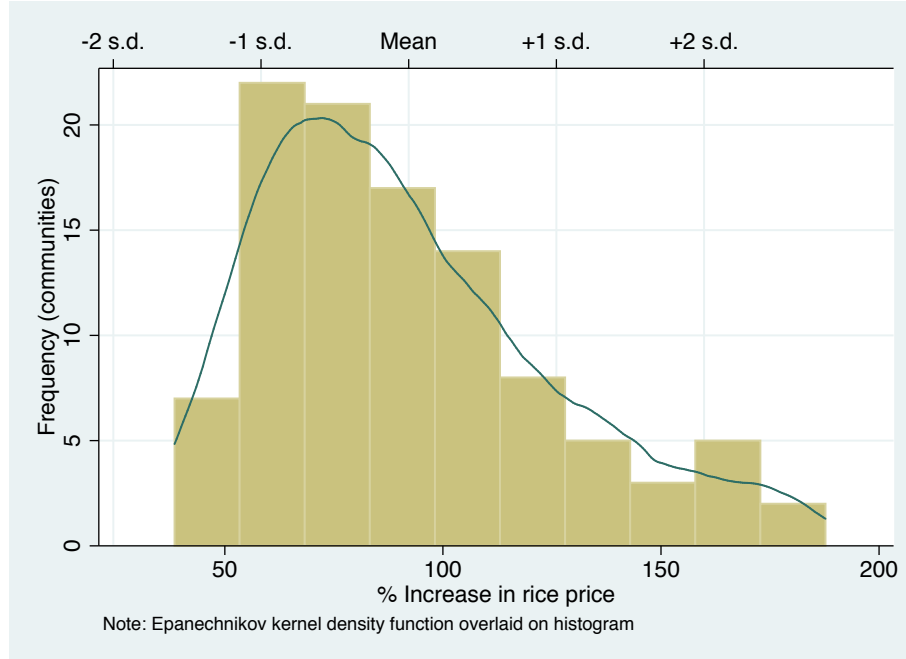
The descriptive statistics presented in the last section reveal that school enrolment and attendance were both lower in 2000, while labour market participation was higher, compared to 1997, but that these average estimates potentially concealed a

Table 1.2: Household and community characteristics

	1997	2000
<i>Household characteristics</i>		
Owns home	0.72	0.75
Modernity index (0-5)	3.56	3.67
Household size	5.94	5.82
Number of children<15 yrs	2.73	2.69
Number of children enrolled in school	2.30	2.22
Number of elders>70 yrs	0.21	0.30
<i>Household-head characteristics</i>		
Age	45.00	45.27
Male	0.89	0.87
Married	0.99	0.99
Employed in past year	0.90	0.89
Employed in public sector	0.14	0.13
Highest schooling level		
Primary or less	0.52	0.60
Jr. secondary	0.17	0.16
Sr. secondary	0.23	0.24
College	0.09	0.00
<i>Community infrastructure (=1 if available)</i>		
Local bus stop	0.05	0.27
Market	0.16	0.22
Public telephone	0.45	0.65
Post office	0.09	0.18
Bank	0.22	0.04
Asphalt roads	0.72	0.80
<i>El Niño shock (=1 if affected)</i>		
Shock in 1998		0.53
<i>Community social programmes (=1 if implemented)</i>		
Medical services		0.17
Rice distribution		0.70
Health cards		0.45
Employment generation		0.62
Food supplements		0.63
Student scholarships		0.79

Notes: Sample includes 124 panel communities surveyed in 1997 and 2000. Values reported are sample means with standard deviations in parentheses. All Indonesian rupiah values are deflated to December 1997.

Figure 1.4: Variation in rice price increases across communities (1997-2000)



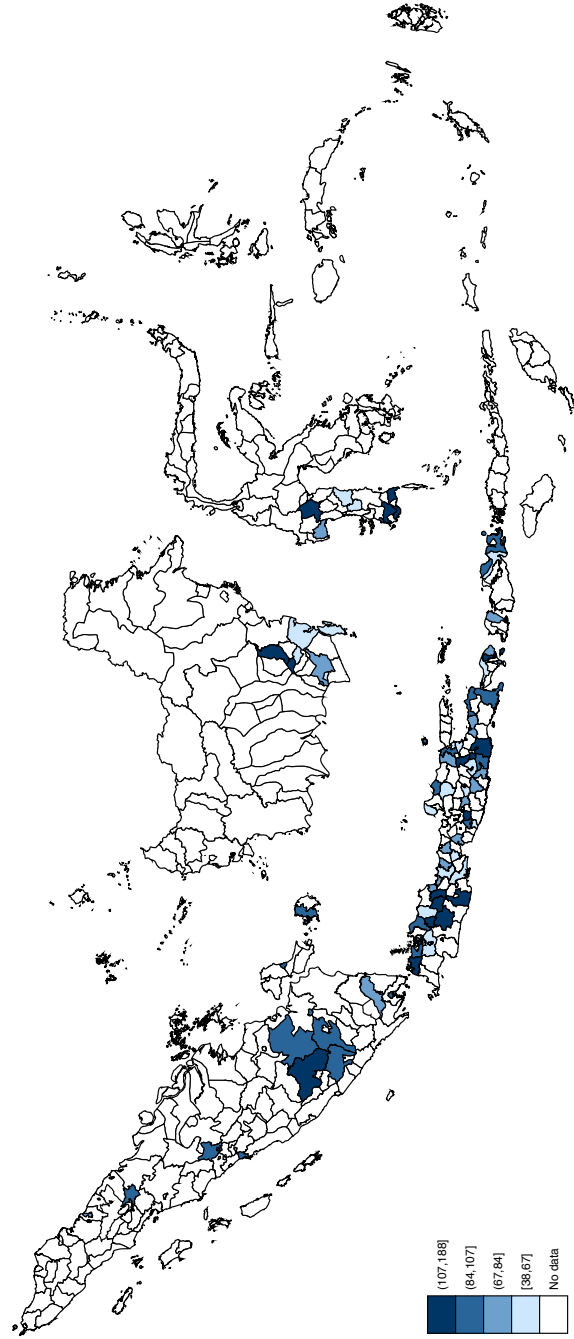
lot of heterogeneity across the sample communities. This section examines the relationship between these outcomes and the terms of trade movement induced by rising real rice prices within communities, in a parametric regression framework. The observed variation in the increase in rice prices gives us variation in the extent to which different urban communities are affected by declining real wages. The variation in rice prices by community is exogenous to households because it is very unlikely that individual households could affect district-level prices. The basic regression equation is the following:

$$y_{ijt} = \beta \ln P_{jt} + \mathbf{X}_{ijt}' \boldsymbol{\gamma} + \lambda_j + d_t + \varepsilon_{ijt} \quad (1.2)$$

where y_{ijt} is the outcome of interest pertaining to child i living in community j at time t , where t is either 1997 or 2000. I consider three outcomes related to schooling – an indicator for whether the child is enrolled in school at time t , the number of hours per week spent by the child in school, and an indicator for whether the child has worked in the past year. $\ln P_{jt}$ is the natural logarithm of the real (deflated) price of one kilogramme of rice in community j at time t .⁸ $\mathbf{X}_{ijt}$ includes child-

⁸I use logs of the real rice price so that the estimated coefficients are easier to interpret as changes to the outcome variable in response to a 1 per cent increase in the real price of rice.

Figure 1.5: Map of rice price increases (%) across communities



specific characteristics that affect schooling outcomes – an indicator for whether the child is male and a quadratic function in age. I also include a control for the season in which the interview took place (an indicator for whether it was the wet season, as opposed to the dry season). Finally, Levinsohn et al. (2003) point out that there was substantial price variation across Indonesia’s 27 provinces in the aftermath of the financial crisis; finding that product prices tend to covary negatively across provinces. In order to isolate β from province-specific heterogeneity, I include interacted province-year indicators in $\mathbf{X}_{ijt}$. λ_j is a community fixed effect, d_t is a year effect captured by a dummy equal to 1 if the year is 2000, with 1997 as the omitted year, and ε_{ijt} is an error term.

β is the estimated coefficient of interest, which measures the impact of rice prices on school attendance. If the income effect from a fall in real wages dominates the substitution effect, children will drop out of school as a result of the decline in real wages: β will be negative. If the substitution effect dominates the income effect, more children will stay in school as a result of the lower real wages and a lower opportunity cost of schooling: β will be positive. If the income effect and the substitution effect balance each other out, β will be insignificantly different from zero.

Since the equation includes community fixed effects, β is estimated from the within-community variation in the price of rice between 1997 and 2000, and measures the increase in the probability that a child is enrolled in school associated with a 1 per cent increase in the real price of a kilogramme of rice within the community. Since rice prices vary across years as well as communities, standard errors are clustered by community of residence. The identifying assumption for β is that if rice prices had not changed between 1997 and 2000, the probability that a child in a certain community is enrolled in school would remain unchanged across communities.

1.4.2 Results

Table 1.3 presents the first set of results. Column 1 shows the estimation of Equation (1.2), which includes controls for child characteristics (functions of age and gender),

However, I have estimated all the results in this chapter with the absolute value of real rice prices as the main control variable as well (results not shown). The inference remains unchanged.

season effects and province-level variation. I find a negative association between school enrolment and rice prices. A one standard deviation increase in the real price of rice (18 per cent) reduces the probability that a child is enrolled in school by approximately 2 percentage points. A two standard deviation increase in the real price of rice (36 per cent) reduces the probability that a child is in school by approximately 3 percentage points. These effects are significant but are not large; indeed, they seem much smaller than the effects observed by Thomas et al. (2004) for 1998, suggesting either a rapid return to school between the worst of the crisis in 1998 and 2000 or an overestimate of the impact on schooling as a result of the recession.

Table 1.3: Impact of rice price increases on school enrolment

	(1)	(2)	(3)
Ln rice price	-0.103*	-0.103	-0.105*
	(0.061)	(0.077)	(0.060)
Year = 2000	0.036	0.006	0.008
	(0.033)	(0.049)	(0.042)
Season indicators	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes
Community social programmes	No	Yes	Yes
Infrastructure improvements	No	Yes	Yes
El Niño shock	No	Yes	Yes
Household characteristics	No	No	Yes
Community fixed effects	Yes	Yes	Yes
Observations	6,473	6,473	6,473
<i>Adj.R</i> ²	0.143	0.143	0.305
p-value (equal coefficients)(2)	0.997		
p-value (equal coefficients)(3)	0.960		

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is an indicator for whether a child is enrolled in school for the given academic year. Standard errors are in parentheses, clustered by community.

One methodological concern is that communities are likely to differ in a range of ways that affect both local rice prices as well child schooling outcomes, potentially biasing the estimates of β . The inclusion of community fixed effects identifies the effect of rising rice prices on schooling-related outcomes from within-community variation. As such, the estimation of Equation (1.2) controls for time-invariant community characteristics. However, changes in community-related characteristics

between 1997 and 2000 could arguably take place in such a way so as to affect rice prices as well as child schooling outcomes. The first source of time-varying community-level variation is the accessibility of the community. Frankenberg et al. (2003) point out that a lot of the variation in prices across Indonesia is explained by the geography of this island nation and considerable variation in the provision of local transport infrastructure. I control for changes in the accessibility of a community by including indicators for whether a community has its own public transport stop, market, post office, public telephone service, bank and asphalt roads. This data is collected in each of the survey years and the inclusion of these covariates isolates the impact of rice price variation on schooling outcomes from changes in community-level infrastructure that have taken place in the same time period.

A second source of community-level variation is the possibility of varying intensities across communities of social safety net programmes introduced during the economic crisis. As the extent of the downturn become clear, the central government introduced a number of social programmes, known as the JPS (*jaring pengaman sosial* or ‘social safety net’) programmes, targeted at the poorest individuals in any community, to mitigate the effects of the sharp drop in economic welfare. The JPS programmes, introduced in 1998, included the following: an assortment of employment generation schemes (*padat karya*) in government departments to bolster purchasing power; the provision of subsidised medical services and food supplements; extension of scholarships and block grants to students and schools; and the sale of subsidised rice to target households. The coverage of these programmes varied greatly – subsidised rice was targeted at 15 per cent of the population, while the coverage of the school scholarships and employment creation programmes was targeted at 8 per cent of the population.⁹ The plans were designed and implemented by what was then a powerful central government, with little scope for variation in execution of the schemes at a local level. Nonetheless, one might expect heterogeneity across communities in how the schemes were implemented. To control for this I include indicators for whether one of six schemes were implemented in the district (employment generation, health card distribution, subsidised medical services,

⁹Under the subsidised rice programme, 10 kgs of rice per month was sold at a subsidised rate to eligible poor households. Since this amount was well below monthly consumption needs for the average household, the scheme acted as an income transfer programme for the poorest (Sumarto et al., 2002).

provision of food supplements, educational scholarships and the sale of subsidised rice).

A related concern is the possibility that changes in school quality varied substantially across districts between 1997 and 2000 due to the varying extents to which the crisis may have affected government funding to education in different areas. This is addressed in three ways: first, the funding to schools is assigned at the province level, and I include controls for province-year variation. Second, an indicator for whether schools in the district received block funding from the central government in the previous year to pay for student scholarships has been included, which provides a blunt estimate of the extent to which funding to the schooling system had been sustained. Third, the community surveys collect information on the actual amount of block funding received by the schools in the district from the government in the previous year. The rupiah value of financial assistance to schools in a district better measures changes to the quality of schools at the district-level, assuming the receipt of funding is correlated with school quality. I separately include both the district-level block funding indicator and block funding value variables interacted with year as controls in the schooling regressions (tables not reported). None of the empirical results are sensitive to the inclusion of the funding variables.

A third source of community-level variation comes from El Niño-related forest fires and droughts that affected certain regions in Indonesia in 1997 and 1998. The effect of these natural disasters was to destroy crops, potentially leading to higher prices. While this makes it difficult to isolate the effect of rice price changes as a response to the financial crisis of 1998, the effect of higher prices on schooling outcomes can be still be tested. I control for whether a community suffered a forest fire or a drought by including the interaction of indicators for the occurrence of a shock with a year indicator. This would control for time-varying changes in a disaster-affected community that could potentially affect child schooling outcomes.

Column 2 of Table 1.3 presents estimates of Equation (1.2) with additional controls for community time-varying characteristics, including community social programmes implemented after 1997, infrastructural developments after 1997 and El Niño-related natural calamities. The results are within a 99 per cent interval of the results without these controls, suggesting that community time-varying charac-

teristics are not a source of spurious correlation between school enrolment and rice prices.

One reason for selecting the district as the relevant community is to select an area that is large enough to ensure that commodity prices are exogenous to the household. It is extremely unlikely that individual households could affect district-level prices. Nonetheless, as a robustness check, I control for observed household characteristics by including a number of covariates: family characteristics (whether the household lives in its own house, the size of the household, the number of children in the house under the age of 15, the number of children under 15 enrolled in school, the number of old people in the house aged above 70, an index of modernity¹⁰) and household head characteristics (age of the household head, dummies indicating if the household head is male, married, employed, employed in the public sector, as well as education dummies for his or her highest level of schooling – no or some elementary school, junior secondary school, senior secondary school, college). To control for unobserved household characteristics that could be correlated with both schooling outcomes and rice prices, I also estimate Equation (1.2) with household fixed effects, using children in panel households that were surveyed in both 1997 and 2000. These results are presented in Section 1.4.5.

Column 3 presents estimates of Equation (1.2) that further includes household characteristics, in addition to all the controls in the Column 2 model. The estimates of β with added community-level and household-level characteristics are negative and within the 95 per cent confidence interval of the Column 1 estimates, suggesting that spurious correlation from observed household characteristics is not a serious concern and that the observed rice price shock was exogenous to individual households.

Table 1.4 shows estimates of the impact of increased rice prices on the intensive margin of school attendance; that is, estimates of Equation (1.2) where the dependent variable is hours spent in school by children every week.¹¹ Variables are added

¹⁰The modernity index takes integer values between 0 and 5 and is calculated, following Chen (2010), by adding together dummies for whether the household has access to electricity, television, a fridge, a stove and its own toilet.

¹¹In calculating the hours per week spent in school by a child, I set the variable equal to zero for all students who are not in school. Therefore, the effect on hours per week at least partly captures the small effect of the crisis on the reduction in school enrolment, and is not purely an intensive

to the estimating equation sequentially, as in Table 1.3. There is a statistically significant decline in hours per week spent in school, but the addition of time-varying community characteristics reduces the size of the estimated β . For a one standard deviation (18 per cent) increase in the real price of rice, students spend 2.7 fewer hours per week in school, which is a 9 per cent decline over the 1997 average of 30 hours per week in school. A two standard deviation (36 per cent) increase in the price of rice leads to students spending 5 fewer hours per week in school, a 16 per cent decline over the 1997 average. This effect is larger than on the extensive margin, particularly in high-inflation communities. The combination of high school enrolment rates but lower effective attendance suggests that households may have continued to enrol children in schools but were less likely to ensure the same level of attendance.

Table 1.4: Impact of rice price increases on hours in school

	(1)	(2)	(3)
Ln rice price	-16.143*** (3.278)	-10.589*** (3.225)	-10.682*** (3.153)
Year = 2000	-5.161*** (1.590)	-14.297*** (2.783)	-14.116*** (2.757)
Season indicators	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes
Community social programmes	No	Yes	Yes
Infrastructure improvements	No	Yes	Yes
El Niño shock	No	Yes	Yes
Household characteristics	No	No	Yes
Community fixed effects	Yes	Yes	Yes
Observations	6,473	6,473	6,473
<i>Adj.R</i> ²	0.207	0.217	0.304

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the average number of hours spent in school per week. This is set to zero for students who do not attend school. Standard errors are in parentheses, clustered by community.

Finally, Table 1.5 presents estimates of the impact of rice price increases on labour market participation of the surveyed children. Participation in labour declines in response to higher rice prices, suggesting that children were less likely to

find employment in communities that faced declines in real wages. A one standard deviation (18 per cent) increase in the real price of rice leads to 3 percentage point decrease in the labour market participation of children; a two standard deviation (36 per cent) increase in the price of rice leads to a 5 percentage point decrease in the labour market participation. However, in this instance, time-varying community characteristics do seem to matter in influencing the labour market participation of children.

Table 1.5: Impact of rice price increases on labour market participation

	(1)	(2)	(3)
Ln rice price	-0.077* (0.045)	-0.166*** (0.042)	-0.170*** (0.040)
Year = 2000	0.118*** (0.024)	0.179*** (0.026)	0.178*** (0.025)
Season indicators	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes
Community social programmes	No	Yes	Yes
Infrastructure improvements	No	Yes	Yes
El Niño shock	No	Yes	Yes
Household characteristics	No	No	Yes
Community fixed effects	Yes	Yes	Yes
Observations	6,473	6,473	6,473
<i>Adj.R</i> ²	0.097	0.099	0.111

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is an indicator for whether the child was reported to be employed in the past year. Standard errors are in parentheses, clustered by community.

In conclusion, higher real rice prices, and the resultant lower real wages, led to declines in school participation on both the extensive and intensive margins; however, the observed effects are larger on the intensive margin with more students reducing attendance in school rather than withdrawing from school altogether. These effects are measured across the medium-term horizon of three years, which suggest that some of the steep declines in school enrolment and attendance documented by Thomas et al. in 1998 had reversed themselves to some extent by 2000, but continued to persist in the worst-hit communities. However, there is a large decline in the labour market participation of young children in response to rising food prices, perhaps in response to the large fall in real wages that occurred as a result of the cri-

sis. The reduced participation of children in both income-earning and school-going activities suggests that they might have been substituting for adults in undertaking chores at home, tending to younger children and other tasks that do not show up as labour market participation but represent a claim on their time. Unfortunately, in the absence of granular data on time-use in 1997, it is not possible to directly test for this effect.

1.4.3 Heterogeneity by age

Observational data suggests that schooling investments in older children were protected by households at the expense of younger children. I examine this relationship in a formal setting using the exogenous shock (with respect to households) to community rice prices. The distinction in age is of interest because we would expect that older children would be more sensitive to real wage movements in the local labour market due to their relative ability to find work, compared to young children.

To assess whether students of different ages are differentially affected by the change in the price of rice, I include interactions of the (natural log of) the real price of rice with an indicator for whether the student is of elementary school age (7-12 years) or of junior secondary school age (13-15 years old) in an estimation of Equation (1.2) for each of the three dependent variables of interest. The results are presented in Table 1.6. Column 1 shows the heterogeneity in the impact of inflation across age on school enrolment. The impact of the increase in rice prices is felt most strongly on the youngest age group, with a one standard deviation increase in the real price of rice reducing the probability that 7-12 year-olds attend school by 2 percentage points while the enrolment for the older cohort appears to be unaffected by rice prices altogether. The hypothesis that the two coefficients are statistically similar is rejected at the 95 per cent confidence level.

For hours spent in school per week (Column 2), the coefficient on rice prices is larger for younger children but the difference between the two coefficients is not statistically significant. Moreover, older children attend more hours of school per week, on average, so any change in school attendance at the intensive margin could reflect this difference. For labour participation, however, there is a significant difference in the impact of rice prices on labour market participation: older children are

Table 1.6: Heterogeneous impact of rice price increases by age group

	Dependent variable		
	School enrolment	School hours	Child labour
Ln rice price $\times$ 7-12 years old	-0.136** (0.058)	-11.551*** (4.273)	-0.140*** (0.051)
Ln rice price $\times$ 13-15 years old	-0.041 (0.070)	-8.659* (4.574)	-0.225*** (0.067)
7-12 years old	0.662** (0.330)	21.529 (14.693)	-0.584** (0.273)
Year = 2000	0.003 (0.041)	-14.118*** (3.750)	0.173*** (0.037)
Season indicators	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes
Observations	6,473	6,473	6,473
<i>Adj.R</i> ²	0.286	0.282	0.099
p-value (equal coefficients)	0.045	0.166	0.028

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in Column 1 is an indicator for whether a child is enrolled in school for the given academic year. The dependent variable in Column 2 is the average number of hours spent in school per week for students who are enrolled in school. The dependent variable in Column 3 is an indicator for whether a child is reported to have worked in the past year. The reference category is children aged between 7 and 12 years. Standard errors are in parentheses, clustered by community. The probability that the two cohort coefficients on (log) rice price are equal is given in the last row.

significantly more responsive to falling real wages than younger children in terms of finding labour market work. Older children are more likely to opt into work over schooling if they are offered a high enough wage; the impact of the crisis, however, reduced real wages to the extent that children were more likely to remain in school. For younger children, the trade-off is less acute due to the relative lack of job opportunities.

Taken together, the results suggest that in urban communities hardest hit by inflation, older children were more likely to remain in school: households protected their investments in the schooling of older children and there was an absence of job opportunities offering at least the reservation wage of older children. Instead, households were more likely to save money by reducing the time spent in school by young children, perhaps by delaying entry into the formal school system.

1.4.4 Heterogeneity by income

The theoretical framework outlined in Section 1.2.2 suggests a potential channel of heterogeneity across household income levels. While I do not have any direct or implicit measure of credit constraints, I exploit the likely heterogeneity by household income in the extent to which households are likely to be credit-constrained. If higher income households are less likely to be credit-constrained, then the income effect of the recession on the decision to invest in schooling is likely to be less important, relative to low-income households. However, the substitution effect of lower wages on the decision for children to take up paid employment will continue to operate. In this section, I present results on heterogeneity by household income in the effect of the recession on schooling and labour decisions.

Equation (1.2) is estimated for each of the three dependent variables but instead of including log real rice prices directly in the estimating equation, I include interactions between the price variable and indicators for the income group to which a household belongs. I construct three income groups: low-income households are those below the 33rd percentile of per capita real monthly household expenditure; mid-income households are between the 33rd and 67th percentile and high-income households are above the 67th percentile. Table 1.7 presents estimates of the impact

of higher rice prices on households from these three categories. For school participation, the coefficient on the interaction between rice price and the highest income group is smaller than that for low- and middle-income households; in fact, school enrolment in high-income households does not respond at all to rice prices. For school enrolment (Column 1), the coefficient on this interaction for the high-income group is significantly different from the other two groups at the 95 per cent level; for school hours per week, the coefficient for the high-income group is significantly different from the middle income group at the 90 per cent level. For the labour market participation of children, the coefficient on high-income households is higher, suggesting that these households have a higher reservation wage for students to drop out of school and take up paid work. However, I cannot reject that the coefficients are equal.

There appears to be some heterogeneity in how households responded to rising rice prices when comparing across income. However, the differences are not large or consistent. The standard argument is that relatively wealthy households are able to smooth consumption by running down their assets and accumulated savings. This is likely to have been ameliorated to some extent by the fact that wide-spread job losses took place among high-skilled workers.

1.4.5 Robustness checks

The estimation of Equation 1.2 relies on the assumption that communities followed a common trend prior to the events of the crisis in 1998. If communities that were differentially affected by the crisis were already on different trajectories with respect to schooling outcomes, then the estimated effect may be capturing the pre-crisis differences. In order to rule out this possibility, I use data from the 1993 round of the household survey to examine whether communities systematically differed on the variables of interest. Table 1.8 presents correlations of community-level changes in schooling outcomes across the pre-crisis period (1993-1997) and the crisis period (1997-2000), with changes in rice prices during the crisis period only (1997-2000). I control for the province in which the community is located in all specifications. While these cannot be interpreted as causal, there appears to be no correlation between increases in community-level school enrolment, school participation and

Table 1.7: Heterogeneous impact of rice price increases by income group

	Dependent variable		
	School enrolment	School hours	Child labour
Ln rice price $\times$ Low-income household	-0.108** (0.054)	-10.577*** (3.686)	-0.190*** (0.049)
Ln rice price $\times$ Mid-income household	-0.127** (0.054)	-11.310*** (3.356)	-0.150*** (0.043)
Ln rice price $\times$ High-income household	-0.046 (0.047)	-8.924*** (3.112)	-0.175*** (0.047)
Low-income household	0.464 (0.358)	12.345 (17.881)	0.104 (0.316)
Mid-income household	0.598* (0.321)	17.493 (12.391)	-0.189 (0.200)
Year = 2000	0.003 (0.030)	-14.029*** (2.696)	0.176*** (0.027)
Season indicators	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes
Observations	6,430	6,430	6,430
<i>Adj.R</i> ²	0.286	0.281	0.098

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in Column 1 is an indicator for whether a child is enrolled in school for the given academic year. The dependent variable in Column 2 is the average number of hours spent in school per week for students who are enrolled in school. The dependent variable in Column 3 is an indicator for whether a child is reported to have worked in the past year. The reference category is the high-income household. The number of observations is lower than the other estimations because 43 child observations have no corresponding household income data. Standard errors are in parentheses, clustered by community. Hypothesis that the coefficients on income groups are equal is rejected at the 5 per cent level in Column 1.

labour participation in the pre-crisis period and whether a community went on to be affected by the crisis in 1998. On the other hand, the results from the regression analysis are confirmed even in the aggregated data, as schooling attendance and labour market participation decline in the communities with the highest rice price increases.

Table 1.8: Placebo test: correlation of pre-crisis changes in schooling outcomes with post-crisis changes in prices

	Dependent variable					
	School enrollment		School hours		Child labour	
	1993-97	1997-2000	1993-97	1997-2000	1993-97	1997-2000
$\Delta \text{Ln rice price}$	-0.000 (0.002)	-0.005* (0.003)	0.015 (0.106)	-0.564*** (0.215)	0.002 (0.002)	-0.002* (0.001)
Province indicators	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,137	3,344	3,136	3,336	3,137	3,344
<i>Adj.R</i> ²	0.007	0.005	0.009	0.009	0.007	0.004

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in Columns 1 and 2 is an indicator for whether a child is enrolled in school for the given academic year. The dependent variable in Columns 3 and 4 is the average number of hours spent in school per week for students who are enrolled in school. The dependent variable in Columns 5 and 6 is an indicator for whether a child is reported to have worked in the past year. Standard errors are in parentheses, clustered by community.

Migration is a potential concern: if households migrate from urban to rural areas in response to high prices during the crisis, the urban communities of 1997 and 2000 may not be comparable. A related concern is the transition of workers from jobs in manufacturing or services to jobs in the agricultural sector, which benefited, in relative terms, from the increase in rice prices. If many workers shifted to jobs in the agricultural sector, the negative effect of the recession may have been mitigated.

Table 1.9 presents data on the movement of households across locations between 1997 and 2000. Only 3 per cent of the households living in urban communities in 1997 had moved to rural communities by 2000, suggesting that little migration took place among the sample households. Table 1.10 similarly presents data on the movement of household heads across sectors of employment. Of the 3,375 urban households in the sample, less than 7 per cent of household heads moved from a job in the non-agricultural sector to a job in the agricultural sector. The low rates of

Table 1.9: Rural-urban migration of panel households

1997 location	2000 location		
	Urban	Rural	Total
Urban	3,119 (96.27)	121 (3.73)	3,240 (100.00)
Rural	135 (3.42)	3,815 (96.58)	3,950 (100.00)
Total	3,254 (45.26)	3,936 (54.74)	7,190 (100.00)

Notes: The numbers in parentheses are the shares of all 1997 households living in urban or rural areas in 2000.

migration between locations and sectors of employment suggest that the results are not being driven by non-random changes in the sample.

Table 1.10: Sector of employment of the heads of panel households

1997 employment	2000 employment		
	Non-agriculture	Agriculture	Total
Non-agriculture	2,783 (93.20)	203 (6.80)	2,986 (100.00)
Agriculture	135 (34.70)	254 (65.30)	389 (100.00)
Total	2,918 (86.46)	457 (13.54)	3,375 (100.00)

Notes: The numbers in parentheses are the shares of all 1997 household heads working in non-agriculture and agriculture in 2000.

To identify whether unobserved household characteristics play a role in mediating the impact of rice prices on schooling choices, I estimate Equation (1.2) with household fixed effects, rather than community effects, which would control for unobserved time-invariant household characteristics. Coefficients are now interpreted as the effect of rising prices within a given household on the decision to enrol in

school, the hours spent in school per week and the decision to participate in the labour market.

