Protecting Education for the Poor in Times of Crisis: An Evaluation of a Scholarship Program in Indonesia

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Abstract

This paper analyses the impact of an Indonesian scholarship program, which was implemented in 1998 to preserve access to education for the poor during the economic crisis. Scholarships were targeted pro-poor and the allocation process followed a decentralised design, involving both geographic and individual targeting. The identification strategy exploits this decentralised structure, relying on instrumental variables constructed from regional mis-targeting at the initial phase of allocation. The program has increased enrolment, especially for primary school aged children from poor rural households. Moreover, the scholarships seem to have assisted households in smoothing consumption during the crisis, relieving pressure on households' investments in education and utilisation of child labour.

JEL Classification: I28, J22, O15

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1 Introduction

In the fall of 1997 Indonesia was hit by a severe economic crisis, exacerbated by social and political turmoil in 1998. Up to the crisis, Indonesia had enjoyed a steady improvement in enrolment rates, reaching almost universal primary school enrolment (Lanjouw *et al.*, 2002). In an attempt to maintain these achievements, a combined scholarship and school subsidy program was introduced nationwide in August 1998, as part of a larger Social Safety Net intervention - *Jaring Pengaman Sosial* (JPS).[1]

Under the program almost 4 million scholarships were made available to primary and secondary schools students. The program followed a partly decentralised allocation process, involving both geographic and individual targeting. The size of the scholarship increased with enrolment level and amounted to about 7 to 18 percent of average per capita household consumption. The scholarships were monthly cash transfers, and students had full discretion on how to use the funds. This paper aims to evaluate the impact of the JPS scholarship program, and in particular the extent to which the program has been able to protect enrolment in education and reduce child labour for the poor during the first year of operation.

Protecting access to education for the poor in times of economic crisis is a primary policy concern in low-income countries, since investment in education is generally considered to be a key factor in reducing poverty (e.g., World Bank, 2001). These investments are compromised when households are faced with unexpected transitory income shocks, such as resulting from the economic crisis. Under typically incomplete financial markets, the investment decisions of households are bound by credit and resource constraints. Households' consumption smoothing strategies may then involve reducing investments in education or relying on child labour to smooth consumption (Jacoby and Skoufias, 1997).[2]

[1] See Ananta and Siregar (1999) for an overview of the JPS program.

[2] In the case of Indonesia, there is some empirical work that explicitly studies the role of human capital investment in household consumption smoothing strategies. Cameron and Worswick (2001)

Targeted scholarship programs can be cost-effective instruments for protecting investments in education for the poor, although their effectiveness critically depends on the ability to identify those most vulnerable to the crisis. There are several studies that provide evidence that school subsidy programs are indeed effective in increasing school participation and reducing child labour.[3] This paper evaluates the effects of such a demand side intervention within the context of an economic crisis. The paper looks at the impact on both enrolment status of children and the actual activities of students, i.e. school attendance and work.

The scholarships can affect school attendance or work activities, even without having an observable effect on enrolment. School attendance and child work are not mutually exclusive or perfect substitutes.[4] Priyambada, Suryahadi and Sumarto (2002), find that in Indonesia schooling and part time work often go together. Although the declining trend in child labour, observed during the past 3 decades, has come to a halt with the onset of the crisis, they find that working does not exclude children from attending school. They even find evidence that students from severely poor families seek employment to finance their own education. There is a growing number of empirical studies that investigate the simultaneous nature of labour and schooling decisions[5]. This paper adds to this work, by estimating the impact of the JPS scholarships on the joint decision of school attendance and child labour.

To deal with the non-random allocation of scholarships, the identification strategy exploits the decentralised targeting design of the program. In principle, the scholarships were targeted pro-poor, at both the individual and the district level. However, due to

find evidence of consumption smoothing through reduced education expenditures (especially for girls) amongst rural households as a reaction to crop loss. Fitzsimons (2003) finds for small Indonesian villages that enrolment is mainly affected by aggregate instead of idiosyncratic risk.

[3]E.g., Ravallion and Wodon (2000), Rawlings and Rubio (2003), Maluccio and Flores (2004), Schultz (2004), Behrman *et al.* (2005) and Parker *et al.* (2005).

[4]With regard to school subsidy programs, Ravallion and Wodon (2000) and Schultz (2004) find that the positive effects on schooling are only partly explained by reduced labour activities.

[5]See, amongst others, Canagarajah and Coulombe (1997), Maitra and Ray (2002), and Rosati and Rossi (2003).

the heterogeneous nature of the crisis across districts, only incomplete information on regional poverty was available to policy makers. For the first year of the program, geographic allocation was therefore based on outdated pre-crisis poverty estimates from 1996. The lack of reliable data at the initial phase of allocation caused some degree of unintended mis-targeting to districts. This exogenous variation in the targeting process is used to identify the treatment effects. Instrumental variables are constructed from the initial selection rule and ex-post information on the poverty profile. The availability of pre-intervention data makes it possible to assess the validity of regional mis-targeting as instrument.

The program appears to have been successful in returning enrolment to pre-crisis levels, especially for children of primary school age from poor rural households. The scholarships also enticed households to reallocate a child's time from work to school. However, in contrast to other studies, labour activities of enrolled students show to be more sensitive to scholarships than school attendance. The results emphasise the relationship between transitory income shocks and households' investment in human capital. The scholarships were most effective for children whose education was especially vulnerable to consumption smoothing during the crisis.

The paper is structured as follows. The next section describes the data. Section 3 gives an account of the economic crisis and trends in education outcomes. The targeting design and allocation rules of the JPS program are outlined in section 4. Section 5 deals with identification and estimation of the program's impact, and section 6 concludes.

2 The data

The main source of data for this analysis is Indonesia's socioeconomic household survey (*Susenas*), which is conducted annually on a national scale. The survey collects information on education attainment, socioeconomic background, and detailed information

on household expenditures. Besides school enrolment the Susenas survey also collects information on the activities of children in the previous week. Children aged 10 and older are asked about school attendance, labour and other activities. In 1999 a special module was included concerning participation in the JPS programs. The Susenas surveys are fielded in February, which means the JPS module only covers the first 6 months of the program. The 1999 survey includes 205,747 households and 864,580 individuals. The 1998 survey was fielded about 6 months prior to the JPS program, and includes 207,645 households and 880,040 individuals. It collects the same information as the 1999 survey, except for the JPS data. The Susenas is representative at the district level (*kabupaten* and *kota*). The 1998 and 1999 cross section data can be used to construct a pseudo-panel of two waves for 294 districts.[6]

Another source of data is a 1996 village census (*Podes*). It contains, amongst others, information on availability of schools in 66,486 villages (*desa*) and urban precincts (*kelurahan*) in Indonesia, and can be merged with the Susenas data. Finally, I use administrative data for the JPS selection criteria and budget allocation for districts, documented in the 1998 *Program Implementation Plan* (Ministry of Education, 1998).

3 The economic crisis and investments in education

3.1 The crisis

By 1998, the effects of the crisis were felt all over Indonesia. According to official estimates the poverty headcount increased from 17.7 in 1996 to 23.5 in 1999. Alternative estimates of poverty during the crisis abound, unambiguously showing a daunting increase in poverty. Suryahadi *et al.* (2003) trace the path of poverty from 1996 to 1999 and find that, after a period of steady decline, the poverty headcount has more than

[6]The districts of East Timor are not included in the analysis due to incomplete data.

doubled during the crisis.[7] There was considerable heterogeneity across regions. Urban areas seem to have been hit harder than rural areas, with Java (the most populous island of the archipelago) experiencing the greatest difficulties (Sumarto, *et al.*, 1998). While per capita income declined, prices soared. 1998 saw an annual increase in the consumer price index of 78 percent, whilst the price of food doubled. There is little evidence of rising overall unemployment. Instead, real wages dropped by about 40 percent in the formal sector during the first year of the crisis, and agriculture seems to have absorbed part of the displaced labour from other sectors. (Cameron, 1999).

