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Targeted Programs in an Economic Crisis: Empirical Findings from the Experience of Indonesia

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Abstract: In response to the economic, natural, and political crisis that enveloped Indonesia from August 1997 (beginning of depreciation) to May 1998 (Soeharto resignation), the new government announced support for a set of "safety net" (JPS) programs in the July 1998 budget. These included: (a) targeted sales of subsidized rice, (b) work creation programs, (c) scholarships to students and block grants to schools, (d) targeted health care subsidies, (e) community block grants. Cross sectional and panel data has been used to examine the targeting of these programs. First, "static participation incidence" (the relationship between program participation and household consumption expenditures) was substantially better than a uniform transfer, but substantially worse than perfect targeting, and remarkably similar for all of the JPS programs. Second, unlike standard static incidence measures, what we define as "dynamic participation incidence" — the relationship between changes in consumption expenditures and program participation — was very different among the JPS programs. The employment creation programs, which relied on self-selection targeting, were much more likely to reach those households experiencing large shocks to their expenditure than programs based on administrative targeting such as subsidized rice sales, scholarships, and health subsidies. Third, larger coverage does not lead to either better or worse targeting: There was no general tendency across the programs for marginal incidence to be above, or below, average incidence. Fourth, the targeting designs of many of the programs were not followed strictly during implementation of the programs. In practice, community and individual characteristics — that were *de jure* irrelevant — played a role in targeting. In the sales of subsidized rice program, community influence led to the program going to many more than the eligible individuals. In other programs, individual characteristics appear to have influenced targeting.

[#] This paper is a synthesis that draws on multi-year research program of the SMERU Research Institute and the World Bank Office in Jakarta. We draw on papers we have co-authored with Emmanuel Skoufias, Yusuf Suharso, Wenefrida Widyanti and on a large body of quantitative and qualitative work of SMERU as well as collaboration and conversations with Vivi Alatas, Lisa Cameron, Deon Filmer, Ben Olken, and Menno Pradhan. We are grateful to BPS and UNICEF for use of the data. We would like to thank Daniel Perwira for research assistance and Rachael Diprose for editing the manuscript.

I. INTRODUCTION

Whether as the result of financial/currency crisis (Thailand, Korea, Indonesia 1997; Russia, Ecuador 1998; Brazil 1999; Turkey 2001; Argentina 2002), delayed systemic transformation (the countries of the FSU), or domestic difficulties (Zimbabwe 2001), a large number of countries have experienced macroeconomic crises. The precipitous falls in macroeconomic aggregates are reflected in negative impacts on individuals — both as households' incomes fall and poverty rises, and as public spending falls reducing available services for the poor.[1] An increasingly frequent response to macroeconomic crisis is the attempt to mitigate the worst consequences of these shocks through crisis "safety net" programs.[2] Indonesia was no different and launched a series of crisis programs known as the JPS (*Jaring Pengaman Sosial)* programs.

This paper draws on several recent household data sets to present four empirical findings about the *targeting* of these Indonesian crisis programs.[3] Within the extensive literature on the many aspects of targeting and benefit incidence of government programs,[4] this paper has three unique features. First, we address the question of the targeting of *crisis* programs that were created deliberately to address the consequences of a specific economic shock. Second, we are able to use multiple data sources, including a panel data set spanning the crisis, to cross validate findings. Third, we are able to make comparisons across the set of programs.

After providing brief background information on the crisis, we describe the targeting design of five major programs: subsidized rice, student scholarships, health care subsidies, employment creation, and community block grants in the next chapter. Each of the following four chapters examines our empirical findings.

[1] See Ravallion (2002a) on fiscal incidence of contractions.

[2] See Ferreira, Prennushi, and Ravallion (1999).

[3] With our focus in the paper on targeting, we do not address other important aspects of program design and evaluation. Design issues such as the match of administrative capacity to program complexity, aligning implementing agency interests to program design, the ease of information dissemination (issues in which economists have no special comparative advantage) are crucial to the success of crisis programs, but these have been addressed elsewhere (e.g. World Bank, 2001). Assessing program impact on household outcomes (consumption, health, school drop-out) depends on the usual difficult issues of identifying the counter-factual (no program) outcome, which requires a separate treatment that both we (Suryahadi *et al.*, 2002) and others (Cameron, 2002 for education; Saadah, Pradhan, and Sparrow, 2000 for health) have addressed elsewhere.

[4] Grosh (1994) on administrative aspects, van de Walle and Nead (1995) is a good overview volume on the issue of targeting. The output of the cottage industry calculating benefit incidence and targeting is well reviewed in Coady, Grosh, and Hoddinott (2002).

II. THE DESIGN AND TARGETING OF THE INDONESIAN CRISIS JPS PROGRAMS

A. Crisis, Impact, and the Launch of the JPS

After nearly thirty years of uninterrupted rapid growth, low inflation, and a stable currency, in August of 1997 Indonesia's currency began to slide in what at first appeared to be only the spillover from the crisis in Thailand. But by May 1998 the country was suffering from the combined effects of a currency, financial, natural, economic, and political crisis. The currency collapsed in waves, from its pre-crisis level of Rp 2,200 to the dollar in mid 1997 to Rp 5,000 by October, to Rp 6,000 by December, to a free fall in January 1998 (following the almost immediate collapse of the second ill-fated IMF program) which took the currency as low as Rp 17,000 per dollar. The effect of the currency devaluation on the substantial unhedged foreign currency denominated borrowing by both the domestic financial and corporate sectors (on top of underlying structural weaknesses of the sector) created a financial crisis. The fear of widespread banking collapse caused the Central Bank to issue a blanket guarantee of inter-bank loans in January 1998 which, in turn, spurred the money supply to nearly triple between early 1998 and late 1999 (Deuster, 2002). In addition nature was unkind as fires burned out of control in large sections of Sumatra in the fall of 1997 and a drought reduced the primary rice crop.

The combination of these impacts caused the economy to contract by an almost unprecedented magnitude — real GDP fell 13.7 percent in 1998 and inflation skyrocketed. The money supply expansion and currency depreciation caused skyrocketing domestic prices particularly for food (since food is a tradable it was more affected by the depreciation — the general inflation rate was 78 percent in 1998 while food prices escalated by 118 percent). All of this, combined with signs of weakness and ill health from Soeharto, led to a political crisis. Student deaths and rioting in the capital Jakarta and several other cities led to the May 1998 resignation of Soeharto, who had been in power since the mid 1960s.[5]

The social impact of the crisis was both immediate, as well as substantial, and is still evolving today. Real wages of formal sector workers fell by around one third between August 1997 and August 1998 before beginning to recover in 1999 as nominal wages began to grow. Given the flexible labor market dominated by informal and self-employment, officially measured open unemployment was never really the issue; it rose only from 4.7 in August 1997, to 5.5 in 1998, and to 6.4 in 1999.[6] One large scale household survey, the "100 Villages Survey" study (see detail below), showed

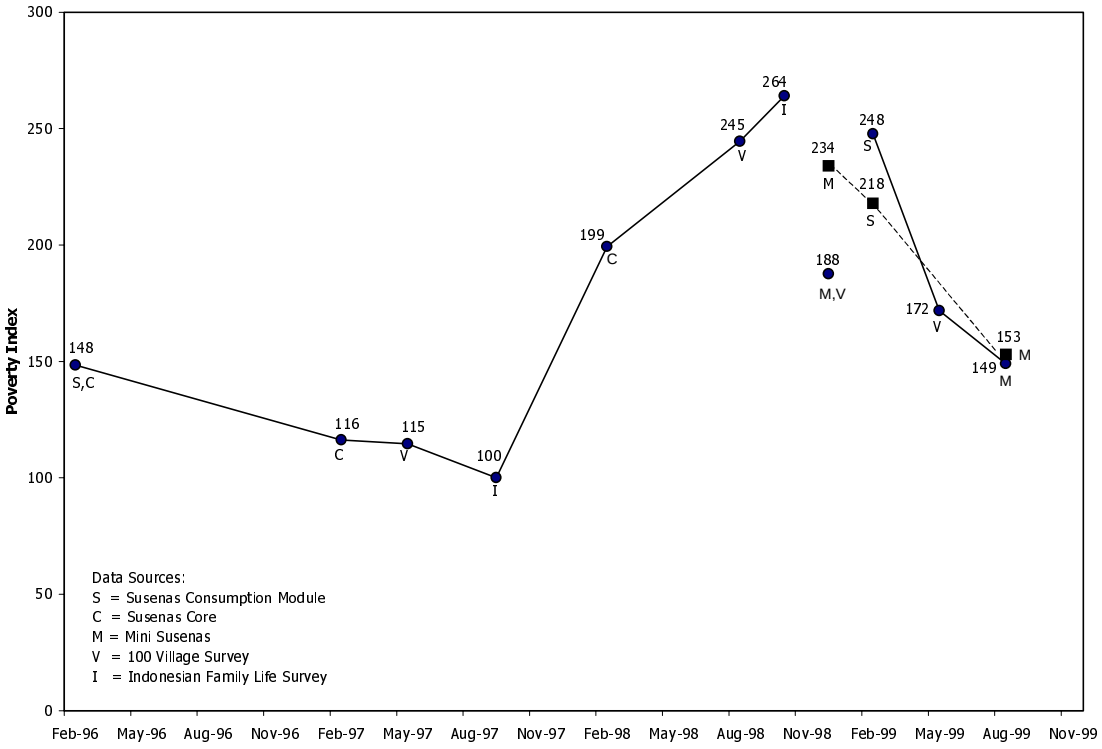
[5] The story of the Indonesian economic crisis and its possible proximate and deep causes has been told many times in academic (e.g. McLeod, 1998), official (e.g. World Bank, 1999), and journalistic (e.g. Blustein, 2001) accounts.

[6] See Feridhanusetyawan (1999), Manning (2000), Papanek and Handoko (1999).

real per capita consumption expenditures falling 17 percent between May 1997 and August 1998.[7]

Our attempt to piece together a consistent series of data, on the headcount measure of absolute consumption expenditure (ACE) poverty from all the various sources suggests ACE poverty increased by 164 percent from the onset of the crisis in mid 1997 to its peak at the end of 1998 (Suryahadi *et al.*, 2000).[8]

Figure 1: The Evolution of the Headcount Poverty Rate in Indonesia, February 1996 to November 1999 (Sept. 1997=100)



Source: Suryahadi *et al.*, 2000

[7] The reconciliation of the national accounts fall in "real" per capita expenditures (PCE) and the household survey based measures is due to the enormous shift in relative prices, which implies that deflators which used a small share for food such as the CPI and the GDP deflator showed small "real" falls, while those using food shares for the poorer households showed much larger "real" falls.

[8] In July 1998 there was considerable debate as estimates of the crisis increase in ACE headcount poverty rates ranged from as high as 30 to as low as 3 percentage points. These attempts at "real time" estimates suffered from a variety of methodological problems (see Poppele, Sumarto, and Pritchett, 1999).

In the face of the economic and political instability and the enormous (and growing) social consequences, there were both pragmatic and benign motivations for/within the programs. First, both the new government and the international financial institutions and bilateral agencies needed to be seen to be responding pro-actively to the impacts of the crisis in order to sharply differentiate themselves from the past. Second, there was a desire to mitigate the impacts of the shock on both households and communities. Third, many were interested in protecting certain key social services, notably health and education. Fourth, there were Keynesian motivations to sustain aggregate demand both nationally and locally to halt the collapse in output. Finally, there were some who wanted to use the crisis and the new program to reorient government attention to poverty. The outcome of the mix of those motivations and the constraints on program design imposed by availability of data produced a set of programs with varying targeting design.