The results are presented in Table 1.11, for the basic regressions, and in Table 1.12, for the heterogeneity in impact by age. The number of observations is smaller (4,802 child observations) because the sample only includes panel households, which raises potential concerns with respect to non-random attrition. The results for school participation are robust to the inclusion of household fixed effects instead of community fixed effects; however, the coefficients on rice prices in the child labour participation estimations are smaller. One interpretation of this decline in the size of the effect on labour participation could be that some children take up employment within family-owned enterprises, which reduces the sensitivity of the labour market decision to rice price movements. However, labour market participation continues to decline with higher prices and older children are more sensitive to these price movements than younger children.

Table 1.11: Impact of rice price increases with household effects

	Dependent variable		
	School enrolment	School hours	Child labour
Ln rice price	-0.158* (0.080)	-13.819** (6.002)	-0.097 (0.073)
Year = 2000	-0.059 (0.062)	-17.230*** (5.252)	0.223*** (0.067)
Season indicators	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes
Observations	4,802	4,802	4,802
<i>Adj.R</i> ²	0.342	0.355	0.194

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in Column 1 is an indicator for whether a child is enrolled in school for the given academic year. The dependent variable in Column 2 is the average number of hours spent in school per week for students who are enrolled in school. The dependent variable in Column 3 is an indicator for whether a child is reported to have worked in the past year. Standard errors are in parentheses, clustered by community.

Table 1.12: Heterogeneous impact of rice price increases by age group with household effects

	Dependent variable		
	School enrolment	School hours	Child labour
Ln rice price $\times$ 7-12 years old	-0.171*** (0.060)	-14.432*** (4.167)	-0.083 (0.054)
Ln rice price $\times$ 13-15 years old	-0.107 (0.081)	-12.165** (4.909)	-0.125* (0.067)
7-12 years old	0.391 (0.415)	16.161 (17.138)	-0.288 (0.295)
Year = 2000	-0.053 (0.047)	-16.849*** (3.744)	0.216*** (0.049)
Season indicators	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes
Observations	4,802	4,802	4,802
<i>Adj.R</i> ²	0.319	0.335	0.183

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in Column 1 is an indicator for whether a child is enrolled in school for the given academic year. The dependent variable in Column 2 is the average number of hours spent in school per week for students who are enrolled in school. The dependent variable in Column 3 is an indicator for whether a child is reported to have worked in the past year. The reference category is children aged between 7 and 12 years. Standard errors are in parentheses, clustered by community.

As a final robustness check of model specification, I re-estimate the models explaining school enrolment and participation in child labour using a probit model, rather than a linear probability model as above. The results are presented in the appendix in Table 1.A.1 on 46. For school enrolment, the sign and magnitude of the coefficients on rice prices is similar; for labour market participation, the coefficients are smaller in size but indicate that in communities with higher prices, labour market participation of children declined.

1.5 Long-term impact of aggregate shock on schooling

The preceding section establishes that the sudden rise in real rice prices in some communities adversely affected child schooling outcomes, at both the extensive and the intensive margins, for younger children (7-12 years), and the decline in the local economy reduced the availability of labour market opportunities at competitive wages for older children (13-15 years). In an environment where students simultaneously hold jobs as well as attend school, the crisis could have a theoretically ambiguous effect on long-term schooling outcomes. On the one hand, if households reduce spending on child schooling, then, depending on the nature of the disruption to schooling, this could have adverse long-term consequences in terms of reduced human capital formation and lower lifetime wages. On the other hand, if students cannot find suitable job opportunities, they might end up focusing on school alone, in which case the fall in wages could actually increase long-term schooling attainment. Even where students are dropping out of school, there may not necessarily be adverse long-term consequences for long-term human capital investments; much will depend on the nature of the disruption to schooling. For instance, less able children could be dropping out of school, which may have little impact on their eventual schooling attainment. Moreover, while younger children may be more vulnerable to income shocks, reduced school attendance at a young age may well be less costly than schooling disruptions to an older child in junior secondary school. For example, entering school a year later than usual could have less of a long-term impact than taking on part-time work during a critical year of schooling (when, say, a student is

required to write a school-leaving examination, which forms the basis for entry into senior secondary school).

To analyse whether these short- to medium-term effects have any long-term consequences on educational attainment requires long-term data which follows the same children into adulthood and examines their final schooling attainment. I construct a panel of children observed in both 1997 and 2007 and identify the impact of a sharp increase in rice prices that took place when they were 7-15 years old, on their eventual schooling outcomes ten years later, when they are 17-25 years old. The long-term schooling outcomes I consider are graduation from junior school (typically at age 15-16), graduation from senior secondary school (typically at age 17-18), and graduation from college or higher education (anywhere between age 19 and 25). I consider the impact of rice price increases in the past on eventual schooling outcomes ten years later, I identify the differential impact on younger children, relative to older children, and I examine heterogeneity with respect to income brackets. I address some concerns with respect to attrition, which could potentially bias the estimates.

1.5.1 Empirical framework

The fourth wave of the IFLS, conducted in 2007, surveyed the panel households from earlier waves. Since it collected data ten years after the initial crisis broke out, this wave provides insights into the long-term impact of the 1998 economic shock. I construct a panel of students aged 7-15 years old in 1997 and 2000, and resurveyed in 2007. I estimate Equation (1.2) but instead of using schooling outcomes from the year 2000, I use schooling outcomes for the year 2007. Now, β measures the *long-term change* in the schooling outcome variable from 1997 to 2007, with a percentage increase in the log of real prices during the crisis period between 1997 and 2000. The schooling outcome variables I consider are long-term measures of schooling attainment, including years of education, and whether a child has graduated junior school, senior school or college, by 2007.

In estimating Equation (1.2), however, an implicit assumption is made that schooling attainment is only determined by the contemporaneous value of the independent variables, after controlling for community fixed effects. For outcome

variables summarising total schooling attainment, this may be an unreasonable assumption. In this context, it may be more appropriate to control for individual fixed heterogeneity, such as innate ability. I construct a panel of children observed in both 1997 and 2007, and I estimate the following ANCOVA model of schooling attainment:

$$y_{ijt+1} = \alpha y_{ijt} + \beta \Delta \ln P_{jt} + \mathbf{X}_{ijt}' \boldsymbol{\gamma} + \varepsilon_{ijt+1} \quad (1.3)$$

where y_{ijt+1} is the schooling outcome of interest as observed in 2007. y_{ijt} is the baseline schooling outcome of interest (the years of schooling acquired by the student in the baseline year or the level of schooling in which the student was enrolled at the time). This lagged measure of schooling attainment captures the individual's past schooling, innate ability, and any other unobservable heterogeneity. $\mathbf{X}_{ijt}$ includes baseline child characteristics, household characteristics and community characteristics as in the previous estimation.

Under the assumption that lagged value of schooling attainment captures all individual fixed heterogeneity, this specification controls for unobserved characteristics such as individual ability or aptitude for schooling. The coefficient of interest is β , which measures the impact of the change in rice prices on any of four long-term schooling outcomes. If the recession of 1998 led to children permanently dropping out of school, β will be negative. If the recession led to children spending more time on schoolwork, or if the disruptions to schooling were temporary, β will be insignificantly different from zero, or even positive. The identifying assumption is that if rice prices had not changed between 1997 and 2000, the schooling trajectory of the student would have remained unchanged.

Any individual-specific time-invariant heterogeneity is captured in the baseline controls for level of schooling of the child in 1997. Additionally, the inclusion of baseline community characteristics identifies the impact of the shock beyond any differences that may have arisen due to infrastructural availability, community programmes and other aggregate shocks. Finally, the introduction of pre-crisis controls for a range of household and household-head characteristics (as in Section 1.4) also controls for any potential endogeneity that could explain schooling outcomes.

Also of interest is whether the long-term impact on schooling outcomes of a period of economic crisis is heterogeneous by the age of the child, as in Section

1.4. Equation (1.3) is estimated with interactions of the change in rice prices in a particular 1997 community and an indicator for whether the child was 7-12 years old in 1997 or 13-15 years. This permits a comparison of the heterogeneous effects on children who were differently affected by the crisis in 1998. Finally, Equation (1.3) is also estimated with interactions of the change in rice prices and an indicator for whether a student belongs to a low-, mid- or high-income household, to identify heterogeneity in impact by household income.

1.5.2 Results

The results of the estimation of Equation (1.2) for long-term schooling attainment are provided in Table 1.13. The measures of schooling outcomes are the following: years of schooling completed by 2007 (Column 1), whether the child has graduated junior school (Column 2), whether the child has graduated senior secondary school (Column 3) and whether the child has graduated college or university (Column 4). The coefficients on the log of rice prices are now significantly positive, suggesting that children in the worst affected urban districts during the 1998 recession were more likely to have acquired more years of schooling, and to have graduated junior school and senior school.

The results of the estimation of Equation (1.3) are presented in Table 1.14. The measures of schooling attainment are the same: years of schooling completed by 2007 (Column 1), whether the child has graduated junior school (Column 2), whether the child has graduated senior secondary school (Column 3) and whether the child has graduated college or university (Column 4). For these estimations, the coefficients are smaller and the standard errors larger, but they remain positive, indicating that higher rice prices are associated with more years of schooling, higher rates of graduation from junior school, senior school as well as college, with the coefficient on the change in rice prices significant at the 90 per cent level for years of schooling. This provides evidence that schooling attainment was higher in worst-hit communities, and I suggest that this is consistent with the narrative that the depressed real economy kept students in school for a longer period of time due to the lack of available job opportunities. A one standard deviation increase in the real price of rice increased the number of years of schooling by 0.2 (alternatively, every

Table 1.13: Long-term impact of rice prices increases – community panel

	Dependent variable			
	Years of schooling	Grad. jr. school	Grad. sr. school	Grad. college
Ln rice price	2.428*** (0.557)	0.156** (0.074)	0.260*** (0.066)	0.028 (0.033)
Year=2007	3.657*** (0.583)	0.393*** (0.051)	0.365*** (0.050)	0.035 (0.025)
Season indicators	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes
Observations	6,069	6,069	6,069	6,069
<i>Adj.R</i> ²	0.777	0.380	0.484	0.051

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables are as follows: total years of schooling completed (Col 1), indicator for graduated junior school (Col 2), indicator for graduated senior secondary school (Col 3) and indicator for graduated college (Col 4). Standard errors are in parentheses, clustered by 1997 community of residence.

fifth child in a badly-affected community will have an additional year of school), and increased the probability that students graduated junior school and senior school by 2 percentage points, and college by 1.5 percentage points.

Table 1.14: Long-term impact of rice prices increases – individual panel

	Dependent variable			
	Years of schooling	Grad. jr. school	Grad. sr. school	Grad. college
$\Delta \ln$ rice price	1.054* (0.545)	0.119 (0.081)	0.116 (0.074)	0.082 (0.058)
Baseline outcomes	Yes	Yes	Yes	Yes
Season indicators	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Observations	2,942	2,942	2,942	2,942
<i>Adj.R</i> ²	0.446	0.235	0.326	0.157

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables are as follows: total years of schooling completed (Col 1), indicator for graduated junior school (Col 2), indicator for graduated senior secondary school (Col 3) and indicator for graduated college (Col 4). Standard errors are in parentheses, clustered by 1997 community of residence.

There is little evidence that the younger children who bore the brunt of the 1998 crisis have suffered adverse long-term effects relative to older children (Table 1.15). Younger children are less likely to have graduated college compared to older children but it is hard to disentangle this effect from the age effect that older children are more likely to have completed college, even after controlling for a quadratic function of age. In explaining high school graduation, the coefficient on the change in rice prices for younger children is larger than for older children, but the hypothesis of equality of coefficients cannot be rejected.

Finally, results for the heterogeneity of the long-term impact of rice price increases by household income are presented in Table 1.16. There is some evidence that middle-income families were more responsive to rice price increases than other income groups but, again, the equality of coefficients cannot be rejected.

Table 1.15: Heterogenous long-term impact of rice prices increases by age

	Dependent variable			
	Years of schooling	Grad. jr. school	Grad. sr. school	Grad. college
$\Delta \text{ Ln rice price} \times 7\text{-}12 \text{ years old}$	1.117* (0.574)	0.110 (0.090)	0.128 (0.083)	0.048 (0.065)
$\Delta \text{ Ln rice price} \times 13\text{-}15 \text{ years old}$	0.647 (0.659)	0.094 (0.095)	0.064 (0.089)	0.120* (0.065)
7-12 years old	0.180 (0.202)	0.024 (0.028)	0.072* (0.037)	-0.028 (0.028)
Baseline outcomes	Yes	No	Yes	Yes
Season indicators	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Observations	2,942	2,942	2,942	2,942
$Adj.R^2$	0.450	0.242	0.329	0.158
p-value (equal coefficients)	0.432	0.867	0.498	0.273

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables are as follows: total years of schooling completed (Col 1), indicator for graduated junior school (Col 2), indicator for graduated senior secondary school (Col 3) and indicator for graduated college (Col 4). The reference category for the 7-12 year-old group of children is the 13-15 year-old group. Standard errors are in parentheses, clustered by 1997 community of residence.

Table 1.16: Heterogenous long-term impact of rice prices increases by income

	Dependent variable			
	Years of schooling	Grad. jr. school	Grad. sr. school	Grad. college
$\Delta \text{ Ln rice price} \times \text{Low-income household}$	0.860 (0.811)	0.133 (0.128)	0.092 (0.125)	0.007 (0.056)
$\Delta \text{ Ln rice price} \times \text{Mid-income household}$	1.465** (0.699)	0.126 (0.089)	0.096 (0.099)	0.180** (0.080)
$\Delta \text{ Ln rice price} \times \text{High-income household}$	0.616 (0.810)	0.072 (0.116)	0.156 (0.122)	0.054 (0.090)
Low-income household	-0.463*** (0.165)	-0.000 (0.028)	-0.075*** (0.027)	-0.058*** (0.016)
Mid-income household	-0.071 (0.137)	0.047** (0.020)	-0.009 (0.022)	-0.048*** (0.015)
Baseline outcomes	Yes	Yes	Yes	Yes
Season indicators	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Observations	2,928	2,928	2,928	2,928
$Adj.R^2$	0.448	0.238	0.328	0.163

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables are as follows: total years of schooling completed (Col 1), indicator for graduated junior school (Col 2), indicator for graduated senior secondary school (Col 3) and indicator for graduated college (Col 4). The reference category is the high-income household. Standard errors are in parentheses, clustered by 1997 community of residence. There are fewer observations than in the regressions presented in tables 1.14 and 1.15 because household income data is not available for 14 households.

The results on the long-term impact of the recession are consistent with a narrative where students remain in school and do not take up additional employment due to the contraction of the local economy and the crash in real wages. As a result of remaining in school through the crisis, children in relatively more affected areas have higher long-term human capital investments than children in less affected areas. Even the youngest children, who were most likely to have been pulled out of school in response to the economic shock, did not suffer adverse long-term consequences in human capital formation.

1.5.3 Robustness checks

The estimates of the impact of the change in rice prices between 1997-2000 on educational attainment in 2007 could be biased if unobserved community characteristics not captured by the baseline controls could be correlated with both the extent of the price increase as well as average schooling investments by children living in those communities. Since the causal variable of interest – the change in rice prices – varies by community, Equation (1.3) cannot be estimated with initial community fixed effects, using the baseline communities of residence. However, the differential effects of the change in rice prices on students by age and income group can be measured with initial community fixed effects. Such a model assumes that in the absence of the price shock, the relative schooling trajectories of children from different age or income groups would be unchanged within a given community. I present results from a model using initial fixed community effects in the appendix. The fact that both sets of results are similar under different model specifications lends some support to the assumption that unobserved community characteristics did not influence rice prices and schooling attainments (beyond the controls for baseline community characteristics that have already been included in the estimating equation).

Another potential concern in a long dataset of ten years is the non-random attrition of households from the survey. The IFLS has remarkably low rates of attrition for developing country surveys, with less than ten per cent of all households dropping out between successive surveys. As a robustness check, I estimate Equation (1.3) while correcting for attrition and find no substantive difference in the estimates (results and methodology are reported in the appendix).

1.6 Conclusion

The impact of an economic recession on schooling outcomes has been recognised as an important research question in the last two decades, particularly in developing countries. Typically, a negative aggregate shock is characterised by an economy-wide decline in real wages, which affects both household income as well as wage-earning opportunities for children in the labour market. Theoretically, the net impact of the resultant income effect and substitution effect of lower real earnings on the school and labour market participation of children is ambiguous. In this paper, I estimate the impact of an aggregate economic shock on children aged 7-15 in a large developing country, Indonesia, by exploiting the heterogeneous impact of the East Asian financial crisis across communities. Using variation in rice price increases across urban communities, which are plausibly exogenous to individual households, I find that rice price increases are correlated with small declines in school enrolment and hours per week spent in school, and large declines in the labour market participation of children. For the youngest children (aged 7-12 years) there is a larger negative impact of higher rice prices on school attendance and a smaller effect on labour market participation. For older children (aged 13-15 years), the impact goes the other way: their schooling enrolment does not respond to rice prices but their labour market participation declines sharply in the worst-hit communities. Older children, who are more sensitive to changes in the opportunity cost of schooling, withdrew from jobs in those communities where real wages declined the most.

The short and medium-run effects identified do not necessarily have adverse long-term consequences on human capital formation, as is commonly assumed. Using panel data on children surveyed in both 1997 and 2007, I do not find any evidence of long-term effects of this economic crisis on schooling attainment. On the contrary, I find that children in the communities with largest price increases were *more* likely to have higher schooling attainment ten years later: they completed 0.2 more years of school, and were no less likely to have graduated junior secondary school, senior secondary school and college, compared to students in less-affected areas. Nor does the younger cohort exhibit any signs of lagging behind the older cohort, despite the larger short-term impacts on school enrolment.

The positive long-term effect on years of schooling between 1997-2007 is puzzling, when one considers the short-run decline in enrolment over the 1997-2000 period. The most likely explanation is that students who did stay in school through the crisis, were more likely to increase schooling attainment in the worst-hit communities. It could be that these results are driven by younger children delaying enrolment into school, but increasing the number of years that they spend in school over the long term. Another potential reason is that older students withdrew from the labour market as a result of the crisis, and were able to spend more time on schoolwork and increase their eventual schooling attainment. Finally, it may have taken years for the labour market to recover from the crisis, leading to students spending prolonged periods of time in school in more affected communities.

This suggests that even in developing countries, sustaining household demand for schooling in the face of an aggregate economic shock may be less of a concern than is commonly believed. Of greater policy interest may be the state's role in preserving funding for education, maintaining school quality and promoting vocation-specific training, which can help students find employment in the years that follow a deep recession.

1.A Appendix

1.A.1 Data and construction of sample

The IFLS interviewed 7,307 children between the ages of 7 and 15 (inclusive) in 1997 and 7,966 children in 2000, creating a sample of 15,273 observations across both waves. First, I dropped all children living in rural areas, leaving a sample of 6,632 observations of children living in urban areas (3,180 observations in 1997 and 3,452 observations in 2000). Second, I dropped 31 child observations for those children living in communities with no price data, and 128 child observations for those children living in communities with no infrastructure data leaving 6,473 observations in the data set (3,156 in 1997 and 3,357 in 2000).

There was no missing data on school enrolment or labour market participation. For hours spent in school, 412 observations were imputed by the average hours spent in school by school-going children of the same age cohort (aged 7-12 or 13-15 years) in the same district and the same year. Hours spent in school was set to zero for students not enrolled in school. The results with imputed observations were not significantly different from results that excluded missing observations.

For the long-run data, of the 3,156 children observed in 1997, 2,942 are observed in 2007. Of these, data on household income in 1997 is only available for 2,928 students.

1.A.2 Robustness check with probit models

Table 1.A.1 presents results from a probit model explaining school enrolment and participation in child labour. For school enrolment, the sign and magnitude of the coefficients on rice prices is similar to the linear probability model of Table 1.3 on page 19. For labour market participation, the coefficients are smaller in size than the linear probability model of Table 1.5 on page 24, but indicate that in communities with higher prices, labour market participation of children declined.

1.A.3 Robustness check with initial community fixed effects

Tables 1.A.2 and 1.A.3 provide estimates of the differential long-term impact of rice price increases on eventual schooling attainment for students of different ages and

Table 1.A.1: Probit models for impact of rice prices on school enrolment and labour market participation

	School enrolment		Child labour	
	(1)	(2)	(3)	(4)
Ln rice price	-0.098*** (0.327)		-0.093** (0.448)	
Ln rice price $\times$ 7-12 years old		-0.131*** (0.381)		-0.065 (0.489)
Ln rice price $\times$ 13-15 years old		-0.081** (0.342)		-0.105** (0.468)
7-12 years old		0.373* (2.124)		-0.275 (2.214)
Year = 2000	-0.052*** (0.189)	-0.053*** (0.186)	0.165*** (0.292)	0.169*** (0.294)
Season indicators	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes
Province-year effects	Yes	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes
Observations	6,182	6,182	5,971	5,971

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. Marginal effects are reported. The dependent variables are as follows: indicator for whether a child is enrolled in school for the given academic year (Columns 1 and 2), and indicator for whether a child is reported to have worked in the past year (Columns 3 and 4). The reference category for the 7-12 year-old group of children is the 13-15 year-old group in Columns 2 and 4. Robust standard errors are in parentheses, clustered by community.

household income, both with and without initial community fixed effects. Columns 1, 3, 5, and 7 in both tables repeat the estimations from Table 1.15 and Table 1.16 on 40 and page 41, but with coefficients represented in a different form in order to facilitate comparisons with the initial community fixed effects regressions. Columns 2, 4, 6, and 8 in both tables present the estimations of the heterogeneous effect of price changes on long-term schooling attainment, but with initial community fixed effects included. Both sets of results are similar, which lends some support to the assumption that unobserved community characteristics did not influence rice prices and schooling attainments (beyond the controls for baseline community characteristics that have already been included in the estimating equation).

1.A.4 Correcting for attrition

One concern is the potential for non-random attrition, whereby respondents who drop out of the survey differ systematically from those who stay in the survey, particularly in the ways in which schooling attainment was affected by the local economy, leading to biased estimates of β . Attrition is a concern for any panel dataset, particularly one that ranges over a period of ten years. The 2007 dataset loses 214 children from the initial sample of 3,156 – a loss of 6.8 per cent. The resultant sample of 2,942 children are drawn from 110 communities, as opposed to the 124 communities for which data was available in 1997 and 2000. The individual, household and community characteristics of attriters compared to non-attriters are presented in Table 1.A.4 and Table 1.A.5. In general, households that attrit from the survey tend to be smaller in size, with higher per capita expenditure, and are less likely to have been living in their own houses in 1997. These households are also disproportionately drawn from communities that were adversely affected by the 1997-98 El Niño shocks.

Since attrition is unlikely to be random, I control for the effect of attrition in the following way: I use observed characteristics of surveyed individuals to estimate inverse probability weights so as to increase the weight on individuals who were more likely to attrit but who remained in the survey. This assumption - also called the ignorability of selection - assumes that selection can be predicted on observable

Table 1.A.2: Heterogeneous impact of rice price increases by age - with and without community fixed effects

	Dependent variable							
	Years of schooling		Grad. jr. school		Grad. sr. school		Grad. college	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta \ln$ rice price	0.745 (0.667)		0.105 (0.096)		0.075 (0.092)		0.127* (0.068)	
$\Delta \ln$ rice price $\times$ 7-12 years old	0.508 (0.595)	0.503 (0.581)	0.024 (0.096)	0.026 (0.098)	0.069 (0.096)	0.073 (0.094)	-0.074 (0.065)	-0.063 (0.064)
7-12 years old	0.179 (0.207)	0.252 (0.210)	0.001 (0.028)	0.011 (0.029)	0.072* (0.037)	0.083** (0.038)	-0.027 (0.028)	-0.029 (0.028)
Baseline outcomes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province effects	Yes	No	Yes	No	Yes	No	Yes	No
Community social programmes	Yes	No	Yes	No	Yes	No	Yes	No
Infrastructure availability	Yes	No	Yes	No	Yes	No	Yes	No
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2,942	2,942	2,942	2,942	2,942	2,942	2,942	2,942
<i>Adj. R</i> ²	0.446	0.468	0.235	0.283	0.327	0.344	0.157	0.170

Notes: *, **, *** denote significance at the 10%, 5, and 1% confidence levels, respectively. The dependent variables are as follows: number of years of schooling completed (Col 1 and 2), indicator for graduated junior school (Col 3 and 4), indicator for graduated senior secondary school (Col 5 and 6) and indicator for graduated college (Col 7 and 8). The reference category for the 7-12 year-old group of children is the 13-15 year-old group. Standard errors are in parentheses, clustered by 1997 community of residence.

Table 1.A.3: Heterogeneous impact of rice price increases by household income - with and without community fixed effects

	Dependent variable							
	Years of schooling		Grad. jr. school		Grad. sr. school		Grad. college	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta \text{Ln rice price}$	0.566 (0.822)		0.068 (0.115)		0.148 (0.121)		0.046 (0.089)	
$\Delta \text{Ln rice price} \times \text{Low-income household}$	0.179 (1.004)	-0.057 (1.067)	0.049 (0.154)	0.009 (0.161)	-0.069 (0.161)	-0.117 (0.175)	-0.040 (0.091)	-0.039 (0.093)
$\Delta \text{Ln rice price} \times \text{Mid-income household}$	0.724 (1.015)	0.837 (1.017)	0.036 (0.120)	0.076 (0.115)	-0.073 (0.164)	-0.050 (0.175)	0.128 (0.101)	0.095 (0.098)
Low-income household	-0.448*** (0.165)	-0.709*** (0.167)	0.002 (0.028)	-0.034 (0.029)	-0.073*** (0.028)	-0.100*** (0.028)	-0.058*** (0.016)	-0.052*** (0.017)
Mid-income household	-0.073 (0.142)	-0.274* (0.141)	0.046** (0.020)	0.013 (0.020)	-0.009 (0.023)	-0.028 (0.024)	-0.049*** (0.015)	-0.043*** (0.015)
Baseline outcomes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province effects	Yes	No	Yes	No	Yes	No	Yes	No
Community social programmes	Yes	No	Yes	No	Yes	No	Yes	No
Infrastructure availability	Yes	No	Yes	No	Yes	No	Yes	No
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2,928	2,928	2,928	2,928	2,928	2,928	2,928	2,928
$Adj. R^2$	0.452	0.474	0.242	0.287	0.330	0.347	0.164	0.174

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables are as follows: number of years of schooling completed (Col 1 and 2), indicator for graduated junior school (Col 3 and 4), indicator for graduated senior secondary school (Col 5 and 6) and indicator for graduated college (Col 7 and 8). The reference category is the high-income household. Standard errors are in parentheses, clustered by 1997 community of residence.

Table 1.A.4: Comparing attriters and non-attriters - child characteristics

	Resurveyed in 2007	Attrited in 2007
Age	11.41 (2.62)	11.51 (2.61)
Male	0.51 (0.50)	0.46 (0.50)
Enrolled in school	0.91 (0.29)	0.84 (0.37)
Schoolhours/week	31.77 (6.81)	31.01 (6.70)
In labour market	0.03 (0.16)	0.02 (0.13)
In elementary school	0.60 (0.49)	0.56 (0.50)
In junior school	0.35 (0.48)	0.36 (0.48)
Observations	2,942	214

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in Columns 1 and 2 is an indicator for whether a child was resurveyed in 2007 or whether the child attrited from the survey. Standard errors are in parentheses.

characteristics in the first time period,

$$P(s_{it} = 1|y_{it}, x_{it}, z_{i1}) = P(s_{it} = 1|z_{i1}), \quad t = 2, 3, \dots, T \quad (1.4)$$

Variables in z_{i1} that are not included in x_{it} are not assumed to be exogenous to $y_{it}|x_{it}$ - they are simply excluded from the model of interest. Under this assumption, Fitzgerald et al. propose the method of inverse probability weighting (IPW) to correct for attrition. This requires the estimation of a probit of s_{it} on z_{i1} for all observations in period $t = 1$ in order to estimate the *a priori* probability of selection, $\hat{p}_{it}$ of a respondent, i , in time period, t , after the baseline. Weights are constructed such that every observation in OLS is weighted by $1/\hat{p}_{it}$, which provides that observations with a higher probability of attrition are given greater weight in the estimation. By construction, $\hat{p}_{i1} = 1$. Following this methodology, I construct inverse probability weights and estimate equation 1.3 by weighting each observation.

The results of the probit to explain selection in 2007 based on individual, household and community characteristics in 1997 are presented in Table 1.A.6. The results of the probit are quite informative in themselves: households that live in their own

Table 1.A.5: Comparing attriters and non-attriters - household and community characteristics

	Resurveyed in 2007	Attrited in 2007
<i>Household characteristics</i>		
Log per capita expenditure	11.53	12.04
Owns home	0.74	0.56
Modernity index (0-5)	3.52	4.17
Household size	5.97	5.54
Number of children<15 yrs	2.75	2.36
Number of children enrolled in school	2.31	2.13
Number of elders>70 yrs	0.21	0.26
<i>Household-head characteristics</i>		
Age	44.98	45.30
Male	0.90	0.86
Married	0.99	0.97
Employed in past year	0.90	0.85
Employed in public sector	0.14	0.13
Highest schooling level		
Primary or less	0.53	0.36
Jr. secondary	0.17	0.17
Sr. secondary	0.22	0.29
College	0.08	0.18
<i>Community infrastructure (=1 if available)</i>		
Local bus stop	0.05	0.07
Market	0.16	0.19
Public telephone	0.44	0.57
Post office	0.09	0.08
Bank	0.22	0.21
Asphalt roads	0.72	0.72
<i>El Niño shock (=1 if affected)</i>		
Shock in 1997	0.52	0.64
Shock in 1998	0.42	0.54
<i>Community social programmes (=1 if implemented)</i>		
Medical services	0.16	0.11
Rice distribution	0.72	0.72
Health cards	0.47	0.44
Employment generation	0.62	0.75
Food supplements	0.62	0.53
Student scholarships	0.77	0.71
Observations	2,942	214

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables in Columns 1 and 2 are indicators for whether a child was resurveyed in 2007 or whether the child attrited. Standard errors are in parentheses.

homes are more likely to be selected, as are households with many young children, particularly with those enrolled in school. Higher income households that are relatively modern are less likely to be found in 2007, perhaps because of the increased opportunities for such households to migrate away from their 1997 communities. Results that have been corrected for attrition are presented in Table 1.A.7. All observations are weighted using the inverse probability weights constructed as in Fitzgerald et al.¹² The results are not appreciably different from the uncorrected ones, suggesting that attrition is not a significant concern in this study.

