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REVISED

October 2004

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

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October 28, 2004

[1] I am indebted to Jan Willem Gunning and Menno Pradhan. I also thank Rob Alessie, Chris Elbers, Wendy Janssens, Lionel Page, Cristobal Ridao-Cano, Sudarno Sumarto, Asep Suryahadi, Wenefrida Widyanti and seminar participants at the SMERU research institute and the World Bank in Jakarta, Tinbergen Institute Amsterdam and the IXth Spring Meeting of Young Economists in Warsaw for helpful comments. Financial support from the Netherlands Foundation for the Advancement of Tropical Research (NWO/WOTRO) is gratefully acknowledged. Address for correspondence: Department of Economics, Vrije Universiteit Amsterdam, De Boelelaan 1105, 1081 HV Amsterdam, The Netherlands. Email: rsparrow@feweb.vu.nl

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

Keywords: *Social safety net, program evaluation, education, child labour, Asian economic crisis*

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 (Pradhan and Sparrow, 2000; 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 - *Jaringan Pengaman Social* (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 the program.

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[2]. 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 (Jacoby and Skoufias, 1997). Households' consumption smoothing strategies may then involve reducing investments in education or relying on child labour to smooth consumption.[3]

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 price subsidy programs are indeed effective in increasing school participation and reducing child labour.[4] This paper evaluates the effects of such a demand side intervention within the

[1] The JPS further included a food security program, labour creation projects and a health program. Ananta and Siregar (1999) and Daly and Fane (2002) provide a good overview of all the JPS programs.

[2] Many empirical studies have stressed the importance of investment in education, in particular basic education, for future earnings. For an overview see, for example, Schultz (1988), Psacharopoulos (1994) and Jimenez (1995). In a study of the effects of basic education on future earnings in Indonesia, Duflo (2001) finds economic returns that range between 6.8 to 10.6 percent.

[3] There is some empirical work that explicitly studies the role of human capital investment in household consumption smoothing strategies. In the case of Indonesia Cameron and Worswick (2001) 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. For empirical studies on the effects of income volatility on schooling and child labour, in relation to credit markets, see Flug, Spilimbergo and Wachtenheim (1998), Dehejia and Gatti (2002), and Beegle, Dehejia and Gatti (2003).

[4] The conditional cash transfer (CCT) component of the Mexican PROGRESA program increased school enrolment and attendance, and reduced child work activities (Skoufias and Parker, 2001; Schultz, 2004). Similar results have been found with other CCT programs in Latin America (Rawlings and Rubio, 2003; Maluccio and

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.[5] 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[6]. 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 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 up 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

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Flores, 2004). Ravallion and Wodon (2000) find increased schooling and decreased child work as a results from a food-for-education program in Bangladesh.

[5] With regard to school subsidy programs, Ravallion and Wodon (2000), Skoufias and Parker (2001) and Schultz (2004) all find that the positive effects on schooling are only partly explained by reduced labour activities.

[6] See, amongst others, Canagarajah and Coulombe (1997) Nielsen (1998), Ridao-Cano (2001), Maitra and Ray (2002), and Rosati and Rossi (2003).

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 survey (*Susenas*), which is conducted annually on a national scale. The survey collects information on education, socioeconomic background of individuals and households, 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, house work, and other activities. In 1999 a special module was included concerning participation in the JPS programs. The 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 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.[7] The 1998 survey was fielded in February 1998, 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.

Another source of data is a village level census (*Podes*), conducted in 1996. It contains, amongst others, information on availability of schools in each village (*desa*) and township (*kelurahan*) in Indonesia. The 1996 Podes includes 66,486 villages and can be merged with the Susenas data at village level. Finally, I use administrative data for the district selection criteria and budget allocation, 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, and poverty rates had increased dramatically. 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, Sumarto and Pritchett (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 doubled during the crisis.[8] Urban areas seem to have been hit harder than rural areas. Also, there was considerable heterogeneity across regions, with Java (the most populous island of the archipelago) experiencing the greatest difficulties (Sumarto, Wetterberg and Pritchett, 1998). As more households moved into poverty, inequality in terms

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

[8] 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.

of household expenditure also increased, especially at the lower end of the income distribution (Skoufias, Suryahadi and Sumarto, 2000). 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. Rice and other staple foods experienced the most severe price increase. There is little evidence of rising overall unemployment during the crisis. Instead, real wages dropped by about 40 percent in the formal wage sector during the first year of the crisis, and agriculture seems to have absorbed part of the displaced labour from other sectors. (Cameron, 1999; Smith *et al.*, 2002; Frankenberg, Smith and Thomas, 2003).

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 human capital (i.e. health and education) decreasing by 37 percent. Thomas *et al.* (2004) find that spending on education declined, in particular for the rural poor. On average education expenditure per enrolled household member decreased by 19 percent from 1997 to 1999, amongst rural households. Moreover, the budget share of education declined strongly, as education spending decreased faster than overall household expenditure. They estimate that as a result of the crisis non-enrolment rates for primary school aged children increased by almost 20 percent. 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[9], 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, decreased in the crisis year 1998, but increased again in 1999. In 1998 net enrolment decreased slightly from 92.3 to 92.1 percent at primary school level, and from 57.8 to 57.1 percent at junior secondary level. The following year, when the JPS program had been initiated, enrolment picked up, exceeding pre-crisis levels. Senior secondary enrolment, however, increased throughout this period, even in 1998. A similar pattern is seen for total enrolment per age group of school aged children.

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; Dhanani and Islam, 2002). However, a comprehensive evaluation of the impact of the program has not been carried out yet. Cameron (2002) does find a positive effect of the program, using a dataset concerning 100 predominantly poor villages. She finds significant effects only for junior secondary education.

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

[9] See, for example, Behrman and Deolalikar (1995).

costs for school uniforms and books. Variable schooling costs include transportation costs and monthly tuition fees.[10] 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, which could reduce time spent at school.[11]

Table 2 looks at school attendance in the past week for enrolled students, in 1999. School attendance is fairly high for all enrolment levels and age groups, varying around 98 percent. However, program participants have a slightly lower attendance rate than non-participants, on average just over half a percentage point. Columns 5 and 6 show that working doesn't prevent children from attending school. However, 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 depicts labour activities for scholarship recipients, enrolled children without 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.[12]

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 in February 1999 was Rp. 137,284, while households

[10] Annual sunk costs for enrolment fees, school uniforms and books constitute 25 percent of average total education expenditures per child in the 1997/1998 school year (Pradhan and Sparrow, 2000). About 11 percent of total expenditures are due to daily transportation, while monthly tuition and BP3 (i.e. parent-teacher association) fees take account of 29 percent.

[11] Qualitative research by Jones *et al.* (2003) finds anecdotal evidence to support this hypothesis.

[12] The DBO block grants could be used to purchase materials, make repairs, and cover other operational costs.

representing the poorest 20 percent of the population spent Rp. 61,470 per capita per month.[13] For the 1997/1998 school year, monthly expenditures on education per student from the poorest quintile were Rp. 4,826, Rp. 15,725 and Rp. 31,549 (in February 1999 prices) for primary, junior secondary and senior secondary, respectively.[14] Thus, for the poorest households the scholarships can be 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 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 [15], 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).[16] 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. Students 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. In remote areas, where transportation costs for students are high, post office officials would travel to the schools to disburse the

[13] Suryahadi *et al.*(2000) calculate this on basis of the 1999 Susenas consumption module.

[14] The reported figures are based on the 1998 Susenas education module, and include school fees, equipment and supplies, transportation and tutors (Pradhan and Sparrow, 2000).

[15] IDT refers to the *Inpres Desa Tertinggal* program, an anti-poverty program for economically less developed villages. For this program, each village or township in Indonesia has been identified as either *developed* or *less developed*. This indicator was not used for the primary school poverty ranking.

[16] 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.

scholarships to the students.

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.