There is some evidence that expenses on education were reduced to smooth consumption during the crisis. Frankenberg *et al.* (2003) find that household consumption declined by 20 percent in 1998, with investment in health and education decreasing by 37 percent. Spending on education declined in particular for the rural poor (Thomas *et al.*, 2004). On average education expenditure per enrolled household member decreased by 19 percent from 1997 to 1999, amongst rural households. Interestingly, households seem to have protected education of the older children at the expense of their younger siblings. An explanation is that expected returns to higher education are larger than for basic education in Indonesia (Behrman and Deolalikar, 1995), and that households have already invested in secondary education of older children.

3.2 Enrolment, school attendance and child labour

At a first glance enrolment seems to have suffered from the crisis, but only for a short period. Table 1 shows that primary and junior secondary school enrolment rates were increasing up to 1997. In 1998 this increase stagnated, with net enrolment decreasing slightly from 92.3 to 92.1 and from 57.8 to 57.1 percent for primary and junior secondary school, respectively. Despite the severity of the crisis, a large scale drop out was not observed. Jones and Hagul (2001) discuss field evidence of strong community support

[7] They estimate that from February 1997 to the height of the crisis, late 1998, the poverty headcount increased from 15.3 to 33.2 percent.

and commitment of schools to maintain enrolment levels during the first year of the crisis. The following year, when the JPS program had been initiated, enrolment picked up, exceeding pre-crisis levels. Senior secondary enrolment increased throughout this period, even in 1998. A similar pattern is seen for total enrolment per age group.

Table 1: Enrolment rates, by education level and age group, 1995 to 1999

		1995	1997	1998	1999
Net enrolment	Primary	91.5	92.3	92.1	92.6
		[0.12]	[0.11]	[0.11]	[0.13]
	Junior secondary	51.0	57.8	57.1	59.2
		[0.33]	[0.32]	[0.32]	[0.37]
	Senior secondary	32.6	36.6	37.5	38.5
		[0.35]	[0.36]	[0.33]	[0.39]
Age group	10 to 12	95.2	96.2	95.9	96.2
		[0.11]	[0.11]	[0.11]	[0.12]
	13 to 15	73.2	77.5	77.3	79.0
		[0.30]	[0.28]	[0.28]	[0.30]
	16 to 18	43.9	47.9	48.7	50.4
		[0.38]	[0.37]	[0.35]	[0.38]
	10 to 18	72.5	74.6	74.5	75.2
		[0.20]	[0.19]	[0.19]	[0.20]

Standard errors in square brackets are adjusted for clustering in survey design.

To a large extent the increase in enrolment in 1999 has been attributed to the JPS program, mainly on the grounds that the program has been fairly successful in targeting the poor (Jones and Hagul, 2001). However, a comprehensive impact evaluation has not been carried out yet. Cameron (2002) does find a positive effect on junior secondary enrolment, using a dataset concerning 100 predominantly poor villages.

Being enrolled does not automatically mean that students actually go to school. Enrolment takes place in August and typically requires sunk costs such as a one-time enrolment fee and costs for school uniforms and books. Variable schooling costs include transportation costs and monthly tuition fees. For consumption smoothing reasons, it could be that enrolled children may not attend school because of these variable costs of schooling. Alternatively, they may decide to work and reduce time spent at school.

Table 2: School attendance in previous week amongst enrolled children (percentage), by age group in 1999

Age group	JPS	Non-JPS	Working	Not working	All
10 to 12	97.8	98.4	94.1	98.5	98.4
	[0.34]	[0.11]	[0.83]	[0.10]	[0.10]
13 to 15	97.5	98.2	92.2	98.6	98.2
	[0.34]	[0.11]	[0.67]	[0.10]	[0.11]
16 to 18	97.4	97.9	92.0	98.5	97.9
	[0.61]	[0.13]	[0.71]	[0.11]	[0.13]
10 to 18	97.6	98.2	92.6	98.6	98.2
	[0.25]	[0.09]	[0.49]	[0.08]	[0.09]
N	8,503	111,519	8,505	111,517	120,022

Standard errors in square brackets are adjusted for clustering in survey design.

Table 2 looks at school attendance in the past week for enrolled students, in 1999.[8] School attendance is fairly high for all age groups, varying around 98 percent. However, program participants have a slightly lower attendance rate than non-participants. Working does not prevent children from attending school, but enrolled children that work are more often absent from school. Working is here defined as activities that contribute to household income, for at least one hour in the last week. This may include wage labour, but also non wage labour such as own farm activities.

Table 3: Labour activities in previous week (percentage), by age group in 1999

Age group	Enrolled - JPS	Enrolled - no JPS	Enrolled	Not enrolled	All
10 to 12	6.5	2.8	3.0	20.4	3.7
	[0.58]	[0.11]	[0.12]	[1.08]	[0.13]
13 to 15	11.8	6.6	7.1	38.5	13.7
	[0.72]	[0.19]	[0.20]	[0.68]	[0.24]
16 to 18	14.8	8.8	9.0	52.2	30.2
	[1.27]	[0.27]	[0.27]	[0.49]	[0.35]
10 to 18	10.2	5.5	5.8	46.6	15.9
	[0.53]	[0.13]	[0.14]	[0.43]	[0.19]
N	8,503	111,519	120,022	40,018	160,040

Standard errors in square brackets are adjusted for clustering in survey design.

Table 3 depicts labour activities for scholarship recipients, enrolled children without

[8] This refers to the full week prior to enumeration, which took place in February, one month into the second semester of the 1999/2000 school year.

a scholarship and non-enrolled children. Enrolled children without a scholarship are less likely to work than those with a scholarship. Scholarship recipients work, on average, twice as much as non recipients (10.2 and 5.5 percent, respectively). Labour activity is highest for non-enrolled children. 46.6 percent of non-enrolled children aged 10 to 18 work at least one hour a week.

4 The JPS education program

4.1 Program design and allocation criteria

The JPS scholarship program was implemented at the start of the 1998/1999 academic year. It was to run for 5 years, financed by the World Bank, the Asian Development Bank and the Government of Indonesia. For the first year the costs amounted to US $ 114 million. The main objective of the program was to keep enrolment rates for primary and secondary education at pre-crisis levels (Ministry of Education, 1998). The program aimed to reach 6 percent of enrolled students at primary schools, 17 percent at junior secondary schools, and 10 percent at senior secondary schools. Schools received block grants from an operational assistance fund - *Dana Bantuan Operasional* (DBO) - to maintain quality of education during the crisis.

The size of the scholarships increases with the enrolment level. The scholarships amounted to Rp. 10,000 per month for students in primary school, Rp. 20,000 for junior secondary school, and Rp. 25,000 in senior secondary school. To put these numbers into perspective, average monthly per capita expenditure reported in the 1999 Susenas was Rp. 131,465, while households representing the poorest 20 percent of the population spent Rp. 62,417 per capita per month. For the 1997/1998 school year, monthly expenditures on education per student from the poorest quintile were Rp. 4,881, Rp. 16,123 and Rp. 30,401 (in February 1999 prices) for primary, junior

secondary and senior secondary, respectively.[9] Thus, for the poorest households the scholarships are quite significant contributions to monthly income and cover a large part of the expenditures on education.

Through the decentralised design of the program, scholarships were allocated in three phases. First, the funds were allocated to districts, based on the level of poverty. At the time of implementation there was no accurate information available on the crisis impact. Therefore a poverty index ($JPS96$) was constructed based on pre-crisis information on regional poverty, from the 1996 Susenas consumption module. Poor districts were allocated relatively more scholarships, proportional to the number of enrolled students.