¹²The total number of observations is 2,928, which is lower than the total number of observed children of 2,942 due to the loss of 14 observations for which household income data is not available.

Table 1.A.6: Probit predicting non-attrition by baseline variables

	Selection in 2007 based on 1997 variables
Enrolled in school	0.046*** (0.139)
In elementary school	0.021 (0.209)
In junior school	0.019 (0.163)
Age	-0.022 (0.155)
Male	0.004 (0.075)
Log per capita expenditure	-0.023*** (0.055)
Owns home	0.051*** (0.085)
Modernity index (0-5)	-0.024*** (0.050)
Household size	-0.002 (0.027)
Number of children<15 yrs	0.016*** (0.045)
Number of children enrolled in school	-0.004 (0.045)
Number of elders>70 yrs	-0.011 (0.076)
Age	0.000 (0.005)
Male	0.008 (0.133)
Married	0.048 (0.301)
Employed in past year	0.027* (0.134)
Employed in public sector	0.032** (0.128)
Household head highest schooling level	
Primary or less	0.014 (0.142)
Jr. secondary	0.017 (0.147)
Sr. secondary	0.008 (0.132)
Season indicators	Yes
Province effects	Yes
Community social programmes	Yes
Infrastructure improvements	Yes
El Niño shock	Yes
Observations	3,136

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. Marginal effects are reported. The dependent variable in Column 1 is an indicator for whether a child observed in 1997 was resurveyed in 2007. Robust standard errors are in parentheses, clustered by 1997 community of residence.

Table 1.A.7: Attrition-corrected long-term impact of rice prices increases

	Dependent variable			
	Years of schooling	Grad. jr. school	Grad. sr. school	Grad. college
$\Delta \text{Ln rice price}$	1.054* (0.545)	0.119 (0.081)	0.116 (0.074)	0.082 (0.058)
Season indicators	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Observations	2,942	2,942	2,942	2,942
$Adj.R^2$	0.446	0.235	0.326	0.157

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables are as follows: total years of schooling completed (Col 1), indicator for graduated junior school (Col 2), indicator for graduated senior secondary school (Col 3) and indicator for graduated college (Col 4). Standard errors are in parentheses, clustered by 1997 community of residence.

Table 1.A.8: Attrition-corrected heterogenous long-term impact of rice prices increases by age

	Dependent variable			
	Years of schooling	Grad. jr. school	Grad. sr. school	Grad. college
$\Delta \text{ Ln rice price} \times 7\text{-}12 \text{ years old}$	1.144* (0.600)	0.127 (0.092)	0.154* (0.084)	0.050 (0.067)
$\Delta \text{ Ln rice price} \times 13\text{-}15 \text{ years old}$	0.779 (0.698)	0.130 (0.097)	0.098 (0.092)	0.139** (0.069)
7-12 years old	0.169 (0.205)	0.004 (0.027)	0.072* (0.037)	-0.024 (0.029)
Baseline outcomes	Yes	Yes	Yes	Yes
Season indicators	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Observations	2,886	2,928	2,928	2,928
$Adj.R^2$	0.446	0.233	0.325	0.161

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables are as follows: total years of schooling completed (Col 1), indicator for graduated junior school (Col 2), indicator for graduated senior secondary school (Col 3) and indicator for graduated college (Col 4). The reference category for the 7-12 year-old group of children is the 13-15 year-old group. Standard errors are in parentheses, clustered by 1997 community of residence.

Table 1.A.9: Attrition-corrected heterogenous long-term impact of rice prices increases by income

	Dependent variable			
	Years of schooling	Grad. jr. school	Grad. sr. school	Grad. college
Δ Ln rice price $\times$ Low-income household	0.682 (0.799)	0.132 (0.127)	0.090 (0.125)	0.003 (0.057)
Δ Ln rice price $\times$ Mid-income household	1.521** (0.715)	0.145 (0.090)	0.120 (0.100)	0.183** (0.080)
Δ Ln rice price $\times$ High-income household	0.519 (0.861)	0.075 (0.116)	0.162 (0.126)	0.063 (0.092)
Low-income household	-0.412** (0.169)	-0.004 (0.029)	-0.080*** (0.028)	-0.058*** (0.016)
Mid-income household	-0.051 (0.145)	0.045** (0.020)	-0.012 (0.023)	-0.049*** (0.015)
Baseline outcomes	Yes	Yes	Yes	Yes
Season indicators	Yes	Yes	Yes	Yes
Child characteristics	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes
Community social programmes	Yes	Yes	Yes	Yes
Infrastructure improvements	Yes	Yes	Yes	Yes
El Niño shock	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Observations	2,886	2,928	2,928	2,928
$Adj.R^2$	0.448	0.236	0.327	0.166

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variables are as follows: total years of schooling completed (Col 1), indicator for graduated junior school (Col 2), indicator for graduated senior secondary school (Col 3) and indicator for graduated college (Col 4). The reference category is the high-income household. Standard errors are in parentheses, clustered by 1997 community of residence.

Chapter 2

Informality, firm size, and labour market segmentation

2.1 Introduction

A common feature of developing country labour markets is the presence of a sprawling informal sector, which employs a large share of the total workforce but is characterised by low productivity and earnings, relative to the formal sector (Maloney, 2004). According to Bacchetta et al. (2009), 50-80 per cent of workers in developing countries across Latin America, Asia and Africa are employed in the informal sector; moreover, this share has remained largely unchanged in the last two decades, even as many of these countries have experienced rapid economic growth. The stylised fact that workers in the informal sector earn, on average, significantly less than observably similar workers in the formal sector has given rise to concerns about whether informal workers could potentially find themselves trapped in poverty for extended periods of time unless they are able to transition into formal sector jobs. This paper argues against the sharp dichotomy between formal and informal employment in the context of one of Asia's largest developing countries – Indonesia – by using data from a longitudinal household survey to identify sectoral earnings differentials between formal and informal employment, as well as along the dimension of firm size.

Traditionally, the inter-sectoral earnings gap has been related to the phenomenon of labour market segmentation, where some exogenous institutional barrier restricts

the entry of informal sector workers into a high-wage, high-productivity formal sector that is not large enough to employ the entire workforce. Firms may also pay efficiency wages to their workers to reduce turnover. The concern with identifying average earnings differentials as evidence of labour market segmentation, however, is that individuals are heterogeneous in preferences and abilities, and may be selecting into types of employment at which they are relatively more productive. Another concern is that the apparent formal sector premium may be related to the observed strong correlation between firm size and worker earnings: workers in larger firms earn more than workers in small firms. Explanations for this phenomenon have been analysed in light of the human capital model, where the firm size premium reflects differences in human capital endowments across workers – large firms pay more than small firms because they are better able to attract and retain productive workers than small firms. Since informal sector enterprises are, on average, smaller than formal sector firms, firm size is likely to play a part in explaining sectoral earnings differentials.

In order to mitigate each of these methodological concerns, I use three waves of data from a longitudinal household survey – the Indonesian Family Life Survey (IFLS) – to investigate earnings differentials across sectors, conditional on observed human capital endowments and firm size. I exploit the panel dimension of the data to control for individual-specific time-invariant heterogeneity such as unobserved ability or preferences. My results suggest that it is only the public sector that commands an earnings premium over employment in other sectors. Given the underlying model for earnings, there is no evidence of any significant earnings differential between employment in the private formal sector, employment in the private informal wage sector and informal self-employment. I examine potential biases arising from the exclusion of unpaid family workers who form a large part of the sample and from the endogenous sorting of individuals into employment sectors over time on the basis of time-varying unobserved characteristics. I also address some concerns with respect to the potential for measurement error in the explanatory variable and account for the possibility of non-random attrition in the sample, which is a particular concern, given the long length of the panel.

Distinguishing between different models of informality has powerful implications for our understanding of structural transformation and the long-term dynamics of

the informal sector, relating to its size and permanence. In particular, labour market segmentation in developing countries is often associated with the introduction of labour market institutions such as minimum wage laws and the legal protection of worker interests. Several interest groups have attempted to causally link the political introduction of these institutions with reduced inter-sectoral labour mobility and widening inequality between the formal and informal sectors of an economy. In this paper, I argue that this traditional perspective is misleading because of the existence of a high degree of mobility between employment sectors in the data and the absence of significant earnings differentials across sectors after accounting for individual heterogeneity in productivity and preferences.

Much of the developing country research on informality and firm size has focused on Latin American and African countries. Given the importance of the informal sector in Asia, particularly self-employment, this is a critical gap in our understanding of labour markets and the paths to a better life in some of the world's largest developing countries.¹ This paper aims to fill part of that gap in the context of Indonesia, one of Asia's largest developing countries. Indonesia is a very interesting example of a rapidly transforming and fast-growing economy that continues to have a large informal sector contribution to both GDP and employment (Newhouse et al., 2010), but, as with much of developing Asia, there is limited research on informality in this country. Rand and Torm (2012) analyse a firm-level panel dataset from Vietnam and find evidence that much of the earnings gap between formal and upper-tier informal micro-enterprises is attributable to observed worker and firm characteristics. A recent paper by Nguyen et al. (2013) uses a panel dataset from Vietnam to argue that the earnings gap between the formal and informal sector varies across the income distribution and depends on the worker's job status. In Indonesia, Comola and de Mello (2011, 2013) estimate a reduced form model of selection into wage-employment, self-employment and unemployment or unpaid employment, but they exclude earnings differentials across sectors by focusing only on the salaried, formal sector. Given the large number of self-employed individuals in Indonesia, an analysis of salaried workers alone can only tell part of the story.

¹According to data from the IILS informality database, 78 per cent of employment in Asia is in the informal sector (Bacchetta et al., 2009).

The rest of the paper is organised in the following way: Section 2.2 examines the key issues with respect to labour market segmentation along informality and firm size. Section 2.3 describes the data and Section 2.4 presents empirical results on inter-sectoral earnings differentials. Section 2.5 concludes.

2.2 Understanding the key issues

2.2.1 Alternative views of the informal sector

The informal sector has traditionally been analysed in the context of labour market segmentation due to institutional factors. The formal segment of the labour market has been envisaged as a modern, urban sector characterised by high productivity and earnings; however, as a result of institutions such as labour unions and minimum wage legislation, it may generate a limited number of jobs at wages set above the market-clearing level. On the other hand, the informal, traditional, often rural, sector is characterised by low productivity and low earnings. This is a free-entry sector that accommodates all those who are unable to find jobs in the formal sector but are forced to work somewhere while they queue for better employment. Consequently, institutional barriers generate large earnings differentials between similar workers in both sectors. This staging hypothesis was developed into a model of rural-urban migration by Harris and Todaro (1970). Fields (1975) extended the model to introduce a “murky” informal urban sector as well, comprised of rural migrants to urban areas who are unable to find work in the high-wage formal sector but are unwilling to remain unemployed. A more recent form of the segmented labour markets hypothesis relies on the argument that formal sector firms pay efficiency wages to their workers (Akerlof and Yellen, 1986; Katz and Summers, 1989), while informal sector firms do not. This dualistic view of formal and informal sectors has informed and shaped public policy in many countries, leading to policies which seek to promote growth in the formal sector so as to absorb informal employment, as well as reform labour market institutions that are believed to generate the earnings disparity across sectors.

The more recent neoclassical model of the informal sector is a variation of a Roy model of selection into different types of employment: heterogeneous workers of dif-

ferent ability types and with different non-monetary preferences exercise a choice to seek employment in formal or informal sectors so as to maximise their net earnings (Heckman and Sedlacek, 1985; Magnac, 1991). Earnings differentials between observably similar workers in different sectors reflect unobserved heterogeneity among these workers. Maloney (1999) uses panel data from Mexico to argue that many workers in the informal sector voluntarily take up jobs in that sector, not merely to fulfil a temporary role till they get employment in the formal sector. Pratap and Quintin (2006) use semi-parametric estimation techniques to argue on the basis of evidence from Argentina that a seeming earnings differential between the formal and informal sectors is simply a reflection of unobserved individual heterogeneity. Falco et al. (2011) use panel data from Ghana and Tanzania and find that conditioning on observed individual and firm characteristics, 50 per cent of the earnings differential across formal and informal sectors is attributed to unobserved individual heterogeneity.

In the Indonesian context, several influential policy publications have suggested that informality is a bad equilibrium that many workers find themselves in because they are unable to get jobs in the formal sector. According to the World Bank, “The informal sector is mostly a trap for Indonesian workers...[because] their actual earnings are less than their wages would be in the formal sector.” (Newhouse et al., 2010, p.65). The resultant policy prescription is “the accelerated creation of ‘better’ jobs” (Newhouse et al., 2010, p.70). According to the OECD, “The large informal sector distorts the labour market in Indonesia” and that the “appropriate reform of labour regulations will...enhance the movement of workers from the informal sector to the formal economy” (Organisation for Economic Co-operation and Development, 2011, p.91). This debate has intensified in the last decade as several legislative measures have been introduced to increase protection for workers, particularly in the formal sector.² To assess the validity of these recommendations, however, we need a better understanding of the underlying cause of the phenomenon of informality.

One factor that is often overlooked in analyses of the formal sector earnings premium is the firm-size effect – the widely documented empirical regularity that

²Labour regulations in Indonesia were tightened in 2003 with the passage of the Manpower Law (No 13/2003), which increased severance rates to be paid when firing employees and reduced the number of temporary employees that could be hired by any firm.

workers in large firms earn more than identical workers in small firms.³ Since informal sector enterprises tend to be smaller than formal sector firms, the firm-size effect may be closely linked to the formal sector earnings premium. Entrepreneurs may choose to stay small and escape the regulatory radar, or to expand in size and experience scale benefits as well as higher regulatory costs. In developing countries such as Indonesia, these regulatory costs can be substantial.⁴ It is of interest, therefore, to identify whether there is any significant earnings premium associated with formality over and above that associated with the size of the firm.

2.2.2 Informality in Indonesia

In Indonesia, few workers sign formal contracts with their employers. Only 5 per cent of the active workforce in the 2007 survey reported themselves as being employers or employees with permanent contracts while only 8 per cent of the active workforce has any kind of contract at all (Newhouse et al., 2010). Similarly, social security insurance coverage is very limited. While workers' social security is protected by legislation – the Jamsostek programme (Law No.3/1992) – this extends only to employees of those firms with 10 or more workers.⁵ In this context, the distinction between formal and informal employment on the basis of legal protections alone seems unhelpful. The Central Bureau of Statistics of the Indonesian government – the *Badan Pusat Statistik* (BPS) – distinguishes between formal and informal self-employment: while professionals and managers are considered a part of formal employment, all other own-account workers are considered a part of informal employment. The self-employed with additional labour are a part of the formal sector only if they operate in the non-agricultural sector. However, BPS considers all employees within the salaried sector to be a part of formal employment – including

³See Oi and Idson (1999) for a review of the firm-size wage effect in developed countries. Papers that identify this effect in developing countries include Bernard et al. (2010), El Badaoui et al. (2008), Marcelle and Strobl (2003), Schaffner (1998), Soderbom and Teal (2004), Soderbom et al. (2005), Strobl and Thornton (2004), Velenchik (1997), Verhoogen (2008).

⁴Indonesia was ranked 128 out of 185 countries in an 'ease of doing business' index, and ranked 165 of 185 in an 'ease of starting a new business' index (World Bank, 2013).

⁵In 2004, the National Social Security System Law (SJSN Law No.40/2004) was passed, which envisages the introduction of universal social security protection for all workers. As of the end of 2007, six pilot projects had been introduced to explore the most effective way to roll out social security protection on a contributory basis to workers in the informal sector (Widarti, 2010).

employees of micro-enterprises, large firms and the entire public sector (see Table 2.1).

Table 2.1: Official definition of informality

Status	Industry	
	Agriculture	Non-agriculture
Self-employed with no workers	Informal	Informal
Self-employed with workers	Informal	Formal
Employees	Formal	Formal

Source: Based on the Newhouse et al. (2010, p.62) mapping of the official Statistics Indonesia (BPS) definition of informality.

This definition makes the strong assumption of homogeneity within salaried work, which may be difficult to identify in practice. Worker protection varies widely across different types of firms, from the micro-enterprise hiring only casual labour to the large plant with organised workers. The ranks of the employees also include workers in the public sector: an important part of the labour markets of most developing countries, including most of Asia. The public sector has unique wage-setting mechanisms: in economies with limited legal protections for workers, government jobs are often the only jobs in which minimum wage legislation is consistently enforced and where there is a low chance of an employee being fired. This often gives rise to large observed public sector earnings differentials over private sector employment; for instance, Kerr and Teal (2012) document a large public sector earnings premium in South Asia even after controlling for unobserved worker characteristics. The public sector is an important source of employment in Indonesia as well: in the Indonesian Family Life Survey (IFLS) dataset, over 10 per cent of the sample of earners is employed by the government.

Given these features, this paper considers the narrow official definition as well as an expanded definition that takes into account differences between the public sector, informal micro-enterprises and larger firms. In an environment where few formal contracts are signed between employers and employees, institutions such as trade unions become extremely important for protecting worker rights. The law on trade unions (Workers/Labour Union Act of Indonesia No. 21/2000) requires a minimum of 10 workers to form a union (International Labour Organization, 2002). As

mentioned earlier, social security protection is currently only extended to employees at firms of 10 or more workers as well. Consequently, this paper uses 10 workers as the benchmark for two different types of firms – “large” and “small” firms; this can be interpreted as firms large enough to be given statutory protections by the state and firms too small to be protected by the state. This distinction between type of firm goes beyond the size of the firm: I argue that the two types of firms are institutionally different in unobservable ways to be proxied by whether they are allowed to organise or protect their labour or not.

Some of these different characteristics of salaried employment are described in Table 2.2, which lists the share of salaried employees in the informal sector (fewer than 10 employees), formal sector (10 or more employees) and public sector who have written contracts with their employers and receive various types of benefits. The data is only available for 2007 and includes all salaried employees who were interviewed that year. The overwhelming share of workers, even within the public sector, do not work on a contract of any kind. Only 8 to 11 per cent of informal salaried employees can claim pension and severance pay or medical benefits. The share of formal salaried workers receiving pension and medical benefits is higher but still below 50 per cent. In the public sector, more than three-quarters of all employees can claim pension and medical benefits. However, informal salaried employees do seem to receive some benefits in kind, with 39 per cent reporting that they were given either some meals or raw food by their employers.

Taking into account these distinctions, this paper reports results for both the official definition as well as an extended definition, which incorporates the following categories: the informal self-employed, the informal salaried (those working for firms that have fewer than 10 workers), those employed in the formal sector (those working for firms with more than 10 workers and those in formal self-employment, as per the BPS definition), and those employed in the public sector. In addition, I introduce controls for firm size (over and above the implicit sorting by size between informal and formal private sector enterprises).

This view of informality broadens the traditional dichotomy of formal and informal into a spectrum of formality, which depends substantially on firm size, the distinction between self-employment and salaried employment, as well as the potential for worker protection. This paper will argue that such a spectrum of formality

Table 2.2: Share of salaried workers who worked on contracts and received other employee benefits in 2007

	Informal salaried	Formal salaried	Public Sector	Total
<i>Contract type</i>				
Fixed time contract	0.02	0.14	0.09	0.08
Non-fixed time contract	0.03	0.07	0.21	0.07
No contract	0.95	0.79	0.70	0.84
<i>Benefits</i>				
Pension/severance	0.06	0.32	0.73	0.27
Food	0.43	0.33	0.18	0.35
Housing	0.04	0.06	0.13	0.06
Transport	0.05	0.19	0.16	0.12
Medical	0.10	0.41	0.67	0.31
Credit	0.18	0.29	0.37	0.26
Observations	4,250	4,247	1,461	9,958

Source: Own calculations based on the fourth wave of the IFLS (2007). Each cell contains the share of workers who are employed on contract or receive a given benefit.

is more likely to be found in practice than the polarised view of two sharply distinct but internally homogeneous sectors – the self-employed and the wage-employed.

2.3 Summary of the data

2.3.1 The Indonesian Family Life Survey

The data is drawn from three waves of the Indonesian Family Life Survey (IFLS), an ongoing longitudinal socio-economic and health survey that collects information at the level of the individual, the family, the household, and the community on a wide range of topics, including those relevant to this paper – economic well-being, employment histories, labour market status and educational attainment. The survey is based on an initial sample of 7,224 households (approximately 22,000 individuals) living in 13 of Indonesia’s 26 provinces in 1993, and representing 83 per cent of the Indonesian population in that year. These IFLS-1 households were subsequently re-contacted in 1997, 2000 and 2007.⁶ Since the IFLS tracks individuals who split off

⁶The data for the first wave of the panel was collected between August and November 1993, and between September 1993 and January 1994. The data for the second wave was collected between

from original survey households, the number of surveyed households has increased with every wave, with 13,535 households (approximately 44,100 individuals) interviewed in 2007. Importantly, the survey collects information on self-employed workers as well as salaried employees.

This paper confines itself to the study of male adults aged between 15 and 65 (inclusive), all of whom were interviewed in 1997 and some of whom were interviewed in 2000 and 2007 as well, who reported themselves to be gainfully employed with a (strictly) positive income in the past month. Due to concerns about attrition that are addressed in Section 2.4, individuals who drop out of the survey after 1997 are not included in the analysis. This allows us to treat attrition as an absorbing state – those who initially drop out of the survey are not allowed to re-enter. Observations from the first wave in 1993 are excluded because respondents did not report data on the size of the firms at which they worked. Females are excluded because of concerns relating to the selection of survey respondents into employment. This issue is discussed in greater detail below.

2.3.2 Overview of the data and summary statistics

Earnings and employment

All individuals interviewed by the IFLS assigned themselves to one of the following categories: self-employed (with or without temporary and permanent employees) or wage-employed (in the private sector or in the government sector). Several reported themselves as being unpaid family workers; in the absence of a reported income, these individuals were excluded from the analysis. Individuals were asked how many people worked at their place of employment, from which a firm size variable was constructed. Information was also collected on the respondent's occupational category⁷

August and December 1997. The third wave of data was collected between June and October 2000. The fourth wave of data was collected between November 2007 and April 2008. Information on all four waves can be found in Frankenberg and Karoly (1995); Frankenberg and Thomas (2000); Strauss et al. (2004, 2009).

⁷Occupational categories are professional or technical; administrative or managerial; clerical; sales; service; agriculture; production-related occupations; and others.

and industry of employment.⁸ The industrial categories have been broadly categorised into three – agriculture; manufacturing, mining and construction (referred to as ‘manufacturing+’) and services, including all other industrial sectors.

For the purposes of this analysis, the following employment groups were created – the informal self-employed, the informal salaried, those employed in the formal sector, and those employed in the public sector. I distinguish between firm size for self-employed workers, private sector firms, and public sector enterprises, in order to allow for asymmetric effects of firm size on earnings from all types of employment. In other words, I do not assume that firm size affects public sector productivity, for example, in the same way as it affects private sector productivity.

Table 2.3 presents information on the distribution of employees across the different sectors of employment. More than half the sample in any given year is categorised as informal employment, about a third is categorised as formal employment and just over 11 per cent of sample is employed in the public sector. The distribution shows that the share of government workers in the sample has increased from 1997 to 2007 while the share of informal salaried workers has declined, suggesting that non-random attrition from the panel is a potential problem. I turn to this concern later.

Comparing the incomes of those who are self-employed to those who are wage-employed is controversial in developing countries because the self-employed in informal businesses may not keep sound accounts and their reported incomes may be subject to measurement error. However, the exclusion of the incomes of the self-employed eliminates more than half of the sample employed in the rural economy and at least a third of those employed in the urban economy. Rather than lose this data and the chance to examine labour market transitions between salaried and self-employment, the incomes of the self-employed are included in this analysis. As part of the survey design, respondents are asked to carefully distinguish between revenues, business expenses and net profits over the last month, which may ameliorate some of the concerns with respect to misreporting of income; however, it is possible

⁸Industry categories are agriculture; mining and quarrying; electricity, water and gas; construction; wholesale, retail, tourism and hospitality; transport, storage and communications; finance and related services; and community and personal services.

Table 2.3: Distribution of earners across employment categories over time

	1997	2000	2007	Total
Informal self-employed	1,431 (28.4)	1,330 (32.8)	853 (31.8)	3,614 (30.7)
Informal salaried	1,156 (22.9)	1,064 (26.3)	605 (22.5)	2,825 (24.0)
Formal salaried	1,898 (37.6)	1,203 (29.7)	904 (33.7)	4,005 (34.0)
Public sector	561 (11.1)	455 (11.2)	322 (12.0)	1,338 (11.4)
Total	5,046 (100.0)	4,052 (100.0)	2,684 (100.0)	11,782 (100.0)

Source: Own calculations based on the IFLS. The share of workers in a particular sector as a proportion of the total number of workers in one survey wave is given in parentheses.

that some of the respondents are not able to precisely distinguish between household and business accounts, and particularly between different types of expenditure.

Data was also collected on the number of hours worked in the last week and number of hours worked most weeks. I use the number of hours worked in the last week and scale it up in order to obtain an estimate of the number of hours worked per month. Rather than construct estimates of hourly earnings, I control for the (log) number of hours worked per month in order to avoid exacerbating any existing measurement errors. Figure 2.1 presents kernel density estimates of (log) real monthly earnings by employment category, which reveals a distinct earnings hierarchy: public sector workers earn substantially more than other workers, followed by formal sector workers, informal salaried workers, and finally the informal self-employed. The distribution of earnings across the public sector suggests that government wage-setting institutions are very different from those of the private economy. This emphasises the need to treat the public sector as distinct from the private formal sector.

Figure 2.1: (Log) Real monthly income densities by employment category

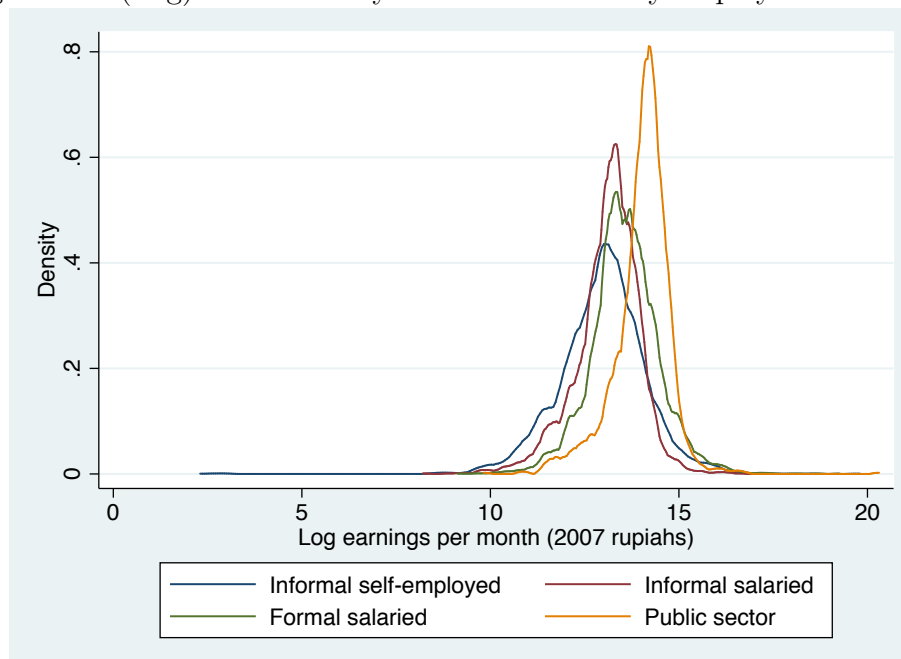


Table 2.4 presents information on average earnings, hours worked per month and firm size across the different employment categories. The comparison of average earnings reveals the same hierarchy of earnings as in Figure 2.1: public sector employees earn the most by a large margin, followed by formal sector employees, informal salaried workers and informal self-employed workers. Again, the most remarkable difference is between workers in the public sector and workers in the private sector: the average public sector employee earns more than twice as much as the average informal sector worker in 2007. Also notable is the small size of the enterprises in which most earners are employed, though this is partly an artefact of how different employment sectors are defined: the average informal self-employed worker hires one or two additional workers, while the average informal salaried employee works in a firm of four. The “firm size” variable for the public sector, however, can be misleading because some of the surveyed public sector employees are likely to work for organisations such as the civil service or essential service providers such as the police force or fire brigade, where firm size is not a very meaningful concept. Others, however, may work for public sector enterprises, which are similar to private sector firms.

Table 2.4: Work characteristics by employment category over time (mean)

	1997	2000	2007
<i>Informal self-employed</i>			
Real monthly income (Rupiah)	627,631	898,453	1,509,216
Hours per month	180	184	186
Firm size	2	3	2
<i>Informal wage-employed</i>			
Real monthly income (Rupiah)	704,997	674,008	704,056
Hours per month	202	212	205
Firm size	4	4	4
<i>Formal sector</i>			
Real monthly income (Rupiah)	1,130,312	1,390,923	1,561,431
Hours per month	211	223	213
Firm size	139	96	145
<i>Public sector</i>			
Real monthly income (Rupiah)	2,613,373	1,430,407	1,867,658
Hours per month	181	175	183
Firm size	219	82	134

Source: Own calculations based on the IFLS.

Education and demographic variables

The Indonesian schooling system is organised into basic, secondary, and higher education. Basic education is a nine-year programme of free, compulsory schooling for all children between the ages of 7 and 15, consisting of elementary school (six years) followed by junior secondary school (three years). At the end of basic education, students who have successfully completed a national examination may enter senior secondary school (three years) through either an academic or a vocational track. After completing the senior secondary national exam, students may gain access to higher education at universities, academies or colleges. I use the categorical variable of the highest grade of schooling completed ('no or some elementary schooling', 'completed elementary schooling', 'completed junior school', 'completed senior school' and 'completed college education') as a measure of educational attainment.