The concentration curve in figure 1 shows a pro-poor distribution of scholarships, but also considerable leakage to students from wealthier households.[17] 62.8 percent of the scholarships are allocated to students from the poorest 40 percent of the population, while the students from wealthiest 20 percent hold 5.6 percent of the scholarships. Figure 2 shows marked differences between enrolment levels. Scholarship distribution to primary school students is more pro-poor than the overall distribution. Students from the two poorest quintiles hold 70.8 percent of the scholarships, while 3.1 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 Identification

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. Poor households are expected to be more likely to take their children out of school or have them participate in labour activities, in response to the effects of the crisis. In the absence of the scholarship program, enrolment and school attendance would be 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 illustrate the problem it is useful to introduce some notation. Denote the outcome for an individual child i as Y_i^T, where T_i is a treatment indicator for the program, with value $T_i = 1$ if a child receives a scholarship and $T_i = 0$ otherwise. Each child then has two potential

[17] Following Lanjouw *et al.* (2002) poverty lines serve as spatial price deflator. The argument behind this approach is that regional poverty lines capture spatial differences in the cost of living, in that they reflect the level of expenses required to obtain some reference level utility in each region. I use poverty lines from Pradhan *et al.* (2001).

outcomes

$$Y_i^0 = f^0(X_i) + u_i^0 \quad \text{(1a)}$$
$$Y_i^1 = f^1(X_i) + u_i^1 \quad \text{(1b)}$$

where X_i are observed characteristics of the child and $\left(u_i^0, u_i^1\right)$ are unobserved.

The effect of the program is $\Delta_i \equiv Y_i^1 - Y_i^0$. However, finding a causal relationship between Y_i and T_i is problematic since it is impossible to observe the pair $\left(Y_i^1, Y_i^0\right)$ for the same child (Heckman, LaLonde and Smith, 1999). For each individual we can only observe $Y_i = Y_i^1 T_i + Y_i^0 (1 - T_i)$. In other words, the counterfactual of the actual observed event T_i and the subsequent outcome Y_i^T is not observed. We do not observe the state of a scholarship recipient in the event that this child had not received the scholarship.

Selection into the program, T_i, will be partly determined by the unobserved characteristics (u_i^0, u_i^1) if the selection criteria are not fully observed. Comparing average outcomes for the recipients and non-recipients (conditional on X_i) will then yield a biased estimate of the average effect of the program.

A variety of approaches can be used to deal with non-random placement of scholarships. A frequently applied method is to use instrumental variables, which relies on finding some source of exogenous variation that affects the probability of receiving a scholarship, but is independent of the potential outcomes. Denote the observed exogenous variation by Z_i, and let T_i^* be a latent variable that describes the decision rule for allocating scholarships to students. Only the outcome of the allocation process, T_i, is observed

$$T_i = 1\left[T_i^* \geq 0\right] = 1\left[g\left(Z_i, X_i\right) + v_i \geq 0\right] \quad (2)$$

where v_i reflects unobserved selection criteria, which may be correlated with (u_i^0, u_i^1). The identifying assumptions are

$$E(u_i^0 \mid Z_i, X_i) = E(u_i^1 \mid Z_i, X_i) = 0 \quad \text{(A1)}$$
$$E(T_i^* \mid Z_i, X_i) \text{ is a non-trivial function of } Z_i \text{, given } X_i \quad \text{(A2)}$$

The latter can easily be verified by estimating $E\left(T_i \mid Z_i, X_i\right)$, but (A1), on the other hand, can not be tested if Z_i consists of only one variable.

5.2 Regional mis-targeting

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.[18] 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. Figure 3 shows that there is a strong positive correlation between the $JPS96$ measure and scholarship coverage, but also that it doesn't fully explain actual allocation. 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 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 $JPS96$ misjudged the degree of poverty in the districts, since only pre-crisis information on regional poverty in 1996 was available. 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, between products (especially food) and across regions (Cameron, 1999; Frankenberg *et al.*, 2003; Friedman and Levinsohn, 2002). Applying the 1996 poverty estimates as allocation rule is implicitly assuming that relative prices have not changed over time.

Table 5 is illustrative for the difficulty of capturing the effect of the crisis using 1996 data. The table shows the Indonesian provinces ranked (from low to high) according to $JPS96$, and the 1996 ($BPS96$) and 1999 ($BPS99$) poverty headcount released by the Indonesian Bureau of Statistics (BPS) in 2000. There is a considerable difference in the rankings between years and methods.[19]

Mis-targeting of 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

$$JPS96_j = \psi' V_j + z_j \tag{3}$$

where V_j reflects the actual poverty in 1998 and the impact of the crisis for district j. The *mis-targeting* term, z_j, is a non-systematic judgement error in the targeting process, and assumed to be independent of $\left(u_i^0, u_i^1\right)$. It reflects the inability to capture the extent of poverty (V_j) during the crisis due to the lack of information on the actual situation in 1998.

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

[19] Both $JPS96$ and $BPS96$ are based on the 1996 Susenas data, but they differ in methodology for setting poverty lines. The $BPS96$ and $BPS99$ have been constructed in similar fashion, using the Susenas household expenditure surveys. See BPS (2000) for details.

With the belated availability of information 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. The identifying assumption is then

$$E(u_i^0 \mid \hat{z}_j) = E(u_i^1 \mid \hat{z}_j) = 0 \tag{A3}$$

For example, if $JPS96_j$ overestimates the actual degree of poverty in 1998 (V_j) 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 from the enrolment rate, and the extent of school attendance and child labour in that district.

With a strategy like this there remains the danger that V_j is not fully observable, and that $\hat{z}_j$ is correlated with $\left(u_i^0, u_i^1\right)$. One way to evaluate the credibility of assumption (A3) is by using pre-intervention data from Susenas 1998, and estimate $E\left(\overline{Y}_j \mid \hat{z}_j\right)$, where $\overline{Y}_j$ are district means. Under assumption (A3), $\hat{z}_j$ should not be correlated with the pre-intervention outcomes (Pradhan, Rawlings and Ridder, 1998). The identifying assumption for estimating the impact of the program is that if (A3) holds for 1998, it also holds for 1999. This seems a reasonable assumption since $JPS96$ is based on historic poverty estimates.

Figures 4 to 9 (appendix B) show the results of the district level regressions for enrolment, school attendance and child labour for children age 10 to 18. The results suggest that given the specification of V_j, assumption (A3) holds. The regional poverty profile V_j includes the 1998 headcount (P_0) and the poverty gap (P_1) for each district.[20] The BPS headcount for 1996 ($BPS96$) is included to capture the impact of the crisis. Without controlling for V_j, enrolment and labour are correlated with $JPS96_j$ (figures 4 and 8), while school attendance is not (figure 6). In those districts that were regarded to be relatively poor, enrolment is lower and incidence of child labour is higher. However, after controlling for the poverty profile there is no correlation between Y_j and $\hat{z}_j$ (figures 5, 7 and 9). The coefficients are small and statistically not significant. Note that for enrolment and attendance it suffices to just include P_0 and P_1 in V_j.

When interpreting the causal effects it is important to keep in mind that the IV approach implicitly assumes unobserved effect homogeneity. If the unobserved effects of receiving a scholarship vary across individual children then strong assumptions are required if IV estimates are to be interpreted as average treatment effects (Heckman, 1997). For example, it would be sufficient to assume that children are not selected into the program based on private information about their individual potential gain from a scholarship (i.e.,

[20] P_0 and P_1 are estimated based on per capita household expenditure c, with $P_\alpha = n^{-1} \sum_{i=1}^{q} \left(\frac{pl - c_i}{pl}\right)^\alpha$ and where pl is the poverty line and q the number of individuals for which $c_i \leq pl$ (following Foster, Greer and Thorbecke, 1984). The expenditure data comes 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).

$E\left(u_i^1 - u_i^0 \mid T_i\right) = 0$).[21] If we are not willing to make these assumptions then IV will only identify the *local* average treatment effect (LATE), proposed by Imbens and Angrist (1994). This is the average treatment effect for those children for whom treatment assignment is affected by the variation in the instrument. In this respect, the mis-targeting term is an appealing instrumental variable. Under the assumption that the probability of receiving a scholarship is conditional on geographic allocation, the LATE can be interpreted as the effect of marginal changes in geographic targeting policy.