At the district level committees were formed to allocate scholarships to schools. This allocation was based on a prosperity measure for the village or sub-district (*kecamatan*) served by the school, the percentage of IDT eligible villages in the area [10], and the average school fees paid by students. The prosperity measure was provided by the National Family Planning Coordinating Agency - *Badan Koordinasi Keluarga Berencana Nasional* (BKKBN).[11] Both private and public schools were eligible. The district committees were allowed to define additional criteria if they felt this would better reflect local conditions.

Finally, school committees selected students for the program. The committees received guidelines on which allocation criteria to consider. These included the BKKBN prosperity status, single parent and large households, and travel distance from home to school. Another aim was to allocate at least half of the scholarships to girls. Stu-

[9] Based on data from an education expenditure module to the 1998 Susenas.

[10] IDT refers to the *Inpres Desa Tertinggal* program, an anti-poverty program for economically less developed villages.

[11] The prosperity measure is based on the so-called *prosperity status* of households. Under this definition a household classifies as poor if it fails at least one of the following 5 basic needs criteria: (i) households can worship according to faith, (ii) eat basic food twice a day, (iii) have different clothing for school/work and home/leisure activities, (iv) have a floor that is made out of something other than earth, and (v) have access to modern medical care for children or access to modern contraceptive methods. The BKKBN regularly collects this information on a census basis.

dents in primary school grades 1 to 3 were not eligible. School committees could also select children that had already dropped out of school due to the crisis. Continuation of scholarships was conditional on enrolment and passing the grade at the end of the school year. However, no formal conditions were placed on school attendance or how the funds had to be spent. A distinctive element of the scholarship and block grants program is the funding mechanism itself. The scholarships and grants were transferred directly to local post offices, where the intended beneficiaries could collect the funds.

4.2 Distribution of scholarships

By February 1999, at the time that the 1999 Susenas survey was administered, the JPS scholarship program had not yet reached its intended targets. Table 4 shows the allocation of scholarships to enrolled students, by enrolment level. The coverage of enrolled students was 4.0 percent, 8.4 percent and 3.7 percent for the respective enrolment levels. Overall, 5.0 percent of all students in primary and secondary school were covered. Table 4 also shows how the JPS program dwarfs all other scholarship programs, as it covers about 83 percent of all scholarships.

Table 4: Scholarships allocated in February 1999 (percent of enrolled students)

Type	Primary	Junior secondary	Senior secondary	All
Government JPS	4.01	8.42	3.71	4.96
Goverment Non-JPS	0.22	0.76	0.62	0.39
Foster Parents Movements	0.28	0.39	0.19	0.29
Private sector	0.08	0.25	0.27	0.15
Other	0.12	0.23	0.22	0.16
Total	4.71	10.05	5.02	5.95
N	122,143	41,367	25,522	189,032

The scholarships have been distributed pro-poor, but with considerable leakage to students from wealthier households. 62.6 percent of the scholarships are allocated to students from the poorest 40 percent of the population, while the students from wealthiest 20 percent hold 5.7 percent of the scholarships. Table 5 shows marked differences

Table 5: Distribution of JPS scholarships, by per capita expenditure quintile (column percentages)

Quintile	Primary	Junior secondary	Senior secondary	All
1 (poorest)	43.84	29.53	18.48	35.77
2	26.85	27.23	25.21	26.83
3	17.27	21.63	23.18	19.54
4	8.82	14.70	19.39	12.16
5 (richest)	3.21	6.91	13.74	5.71
N	5,204	3,712	996	9,912

between enrolment levels. Scholarship distribution to primary school students is more pro-poor than the overall distribution. Students from the two poorest quintiles hold 70.7 percent of the scholarships, while 3.2 percent went to the richest quintile. Allocation of scholarships to junior secondary school is also pro-poor, but slightly less than the overall allocation. In contrast, allocation at senior secondary level is not pro-poor at all.

5 The impact of the scholarship program

5.1 Regional mis-targeting

The foremost and obvious problem for measuring the effect of the program is that the scholarships were not assigned randomly, but have been targeted to students from poor households instead. In the absence of the scholarship program, enrolment and school attendance is expected to be lower for scholarship recipients, given that they come from, on average, poorer households than non-recipients. For the same reason the probability of working is expected to be higher. Consequently, children without a scholarship do not form a suitable control group for children that are selected for the program. To deal with this non-random placement of scholarships the identification strategy relies on instrumental variables. The decentralised targeting design can provide a source of exogenous variation that affects the probability of receiving a scholarship, but is

independent of the potential outcomes.

With regard to the JPS program, the endogeneity has its source with both geographic and individual targeting. Ravallion and Wodon (2000) exploit the decentralised nature of the allocation process to find a valid instrument. They argue that partial decentralisation creates *geographic separability*, where the probability of selection into the program is conditional on geographic allocation, and independent between areas.[12] Under the assumption of geographic separability, exogenous variation in geographic targeting can be used to identify the effect of the program. It may be easier to find an instrument at district level than at individual level since the dimensions of the targeting process (and possible unobservables) are smaller.

Targeting of scholarships to districts was based on just two criteria: the 1996 poverty estimate ($JPS96$) and the number of enrolled students in the district. A district level regression shows that the $JPS96$ measure and the 1998 enrolment rates explain 69 percent of the variation in the fraction of scholarships recipients across districts. There is a strong positive correlation between the $JPS96$ measure and scholarship coverage, but it doesn't fully explain actual allocation. There can be several reasons for actual allocation in districts to deviate from the targeting rule. First, the timing of the program and the Susenas may introduce unobserved variables that affect allocation. Remember that at the time of the survey, February 1999, not all the targets had been met yet, and this delay in implementation varied across districts. Moreover, there may be differences in the effectiveness and efficiency of the allocation systems between districts.

Given the heterogeneous nature of the crisis, it is likely that the pre-crisis indicator $JPS96$ misjudged the degree of poverty in, and variation across, districts. There are two main reasons for this. The effect of the crisis varied strongly between regions and was only weakly correlated with the initial level of poverty (Sumarto, Wetterberg and Pritchett, 1998). The crisis has also given rise to large relative price changes,

[12] This assumes no inter-district migration due to the program.

between products (especially food) and across regions (Cameron, 1999; Frankenberg *et al.*, 2003; Friedman and Levinsohn, 2002). This variation is ignored in the $JPS96$ poverty measure.

Mis-targeting to districts can provide the exogenous variation needed to identify the effect of the scholarship program. To put the argument more formally, decompose the $JPS96$ measure into two components

$$JPS96_j = \psi' V_j + z_j \tag{1}$$

where V_j reflects the actual poverty profile in 1998 for district j. The *mis-targeting* term, z_j, is a non-systematic judgement error in the targeting process. It reflects the inability to capture the extent of poverty during the crisis due to the lack of information on the actual situation in 1998.

With the belated availability of data on the regional poverty profile in 1998, z_j can be estimated by taking the residual of the regression $E[JPS96_j \mid V_j]$. If conditioning on V_j indeed purges $JPS96_j$ of all systematic variation then $\hat{z}_j$ would be a suitable instrument. For example, if $JPS96_j$ overestimates the actual degree of poverty in 1998 for district j, then $z_j > 0$. Given sufficient available information on poverty profile V_j, the estimated overestimation $\hat{z}_j$ should be independent of enrolment, school attendance and child labour supply in that district.