Other demographic variables on which information has been collected and which I use in the analysis include age as a continuous variable, which I use as a proxy for work experience, and indicators for whether a respondent is male, married, Muslim, and located in an urban area. I also use indicators for residence in each of the 20 provinces from which the sample has been drawn.

At least some part of the public and formal sector premium noted in the previous section can be attributed to the difference in human capital endowment of workers across sectors. Table 2.5 shows that the mean level of schooling for those in the public sector is about 12 years (a high school degree), while the informal self-employed, on average, have only completed 6 years of schooling (equivalent to primary school). Private sector employees are, on average, younger than the self-employed and public sector employees, and, accordingly, are less likely to be married. There is also a strong association between home-ownership and self-employment.

2.4 Explaining intersectoral earnings differentials

2.4.1 Empirical framework

Explaining the variation of earnings across different components of human capital is a widely researched subject and the point of departure for most studies is the Mincer

Table 2.5: Demographic characteristics by employment category (mean)

	Informal self-employed	Informal salaried	Formal salaried	Public sector
Years of schooling	6.35	7.17	8.64	11.80
Age	43.29	36.73	38.04	43.04
Married	0.92	0.78	0.83	0.94
Home-owner	0.87	0.78	0.75	0.74
Urban	0.36	0.56	0.67	0.64

Source: Own calculations based on the IFLS. These characteristics are treated as time-invariant over the length of the survey.

earnings regression (Mincer, 1974). In the context of panel data on i individuals across t time periods, the Mincer earnings regression can be represented as

$$y_{it} = \theta + \mathbf{X}'_{it}\boldsymbol{\beta} + \alpha_i + d_t + \varepsilon_{it} \quad (2.1)$$

where y_{it} are the (log) monthly earnings for individual i at time t , θ is an intercept common to all workers, $\mathbf{X}_{it}$ is a $K \times 1$ vector of time-varying characteristics of individual i at time t , α_i is a vector of time-invariant observable and unobservable characteristics of individual i , d_t contains indicators for time periods, and ε_{it} is an idiosyncratic error term. This regression is based on Mincer's accounting-identity model, which captures the relationship between observed earnings, potential earnings and human capital investment through schooling as well as on-the-job training. The human capital determinants of earnings, according to Mincer, are captured in a linear function of years of schooling and a quadratic function of work experience.

Of interest is the earnings premium associated with working in a particular segment of the labour market over and above the human capital controls of the Mincerian earnings model. This requires the estimation of a reduced form earnings function, including controls for sector of employment. If the coefficients on the sector of employment can be estimated consistently, one can estimate the earnings premium associated with employment in a given sector. This estimating equation is given as follows:

$$y_{it} = \theta + \mathbf{X}'_{it}\boldsymbol{\beta} + \mathbf{Sector}'_{it}\boldsymbol{\gamma} + \alpha_i + d_t + \varepsilon_{it} \quad (2.2)$$

Measures of human capital endowment included in $\mathbf{X}_{it}$ are a quadratic function of age, as a proxy for work experience, and four dummies representing whether the individual completed primary schooling, junior school, senior school and post-secondary education. Job characteristics included in $\mathbf{X}_{it}$ are indicator variables representing the industry of employment at time t – one of agriculture, manufacturing+, and services – as well as a variable measuring the size of the firm at which the individual works. $\mathbf{Sector}_{it}$ is a vector of indicators representing the sector in which individual i is employed at time t : I use the official sectoral distinction, with a single indicator for whether the individual is employed in the formal sector, as well an extended definition, with indicators for whether the individual is employed in the informal salaried sector, formal sector or public sector. The γ s are the estimated earnings effects associated with working in a given sector of employment, over and above observed human capital endowments, industry choice and firm size.

There are several methodological concerns to be tackled. First, variation in unobserved abilities or preferences of individual workers could be influencing both their decision and ability to work in a particular sector as well as their earnings from employment; for example, we might expect workers in the formal sector to have not only a greater human capital endowment than those in the informal sector but also a greater degree of professional ability or motivation that increases their desire to work in the formal sector, the probability that they will find a job in the formal sector, as well as their earnings in that employment. More generally, the existence of unobserved time-invariant heterogeneity at the level of the individual could bias the estimates of γ . I control for this by exploiting the panel component of the dataset to observe the same individuals in different sectors and estimate Equation (2.2) to allow for individual fixed effects as well as in first differences. This also controls for endogenous selection into a given sector of employment as long as the unobserved factors affecting selection are time-invariant. There may be reason to believe, however, that the unobservables affecting selection into employment may be time-varying, in which case the fixed effects estimates may be subject to bias due to endogenous sample selection. This is of particular concern because of the large number of unpaid family workers in the sample, which is typical of many developing countries. I address this concern by incorporating a selection model and find no evidence of bias due to non-random selection.

A second concern is that the fixed effects approach does not control for time-varying unobservable factors which could influence selection into any given employment sector. I look at the characteristics that predict switching between employment sectors and present some evidence on the size and direction of this potential bias in the estimated γ s by exploiting the fact that the East Asian crisis of 1998 generated a number of involuntary labour market transitions between different sectors. I also rely on the panel dimension of the data to instrument the endogenous variables with their lagged values.

A third concern is that of measurement error in both the dependent and explanatory variables. Measurement error in the dependent variable leads to inconsistent estimation of γ if the measurement error is correlated with some of the correctly measured explanatory variables. Measurement error in the explanatory variables will lead to an attenuation bias in the γ estimate. This can be exacerbated with the use of panel data – leading to an even greater bias in the estimated coefficients towards zero. I attempt to gauge the extent of measurement error by comparing fixed effects and first-differenced estimates. I also implement a simple test of the extent of measurement error by relying on the interviewer’s assessment of the reliability of the respondent’s answers.

A fourth concern is the potential for attrition, whereby respondents who drop out of the survey differ systematically from those who stay in the survey, particularly in the ways in which employment sector affects earnings, leading to biased estimates of γ . Attrition is a concern for any panel dataset, particularly one which ranges over a period of 10 years. I control for the effect of attrition in the following ways: first, I assume attrition is an absorbing state and exclude individuals who exit the survey and re-enter at a later point. I then use observed characteristics of surveyed individuals to estimate inverse probability weights, so as to increase the weight on individuals who were more likely to attrit but who remained in the survey. I also estimate reduced form models to explain attrition and use these models to correct for attrition in future waves of the survey.

2.4.2 Results

A straightforward estimation of Equation (2.2) by pooled Ordinary Least Squares (OLS) produces consistent estimates of γ if the explanatory variables, $\mathbf{X}_{it}$, are uncorrelated with the unobserved individual-specific effects, α_i . I include geographical controls for province of residence and whether the individual lives in an urban area, demographical characteristics, such as whether the individual is male, married or a Muslim, controls for occupational type and time indicators for the year in which the survey was conducted. Robust standard errors are used to allow for autocorrelation in the ε_i s. The results of the OLS estimate are presented in Column 1 of Table 2.6 as a benchmark.

The baseline results in Table 2.6 consider the official definition of formality – categorising all employees, public sector workers, and non-agricultural self-employed workers as part of the formal sector. There is a significant and large earnings premium of 21 per cent associated with working in the formal sector. This formal sector premium persists over and above controls for firm size. Firm size has a significant impact on earnings for all workers – the self-employed (28 per cent) and the salaried in both the private sector (6 per cent) and the public sector (12 per cent). With respect to human capital, the Indonesian experience is typical of many developing countries: the earnings function is concave with respect to age and displays some convexities with respect to schooling, with very high economic returns associated with the completion of a university degree.

2.4.3 Controlling for unobserved heterogeneity

The strong assumption of uncorrelated unobserved effects can be relaxed by using a panel dataset to identify inter-sectoral earnings differentials by exploiting movements by individuals into different sectors between panels to control for time-invariant individual-specific heterogeneity. A number of the workers surveyed in the IFLS moved between different occupational sectors across the three waves: these transitions are shown in Table 2.7 and Table 2.8. There is significant movement between the categories of private self- and wage-employment between successive waves – between 30 to 55 per cent of private sector workers across informal and formal employment move between different occupational sectors. Movement in and out of

Table 2.6: OLS, fixed effects and first-differenced earnings regressions - official definition of formality

	OLS	Fixed Effects	First Differenced
Official formal sector	0.210*** (0.027)	0.018 (0.034)	0.032 (0.037)
Log firm size	0.055*** (0.006)	0.044*** (0.008)	0.038*** (0.009)
Log no. of employees	0.280*** (0.022)	0.122*** (0.023)	0.119*** (0.025)
Log PSE size	0.118*** (0.009)	0.042*** (0.014)	0.047*** (0.011)
Log hours per month	0.272*** (0.020)	0.200*** (0.023)	0.185*** (0.024)
Manufacturing+	0.282** (0.113)	0.250*** (0.044)	0.228*** (0.045)
Services	0.170 (0.112)	0.187*** (0.046)	0.195*** (0.047)
Age	0.059*** (0.006)	0.054*** (0.011)	0.061*** (0.012)
Age-squared/100	-0.069*** (0.007)	-0.056*** (0.011)	-0.064*** (0.012)
Completed elementary	0.176*** (0.025)		
Completed junior	0.355*** (0.032)		
Completed senior	0.558*** (0.028)		
Completed college	0.938*** (0.046)		
Year dummies	Yes	Yes	Yes
Occupation dummies	Yes	No	No
Province dummies	Yes	No	No
Demographic variables	Yes	No	No
Observations	11,782	11,782	6,736
<i>Adj.R</i> ²	0.312	0.051	0.040
No. of workers		5,046	

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly earnings. The reference category for employment is informal employment. Standard errors are clustered by individual. Demographic variables include gender, marital status and a dummy for whether the person is located in an urban area. All regressions include time dummies.

the public sector, however, is relatively limited with 17 per cent of public sector workers exiting this sector between successive waves. The most interesting feature of these two tables is the significant movement out of the formal sector in the three years between 1997 and 2000, compared to the seven years between 2000 and 2007; this phenomenon is closely related to the employment effects of the East Asian crisis of 1998 and will be discussed further in Section 2.4.5.

Table 2.7: Transitions between sectors - 1997 to 2000

1997 to 2000	Informal self-employed	Informal salaried	Formal salaried	Public Sector	Total
Informal self-employed	535 (59.78)	128 (14.30)	221 (24.69)	11 (1.23)	895 (100.00)
Informal salaried	66 (15.75)	242 (57.76)	105 (25.06)	6 (1.43)	419 (100.00)
Formal salaried	491 (22.97)	606 (28.34)	983 (45.98)	58 (2.71)	2,138 (100.00)
Public sector	21 (4.24)	23 (4.65)	39 (7.88)	412 (83.23)	495 (100.00)
Total	1,113 (28.20)	999 (25.31)	1,348 (34.15)	487 (12.34)	3,947 (100.00)

Notes: Each cell gives the total number of workers who transited from one sector to another, with the share of a given sector that transited to another sector in parentheses.

Since these inter-sectoral transitions form the basis of the estimation strategy that will follow, I compare the baseline characteristics of individuals who transition between sectors in two consecutive measurement periods and those who don't (that is, the 1997 characteristics of those who switch sectors in 1997-2000 versus those who don't, and the 2000 characteristics of those who switch between sectors in 2000-07 versus those who don't) in Table 2.9. The first two columns compare switchers and non-switchers between the officially defined formal and informal sectors. Since the comparison is between individuals who switch in and out of the formal sector only, switchers are more likely to be younger, more educated and urban. They are more likely to be employed in manufacturing and services sectors, which is where formal

Table 2.8: Transitions between sectors - 2000 to 2007

2000 to 2007	Informal self-employed	Informal salaried	Formal salaried	Public Sector	Total
Informal self-employed	486 (68.84)	82 (11.61)	131 (18.56)	7 (0.99)	706 (100.00)
Informal salaried	161 (22.64)	346 (48.66)	186 (26.16)	18 (2.53)	711 (100.00)
Formal salaried	190 (20.13)	225 (23.83)	498 (52.75)	31 (3.28)	944 (100.00)
Public sector	9 (2.79)	15 (4.64)	33 (10.22)	266 (82.35)	323 (100.00)
Total	846 (31.52)	668 (24.89)	848 (31.59)	322 (12.00)	2,684 (100.00)

Notes: Each cell gives the total number of workers who transited from one sector to another, with the share of a given sector that transited to another sector in parentheses.

employment is located, and they work at larger firms. When considering switching between four disaggregated sectors (Columns 3 and 4), some of these differences are less acute. However, switchers are, on average, less educated than non-switchers, and are most likely to be employed in the manufacturing sector. They earn less than non-switchers and work at smaller firms.

Figure 2.2 illustrates the earning density functions across different categories of employment among switchers only and provides a striking contrast to Figure 2.1 on page 69. Among switchers, the original hierarchy of earnings dissolves, and only public sector workers earn appreciably more than the others. There is considerable overlap between workers in the private sector, suggesting that the observable differences between formal and informal sectors, and between wage and salaried employment, mask heterogeneity in individual preferences and abilities.

I exploit these inter-sectoral transitions to estimate Equation (2.2) with the following fixed effects estimator:

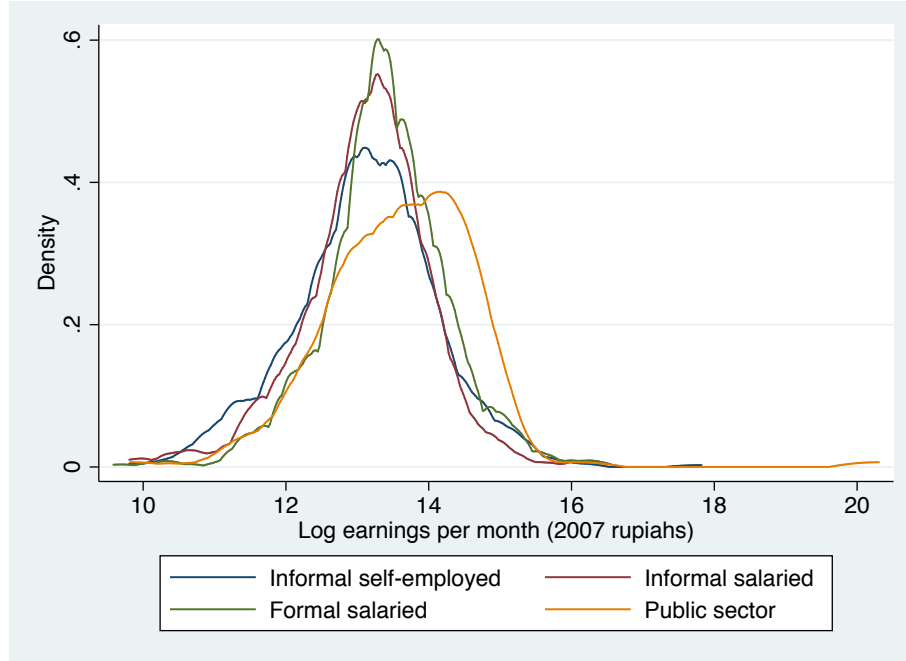
$$y_{it} - \bar{y}_i = (\mathbf{X}_{it} - \bar{\mathbf{X}}_i)' \boldsymbol{\beta} + (\text{Sector}_{it} - \overline{\text{Sector}_i})' \boldsymbol{\gamma} + (\varepsilon_{it} - \bar{\varepsilon}_i). \quad (2.3)$$

Table 2.9: Comparing switchers and non-switchers

	Official definition		Disaggregated definition	
	Non-switcher	Switcher	Non-switcher	Switcher
<i>Job characteristics</i>				
Log income	13.29 (0.99)	13.36 (0.88)	13.34 (0.98)	13.26 (0.93)
Hours worked per month	196.57 (83.61)	209.95 (88.50)	197.60 (81.48)	204.66 (90.41)
Log firm size	2.02 (1.49)	2.37 (1.52)	2.14 (1.56)	2.09 (1.43)
Agriculture	0.26 (0.44)	0.14 (0.34)	0.24 (0.43)	0.20 (0.40)
Manufacturing+	0.26 (0.44)	0.36 (0.48)	0.26 (0.44)	0.34 (0.47)
Services	0.48 (0.50)	0.50 (0.50)	0.49 (0.50)	0.47 (0.50)
<i>Demographic characteristics</i>				
Years of Schooling	8.01 (3.97)	8.03 (3.79)	8.23 (3.98)	7.70 (3.80)
Age	37.44 (10.27)	35.56 (10.12)	37.38 (10.19)	36.18 (10.33)
Married	0.86 (0.35)	0.82 (0.39)	0.85 (0.35)	0.83 (0.38)
Urban	0.52 (0.50)	0.58 (0.49)	0.53 (0.50)	0.54 (0.50)
<i>Household characteristics</i>				
Home-owner	0.80 (0.40)	0.75 (0.43)	0.79 (0.40)	0.77 (0.42)
Household size	5.90 (2.47)	5.89 (2.67)	5.90 (2.50)	5.88 (2.58)
Number of children	1.74 (1.31)	1.72 (1.38)	1.73 (1.31)	1.74 (1.36)
Number of old people	0.21 (0.49)	0.19 (0.45)	0.21 (0.49)	0.20 (0.46)
Is head of household	0.66 (0.47)	0.58 (0.49)	0.66 (0.47)	0.61 (0.49)
Household head entered senior secondary school	0.27 (0.44)	0.24 (0.43)	0.29 (0.45)	0.22 (0.41)
Household head is employed	0.94 (0.24)	0.93 (0.25)	0.94 (0.24)	0.94 (0.24)
Observations	4,785	1,951	4,017	2,719

Notes: Values reported are sample means, with standard deviations in parentheses. The total sample of 6,736 transition states includes all pairs of observations of the same individuals observed in two consecutive time periods (transitions that have taken place in 1997-2000 and 2000-07).

Figure 2.2: (Log) Real monthly income densities by employment category – switchers



For consistency, the fixed effect estimator requires strict exogeneity of the time-varying characteristics conditional on the unobserved effect: $E[\varepsilon_{it} | \mathbf{X}_{it}, \mathbf{Sector}_{it}, \alpha_i] = 0$. Equation (2.2) can also be estimated by pooled OLS on the first-differenced transformation of the data:

$$y_{it} - y_{i,t-1} = (\mathbf{X}_{it} - \mathbf{X}_{i,t-1})' \boldsymbol{\beta} + (\mathbf{Sector}_{it} - \mathbf{Sector}_{i,t-1})' \boldsymbol{\gamma} + (\varepsilon_{it} - \varepsilon_{i,t-1}) \quad (2.4)$$

For consistency, this estimator requires the same assumption of strict exogeneity of the time-varying characteristics conditional on the unobserved effect. Standard errors are clustered by individual worker to allow for serial correlation in the ε_{it} s over time. If the errors are uncorrelated, estimation on the first-differenced data will be less efficient than the fixed effects estimation, while if the errors follow a random walk, then the OLS estimation on the pooled first-differenced data will be more efficient (see Wooldridge, 2002, p.284-285). The first-differenced estimation will be important for comparison when we look for evidence of measurement error in the explanatory variables and later correct for possible non-random attrition in the panel. All time-invariant characteristics, such as demographic variables and schooling attainment drop out of the regressions. Time-varying characteristics, such

as age, sector of employment, firm size and employing industry remain in the regression. Time indicators are also included to control for common effects associated with a given year.

I present the results from both models in Columns 2 and 3 of Table 2.6 on page 76. After controlling for unobserved time-invariant, individual-specific heterogeneity, the large formal sector premium of the OLS disappears in the fixed effects and first-differenced regressions – emphasising the importance of sorting across employment sectors according to individual ability and preferences. The estimated coefficient on the formal sector variable is both small (1.8 - 3.2 per cent) and insignificantly different from zero at the 10 per cent confidence level. Firm size continues to be important in both specifications, as well as asymmetric across different sectors of employment, with the impact of firm size on the incomes of the self-employed (12 per cent) being much larger than the impact of firm size on the incomes of the salaried (3.8 - 4.7 per cent). The size of the coefficient on firm size for the self-employed is much smaller than the OLS estimates, suggesting that highly able entrepreneurs are both more productive and more likely to be able to expand their enterprises. Manufacturing+ as well as the services industry are associated with significantly higher earnings relative to the omitted category of agriculture.

One potential source of bias in this estimation is from the non-random selection of the observed sample into employment for a positive income. The fixed effects and first differenced estimators will be consistent if the unobserved variables influencing selection of individual workers into the workforce are time-invariant. In this case, both estimators account for the selection process by accounting for the presence of correlated individual effects, or the α_i s of Equation (2.2). If, however, the selection process is related to the ε_{it} s of Equation (2.2) – or the unobservables affecting selection into the workforce are time-varying – then these estimates may be biased. This is likely to be particularly true of female respondents, many of whom report themselves to be “unemployed” but work in households or household-owned businesses. I restrict the sample of workers to males only because close to 90 per cent of the male working age population is reported to be employed, reducing the concerns related to selection into employment. However, an additional concern is that of the respondents who claim to be employed, over 20 per cent cannot report a positive

income because they are employed as unpaid family workers in a household business. These workers are also excluded from this analysis and non-random selection into income-earning positions may be a source of bias in the fixed effects estimates. As a robustness check, I examine and test for the presence of this selection bias using the panel estimator developed by Wooldridge (1995). Following Chamberlain (1984), Wooldridge's estimator parametrises the individual unobservable effect in the wage equation (the α_i), as well as individual unobserved effects in the selection process as linear functions of the lags and leads of all observable variables. With some additional assumptions detailed in the appendix, this parametrisation forms the basis of both a test for and a correction of the presence of selection bias. The estimates for the corrected sample, reported in the appendix, are not considerably different from the uncorrected sample, which gives some reassurance that selection into paid work is not biasing the results.

2.4.4 Disaggregating the formal sector

After controlling for firm size and individual-specific heterogeneity, there is little evidence of labour market segmentation along the lines of the official definition of formality in Indonesia. However, it is possible that the definition of the formal sector is much too broad to be useful, and masks considerable heterogeneity within the defined categories of informal and formal employment. In other words, there may not seem to be any institutional segmentation between formal and informal employment as officially defined by the government, but there may be barriers to entry within different parts of the formal and informal sectors. To explore this possibility, a more granular definition of employment sectors is adopted, dividing employment into informal self-employment, informal salaried employment, formal employment and public sector employment (these distinctions are discussed in Section 2.2). The same estimation strategy of the previous section is used and results are presented in Table 2.10.

In the OLS regression (Column 1 of Table 2.10), a sectoral hierarchy of earnings is generated whereby the public sector is associated with the highest earnings with a very large premium of 62 per cent over the earnings of the informally self-employed, followed by formal employment, informal salaried employment and, finally, informal

Table 2.10: OLS, fixed effects and first-differenced earnings regressions - broader employment sector definition

	OLS	Fixed Effects	First Differenced
Formal sector	0.190*** (0.031)	0.026 (0.037)	0.043 (0.041)
Informal salaried	0.177*** (0.030)	-0.007 (0.038)	0.011 (0.041)
Public sector	0.620*** (0.064)	0.221** (0.103)	0.167* (0.088)
Log firm size	0.068*** (0.009)	0.044*** (0.011)	0.036*** (0.011)
Log no. of employees	0.298*** (0.027)	0.119*** (0.027)	0.113*** (0.029)
Log PSE size	0.030** (0.013)	0.007 (0.022)	0.022 (0.015)
Log hours per month	0.271*** (0.020)	0.200*** (0.023)	0.184*** (0.024)
Manufacturing+	0.288*** (0.109)	0.246*** (0.044)	0.225*** (0.045)
Services	0.170 (0.109)	0.180*** (0.047)	0.190*** (0.047)
Age	0.057*** (0.006)	0.052*** (0.011)	0.060*** (0.012)
Age-squared/100	-0.067*** (0.007)	-0.055*** (0.011)	-0.063*** (0.012)
Completed elementary	0.171*** (0.025)		
Completed junior	0.345*** (0.032)		
Completed senior	0.539*** (0.029)		
Completed college	0.915*** (0.046)		
Year dummies	Yes	Yes	Yes
Occupation dummies	Yes	No	No
Province dummies	Yes	No	No
Demographic variables	Yes	No	No
Observations	11,782	11,782	6,736
<i>Adj.R</i> ²	0.315	0.052	0.041
No. of workers		5,046	

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly earnings. The reference category for employment is informal self-employment. Standard errors are clustered by individual. Demographic variables include gender, marital status and a dummy for whether the person is located in an urban area. All regressions include time dummies.

self-employment. Firm size continues to have an important effect on earnings, particularly for the self-employed. In the fixed effects and first-differenced regressions (Columns 2 and 3 of Table 2.10), however, sectoral earnings premiums, after controlling for firm size and individual-specific heterogeneity, are much smaller. There is no evidence that earnings from informal self-employment are significantly different from earnings from informal salaried employment or formal employment. Public sector workers, on the other hand, earn substantial premiums of 17 - 22 per cent over similar workers in informal self-employment. This large premium partly captures features of public sector wage-setting mechanisms. Firm size is an important predictor of earnings, particularly for the self-employed.

The evidence underscores the importance of individual heterogeneity in the determination of earnings – and suggests that Indonesia is not characterised by a labour market with barriers to movement across sectors. Further, the evidence implies that a dichotomous view of informality and formality that differentiates between self-employment on the one hand, and wage-employment on the other, should be rejected in favour of a view of labour market segmentation that varies along the firm size dimension and by specific wage-setting institutions peculiar to the public sector.

2.4.5 Endogenous switching between sectors

The estimates of earnings differentials across sectors, conditioning on observed firm and employer characteristics, are much larger than the estimates of earnings differentials that also control for individual worker heterogeneity. Clearly, a substantial part of earnings variation is a result of worker heterogeneity, although a large public sector earnings premium exists even in the fixed effects regressions. This is not conclusive proof of labour market segmentation, however, because identification relies on the fact that workers exogenously switch between employment sectors. The earnings sector differentials themselves – the γ s of Equation (2.2) – could be heterogeneous across individuals, in which case individuals endogenously switch into sectors over time so as to maximise their earnings.

This could be on account of time-varying characteristics: for example, workers may become more productive over time as they acquire on-the-job training, leading

them to switch into more productive sectors. In this case, the variation in the unobservable ability of individual workers is correlated with both sector choice and earnings, biasing the estimated γ s. Selection into different employment sectors could also be driven by unobserved individual abilities that are specific to sectors. For example, those with strong entrepreneurial abilities may not necessarily be as productive in public sector employment, and vice versa. Individuals may learn about their sector-specific abilities over time and select into sectors in which they are relatively more productive.

As Heckman et al. (2006, section 8.3) have argued, in an OLS framework, the direction of bias in the estimated γ s is difficult to determine. If, for example, we believe that there is a single measure of unobserved skill determining sorting across employment sectors, then it is plausible that those who have more skill are able to sort into high-paying sectors (for example, public sector employment). These individuals would earn more in any sector in which they were employed because of their high ability, suggesting that ignoring endogenous switching would bias the estimates of the earnings premium upward. However, with multidimensional or sector-specific skills, it is not obvious that an individual employed in a high-paying sector, such as the public sector, would necessarily earn more than the average self-employed individual if she were exogenously transferred to the self-employment sector. This sorting across employment sectors by different types of abilities could yield a downward bias in the size of the estimated γ s.

In a panel data setting, I argue that the resultant bias is likely to be positive: if individuals, over time, are able to switch into employment sectors in which they are more productive, the γ s estimated by fixed effects or first-differenced methods on voluntary employment changes will be biased upward. To consistently identify the average earnings premium associated with any sector in the presence of endogenous switching between sectors, we require an instrument that explains switching between sectors, but not earnings within a given sector. I distinguish between labour market transitions in two periods – between the first two waves (1997 and 2000) and between the last two waves (2000 and 2007). The 1998 East Asian crisis led to a sharp fall in real wages and considerable churning within the labour market between 1998 and 1999, even as aggregate employment remained largely stable (see Thomas et al., 2000). Given the impact of the crisis, it is plausible that a much larger share of the

job separations in the 1997-2000 period would have been involuntary, compared to the 2000-2007 period, particularly in movements out of the formal sector.

Table 2.11 presents results of probits for switching between sectors in the entire sample and in the crisis period (1997-2000) and post-crisis period (2000-2007) alone. Switching in the entire sample is not predicted by the original sector of employment but this masks very different results in the two periods. During the crisis, formal sector workers were much more likely to switch sectors of employment while in the post-crisis period, they were less likely to switch sectors. This is a consequence of the large number of involuntary job transitions in the 1997-2000 period due to formal sector exits. The formal sector bore the brunt of the East Asian crisis and the recession that followed. A large number of formal sector workers lost their jobs and were pushed into agriculture, where employment increased by 1.3 percentage points annually between 1997 and 1999; moreover, this decline in formality was sustained till 2003 (Newhouse et al., 2010, p.34,37). Even within the sample of involuntary formal sector exits, workers may differ by unobservable characteristics. Many formal sector units either downsized or shut down altogether. The workers who lost jobs in the formal sector if their firms were shut down and were forced to enter another employment sector could plausibly be said to have exogenously transitioned from one sector to another.⁹ If, on the other hand, firms were downsized, we would expect that less-productive workers were more likely to lose their jobs, biasing the estimated earning differential downward. Unfortunately, there is no information on the reasons for formal sector job loss between 1997 and 2000. Correspondingly, it is plausible that those who were able to get formal sector jobs in this period may have been particularly productive.

Separate first-differenced regressions for both periods are presented in Columns 2 and 3 of Table 2.12. Column 1 repeats the first-differenced regression for the entire period from Table 2.6. Only the results for the official definition of formality are presented. The coefficients on the formal sector as well as the two variables for firm size are smaller in the 1997-2000 period than in the 2000-2007 period, consistent with the hypothesis that endogenous switching is more important in the later period, biasing those estimates upward. This evidence would suggest that the

⁹Gibbons and Katz (1992) use job losses due to plants shutting down as a source of exogenous job transitions from one firm to another.