The resignation of Soeharto in May 1998 created three inter-related difficulties: the budget approved for the fiscal year running from April 1998 to March 1999 was no longer operative, there needed to be a new IMF program approved (which required a new budget), and the IMF program required the arrangement of external financing of the government deficit. In July 1998 such a budget was produced, which contained (within the new FY 1998/99 budget) a line item for "safety net" or JPS (*Jaring Pengaman Sosial)* programs.[9] The magnitude of the budget for the JPS programs was not the result of costing out programs or based on any historical baseline but was an amount determined by macroeconomic, political, and financing forces with which programs could be designed. This meant that programs needed to be designed. However, the Indonesian people had never relied to any significant extent on government safety net programs as prior to the crisis. The country had neither the economic apparatus nor the political mechanisms (nor the inclination) required to deliver large scale transfer programs all over the archipelago.[10] Therefore, establishing crisis programs in Indonesia in 1998 did not merely mean expanding an existing net, but casting an entirely new one.

[9] We will refer to the programs by the Indonesian acronym JPS because this is more neutral than the term "safety net" — and as we dislike the "safety net" metaphor, this will save us the constant use of scare quotes.

[10] Before the crisis, Indonesia was one of the most rapidly growing economies in the world, where "official" absolute consumption expenditure poverty fell by almost 50 percentage points (from 60 to 11 percent) between 1970 and 1996. The general poverty strategy of the previous government was: (a) social spending, largely focused on the provision of 'social services' such as health, family planning, and education, (b) "development" programs that aimed at poverty reduction through increasing productivity (such as credit subsidies, left-behind villages program (IDT), etc), (c) some small programs for very limited disadvantaged groups (e.g. disabled, orphans), and (d) family and communities providing 'mutual social insurance' in times of difficulty (there was some subsidized health care, compulsory social security program, and unemployment protection for formal sector employees but this was of very limited reach).

B. Targeting the JPS Programs: Data Availability

Program targeting design needs to give a complete specification of how resources will flow from the public sector budget to the program participants. There are two basic dimensions of program targeting — the *geographic* scope and allocation of the budget to the relevant implementing jurisdictions of the government; and the mechanisms for the choice of specific *households/individuals* which will participate.

Amongst developing countries, Indonesia is a data rich environment. It has an annual household National Socio-Economic Survey (the SUSENAS), National Labor Force Survey (the SAKERNAS), Survey of Manufacturing, Population Census, Agricultural Census, Economic Census, Village Census (the PODES), and so on. It has also a functioning administrative apparatus that reaches into every rural village/urban neighborhood in the country. However, while in this data rich environment there was relatively recent information about the *levels* of various regional and household indicators of well-being, there was no *timely, complete, and administratively acceptable* data on the *impact of the shock* that would have allowed accurate allocation of the JPS either to regions or individuals.

Regional allocations. At the time the JPS programs were being designed, it was increasingly clear that the regional impact of the crisis was heterogeneous — and that there was little connection between the regional distribution of the impact of the shock and the regional distribution of pre-shock poverty.[11] In particular, urban areas on the island of Java — which were among the wealthiest areas before the crisis — were the epicenter of the financial and modern sector crisis. In contrast, traditionally poorer natural resource exporting areas actually benefited from the crisis.[12] Because the crisis affected food prices, the impact on real wages and living standard spilled over from urban to rural areas — but then differentially affected different cropping areas (and within areas landed versus non-landed households).

The Indonesian budget process required that the actual amounts to be allocated to each region be decided at the time the project design was approved. *No* money could be spent on a project until the regional allocations were made. The indicators of the regional severity of the crisis that were timely, were either not complete or administratively unacceptable. Data showing that particular areas were hard hit, or even that, in general, urban areas were hard hit, was not exactly comparable data for all administrative regions (provinces, districts) and hence could not be used. Finally, given the overall drastic reductions in real budgets (and real wages), the regional allocation of the JPS budget was hotly contested by the regional governments and it was impossible to use anything other than administratively generated data.

Individually available data. In economies dominated by agriculture, self-employment, and informal employment, there is no reliable administrative data on current income.

[11] This was documented with qualitative data from a nationwide "rapid response" survey even as early as October 1998 (see Sumarto, Wetterberg, and Pritchett, 1998).

[12] On some of the outer islands, the common reaction to queries to the impact of the crisis was *hidup krismon* (long live the monetary crisis) and there were anecdotes of motorbike dealers shipping their urban inventories to (some) rural areas to meet new demand.

For those with formal sector employment, one can talk about "losing a job" or "becoming unemployed", but this is not a key determinant of income or consumption changes. Indonesia did in fact have a household list on something like poverty that was complete and administratively acceptable — but was not timely in that it was about levels of long-run "prosperity", not the impact of the crisis.

The National Family Planning Coordinating Agency (BKKBN) in Indonesia classified every household in Indonesia into one of four levels of "prosperity" (BKKBN 1994). A household was in the lowest welfare category ("pre-prosperous") if any one of the five statements was true: (i) the household cannot practice their religious principles, (ii) all household members do not eat at least twice a day, (iii) not all household members have different sets of clothing for home, work, school, and visits, (iv) the household cannot seek modern medical assistance for sick children and family planning services for contraceptive users, (v) the largest floor area of house is made of earth. This list had been compiled and updated annually by the village level workers (*cadres*) of the family planning agency for use in targeting contraceptive subsidies and efforts.[13] Prior to the advent of the crisis programs there was little incentive for either individuals to be classified as family planning poor (or for regional governments to have a larger fraction of poor).

C. The JPS Programs and Their *De Jure* Targeting Design

The outcome of the mixed motivations, political pressures, and targeting constraints outlined above was a set of JPS programs, each with its own objectives and targeting criteria. The Indonesian government's approach was to group the JPS effort in five major areas: food security, employment creation, education, health, and community empowerment. Table 1 summarizes the objectives, intended magnitude, and targeting of the major JPS programs, which we describe briefly before moving on to the four sections that describe the targeting results.

[13] There is considerable uncertainty about how the *cadres* actually compiled the list (even after several discussions with the *cadres* in various regions). It was supposed to be based on household visits, but to say it was based on a "survey" overstates the formality and rigor of the process. Also the list was not centralized or computerized, but the list of household names was maintained at the local level and the higher levels of government only had access to summary reports of the numbers of households in each group.

Table 1. Summary of JPS programs

Program	Description	FY 98/99 Budget billion Rp. (US$ 100,000)[a]	Planned coverage	Targeting in Fiscal Year 1998/99	
OPK "subsidized rice"	Sales of subsidized rice to targeted households	5,450	12.8 million KPS and KS-I households	Geographic	None
				Household	BKKBN list
Padat Karya programs "labor creation"	A loose, uncoordinated, collection of several 'labor intensive' programs in a variety of government departments	2,066	12.7 million man-days	Geographic	None, various ministries (e.g. Manpower, Forestry, Public Works)
				Household	Weak self selection (by wage rate, but varied)
SBG "scholarships"	Providing scholarships directly to elementary, lower secondary, and upper secondary students and block grants to selected schools	1,138	6% of primary, 17% of lower secondary, 10% of upper secondary school students	Geographic	Data on enrollment in 1997
				Household	School committees following criteria
JPS-BK "health cards"	Providing subsidies for medical services, operational support for health centers, medicine and imported medical equipment, family planning services, supplemental food, midwife services	1,043	7.4 million KPS households	Geographic	BKKBN pre-prosperous rates
				Household	BKKBN list
PDM-DKE "village block grants"	A 'community fund' program that provides block grants directly to villages for either public works or revolving fund for credit	1,701		Geographic	Pre-crisis (1997) data on poverty rate by district
				Household	Local decision making

a) US$ figures are indicative only, calculated at an exchange rate of Rp 10,000/US$ dollar.

Subsidized rice. The largest program was a program called OPK (*Operasi Pasar Khusus)*[14] that allowed each eligible household to purchase 20 kilograms of rice at the price of Rp 1,000/kg (in August 1998 the market price for medium quality rice was around Rp 3,000/kg).[15] Households were eligible if they fell into either of the two lowest categories of the family planning agency's "prosperity" ranking.[16] A village representative was authorized to purchase each month an allotment of rice equal to 20 kilograms times the number of eligible households in that village from the logistic agency's (BULOG) local warehouse. Households then purchased this rice in the village at the stipulated price (with some allowance for transport costs).

Labor creating programs. Unlike the above programs that had centralized design and implementation, the "labor creating" programs were really a diverse set of programs operated by different ministries and with different criteria — at one point there were seventeen different "labor creating" programs. The unifying features were that individuals were paid for labor services and that the principal targeting was geographic and based on self-selection — individuals chose whether or not to work for the specified wages. Ferreira, Prennushi, and Ravallion (1999) stress that the impact was highest when the programs used a relatively low wage rate, as this ensures that only those in need work (people will take, and return to, regular employment when available) and that as many people as possible can be employed. This was not always the case in the programs and there was wide variability — the drought relief programs tended to pay wages in kind, the urban construction programs tended to have high wages, and many programs used the government legal minimum wage, which was higher than the market in some areas while lower in others.

Scholarships and block grants. This program had two major components: scholarships to individual students and block grants to schools, which, although it was an important part of the program, we do not examine its targeting issues.[17] Scholarships were provided for three levels: primary (SD), lower secondary (SLTP), and upper secondary school (SMU). The scholarship amount was substantial (Rp. 10,000, Rp. 20,000, and Rp. 30,000 per month for primary, lower secondary, and upper secondary school students respectively). This was intended to exceed both official and unofficial school fees. The scholarships were paid directly to the students (or their families) twice a year via a cash transfer handled by the local post office.

[14] This means "special market operation", which was meant to distinguish these sales of rice by the logistics agency from their traditional market operations in which they bought and sold rice to stabilize prices.

[15] See 'Recent Volatility in the Rice Market: Results of a SMERU Rapid Appraisal in Central and East Java', *SMERU Newsletter*, No. 01, November 1998.

[16] Originally only the lowest category (KPS) was eligible. In response to reports that due to the prolonged economic crisis many KS-I households had fallen into KPS, the government expanded eligibility to include KS-I households. In effect, this increased the number of target from 7.3 to 12.8 million households (see Rahayu *et al.*, 1998).

[17] Since the block grants were targeted to schools, at least conceivable we could examine the incidence of this program by examining the children who were in schools who received block grants — but households were not asked this in any of the available data sets.

Each school received a number of scholarships, which they allocated to students by a school committee, consisting of the principal, a teacher representative, a student representative, the head of parent's association (BP3, as the representative of the local community), and the village head. The scholarship recipients were selected according to their score on an administrative criteria, the main element of which was that the child came from a household in the lowest two categories of the family planning "prosperity" status, but which also included the size of the family, the likelihood of a student dropping out, and the constraint that 50 percent of the scholarships went to girls. The decisions had to be approved by the committee.[18]

Health. One major health program was the provision of a health card (*kartu sehat*) to eligible households which entitled all members of the household to obtain free services from designated public health care centers for medical and family planning purposes, and services for ante-natal and child birth services from designated providers.[19] Household eligibility in this program was based on village level lists, which primarily encompassed family planning prosperity status, with some modifications by local administration via a "health committee". While health services were to be provided free, the health units were not reimbursed for the provision of those services. Rather there was a complicated financing scheme where, among other features, local clinic funds received through the program were based on the number of health card holders in their administrative area and could only be used for certain items.