With a strategy like this there remains the danger that V_j is not fully observable, in which case the omitted variables will cause $\hat{z}_j$ to contain some poverty related variation. One way to evaluate the credibility of the identification strategy is to test whether $\hat{z}_j$ is correlated with the pre-intervention outcomes, using data from Susenas 1998. The identifying assumption for estimating the impact of the program is that if the exclusion restriction is valid for 1998, it also is for 1999. This seems a reasonable assumption since $JPS96$ is based on historic poverty estimates.

Table 6 shows the results from district level regressions of 1998 enrolment, school

attendance and child labour rates for children age 10 to 18, on $\hat{z}_j$ and $JPS96_j$, respectively. The regional poverty profile V_j includes the 1998 headcount (P_0) and the poverty gap (P_1) for each district.[13] An alternative poverty headcount estimate for 1996 ($BPS96$), released by the Indonesian Bureau of Statistics in 2000, is included to capture the impact of the crisis.[14] The results suggest that given the specification of V_j, the exclusion restriction is justified. Enrolment and labour are strongly correlated with $JPS96_j$, while school attendance is not (columns 2-4). Districts that the JPS program regarded as relatively poor, experience lower enrolment and higher incidence of child labour. However, the mis-targeting residual $\hat{z}_j$ shows no correlation with the outcome variables, as the coefficients are small and statistically not significant (columns 5-7).[15]

Table 6: Relation between pre-intervention outcomes and geographic (mis-) targeting

Outcome	$JPS96_j$	[s.e.]	R^2	$\hat{z}_j$	[s.e.]	R^2
Enrolment rate 1998	-0.342	[0.065]**	0.0870	-0.083	[0.087]	0.0031
Shool attendance rate 1998	-0.017	[0.015]	0.0043	-0.004	[0.019]	0.0001
Child labour rate 1998	0.188	[0.040]**	0.0707	0.046	[0.062]	0.0019

Significance levels: † : 10% * : 5% ** : 1%

Note: OLS estimates. Outcome variables are district means, from Susenas 1998. N = 294.

5.2 The effect on enrolment

5.2.1 Estimation

The overall effect of the JPS scholarships on enrolment is estimated at the district level, by explaining regional variation in the enrolment rate by the variation in the size of the program across districts.[16] For each district j the enrolment rate in year t is modelled

[13] P_0 and P_1 are estimated based on per capita household expenditure from Susenas. The povertylines are set such that the average head count for Indonesia is 24.1% in February 1998 and 27.1% in February 1999 (Suryahadi, Sumarto and Pritchett, 2003).

[14] The estimates and methodology are reported in BPS (2000).

[15] Note that for enrolment and attendance it suffices to just condition on P_0 and P_1 to remove systematic variation from in $\hat{z}_j$.

[16] This cannot be analysed at the individual level since we do not observe children that receive a scholarship, but are no longer enrolled. Therefore, there is no variation in treatment assignment T_i for non-enrolled students. Ideally, I would like to have information on students histories of receiving

as a linear function of the intensity of the scholarship program

$$s_{jt} = \alpha_j + \left(\tau + \eta_j\right) \overline{T}_{jt} + \phi' W_{jt} + \theta_0 d_t + \sum_{r=2}^{5} \theta_r d_r d_t + \varepsilon_{jt} \tag{2}$$

where s_{jt} is the enrolment rate for a specific age group or enrolment level in district j, as reported in table 1. $\overline{T}_{jt}$ is the fraction of children that received a scholarship in that group. Thus, both s_{jt} and $\overline{T}_{jt}$ refer to the same potential JPS target group of school age children. The average effect of the program is defined as $E\left(\tau_j\right) = \tau$, where τ_j is the idiosyncratic effect for each district. Effect heterogeneity is then reflected by $\eta_j = \tau_j - \tau$. This is the average deviation from τ in a specific district, with $E\left(\eta_j\right) = 0$. The total number of children (for the specific group) in district j is denoted by N_j. Time is indicated by subscript t, which is either 1998 (pre-intervention) or 1999 (post-intervention). In 1998 no JPS scholarships have been allocated, thus $\overline{T}_{j1998} = 0$ for all j. W_j is a set of control variables that capture labour market, welfare and demographic characteristics in the districts. The time dummy variable d_t takes value 1 if $t = 1999$ and 0 if $t = 1998$. Some flexibility is given to capturing the time trend by interacting time variable d_t with region specific fixed effects, d_r.[17] α_j is a time invariant fixed effect. This accounts for all endogeneity that has its source with non-random placement based on district specific time invariant variables. The bias due to targeting of poorer districts (using the historic $JPS96$ measure) is thereby removed, as well as any bias due to time invariant unobservables.

Taking first differences of (2) yields

$$\Delta s_j = \left(\tau + \eta_j\right) \overline{T}_j + \phi' \Delta W_j + \theta_0 + \sum_{r=2}^{5} \theta_r d_r + \Delta \varepsilon_j \tag{3}$$

scholarships, but, unfortunately, the Susenas does not contain these data. But even if these had been available, it would be likely to find very few scholarship recipients to drop out of school so early into the program, providing very little variation in the outcome variable for recipients.

[17] The 5 regions are (i) Java and Bali, (ii) Sumatra, (iii) Sulawesi, (iv) Kalimantan and (v) Other Islands. Java and Bali serve as the reference group.

OLS will give unbiased estimates for (3) under two assumptions. First, the time trend is assumed to be constant within the five regions. This assumption is violated if there is any geographical variation in the change of the average economic conditions that is not captured by the time dummies or ΔW_j. For example, the crisis may have systematically different effects for rich districts than for less wealthy districts, within the regions. The second assumption is that there are no time varying unobservables that are in any way correlated with the allocation process. If either of these two assumptions does not hold then $\overline{T}_j$ will be correlated with $\Delta\varepsilon_j$. In this case the bias can be removed by IV estimation using $\hat{z}_j$ as instrument.[18]

Interpretation of the estimates depends on assumptions regarding the expected effect heterogeneity. Unobserved effect heterogeneity requires strong assumptions if IV estimates are to be interpreted as average treatment effects (Heckman, 1997). For example, it would be sufficient to assume that regional allocation is independent of the unobserved effect heterogeneity, $E[\eta_j \mid \overline{T}_j] = 0$.[19] In this case both IV and OLS identify the average treatment effect, $E[\hat{\tau}_{ATE}] = \tau$. This seems to be a reasonable assumption, since geographic targeting was not based on the expected average gains within districts. However, actual allocation $\overline{T}_j$ depends on 1996 poverty estimates and the speed of program implementation per district. If these are correlated with the heterogeneous effect of the program then $E[\eta_j \mid \overline{T}_j] \neq 0$, even if this was not known a priori to program managers. In this case OLS will retrieve the average treatment effect on the treated, $E[\hat{\tau}_{ATT}] = \tau + E[\eta_j \mid \overline{T}_j]$. This captures the fraction of the actual program participants that would have dropped out of school if they had not received a scholarship. IV, on the other hand, will identify the *local* average treatment effect,

[18] An additional source of bias is measurement error in the Susenas data, since the JPS module records scholarship information only for enrolled students. The extent of this bias depends on the number of scholarship recipients dropping out of school in the first months of the program. While this bias is not likely to be large (e.g. footnote 16), it cannot be ignored when interpreting the OLS estimates.

[19] Note that $E[\Delta s_j] = \tau\overline{T}_j + \phi'\Delta W_j + \theta_0 + \sum \theta_r d_r + e_j$, where the unobserved $e_j = E[\eta_j T_j] + E[\Delta\varepsilon_j]$. Since z_j is correlated with $\overline{T}_j$ by assumption, z_j is correlated with e_j if $E[\eta_j \mid \overline{T}_j] \neq 0$, even if the instrument is not correlated with the outcome variable, $E[\Delta\varepsilon_j \mid \overline{T}_j, \Delta W_j] = E[\Delta\varepsilon_j \mid \Delta W_j] = 0$.