Table 2.11: Probits explaining switching between informal and formal sectors

	Entire period	1997-2000	2000-2007
<i>Original job characteristics</i>			
Official formal sector	0.023* (0.050)	0.070*** (0.065)	-0.064*** (0.082)
Log real monthly earnings	-0.009 (0.021)	-0.020** (0.029)	0.005 (0.031)
Log firm size	-0.031*** (0.016)	-0.029*** (0.021)	-0.033*** (0.028)
Log no. of employees	0.051*** (0.032)	0.066*** (0.042)	0.016 (0.053)
Log hours per month	-0.019* (0.035)	-0.018 (0.046)	-0.014 (0.056)
Manufacturing+	0.011 (0.058)	0.005 (0.077)	0.017 (0.090)
Services	0.049*** (0.050)	0.047*** (0.068)	0.049** (0.077)
<i>Individual characteristics</i>			
Age	0.004 (0.013)	0.008* (0.016)	-0.007 (0.027)
Age-squared/100	-0.004 (0.015)	-0.009* (0.019)	0.009 (0.029)
Completed elementary	-0.004 (0.046)	0.012 (0.060)	-0.032 (0.074)
Completed junior	-0.030* (0.061)	-0.033 (0.080)	-0.027 (0.095)
Completed senior	-0.053*** (0.058)	-0.049** (0.077)	-0.052** (0.090)
Completed college	-0.111*** (0.100)	-0.111*** (0.139)	-0.107** (0.147)
Urban	-0.025** (0.041)	-0.024* (0.054)	-0.024 (0.064)
Married	0.030 (0.070)	0.036 (0.085)	0.003 (0.126)
Muslim	0.022 (0.078)	0.035 (0.099)	-0.000 (0.127)
Observations	6,735	4,051	2,682

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is an indicator equal to 1 if the individual has switched sectors between successive waves. Marginal effects are reported. The reference category for employment is informal employment. Standard errors are clustered by individual.

fixed effects estimates for the sample as a whole are, in fact, an upper bound on the inter-sectoral earnings premia, further weakening the case for labour market segmentation and protected high wages in the formal sector.

Table 2.12: First-differenced earnings regressions across different periods

	Entire period	1997-2000	2000-2007
Official formal sector	0.032 (0.037)	0.040 (0.042)	0.026 (0.055)
Log no. of employees	0.119*** (0.025)	0.091*** (0.029)	0.161*** (0.038)
Log firm size	0.038*** (0.009)	0.031*** (0.011)	0.048*** (0.013)
Log PSE size	0.047*** (0.011)	0.023* (0.013)	0.078*** (0.018)
Log hours per month	0.185*** (0.024)	0.166*** (0.029)	0.208*** (0.035)
Manufacturing+	0.228*** (0.045)	0.161*** (0.053)	0.312*** (0.068)
Services	0.195*** (0.047)	0.109** (0.055)	0.299*** (0.070)
Age	0.061*** (0.012)	0.040* (0.021)	0.079*** (0.019)
Age-squared/100	-0.064*** (0.012)	-0.045* (0.025)	-0.071*** (0.016)
Observations	6,736	4,052	2,684
R^2	0.042	0.022	0.054

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly net earnings. The reference category for employment is informal employment. Standard errors are clustered by individual level. All regressions include time dummies.

I test for sensitivity of results to the direction of the transition (from the formal to the informal sector or from the informal to the formal sector) in Table 2.13. For the sample as a whole, the formal sector earnings premium does not depend on the direction of transition. However, the two periods under consideration are significantly different in this regard: entrants to the formal sector in the crisis period have much larger income increases compared to exits; suggesting that individuals who are able to find jobs in the formal sector in this period are more able than the ones who lose their jobs. Entrants to the formal sector in the post-crisis period actually

experience smaller income increases than exiters from the formal sector, arguing against the perception of a formal sector that pays a premium to its employees.

Table 2.13: First-differenced earnings regressions testing for sensitivity to direction of transition

	Entire Period	1997-2000	2000-2007
Official formal sector	0.033 (0.046)	-0.016 (0.053)	0.125 (0.082)
Official formal sector*Entry	-0.001 (0.061)	0.153* (0.085)	-0.197* (0.106)
Log firm size	0.038*** (0.009)	0.030*** (0.011)	0.046*** (0.013)
Log no. of employees	0.119*** (0.025)	0.092*** (0.029)	0.161*** (0.038)
Log PSE size	0.047*** (0.011)	0.021 (0.013)	0.073*** (0.018)
Log hours per month	0.185*** (0.024)	0.166*** (0.029)	0.208*** (0.035)
Manufacturing+	0.228*** (0.045)	0.163*** (0.053)	0.311*** (0.068)
Services	0.195*** (0.046)	0.111** (0.055)	0.297*** (0.070)
Age	0.061*** (0.012)	0.041* (0.021)	0.077*** (0.019)
Age-squared/100	-0.064*** (0.012)	-0.047* (0.025)	-0.070*** (0.016)
Observations	6,736	4,052	2,684
<i>Adj.R</i> ²	0.040	0.021	0.052

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly net earnings. Official formal sector*Entry is a dummy variable indicating if a switcher entered the formal sector or not. The reference category for employment is informal employment. Standard errors are clustered by individual.

Given the evidence of endogeneity in the switching of workers between different sectors, I follow Falco et al. (2011) in instrumenting the endogenous variables with their lagged levels and differences in the system GMM estimator developed by Blundell and Bond (1998). These estimates are presented in Table 2.14 and are consistent under the assumption that the lagged differences of the endogenous variables are conditionally uncorrelated with the current unobservables. I instrument

for all time-varying job characteristics. In both specifications, the coefficients on the formal sector variable are, in fact, negative, though not significant due to the large standard errors. The coefficients on firm size remain large, positive, and, in the case of the public sector, significant. As Falco et al. (2011) point out, a system GMM estimation on a three-wave panel is likely to be imprecise; with more data, concerns about endogeneity of switching across sectors could be conclusively ruled out.

2.4.6 Robustness checks

One possible reason for the smaller size of the estimated coefficients in the fixed effects and first-differenced estimators compared to the pooled OLS estimator is the possibility of measurement error in the explanatory variable, which can cause a large attenuation bias in panel estimators. I provide evidence that this is not the case in this study by comparing the results of the fixed effects, first-differenced and second-differenced regressions, and by exploiting survey questions for the enumerators on the perceived quality of a respondent's answers. The results of both simple tests of measurement error are included in the appendix.

Another source of concern is the importance of attrition over a ten-year long survey. If panel respondents randomly drop out of the sample, then the precision of the estimated coefficients is reduced but the coefficients will not be biased. However, if respondents who attrit from the panel survey are systematically different from those who do not, the estimated coefficients may be biased. Accordingly, the results ought to be robust to endogenous attrition. I provide tests of the importance of attrition, as well as first-differenced estimates of employment sector earnings premiums that are corrected for attrition. I use both the method of Inverse Probability Weights, as well as a more straightforward model of selection into non-attrition to explain the process of attrition. The attrition-corrected results are not significantly different from the primary results and there is no evidence that attrition is creating a serious bias in the estimated coefficients. The results are included in the appendix.

2.5 Conclusion

In this paper, I have examined inter-sectoral earnings differentials between employment in formal and informal sectors in Indonesia while controlling for observed

Table 2.14: System GMM estimates of the effect of formality

	(1)	(2)
Official formal sector	-0.198 (0.447)	
Formal sector		-0.259 (0.648)
Informal salaried		0.207 (0.482)
Public sector		-0.736 (0.612)
Log firm size	0.128 (0.104)	0.091 (0.124)
Log no. of employees	0.266 (0.325)	0.194 (0.337)
Log PSE size	0.274*** (0.075)	0.414*** (0.135)
Age	0.039** (0.016)	0.046*** (0.017)
Age-squared/100	-0.045** (0.019)	-0.053** (0.021)
Log hours per month	0.460* (0.273)	0.480* (0.276)
Manufacturing+	0.457 (0.472)	0.278 (0.503)
Services	0.376 (0.324)	0.500 (0.347)
Year dummies	Yes	Yes
Observations	11,782	11,782
No. of workers	5,046	5,046
No. of instruments	36	37
Hansen J statistic	36.035	37.051

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly net earnings. All time-varying variables are treated as endogenous. Estimates are obtained using a two-step procedure with standard errors corrected by the Windmeijer method.

human capital endowments, unobserved individual heterogeneity and attrition. I find that most of the average earnings gap between employment in different sectors is driven by individual heterogeneity and firm size. First, individuals appear to be selecting into employment sectors in which they will be most productive on the basis of preferences and abilities. However, the analysis excludes non-monetary benefits of employment, which could bias my estimates of the true return to employment in a particular sector (for example, the benefits of job security might be higher in a formal sector job but cannot be accurately measured). Second, workers in large firms earn more than workers in small firms; this is particularly true of self-employed individuals with hired workers compared to firm employees. Endogenous switching into employment sectors over time because of time-varying characteristics, such as changing private information about earnings prospects, is a potentially important source of bias. I argue that any bias will be upward and, as such, the estimated results are an upper bound on the true effects. By splitting the sample into two different periods, I show that this is the case. I also exploit the panel dimension of the data to instrument for the endogenous variable, and find no evidence that the results are biased.

The sharp dichotomy between formal and informal employment or between salaried and self-employment must be abandoned in favour of a broader understanding of the labour earnings spectrum along the dimension of firm size. This has important implications for our understanding of the informal sector: if individuals enter this sector by choice, then it is likely to remain a permanent fixture in developing countries like Indonesia, given present labour market institutions. The process of structural transformation will not necessarily be associated with a movement of workers from self-employment to wage-employment or from the informal to the formal sector. The present focus on transitions between informal and formal employment may need to be shifted to understanding why larger firms are associated with such higher levels of labour productivity, relative to smaller firms. There is little evidence that labour market institutions such as minimum wage laws and mandated worker benefits are creating artificial barriers between employment sectors, nor of the payment of efficiency wages to workers in the formal sector.

The one sector that is associated with a large earnings premium is the public sector, where workers earn 20 per cent more than in the informal private sector. This

is likely to be partly related to the relatively large size of public sector enterprises, and, more importantly, to the wage-setting mechanisms peculiar to the public sector. In the absence of detailed information on non-pecuniary public sector job benefits, I leave the examination of how these mechanisms operate and affect the wider labour market to future research.

2.A Appendix

2.A.1 Data and construction of sample

Incomes were reported in monthly terms. Real monthly income in terms of 2007 rupiahs was calculated by dividing the reported nominal incomes by annual price indices published by the BPS. Hours worked per week were multiplied by 4.3 to obtain hours worked per month.

The process of data selection is described below: The initial sample comprised 79,216 observations on 38,733 unique individuals who were asked about their educational and employment status across three waves (1997, 2000 and 2007). Each individual appears in the sample between one and three times.

First, females were excluded from the analysis, leaving 37,808 observations on 18,886 individuals.

Second, those below the age of 16 and above the age of 65 were dropped from the sample, leaving 34,030 observations on 17,533 individuals.

Third, those who reported themselves to be unemployed were dropped from the sample, leaving 28,565 observations on 15,279 individuals. This includes 22,844 observations on 13,410 individuals who report earning a positive income; the others are listed as unpaid family members, who are necessarily excluded from the earnings regressions but who are incorporated in the selection model explaining selection into employment positions with explicit incomes.

Fourth, to ensure that attrition is an absorbing state, observations on all individuals who were not present in the first wave (1997) or who were present in 1997 and 2007, but not in 2000, are dropped from the sample. This ensures that if an individual is interviewed at time $t + 1$, he or she must have been interviewed at time t . This leaves 11,782 observations on earners in the sample on 5,046 individuals.

2.A.2 Sample selection bias

As detailed above, over 1,800 workers are listed as unpaid family workers (over 12 per cent of the sample of workers). Endogenous selection into employment for an explicit money income could potentially bias the estimates above. To correct for selection into an employment position reporting a positive income, I use the panel

estimator developed in Wooldridge (1995). The primary earnings regression is given in Equation (2.2). Following Wooldridge, I assume the selection into reported income positions of employment follow a selection process given by

$$s_{it} = 1[Z_{it} + \eta_i + u_{it} > 0] \quad (2.5)$$

Wooldridge utilises Chamberlain's (1984) parametrisation of the individual effects in the primary equation (the α_i s) and the selection equation (the η_i s) to be linear functions of the leads and lags of all observable variables. Under the assumption that the selection model is correctly specified (the errors, u_{it} are independent of the variables in $\mathbf{Z}$), Wooldridge derives an expression for the selection-corrected panel estimator as

$$y_{it} = \mathbf{X}'_{it}\boldsymbol{\beta} + \mathbf{X}'_i\boldsymbol{\gamma} + \rho_1 d_1 \hat{\lambda}_{i1} + \rho_2 d_2 \hat{\lambda}_{i2} + \rho_3 d_3 \hat{\lambda}_{i3} + \varepsilon_{it} \quad (2.6)$$

where d_t s are time dummies for each of the t waves of the survey and λ_{it} s are the estimated Inverse Mill's Ratios calculated from the probit estimates of Equation (2.5). I first estimate three probit equations for selection into an income-earning position in each time period (these are not reported in this paper). In addition to the covariates in the primary equation, I use additional exclusion restrictions in the form of family-related variables, including whether an individual is the head of the house, the education level of the head of the house, whether the head of the house is employed, whether the head of the house is male, number of children under the age of 15 in the house, number of adults who are above the age of 70 in the house, and whether the house is owned by the respondent. I construct Inverse Mill's Ratios from these probits and add them to the earnings equation, along with additional covariates in the form of the leads and lags of all time-varying covariates. A comparison of the corrected estimates with the original fixed effects estimates is presented in Table 2.A.1. The coefficients are found to be very similar, giving reassurance that, provided the underlying selection process is correctly specified, there is no evidence for any selection bias.

2.A.3 Measurement error

One possible reason for the smaller size of the estimated coefficients in the fixed effects and first-differenced estimators compared to the pooled OLS estimator is

Table 2.A.1: Fixed effects estimator on full sample and Wooldridge estimator corrected for selection into employment

	Fixed Effects	Wooldridge Estimator
Official formal sector	0.003 (0.026)	−0.056 (0.038)
Log firm size	0.056*** (0.008)	0.041*** (0.011)
Log no. of employees	0.139*** (0.018)	0.117*** (0.025)
Log PSE size	0.060*** (0.013)	0.034* (0.019)
Log hours per month	0.235*** (0.016)	0.216*** (0.023)
Manufacturing+	0.245*** (0.032)	0.224*** (0.056)
Services	0.164*** (0.030)	0.153*** (0.053)
Age	0.072*** (0.009)	0.042*** (0.013)
Age-squared/100	−0.078*** (0.008)	−0.038*** (0.012)
Year dummies	Yes	Yes
Demographic variables	No	Yes
Observations	22,844	10,424
R^2	0.080	0.264

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly net earnings. The reference category for employment is informal employment. Standard errors are clustered at the individual level to correct for serial correlation but are not robust to heteroskedasticity.

the possibility of measurement error in the explanatory variable, which can cause a large attenuation bias in panel estimators. If the categorical variables for occupational sector are incorrectly measured, then the estimated sectoral earnings premiums could be biased towards zero. One way to assess the importance of measurement error in the explanatory variables is to compare the fixed effects estimator with the first-differenced estimator. Under classical assumptions, measurement error in the explanatory variable can lead to increased attenuation bias in first-differenced estimates compared to fixed effects estimates (Griliches and Hausman, 1986). Following Griliches and Hausman, under the assumption of classical errors-in-variables (stationary and uncorrelated measurement errors in the explanatory variables), the estimated bias in the first-differenced estimator, b_d , and the within estimator, b_w , is given by

$$\text{plim } b_d = \beta(1 - \frac{2\sigma_v^2}{\text{var}dx}), \quad \text{plim } b_w = \beta(1 - \frac{T-1}{T} \frac{\sigma_v^2}{\text{var}\tilde{x}}) \quad (2.7)$$

where $dx = x_{it} - x_{i,t-1}$ and $\tilde{x} = x_{it} - \bar{x}_{it}$. When the explanatory variables, xs , are positively correlated with a declining correlogram ($\rho_1 > \rho_2$, where ρ_i is the i th serial correlation coefficient between the xs), then $\text{var}(dx) < \frac{2T}{T-1} \text{var}(\tilde{x})$, or the bias in the first-differenced estimator will be more than the bias in the within estimator. Both will be biased downward with respect to the OLS estimator. The results of the first-differenced estimator are presented in Column 3 of Table 2.10. The estimated coefficients are not significantly different from the fixed effects estimator; if anything, they are sometimes a little higher. While this does not constitute evidence of the absence of measurement error, it is reassuring to see similar estimates across both estimators.

Interviewers were also asked to assess the quality of responses of the respondent by ranking the accuracy of the answers on a scale of 1 to 5 (where 1 was considered “excellent” and 5 was considered “very bad”). Table 2.A.2 shows the breakdown of interviewer assessments by employment sector. The share of responses that were considered “not so good” or “very bad” is low at about 0.5 per cent of the total sample. The two worst categories of answers were excluded from the baseline regressions as an additional robustness check; these results are reported in Table 2.A.3 and they are not substantively different from results for the entire sample.

Table 2.A.2: Interviewer assessment of quality of responses

Accuracy of respondent answers	Informal self-employed	Informal wage-employed	Formal sector	Public sector	Total
Excellent	316 (8.74)	255 (9.03)	378 (9.44)	233 (17.41)	1,182 (10.03)
Good	2,469 (68.32)	1,920 (67.96)	2,837 (70.84)	946 (70.70)	8,172 (69.36)
Fair	809 (22.39)	636 (22.51)	770 (19.23)	157 (11.73)	2,372 (20.13)
Not so good	18 (0.50)	13 (0.46)	18 (0.45)	1 (0.07)	50 (0.42)
Very bad	2 (0.06)	1 (0.04)	2 (0.05)	1 (0.07)	6 (0.05)
Total	3,614 (100.00)	2,825 (100.00)	4,005 (100.00)	1,338 (100.00)	11,782 (100.00)

Notes: Own calculations based on IFLS. Column percentages are given in parentheses.

Table 2.A.3: OLS, fixed effects and first-differenced earnings regressions - excluding responses assessed to be of poor quality

	OLS	Fixed Effects	First Differenced
Formal sector	0.192*** (0.031)	0.028 (0.038)	0.045 (0.041)
Informal salaried	0.180*** (0.030)	-0.007 (0.038)	0.013 (0.041)
Public sector	0.621*** (0.064)	0.215** (0.103)	0.164* (0.088)
Log firm size	0.068*** (0.009)	0.044*** (0.011)	0.037*** (0.011)
Log no. of employees	0.297*** (0.027)	0.118*** (0.027)	0.113*** (0.029)
Log PSE size	0.030** (0.013)	0.007 (0.022)	0.023 (0.015)
Log hours per month	0.271*** (0.020)	0.201*** (0.023)	0.184*** (0.024)
Manufacturing+	0.287*** (0.109)	0.246*** (0.044)	0.220*** (0.045)
Services	0.170 (0.109)	0.180*** (0.047)	0.185*** (0.047)
Age	0.058*** (0.006)	0.054*** (0.011)	0.062*** (0.012)
Age-squared/100	-0.068*** (0.007)	-0.055*** (0.011)	-0.063*** (0.012)
Completed elementary	0.169*** (0.025)		
Completed junior	0.341*** (0.032)		
Completed senior	0.535*** (0.029)		
Completed college	0.908*** (0.046)		
Year dummies	Yes	Yes	Yes
Occupation dummies	Yes	No	No
Province dummies	Yes	No	No
Demographic variables	Yes	No	No
Observations	11,726	11,726	6,702
<i>Adj.R</i> ²	0.315	0.052	0.040
No. of workers		5,043	

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly net earnings. The reference category for employment is informal self-employment. Standard errors are clustered by individual. Demographic variables include gender, marital status and a dummy for whether the person is located in an urban area. All regressions include time dummies.

2.A.4 Correcting for attrition

One concern with regard to panel data is the presence of non-random attrition. If panel respondents randomly drop out of the sample, then the precision of the estimated coefficients is reduced but the coefficients will not be biased. However, if respondents who attrit from the panel survey are systematically different from those who do not, the estimated coefficients may be biased. Accordingly, the results ought to be robust to endogenous attrition.

Attrition in the IFLS has been low compared to many other panel surveys, not only in developing countries, but in developed countries as well, on account of the survey's efforts in tracking individuals who left their original homes and migrated to a different province (Thomas et al., 2001, 2012). Of the 7,200 households contacted in 1993, re-contact rates in subsequent waves of the survey have been very high: 94.4 per cent (1997), 95.3 per cent (2000) and 93.6 per cent (2007). 87.6 per cent of the IFLS-1 households have been interviewed in all four waves – leading to an attrition rate of just over 12 per cent over 14 years at a rate of less than 1 per cent per year. Nonetheless, attrition is unlikely to have been non-random. Thomas et al. (2012) point out that observable household characteristics can be significantly different between those who attrit and those who do not. Several job, individual and household characteristics of attriters and non-attriters are compared in Table 2.A.4. In job characteristics, attriters tend to have lower incomes and work for smaller firms. They are also more likely to be employed in the informal sector, particularly in agriculture. In terms of individual and household characteristics, attriters tend to be older, less educated and female. They also tend to be drawn from large households with several old people but fewer young children.

To correct for attrition, we require a model explaining selection in subsequent rounds of the survey. Following Wooldridge (2002, p.585), I assume that in the first period, $t = 1$, the chosen sample is randomly selected, and in subsequent periods, respondents may drop out of the sample. A selection indicator, s_{it} , denotes whether a respondent is “found” in the subsequent wave, where $s_{it} = 1$ if $(y_{it}, \mathbf{X}_{it})$ is observed and 0 otherwise. Attrition is assumed to be an *absorbing* state: if a respondent drops out of the panel, they are assumed to be lost forever. In other words, $s_{it} = 1$ implies

Table 2.A.4: Comparing attriters and non-attriters

	Surveyed in 1997		Surveyed in 2000	
	Resurveyed in 2000	Attrited in 2000	Resurveyed in 2007	Attrited in 2007
<i>Job characteristics</i>				
Log income	13.31	13.11	13.30	13.27
Hours worked per month	199.02	187.51	202.60	200.15
Log firm size	2.24	2.00	1.93	1.83
Informal self-employed	0.26	0.38	0.31	0.37
Informal salaried	0.23	0.21	0.28	0.24
Formal sector	0.38	0.34	0.30	0.28
Public sector	0.12	0.07	0.11	0.11
Agriculture	0.22	0.34	0.24	0.28
Manufacturing+	0.31	0.26	0.26	0.20
Services	0.48	0.40	0.50	0.51
<i>Demographic characteristics</i>				
Years of Schooling	7.93	7.48	8.14	7.80
Age	36.73	41.94	37.15	44.30
Male	1.00	1.00	1.00	1.00
Married	0.82	0.75	0.88	0.85
Urban	0.54	0.47	0.54	0.55
<i>Household characteristics</i>				
Home-owner	0.79	0.79	0.78	0.82
Household size	5.88	6.18	5.91	6.22
Number of children	1.74	1.53	1.72	1.47
Number of old people	0.18	0.21	0.23	0.26
Is head of household	0.61	0.58	0.68	0.76
Household head entered senior secondary school	0.25	0.21	0.28	0.26
Household head is employed	0.93	0.92	0.95	0.96
Observations	4,052	994	2,684	1,368

Notes: Calculations based on the IFLS. Standard deviations are given in parentheses.

$s_{i,t-1} = 1$. A reduced form selection equation for non-attrition can be written as

$$s_{it} = 1[\mathbf{Z}'_{it}\boldsymbol{\delta}_t + v_{it} > 0], \quad v_{it}|\{\mathbf{X}_{it}, \mathbf{Z}_{it}, s_{i,t-1}\} \sim Normal(0, 1) \quad (2.8)$$

where $\mathbf{Z}_{it}$ contains individual, household and community characteristics that can explain selection in time period t .

Selection on observables and unobservables

Following the terminology used by Fitzgerald et al. (1998), attrition may be based on observable characteristics or on unobservable characteristics. For selection on unobservable characteristics, a selection model will have to be estimated for every period to explain non-random selection in the panel, and consistent estimation will require the use of exclusion restrictions – instruments that influence selection but not the response variable, that is, earnings. Such instruments, $\mathbf{Z}_{it}$, are exogenous to $y_{it}|\mathbf{X}_{it}$. Fitzgerald et al. argue that the most suitable instruments in this case are characteristics of the survey interviewers and the interviewing process itself, which influences attrition but has no plausible correlation with the economic outcome of interest. In the absence of such instruments, however, I use household variables as instruments. In the instance of the first-differenced estimation, following Wooldridge (2002, p.585-586), I correct for attrition under the additional assumptions that selection does not depend on $\Delta\mathbf{X}_{it}$ once $\mathbf{Z}_{it}$ has been controlled for, and that $\Delta\varepsilon_{it}$ and v_{it} are jointly normal. Then it can be shown that

$$E(\Delta y_{it}|\Delta\mathbf{X}_{it}, \mathbf{Z}_{it}, s_{it} = 1) = \Delta\mathbf{X}'_{it}\boldsymbol{\beta} + \rho_t\lambda(Z_{it}\delta) \quad (2.9)$$

where $\lambda(Z_{it}\delta)$ is the inverse Mill's ratio from the probit estimation of Equation (2.11). Accordingly, the following estimation will be consistent,

$$\Delta y_{it} = \Delta\mathbf{X}'_{it}\boldsymbol{\beta} + \rho_2 d_2 \hat{\lambda}_{it} + \rho_3 d_3 \hat{\lambda}_{it} + error_{it} \quad (2.10)$$

where d_2 and d_3 are time dummies for the second and third waves of the survey, corresponding to years 2000 and 2007 respectively.

Alternatively, selection may be based on observable characteristics. This assumption – also called the ignorability of selection – assumes that selection can be predicted on observable characteristics in the first time period,

$$P(s_{it} = 1|y_{it}, \mathbf{X}_{it}, \mathbf{Z}_{i1}) = P(s_{it} = 1|\mathbf{Z}_{i1}), \quad t = 2, 3, \dots, T \quad (2.11)$$

Unlike in the previous case, variables in $\mathbf{Z}_{i1}$ that are not included in $\mathbf{X}_{it}$ are not assumed to be exogenous to $y_{it}|\mathbf{X}_{it}$ – they are simply excluded from the model of interest. Under this assumption, Fitzgerald et al. propose the method of inverse probability weighting (IPW) to correct for attrition. This requires the estimation of a probit of s_{it} on $\mathbf{Z}_{i1}$ for all observations in period $t = 1$ in order to estimate the *a priori* probability of selection, $\hat{p}_{it}$, of a respondent, i , in time period, t , after the baseline. Weights are constructed such that every observation in OLS is weighted by $1/\hat{p}_{it}$, which provides that observations with a higher probability of attrition are given greater weight in the estimation. By construction, $\hat{p}_{i1} = 1$.

The results of three attrition probits are in Table 2.A.5: the first probit uses 1997 characteristics of individuals to predict whether they were found in 2000, the second probit uses 2000 characteristics of individuals to predict whether they were found in 2007 and the third probit uses 1997 characteristics of individuals to predict whether they were found in 2007. Variables used to predict whether a person would be found in a wave include all the individual-level characteristics used to determine earnings in Table 2.10, as well as a number of household-level characteristics, including whether an individual is the head of the house, the education level of the head of the house, whether the head of the house is employed, whether the head of the house is male, number of children under the age of 15 in the house, number of adults who are above the age of 70 in the house, and whether the house is owned by the respondent.

In themselves, the probits are quite informative about the nature of attrition in the survey. Important predictors of being found by the surveyors in the subsequent wave include age and whether or not the respondent has a college degree. Older, married urban-dwellers are more likely to be found. Holding a college degree, however, reduces the probability of finding a respondent, presumably because of the potential for migration in search of a better job. Public sector workers are much more likely to be found in subsequent waves relative to other types of workers.

Attrition, therefore, would appear to be non-random. I correct for attrition under both sets of assumptions – selection on observables and the stronger assumption of selection on unobservables. In Table 2.A.6, inverse probability weights are created as described above and are used to correct the estimation of the pooled OLS earnings regression and the first-differenced earnings regression (Columns 1 and 2). Also presented is the first-differenced earnings regression for only those individuals who

Table 2.A.5: Non-attrition probit regressions predicting selection in next wave

	1997-2000	2000-2007	1997-2007
<i>Job characteristics</i>			
Formal sector	0.031 (0.080)	-0.006 (0.079)	0.031 (0.071)
Informal salaried	0.012 (0.074)	0.003 (0.075)	-0.005 (0.066)
Public sector	0.076 (0.187)	0.051 (0.169)	0.099* (0.150)
Log no. of employees	-0.016 (0.052)	0.006 (0.052)	-0.022 (0.047)
Log firm size	-0.004 (0.023)	0.004 (0.025)	-0.011 (0.020)
Log PSE size	-0.007 (0.045)	-0.020 (0.045)	-0.020 (0.035)
<i>Individual characteristics</i>			
Age	0.026*** (0.013)	0.046*** (0.018)	0.048*** (0.014)
Age-squared/100	-0.042*** (0.016)	-0.076*** (0.021)	-0.087*** (0.018)
Completed elementary	0.041*** (0.058)	0.015 (0.061)	0.044** (0.052)
Completed junior	-0.006 (0.084)	0.047 (0.095)	0.049 (0.075)
Completed senior	0.061*** (0.090)	0.075* (0.108)	0.107*** (0.078)
Completed college	0.038 (0.154)	0.145** (0.166)	0.082 (0.134)
Urban	0.042*** (0.053)	0.002 (0.053)	0.042** (0.046)
Married	0.052*** (0.073)	0.119*** (0.081)	0.097*** (0.067)
Muslim	0.056*** (0.084)	0.076** (0.087)	0.112*** (0.077)
Manufacturing+	-0.135 (0.499)	-0.378** (0.529)	0.043 (0.358)
Services	-0.145 (0.498)	-0.395** (0.526)	0.042 (0.357)
Household characteristics	Yes	Yes	Yes
Occupation dummies	Yes	Yes	Yes
Province dummies	Yes	Yes	Yes
Observations	5,046	4,049	5,046

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. Marginal effects are reported. The dependant variable is a dummy taking the value of 1 if the individual was observed. The regression includes controls for household characteristics, occupation, and province. Household characteristics include whether an individual is the head of the house, the education level of the head of the house, whether the head of the house is employed, whether the head of the house is male, number of children under the age of 15 in the house, number of adults who are above the age of 70 in the house, and whether the house is owned by the respondent.

are present in all three waves of the survey (Column 3). In Table 2.A.7, the fixed effects and first-differenced earnings regressions are corrected for attrition under the assumption of selection on unobservables. While the first-differenced estimator, corrected for attrition as above in Equation (2.9), will produce consistent estimates of β , the fixed effects estimator will not (see Wooldridge, 2002). However, the inclusion of the $\hat{\lambda}_{it}$ s in the fixed effects regression does provide a test for the importance of attrition, via a joint F test on the coefficients on $d_2\hat{\lambda}_{it}$ and $d_3\hat{\lambda}_{it}$. The results of this test are presented in Table 2.A.7.