5.3 The effect on enrolment

5.3.1 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.[22] For each district j the enrolment rate in year t is modelled as a linear function of the intensity of the scholarship program

$$\bar{S}_{jt} = \alpha_j + \left(\tau + \eta_j\right) \overline{T}_{jt} + \phi' W_{jt} + \theta_0 d_t + \sum_{r=2}^{5} \theta_r d_r d_t + \varepsilon_{jt} \tag{4}$$

where

$$\overline{T}_{jt} = \frac{1}{N_{jt}} \sum_{i=1}^{N_{jt}} T_{ijt}$$

$\bar{S}_j$ 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. 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). Note that 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

[21] Note that $E\left(Y_i\right) = f^0(X_i) + T_i\left(f^1\left(X_i\right) - f^0\left(X_i\right)\right) + e_i$, where the unobserved $e_i = E\left(u_i^0|T_i\right) + T_i E\left(u_i^1 - u_i^0|T_i\right)$. Since T_i is correlated with the instrument by assumption (A2), the instrument is correlated with e_i if $E\left(u_i^1 - u_i^0|T_i\right) \neq 0$, even if assumption (A1) holds. See Angrist (2004) for a discussion on assumptions (weaker than effect homogeneity) that allow IV to identify average treatment effects.

[22] 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 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.

time trend by interacting time variable d_t with region specific fixed effects, d_r.[23] 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 (4) yields

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

OLS will give unbiased estimates for (5) 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.

Interpretation of the estimates depends on assumptions regarding the expected effect heterogeneity. If $\overline{T}_j$ is independent of η_j, in which case $E\left(\eta_j \mid \overline{T}_j\right) = 0$, then both IV and OLS identify the average treatment effect, $E\left(\hat{\tau}_{ATE}\right) = \tau$. This is not an unreasonable 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 $E\left(\eta_j \mid \overline{T}_j\right) \neq 0$, even if this was not known a priori to program managers. In this case OLS will retrieve the average treatment effect on the treated, $E\left(\hat{\tau}_{ATT}\right) = \tau + E\left(\eta_j \mid \overline{T}_j\right)$. 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, $E\left(\hat{\tau}_{LATE}\right) = \tau + E(\eta_j \mid \overline{T}_j(\hat{z}_j') > \overline{T}_j(\hat{z}_j''))$.[24] This is the average effect for those districts for which allocation $\overline{T}_j$ is affected by a change in the mis-targeting term. This would seem to hold for all districts since $\hat{z}_j$ determines the selection rule for geographic targeting by construction.

The overall effect of the program on the enrolment rate is given by a population weighted

[23] 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.

[24] 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. This seems plausible in the JPS setting.

average of the effects for the districts

$$E(\bar{S}^1_{1999}) - E(\bar{S}^0_{1999}) = \hat{\tau} \sum_{j=1}^{J} \frac{N_j}{N} \frac{1}{N_j} \sum_{i=1}^{N_j} T_{ij} = \hat{\tau}\overline{T} \quad (6)$$

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

5.3.2 Results

Table 6 shows the OLS and IV impact estimates for equation (5) and the effect on the enrolment rate, $\hat{\tau}\overline{T}$ (equation (6)), for all children aged 10 to 18, and for the three age groups.[25] The estimated effects for net enrolment are given in table 7. The tables also report $\overline{T}$. The welfare variables, W_j, include the share of rural population, the average age, household size in the district, and poverty indictors P_0 and P_1. 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. Overestimation 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-constant time trend due to regional variation in the crisis effect. It could also be that IV retrieves a LATE that differs strongly from ATT or ATE. But this seems unlikely since $\overline{T}_j$ is highly responsive to $\hat{z}_j$. This is illustrated by figure 10, which shows a partial regression leverage plot for the effect of $\hat{z}_j$ in the first stage regression of overall enrolment.[26]

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. 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.

Turning to net enrolment, there is only an effect for primary school level (18 percent increase). So the bulk of the effect picked up for 13 to 15 year olds is due to students who are still in primary school (either because of delayed enrolment or grade repetition). This is an important result. These students are likely to be in the higher grades of primary school. In

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

[26] Delays in program implementation could disturb this relationship. 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 (see table 10). The dummy coefficients ($\hat{\theta}_{no-treat}$) are small and not significant in all regressions.

absence of the program they would have dropped out of school just prior to finishing primary education.

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.[27]

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 a little 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 groups is not due to bad targeting. Enrolment simply is less sensitive to income shocks. Table 9 gives the results for net enrolment by age group, confirming that the scholarships are effective only at primary school. The significant and negative OLS estimates further confirm that difference approach in equation (5) is not sufficient to deal with the selective targeting of districts.

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.

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

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

as a regressor.[28] Table 11 shows that enrolment is not affected by per capita DBO allocation to districts. The estimates for the effects of scholarships change little (slightly larger) with this specification. This suggests that the block grants do not interfere with the estimates of the scholarships.

5.4 The effect on school attendance and child labour

5.4.1 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 method.[29] 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 decision process for selection is described by equation (2). The relationship between program participation and the outcomes is given by a latent variable model

$$\begin{aligned}
A_i^0 &= 1\left[A_i^{*0} \geq 0\right] = 1\left[\beta_a' X_i + u_{ai}^0 \geq 0\right] && \text{(7a)}\\
A_i^1 &= 1\left[A_i^{*1} \geq 0\right] = 1\left[\beta_a' X_i + \gamma_a T_i + u_{ai}^1 \geq 0\right] && \text{(7b)}\\
L_i^0 &= 1\left[L_i^{*0} \geq 0\right] = 1\left[\beta_l' X_i + u_{li}^0 \geq 0\right] && \text{(7c)}\\
L_i^1 &= 1\left[L_i^{*1} \geq 0\right] = 1\left[\beta_l' X_i + \gamma_l T_i + u_{li}^1 \geq 0\right] && \text{(7d)}\\
T_i &= 1\left[T_i^* \geq 0\right] = 1[\beta_T' X_i + \delta Z_i + v_i \geq 0] && \text{(7e)}
\end{aligned}$$

$\left(A_i^{*0}, L_i^{*0}\right)$ 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. 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 outcomes that we actually observe are

$$\begin{aligned}
A_i &= A_i^1 T_i + A_i^0 (1 - T_i) = 1\left[A_i^* \geq 0\right] = 1\left[A_i^{*1} T_i + A_i^{*0} (1 - T_i) \geq 0\right] && \text{(8a)}\\
L_i &= L_i^1 T_i + L_i^0 (1 - T_i) = 1\left[L_i^* \geq 0\right] = 1\left[L_i^{*1} T_i + L_i^{*0} (1 - T_i) \geq 0\right] && \text{(8b)}
\end{aligned}$$

[28] 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.

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

where the conditional expectations of the latent outcomes are

$$\begin{aligned} E\left(A_i^* \mid X_i, Z_i\right) &= \beta_a' X_i + \gamma_a T_i + T_i E\left(u_{ai}^1 \mid T_i = 1\right) + \left(1 - T_i\right) E\left(u_{ai}^0 \mid T_i = 0\right) && \text{(9a)} \\ E\left(L_i^* \mid X_i, Z_i\right) &= \beta_l' X_i + \gamma_l T_i + T_i E\left(u_{li}^1 \mid T_i = 1\right) + \left(1 - T_i\right) E\left(u_{li}^0 \mid T_i = 0\right) && \text{(9b)} \end{aligned}$$

The unobservables $\left(u_{ai}^0, u_{ai}^1, u_{li}^0, u_{li}^1, v_i\right)$ are assumed to be independent of Z_j and X_i. Note that this is a stronger assumption than that underlying the exclusion restriction with IV. In contrast to standard IV, the instrument now is assumed to be exogenous to the allocation of scholarships. For the targeting error $\hat{z}_j$ this is a reasonable assumption. Assume further that the unobservables have a joint standard normal distribution

$$\begin{bmatrix} u_a^0 \\ u_a^1 \\ u_l^0 \\ u_l^1 \\ v \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \\ 0 \\ 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho_a & \rho_{al0} & \rho_{01} & \rho_{a0} \\ \rho_a & 1 & \rho_{10} & \rho_{al1} & \rho_{a1} \\ \rho_{al0} & \rho_{10} & 1 & \rho_l & \rho_{l0} \\ \rho_{01} & \rho_{al1} & \rho_l & 1 & \rho_{l1} \\ \rho_{a0} & \rho_{a1} & \rho_{l0} & \rho_{l1} & 1 \end{bmatrix}\right) \qquad \text{(A4)}$$