[18] The criteria were explicit and disseminated to the schools. In principle, students selected to receive the scholarships were supposed to be from the poorest backgrounds. As a guidance, scholarships were to be allocated to children from household in the two lowest BKKBN rankings at first. If there were a large number of eligible students such that not all of the poor students could receive a scholarship, then additional indicators were used to identify the neediest students. These additional indicators included living far from school, having physical handicaps, and those coming from large or single parent families. The only subjective input was school committee's assessment of the recipients' probability to drop out of schools without scholarships.

[19] This was primarily the primary health care clinics (*Puskesmas)* but also included *some* but not all services at a hospital. This aspect of all services at some levels but only some services at others created a fair bit of confusion as health card holders felt they were being denied or charged for what should have been free services.

D. Data Sources Used to Examine Targeting: Program Participation and Consumption

We used two principal sources to examine the targeting of the JPS programs: the national household survey (SUSENAS) and a panel survey carried out by BPS and UNICEF called the "100 Villages Survey". The SUSENAS is a nationally representative household survey covering over 200,000 households in each of the countries 341 districts.[20]

The 100 Villages Survey is a panel with five rounds: May 1997 (pre-crisis), August 1998, December 1998, May 1999 and October 1999. The May 1997 round of the 100 Village Survey interviewed 120 households in each of ten villages in ten districts purposively chosen to represent various disadvantaged economic types (e.g. fishing villages, remote). In subsequent rounds 80 of the original 120 households in each village were re-interviewed to create a panel of around 8,000 households.[21]

JPS module. A module which asked households about their awareness of, participation in, and benefits from the JPS programs was fielded in December 1998 round of the 100 Villages Survey, and then a substantially expanded version was included in the February 1999 SUSENAS. The module, particularly in the 100 Villages Survey, asked whether households reported "participating" or "receiving benefits" from the JPS programs in the last three months.

Consumption module. The "short form" questions on consumption expenditures were used in both the core SUSENAS as well as the 100 Villages survey, and the consumption expenditures aggregate based on these questions was used throughout, with the exception in the footnote.[22]

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[20] For the five levels of the administrative jurisdictions of Indonesia as of February 1999, we use the words: national, provincial (26, excluding East Timor), district (341), sub-district (4,044), village (roughly 68,783). Unless otherwise noted "districts" denotes the "level II local government" which includes both *kabupaten* (rural districts) and *kota* (cities that are districts). Unless otherwise specified, "village" includes both rural *desa* and urban *kelurahan* (note the Indonesian *desa* is not a "village" in the sense of a self-contained cluster of residences but is an administrative jurisdiction that may include several such clusters).

[21] In the original survey the 120 households were 60 from each of two census enumeration areas within the village. In the later rounds this was changed to 40 from each of three enumeration areas. Forty panel households in each enumeration area were chosen from the sixty original households, but by a process in which attrition was not documented: if a panel selected household was not present then another was chosen to have a complete panel. Since the rounds are relatively close together we believe that actual attrition was small but the caveat persists that the results are for those households located in subsequent rounds and are hence not representative of all households.

[22] There was actually a big problem with the SUSENAS data because in 1999 the detailed "consumption module" was administered to 65,000 households. While in theory these households also received the short-form consumption questions, it turns out that those households who received both the short form *and* detailed module set of questions had much higher recorded consumption than those households only asked the core questions (suggesting the prompted recall of the detailed module produced higher consumption and the enumerators translated this over into the matching categories of the short-form). Therefore we create separate quintiles for core and module households (see Sumarto, Suryahadi, and Widyanti, 2001).

III. JPS PROGRAM PARTICIPATION INCIDENCE: (MILDLY) PROGRESSIVE, REMARKABLY UNIFORM

A. Empirical Findings

Since we cannot estimate the actual magnitude of the benefit from each of the programs, we focused on "participation incidence." We define a program's (*static) participation incidence* as the relationship between program coverage and household per capita expenditures. The coverage ratio (CR) of the j^{th} JPS program in the i^{th} region for the q^{th} group of consumption expenditures (usually quintiles or deciles or, more broadly, any range of centiles such as poor vs. non-poor) is the fraction of those households in the region in the given group who report having participated in the program in the recall period.

$$CR_{\text{Program } j,\text{region } i,\text{quintile } q} = \frac{\sum_h I[h \in \textit{Participated in } j^{th} \textit{ JPS program and } h \in Q_{q,i}]}{\sum_h I[h \in Q_{q,i}]}$$

We compute the coverage ratio separately for each district. This is for two reasons. First, we wish to avoid comparisons of "real" income across districts which, given the lack of comparable price data across urban and rural areas in Indonesia, we regard as still problematic. Second, rather than conflating the issues of how well the geographic targeting identified regions and how well within regions the programs reached poorer household, we only examine how well programs targeted households relative only to other households within those regions. The summary statistics are not the national average targeting, but the average of targeting within districts.

Given the coverage ratios across the quintiles we can calculate three summary indicators of participation incidence that reflect consumption expenditure targeting. First, the ratio of the coverage of the middle to the poorest quintile:

$$\frac{Q_{III}}{Q_I} = \frac{CR_{j,i,Q^i_{III}}}{CR_{j,i,Q^i_I}}.$$

Second, the same ratio of coverage ratios for the richest (QV) to poorest (QI) quintile. Third, we define the "targeting ratio" (TR) as the ratio of the fraction of the benefits of a program received by the non-poor relative to the fraction of the non-poor in the population:

$$TR_{\text{Program } j,\text{ region } i} = \frac{\textit{Fraction program } j \textit{ benefits to non} - \textit{poor}_i}{\textit{Fraction population non} - \textit{poor}_i}$$

In terms of coverage ratios and population shares (S) the targeting ratio is:

$$TR_{\text{Program } j,\text{ region } i} = \frac{\left(\frac{CR_{non-poor,j,i} * S_{non-poor,i} CR_{non-poor,j,i} * S_{non-poor,i}}{CR_{All,j.i}} \right)}{S_{non-poor,i}}$$

This definition is compatible with either an absolute consumption expenditure (ACE) definition of poverty (which produces as an outcome the fraction of the population in any given region poor and non-poor) or with the use of consumption expenditure percentiles. We implement the targeting ratio by defining the bottom 20 percent of households by per capita consumption expenditures in each district as the region's "poor."

If a program were untargeted in its availability, and the uptake was not related to expenditures, then all of the targeting indicators (QIII/QI, QV/QI, TR) would be equal to 1 (on average). We can also define the best *imaginable* targeting indicators (lower is better targeting) as the targeting indicators that would result if program participation were strictly ordered by consumption expenditures. In this case the graph of participation incidence would be step function equal to one up to some threshold level of expenditure (which is a function of total program budget/coverage) and zero thereafter.

Table 2 reports the average coverage and the targeting indicators from the February 1999 SUSENAS — along with the maximum imaginable coverage of the bottom quintile and best imaginable targeting indicators. The targeting glass is definitely either half-full or half-empty. On the half-full side, all of the programs demonstrate some "pro-poor" targeting. A household in the middle quintile was only 59 percent (medical services) to 80 percent (subsidized rice) as likely as a household in the bottom quintile to have received benefits. Households in the top quintile of expenditures were only around 30 percent (medical services, employment creation) to 46 percent (subsidized rice) as likely to have participated in the JPS. The targeting ratios were consistently better than a uniform transfer (which is itself progressive relative to the pre-tax/transfer distribution).

Table 2. Average targeting of various JPS programs across districts of Indonesia

	SUSENAS, Feb 1999				Maximum *imaginable* coverage and best *imaginable* expenditure targeting (uniform transfer=1)			
	Quintile I	Ratio		Targeting	Quintile I	Ratio		Targeting
Program	Coverage	QIII/QI	QV/QI	Ratio	coverage	Q3/qi	Q5/q1	Ratio
Subsidized Rice	52.64	0.79	0.46	0.92	100.00	0.63	0.00	0.78
Employment Creation	8.31	0.70	0.30	0.88	41.55	0.00	0.00	0.00
Primary Scholarship	5.80	0.69	0.35	0.89	29.00	0.00	0.00	0.00
Lower Secondary Scholarship	12.15	0.69	0.40	0.89	60.75	0.00	0.00	0.00
Upper Secondary Scholarship	5.40	0.61	0.36	0.90	27.00	0.00	0.00	0.00
Used Health Card	10.60	0.59	0.29	0.83	53.00	0.00	0.00	0.00
Notes: a) Based on the unweighted average across districts.								

On the half empty side, the lack of perfect targeting meant that coverage amongst the poorest was much less than imaginable. Had the "health card" been strictly targeted by expenditures, 53 percent of the bottom quintile could have received the program — whereas only 10.6 percent of the poorest quintile actually did (and similarly for other programs). The targeting ratios suggest that participation of the non-poor (defined as the top 80 percent of the population) in JPS programs was about 90 percent of their share in the population. About 75 percent of all program participants were not in the bottom 20 percent of the expenditure distribution.

The results in table 2 are averages across districts, which conceal the enormous differences across districts in the measured targeting. While part of this variability is sampling error (the median sample size by district was 606 households), there appears to be substantial variation in the extent to which the programs reached the poorest in each district (we return to this below). Table 3 shows that in each program, roughly half the districts have either uniform or pro-rich targeting while roughly half had targeting substantially better than a uniform transfer.[23]

Table 3. Distribution of targeting ratios across the districts of Indonesia

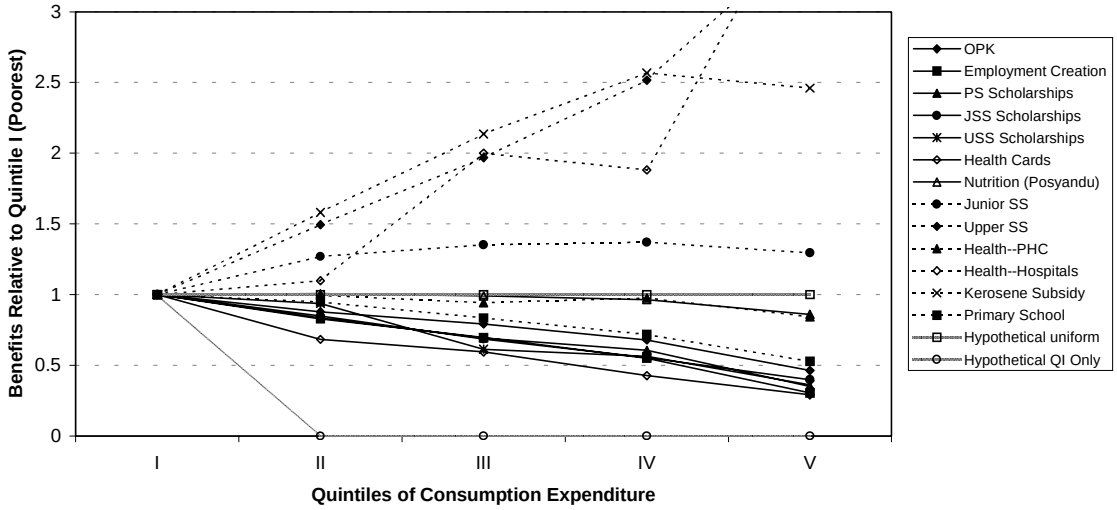
Program	Mean TR	Std Dev	Percent of districts in each targeting ratio class				
			<0.7 (sharply pro-poor)	0.7-0.9 (pro-poor)	0.9-1.1 (near uniform)	>1.1 (anti-poor)	Total
Subsidized rice	0.91	0.10	2.39	38.57	57.00	2.05	100
Employment creation	0.87	0.27	23.02	26.98	32.01	17.99	100
Primary scholarship	0.86	0.26	20.73	31.64	34.55	13.09	100
Lower secondary scholarship	0.86	0.25	23.10	28.16	35.38	13.36	100
Upper secondary scholarship	0.86	0.38	33.47	14.88	14.05	37.60	100
Used Health Cards	0.83	0.29	25.69	35.42	20.14	18.75	100
Source: Author's calculations based on February 1999 SUSENAS.							