The introduction of inverse probability weights changes the size of some of the OLS regression estimates but not those of the first-differenced regression, suggesting that unobserved individual heterogeneity also partly explains the probability of attrition. Under the assumption of selection on unobservables, the correction for non-random attrition does not significantly change the point estimates on employment types, nor are the variables representing the attrition correction significant in either the fixed effects or the first-differenced pooled earnings regression. The joint F test on the coefficients on the $\hat{\lambda}_{it}$ s cannot reject that they are equal to zero. This is reassuring in that it suggests attrition may not be as much of a concern as feared; however, the validity of this correction depends on the validity of the underlying model of selection, particularly the exclusion restrictions used to explain attrition.

Table 2.A.6: Inverse probability weighted OLS and first-differenced earnings regressions

	OLS IPW	First Differenced IPW	First Differenced non-attributers
Formal sector	0.192*** (0.031)	0.047 (0.041)	0.039 (0.047)
Informal salaried	0.178*** (0.030)	0.012 (0.041)	0.015 (0.047)
Public sector	0.625*** (0.064)	0.168* (0.087)	0.161 (0.105)
Log firm size	0.068*** (0.009)	0.036*** (0.011)	0.037*** (0.013)
Log no. of employees	0.297*** (0.027)	0.112*** (0.029)	0.128*** (0.032)
Log PSE size	0.029** (0.013)	0.023 (0.015)	0.021 (0.019)
Log hours per month	0.272*** (0.020)	0.184*** (0.024)	0.171*** (0.027)
Manufacturing+	0.293*** (0.110)	0.227*** (0.045)	0.291*** (0.053)
Services	0.175 (0.109)	0.194*** (0.046)	0.267*** (0.053)
Age	0.057*** (0.006)	0.062*** (0.012)	0.061*** (0.013)
Age-squared/100	-0.067*** (0.007)	-0.065*** (0.012)	-0.059*** (0.012)
Completed elementary	0.171*** (0.025)		
Completed junior	0.345*** (0.032)		
Completed senior	0.539*** (0.029)		
Completed college	0.918*** (0.046)		
Year dummies	Yes	Yes	Yes
Occupation dummies	Yes	No	No
Province dummies	Yes	No	No
Demographic variables	Yes	No	No
Observations	11,782	6,736	5,368
No. of workers	0.318	0.043	0.046

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly net earnings. The reference category for employment is informal self-employment. Standard errors are clustered by individual. Demographic variables include gender, marital status and a dummy for whether the person is located in an urban area. All regressions include time dummies.

Table 2.A.7: Attrition-corrected fixed effects and first- and second-differenced earnings regressions

	Fixed Effects	First Differenced	Second Differenced
Formal sector	0.027 (0.037)	0.043 (0.041)	-0.020 (0.055)
Informal salaried	-0.007 (0.038)	0.010 (0.041)	-0.050 (0.058)
Public sector	0.222** (0.103)	0.167* (0.088)	0.374* (0.201)
Log firm size	0.044*** (0.011)	0.036*** (0.011)	0.063*** (0.016)
Log no. of employees	0.119*** (0.027)	0.113*** (0.029)	0.151*** (0.038)
Log PSE size	0.006 (0.022)	0.022 (0.015)	-0.039 (0.052)
Log hours per month	0.199*** (0.023)	0.184*** (0.024)	0.228*** (0.034)
Manufacturing+	0.245*** (0.044)	0.225*** (0.045)	0.361*** (0.072)
Services	0.179*** (0.046)	0.190*** (0.047)	0.231*** (0.074)
Age	0.045*** (0.013)	0.054*** (0.014)	0.027 (0.017)
Age-squared/100	-0.047*** (0.013)	-0.057*** (0.014)	-0.027* (0.015)
λ 2000	0.204 (0.235)	0.227 (0.235)	
λ 2007	-0.375 (0.303)	-0.406 (0.329)	
λ 1997-2007			-0.379* (0.222)
Year dummies	Yes	Yes	No
Observations	11,782	6,736	2,684
No. of workers	5,046		
R^2	0.054	0.043	0.064
F test	1.429	1.294	
p-value (F)	0.240	0.274	

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of real monthly net earnings. The reference category for employment is informal self-employment. Standard errors are clustered by individual. All regressions include time dummies. The F statistic is from a joint test of the significance of the λ s.

Chapter 3

The effect of a currency depreciation on importers: a firm-level analysis

3.1 Introduction

The unexpectedly large decline in trade flows during the 2008 global recession has led to a renewed focus on the volatility of international trading activity (Ahn et al., 2011). International transactions are more uncertain and time-consuming than domestic transactions, and firms that trade are particularly vulnerable to fluctuations in the exchange rate. While the effect of exchange rate movements on exporters is well-documented, much less attention has been directed at the effect on firms that import intermediate inputs. This is despite the fact that 40 per cent of non-fuel world trade is accounted for by the import of intermediate manufactured goods (World Trade Organization, 2009). Even when considering only exports, 24 per cent of the value of gross global exports is derived from imported intermediate inputs (World Trade Organization, 2014).

The increasing importance of trade in intermediate inputs implies that the management of exchange rate volatility is critical, not just for firms that export, but many other domestic manufacturers as well. Importers are especially reliant on access to external sources of finance to meet their working capital requirements and mitigate the effect of sudden shocks to the costs of their inputs. In this context, multinational firms are typically better placed to respond to cost shocks since they

are better able to access international capital markets, as well as raise internal finances from a network of multinational affiliates (Desai et al., 2004). Multinational firms may also be better able to hedge against exchange rate risks due to their exposure to multiple currencies.

In this paper, I identify the effect of a large and unexpected exchange rate depreciation on the performance of firms that import intermediate inputs. I identify the differential impact on the performance of foreign-owned firms compared to domestic firms, and suggest that this is because foreign-owned firms have access to external sources of finance from their affiliates, trading partners and from banks, as well as strategies to manage exchange rate risks. The data I use is drawn from a census of all manufacturing firms in Indonesia with at least twenty employees. I consider the years 1991 to 1999, spanning the period of the East Asian financial crisis of 1997-98, which was characterised by a steep depreciation in the Indonesian rupiah in real terms, driving up costs for all Indonesian importers. By observing the same firms before and after the crisis event, I am able to identify the impact of the depreciation on foreign-owned importers relative to domestically-owned importers, after controlling for firm-specific time-invariant heterogeneity. Since the dataset is unusually rich for a developing country, I am able to control for a number of other potentially confounding firm characteristics as well.

I find that while the performance of domestic importers declined during the crisis, foreign-owned importers fared relatively better. Foreign firms used 22 per cent more materials, hired 16 per cent more workers, and increased productivity by 16 per cent, compared to domestic-owned firms. However, I find no evidence that foreign firms built up relatively more capital compared to domestic firms during this period, suggesting that the main channel through which foreign firms benefited was the use of trade finance intended to help them meet their working capital requirements, as opposed to increasing investment.

This paper builds upon several strands of the existing microeconomic literature on internationally trading firms. Starting from the work of Aw and Hwang (1995) and Bernard and Jensen (1999), a large literature has developed that uses microdata on firms to determine the characteristics of firms that export, and the effect of exporting on firm performance. The importing activity of firms has received far less research attention, even though there exists a considerable overlap between exporters

and importers (Bernard et al., 2009a). Recent literature presents empirical evidence on the import decision of firms, particularly of two-way traders who both export and import.¹ By using data from a census of all manufacturing units in Indonesia, I am able to document observable differences between non-trading firms, exporters, importers and two-way traders, and identify the effect of the import intensity of a firm on its performance in the face of a sudden increase in input costs.

Following the 2008 trade collapse, recent research has suggested that firms which are reliant on trade finance are adversely affected by a credit contraction (Iacovone and Zavacka, 2009; Berman and Martin, 2010; Chor and Manova, 2010; Amiti and Weinstein, 2011; Manova, 2013). Firms without access to trade credit are unable to finance short-term payments for inputs, or make payments for outputs. During the Asian crisis, the high reliance of Indonesian firms on imported inputs (Auboin, 2009) is likely to have had important effects on firm performance. Blalock et al. (2008) point out that international firms in Indonesia had better access to sources of external finance than domestic firms, with important implications for their performance. Using foreign-ownership as a proxy for access to international finance, I compare changes in the performance of importers with non-importers as a result of the economic crisis and find strong evidence of heterogeneity by type of ownership.

This finding is important because it identifies another channel through which FDI can add value to a firm in a developing country, especially with the increasing importance of the global production chain. In Indonesia, almost a quarter of all firms rely on some imported inputs; for Indonesian exporters, this figure is over 40 per cent. Access to finance for such firms can help them sustain profitability, trading volumes, hiring and productivity, in response to an exchange rate shock.

The approach I use is similar to Blalock et al., with some important differences. Blalock et al. restrict their analysis to only large firms employing over 100 workers, while I consider the entire dataset of surveyed manufacturing establishments, which have a median size of only 50 workers. I focus on importers, as opposed to exporters,

¹This research spans firms in countries such as the United States (Bernard et al., 2009b), France (MacGarvie, 2006), Germany (Vogel and Wagner, 2010), Belgium (Muûls and Pisu, 2009; Amiti et al., 2014), Hungary (Halpern et al., 2011), eastern Europe and transition economies (Aristei et al., 2013), Chile (Kugler and Verhoogen, 2009; Gibson and Graciano, 2011) and China (Wang and Yu, 2012).

which illuminates the growing importance of the trade in intermediate goods. Finally, by comparing importers with non-importers, within a sample of exporters, I am able to shed light on the phenomenon of two-way trading and identify the effect of financial constraints for a more appropriate counterfactual group.

The rest of the paper is organised as follows: Section 3.2 provides the background to the crisis and the theoretical motivation for this paper. Section 3.3 describes the data. Section 3.4 presents the empirical framework and estimation results on the impact of the East Asian crisis on the performance of importers. Section 3.5 concludes.

3.2 Background and theoretical motivation

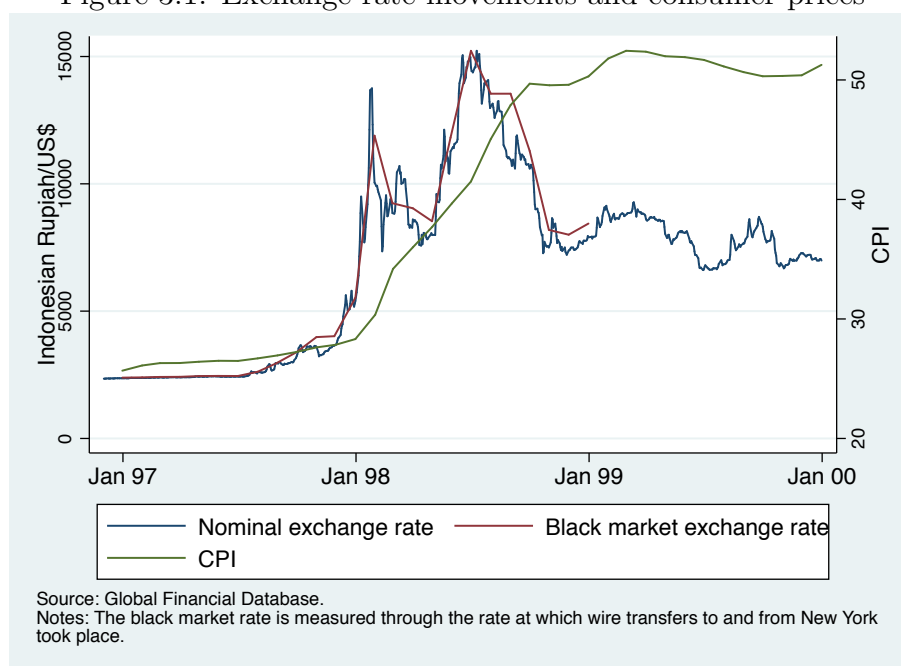
3.2.1 Background to the Indonesian crisis

For almost two decades before the East Asian crisis, the Indonesian authorities implemented a ‘managed float’ exchange rate regime where the rupiah was only permitted to move within a narrow band. International investors launched a speculative attack on the Thai baht in June 1997, following which the currencies of neighbouring countries came under speculative pressure as well. After widening the band failed to stem rupiah selling, the Indonesian government abandoned it altogether on August 14, leading to a sharp depreciation in the currency. In December, the regional crisis intensified as deposit runs ensued on almost half of the assets of the Indonesian banking system. In January 1998, the Indonesian rupiah depreciated from Rp 4,800/\$ to above 16,000/\$, plunging Indonesia into a deep and long-drawn economic crisis. While the real exchange rate gradually appreciated over the next two years, in mid-2000 it was still 40 per cent lower than its mid-1997 level (Global Financial Data, 2015).

The causes of the financial crisis have been widely debated and among the several reasons put forward are a balance sheet mismatch caused by a reliance on short-term dollar-denominated debt, excessive risk-taking by domestic firms and banks that was encouraged by implicit government guarantees, an inadequate regulatory system for the financial sector, critical policy errors in dealing with the crisis and blind panic

by international financial investors.² Whatever the cause, the extent of the crisis was largely unanticipated. Figure 3.1 shows the sharp depreciation in the nominal exchange rate between the rupiah and the dollar that took place in 1997-98, and plots this change against the black market exchange rate. The black market rate closely tracks the nominal rate, suggesting that investors and firms were not aware of the dramatic forthcoming exchange rate movements that began in July 1997.

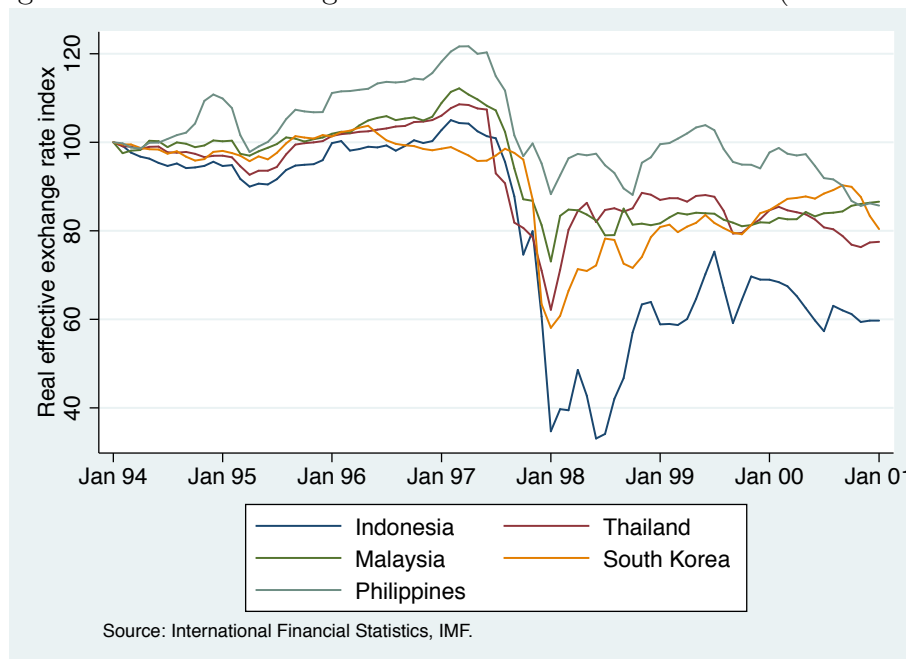
Figure 3.1: Exchange rate movements and consumer prices



In a regional crisis, the predicted effects of an exchange rate depreciation in one country may not take place if the regional trading partners of the country are also affected. However, the sharp fall in the exchange rate was larger than that of any of the neighbouring countries of Thailand, Korea, Malaysia, Philippines. Figure 3.2 illustrates that even within the context of the East Asian crisis, the decline in the value of the rupiah was extraordinary.

²There is a large literature on the causes of the East Asian currency crisis; some prominent reviews include Radelet and Sachs (1998), Mishkin (1999), Wade (1998), Corsetti et al. (1999), Krugman (1998).

Figure 3.2: Real Exchange Rate movements in East Asia (1994-2001)



3.2.2 Theoretical motivation

Changes in relative prices

Trade theory predicts that when the currency of a country depreciates, the relative price of the goods manufactured by its firms declines. For domestic firms, this would lead to a boost in foreign demand for their output, leading to higher foreign sales. However, for firms that import inputs, the relative increase in the price of inputs will lead to them face higher costs. For firms that are both exporters and importers, the benefit of the increased price competitiveness of their output on international markets will be mitigated by the increase in production costs.

Some papers use firm-level data to examine the effect of a change in the price of imported inputs on firm performance, mostly estimating the effect of a reduction in the tariffs of imported intermediate goods on a measure of the firm's productivity (Schor, 2004; Amiti and Konings, 2007; Topalova and Khandelwal, 2011; Bas, 2012; Kasahara and Lapham, 2013). Amiti and Konings (2007) use the same firm-level dataset as I do in this paper to provide evidence of an increase in total factor productivity in response to a phased reduction in the import tariffs on intermediate

inputs. In the closest setting to this paper, Gopinath and Neiman (2014) use data on Argentinian firms during the financial crisis of 2001 to identify the margins at which importing firms adjust to a large depreciation in the exchange rate. They find that much of the adjustment took place at the intensive margins; the entry and exit of firms and products into the import trade played a minor role.³

Financial constraints

The Indonesian exchange rate depreciation was also accompanied by a financial crisis precipitated by a run on bank deposits. By mid-1999, 64 of 237 banks had to be closed down, the share of non-performing loans to all loans peaked at 70 per cent, and over three-quarters of the deposits in the banking system were owned by the state (Claessens et al., 1999; World Bank, 2000, chap. 2). The collapse in the banking system led to a 50 per cent fall in the value of credit extended by banks to manufacturing establishments in the period between 1996 and 2000 (Blalock et al., 2008).

A large literature, surveyed by Bond and Van Reenen (2007), focuses on the effect of financial constraints on firm investment. In the wake of the 2008 global financial crisis, however, another dimension of tightening financial constraints has been proposed – a decline in the availability of trade finance. Importing and exporting firms face both higher shipping costs and times, as well as higher uncertainty over payment default compared to firms that only trade domestically. Trade finance comprises a number of credit instruments designed for firms to meet short term working capital requirements, such as letters of credit extended by banks to importers and exporters in order to pay for goods or bridge the period between supplying goods and receiving payment for them. As much as 90 per cent of all international transactions could involve some component of trade finance (Auboin, 2009). Since most trade finance is supplied by banks, a decline in the health of the banking sector could reduce the availability of credit extended to banks to meet their working capital requirements,

³There is a larger literature on the response of exporters to changes in the exchange rate. See, for example, Forbes (2002); Bugamelli and Infante (2003); Bernard and Jensen (2004b,a); Campa (2004); Bernard et al. (2009a); Berman et al. (2012); Greenaway et al. (2012); Fitzgerald and Haller (2014).

leading to a decline in the profitability of individual firms, as well as a decline in the volume of international trade.

Foreign ownership and access to finance

Firms vary in their ability to access finance depending on whether or not they are foreign-owned. Desai et al. (2008) find that, following a currency crisis, US-owned firms increased investments in overseas operations more than domestic firms. Blalock et al. (2008) find that foreign-owned exporters in Indonesia increased investments relative to domestically-owned exporters during the East Asian crisis. Foreign firms also frequently own a portfolio of assets and liabilities in multiple currencies, and engage in transactions in multiple currencies, which offsets the impact of the depreciation of any one currency. They may also explicitly hedge foreign exchange rate risk by trading on well-developed forward markets (Clark et al., 2004; Greenaway et al., 2012). Finally, for industries where firms are reliant on trade credit, foreign-owned firms will be more likely to be able to trade through access to internal finance.

Firms that export may also be better able to access external finance. In the presence of sunk costs necessary to start exporting, firms facing financial constraints will find it more difficult to become exporters, and exporters will have better financial health than non-exporters. Several theoretical and empirical papers develop models of the firm decision to export in the presence of costs, and find evidence of financial frictions (Roberts and Tybout, 1997; Campa, 2004; Bernard and Jensen, 2004b; Chaney, 2013). In the presence of costly screening of firms seeking finance, an exporter sends a positive signal about its financial health and may be better able to raise external finance (Ganesh-Kumar et al., 2001). Foreign exchange revenues also provide better collateral for loans (Tornell and Westermann, 2003). Greenaway et al. (2007) use data from a sample of UK firms to show that those firms that have been continuously exporting have better financial health than those that have just entered the export market. Since exporters may find it easier to raise external finances, I analyse two samples of firms – one comprising all Indonesian manufacturing plants, and one comprising all Indonesian exporters.

3.3 Data

3.3.1 Survei Industri

The data I use is drawn from the *Survei Tahunan Perusahaan Industri Pengolahan* (SI), which is an annual survey of all manufacturing plants in Indonesia that employ at least 20 workers. The data has been collected since 1975 by the *Badan Pusat Statistik* (BPS), the Central Bureau of Statistics of the government of Indonesia, and is designed to be a census, or a complete enumeration, of all manufacturing units in the country. Enumeration strategies were substantially changed between the 1980s and the 1990s (Aswicahyono, 2009), so I only consider data from the period 1991 to 1999, spanning the six years leading up to the financial crisis (1991-96), and the three crisis years (1997-1999). The survey contains information on a firm's industrial classification (at the 5-digit ISIC level), ownership of shares (private, foreign, government), total output (domestic sales and exported volume), input use (domestic and imported raw materials, labour and capital), and other related data.

The main variables in the analysis are value-added, materials, labour, capital, export and import status, and ownership status of a firm. Value-added is reported by the firm for the year as the difference between the value of total output and the value of intermediate materials and services. I exclude firms with negative value-added from the sample. Materials are divided into two categories – raw materials bought from domestic firms, and imported raw materials. The measure of imported raw materials includes the purchase of intermediate inputs from domestic distributors (Amiti and Konings, 2007). Labour is measured by the average number of workers employed per day in a given year.

Capital is measured as the market value of all fixed capital owned by the firm at the end of the measurement year. Capital stock data was not collected in 1996, so I follow Hallward-Driemeier and Rijkers (2013) in imputing capital stock from 1991-95 data on output, raw materials, labour use, lagged capital stock, investment, whether a firm exports, and the province where the firm is located.⁴ The results for capital stock are robust to the exclusion of data from the year 1996. All data is

⁴For firms that entered the survey only in 1996, lead data on the same variables from 1997-1999 is used to predict capital stock in 1996.

deflated to 1991 rupiahs, using sector-specific deflators; more details on the deflators are provided in the appendix.

A firm is considered a pre-crisis exporter if it exported any share of its output in the pre-crisis years (1991-1996). A firm is considered a pre-crisis importer if it reported any use of imported raw materials. I consider any firm with a foreign ownership of at least 25 per cent at any time in the pre-crisis years to be foreign-owned. A firm is considered to be government-owned if any share of the firm is owned by the central government. I discuss the sensitivity of the results to these definitions in Section 3.4.3.

While the SI is intended to be a complete census of all manufacturing units, in practice, some firms may fail to complete the survey. However, the sampling error resulting from missing establishments is believed to be quite small (AswicaHyono et al., 2010) since the SI data closely tracks the aggregate national accounts data. As in most firm surveys, there is likely to be measurement error in the reporting of several variables, particularly the capital stock. In order to reduce the effect of measurement error, I exclude some problematic observations. This process is outlined in the appendix.

3.3.2 Overview of the data and summary statistics

A total of 29,674 unique establishments were surveyed 159,954 times over 9 years, with the median firm being surveyed five times. Table 3.1 shows the distribution of the pre-crisis panel of firms across import and export status, and foreign ownership. Between the years 1991 and 1996, 27,912 firms are classified as domestically-owned and 1,762 firms as foreign-owned. More than 21 per cent of domestic firms are exporters, while around 18 per cent are importers, but over 1,800 firms, 6.5 per cent of the sample, are two-way traders, both exporting a share of their output and importing intermediate inputs. The share of traders is much higher among foreign firms, with over 70 per cent exporting some share of their output, 69 per cent importing inputs, and over half the sample participating in both importing and exporting activities. Considering all firms together, two thirds are non-traders, 18 per cent are exporters only, 10 per cent are importers only, while 10 per cent are

both importers and exporters. Two-way trading is an important feature of the firms in this sample.

Table 3.1: Pre-crisis distribution of trading and non-trading firms

Export status	Import status			
	Domestic Non-importer	Importer	Foreign Non-importer	Importer
Non-exporter	18,797 (67.3)	3,203 (11.5)	230 (13.1)	276 (15.7)
Exporter	4,086 (14.6)	1,826 (6.5)	312 (17.7)	944 (53.6)
Total firms	27,912		1,762	

Notes: Sample includes all firms in the pre-crisis period from 1991-1996. The share of all domestic or foreign firms is given as a percentage in parentheses.

Firm performance varies widely across different categories of firms. Table 3.2 reports summary statistics of the sample firms by trading and ownership category. As has been well-documented in the empirical literature, including in Indonesia (Arnold and Javorcik, 2009), foreign-owned firms have higher value-added and are more productive than domestic firms. They use more inputs, employ more labour, including skilled labour, and use more capital. Foreign firms also export a greater share of their output than domestic firms. Along the dimension of trade, trading firms have both higher value-added and are more productive than non-trading firms, with importers being more productive than non-importers. The performance gap between foreign and domestic firms is narrowest for the two-way traders – firms that both export and import. The analysis that will follow will focus on two samples – all manufacturing firms surveyed by the SI and a sample of exporters only, which allows me to compare foreign importers to relatively similar domestic importers.

Table 3.2: Descriptive statistics of sample firms

	Domestic firms				
	Non-trader	Exporter only	Importer only	Two-way trader	Total
Log value-added	12.50 (1.51)	13.95 (1.92)	13.54 (2.02)	15.44 (2.02)	13.11 (1.93)
Log value-added/worker	9.82 (1.11)	10.37 (1.24)	10.49 (1.26)	10.99 (1.24)	10.10 (1.23)
Log materials	12.64 (1.79)	14.22 (2.09)	13.97 (1.99)	15.82 (2.08)	13.35 (2.14)
Employment	70.31 (182.87)	239.79 (456.43)	145.38 (306.54)	651.55 (1,183.57)	155.47 (457.13)
Log Capital	12.99 (1.63)	14.43 (1.99)	14.19 (1.96)	15.98 (2.00)	13.67 (2.01)
Share of output exported	0.00 (0.00)	47.17 (43.48)	0.00 (0.00)	35.70 (40.11)	10.37 (27.89)
	Foreign firms				
	Non-trader	Exporter only	Importer only	Two-way trader	Total
Log value-added	13.88 (1.95)	15.28 (1.77)	16.05 (1.98)	16.15 (1.77)	15.71 (1.97)
Log value-added/worker	10.60 (1.37)	11.10 (1.34)	12.19 (1.39)	11.47 (1.26)	11.41 (1.38)
Log materials	14.02 (2.27)	15.57 (1.96)	16.29 (2.11)	16.57 (1.77)	16.07 (2.09)
Employment	148.25 (226.13)	359.91 (449.93)	287.27 (559.83)	725.80 (1,026.79)	527.76 (855.01)
Log Capital	14.68 (2.02)	15.46 (1.78)	16.28 (1.81)	16.66 (1.65)	16.19 (1.87)
Share of output exported	0.00 (0.00)	57.25 (43.32)	0.00 (0.00)	49.63 (43.20)	37.17 (43.44)

Notes: Sample includes all firms in the pre-crisis period from 1991-1996. Values reported are sample means with standard deviations in parentheses.

3.4 The impact of exchange rate movements on firm performance

3.4.1 Empirical framework

The key question asked by this paper is whether foreign-owned firms are able to mitigate the effect of a trade shock on their costs of production due to better access to finance and the ability to hedge currency risk. The unexpected and large exchange

rate depreciation of 1997 and 1998 led to an increase in the production costs for all importers of intermediate inputs. The exchange rate movement would have also benefited exporters, but within this group, exporters reliant on intermediate inputs would benefited to a lesser extent.

The first hypothesis I test is whether the value-added of Indonesian importers was reduced by the East Asian crisis. I estimate the following equation:

$$y_{it} = \beta_1(Importer * Post)_{it} + \lambda_i + d_t + \varepsilon_{it} \quad (3.1)$$

where y_{it} is a measure of the value-added of firm i in year t . $Importer * Post_{it}$ is an interaction of two indicator variables – an indicator for whether a firm was a pre-crisis importer of intermediate inputs and a post-crisis time indicator (for the years 1997-1999), λ_i is a firm fixed effect, d_t is a year effect and ε_{it} is a disturbance. β_1 is the estimated coefficient of interest. Under the assumption that firms that import intermediated inputs would have followed the same trend as firms that do not in the absence of the crisis, β_1 is an estimate of the effect of the crisis on the value-added of firms that imported intermediate inputs compared to those that did not. Since the crisis led to an increase in the price of imported inputs, importing firms should face lower value-added, that is, β_1 should be negative.