$E[\hat{\tau}_{LATE}] = \tau + E[\eta_j \mid \overline{T}_j(\hat{z}'_j) > \overline{T}_j(\hat{z}''_j)]$.[20] This is the average effect for those districts where allocation $\overline{T}_j$ is affected by $\hat{z}_j$ (Imbens and Angrist, 1994).

The overall effect of the program on the enrolment rate is given by a population weighted average of the effects for the districts

$$E(s^1_{1999}) - E(s^0_{1999}) = \hat{\tau} \sum_{j=1}^{J} \frac{N_j}{N} \frac{1}{N_j} \sum_{i=1}^{N_j} T_{ij} = \hat{\tau}\overline{T} \tag{4}$$

where $\overline{T}$ is the fraction of the relevant (subset of the) population that has received a scholarship, and J the number of districts. s^1_{1999} is the actual enrolment rate that we observe in 1999 with the program in place. The counterfactual s^0_{1999} is the enrolment rate that would have been if the program was not implemented.

5.2.2 Results

Table 7 shows the OLS and IV impact estimates for equation (3) and the overall effect on the enrolment rate, $\hat{\tau}\overline{T}$ (equation (4)), for different three age groups.[21]. The tables also report $\overline{T}$. The welfare variables, W_j, include the share of rural population, average age and household size, and poverty indictors P_0 and P_1 in the districts. The coefficients for the covariates are omitted from the table for convenience. The number of observations is 294. The first stage coefficient for the instrument (denoted by $\hat{\delta}_z$) is positive and strongly significant in all regressions. Over-estimation of poverty increases the intensity of the program in a district.

There is a significant effect of the program on enrolment. The IV estimates of the program are larger and more precise than the OLS estimates. This suggest some correlation between $\overline{T}_j$ and $\Delta\varepsilon_j$. The most likely explanation would seem to be a non-

[20] LATE imposes a monotonicity assumption. Let $\overline{T}_j(\hat{z}'_j)$ be $\overline{T}_j$ given $\hat{z}_j = \hat{z}'_j$. Monotonicity requires that for $\hat{z}'_j$ and $\hat{z}''_j$, in the support of $\hat{z}_j$, it must hold that either $\overline{T}(\hat{z}'_j) \leq \overline{T}(\hat{z}''_j)$ or $\overline{T}(\hat{z}'_j) \geq \overline{T}(\hat{z}''_j)$ for all j. Intuitively, this would imply that when the degree of poverty-overestimation ($\hat{z}_j$) in a district increases this will never decrease the probability of receiving a scholarship for any child in that district.

[21] Weights are applied to take account of the underlying number of observations used for calculating district means.

Table 7: Effect of the JPS scholarships on enrolment (equations (3) and (4))

Age group	$\hat{\tau}$	[s.e.]	$\hat{\tau}\overline{T}$	$\overline{T}$	$\hat{\delta}_z$	[s.e.]	N
OLS							
10 to 12	0.076	[0.026]**	0.0044	0.058			294
13 to 15	0.037	[0.051]	0.0025	0.068			294
16 to 18	0.046	[0.138]	0.0011	0.024			294
10 to 18	0.053	[0.048]	0.0027	0.050			294
IV							
10 to 12	0.100	[0.035]**	0.0058	0.058	0.772	[0.043]**	294
13 to 15	0.117	[0.074]	0.0079	0.068	0.801	[0.050]**	294
16 to 18	−0.002	[0.236]	-0.0000	0.024	0.305	[0.025]**	294
10 to 18	0.126	[0.065]†	0.0063	0.050	0.642	[0.035]**	294

Significance levels: † : 10% ∗ : 5% ∗∗ : 1%

constant time trend due to regional variation in the crisis effect.[22] According to the IV estimates, 13 percent of program participants would have dropped out of school if they had not received a scholarship. The effect for children aged 10 to 12 is 10 percent. This is an important result. These students are likely to be in the higher grades of primary school. In absence of the program they would have dropped out of school just prior to finishing primary education. For children between ages 13 and 15 it is slightly higher, at 12 percent, but this estimate is not precise. For the age group 16-18 there is no significant effect on enrolment.

The effects for different groups in the population are given in table 8. The table shows the estimates by per capita consumption group, gender and rural/urban area. Three per capita consumption groups are defined: the 1st-25th percentile (i.e., the poorest quarter of the population), 25th-50th percentile and the 50th-100th percentile. The poorest quartile roughly represents the population that lives of a consumption level below the poverty line.[23]

[22]It could also be that IV retrieves a LATE that differs strongly from ATT or ATE. For example, if some districts experience delays in program implementation. In 7 out of 294 districts used in estimation, no children reported to have received a scholarship yet ($\overline{T}_j = 0$). Using the terminology of Angrist, Imbens and Rubin (1996), these districts can be thought of as *never takers*. Neither the LATE nor ATT reflect the effects for these districts. However, the estimates are not sensitive to including a dummy variable that indicates the 7 never takers.

[23]Due to the number of observations in the intervention group, the analysis had to be restricted to

Table 8: Effect of the JPS scholarships on enrolment, by per capita consumption, gender and urban/rural (IV estimates for equation (3))

Age group	Sub group	OLS		IV		
		$\hat{\tau}$	[s.e.]	$\hat{\tau}$	[s.e.]	N
10 to 12						
	1-25 percentile	0.063	[0.042]	0.122	[0.059]*	293
	25-50 percentile	0.081	[0.042]†	0.043	[0.066]	294
	50-100 percentile	−0.015	[0.039]	−0.021	[0.064]	294
	male	0.087	[0.031]**	0.098	[0.045]*	294
	female	0.068	[0.033]*	0.109	[0.046]*	294
	urban	0.064	[0.032]*	0.055	[0.060]	287
	rural	0.046	[0.031]	0.105	[0.042]*	277
13 to 15						
	1-25 percentile	0.208	[0.073]**	0.201	[0.108]†	293
	25-50 percentile	−0.077	[0.083]	−0.023	[0.129]	294
	50-100 percentile	−0.009	[0.080]	0.074	[0.143]	294
	male	0.111	[0.063]†	0.163	[0.096]†	294
	female	0.042	[0.059]	0.067	[0.090]	294
	urban	0.055	[0.064]	0.084	[0.111]	287
	rural	0.035	[0.055]	0.134	[0.081]†	277
16 to 18						
	1-25 percentile	0.158	[0.186]	−0.076	[0.396]	291
	25-50 percentile	0.007	[0.208]	−0.291	[0.443]	294
	50-100 percentile	0.023	[0.179]	−0.141	[0.340]	294
	male	0.119	[0.168]	−0.098	[0.293]	294
	female	−0.040	[0.159]	0.088	[0.317]	294
	urban	0.026	[0.146]	−0.115	[0.319]	287
	rural	0.127	[0.154]	0.108	[0.266]	277
10 to 18						
	1-25 percentile	0.109	[0.059]†	0.161	[0.080]*	293
	25-50 percentile	−0.024	[0.080]	0.049	[0.113]	294
	50-100 percentile	−0.085	[0.086]	0.011	[0.132]	294
	male	0.117	[0.057]*	0.151	[0.078]†	294
	female	−0.010	[0.057]	0.100	[0.079]	294
	urban	−0.010	[0.069]	0.046	[0.113]	287
	rural	0.051	[0.050]	0.146	[0.068]*	277

Significance levels: † : 10% ∗ : 5% ∗∗ : 1%

The results show a strong heterogeneous pattern, and suggest that the program was most effective for those most vulnerable to the crisis. The largest effects are found for children aged 10 to 12 from rural areas who live below the poverty line. This is exactly the group for which investment in education was most affected by households' consumption smoothing during the crisis (Thomas *et al.*, 2004). A similar pattern is found for the 13-15 age group, although the estimates are less precise. Overall, the effect of the scholarships seem to favour boys over girls. For 10-12 year olds the effects are fairly similar for boys and girls, but for children aged 13-15 the scholarships are more effective for boys. For the oldest age group there is no statistically significant effect for any of the population groups, indicating that the absence of an overall effect for this group is not only due to bad targeting, but that enrolment is also less sensitive to income shocks.