Given the normality assumption, the conditional expectations of $\left(u_{ai}^0, u_{ai}^1, u_{li}^0, u_{li}^1\right)$ in equations (9a) and (9b) are

$$\begin{aligned} E\left(u_{ai}^0 \mid T_i = 0\right) &= E\left(u_{ai}^0 \mid v_i < -g\left(Z_i, X_i\right)\right) = \rho_{a0}\lambda_{0i} \\ E\left(u_{ai}^1 \mid T_i = 1\right) &= E\left(u_{ai}^1 \mid v_i \geq -g\left(Z_i, X_i\right)\right) = \rho_{a1}\lambda_{1i} \\ E\left(u_{li}^0 \mid T_i = 0\right) &= E\left(u_{li}^0 \mid v_i < -g\left(Z_i, X_i\right)\right) = \rho_{l0}\lambda_{0i} \\ E\left(u_{li}^1 \mid T_i = 1\right) &= E\left(u_{li}^1 \mid v_i \geq -g\left(Z_i, X_i\right)\right) = \rho_{l1}\lambda_{1i} \end{aligned}$$

where

$$\lambda_{0i} = \frac{-\phi\left(g\left(Z_i, X_i\right)\right)}{1 - \Phi\left(g\left(Z_i, X_i\right)\right)}, \qquad \lambda_{1i} = \frac{\phi\left(g\left(Z_i, X_i\right)\right)}{\Phi\left(g\left(Z_i, X_i\right)\right)}$$

and ϕ and Φ denote the standard normal df and cdf, respectively. The inverse Mills ratio's λ_0 and λ_1 can be computed from (consistent) first stage probit estimates of (7e).

This provides an empirical specification for (8a) and (8b)

$$\begin{aligned} A_i &= 1\left[\beta_a' X_i + \gamma_a T_i + \rho_{a1}\lambda_{1i} T_i + \rho_{a0}\lambda_{0i}\left(1 - T_i\right) + \varepsilon_{ai} \geq 0\right] && \text{(10a)} \\ L_i &= 1\left[\beta_l' X_i + \gamma_l T_i + \rho_{l1}\lambda_{1i} T_i + \rho_{l0}\lambda_{0i}\left(1 - T_i\right) + \varepsilon_{li} \geq 0\right] && \text{(10b)} \end{aligned}$$

Under the normality assumption, equations (10a) and (10b) can be estimated as a bivariate probit, with $\left(\varepsilon_{ai}, \varepsilon_{li}\right) \sim N\left(0, \Sigma\right)$. The simultaneous nature of labour and schooling decisions is now expressed by the parameter $\rho = corr\left(\varepsilon_{ai}, \varepsilon_{li}\right)$.[30] This follows Canagarajah and Coulombe

[30] This implicitly assumes that correlation between u_a and u_l is constant between treatment states (i.e., $\rho = \rho_{al0} = \rho_{al1}$).

(1997) and Nielsen (1998), who also analyse the joint decision of school attendance and child labour with a bivariate probit. Both studies find a negative and statistically significant correlation coefficient.

In this framework the bias due to endogenous program participation is captured by the estimated parameters $(\rho_{a0}, \rho_{a1}, \rho_{l0}, \rho_{l1})$. Selection on unobservables (in the base state) implies that $\left(u^0_{ai}, u^0_{li}\right)$ are not independent of v_i. In this case cov $\left(u^0_a, v\right) \neq 0$ (i.e., $\rho_{a0} \neq 0$) or cov $\left(u^0_l, v\right) \neq 0$ (i.e., $\rho_{l0} \neq 0$). Selection on potential gains means that cov $\left(u^0_a, v\right) \neq$ cov $\left(u^1_a, v\right)$ or cov $\left(u^0_l, v\right) \neq$ cov $\left(u^1_l, v\right)$. This can be evaluated by testing whether $\rho_{a0} = \rho_{a1}$ and $\rho_{l0} = \rho_{l1}$. Note that $(\rho_a, \rho_l, \rho_{01}, \rho_{10})$ are not identified, since we never observe the outcomes of an individual child in both states.

The average effects of the scholarships are calculated as

$$\Pr\left(A^1 = 1\right) - \Pr\left(A^0 = 1\right) = \Phi\left(\beta'_a X + \gamma_a T\right) - \Phi\left(\beta'_a X\right) \tag{11a}$$

$$\Pr\left(L^1 = 1\right) - \Pr\left(L^0 = 1\right) = \Phi\left(\beta'_l X + \gamma_l T\right) - \Phi\left(\beta'_l X\right) \tag{11b}$$

which are the marginal effects of γ_a and γ_l.

5.4.2 Results

The bivariate probit estimates for the effect of JPS scholarships on school attendance and child labour are summarised by age group in table 12. 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. For convenience, the coefficients for covariates are omitted from the table.[31] The first stage probit (7e) includes the same covariates. $JPS96$ serves as instrument with P_0, P_1 and $BPS96$ controlling for non-random geographic targeting. The instrument is 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.

The results show that overall the scholarships do 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. Looking at the three different age groups, the treatment parameters are always jointly

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

significant for labour. 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 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 significant at a 1 percent level. The correlation between both decisions is negative and becomes stronger with age (varying from -0.20 to -0.34).

The average effects are given in table 13. Starting with the aggregate effects of the scholarships, 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 is a small change, given attendance rates of 97 to 98 percent (table 2). The effect on child labour is larger, with the probability of working decreasing 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 increase relative to the base state (see 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 labour supply seems to be more sensitive to the program than school attendance. Increased school attendance takes account of at most half of the time reallocated away from labour activities.

The size of the effects on labour increase with age. This results is in part due to the fact that the incidence of child labour is higher amongst older students. Also, the size of the scholarships increases with enrolment level. Impact is smallest for the youngest age group (10-12), where the probability of working is reduced by 1.7 percentage point (20 percent relative increase). For students aged 13-15 the effect on labour is larger, at 5.1 percentage point (30 percent relative increase). For the oldest students a scholarship decreases the probability of working by 10.0 percentage point (40 percent relative increase).

The effects vary with the characteristics of the students. Generally, the effects on 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 wage labour, while girls may be committed to own farm and domestic work. 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.

In case of school attendance the differences in the effects are small, and increase slightly with age. Attendance is increased by 1.2 percentage point for students age 10 to 12, and by 2.9 percentage point for the older students.

The bivariate probit estimates and average effects by (net) enrolment level are given in tables 14 and 15 show similar results.

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 and the fact that 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 some 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. For the age group 13-15 the program increased the enrolment rate by 0.8 percentage point, accounting for half of the observed increase in enrolment. However, most of this effect concerns children in primary school. 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. 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, and the fact that households have already invested in secondary education of older children. 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 the decisions regarding 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. Although it was not an explicit goal of the program, the scholarships raised 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 attendance. This result differs from studies by Ravallion and Wodon (2000), Skoufias and Parker (2001) 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.