Even though the programs had very different administrative and targeting designs, the participation incidence of the various JPS programs appears remarkably similar. While there are differences in the targeting indicators (the range of QIII/QI (middle/poor) ratios is 20 percentage points (0.59 to 0.79)), the JPS program incidence is similar when compared to the benefit incidence of other general,

[23] This heterogeneity of targeting performance across regions with the same project design is similar to the finding in Coady, Grosh, and Hoddinott (2002) that across the range of programs they examine there were large variations across countries within the same type of program.

untargeted, government expenditures.[24] Figure 2 compares the participation incidence of the JPS programs to the spending on education (primary, junior secondary, upper secondary), health (PHCs, hospitals, the nutritional program), and the subsidy to kerosene. While the JPS programs were not perfectly targeted to consumption expenditures, they stand out as being similarly well targeted relative to these categories of general expenditures. Only spending on primary education has anything like the incidence of the JPS programs.[25]

Figure 2: JPS Participation Incidence versus Benefit Incidence of Education, Health, Kerosene Subsidy



B. Interpretations

The joint finding that the JPS programs were mildly progressively targeted to households with low consumption expenditures and that the degree of targeting was similar across programs raises two related questions: (a) why were the programs targeted as they were, and (b) what accounts for the similarity in the targeting. There are three possible explanations. First, the data did not exist that would have allowed the programs to be targeted to consumption expenditures and the programs were similar because they shared targeting criteria. Second, targeting household's current

[24] And these are categories that are generally thought to be desirable and reasonably well targeted — we do not even include items that are obviously biased towards the rich: tertiary education, gasoline subsidies, the bailout of the banks in the financial crisis (World Bank, 2001).

[25] And this is driven at least in part by the arbitrary assumption of zero economies of scale in household consumption (e.g. that households are ranked by $\frac{Total\ Expenditures}{N^{\theta}}$, θ=1). Since larger household have more children, even if all children are enrolled in public schooling and hence the subsidy is equal *per child*, the assumption makes it appear that more of the benefit accrues to poor *households* by making larger households appear poorer. With θ=1 the QIII/QI ratio is 0.835, but if the economy of scale parameter assumes empirically plausible values the benefit appears much more uniform: at θ=0.8 QIII/QI=0.888, at θ=0.6 then QIII/QI=0.984.

consumption expenditures was not really the program objective and consumption expenditures are only weakly related to those objectives. Third, the underlying political economy precludes sharp targeting.

The first potential explanation is that the household targeting in several of the programs relied on the family planning classification and that this classification was only weakly related to consumption expenditures. This explanation has some pluses and minuses. The "prosperity" classification is only weakly related to expenditures. Table 4 compares those who were "poor" and "non-poor" by ACE poverty and the family planning classification. The fact that 57 percent of those who were family planning poor were also ACE poor implies some association between the two, but also implies fully that 43 percent of the family planning poor were not ACE poor. By the same token, only 57 percent of those who were ACE poor were also family planning poor.[26]

Table 4. The association between the administrative data on household poverty (family planning agency "pre-prosperous") and ACE poverty

	Poor[a]	Non-Poor	Total
Number of Pre-Prosperous households	3,357	2,523	5,880
Percent of "Pre-Prosperous" households who are:	*57.1*	*42.9*	*100.0*
Percent of households who are "pre-prosperous" (by poor/non-poor)	57.0	41.3	49.0
Total	5,889	6,108	11,997
% Row	*49.0*[a]	*51.0*	*100.0*

Source: Based on Suryahadi, Suharso, and Sumarto (1999).
[a] The poverty line is chose so that ACE headcount poverty equals the proportion "preprosperous".

However, it is not clear that the weak association between family planning "prosperity" status and consumption expenditures is the explanation for the similarly mild JPS program targeting by expenditure, for two reasons. First, even for the subsidized rice program, in which the family planning classification was the *only* criteria, the 100 Villages Survey data show only a weak connection between family planning "prosperity" status and receiving OPK rice. Table 5 shows that in the ten

[26] This lack of association is worrisome, but there is no obvious conclusion one way or the other. On the one hand, there has been a long literature arguing that both because of more accurate measurement and because of purposive consumption smoothing by households, current consumption expenditures are a better proxy for household's long-run income than is current income. But on the other, consumption expenditures has its own problems as a proxy of long-run household standard of living and it may well be that an asset based proxy like the family planning method provides a reasonable indicator of long-run welfare. Filmer and Pritchett (2001) show that a principal components based index based on asset ownership (e.g. owning a bicycle) and housing characteristics (e.g. having a toilet) performs at least as well as consumption expenditures in predicting child school attainment.

districts of the 100 Villages Survey the median coverage of those apparently not eligible (the family planning "prosperous") was 70 percent of those eligible and the targeting ratio is only 0.85. This is no better targeting (in fact, a bit worse, 0.7 versus 0.54) than if the ratio of non-poor coverage is computed on a comparable basis using consumption expenditures (final column of table 6).[27] This suggests that the program was as weakly targeted according to the administrative eligibility criteria as according to consumption expenditures — which corresponds to observations about actual program administration (see Chapter VI).

Table 5. Evidence from the 100 Villages Survey about the relationship between observed family planning agency "poverty" (BKKBN KPS) and OPK program participation

District	Percent BKKBN"Poor" (KPS)	Coverage of the "BKKBNpoor" (KPS)	Ratio of coverage of Non-poor to poor (KPS/PS)	Targeting Ratio (proportion to non-poor/non-poor in population)	Maximum Imaginable Targeting		Coverage of consumption expenditure Non-poor/poor[1]
					Maximum coverage of "poor" (KPS)	Minimum Targeting Ratio	
Banjarnegara, Central Java	60.33	91.16	0.91	0.95	100.0	0.79	0.83[a]
Indragiri Hilir, Riau	41.42	23.54	0.51	0.72	40.6	0.00	0.36[b]
Karang Asem, Bali	37.42	20.27	1.34	1.10	65.7	0.00	0.45[b]
Kendari, Southeast Sulawesi	76.08	62.87	0.48	0.55	72.4	0.00	0.53[c]
Kupang, East Nusa Tenggara	75.04	41.94	0.51	0.58	49.1	0.00	0.50[c]
Kutai, East Kalimantan	19.93	21.34	0.72	0.93	83.3	0.00	3.58[d]
Lampung Selatan, Lampung	59.75	49.65	0.88	0.92	79.1	0.00	0.81[a]
Pandeglang, West Java	27.25	23.24	0.33	0.64	43.4	0.00	0.68[b]
Rembang, Central Java	61	87.98	0.67	0.77	100.0	0.52	0.56[a]
Sumedang, West Java	31.92	7.83	1.98	1.19	41.0	0.00	0.48[b]
Median:			0.70	0.85			0.54

Source: Based on Suryahadi, Suharso, and Sumarto (1999), tables 2a and 2b.
Using the quintiles corresponding most closely to the fraction "pre-prosperous" (KPS) as the "non-poor" depending on the BKKBN poverty rate in teach district for this with (a) we use QI-QIII at the consumption expenditure poor, while for other districts it is (b) QI-QII, (c) QI-QIV poor, (d) QI poor.

[27] Cameron (2002) shows that in the 100 Villages Survey data using the self-reported BKKBN status, the scholarships also did not strictly follow the classification, as the proportion of households receiving scholarships (either PS, JSS, USS) was: KPS 11.6%, KS-I 7.11%, KS-II 6.52%, KS-III and above 1.22%. She also finds that in explaining scholarship receipt, the level and change expenditures were statistically significant, even controlling for BKKBN status.

This weak relationship between family planning agency poverty and OPK receipt suggests that even though the program nominally used this criteria in targeting, this likely does not account for the similarity in program incidence. Table 6 shows the relationship (in the October 1999 round of the 100 Villages Survey) between whether a household reports receiving a health card (which provided eligibility for free medical services) and having received OPK rice. Even by October 1999 the health card program coverage was still much smaller than OPK (12.4 percent versus 56.1 percent). Interestingly, only 1.9 percent of households did have a health card but did not receive rice, so only 15 percent of health card recipients did not receive rice. But the converse is not true: only one in five households who received rice also had a health card.

Table 6. Relationship between participation in two JPS programs (subsidized rice and health cards) which both used family planning poverty (BKKBN PS) status as an eligibility criteria

		HH received Subsidized Rice (OPK)		
		Yes	No	Total
HH Received Health Card	Yes	1,121 (10.54)	202 (1.90)	1,323 (12.43)
	No	4,850 (45.58)	4,467 (41.98)	9,317 (87.57)
	Total	5,971 (56.12)	4,669 (43.88)	10,640 (100.00)

Source: October 1999 round of 100 Villages Survey.

While the family planning classification was a *de jure* element of eligibility for several of the programs, this does not entirely account for the similar mild program participation incidence according to consumption expenditures. While it is true the family planning criteria are only weakly related to consumption expenditures (Table 4), the family planning poverty criteria were not followed so closely so as to produce program incidence similarity (Tables 5 and 6).

The second explanation of the mild relationship of program participation to consumption expenditures is that the JPS programs were not *intended* to be exclusively targeted to the consumption expenditure poor. The targeting of each JPS program must be assessed relative to the complex mix of *actual objectives*, not merely the idealized or rhetorical objectives.[28] At least three classes of objectives that could

[28] While occasionally there was a simplistic statement that the JPS programs were intended to "reduce poverty", more frequently the government and donor descriptions said something like the JPS programs were intended to "reduce poverty, mitigate the social impact, maintain human capital investment and sustain employment" (TKPPJPS, 1999), which was reflective of the true range of program objectives.

account for only mild expenditure targeting played a role in the design and targeting of the JPS programs.[29]

First, the education and health programs were intended to avert specific adverse outcomes (drop-out, worsening health status) not to reduce ACE poverty, as the protection of household's consumption/investment of certain "merit goods" was regarded as a concern *independent* of poverty. Therefore nothing about the programs being strongly or weakly targeted, with respect to expenditures, can be used to infer whether the programs were targeted as designed, or whether that design was optimal relative to its objective. That is, there should be no confusion between "targeting leakage" in the sense that households with high consumption expenditure received program benefits and "administrative leakage" in the sense that program benefits were diverted from the intended recipients. For instance, the scholarships program has had close, independent monitoring which suggests that the targeting procedures were adhered to reasonably closely in designating the individual students to receive the scholarships.