What would have been the trend in overall enrolment if the JPS scholarship program had not been implemented? The overall increase of the enrolment rate due to the program ($\hat{\tau}\overline{T}$) for 10 to 18 year olds is 0.6 percentage point. The trend in the enrolment rate from 1997 to 1999 (table 1) shows a slight decrease in 1998 and then a 0.7 percentage point increase a year later. The estimated effect suggests that in the absence of the program, enrolment would have remained unchanged from 1998 to 1999. Moreover, the JPS has pushed overall enrolment above the pre-crisis level. For children aged 10-12 enrolment decreased by 0.3 percentage point in 1998, and returned to it's pre-crisis level in 1999. The program increased the enrolment rate by 0.6 percentage point. This means that if the program had not been implemented, enrolment for this age group would have decreased further in 1999. For the age group 13-15 the increase in enrolment from 1998 to 1999 is 1.7 percentage point, of which about half (0.8 percentage point) is due to the JPS program.

these three per capita consumption groups. A breakdown by quintile posed problematic, especially for the non-poor, as the means were based on too few observations, leaving very little variation in the treatment variable.

The JPS program also included budgetary support to schools. If these grants affected enrolment then the estimates above measure the confounding effect of both components of the program. This is tested by adding a variable with per capita DBO transfers per district as a regressor.[24] There is no evidence that the DBO transfers interfere with the estimation of the treatment effects. Enrolment is not affected by per capita DBO allocation to districts, and the estimated effects of the scholarships change little (slightly larger) with this specification.

5.3 The effect on school attendance and child labour

5.3.1 Estimation

The effect of the JPS scholarship program on the simultaneous decision regarding school attendance and work activities of enrolled children is analysed at the individual level. Endogenous program participation is dealt with by using a control function approach.[25] Like standard IV this method requires an exclusion restriction, but it is better suited to deal with unobserved effect heterogeneity. The correlation between unobserved effect heterogeneity and program selection is explicitly estimated, instead of relying on strong assumptions about this relationship.

Let A_i^* and L_i^* describe the latent processes that underlie the decision to have an enrolled child attend school ($A_i = 1$) and undertake labour activities ($L_i = 1$). These decisions may be correlated with each other and both may be affected by selection into the scholarship program, T_i. The selection decision is determined by the child's unobserved eligibility, T_i^*. The relationship between program participation and the outcomes is given by a latent variable model

[24] This information comes from the administrative data of the program. It reflects DBO budget allocation for the 1998/1999 school year. Although there may have been delays in allocation, by the time of the Susenas survey all districts and schools were informed about the budget. The estimation results are not shown here, but are available upon request.

[25] See Heckman and Navarro-Lozano (2004) for a discussion on control function methods.

$$A_i^0 = 1\left[A_i^{*0} \geq 0\right] = 1\left[\beta_a' X_i + u_{ai}^0 \geq 0\right] \tag{5a}$$

$$A_i^1 = 1\left[A_i^{*1} \geq 0\right] = 1\left[\beta_a' X_i + \gamma_a T_i + u_{ai}^1 \geq 0\right] \tag{5b}$$

$$L_i^0 = 1\left[L_i^{*0} \geq 0\right] = 1\left[\beta_l' X_i + u_{li}^0 \geq 0\right] \tag{5c}$$

$$L_i^1 = 1\left[L_i^{*1} \geq 0\right] = 1\left[\beta_l' X_i + \gamma_l T_i + u_{li}^1 \geq 0\right] \tag{5d}$$

$$T_i = 1\left[T_i^* \geq 0\right] = 1[\varphi' X_i + \delta z_j + v_i \geq 0] \tag{5e}$$

where $1[\cdot]$ is a binary indicator function. (A_i^{*0}, L_i^{*0}) are the latent states when a student does not receive a scholarship, and $\left(A_i^{*1}, L_i^{*1}\right)$ if the student does. X_i is a set of observed characteristics, v_i reflects the unobserved selection criteria, and the mis-targeting residual again serves as instrument. In this specification the effect of the program enters additively. Observed effect heterogeneity can be introduced by interaction terms of X_i and T_i. The unobserved $(u_{ai}^0, u_{ai}^1, u_{li}^0, u_{li}^1, v_i)$ are assumed to be independent of z_j and X_i. Selection on unobservables (in the base state) implies that (u_{ai}^0, u_{li}^0) are correlated with v_i. Selection on potential gains means that $\operatorname{cov}(u_a^0, v) \neq \operatorname{cov}(u_a^1, v)$ or $\operatorname{cov}(u_l^0, v) \neq \operatorname{cov}(u_l^1, v)$.

The outcomes that we actually observe are

$$A_i = A_i^1 T_i + A_i^0 (1 - T_i) = 1\left[A_i^* \geq 0\right] = 1\left[A_i^{*1} T_i + A_i^{*0} (1 - T_i) \geq 0\right] \tag{6a}$$

$$L_i = L_i^1 T_i + L_i^0 (1 - T_i) = 1\left[L_i^* \geq 0\right] = 1\left[L_i^{*1} T_i + L_i^{*0} (1 - T_i) \geq 0\right] \tag{6b}$$

Assuming the unobservables have a joint standard normal distribution, the conditional expectations of $(u_{ai}^0, u_{ai}^1, u_{li}^0, u_{li}^1)$ are

$$
\begin{aligned}
E[u_{ai}^0 \;\; | \;\; T_i = 0] &= E[u_{ai}^0 \mid v_i < -(\varphi' X_i + \delta z_j)] = \rho_{a0}\lambda_{0i} \\
E[u_{ai}^1 \;\; | \;\; T_i = 1] &= E[u_{ai}^1 \mid v_i \geq -(\varphi' X_i + \delta z_j)] = \rho_{a1}\lambda_{1i} \\
E[u_{li}^0 \;\; | \;\; T_i = 0] &= E[u_{li}^0 \mid v_i < -(\varphi' X_i + \delta z_j)] = \rho_{l0}\lambda_{0i} \\
E[u_{li}^1 \;\; | \;\; T_i = 1] &= E[u_{li}^1 \mid v_i \geq -(\varphi' X_i + \delta z_j)] = \rho_{l1}\lambda_{1i}
\end{aligned}
$$

where

$$
\lambda_{0i} = \frac{-\phi(\varphi' X_i + \delta z_j)}{1 - \Phi(\varphi' X_i + \delta z_j)}, \qquad \lambda_{1i} = \frac{\phi(\varphi' X_i + \delta z_j)}{\Phi(\varphi' X_i + \delta z_j)}
$$

These inverse Mills ratio's are computed from consistent first stage probit estimates of (5e). This provides an empirical specification for (6a) and (6b)

$$
A_i = 1\left[\beta_a' X_i + \gamma_a T_i + \rho_{a1}\lambda_{1i} T_i + \rho_{a0}\lambda_{0i}\left(1 - T_i\right) + \varepsilon_{ai} \geq 0\right] \tag{7a}
$$

$$
L_i = 1\left[\beta_l' X_i + \gamma_l T_i + \rho_{l1}\lambda_{1i} T_i + \rho_{l0}\lambda_{0i}\left(1 - T_i\right) + \varepsilon_{li} \geq 0\right] \tag{7b}
$$

In this switching regression framework the selection terms capture the bias due to endogenous program participation trough the parameters $(\rho_{a0}, \rho_{a1}, \rho_{l0}, \rho_{l1})$. Selection on unobservables implies $\rho_{a0} \neq 0$ or $\rho_{l0} \neq 0$. Selection on potential gains is evaluated by testing $\rho_{a0} = \rho_{a1}$ and $\rho_{l0} = \rho_{l1}$.