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A Tables

Table 1: Enrolment rates, by education level and age group in 1995, 1997, 1998 and 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

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

		JPS	Non-JPS	Work	No work	All
Enrolment	Primary	97.6	98.2	91.5	98.4	98.1
		[0.33]	[0.11]	[0.80]	[0.11]	[0.11]
	Junior Secondary	97.6	98.2	92.2	98.6	98.1
		[0.36]	[0.11]	[0.66]	[0.10]	[0.11]
	Senior Secondary	97.8	98.5	94.7	98.8	98.5
		[0.80]	[0.13]	[0.71]	[0.12]	[0.13]
Age group	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 3: Labour activities in previous week (percentage), by enrolment level and age group in 1999

		Enrolled			Not enrolled	All
		JPS	Non-JPS	All		
Enrolment	Primary	7.9	3.6	3.9		
		[0.59]	[0.13]	[0.13]		
	Junior Secondary	12.1	7.3	7.7		
		[0.75]	[0.22]	[0.22]		
	Senior Secondary	13.6	7.2	7.5		
		[1.56]	[0.26]	[0.26]		
Age group	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 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
GN-OTA*	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

* National Foster Parents Movements

Table 5: Provinces ranked by JPS geographic allocation rule (1996) and BPS poverty estimates (1996 and 1999)

Province	JPS 1996*	BPS 1996†	BPS 1999†
Aceh	16	8	6
North-Sumatra	17	9	8
West-Sumatra	7	5	3
Riau	3	7	4
Jambi	8	11	17
South-Sumatra	15	12	14
Bengkulu	10	14	12
Lampung	14	21	20
Jakarta	1	1	1
West-Java	11	6	11
Central-Java	20	17	18
Yogyakarta	12	16	15
East-Java	19	18	21
Bali	2	2	2
NTB	22	23	23
NTT	24	24	25
West-Kalimantan	25	20	16
Central-Kalimantan	18	10	7
South-Kalimantan	21	3	5
East-Kalimantan	9	4	13
North-Sulawesi	13	15	9
Central-Sulawesi	5	19	19
South-Sulawesi	4	13	10
South-East-Sulawesi	6	22	22
Maluku	23	26	24
Irian-Jaya	26	25	26

Note: The table depicts ranking from lowest (1) to highest (26) poverty headcount.
Source: * Ministry of Education (1998) and † BPS (2000).

Table 6: Effect of the JPS scholarships on enrolment (equations (5) and (6))

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: $\dagger$: 10% $*$: 5% $**$: 1%

Table 7: Effect of the JPS scholarships on net enrolment (equations (5) and (6))

School level	$\hat{\tau}$	[s.e.]	$\hat{\tau}\overline{T}$	$\overline{T}$	$\hat{\delta}_z$	[s.e.]	N
OLS							
Primary	0.150	[0.049]**	0.0088	0.058			294
Junior Secondary	−0.057	[0.045]	−0.0039	0.068			294
Senior Secondary	−0.043	[0.057]	−0.0010	0.024			294
IV							
Primary	0.178	[0.067]**	0.0104	0.058	0.819	[0.045]**	294
Junior Secondary	0.022	[0.069]	0.0015	0.068	1.046	[0.072]**	294
Senior Secondary	0.021	[0.111]	0.0005	0.024	0.605	[0.059]**	294

Significance levels: $\dagger$: 10% $*$: 5% $**$: 1%

Table 8: Effect of the JPS scholarships on enrolment, by per capita consumption, gender and urban/rural (IV estimates for equation (5))

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%

Table 9: Effect of the JPS scholarships on net enrolment, by per capita consumption, gender and urban/rural (IV estimates for equation (5))

School level	Sub group	OLS		IV		
		$\hat{\tau}$	[s.e.]	$\hat{\tau}$	[s.e.]	N
Primary						
	1-25 percentile	0.014	[0.046]	0.102	[0.063]	293
	25-50 percentile	0.109	[0.060]†	0.108	[0.092]	294
	50-100 percentile	0.064	[0.096]	0.104	[0.160]	294
	male	0.169	[0.053]**	0.170	[0.076]*	294
	female	0.130	[0.053]*	0.189	[0.074]*	294
	urban	0.121	[0.077]	0.267	[0.152]†	287
	rural	0.034	[0.043]	0.126	[0.058]*	277
Junior Secondary						
	1-25 percentile	0.007	[0.047]	0.033	[0.081]	290
	25-50 percentile	−0.129	[0.061]*	−0.025	[0.106]	293
	50-100 percentile	−0.148	[0.082]†	0.012	[0.162]	294
	male	−0.047	[0.061]	0.048	[0.097]	294
	female	−0.027	[0.049]	0.003	[0.079]	294
	urban	−0.137	[0.068]*	−0.079	[0.123]	287
	rural	−0.006	[0.044]	0.031	[0.067]	276
Senior Secondary						
	1-25 percentile	−0.044	[0.045]	0.050	[0.112]	275
	25-50 percentile	−0.109	[0.062]†	−0.175	[0.149]	292
	50-100 percentile	−0.172	[0.111]	−0.182	[0.263]	293
	male	−0.004	[0.070]	0.042	[0.143]	294
	female	−0.135	[0.065]*	−0.031	[0.149]	294
	urban	−0.124	[0.108]	−0.244	[0.316]	286
	rural	0.020	[0.049]	0.040	[0.091]	273

Significance levels: $\dagger$: 10% $*$: 5% $**$: 1%

Table 10: Effect of the JPS scholarships on enrolment, controlling for "no-treatment" districts

Age group	$\hat{\tau}$	[s.e.]	$\hat{\theta}_{no-treat}$	[s.e.]	N
OLS					
10 to 12	0.074	[0.026]**	−0.002	[0.008]	294
13 to 15	0.021	[0.052]	−0.022	[0.017]	294
16 to 18	0.083	[0.147]	0.010	[0.013]	294
10 to 18	0.054	[0.048]	0.002	[0.018]	294
IV					
10 to 12	0.100	[0.037]**	−0.000	[0.008]	294
13 to 15	0.106	[0.077]	−0.016	[0.017]	294
16 to 18	0.031	[0.261]	0.009	[0.015]	294
10 to 18	0.130	[0.066]†	0.007	[0.019]	294

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

Table 11: Effect of the JPS scholarships on enrolment, controlling for per capita DBO transfers

Age group	$\hat{\tau}$	[s.e.]	$\hat{\tau}_{DBO}$	[s.e.]	N
OLS					
10 to 12	0.086	[0.028]**	−0.001	[0.001]	294
13 to 15	0.036	[0.053]	0.000	[0.004]	294
16 to 18	0.067	[0.142]	−0.003	[0.004]	294
10 to 18	0.053	[0.051]	−0.000	[0.001]	294
IV					
10 to 12	0.117	[0.040]**	−0.001	[0.001]	294
13 to 15	0.127	[0.081]	−0.002	[0.004]	294
16 to 18	0.035	[0.249]	−0.003	[0.004]	294
10 to 18	0.140	[0.074]†	−0.000	[0.001]	294

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

Table 12: Bivariate probit estimates for the effect of JPS scholarships on school attendance and child labour, conditional on enrolment (equations (10a) and (10b))

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 13: Average effects of JPS scholarships on school attendance and child labour, conditional on enrolment (equations (11a) and (11b))

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 (12)

[1] Bootstrapped standard errors with 100 replications

Table 14: Bivariate probit estimates for the effect of JPS scholarships on school attendance and child labour, conditional on net enrolment (equations (10a) and (10b))