One cannot infer from "weak" expenditure targeting that the program was not implemented as designed. Moreover, it is possible that in a program perfectly targeted to avert drop-out, the targeting would be only weakly related to expenditures because expenditures are only a weak predictor of drop-out. In fact, Cameron (2002) uses the 100 Villages Survey data to show that although the level and change in expenditures had some explanatory power for primary school drop-out, neither of these were actually statistically significant in predicting lower secondary or upper secondary drop out. But the "perfectly targeted to drop-out" explanation of the weak relationship to expenditures is made suspect by the fact that Cameron (2002) finds no impact of scholarships on primary or upper secondary school drop-out and only a modest impact in lower secondary schools (3.5 percent).

Second, an explicit "non-poverty" objective of the employment creation program was to mitigate the loss of household income — even for non-poor households. Since the family planning poverty classification was essentially "static" — both because it was updated only once a year and many of the criteria are based on relatively persistent criteria (housing conditions, clothing ownership) — it could easily miss large changes in household welfare.

Third, there were a number of other objectives of the JPS programs that militated against sharp targeting. By late 1998 it was felt that a problem in Indonesia was *insufficient* fiscal stimulus and that the government deficit was not *large* enough on a cyclically corrected basis and hence that the government should *increase* spending. This meant that being able to rapidly disburse relatively large amounts of purchasing power in order to maintain adequate absorption was an additional JPS program objective. Programs which disbursed money for immediate labor creation (especially in a regionally balanced way) were attractive. The JPS programs were being designed

[29] The *existence* of the JPS programs owed a great deal to the politically driven desire of both the government and the international financial institutions to be *seen* to be acting in this area, but did not determine design.

literally in the shadow of burned out buildings and with ongoing protests and hence there was a desire to design programs that could generate political support.[30]

The final possible explanation of the mild (better than uniform but not sharply pro-poor) and similar expenditure targeting of the JPS programs is that some mix of national and local political economy considerations preclude sharp expenditure targeting (Gelbach and Pritchett, 2002). If this is the case, then while some targeting will produce a pro-poor distribution of benefits, it is possible there is no program design that would produce a substantially more targeted distribution of benefits than that observed. If this is the case then some targeting mechanisms will produce more progressive benefit incidence than no targeting, but no *de jure* targeting design would be able to produce extremely sharp targeting (except perhaps for very small programs) as it would be thwarted politically — we return to this issue in the final section.

[30] In addition to the above, which we suspect are quite universal in the launch of crisis programs, we also suspect that the interests of agencies and units of the government played a considerable role in the composition of the JPS programs. Three instances are worth mentioning. One, the logistics agency (BULOG) had a long history of price stabilization activity in basic staples, but was under fire for having been deeply involved in a fair bit of corrupt activity in connection with the monopoly in trade in certain commodities (in connection with the first family). Many people (including the IMF and World Bank) were recommending not just an end to the monopoly restrictions in items like wheat flour but also abolition of the agency itself. A second example is that with the budget under pressure, the only avenue for protecting existing programs or gaining incremental revenue was to classify the activities as under the umbrella of JPS. In this way agencies had the incentive to simply re-label any activity that used labor as a "labor creating" program. A third example is that the take home pays of local officials depended in complicated ways on the implementation of "development budget" programs. Since nearly all new investment was being cut from the budget, this meant that unless local officials were responsible for the implementation of JPS programs in the development budget, their own effective take home pays would be cut.

IV. DYNAMIC VERSUS STATIC PARTICIPATION INCIDENCE: SAFETY NETS VERSUS SAFETY ROPES

A. Empirical Findings

As has been emphasized in recent literature on the changes in poverty status over time (Baulch and Hoddinott, 2000; Dercon and Krishnan, 2000; Jalan and Ravallion, 2000), the change in the average poverty rate masks enormous "churning" as households move in and out of poverty. All of the panel surveys in Indonesia show enormous volatility in poverty rates over this period (for the IFLS see Frankenberg, Thomas, and Beegle, 1999). Table 7 shows the pattern of changes in household poverty status across four rounds of the 100 Villages Survey spanning 14 months. While 42 percent of this population were never poor, 58 percent were ACE poor at least once — even though only less than a third of that figure were always poor (17.5 percent) and the average poverty rate was 37 percent. While at least some substantial fraction of the *measured* changes in household consumption reflect the difficulty of measuring expenditures accurately, this also reflects the large changes in households fortunes even over short periods of time — as households gain and lose jobs, harvests are good or bad, business goes well or badly.

This volatility creates the demand not just for transfer programs to those whose incomes are chronically low (safety nets), but also for informal and formal *insurance* — like mechanisms and programs that would pay off not only when income was absolutely low, but also when households experienced negative shocks (safety ropes).[31]

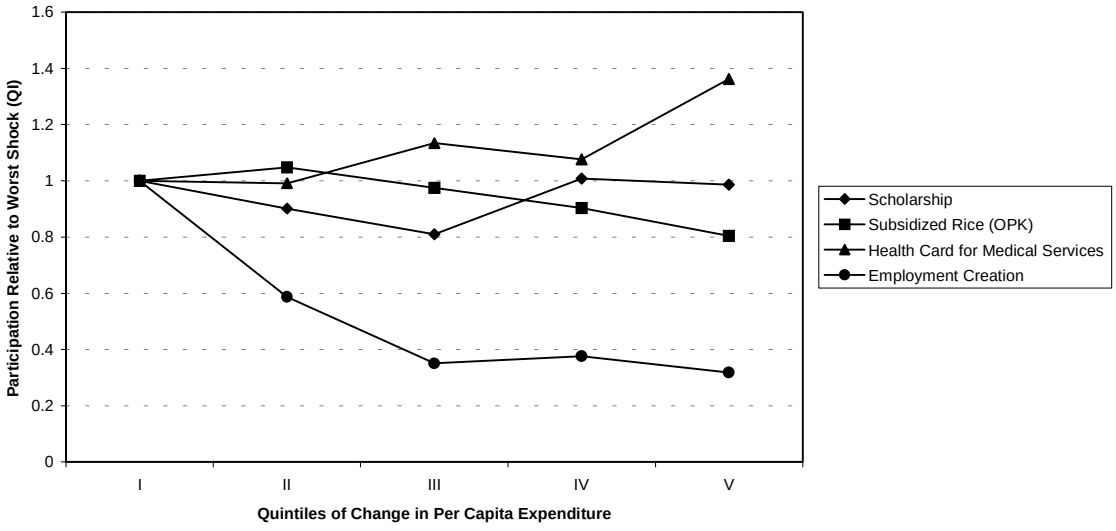
Static participation incidence is the relationship between program participation and the level of expenditure. Graphically the coverage ratio is the height at any given level (or range) of expenditures and hence the targeting is the slope (either at a point or over a range, such as comparing coverage for different quintiles). The exact analogy can be made for *dynamic* participation incidence as a relationship between program participation and *changes* (absolute or proportionate) in consumption expenditures (or income). Figure 3 shows the exact analogy of the *static* participation incidence in Figure 2, i.e. showing the relationship between JPS program participation in December 1998 and the natural log *change* in household per person expenditures between May 1997 and August 1998. Unlike static incidence the dynamic incidence is very different across the programs. Only for the employment creation programs were households substantially more likely to participate if they experienced a bad shock to consumption expenditures.

[31] Sumarto, Suryahadi, and Pritchett (2000).

Table 7. The patterns of changes in household poverty status

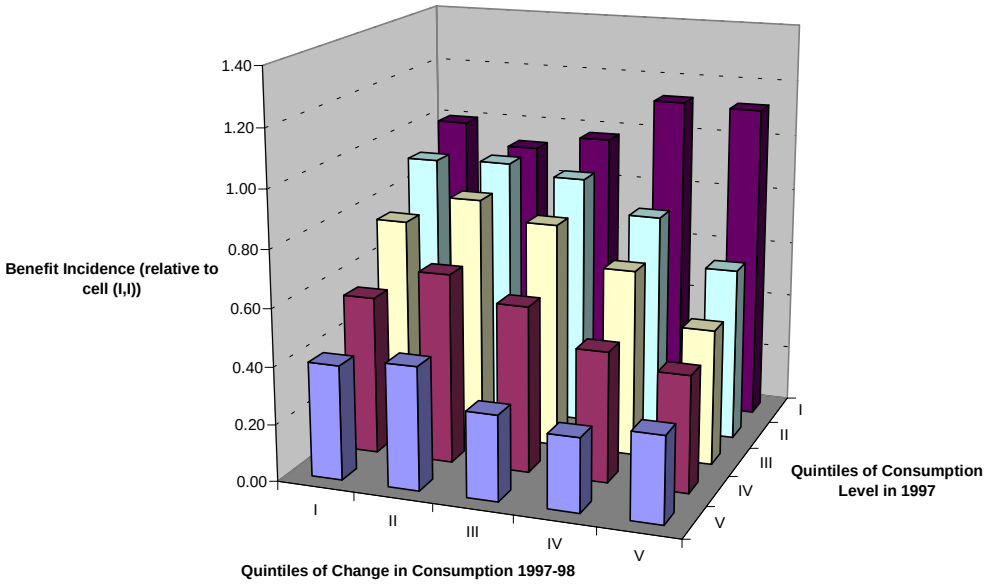
Pattern of poverty status	Poverty Status in:				Household Frequency (%)
	Aug '98	Dec '98	May '99	Oct '99	
Always poor	Poor	Poor	Poor	Poor	17.5
Three times poor (12.0%)	Poor	Poor	Poor	Non-poor	4.6
	Poor	Poor	Non-poor	Poor	2.0
	Poor	Non-poor	Poor	Poor	2.9
	Non-poor	Poor	Poor	Poor	2.5
Twice poor (12.4%)	Poor	Poor	Non-poor	Non-poor	3.7
	Poor	Non-poor	Poor	Non-poor	3.0
	Poor	Non-poor	Non-poor	Poor	1.4
	Non-poor	Poor	Poor	Non-poor	1.7
	Non-poor	Poor	Non-poor	Poor	1.0
	Non-poor	Non-poor	Poor	Poor	1.6
Once poor (15.9%)	Poor	Non-poor	Non-poor	Non-poor	7.9
	Non-poor	Poor	Non-poor	Non-poor	3.2
	Non-poor	Non-poor	Poor	Non-poor	2.7
	Non-poor	Non-poor	Non-poor	Poor	2.1
Never poor	Non-poor	Non-poor	Non-poor	Non-poor	42.2
Source: Widyanti, Sumarto, and Suryahadi, 2001, table 3.					