Under the normality assumption, equations (7a) and (7b) are estimated as a bivariate probit, with $(\varepsilon_{ai}, \varepsilon_{li}) \sim N(0, \Sigma)$. The simultaneous nature of labour and schooling decisions is now expressed by the parameter $\rho = corr\left(\varepsilon_{ai}, \varepsilon_{li}\right)$.[26] With Φ denoting the standard normal cdf, the average effects of the scholarships are calculated as

$$
\Pr\left(A^1 = 1\right) - \Pr\left(A^0 = 1\right) = \Phi\left(\beta_a' X + \gamma_a T\right) - \Phi\left(\beta_a' X\right) \tag{8a}
$$

$$
\Pr\left(L^1 = 1\right) - \Pr\left(L^0 = 1\right) = \Phi\left(\beta_l' X + \gamma_l T\right) - \Phi\left(\beta_l' X\right) \tag{8b}
$$

[26] This implicitly assumes that correlation between u_a and u_l is constant between treatment states, $\rho = corr(u_{ai}^0, u_{li}^0) = corr(u_{ai}^1, u_{li}^1)$.

5.3.2 Results

The bivariate probit estimates for the effect of JPS scholarships on school attendance and child labour are summarised by age group in table 9. The table provides the estimated treatment parameters and correlation coefficients. The scholarship variable T_i is interacted with gender, per capita consumption group, and a rural area dummy variable. The covariates further include age, household size, main source of household income (agriculture/non-agriculture), head of household characteristics (gender and level of education) and a variable indicating whether the child goes to public or private school. The specification also includes regional welfare indicators P_0, P_1, $BPS96$, the BKKBN poverty estimates for districts and sub-districts, IDT status of the village, and 6 variables indicating the presence of schools in the village (primary, junior secondary, and senior secondary, by public/private). Finally, the model includes a set of province dummy variables.[27] For convenience, the coefficients for covariates are omitted from the table. The first stage probit (5e) includes the same covariates and $JPS96$ as instrument. With P_0, P_1 and $BPS96$ controlling for non-random geographic targeting, $JPS96$ reflects the mis-targeting residual. The $JPS96$ coefficient is positive and significant at a 1 percent level for all age groups. As poverty is overestimated in the geographic targeting stage the probability of receiving a scholarship increases.[28]

Overall, the scholarships have an effect on school attendance and child labour. Although not all coefficients are statistically significant, they are jointly significant for both outcomes. The test statistic for joint significance is given in row 8 of each panel. The treatment parameters are jointly significant for labour in all age groups. For school attendance there seems to be only an effect for students aged 13 to 15.

There is some evidence of selection on unobservables (indicated by ρ_0). Especially for labour of the older students there is a strong correlation between u_{li}^0 and v_i. The results also suggest that students are selected based on potential gains from the program, as

[27] In 1999 Indonesia counted 27 provinces.

[28] Detailed estimation results are available upon request from the author.

the hypothesis that $\rho_0 = \rho_1$ is rejected. Again, for labour this result is stronger. The schooling and labour decisions of students are not independent, given the covariates. The correlation coefficient ρ is negative and becomes stronger with age.

The average effects are given in table 10. The probability of attending school in the previous week is 1.5 percentage point higher for students with a scholarship than for non-recipients. This seems a small change in nominal terms, but given attendance rates of around 98 percent (table 2) this implies that non-attendance has decreased by about 38 percent relative to the counterfactual situation of no JPS program. In case of child labour, the probability of working decreases by 3.8 percentage point for students with a scholarship. This suggests that the program reduced the incidence of child work from 14.0 to 10.2 percent, a 27 percent decrease relative to the base state (table 3).

These results suggest that the scholarships reduced the need for child labour to smooth household income during the crisis, raising the reservation wage for students. Note that given the high initial attendance rates, increased school attendance accounts for at most half of the time reallocated away from labour activities.

The absolute size of the effects on labour increase with age, partly because the prevalence of child labour is higher amongst older students. Also, the size of the scholarships increases with enrolment level. For the youngest age group the probability of working is reduced by 1.7 percentage point (20 percent relative decrease). For students aged 13-15 the effect on labour is larger, at 5.1 percentage point (30 percent relative decrease). For the oldest students a scholarship decreases the probability of working by 10.0 percentage point (40 percent relative decrease). For school attendance the effects increase slightly with age. Attendance is increased by 1.2 percentage point for students age 10 to 12, and by 1.8 percentage point for students age 13 to 15. This implies a 35 and 42 percent relative decrease in non-attendance, respectively. For older students the effects are imprecise.

The effects vary with the characteristics of the students. Generally, the effects on

Table 9: Bivariate probit estimates for the effect of JPS scholarships on school attendance and child labour, conditional on enrolment (equations (7a) and (7b))

Age group	Parameter	School attendance		Child labour	
		Coefficient	[s.e.][1]	Coefficient	[s.e.][1]
10 to 12	γ	0.569	[0.365]	0.041	[0.222]
N=48,798	$\gamma_{1-25\,pctile}$	−0.092	[0.123]	0.018	[0.101]
ρ= -0.197**	$\gamma_{25-50\,pctile}$	0.037	[0.139]	−0.166	[0.103]
	γ_{female}	−0.129	[0.103]	0.046	[0.077]
	γ_{rural}	−0.115	[0.166]	−0.333	[0.123]**
	ρ_1	−0.207	[0.135]	0.213	[0.101]*
	ρ_0	0.064	[0.184]	0.205	[0.159]
	Test joint sig. γ, $\chi^2(5)$	4.16		14.69*	
	Test $\rho_0 = \rho_1$, $\chi^2(1)$	1.91		0.00	
13 to 15	γ	0.719	[0.350]*	−0.229	[0.187]
N=39,561	$\gamma_{1-25\,pctile}$	−0.121	[0.130]	−0.117	[0.064]*
ρ=-0.331**	$\gamma_{25-50\,pctile}$	0.035	[0.141]	−0.081	[0.074]
	γ_{female}	−0.186	[0.106]†	−0.060	[0.057]
	γ_{rural}	0.027	[0.132]	−0.113	[0.092]
	ρ_1	−0.345	[0.148]*	0.245	[0.083]**
	ρ_0	−0.047	[0.214]	0.573	[0.156]**
	Test joint sig. γ, $\chi^2(5)$	11.05*		17.13**	
	Test $\rho_0 = \rho_1$, $\chi^2(1)$	2.48		5.56*	
16 to 18	γ	0.749	[0.592]	−0.883	[0.288]**
N=24,828	$\gamma_{1-25\,pctile}$	0.015	[0.222]	−0.135	[0.105]
ρ=-0.343**	$\gamma_{25-50\,pctile}$	−0.081	[0.585]	−0.093	[0.115]
	γ_{female}	0.040	[0.177]	−0.125	[0.095]
	γ_{rural}	−0.123	[0.206]	0.087	[0.106]
	ρ_1	−0.238	[0.265]	0.470	[0.119]**
	ρ_0	−0.330	[0.363]	0.863	[0.263]**
	Test joint sig. γ, $\chi^2(5)$	3.33		18.97**	
	Test $\rho_0 = \rho_1$, $\chi^2(1)$	0.06		2.60	
10 to 18	γ	0.627	[0.230]**	−0.213	[0.137]
N=113,187	$\gamma_{1-25\,pctile}$	−0.092	[0.087]	−0.075	[0.052]
ρ=-0.295**	$\gamma_{25-50\,pctile}$	0.025	[0.087]	−0.104	[0.048]*
	γ_{female}	−0.122	[0.060]*	−0.034	[0.043]
	γ_{rural}	−0.071	[0.087]	−0.113	[0.060]†
	ρ_1	−0.245	[0.094]**	0.242	[0.061]**
	ρ_0	−0.006	[0.129]	0.483	[0.124]**
	Test joint sig. γ, $\chi^2(5)$	11.99*		27.18**	
	Test $\rho_0 = \rho_1$, $\chi^2(1)$	3.41†		5.80*	

Significance levels: † : 10% * : 5% ** : 1%

[1] Bootstrapped standard errors with 100 replications.