The second hypothesis I test is whether importers are adversely affected by the trade shock even after controlling for a firm's access to finance. Foreign-owned firms are more likely to have greater access to finance, both from banks and from within the multinational corporation network of which they are a part, compared to domestically-owned firms. Since access to finance is one of the most important tools available to firms to mitigate the effect of an unexpected increase in the costs of production, I control for whether a firm was under foreign ownership during the pre-crisis period. I estimate the following equation:

$$y_{it} = \beta_1(Importer * Post)_{it} + \beta_2(Foreign * Post)_{it} + \lambda_i + d_t + \varepsilon_{it} \quad (3.2)$$

where $Foreign * Post_{it}$ is an interaction of two indicator variables – an indicator for whether a firm was foreign-owned in the pre-crisis years and a post-crisis time indicator (for the years 1997-1999). β_1 measures the effect of the crisis on the value-added of importing firms compared to non-importing firms, having controlled for the

foreign ownership premium. β_2 is an estimate of the value-added premium earned by foreign-owned firms during the crisis years. If foreign firms were less financially constrained than domestic firms and this affected the value-added they earned, the sign of the estimated β_2 will be positive.

The third hypothesis I test is whether foreign firms were better able to manage the increase in input costs compared to domestic firms. If, due to differences in the level of trade finance or access to outsourced raw materials available to them, foreign firms are able to mitigate the effect of expensive imported inputs, their value-added should fall by less than domestically-owned importers. I estimate the following equation:

$$y_{it} = \beta_1(Importer * Post)_{it} + \beta_2(Foreign * Post)_{it} + \beta_3(Importer * Foreign * Post)_{it} + \lambda_i + d_t + \varepsilon_{it} \quad (3.3)$$

where $Importer * Foreign * Post_{it}$ is the triple interaction between indicators for whether the firm was a pre-crisis importer, for whether it was a pre-crisis foreign-owned firm, and an indicator for the post-crisis years. In this equation, β_1 is an estimate of the effect of the crisis on domestically-owned importers compared to domestically-owned non-importers. β_2 is the estimate of the effect of the crisis on foreign-owned non-importers compared to domestically-owned non-importers. β_3 , the triple difference estimator, measures the estimated impact of the crisis for importers among foreign firms, over and above the estimated impact of the crisis for importers among domestic firms. In other words, β_3 measures the relative effect of the crisis on foreign importer performance compared to domestic importer performance, after controlling for the foreign firm performance premium for non-importers. If, due to greater access to finance and better strategies for managing exchange rate risk, foreign-owned firms were better able to mitigate the effects of increase in their input costs than domestic firms, the estimated β_3 will be positive.

I estimate all three equations on two samples – a sample of all firms, and a sample of only exporters. The descriptive statistics in Table 3.2 indicate that the observable performance gap between foreign and domestic firms is narrowest for exporter-importers. In an estimation of Equation (3.3) on a sample of only exporters, β_3 will be estimated by comparing domestic-owned importer-exporters to

foreign-owned importer-exporters in the post-crisis period, which is arguably a more appropriate counterfactual than the comparison between foreign-owned importers and domestic-owned importers. These estimations will provide a useful check on the robustness of the main result as well as shed light on the ambiguous effect of exchange rate movements on the important category of two-way traders.

As all three estimations include firm fixed effects, the β s are estimated by the within-firm variation in value-added before and after the crisis. This rules out any variation due to time-invariant firm characteristics that could be affecting the firm's response to the crisis. In all cases, I cluster standard errors by firm to allow for any correlation in the observations within a firm, over time.

3.4.2 Results

The main results of this paper are presented in Table 3.3. Columns 1-3 show the results of all three estimations on the sample of all firms, while Columns 4-6 show the results for the sample of exporters only. For both samples, the sign of the estimated β_1 of Equation (3.1) is positive, rather than negative, which is a counter-intuitive result, suggesting that importers benefited from the increase in input costs. This, however, conceals considerable heterogeneity. The inclusion of a control for foreign ownership reduces the estimated β_1 to zero in Columns 2 and 4, while the estimated β_2 is large and positive, signifying a foreign ownership premium of almost 28 per cent on value-added earned by firms during the crisis, similar to evidence presented in Blalock et al. (2008). The core result, presented in Columns 3 and 6, shows the estimated effect of the crisis on foreign-owned importers relative to domestically-owned importers, and the difference is striking. The estimated β_1 measures the effect of the crisis on domestic importers compared to domestic non-importers, which is negative. Domestic importers saw a 4-7 per cent decline in their value-added relative to domestic non-importers, as a result of the crisis. β_3 measures the gap between foreign importers and non-importers, after controlling for the foreign firms earnings premium. The estimated β_3 is positive and significant, suggesting that foreign importers were better able to manage the effect of input costs shock, compared to domestic importers, and widening the performance gap between them by 32-36 per cent in the post-crisis period.

Table 3.3: Effect of exchange rate depreciation on firm value added

	All firms			Exporters		
	(1)	(2)	(3)	(4)	(5)	(6)
Importer*Post-crisis	0.036** (0.016)	-0.012 (0.017)	-0.041** (0.017)	0.060** (0.029)	-0.017 (0.030)	-0.072** (0.032)
Foreign*Post-crisis		0.276*** (0.031)	0.055 (0.051)		0.290*** (0.041)	0.035 (0.073)
Importer*Foreign*Post-crisis			0.327*** (0.064)			0.360*** (0.087)
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	159,954	159,954	159,954	43,502	43,502	43,502
No. of firms	29,674	29,674	29,674	7,168	7,168	7,168
Adj.R ²	0.0192	0.0209	0.0215	0.0278	0.0309	0.0319

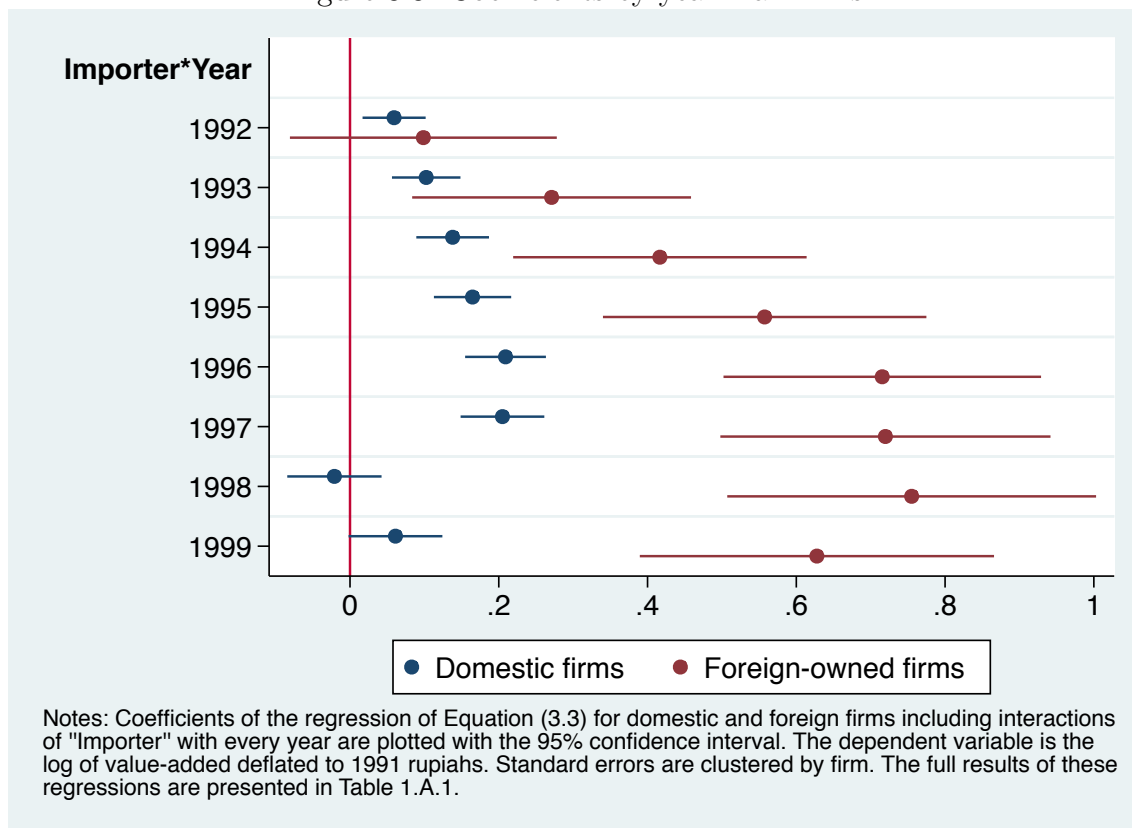
Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of value added deflated to 1991 rupiahs. Standard errors are in parentheses, clustered by firm.

To ensure that the results are not driven by pre-crisis trends, I estimate Equation 3.1 by including interactions of the firm-level import indicator with individual years in the dataset. For ease of interpretation, I estimate the equations separately by domestically-owned firms and foreign-owned firms and plot the resulting coefficients in Figure 3.3 and Figure 3.4. The widening performance gap between domestically-owned firms and foreign-owned firms is clearly identified, both when pooling all firms together and when considering just exporters. Relative to 1991, domestically-owned importers experienced a significant decline in value-added in 1998 and 1999, after the start of the East Asian crisis. Foreign-owned firms, however, were able to mitigate the effect of rising costs and sustained the same value-added through the crisis years, widening the performance gap over and above domestically-owned firms. The full results are presented in the appendix in Table 3.A.1.

3.4.3 Robustness to added controls

The results in Section 3.4.2 are robust to the inclusion of a number of variables which could potentially bias the estimated β s. First, there is potentially significant heterogeneity with respect to the size of the firm. Larger firms may be better able to hedge against movements in the exchange rate, or may be better placed to exploit

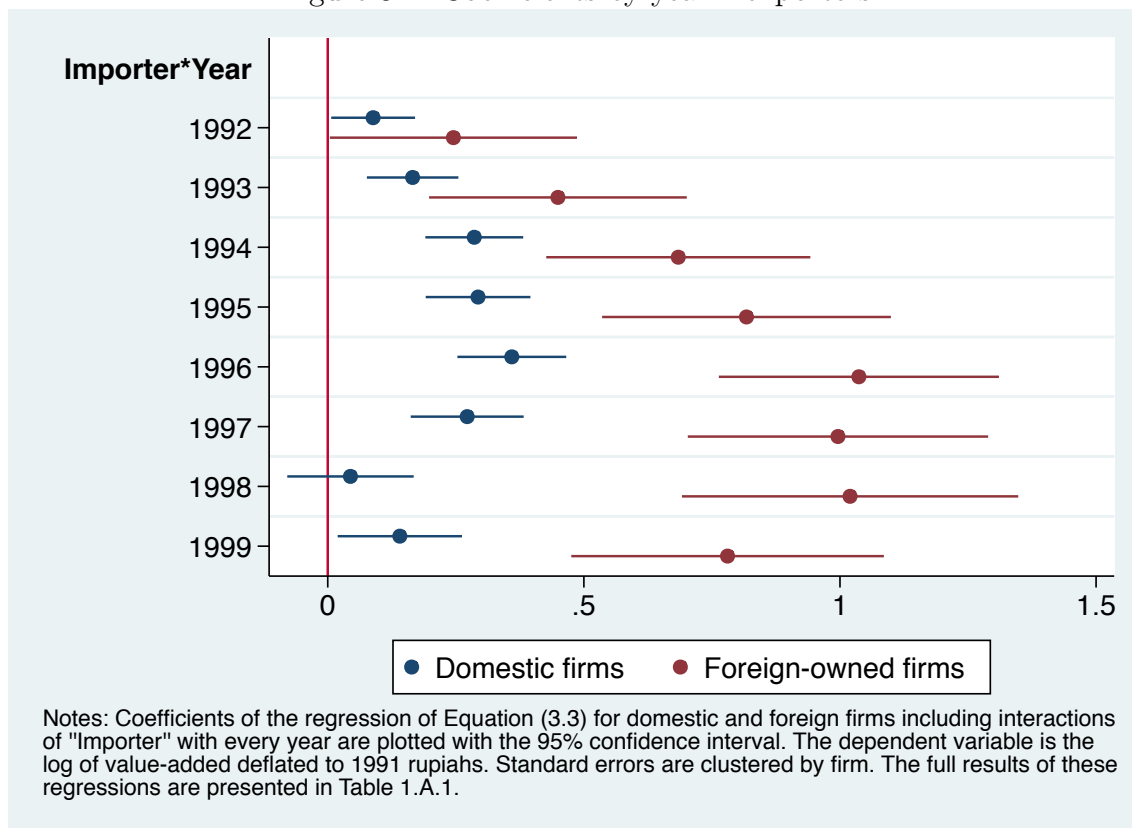
Figure 3.3: Coefficients by year – all firms



the export opportunities associated with a depreciation in the real exchange rate. Since foreign-owned firms are larger, on average, than domestic firms, the foreign-ownership dummy may be partially picking up an effect associated with firm size. To control for this, I include an interaction of the size of the firm in 1996 – measured by the number of employees – with the post crisis variable. These results are presented in Columns 2 of Tables 3.4 and 3.5. Larger importers are indeed associated with smaller declines in value-added, with the average-sized domestic importer facing a decline in value-added in response to the crisis. However, the core results of an attenuated effect on the value-added of foreign-owned firms compared to domestic firms continues to hold.

Government-owned firms may respond differently to the exchange rate movement than privately owned firms. During the years preceding the crisis, relatively unproductive government-owned firms with strong personal connections to President

Figure 3.4: Coefficients by year – exporters



Suharto's family were supported by explicit and implicit public subsidies, including favourable interest rates for loans (Fisman, 2001; Blalock et al., 2008). In October 1997, however, after the crisis had begun, the government of Indonesia agreed on a package with the IMF that required it to maintain a budget surplus of 1 per cent from 1998-99, leading to a collapse in public spending (Soesastro and Basri, 1998). In May 1998, Suharto resigned as president, potentially leaving many government-owned firms bereft of political support as well. Such firms could have faced a sudden decline in access to credit and been forced to scale back operations, driving the negative effect we see on domestic firms. The survey includes 3,356 firms with some government ownership, of which 2,689 are fully owned by the government. To control for any confounding effects related to government ownership, I include in the regression the interaction of an indicator for government ownership with post-crisis years. These results are presented in Columns 2 of Tables 3.4 and 3.5. The coeffi-

Table 3.4: Effect of exchange rate depreciation on firm value added with additional controls – all firms

	(1)	(2)	(3)	(4)
Importer*Post-crisis	−0.052*** (0.018)	−0.053*** (0.018)	−0.041** (0.017)	−0.044** (0.018)
Foreign*Post-crisis	0.042 (0.052)	0.042 (0.052)	0.024 (0.052)	0.050 (0.054)
Importer*Foreign*Post-crisis	0.329*** (0.064)	0.329*** (0.064)	0.354*** (0.065)	0.291*** (0.068)
Firm size*Post-crisis	0.013** (0.006)	0.014** (0.007)	0.025*** (0.006)	0.026*** (0.007)
Government-owned*Post-crisis		−0.026 (0.045)	−0.026 (0.043)	0.089** (0.044)
Productivity*Post-crisis			−0.011*** (0.002)	−0.019*** (0.004)
Year effects	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes
Year-sector effects	No	No	No	Yes
Observations	159,954	159,954	137,949	137,949
No. of firms	29,674	29,674	23,515	23,515
<i>Adj.R</i> ²	0.0215	0.0215	0.0249	0.0557

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of value added deflated to 1991 rupiahs. Standard errors are in parentheses, clustered by firm. For the specifications with TFP, standard errors are bootstrapped using 100 replications.

cient on government ownership is, in fact, negative, though they are not significantly different from zero. The estimated β s are unchanged.

More productive firms may be better able to sustain lower costs while maintaining quality during the crisis, and are also likely to find it easier to raise funds. Foreign-owned firms are also more likely to be productive, which could bias the estimated coefficients. To control for changes in productivity, I include an estimated measure of pre-crisis total factor productivity (TFP) as a control in the main regression.⁵ The results, reported in Columns 3 of Tables 3.4 and 3.5, interestingly, find that more productive firms in the pre-crisis period faced a greater decline in value-added during the crisis. The estimates of β_1 and β_3 , however, do not change significantly.

⁵I estimate TFP using the Akerberg et al. (2015) procedure as proposed by Loecker and Warzynski (2012) from pre-crisis 3-digit ISIC production functions. Further details on this estimation are provided in the appendix.

Table 3.5: Effect of exchange rate depreciation on firm value added with additional controls – exporters

	(1)	(2)	(3)	(4)	(5)
Importer*Post-crisis	−0.059*	−0.062*	−0.037	−0.049	−0.025
	(0.035)	(0.035)	(0.031)	(0.036)	(0.036)
Foreign*Post-crisis	0.042	0.039	0.016	0.091	0.083
	(0.073)	(0.073)	(0.087)	(0.081)	(0.082)
Importer*Foreign*Post-crisis	0.356***	0.356***	0.378***	0.216**	0.197*
	(0.087)	(0.087)	(0.096)	(0.104)	(0.104)
Firm size*Post-crisis	−0.014	−0.012	0.000	0.006	0.000
	(0.012)	(0.012)	(0.013)	(0.014)	(0.014)
Government-owned*Post-crisis		−0.073	−0.077	0.164***	0.164***
		(0.062)	(0.059)	(0.063)	(0.063)
Productivity*Post-crisis			−0.013**	−0.020***	−0.020***
			(0.006)	(0.006)	(0.006)
Export share*Post-crisis					0.002***
					(0.000)
Year effects	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes
Year-sector effects	No	No	No	Yes	Yes
Observations	43,502	43,502	40,116	40,116	40,116
No. of firms	7,168	7,168	6,292	6,292	6,292
Adj.R ²	0.0320	0.0320	0.0347	0.0708	0.0717

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of value added deflated to 1991 rupiahs. Standard errors are in parentheses, clustered by firm. For the specifications with TFP, standard errors are bootstrapped using 100 replications.

To control for sector-specific time trends and to guard against measurement error in the deflators used to convert all values to 1991 rupiahs, I include interactions of three-digit ISIC sector indicators with the year variable. The core results, presented in Columns 4 of Tables 3.4 and 3.5, are robust to their inclusion, even though standard errors increase. The estimated β_1 in the sample of exporters declines with the addition of controls but continues to be negative, while β_3 is positive, implying that foreign-owned importers do not face the same downward pressure on value-added as domestically-owned importers.

Among exporters, there may be heterogeneity across firms that export varying shares of their output. Foreign-owned firms are more likely to export a higher share of their output: foreign-owned exporters export 45 per cent of their output while domestically-owned exporters export 36 per cent of their output, on average. Firms

which export a greater share of their output may have stronger connections to overseas markets and be better placed to take advantage of the terms of trade movement in their favour. I include a control for the pre-crisis share of output interacted with the post-crisis indicator. These results are presented in Column 5 of Table 3.5, and show that export shares do not affect the core results.

Indonesia joined the World Trade Organization in 1995 and embarked upon a ten-year process of trade liberalisation, with a phased reduction in output and input tariffs. Part of what I term the pre-crisis period – 1995 and 1996 – and the entire post-crisis period are also marked by a reduction in import tariffs, which would ameliorate the effects of the large increase in input prices. However, the depreciation of the exchange rate dwarfs the reduction in tariff rates, and input prices rose substantially during the crisis period (Amiti and Konings, 2007). Moreover, the coefficients plotted in Figures 3.3 and 3.4 provide added assurance that the effect of the 1998 depreciation dominates any offsetting effects arising from the reduction of tariffs.

Finally, the results are not sensitive to the definition of foreign ownership. I classify all firms which have at least 25 per cent ownership as foreign, where 25 per cent is the median value of the foreign shareholding among firms with any foreign ownership. The results are unchanged if a limit of 10 per cent (the 40th percentile of foreign shareholding) or 50 per cent (the 70th percentile of foreign shareholding) is used.

3.4.4 Components of value-added

Were foreign firms able to outperform domestic firms in the post-crisis period by maintaining their use of inputs in the face of a cost shock, or through the more productive use of fewer inputs? To develop a better understanding of the mechanisms through which this differential impact may be taking place, I examine the effect of the exchange rate movement on the use of inputs, such as raw materials, labour and capital stock, by firms. The theoretical effect on the use of materials by importers is ambiguous because though material costs went up, firms would have been likely to switch to domestic substitutes where available. The price of labour, however, fell by a third, and it is likely that foreign importers would have relatively increased their

use of labour compared to domestic importers. Similarly, with the severe credit rationing that took place during the crisis, foreign importers would have had relatively greater access to finances, allowing them to increase capital stock, compared to domestic importers. Finally, foreign importers may have used their inputs more productively to increase output during the crisis, and I consider the effect of the crisis on productivity, as measured by value-added per worker. These results are presented in Tables 3.6 and 3.7.

Table 3.6: Effect of exchange rate depreciation on input use and productivity – all firms

	Dependent variable			
	Materials	Labour	Capital	Value added per worker
Importer*Post-crisis	-0.067*** (0.018)	-0.005 (0.008)	-0.066*** (0.023)	-0.036** (0.016)
Foreign*Post-crisis	0.117** (0.050)	0.002 (0.026)	0.003 (0.075)	0.053 (0.045)
Importer*Foreign*Post-crisis	0.225*** (0.063)	0.166*** (0.030)	0.051 (0.089)	0.161*** (0.057)
Year effects	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes
Observations	153,605	159,954	119,080	159,954
No. of firms	28,956	29,674	26,572	29,674
<i>Adj.R</i> ²	0.0199	0.0115	0.0339	0.0139

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of value added deflated to 1991 rupiahs. Standard errors are in parentheses, clustered by firm.

As expected, foreign importers were able to exploit the drop in real wages precipitated by the crisis to hire more labour compared to domestic importers. While domestic firms did not change their employment strength, foreign importers increased labour use by over 14 per cent. They were also able to use more materials, relatively increasing input use during a period of higher input costs. Domestic importers reduced their use of materials by 9 per cent relative to domestic non-importers, while this effect was mitigated for foreign importers. Foreign importers also increased the productivity gap to domestic importers during the crisis, by 19 and 22 per cent for all firms and exporters, respectively. However, there is no gap between the two in changes made to the capital stock during the crisis. Since capital stock data is

Table 3.7: Effect of exchange rate depreciation on input use and productivity – exporters

	Dependent variable			
	Materials	Labour	Capital	Value added per worker
Importer*Post-crisis	0.006 (0.032)	0.002 (0.016)	-0.034 (0.039)	-0.074** (0.029)
Foreign*Post-crisis	0.165** (0.069)	0.022 (0.039)	0.138 (0.106)	0.013 (0.064)
Importer*Foreign*Post-crisis	0.085 (0.084)	0.138*** (0.044)	-0.046 (0.122)	0.222*** (0.077)
Year effects	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes
Observations	42,425	43,502	33,466	43,502
No. of firms	7,106	7,168	6,787	7,168
<i>Adj.R</i> ²	0.0343	0.0242	0.0299	0.0161

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of value added deflated to 1991 rupiahs. Standard errors are in parentheses, clustered by firm.

partially imputed and potentially more prone to errors in calculation, it may be difficult to draw inferences from this estimation, but there is no evidence to suggest that foreign firms were building up capital stock during the crisis period, relative to domestic firms.

Were foreign-owned firms also more likely to continue importing inputs after the crisis? To examine the import decision of the firm in the post-crisis period, I follow Amiti et al. (2014) in constructing a measure of the import intensity of a firm as the share of imported raw materials in total variable costs, which is comprised of a firm's total material costs and its wage bill. I expect that all importers would have reduced their expenditure on imported raw materials by switching to domestic substitutes, where possible, leading to a decline in both the probability that a firm imports as well as the import intensity. Columns 1 and 3 of Table 3.8 show the results of the estimation of the probability that a firm continues to import during the crisis period. Domestic pre-crisis importers are less likely to import during crisis period than foreign-owned pre-crisis importers. However, all firms, domestic and foreign, reduce their reliance on imported inputs; Columns 2 and 4 show that importers reduced their import intensity ratio – the ratio of imported material costs to total

variable costs – by between 4 and 6 percentage points in response to the crisis.

Table 3.8: Effect of exchange rate depreciation on the import decision

	All firms		Exporters	
	Importing	Import Intensity	Importing	Import Intensity
Importer*Post-crisis	-0.145*** (0.006)	-4.187*** (0.322)	-0.105*** (0.011)	-2.187*** (0.512)
Foreign*Post-crisis	0.065*** (0.012)	2.292*** (0.579)	0.073*** (0.018)	2.541*** (0.815)
Importer*Foreign*Post-crisis	0.019 (0.018)	0.268 (0.977)	-0.019 (0.024)	-1.964 (1.250)
Year effects	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes
Observations	159,954	159,954	43,502	43,502
No. of firms	29,674	29,674	7,168	7,168
<i>Adj.R</i> ²	0.0238	0.0105	0.0107	0.0034

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of value added deflated to 1991 rupiahs. Standard errors are in parentheses, clustered by firm.

This evidence is consistent with the hypothesis that foreign-owned firms were able to mitigate the effects of the cost shock, not by increasing capital stocks, but by increasing hires during a period of low wages and by sustaining the use of raw materials. They were also more likely to continue to import some share of their inputs, which could account for the widening in the productivity gap over domestic firms in the post-crisis period. There is no evidence that foreign firms were relatively better at taking up investment opportunities during a period of credit rationing in domestic market. However, their superior access to trade credit and to strategies that permit firms to hedge against exchange rate risks would have played a role in allowing them to weather the temporary increase in costs.

3.4.5 The survival of firms

Were foreign firms outperforming domestic firms simply by surviving through the crisis period? I consider the effect of having been an importer heading into the crisis on the probability that the firm survived the crisis.⁶ I estimate a linear probability

⁶Firms are considered to have survived the crisis if they were surveyed by the BPS for the year 1999. The survey, however, is designed to only cover firms with at least twenty employees. It may

model of survival until 1999 for a pooled sample of all firm-year observations in the period from 1991 to 1996. The model includes indicators for whether the firm is an importer, whether it is foreign-owned, and the interaction between the two. Existing evidence from the developing world underscores the positive correlation between the age and the size of a firm, on the one hand, and the probability of its survival, on the other (Bernard and Sjöholm, 2003; Frazer, 2005; Soderbom et al., 2006; Hallward-Driemeier and Rijkers, 2013). Accordingly, I control for the (log) age of the firm, as well as a quadratic function in the number of workers employed by the firm, and the capital stock in use. Hallward-Driemeier and Rijkers find in Indonesia that, while more productive firms were generally less likely to exit, the correlation between productivity and survival became attenuated during the crisis. To control for productivity, I include an estimated measure of TFP as well. I control for government ownership of the firm, and include a full set of three-digit ISIC sector and province controls.

The correlates of survival are presented in Table 3.9. The first two columns show the effect of the crisis on the survival of importers compared to all other firms using a pooled sample of all pre-crisis observations, with standard errors clustered by firm. Columns 3 and 4 show the effect of the crisis on the survival of importers, and foreign-owned importers in a pooled sample of pre-crisis exporting firms only. Columns 2 and 4 include controls for estimated TFP.

The results agree with evidence from other research. Older firms employing more labour and capital, and more productive firms, are more likely to survive the crisis period. However, survival probabilities do not vary by ownership of the firm. Importers are more likely to survive than non-importing firms, both within the sample of all firms as well as the sample of exporters only. Nor is there any difference in the probability of survival between foreign-owned importers and domestic importers. This suggests that non-random exits are not likely to be driving the main results in this paper.

be the case that firms have dropped out of the survey but continue to operate with fewer than twenty employees. In this analysis, firms that are no longer covered by the SI are considered to have exited.

Table 3.9: Effect of exchange rate depreciation on firm survival

	All firms		Exporters	
	(1)	(2)	(3)	(4)
Pre-crisis importer	0.037*** (0.008)	0.038*** (0.008)	0.041*** (0.015)	0.040*** (0.015)
Pre-crisis foreign-owned	0.041** (0.021)	0.038* (0.021)	0.037 (0.027)	0.028 (0.024)
Pre-crisis importer and foreign-owned	-0.006 (0.024)	-0.009 (0.023)	-0.011 (0.032)	-0.006 (0.027)
Log Labour	0.306*** (0.017)	0.300*** (0.019)	0.204*** (0.029)	0.191*** (0.028)
Log Capital	0.077*** (0.012)	0.082*** (0.016)	0.047** (0.021)	0.077*** (0.030)
Log Labour ²	-0.046*** (0.003)	-0.046*** (0.004)	-0.027*** (0.005)	-0.026*** (0.005)
Log Capital ²	-0.005*** (0.001)	-0.005*** (0.001)	-0.002* (0.001)	-0.004** (0.002)
Government-owned	-0.012 (0.016)	-0.016 (0.017)	-0.006 (0.019)	-0.011 (0.017)
Firm age	0.001*** (0.000)	0.001*** (0.000)	0.002*** (0.000)	0.002*** (0.000)
Productivity		0.006*** (0.002)		0.004 (0.004)
ISIC 3 sector effects	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes
Observations	82,519	80,626	23,783	23,113
Adj.R ²	0.0980	0.0974	0.1044	0.1047

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the probability that the firm survives till 1999. Standard errors are in parentheses, clustered by firm. For the specifications with TFP, standard errors are bootstrapped using 100 replications.

3.5 Conclusion

The findings in this paper suggest that foreign-owned firms are better able to manage a sudden increase in the cost of their inputs, compared to domestic firms. Typically, foreign firms with strong international links have access to external sources of finance, and are able to hedge exchange rate risk. This is particularly important during a financial crisis, when a developing country is struck by both a depreciation in the currency as well as a contraction in the availability of credit. Foreign importers in Indonesia were able to earn higher value-added, use more raw mate-

rial, hire more labour and maintain higher productivity than domestic firms during the East Asian crisis. They were more likely to continue to import than domestic firms, even though they were no more likely to survive the crisis. These findings hold both for the sample of all Indonesian firms, as well as a sample of Indonesian exporters only. However, foreign firms were not increasing investments or capital stocks relative to domestic firms; even exporters who arguably benefited from the movement in exchange rates did not significantly increase investment. This suggests another channel for mitigating the effect of higher input costs – the use of trade finance to meet working capital requirements. This possibility is consistent with recent evidence on the trade collapse during the global recession of 2008, and suggests an important role for finance in sustaining international trade volumes and firm performance during a credit crunch.