Figure 3: Dynamic Participation Incidence of the JPS Programs



Different programs can have different coverage and targeting performances, with respect to expenditure levels and shocks.[32] Hence one program's incidence graph could be steep with respect to expenditures at all levels of shock, flat (non-targeted) with respect shocks, or vice versa. A pure "safety rope" program could be sharply targeted with respect to shocks but flat in the expenditures dimension. Figures 4a and 4b show the combined static and dynamic participation incidence for two of the programs: OPK and employment creation. Not surprisingly, the self-selection program has enormously better dynamic benefit incidence than the administratively targeted OPK program as households who need assistance have an easier time accessing program benefits.[33]

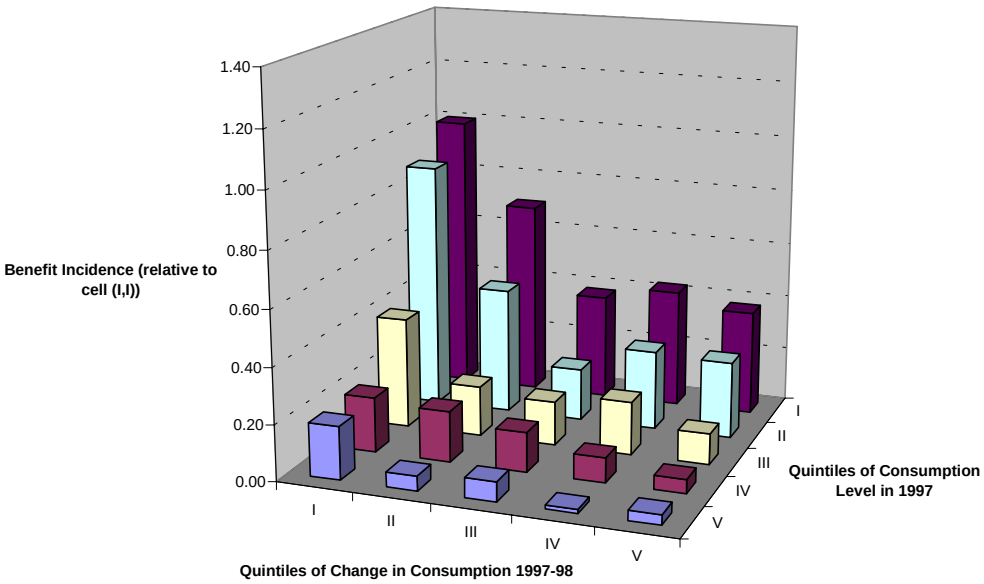
Figure 4a: Probability of Household Participation in OPK Program by Quintiles of Level and Changes in Per Capita Consumption Expenditures



[32] The graph of participation incidence including both *levels* and *changes* in (natural log) consumption expenditure is a three dimensional surface.

[33] This suggests that the *screening* argument for the use of "workfare" requirements (Besley and Coate, 1992) appears to work well in *changes* but no in *levels*. These results contrasting the labor creating schemes to other programs are in some ways similar and in some ways different from findings about the public employment scheme TRABAJAR in Argentina (Jalan and Ravallion 2002, Ravallion 2002b). They find that TRBAJAR was much better targeted in static benefit incidence — 76 percent of participants were from households in the bottom quintile — and had much better benefit incidence than other "protected" expenditures.

Figure 4b: Probability of Household Participation in Employment Creation Programs by Quintiles of Level and Changes in Per Capita Consumption Expenditures



B. Interpretations

The now ubiquitous metaphor of a "social safety net" conflates two distinct objectives in the design of transfer programs. One objective might be to minimize a measure of income or expenditure *poverty*.[34] An alternative objective might be to *mitigate risk* — reduce household vulnerability to the wide variety of potential adverse shocks they could face (death, accident, fire, crop loss, job loss) — whether or not the shocks push households below some absolute threshold. If the targeting of social programs is judged exclusively on poverty or benefit incidence based on a cross sectional snapshot, then risk mitigation programs benefiting households who have suffered large shocks but who are not "poor" may appear to have large "leakage" (a type II targeting error, reaching people who are not intended beneficiaries) when in fact they are simply serving an alternative social objective.

[34] This general definition of poverty covers any of the class of Foster-Greer-Thorbecke (1984) measures of poverty which adjust for the intensity or depth of poverty and is consistent with either absolute or relative poverty lines.

The undifferentiated metaphor of "safety net" can also confuse thinking about the political economy of transfer programs.[35] Government may want to implement "safety net" and "safety rope" programs for completely different reasons.[36] While a "safety net" program might be more popular the more effectively it transfers from richer to poorer households, a "safety rope" program might cause little net redistribution but be popular because it serves an important insurance function in transferring resources from good states to bad states.

Designing programs that deal with *shocks* to income (either at the economy wide or individual level) requires mechanisms that allow individuals to be added to program participation dynamically. This requires either self-selection targeting or "open" administrative criteria — that is, the eligibility criteria need to have a mechanism for households to be added (and subtracted).

[35] In OECD countries, the distinction has been used to characterize different "systems", that is those that rely on means testing versus those that provide universal benefits (Esping-Andersen, 1990; Goodin *et al.*, 1999).

[36] Economists would recommend poverty programs to a hypothetical benign social welfare maximizer if the social welfare function was built up from individual (household) utility functions with declining marginal utility, in which case a (costless) transfer from rich to poor is not a Pareto improvement but does raise social welfare. There is also an argument for poverty programs from an externality in altruism. In contrast, the normative case for government involvement in mitigation of risk is based on the argument that, if moral hazard and adverse selection are sufficiently large then welfare improving markets for insurance against these risks will not exist (and they will be "too small" in any case). This is potentially the case in a wide variety of insurance markets — but particularly affect the market for insurance of incomes.

V. MARGINAL AND AVERAGE INCIDENCE IN THE JPS PROGRAMS

In an important paper Lanjouw and Ravallion (1999) stress that for decisions about program *expansion*, the relevant incidence measure is the *marginal* not average incidence. In what they characterize as "early" capture programs, the non-poor are more likely to receive benefits than the poor from the beginning of the program.[37] If this is so then marginal incidence will be higher than average incidence. An example might be secondary education in which the richer households are likely to be the first to enroll, so at low levels average incidence will be regressive. However as schooling moves towards universality, the incremental child is likely to be from a poorer household than the average child already enrolled, so the marginal incidence will be more progressive than average incidence. In contrast, with "late" capture, the program is well targeted to the poor at low levels of coverage but as the program expands the marginal incidence is worse than average and hence targeting falls.

A. Empirical Findings

We find no systematic pattern of either "early" or "late" capture in the Indonesian JPS programs:[38] (a) larger programs were not systematically more or less targeted than smaller programs; (b) within the JPS programs, districts with higher coverage had only a weak tendency to have worse static participation incidence (as measures by the targeting ratio); (c) as programs evolved over time, they did not become more or less targeted.

Coverage and targeting across programs. Looking at Table 2 one can see that programs with higher total coverage did not in fact have better targeting. OPK was substantially larger than any of the programs, but the targeting was roughly equal.

Coverage and targeting across regions. The substantial regional variation in the resources available for each program (principally driven by the geographic targeting rules) created substantial variation in the coverage across regions for the same programs. If incidence is a function of coverage then, $TR_j = f(CR_j, Z_j)$ and if more goes to the expenditure poor as coverage expands then the targeting ratio would fall ($\frac{\partial TR}{\partial CR} < 0$), while if there is "late" capture then the targeting ratio should rise as coverage expands ($\frac{\partial TR}{\partial CR} > 0$).

[37] "Early" and "late" do not necessarily imply timing, it could also be "small" and "large" in a cross section.

[38] We should point out that our "targeting ratios" results are not based on the same measure as the "targeting differential" (TD) given by the difference between per-capita allocations to the poor versus non-poor used in Lanjouw and Ravallion (1999).

Table 8 examines this issue in two ways and shows that there is some consistent, but weak, evidence of "late" capture — targeting ratios generally were higher (less targeted) in regions where program coverage was larger. But the impact is very weak: in a power series approximation to a non-parametric regression, the *total* explanatory power of all transformations of the coverage ratios is between 3 and 8 percent of the variation across districts in targeting. Moving from low to moderate coverage (5 to 15 percent) increased the targeting ratio (made programs less targeted) by between 0.03 (Rice, Employment) to 0.06 to 0.08 (the scholarship programs). So, there is some evidence that the programs did a modestly better job of limiting the benefits to the poor at very low levels of coverage.

Table 8. The (weak) relationship between targeting ratios and program coverage

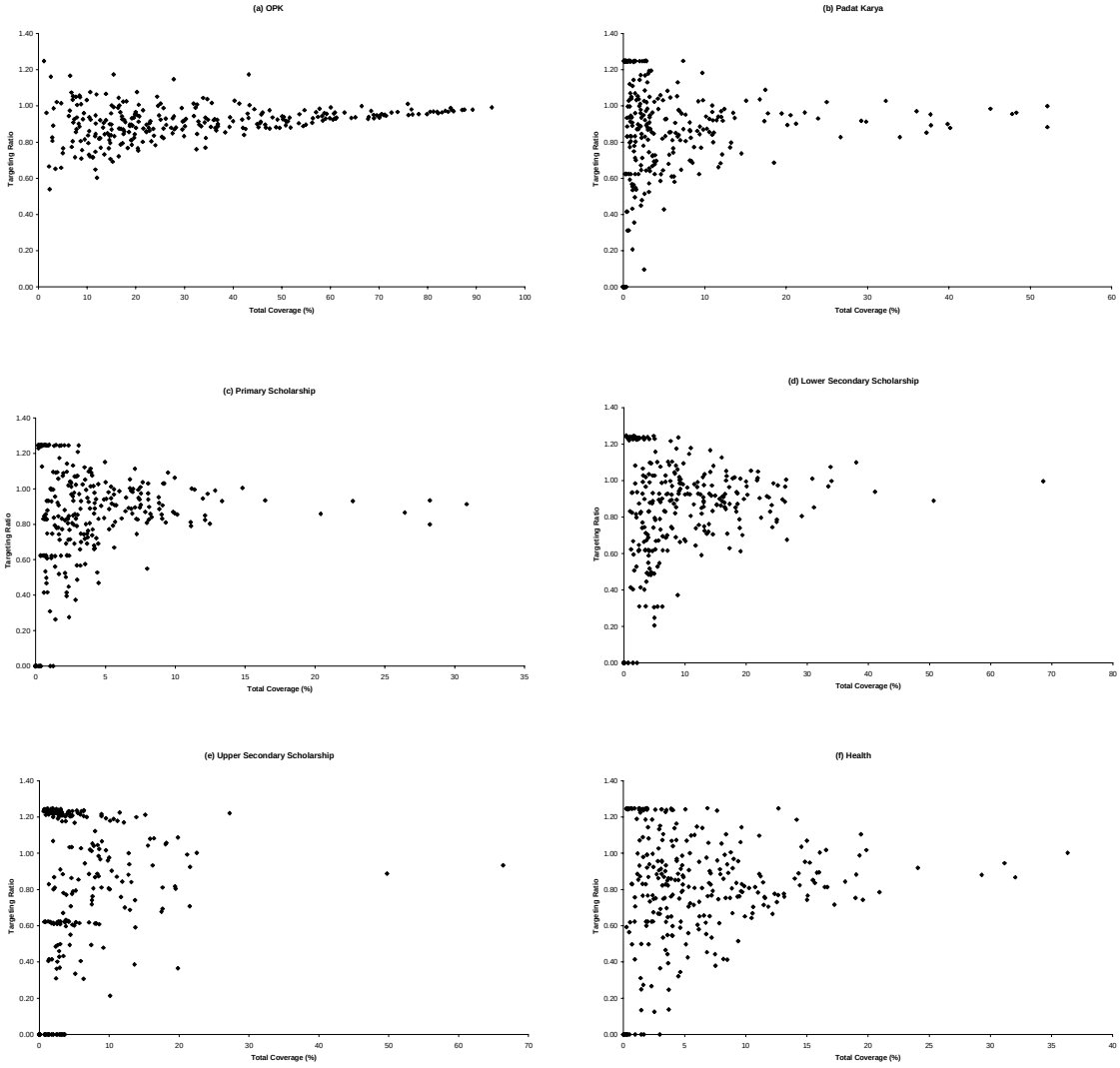
A) Mean and Standard Deviation (in parenthesis) of Targeting Ratio By Groups of Coverage						
Coverage Ratio (%)	OPK	Employment Creation	Scholarships			Health
			Primary	Junior	Senior	
CR< 5	0.87 (0.21)	0.86 (0.33)	0.84 (0.30)	0.81 (0.34)	0.83 (0.45)	0.82 (0.35)
5 < CR <10	0.92 (0.12)	0.86 (0.15)	0.92 (0.10)	0.90 (0.22)	0.90 (0.26)	0.82 (0.22)
10 < CR <20	0.87 (0.11)	0.89 (0.11)	0.91 (0.08)	0.89 (0.13)	0.89 (0.25)	0.86 (0.14)
CR > 20	0.93 (0.06)	0.93 (0.57)	0.88 (0.05)	0.92 (0.10)	0.95 (0.15)	0.90 (0.74)
Total	0.91 (0.10)	0.87 (0.27)	0.86 (0.26)	0.86 (0.25)	0.86 (0.38)	0.83 (0.29)
B) Results of non-parametric regression of Targeting Ratio on Coverage Across districts[1]						
R2 of regression	.115	.007	.055	.052	.008	.053
Predicted Targeting ratio at CR=5%	0.86	0.86	0.89	0.82	0.86	0.81
Predicted Taregeting ratio at CR=15%	0.89	0.89	0.88	0.90	0.92	0.87
Change in predicted TR from 10 percentage point increase in coverage (positive is weaker targeting)	0.03	0.03	-0.01	0.08	0.06	0.06