Table 10: Average effects of JPS scholarships on school attendance and child labour, conditional on enrolment (equations (8a) and (8b))

Age group	Sub group	School attendance		Child labour	
		ATE	[s.e.][1]	ATE	[s.e.][1]
10 to 12	Average	0.012	[0.006]*	−0.017	[0.010]†
	1-25 percentile	0.010	[0.006]	−0.019	[0.014]
	25-50 percentile	0.014	[0.006]*	−0.022	[0.008]**
	50-100 percentile	0.011	[0.006]*	−0.012	[0.010]
	Male	0.013	[0.005]*	−0.020	[0.012]†
	Female	0.010	[0.006]	−0.013	[0.009]
	Urban	0.010	[0.005]*	0.001	[0.007]
	Rural	0.012	[0.006]*	−0.024	[0.012]*
13 to 15	Average	0.018	[0.005]**	−0.051	[0.017]**
	1-25 percentile	0.020	[0.007]**	−0.076	[0.022]**
	25-50 percentile	0.019	[0.005]**	−0.055	[0.017]**
	50-100 percentile	0.017	[0.004]**	−0.038	[0.016]*
	Male	0.019	[0.005]**	−0.056	[0.021]**
	Female	0.017	[0.006]**	−0.047	[0.014]**
	Urban	0.011	[0.003]**	−0.017	[0.009]†
	Rural	0.022	[0.006]**	−0.069	[0.022]**
16 to 18	Average	0.022	[0.018]	−0.100	[0.018]**
	1-25 percentile	0.034	[0.019]†	−0.149	[0.027]**
	25-50 percentile	0.025	[0.025]	−0.112	[0.020]**
	50-100 percentile	0.019	[0.016]	−0.085	[0.017]**
	Male	0.022	[0.017]	−0.114	[0.023]**
	Female	0.023	[0.019]	−0.086	[0.014]**
	Urban	0.015	[0.016]	−0.053	[0.009]**
	Rural	0.029	[0.021]	−0.147	[0.028]**
10 to 18	Average	0.015	[0.004]**	−0.038	[0.010]**
	1-25 percentile	0.015	[0.005]**	−0.040	[0.009]**
	25-50 percentile	0.018	[0.004]**	−0.039	[0.008]**
	50-100 percentile	0.017	[0.005]**	−0.033	[0.010]**
	Male	0.016	[0.004]**	−0.042	[0.013]**
	Female	0.014	[0.004]**	−0.033	[0.009]**
	Urban	0.011	[0.003]**	−0.014	[0.006]*
	Rural	0.017	[0.005]**	−0.051	[0.013]**

Significance levels: † : 10% * : 5% ** : 1%

The calculated average effects are based on estimation results reported in table (9).

[1] Bootstrapped standard errors with 100 replications.

labour were largest for students from poor households, in rural areas, and for boys. This suggest that reservation wages are lower for the poor, and in rural areas. The fact that labour supply is more responsive for boys may reflect the fact that boys are more often engaged in own farm and wage labour. This pattern is seen for all but the youngest age groups. The biggest differences are found for urban and rural areas. The probability of working in rural areas decreased by 5.1 percentage point, against 1.4 in urban areas.

6 Conclusion

This paper analyses the effectiveness of the Indonesian Social Safety Net scholarship program, which was introduced in August 1998 to protect the educational sector during the East Asian economic crisis. The program appears to have been effective in protecting access to education, despite considerable problems concerning geographical targeting in the initial year. Targeting was pro-poor for primary and junior secondary school, but there was also a lot of leakage to wealthier groups. For senior secondary school the scholarships were not allocated pro-poor at all, but instead distributed quite evenly across the per capita consumption quintiles.

The impact of the program is identified by exploiting the decentralised structure of the program design. At the initial stage of the program only incomplete information on the effects of the crisis was available to policy makers. This incomplete information on regional poverty gave rise to geographic mis-targeting. Instrumental variables are constructed from this mis-targeting, using data on the selection rules and ex-post information on the regional poverty profile. The availability of pre-intervention data makes it possible to verify the credibility of the identifying assumptions and the validity of the instrument.

Without the JPS program enrolment would have dropped substantially, especially

in primary school. 10 percent of program participants between 10 and 12 years old would have dropped out of school if they had not received a scholarship. In absence of the program, the enrolment rate for this group would have been 0.6 percentage point lower. This suggests that the program has actually prevented enrolment to decrease from 1998 to 1999. This is an important result because this is the age group where, in general, the transition from primary to junior secondary school takes place. It is at this transition point that many students leave school. For the age group 13-15 the program increased the enrolment rate by 0.8 percentage point, although these estimates are not precise. Amongst children aged 16 to 18 no significant effect was found. These results suggest that secondary school scholarships did little to affect enrolment.

The scholarship were especially effective for children whose education attainment was most vulnerable to the effects of the crisis. In response to the crisis, poor rural households facing resource constraints reduced investment on education of the youngest children in the household for consumption smoothing reasons, and protected the education of older children (Thomas *et al.*, 2004). This reflects the differences in future earnings from secondary and primary education, the fact that households have already invested in secondary education of older children, and the relatively low secondary school enrolment amongst students from poor families. Accordingly, the strongest effects of the scholarships were found amongst children at primary school in rural areas, from households that live below the poverty line.

The JPS program also affected school attendance and labour activities of enrolled children. Scholarship recipients were more likely to go to school and less likely to work, but only for students of secondary school age. By raising the reservation wage for students, the cash transfers relieved the pressure on households to draw on the labour of their children to smooth income. The effects on child labour are largest for the poor, suggesting that reservation wages for the poor are lower than for the non-poor.

Labour supply is much more sensitive to program participation than school atten-

dance, in absolute terms. This result differs from studies by Ravallion and Wodon (2000) and Schultz (2004), who find that increased schooling is only partly explained by a reduction in labour. The difference in these results is most likely explained by the extreme setting of the East Asian economic crisis. Under these circumstances the pressure on households to draw on child labour strongly increased. The estimation results then suggest that this has come only partly at the expense of school attendance. This supports the notion by Priyambada *et al.* (2002) that schooling and part time work often go together in Indonesia.

Concluding, the JPS scholarships have proved to be an effective instrument for protecting access to education. On the other hand, the allocation committees appear to have been only partly capable of identifying the poor. A large part of the funds have been allocated to students who would not have dropped out of school. More accurate targeting would greatly improve the program's effectiveness. Furthermore, priority should have been placed with protecting primary school enrolment, where the scholarships seem most effective, and with providing support for children from the poorest households in the transition from primary to secondary schooling.

The results and methodology presented in this paper contribute to the discussion on the viability of social safety net programs in a crisis situation. However, to gain full insight in the cost-effectiveness of intervention programs, future research is needed to investigate how the short term impact of cash transfer programs and economic crises translate into long term effects on education attainment.

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