Level	Parameter	School attendance		Child labour	
		Coefficient	[s.e.][1]	Coefficient	[s.e.][1]
Primary	γ	0.779	[0.609]	-0.011	[0.279]
N=46,253	$\gamma_{1-25\,pctile}$	-0.105	[0.140]	-0.021	[0.107]
ρ= -0.190**	$\gamma_{25-50\,pctile}$	0.095	[0.161]	-0.184	[0.112]
	γ_{female}	-0.067	[0.118]	0.017	[0.072]
	γ_{rural}	-0.287	[0.504]	-0.272	[0.126]*
	ρ_1	-0.255	[0.150]†	0.239	[0.116]*
	ρ_0	0.043	[0.194]	0.220	[0.183]
	Test joint sig. γ, $\chi^2(5)$	6.57		10.86†	
	Test $\rho_0 = \rho_1$, $\chi^2(1)$	2.27		0.01	
Junior Secondary	γ	1.049	[0.431]**	-0.552	[0.243]*
N=27,840	$\gamma_{1-25\,pctile}$	-0.157	[0.169]	-0.064	[0.089]
ρ=-0.326**	$\gamma_{25-50\,pctile}$	-0.024	[0.164]	-0.046	[0.090]
	γ_{female}	-0.097	[0.136]	-0.104	[0.071]
	γ_{rural}	0.112	[0.158]	-0.124	[0.084]
	ρ_1	-0.541	[0.183]**	0.421	[0.111]**
	ρ_0	-0.445	[0.251]†	0.688	[0.185]**
	Test joint sig. γ, $\chi^2(5)$	13.65*		23.52**	
	Test $\rho_0 = \rho_1$, $\chi^2(1)$	0.15		2.48†	
Senior Secondary	γ	0.788	[1.310]	-1.138	[0.541]*
N=17,911	$\gamma_{1-25\,pctile}$	0.079	[1.145]	0.090	[0.184]
ρ=-0.261**	$\gamma_{25-50\,pctile}$	0.066	[0.709]	-0.094	[0.152]
	γ_{female}	-0.302	[0.549]	-0.192	[0.140]
	γ_{rural}	0.051	[0.898]	0.220	[0.162]
	ρ_1	-0.150	[0.410]	0.512	[0.224]*
	ρ_0	-1.670	[0.668]*	1.738	[0.424]**
	Test joint sig. γ, $\chi^2(5)$	3.05		12.39*	
	Test $\rho_0 = \rho_1$, $\chi^2(1)$	6.13*		9.96**	

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

[1] Bootstrapped standard errors with 100 replications

Table 15: Average effects of JPS scholarships on school attendance and child labour, conditional on net enrolment (equations (11a) and (11b))

Level	Sub group	School attendance		Child labour	
		ATE	[s.e.][1]	ATE	[s.e.][1]
Primary	Average	0.013	[0.005]**	−0.018	[0.013]
	1-25 percentile	0.011	[0.006]†	−0.022	[0.016]
	25-50 percentile	0.017	[0.006]**	−0.023	[0.010]*
	50-100 percentile	0.013	[0.005]*	−0.012	[0.013]
	Male	0.014	[0.005]**	−0.022	[0.015]
	Female	0.012	[0.005]*	−0.015	[0.011]
	Urban	0.012	[0.003]**	−0.001	[0.008]
	Rural	0.014	[0.006]*	−0.025	[0.015]†
Junior Secondary	Average	0.023	[0.004]**	−0.077	[0.016]**
	1-25 percentile	0.028	[0.007]**	−0.106	[0.022]**
	25-50 percentile	0.026	[0.005]**	−0.085	[0.018]**
	50-100 percentile	0.021	[0.004]**	−0.063	[0.015]**
	Male	0.023	[0.004]**	−0.085	[0.020]**
	Female	0.024	[0.005]**	−0.069	[0.013]**
	Urban	0.013	[0.003]**	−0.030	[0.008]**
	Rural	0.030	[0.005]**	−0.106	[0.022]**
Senior Secondary	Average	0.022	[0.048]	−0.101	[0.028]**
	1-25 percentile	0.038	[0.036]	−0.130	[0.043]**
	25-50 percentile	0.028	[0.048]	−0.121	[0.030]**
	50-100 percentile	0.018	[0.051]	−0.091	[0.027]**
	Male	0.024	[0.032]	−0.112	[0.034]**
	Female	0.021	[0.065]	−0.090	[0.023]**
	Urban	0.016	[0.052]	−0.062	[0.016]**
	Rural	0.030	[0.045]	−0.151	[0.046]**

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

The calculated average effects are based on estimation results reported in table (14)

[1] Bootstrapped standard errors with 100 replications

B Figures

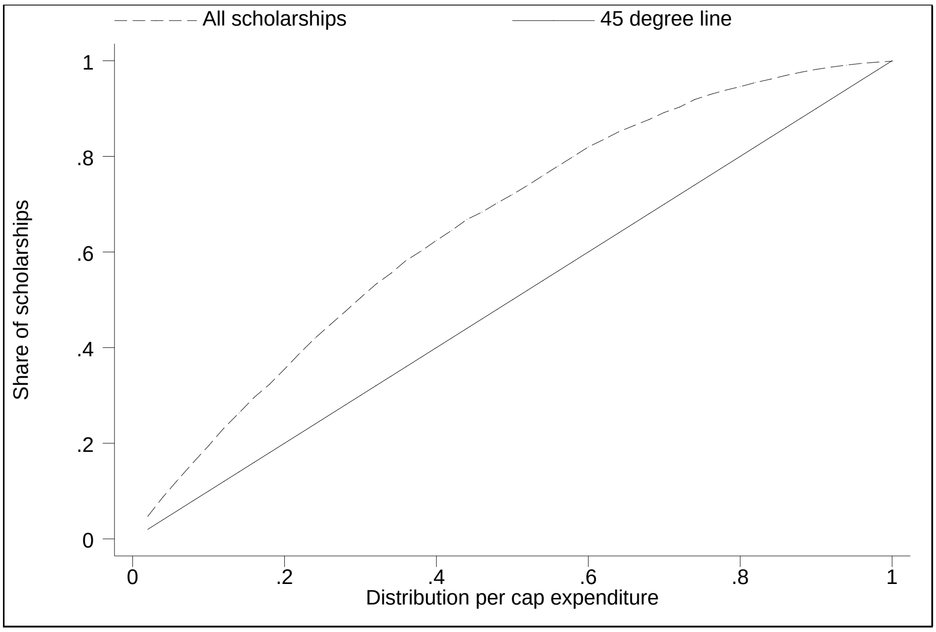

Figure 1: Concentration curve for scholarship allocation.

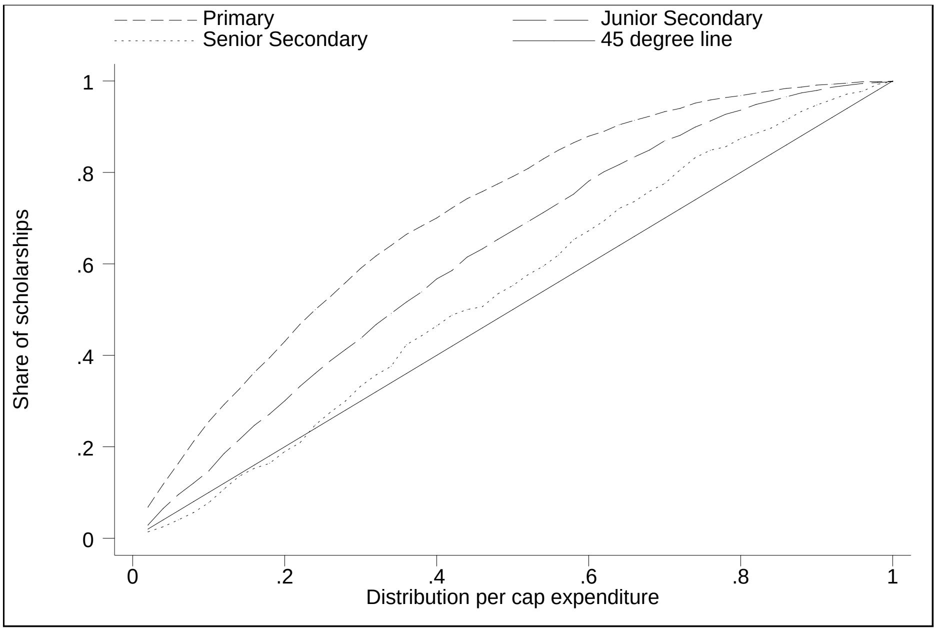

Figure 2: Concentration curves for scholarship allocation, by enrolment level.