1) Since we are completely agnostic about the functional form (we have no reason to believe the relationship is linear) we estimate a functional form that includes powers and transformations of the coverage ratio. This can be thought of as a non-parametric approximation to any functional form. Since the slope is not constant we simply calculate the change across two points to give an "arc slope."

$$TR_j = \beta_0 + \beta^1 * CR_j + \beta^2 * CR_j^2 + \beta^3 * CR_j^3 + \beta^4 * CR_j^4 + \beta^5 (1/CR_j) + \beta^6 \ln(CR_j) + \varepsilon_j$$

Figure 5 shows the graph of the targeting ratio and the coverage ratio across districts. The principal relationship between coverage and targeting is that programs in districts with very low coverage show, not surprisingly, very variable results in the targeting ratio. In districts with few recipients at times all benefits went to non-poor households and at times all benefits went to poor households (TR=0).

Figure 5: Relationship between Targeting Ratio and Total Coverage



Coverage and targeting over time. The 100 Villages Survey asked households about JPS participation in the Dec 1998 to October 1999 rounds, which allows some examination of the dynamics of targeting during the expansion in program coverage in the subsidized rice and health card (medical services) programs.[39] As Lanjouw and

[39] The scholarship program was at full scale from its beginning and the employment creation programs actually contracted over this period.

Ravallion (1999) explain, with early capture (in which richer households receive benefits first) the expansion in coverage of the poor should be higher than the expansion in average coverage (and vice versa for "late" capture). These two programs have different patterns. The OPK program appears to have expanded in these 10 districts roughly neutrally — over the 10 months average coverage increased 15.4 percent and quintile I coverage by 18.8 percent.

By contrast, the medical services/health card program shows an interesting dynamic of early capture. This program was slow to gain momentum as a JPS program and when the program was very small (only 3.8 percent) it had worse than uniform targeting, with more going to the richer than poorer households. As the program first came to scale from December 1998 to May 1999 as a JPS program, it went disproportionately to the poor households — coverage among the poor increased by 11.7 percentage points (from 3.8 to 15.5) while average coverage increased by only 6 percentage points. The expansion from May to October appears to have been roughly neutral — with quintile I coverage increasingly modestly less than average coverage (2.7 versus 3.9 percentage points). This suggests that, as the program of health cards and subsidized medical services went from being a "regular" program to a crisis JPS program, it moved from the typical pattern of middle class capture to decidedly pro-poor in the early phases of expansion. However, as the program matured it became less targeted.

Table 9. Changes in targeting during program expansion

	Quintile I coverage	Ratio of coverage ratios		Targeting Ratio	Average coverage
		QIII/QI	QV/QI		
Subsidized Rice					
Dec-98	47.93	0.90	0.62	0.96	40.73
May-99	66.29	0.87	0.68	0.96	56.45
Oct-99	66.74	0.86	0.65	0.95	56.11
Change Dec 98 to Oct 99	18.81	-0.04	0.03	0.00	15.38
Medical Services					
Dec-98	3.80	1.44	1.52	1.07	5.22
May-99	15.49	0.65	0.49	0.91	11.23
Oct-99	18.20	0.84	0.72	0.95	15.13
Change Dec 98 to Oct 99	14.40	-0.60	-0.80	-0.12	9.91
Source: Suryahadi *et al.*, 2001 (adapted from table 2).					

B. Interpretations

It is understandable that many government programs are "early capture" as richer and more informed and powerful households would be the first to gain benefits but that expansion would lead to better incidence.[40] But the Indonesian experience with the JPS program gives some (weak) indications that "safety net" programs were likely to be "late capture" programs — that is, it would be easier to create strong targeting of benefits in smallish programs, designed and designated for "the poor".

[40] Lanjouw and Ravallion (1999) show that even the anti-poverty programs tend to be "early capture" with higher marginal than average odds of participation (e.g. the Integrated Rural Development Program and the Public Distribution System), although the public works program is better targeted than either and marginal and average odds of participation are more similar than for the other two programs.

VI. *DE JURE* VERSUS *DE FACTO* TARGETING: COMMUNITY INFLUENCE ON TARGETING

A recent strand of the literature on targeting has examined the possibilities of using "communities" to target benefits to households. The idea is that local communities have quite good information on both the level and shocks to households welfare, and hence *if* local decision making structures could be employed to use that local knowledge, then targeting could be improved without the high cost of improving administrative targeting or self-selection targeting. However, note the "if" in the previous sentence is a very big if.

To use the superior local knowledge about conditions, "communities" must be given discretionary power to allocate benefits. But this discretion could either lead to superior targeting or to elite capture (Platteau 2000, Bardhan and Mookerjee 2000) or to community pressures for more "spreading" of the benefits. Galasso and Ravallion (2002) use data from Bangladesh's Food-for-Education Program to show that power in community decision making clearly affected outcomes (and that centralized allocations to villages were not particularly pro-poor). We examine this issue with the Indonesian JPS with in three bits of evidence: discussion of the OPK, examining the non-income determinants of program receipt, and evidence from the Local Level Institutions study linking individual and community social characteristics and program distribution.

A. Empirical Findings

OPK distribution at the village. The SUSENAS and 100 Villages Survey data indicate unambiguously that the *de jure* program distribution was not followed. While the rice allocated to a village area was (nearly always) based on the number of eligible households, the rice was allocated to more than the eligible households. Tables 1 and 2 show that almost twice as many households received the rice than if the program guidelines have been followed and almost a quarter of households in the richest group still receive the program benefits.

These quantitative findings accord well with anecdotal reports, as from the beginning of the program observers noticed that the local leaders responsible for implementation were not adhering to the list of eligible households, but rather were distributing the rice amongst a much larger group. As a result, while each eligible household in many cases received lesser amount of rice than stipulated in the program, other households, which were not officially eligible, also received an allocation (Tim Dampak Krisis SMERU, 2000). The centrally planned administrative guidelines apparently often proved socially unacceptable at the community level (Sumarto and Suryahadi, 2002).

It is not at all clear whether the deviations at the local level were a "good" or "bad" thing. There are three main arguments made by village heads (*kepala desa*) to justify

this practice.[41] First, the official list is not an accurate list of those who are in need, as due to the crisis, many households which formerly were not poor are also now in need of assistance. Second, even if the list is accurate, the distinctions drawn are too fine: the differences between those households who are entitled and those who are not does not justify one group receiving 10-20 kg of rice while the remainder receive nothing. Third, village heads and community leaders argue that the targeted distribution of this central government benefit is inconsistent with the spirit of community solidarity and self-help (*gotong royong*). The village heads point out that, if everyone is expected to contribute their labor to community projects, then everyone should also benefit from the unexpected windfall of assistance from the central government.

These arguments raise important questions about the structure of the "optimal feasible" targeting as communities may know better than the central government and the BKKBN classification about the best distribution of rice within any given community. Furthermore, local leaders may know better what is socially and politically feasible. Rice that went to "non-eligible" households, while inconsistent with the program guidelines was not necessarily mis-targeting. Moreover, local social pressures around "fairness" clearly led to more uniform distribution, but which implied lesser benefit for the poor.

On the other hand, providing too much discretion, and particularly discretion without conditions for adequate local oversight can lead to abuses. Olken *et al*. (2001) show that local politics appeared to play some role in the way village heads distribute OPK rice. Anecdotal reports exist of local leaders abusing discretion in the OPK and in other social programs.

Household demographic and social characteristics and JPS program allocation. The 100 Villages Survey, in addition to having multiple rounds, also has information on household's participation in a variety of social organizations. We can examine empirically whether these social characteristics played a role in program allocations, over and above the household's wealth (as proxied by assets), education, and consumption expenditures as well as other characteristics. Table 10 reports the percentage change in the likelihood of program participation for the various programs for households of differing demographic composition and social behaviors.

[41] This draws on reports of visits as well as the authors' own experiences.

Table 10. Influence of household demographic and social characteristics on participation in JPS programs, from three rounds of the 100 Villages Survey

	Mean of variable	OPK		Scholarship		Health		PK	
		Percentage change in likelihood of program receipt	Sig	Percentage change in likelihood of program receipt	Sig.	Percentage change in likelihood of program receipt	Sig.	Percentage change in likelihood of program receipt	Sig.
Individual Membership in government/social organizations (from previous 100 Villages Round)									
Women's organization (PKK)	0.276	-2.38%	*	24.10%	**	16.75%	**	-29.12%	**
Neighborhood organization	0.223	3.44%	**	-13.50%	**	-12.34%	**	-24.14%	**
Youth Organization	0.132	3.65%	**	3.79%		-8.70%	**	-10.16%	**
Burial society	0.518	4.06%	**	-2.12%		-3.49%		60.41%	**
Sport Organization	0.195	5.07%	**	20.46%	**	-6.45%	**	-18.42%	**
Religious	0.660	-9.74%	**	-10.49%	**	-0.02%		-24.68%	**
Rotating help/credit (*arisan)*	0.331	-2.38%	*	24.10%	**	16.75%	**	-29.12%	**
Household characteristics									
Age of the HH head	43.868	-0.34%	**	0.65%	**	-0.44%	**	-0.44%	**
Number of HH members	5.503	-0.31%		11.39%	**	-0.57%		-1.15%	
Female headed household	0.047	0.62%		43.65%	**	1.10%		-42.43%	**
Married head of household	0.939	2.65%		-23.48%	**	-6.91%		-14.24%	*
pred p		0.514		0.0658		0.069		0.0486	
Psuedor2		0.242		0.112		0.103		0.217	
N		133637		88853		133637		133637	

Note: The "percentage change" in the likelihood of program receipt is the marginal effect from the probit estimates divided by the underlying predicted probability for each program.
Also included in all regressions: consumption expenditures, change in consumption expenditures (since last round), binary variables for nine asset ownership (e.g. radio, TV, bicycle), education of household head, sector of employment, employment status (see Appendix table A.1).