3.A Appendix

3.A.1 Data and construction of sample

Problematic observations

I exclude or correct some invalid, missing or extreme observations in the data using techniques similar to Blalock et al. (2008) and Hallward-Driemeier and Rijkers (2013). For example, a few firms which have incorrectly reported basic data, such as number of employees, output or value added, are removed. I trim the data to exclude firms in the top and bottom 1% of value added labour productivity. I also correct data where percentage values, such as share of exports or share of foreign ownership are reported to fall outside the 0-100 range. The data for capital stock in 1996 has not been collected, so I follow Hallward-Driemeier and Rijkers (2013) by using 1991-95 data on output, raw materials, labour use, lagged capital stock, investment, whether a firm exports, and the province where the firm is located, to predict the level of capital stock in 1996. For firms that entered the survey only in 1996, lead data on the same variables from 1997-1999 is used to predict capital stock in 1996.

Deflators

All data is deflated to 1991 rupiahs, using sector-specific deflators. Output is deflated using a 5-digit sector deflator, inputs by a 2-digit sector deflator, and capital and investment are deflated by using a manufacturing sector deflator on an annual basis. Where capital stock is disaggregated into its constituent components, I deflate capital by parts, using annual manufacturing sector deflators for construction, machinery, and vehicles. The wage bill is deflated using the national consumer price index.

3.A.2 Including interactions by year

To ensure that the results are not driven by pre-crisis trends, I estimate Equation 3.1 by including interactions of the firm-level import indicator with individual years in the dataset. For ease of interpretation, I estimate the equations separately by

domestically-owned firms and foreign-owned firms. The results are presented in Table 3.A.1.

Table 3.A.1: Effect of exchange rate depreciation on value added – interactions by year

	All firms		Exporters	
	Domestic-owned	Foreign-owned	Domestic-owned	Foreign-owned
Importer*				
1992	0.059*** (0.022)	0.099 (0.091)	0.089** (0.042)	0.245** (0.123)
1993	0.102*** (0.023)	0.271*** (0.096)	0.166*** (0.046)	0.449*** (0.128)
1994	0.138*** (0.025)	0.417*** (0.101)	0.286*** (0.049)	0.684*** (0.131)
1995	0.165*** (0.026)	0.557*** (0.111)	0.293*** (0.052)	0.817*** (0.144)
1996	0.209*** (0.028)	0.716*** (0.109)	0.359*** (0.054)	1.037*** (0.139)
1997	0.205*** (0.029)	0.720*** (0.113)	0.272*** (0.056)	0.996*** (0.149)
1998	-0.021 (0.032)	0.755*** (0.126)	0.044 (0.063)	1.020*** (0.167)
1999	0.061* (0.032)	0.628*** (0.121)	0.141** (0.062)	0.781*** (0.156)
Year effects	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes
Observations	149,103	10,851	35,614	7,888
No. of firms	27,912	1,762	5,912	1,256
<i>Adj.R</i> ²	0.0180	0.0857	0.0212	0.1092

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the log of value added deflated to 1991 rupiahs. Standard errors are in parentheses, clustered by firm.

3.A.3 Estimation of productivity

The value added regressions also control for a measure of total factor productivity as a proxy for unobserved firm quality. I estimate TFP using the Akerberg et al. (2015) procedure as proposed by Loecker and Warzynski (2012).⁷ Following their

⁷I carry out this estimation in Stata, using an adaptation of Loecker and Warzynski's publicly available code.

notation, a firm has the following value-added production function:

$$y_{it} = f(l_{it}, k_{it}; \beta) + \omega_{it} + \varepsilon_{it} \quad (3.4)$$

where y_{it} is the value-added reported by firm i at time t , l_{it} contains labour use, k_{it} contains capital use, and β contains the input-specific coefficient. All values are in logs. $f(\cdot)$ may be estimated in a number of ways: I estimate both a Cobb-Douglas production function, which is linear in inputs, and the more flexible translog production function, which is linear in inputs, the square of inputs, and the interaction terms between the inputs.

Productivity is proxied by the demand for intermediate inputs:

$$\omega_{it} = h_t(m_{it}, k_{it}, \mathbf{z}_{it}) \quad (3.5)$$

where m_{it} contains materials used by firm i at time t , and $\mathbf{z}_{it}$ contains any additional control variables that may affect the firms's input demand. I include an indicator for whether the firm exports, an indicator for whether it imports, and an indicator if it is foreign-owned.

Consider the Cobb-Douglas production function. In the first stage, the following equation is estimated for each three-digit ISIC industrial sector:

$$y_{it} = \beta_l l_{it} + \beta_k k_{it} + h_t(m_{it}, k_{it}, \mathbf{z}_{it}) + \varepsilon_{it} \quad (3.6)$$

After the first stage, estimates of $\hat{y}_{it}$ and $\hat{\varepsilon}_{it}$ are obtained. An estimate of $\omega_{it}(\beta)$ can be obtained for any value of β from $\hat{y}_{it} - \beta_l l_{it} - \beta_k k_{it}$. In the second stage, the following law of motion for productivity is assumed to hold:

$$\omega_{it} = g_t(\omega_{it-1}) + \epsilon_{it} \quad (3.7)$$

such that $\omega_{it}(\beta)$ is regressed on its lagged value, $\omega_{it-1}(\beta)$, to obtain estimates of $\epsilon(\beta)$. Finally, Loecker and Warzynski use lagged labour to identify the coefficient on labour, while assuming that capital is decided a period ahead and is not correlated with any shock to productivity, to recover the production function coefficients from the following moments:

$$E \left(\epsilon(\beta) \begin{pmatrix} l_{it-1} \\ k_{it} \end{pmatrix} \right) = 0 \quad (3.8)$$

References

- Akerberg, D. A., Caves, K., and Frazer, G. (2015). Identification properties of recent production function estimators. *Econometrica*, 83(6):2411–2451.
- Ahn, J., Amiti, M., and Weinstein, D. E. (2011). Trade finance and the great trade collapse. *American Economic Review*, 101(3):298–302.
- Akerlof, G. A. and Yellen, J. L. (1986). *Efficiency wage models of the labor market*. Cambridge University Press.
- Amiti, M., Itskhoki, O., and Konings, J. (2014). Importers, exporters, and exchange rate disconnect. *American Economic Review*, 104(7):1942–78.
- Amiti, M. and Konings, J. (2007). Trade liberalization, intermediate inputs, and productivity: Evidence from Indonesia. *American Economic Review*, 97(5):1611–1638.
- Amiti, M. and Weinstein, D. E. (2011). Exports and financial shocks. *The Quarterly Journal of Economics*, 126(4):1841–1877.
- Aristei, D., Castellani, D., and Franco, C. (2013). Firms’ exporting and importing activities: Is there a two-way relationship? *Review of World Economics (Weltwirtschaftliches Archiv)*, 149(1):55–84.
- Arnold, J. M. and Javorcik, B. S. (2009). Gifted kids or pushy parents? Foreign direct investment and plant productivity in Indonesia. *Journal of International Economics*, 79(1):42–53.

REFERENCES

- Aswicahyono, H. (2009). A survey of micro-data analyses in Indonesia. In Corbett, J. and Umezaki, S., editors, *Deepening East Asian economic integration*, volume 2008-1 of *ERIA Research Project Report*, pages 456–472. Jakarta: ERIA.
- Aswicahyono, H., Hill, H., and Narjoko, D. (2010). Industrialisation after a deep economic crisis: Indonesia. *Journal of Development Studies*, 46(6):1084–1108.
- Auboin, M. (2009). Restoring trade finance during a period of financial crisis: Stock-taking of recent initiatives. Staff working paper ersd-2009-16, World Trade Organization.
- Aw, B.-Y. and Hwang, A. (1995). Productivity and the export market: A firm-level analysis. *Journal of Development Economics*, 47(2):313 – 332.
- Bacchetta, M., Ernst, E., Bustamante, J., International Labour Office, and World Trade Organization (2009). *Globalization and informal jobs in developing countries: A joint study of the International Labour Office and the Secretariat of the World Trade Organization*. Services des publications de l’OMC, Organisation mondiale du commerce.
- Banerjee, A. V. and Duflo, E. (2005). Growth theory through the lens of development economics. In Aghion, P. and Durlauf, S., editors, *Handbook of Economic Growth*, volume 1 of *Handbook of Economic Growth*, chapter 7, pages 473–552. Elsevier.
- Barrett, C. B. (2001). Measuring integration and efficiency in international agricultural markets. *Review of Agricultural Economics*, 23(1):19–32.
- Bas, M. (2012). Input-trade liberalization and firm export decisions: Evidence from Argentina. *Journal of Development Economics*, 97(2):481 – 493.
- Beegle, K., Dehejia, R., and Gatti, R. (2009). Why should we care about child labor?: The education, labor market, and health consequences of child labor. *Journal of Human Resources*, 44(4).
- Beegle, K., Dehejia, R. H., and Gatti, R. (2006). Child labor and agricultural shocks. *Journal of Development Economics*, 81(1):80–96.

REFERENCES

- Berman, N. and Martin, P. (2010). The vulnerability of sub-Saharan Africa to the financial crisis: The case of trade. CEPR Discussion Papers 7765, Centre for Economic Policy Research.
- Berman, N., Martin, P., and Mayer, T. (2012). How do different exporters react to exchange rate changes? *The Quarterly Journal of Economics*, 127(1):437–492.
- Bernard, A. B. and Jensen, J. B. (1999). Exceptional exporter performance: Cause, effect, or both? *Journal of International Economics*, 47(1):1–25.
- Bernard, A. B. and Jensen, J. B. (2004a). Entry, expansion, and intensity in the US export boom, 1987-1992. *Review of International Economics*, 12(4):662–675.
- Bernard, A. B. and Jensen, J. B. (2004b). Why some firms export. *The Review of Economics and Statistics*, 86(2):561–569.
- Bernard, A. B., Jensen, J. B., Redding, S. J., and Schott, P. K. (2009a). The margins of US trade. *American Economic Review*, 99(2):487–93.
- Bernard, A. B., Jensen, J. B., and Schott, P. K. (2009b). Importers, exporters and multinationals: A portrait of firms in the U.S. that trade goods. In *Producer Dynamics: New Evidence from Micro Data*, NBER Chapters, pages 513–552. National Bureau of Economic Research, Inc.
- Bernard, A. B., Robertson, R., and Schott, P. K. (2010). Is Mexico a Lumpy Country? *Review of International Economics*, 18(5):937–950.
- Bernard, A. B. and Sjöholm, F. (2003). Foreign owners and plant survival. NBER Working Papers 10039, National Bureau of Economic Research, Inc.
- Betts, J. R. and McFarland, L. L. (1995). Safe port in a storm: The impact of labor market conditions on community college enrollments. *Journal of Human Resources*, 30(4):741–765.
- Binder, M. (1999). Schooling indicators during Mexico’s “lost decade”. *Economics of Education Review*, 18(2):183–199.

REFERENCES

- Blalock, G., Gertler, P. J., and Levine, D. I. (2008). Financial constraints on investment in an emerging market crisis. *Journal of Monetary Economics*, 55(3):568–591.
- Blundell, R. and Bond, S. (1998). Initial conditions and moment restrictions in dynamic panel data models. *Journal of Econometrics*, 87(1):115–143.
- Bond, S. and Van Reenen, J. (2007). Microeconomic models of investment and employment. In Heckman, J. and Leamer, E., editors, *Handbook of Econometrics*, volume 6 of *Handbook of Econometrics*, chapter 65. Elsevier.
- Bugamelli, M. and Infante, L. (2003). Sunk costs of exports. Temi di discussione (Economic working papers) 469, Bank of Italy, Economic Research and International Relations Area.
- Calvo, G. A., Izquierdo, A., and Talvi, E. (2006). Phoenix Miracles in Emerging Markets: Recovering without Credit from Systemic Financial Crises. NBER Working Papers 12101, National Bureau of Economic Research, Inc.
- Campa, J. M. (2004). Exchange rates and trade: How important is hysteresis in trade? *European Economic Review*, 48(3):527–548.
- Chaney, T. (2013). Liquidity constrained exporters. NBER Working Papers 19170, National Bureau of Economic Research, Inc.
- Chen, D. L. (2010). Club goods and group identity: Evidence from Islamic resurgence during the Indonesian financial crisis. *Journal of Political Economy*, 118(2):300–354.
- Chor, D. and Manova, K. (2010). Off the cliff and back? Credit conditions and international trade during the Global Financial Crisis. NBER Working Papers 16174, National Bureau of Economic Research, Inc.
- Claessens, S., Djankov, S., and Klingebiel, D. (1999). Financial restructuring in East Asia: Halfway there? Financial Sector Discussion Paper 3, World Bank Washington DC.

REFERENCES

- Clark, P. B., Wei, S.-J., Tamirisa, N. T., Sadikov, A. M., and Zeng, L. (2004). A new look at exchange rate volatility and trade flows. IMF Occasional Papers 235, International Monetary Fund.
- Comola, M. and de Mello, L. (2011). How does decentralized minimum wage setting affect employment and informality? The case of Indonesia. *Review of Income and Wealth*, 57:S79–S99.
- Comola, M. and de Mello, L. (2013). Salaried employment and earnings in Indonesia: New evidence on the selection bias. *Applied Economics*, 45(19):2808–2816.
- Corsetti, G., Pesenti, P., and Roubini, N. (1999). Paper tigers?: A model of the Asian crisis. *European Economic Review*, 43(7):1211 – 1236.
- Desai, M. A., Foley, C. F., and Forbes, K. J. (2008). Financial constraints and growth: Multinational and local firm responses to currency depreciations. *Review of Financial Studies*, 21(6):2857–2888.
- Desai, M. A., Foley, C. F., and Hines, J. R. (2004). A multinational perspective on capital structure choice and internal capital markets. *Journal of Finance*, 59(6):2451–2487.
- Duryea, S. and Arends-Kuenning, M. (2003). School attendance, child labor and local labor market fluctuations in urban Brazil. *World Development*, 31(7):1165–1178.
- Duryea, S., Lam, D., and Levison, D. (2007). Effects of economic shocks on children’s employment and schooling in Brazil. *Journal of Development Economics*, 84(1):188–214.
- Edmonds, E. V. and Pavcnik, N. (2006). Trade liberalization and the allocation of labor between households and markets in a poor country. *Journal of International Economics*, 69(2):272–295.
- El Badaoui, E., Strobl, E., and Walsh, F. (2008). Is there an informal employment wage penalty? Evidence from South Africa. *Economic Development and Cultural Change*, 56:683–710.

REFERENCES

- Falco, P., Kerr, A., Rankin, N., Sandefur, J., and Teal, F. (2011). The returns to formality and informality in urban Africa. *Labour Economics*, 18, Supplement 1(0):S23 – S31.
- Ferreira, F. H. G. and Schady, N. (2009). Aggregate economic shocks, child schooling and child health. *World Bank Research Observer*, 24(2):147–181.
- Fields, G. S. (1975). Rural-urban migration, urban unemployment and underemployment, and job-search activity in LDCs. *Journal of Development Economics*, 2(2):165 – 187.
- Fisman, R. (2001). Estimating the value of political connections. *American Economic Review*, 91(4):1095–1102.
- Fitzgerald, D. and Haller, S. (2014). Exporters and shocks: Dissecting the international elasticity puzzle. NBER Working Papers 19968, National Bureau of Economic Research, Inc.
- Fitzgerald, J., Gottschalk, P., and Moffitt, R. (1998). An analysis of sample attrition in panel data: The Michigan Panel Study of Income Dynamics. *Journal of Human Resources*, 33(2):251–299.
- Food and Agricultural Organization of the United Nations (2015). FAOSTAT. Producer prices - Annual. Rome, Italy: FAO. Retrieved from <http://faostat3.fao.org/download/P/PP/E>.
- Forbes, K. J. (2002). How do large depreciations affect firm performance? NBER Working Papers 9095, National Bureau of Economic Research, Inc.
- Frankenberg, E. and Karoly, L. (1995). The 1993 Indonesian Family Life Survey: Overview and field report. Technical report, RAND, Santa Monica, CA.
- Frankenberg, E., Smith, J. P., and Thomas, D. (2003). Economic shocks, wealth and welfare. *Journal of Human Resources*, 38(2).
- Frankenberg, E. and Thomas, D. (2000). The Indonesia Family Life Survey (IFLS): Study design and results from Waves 1 and 2. Technical report, RAND, Santa Monica, CA.

REFERENCES

- Frazer, G. (2005). Which firms die? A look at manufacturing firm exit in Ghana. *Economic Development and Cultural Change*, 53(3):585–617.
- Ganesh-Kumar, A., Sen, K., and Vaidya, R. (2001). Outward orientation, investment and finance constraints: A study of Indian firms. *The Journal of Development Studies*, 37(4):133–149.
- Gibbons, R. and Katz, L. F. (1992). Does unmeasured ability explain inter-industry wage differentials? *Review of Economic Studies*, 59(3):515–35.
- Gibson, M. J. and Graciano, T. A. (2011). The decision to import. *American Journal of Agricultural Economics*, 93(2):444–449.
- Global Financial Data (2015). Exchange rates. Retrieved from www.globalfinancialdata.com.
- Goldin, C. (1999). Egalitarianism and the returns to education during the great transformation of American education. *Journal of Political Economy*, 107(S6):S65–S94.
- Gopinath, G. and Neiman, B. (2014). Trade adjustment and productivity in large crises. *American Economic Review*, 104(3):793–831.
- Greenaway, D., Guariglia, A., and Kneller, R. (2007). Financial factors and exporting decisions. *Journal of International Economics*, 73(2):377–395.
- Greenaway, D., Kneller, R., and Zhang, X. (2012). The effect of exchange rates on firm exports and the role of FDI. *Review of World Economics (Weltwirtschaftliches Archiv)*, 148(3):425–447.
- Griliches, Z. and Hausman, J. A. (1986). Errors in variables in panel data. *Journal of Econometrics*, 31(1):93 – 118.
- Guarcello, L., Mealli, F., and Rosati, F. (2010). Household vulnerability and child labor: The effect of shocks, credit rationing, and insurance. *Journal of Population Economics*, 23(1):169–198.

REFERENCES

- Hallward-Driemeier, M. and Rijkers, B. (2013). Do crises catalyze creative destruction? Firm-level evidence from Indonesia. *The Review of Economics and Statistics*, 95(5):1788–1810.
- Halpern, L., Koren, M., and Szeidl, A. (2011). Imported inputs and productivity. CeFiG Working Papers 8, Center for Firms in the Global Economy.
- Harris, J. R. and Todaro, M. P. (1970). Migration, unemployment and development: A two-sector analysis. *The American Economic Review*, pages 126–142.
- Heckman, J. J. (2001). Micro data, heterogeneity, and the evaluation of public policy: Nobel lecture. *Journal of Political Economy*, 109(4):673–748.
- Heckman, J. J., Lochner, L. J., and Todd, P. E. (2006). *Earnings Functions, Rates of Return and Treatment Effects: The Mincer Equation and Beyond*, volume 1 of *Handbook of the Economics of Education*, chapter 7, pages 307–458. Elsevier.
- Heckman, J. J. and Sedlacek, G. (1985). Heterogeneity, aggregation, and market wage functions: An empirical model of self-selection in the labor market. *Journal of Political Economy*, 93(6):1077–1125.
- Iacovone, L. and Zavacka, V. (2009). Banking crises and exports : Lessons from the past. Policy Research Working Paper Series 5016, World Bank.
- International Financial Statistics (2015). Consumer price indices. Washington DC: International Monetary Fund. Retrieved from data.imf.org.
- International Labour Organization (2002). Workers/Labor union act of Indonesia (Act no. 21/2000): User guide. Technical report, International Labour Organization.
- Jacoby, H. G. (1994). Borrowing constraints and progress through school: Evidence from Peru. *The Review of Economics and Statistics*, 76(1):151–60.
- Jacoby, H. G. and Skoufias, E. (1997). Risk, financial markets, and human capital in a developing country. *Review of Economic Studies*, 64(3):311–35.

REFERENCES

- Jensen, R. (2000). Agricultural volatility and investments in children. *American Economic Review*, 90(2):399–404.
- Kasahara, H. and Lapham, B. (2013). Productivity and the decision to import and export: Theory and evidence. *Journal of International Economics*, 89(2):297 – 316.
- Katz, L. F. and Summers, L. H. (1989). Industry Rents: Evidence and Implications. *Brookings Papers on Economic Activity: Microeconomics*, pages 209–90.
- Kerr, A. and Teal, F. (2012). The determinants of earnings inequalities: Panel data evidence from South Africa. CSAE Working Paper Series 2012-04, Centre for the Study of African Economies, University of Oxford.
- Krugman, P. (1998). Bubble boom crash: Theoretical notes on Asia’s crisis. Mimeo, Massachusetts Institute of Technology.
- Kugler, M. and Verhoogen, E. (2009). Plants and imported inputs: New facts and an interpretation. *American Economic Review*, 99(2):501–07.
- Levinsohn, J. A., Berry, S. T., and Friedman, J. (2003). Impacts of the Indonesian economic crisis: Price changes and the poor. In *Managing Currency Crises in Emerging Markets*, NBER Chapters, pages 393–428. National Bureau of Economic Research, Inc.
- Loecker, J. D. and Warzynski, F. (2012). Markups and firm-level export status. *American Economic Review*, 102(6):2437–71.
- MacGarvie, M. (2006). Do firms learn from international trade? *The Review of Economics and Statistics*, 88(1):46–60.
- Magnac, T. (1991). Segmented or competitive labor markets. *Econometrica*, 59(1):pp. 165–187.
- Maloney, W. F. (1999). Does informality imply segmentation in urban labor markets? Evidence from sectoral transitions in Mexico. *World Bank Economic Review*, 13(2):275–302.

REFERENCES

- Maloney, W. F. (2004). Informality revisited. *World Development*, 32(7):1159–1178.
- Manova, K. (2013). Credit constraints, heterogeneous firms, and international trade. *The Review of Economic Studies*, 80(2):711–744.
- Marcelle, M. and Strobl, E. (2003). Do smaller firms pay less in the Caribbean? The case of Trinidad and Tobago. *Journal of Development Studies*, 39(5):181–198.
- McKenzie, D. J. (2003). How do households cope with aggregate shocks? Evidence from the Mexican peso crisis. *World Development*, 31(7):1179–1199.
- Mincer, J. A. (1974). *Schooling, Experience, and Earnings*. Columbia University Press.
- Mishkin, F. S. (1999). Lessons from the Asian crisis. *Journal of International Money and Finance*, 18(4):709 – 723.
- Muûls, M. and Pisu, M. (2009). Imports and exports at the level of the firm: Evidence from Belgium. *World Economy*, 32(5):692–734.
- Newhouse, D., Alatas, V., and World Bank (2010). *Indonesia jobs report: Towards better jobs and security for all*. Jakarta: World Bank.
- Nguyen, H. C., Nordman, C. J., and Roubaud, F. (2013). Who suffers the penalty? A panel data analysis of earnings gaps in Vietnam. IZA Discussion Papers 7149, Institute for the Study of Labor.
- Oberfield, E. (2013). Productivity and misallocation during a crisis: Evidence from the Chilean crisis of 1982. *Review of Economic Dynamics*, 16(1):100–119.
- Oi, W. Y. and Idson, T. L. (1999). Chapter 33: Firm size and wages. In Ashenfelter, O. C. and Card, D., editors, *Handbook of Labor Economics*, volume 3, Part B of *Handbook of Labor Economics*, pages 2165 – 2214. Elsevier.
- Organisation for Economic Co-operation and Development (2011). *Southeast Asian Economic Outlook 2011/12*. OECD Publishing.

REFERENCES

- Pratap, S. and Quintin, E. (2006). Are labor markets segmented in developing countries? A semiparametric approach. *European Economic Review*, 50(7):1817–1841.
- Radelet, S. and Sachs, J. (1998). The onset of the East Asian financial crisis. Working Paper 6680, National Bureau of Economic Research.
- Rand, J. and Torm, N. (2012). The informal sector wage gap among Vietnamese micro-firms. *Journal of the Asia Pacific Economy*, 17(4):560–577.
- Roberts, M. J. and Tybout, J. R. (1997). The decision to export in Colombia: An empirical model of entry with sunk costs. *American Economic Review*, 87(4):545–64.
- Schady, N. R. (2004). Do macroeconomic crises always slow human capital accumulation? *World Bank Economic Review*, 18(2):131–154.
- Schaffner, J. A. (1998). Premiums to employment in larger establishments: Evidence from Peru. *Journal of Development Economics*, 55(1):81 – 113.
- Schor, A. (2004). Heterogeneous productivity response to tariff reduction: Evidence from Brazilian manufacturing firms. *Journal of Development Economics*, 75(2):373 – 396. 15th Inter American Seminar on Economics.
- Soderbom, M. and Teal, F. (2004). Size and efficiency in African manufacturing firms: Evidence from firm-level panel data. *Journal of Development Economics*, 73(1):369–394.
- Soderbom, M., Teal, F., and Harding, A. (2006). The determinants of survival among African manufacturing firms. *Economic Development and Cultural Change*, 54(3):533–55.
- Soderbom, M., Teal, F., and Wambugu, A. (2005). Unobserved heterogeneity and the relation between earnings and firm size: Evidence from two developing countries. *Economics Letters*, 87(2):153–159.
- Soesastro, H. and Basri, M. C. (1998). Survey of recent developments. *Bulletin of Indonesian Economic Studies*, 34(1):3–54.

REFERENCES

- Strauss, J., Beegle, K., Sikoki, B., Dwiyanto, A., Herawati, Y., and Witoelar, F. (2004). The third wave of the Indonesia Family Life survey (IFLS): Overview and field report. Technical report, RAND, Santa Monica, CA. WR-144/1-NIA/NICHD.
- Strauss, J., Witoelar, F., Sikoki, B., and Wattie, A. M. (2009). The fourth wave of the Indonesian Family Life Survey (IFLS4): Overview and field report. Technical report, RAND, Santa Monica, CA. WR-675/1-NIA/NICHD.
- Strobl, E. and Thornton, R. (2004). Do large employers pay more? The case of five developing African countries. *Journal of Economic Development*, 29(1):137 – 162.
- Sumarto, S., Suryahadi, A., and Widyanti, W. (2002). Designs and implementation of Indonesian social safety net programs. *The Developing Economies*, 40(1):3–31.
- Thomas, D., Beegle, K., and Frankenberg, E. (2000). Labour market transitions of men and women during an economic crisis: Evidence from Indonesia. Working Papers 00-11, RAND Corporation Publications Department.
- Thomas, D., Beegle, K., Frankenberg, E., Sikoki, B., Strauss, J., and Teruel, G. (2004). Education in a crisis. *Journal of Development Economics*, 74(1):53–85.
- Thomas, D. and Frankenberg, E. (2007). Household responses to the financial crisis in Indonesia: Longitudinal evidence on poverty, resources, and well-being. In *Globalization and Poverty*, NBER Chapters, pages 517–560. National Bureau of Economic Research, Inc.
- Thomas, D., Frankenberg, E., and Smith, J. P. (2001). Lost but not forgotten: Attrition and follow-up in the Indonesia Family Life Survey. *Journal of Human Resources*, 36(3):556–592.
- Thomas, D., Witoelar, F., Frankenberg, E., Sikoki, B., Strauss, J., Sumantri, C., and Suriastini, W. (2012). Cutting the costs of attrition: Results from the Indonesia Family Life Survey. *Journal of Development Economics*, 98(1):108–123.
- Topalova, P. and Khandelwal, A. (2011). Trade liberalization and firm productivity: The case of India. *The Review of Economics and Statistics*, 93(3):995–1009.

REFERENCES

- Tornell, A. and Westermann, F. (2003). Credit market imperfections in middle income countries. NBER Working Papers 9737, National Bureau of Economic Research, Inc.
- Varela, G., Aldaz-Carroll, E., and Iacovone, L. (2012). Determinants of market integration and price transmission in Indonesia. Policy Research Working Paper Series 6098, World Bank.
- Velenchik, A. D. (1997). Government intervention, efficiency wages, and the employer size wage effect in Zimbabwe. *Journal of Development Economics*, 53(2):305 – 338.
- Verhoogen, E. A. (2008). Trade, quality upgrading, and wage inequality in the Mexican manufacturing sector. *The Quarterly Journal of Economics*, 123(2):489–530.
- Vogel, A. and Wagner, J. (2010). Higher productivity in importing German manufacturing firms: Self-selection, learning from importing, or both? *Review of World Economics*, 145(4):641–665.
- Wade, R. (1998). The Asian debt-and-development crisis of 1997-?: Causes and consequences. *World Development*, 26(8):1535 – 1553.
- Wang, Z. and Yu, Z. (2012). Trading partners, traded products and firm performances of China’s exporter-importers: Does processing trade make a difference? *The World Economy*, 35(12):1795–1824.
- Widarti, D. (2010). Extending social security coverage to informal economy workers: Way forward. Working Paper, International Labour Organization.
- Wooldridge, J. M. (1995). Selection corrections for panel data models under conditional mean independence assumptions. *Journal of Econometrics*, 68(1):115–132.
- Wooldridge, J. M. (2002). *Econometric analysis of cross section and panel data*. MIT Press.
- World Bank (2000). *East Asia: Recovery and beyond*. Washington DC: World Bank.

REFERENCES

- World Bank (2013). *Doing Business 2013: Smarter Regulations for Small and Medium-Size Enterprises*. DOI: 10.1596/978-0-8213-9615-5. Washington, DC: World Bank Group.
- World Bank (2014). Gross domestic product 2014, PPP. Retrieved from <http://data.worldbank.org/data-catalog/GDP-PPP-based-table>.
- World Economic Outlook Database (2015). Gross Domestic Product, constant prices. Washington DC: International Monetary Fund. Retrieved from <https://www.imf.org/external/pubs/ft/weo/2015/01/weodata/index.aspx>.
- World Trade Organization (2009). *International Trade Statistics*. Geneva: World Trade Organization.
- World Trade Organization (2014). *International Trade Statistics*. Geneva: World Trade Organization.
- Yang, D. (2006). Why do migrants return to poor countries? Evidence from Philippine migrants' responses to exchange rate shocks. *The Review of Economics and Statistics*, 88(4):715–735.