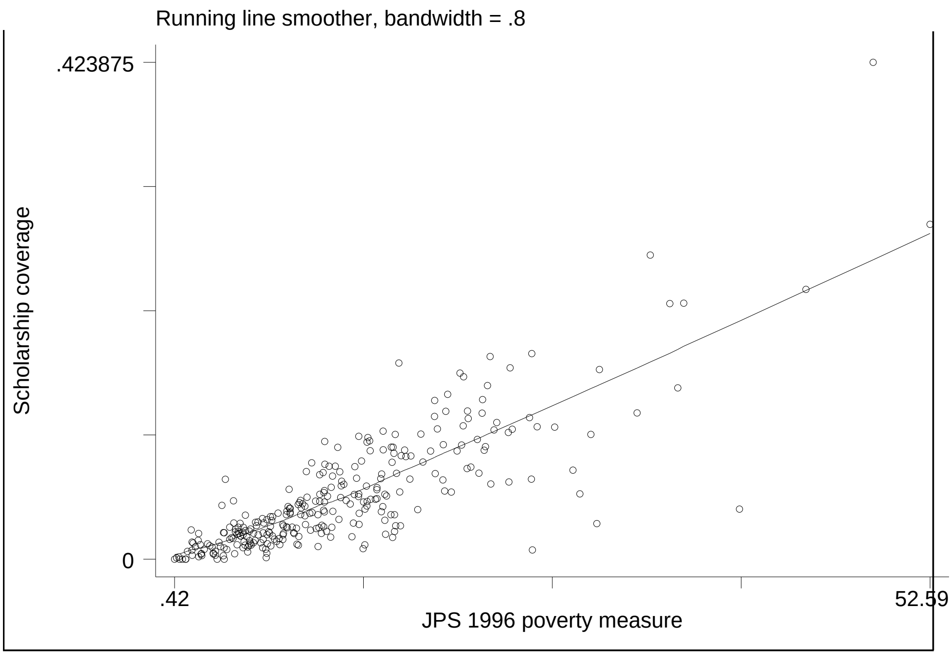

Figure 3: Correlation between $JPS96_j$ and scholarship coverage ($\overline{T}_j$).

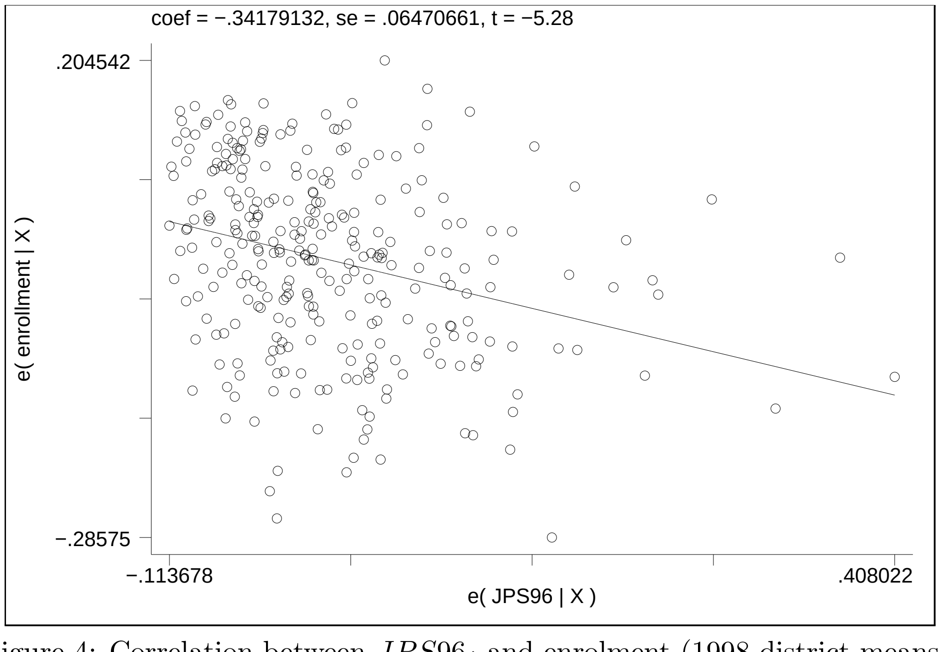

Figure 4: Correlation between $JPS96_j$ and enrolment (1998 district means).

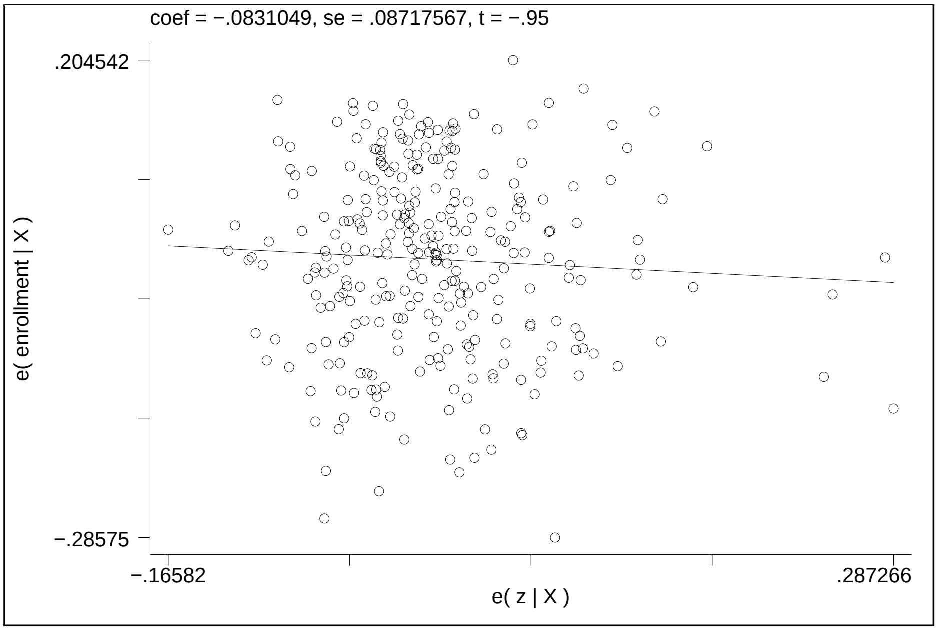

Figure 5: Correlation between $\hat{z}_j$ and enrolment (1998 district means).

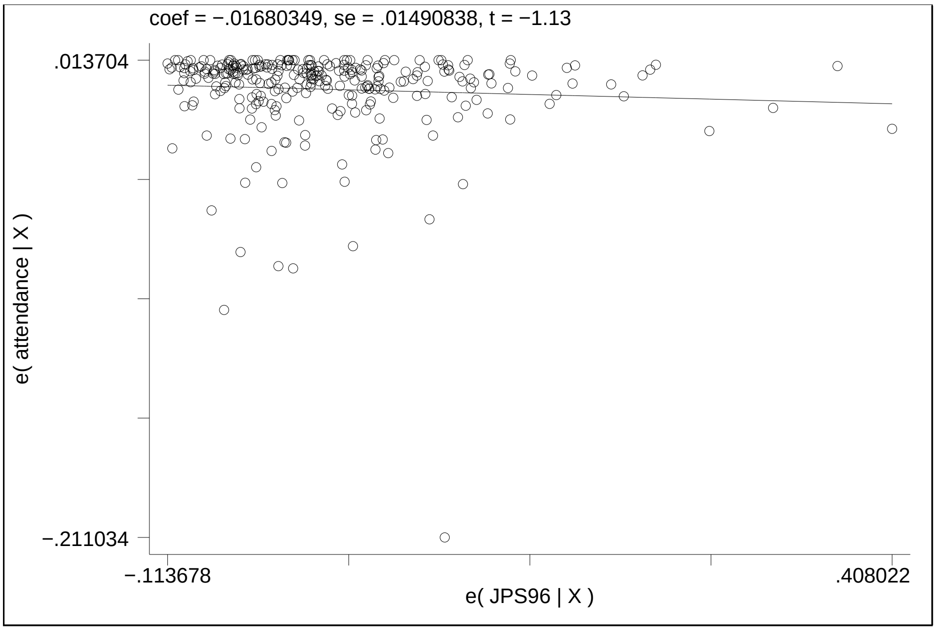

Figure 6: Correlation between $JPS96_j$ and school attendance (1998 district means).