The demographic characteristics of households do not appear to play a large role, with two prominent exceptions. First, children in female-headed households were much more likely to receive scholarships, which is reassuring as this was part of program design. Second, people in female headed households were much less likely to participate in labor creating programs, again not surprisingly as the "first round" programs did little to encourage female participation.

The social characteristics present a different pattern. There is no question that, statistically speaking, household membership in various social organizations played a role in the receipt (or not) of the JPS programs. Nearly all of the coefficients are statistically significant, and, for various variables, quantitatively significant as well (10 percent or more change in the probability of program participation). But the pattern of signs is difficult to interpret. For instance, members of religious organizations were consistently less likely to have received program benefits — but was this because decision making procedures were biased against them somehow, or because they were less likely to attempt to access the programs. Having a member of the household in the government organized women's association (PKK) makes individuals more likely to receive scholarships or use the health card, but much less likely to participate in a labor creating program.

Village characteristics and JPS program allocations. The Local Level Institutions study collected detailed information about social activities from individuals in 48 villages in rural Indonesia. From these detailed data four measures of "social" activity can be derived (Alatas, Pritchett, and Wetterberg, 2002): participation in "social organizations" (social activities in groups with fixed membership, leadership), "social network" (social activities in spontaneous groups), "sociability" (frequency of household visits to friends and relatives), "participation in village governance organizations" (membership in the groups that were created to administer the *desa*).

Alatas, Pritchett and Wetterberg (2002) show these distinctions have different impacts on *desa* governance. Using the distinction between a households own social activities and the average frequency of the sampled households in the same village (less the household) we can examine whether the households' own social connections affect distribution (private benefits) versus whether the density of social activity in the village itself makes a difference (externality type effects). Table 11 shows that the associations are weak and vary across types of activity. Living in a village where social network activity is high is associated with higher program participation (for labor creation, health significantly so, less so for scholarships and credit). Similarly, living in a village with high level of social organizations raises the likelihood of household participation (which, since the average village participation is fixed likely reflects *greater* spreading of the benefits).

Table 11: Likelihood of program receipt and social characteristics of individuals and villages

Average engagement in social activities		OPK		Labor Creating		Health		Scholarships		Credit	
		Percentage change in likelihood of program receipt	Sig.	Percentage change in likelihood of program receipt	Sig.	Percentage change in likelihood of program receipt	Sig.	Percentage change in likelihood of program receipt	Sig.	Percentage change in likelihood of program receipt	Sig.
Social Organizations	Household	-0.61%		-0.38%		-3.40%		-14.75%		7.60%	
	Village (less household)	20.75%	***	-0.19%		-11.54%		6.60%		13.36%	
Social Network	Household	0.65%		-0.13%		-1.73%		0.93%		0.72%	
	Village (less household)	-3.79%	*	4.75%	***	20.52%	***	6.71%		5.32%	
Village Government	Household	0.27%		2.01%	***	5.19%		-9.55%		16.17%	***
	Village (less household)	2.98%		-0.65%		-6.12%		21.33%		-16.43%	
Sociability	Visits to others	0.62%		-0.38%		0.69%		5.70%	**	0.37%	

Note: The "percentage change" in the likelihood of program receipt is the marginal effect from the probit estimates divided by the underlying predicted probability for each program.
Also included in all regressions: quintiles consumption expenditures, change in consumption expenditures (since the previous round in 1996), a principal components index of assets, education of household head, age of the head, and binary variables for female headship, agricultural worker, government worker, employment status (of head and spouse) and district.

Perhaps the most interesting result is the contrast between the JPS programs and the "credit" programs run by the government that channeled resources to specific activities. A person who belonged to the government administrative apparatus was much more likely to have gotten credit than a household who was not. At the same time, living in a village in which *other people* participate in the village governance organizations reduces the likelihood of receiving credit. JPS programs do not appear to have been "captured" by local governments or their agents.

We try by examining the quality of village targeting and its relation to social characteristics in a two-step procedure. First, we regress program participation only on economic criteria (e.g. linear to quartic terms in (natural log) consumption expenditures, change in expenditures, wealth, whether the HH head or spouse is unemployed, education, and HH size) and rank households by the predicted values of this equation. Our measure of targeting performance is the fraction of the N^v households in each village who did receive the j^{th} JPS program who were among the

N highest (most likely) ranked recipients. We find almost no association between targeting performance (as we measure it) and social characteristics of the villages.[42]

Table 12: Quality of village targeting (fraction of the program participants that were among those in the village that would be chosen on economic criteria alone, 0 is worst, 1 is best) and village social activity

Average engagement in social activities	OPK		Labor Creating		Health		Scholarships		Credit	
	Coeff.	P level	Coeff.	P level	Coeff.	P level	Coeff.	P level.	Coeff.	P level
Social Organizations	0.068	0.35	-0.182	0.09	-0.11	0.081	-0.095	0.35	-0.069	0.473
Social Networks	-0.008	0.83	-0.003	0.96	0.066	0.046	0.049	0.49	0.006	0.910
Village Government	0.0039	0.95	0.202	0.04	-0.004	0.926	-0.035	0.73	0.136	0.101
Sociability	-0.002	0.91	-0.016	0.63	-0.001	0.959	0.016	0.64	0.011	0.729
N	42		26		41		32		37	
R2	0.511		0.416		0.679		0.17		0.277	
Note: Also included are binary variables for the district.										

B. Interpretations

The implementation of targeting will be influenced by household and community characteristics and that influence does not necessarily lead to more "pro-poor" targeting. The *de jure* design can work either to limit these influences or to accommodate them by providing explicit local discretion, along with checks on that discretion.

[42] Galasso and Ravallion (2002) find no association of the intra-community targeting performance of FFE (Food for Education) funds and village social characteristics (a cooperative society in the village, club/recreation in the village) but do find that land inequality, shocks, isolation (no telephone in the village) and higher private transfers lead to worse targeting of that public program.

VII. CONCLUSION

We will not recapitulate the empirical findings, nor lessons about crisis programs more broadly, as there are many aspects of program design (e.g. institutional and organizational capacity) and program impact on beneficiaries that we have not addressed, and there were even aspects of targeting we have not addressed (e.g. the cost of the various targeting mechanisms). Let us attempt four summary statements.

First, "static participation incidence" (the relationship between program participation and household consumption expenditures) was substantially better than a uniform transfer, but substantially worse than perfect targeting, and remarkably similar for all of the JPS programs. This implies that some designed targeting produces much better targeting incidence than no targeting, but the *de jure* design is not as critical.

Second, unlike standard static incidence measures, what we define as dynamic participation incidence — the relationship between changes in consumption expenditures and program participation — was very different among the JPS programs. The employment creation programs, which relied on self-selection targeting, were much more likely to reach those households with large shocks to their expenditures than programs based on administrative targeting such as subsidized rice sales, scholarships, and health subsidies. This implies that, especially in a crisis, the dynamics of household welfare is important and this requires dynamics in targeting, either through self-selection (which is very expensive in benefits delivered per program expenditure) or through administrative flexibility.

Third, larger coverage does not lead to either better or worse targeting: there is no general tendency across the programs for marginal incidence to be above, or below, average incidence. This implies that neither small nor large is necessarily beautiful.

Fourth, the targeting design of many of the programs was not followed strictly in the implementation of all of the programs. Community and individual characteristics that were *de jure* irrelevant played a role in targeting in practice. In the cheap rice program, community influence led to the program going to many more than the eligible individuals. In other programs, individual characteristics appear to have influenced targeting. This implies that local conditions will influence implementation, but "communities" may well demand less sharp targeting than centralized administrative criteria.

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Appendix Table A1: Influence of household characteristics on participation in JPS programs, from three rounds of the 100 Villages Survey

	means	OPK		Scholarship		Health		PK	
		Percent	Sig	Percent	Sig.	Percent	Sig.		Sig.
Ln(real per capita household expenditures)	11.200	-0.271	**	-0.173	**	-0.437	**	-0.623	**
Change in real per capita expenditures	0.028	-0.110	**	0.175	**	-0.037		-0.194	**
Membership in government/social organizations (from previous round)									
Women's organization (PKK)	0.276	-0.029	**	-0.004		0.283	**	0.083	*
Neighborhood organization (*Dasa Wisma*)	0.223	-0.024	*	0.241	**	0.168	**	-0.291	**
Youth Organization (*Karang Taruna*)	0.132	0.034	**	-0.135	**	-0.123	**	-0.241	**
Burial society	0.518	0.037	**	0.038		-0.087	**	-0.102	**
Sport Organization	0.195	0.041	**	-0.021		-0.035		0.604	**
Religious	0.660	0.051	**	0.205	**	-0.065	**	-0.184	**
Rotating help/credit (*arisan)*	0.331	-0.097	**	-0.105	**	0.000		-0.247	**
Household characteristics									
Age of the HH head	43.868	-0.003	**	0.006	**	-0.004	**	-0.004	**
Number of HH members	5.503	-0.003		0.114	**	-0.006		-0.011	
Female headed household	0.047	0.006		0.437	**	0.011		-0.424	**
Married head of household	0.939	0.027		-0.235	**	-0.069		-0.142	*
Highest level of Education of household head (none is default)									
Primary	0.571	-0.079	**	0.011		-0.039		0.139	**
Lower secondary	0.092	-0.232	**	-0.026		-0.108	**	0.213	**
Higher secondary	0.098	-0.411	**	-0.220	**	-0.109	*	-0.455	**
Tertiary	0.025	-0.709	**	-0.347	**	-0.487	**	-0.616	**
Sector of "main source of household income"									
Industry	0.059	0.074	**	0.184	**	-0.059		-0.278	**
Trade	0.112	0.017		-0.047		-0.213	**	-0.243	**
Service	0.217	0.003		-0.016		-0.114	**	-0.177	**
Other	0.012	-0.042		0.368	**	0.042		-0.558	**
Employment status of the individual (unemployed is default)									
Self employed/employer	0.691	0.010		0.260	**	-0.082	*	0.210	**
Employee	0.235	-0.041	**	0.451	**	-0.158	**	0.400	**
Family worker	0.010	-0.038		0.903	**	-0.081		0.460	**
Ownership of various assets									
radio* |	0.651	-0.015	*	-0.135	**	-0.209	**	0.180	**
tv* |	0.406	-0.150	**	-0.002		-0.217	**	-0.206	**
refri* |	0.091	-0.295	**	-0.424	**	-0.469	**	-0.280	**
phone* |	0.511	-0.055	**	-0.002		-0.041		-0.136	**
Dishant* |	0.070	-0.270	**	-0.290	**	-0.382	**	0.098	
bike* |	0.381	-0.030	**	-0.040		0.359	**	0.472	**
Mtrbike* |	0.168	-0.058	**	-0.292	**	-0.082	*	-0.181	**
car* |	0.046	-0.050	**	-0.336	**	0.050		-0.281	**
land* |	0.737	-0.032	**	-0.003		-0.492	**	0.720	**
Predicted P		0.514		0.0658		0.069		0.0486	
Pseudo R^2		0.242		0.112		0.103		0.217	
N		133,637		88,853		133,637		133,637	