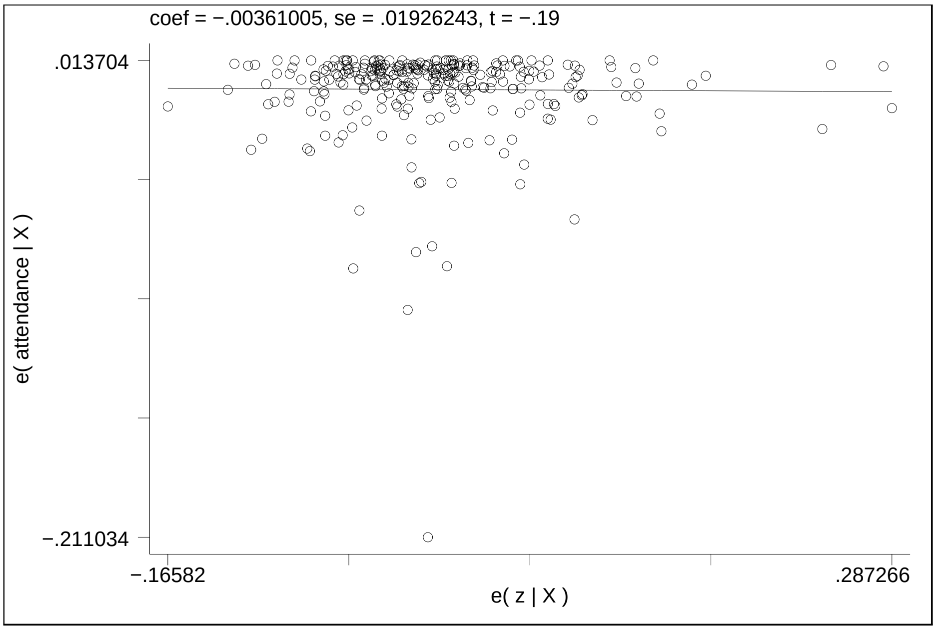

Figure 7: Correlation between $\hat{z}_j$ and school attendance (1998 district means).

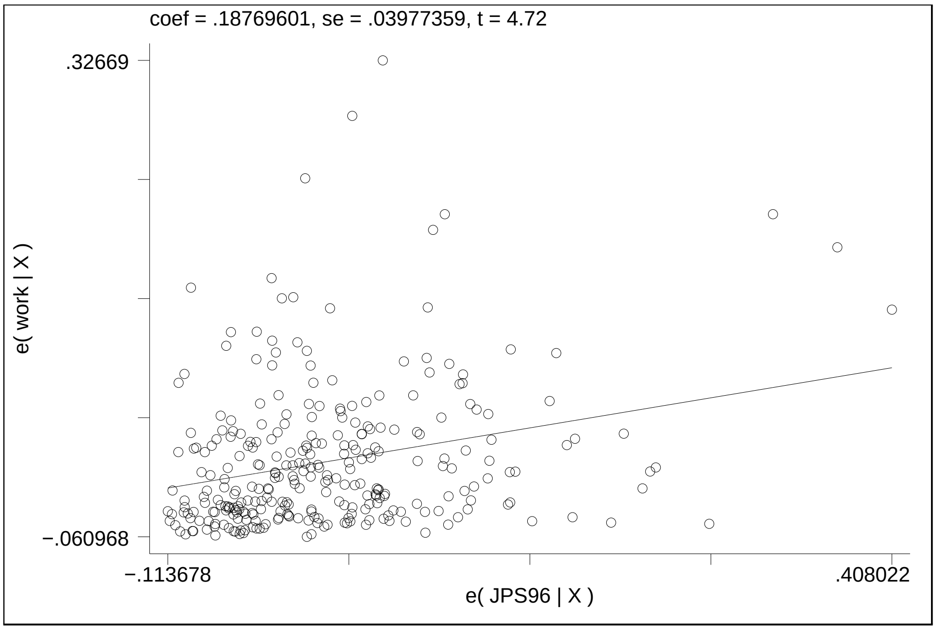

Figure 8: Correlation between $JPS96_j$ and child labour (1998 district means).

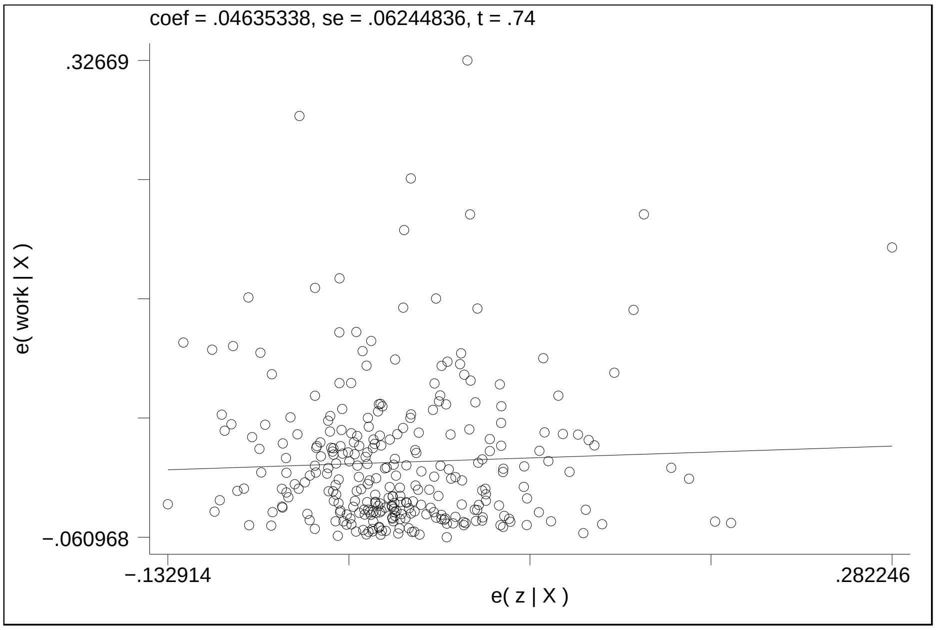

Figure 9: Correlation between $\hat{z}_j$ and child labour (1998 district means).

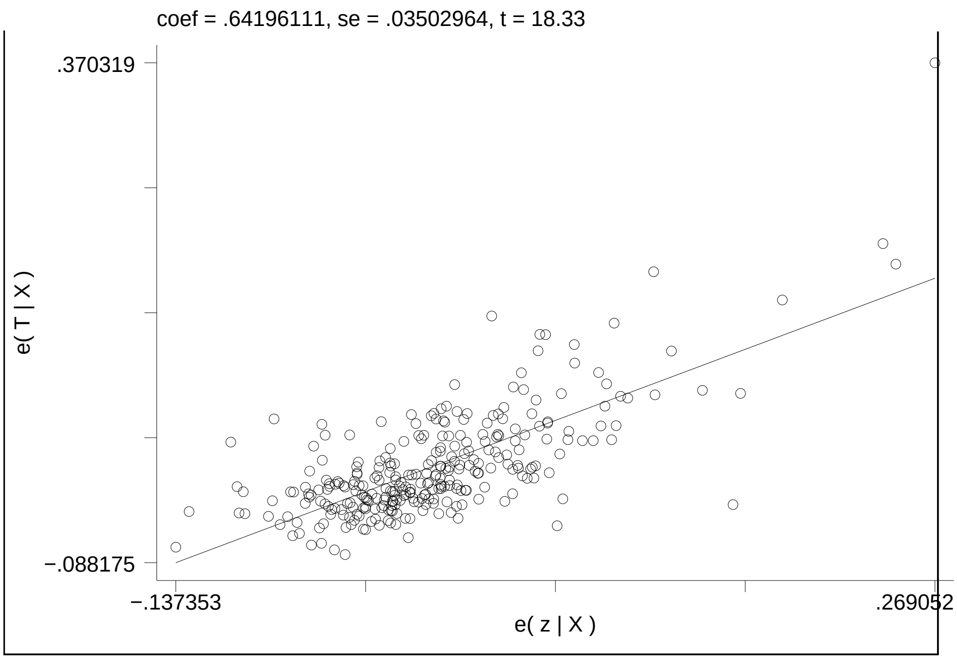

Figure 10: Partial regression leverage plot for $\hat{z}_j$ in first stage regression of overall enrollment.