

# Did the Health Card Program Ensure Access to Medical Care for the Poor during Indonesia's Economic Crisis?

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The Indonesian Social Safety Net health card program was implemented in response to the economic crisis that hit Indonesia in 1997, to preserve access to health care services for the poor. Health cards were allocated to poor households, entitling them to subsidized care from public health care providers. The providers received budgetary support to compensate for the extra demand. This article focuses on the effect of the program on primary outpatient health care use, disentangling the direct effect of allocating health cards from the indirect effect of government transfers to health care facilities. For poor health card owners the program resulted in a net increase in use of outpatient care, while for nonpoor health card owners the program resulted mainly in a substitution from private to public health care. The largest effect of the program seems to have come from a general increase in the supply of public services resulting from the budgetary support to public providers. These benefits seem to have been captured mainly by the nonpoor. As a result, most of the benefits of the health card program went to the nonpoor, even though distribution of the health cards was propoor. The results suggest that had the program, in addition to targeting the poor, established a closer link between provision of services to the target groups and funding, the overall results would have been more propoor. JEL codes: H51, I11, I38.

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In the current debate on the provision of health care services in developing countries, many researchers have found high inequalities in the use of public health care and hence in the benefit incidence of public spending.

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Supply-driven public policy typically lacks incentives for health care providers to serve the poor. Ensuring that the poor benefit from health care and receive a basic package is a widely shared policy objective (World Bank 2004). Targeted price subsidies for medical care are often advocated to increase access to medical care for the poor. However, there is little empirical evidence about the efficacy of such systems.

This case study looks at a particular kind of health care intervention that was implemented in Indonesia, which included both a targeted price subsidy and a public spending component. This combined program was part of a larger Social Safety Net (SSN) program, initiated in 1998 to protect the poor from the effects of the Southeast Asian economic crisis.<sup>1</sup> Households that were thought to be most vulnerable to economic shocks were allocated health cards, which entitled household members to the price subsidy. Health care facilities that provided the subsidized care received extrabudgetary support to compensate for the increased demand.

There are some distinct features to the SSN health program. First, the price subsidy applied only to public service providers. Second, the program followed a decentralized design, with both geographic and community-based targeting. Third, there was a weak link between use of the health card and compensation of health care providers. Compensation was allocated to districts based on the estimated number of households eligible for the health card program and not on actual use of health cards.

This article focuses on the effect of the Indonesian health card program on demand for primary outpatient health care. The design of the program makes it possible to investigate several interesting questions. First, it provides the opportunity for an *ex post* evaluation of a targeted price subsidy for health care that was implemented on a national scale. There are relatively few empirical studies that evaluate actual pricing policies in health care. Of those that do, only a handful take account of the endogenous nature of public interventions in their estimation strategy.<sup>2</sup> Most studies that discuss the effectiveness of health policy draw on health care demand models that make *ex ante* predictions of possible policy scenarios. The drawback of these simulations is that the underlying estimates are often based on cross-sectional data that typically show little (spatial) variation. In effect, these simulations concern forecasted

1. The program also included an education program, a labor creation program, and food assistance. See Daly and Fane (2002) for an overview of the programs.

2. Using data from a randomized health insurance and cost sharing experiment in the United States, Manning and others (1987) estimate the demand for outpatient care. Gertler and Molyneaux (1997) use panel data to evaluate an experiment with a user fee increase for outpatient services in Indonesia. The Medicaid program in the United States is probably the most studied targeted health care subsidy program. Currie and Gruber (1996) and Currie and Thomas (1995) exploit variation in legislature across states to control for endogeneity of the program. In an analysis of a school-based health insurance scheme in Egypt, Yip and Berman (2001) treat participation as selection on observed characteristics.

interventions that lie outside the range of the observed price data (Gertler and Hammer 1997).

Second, since the health card entitles users to free services only at public providers, substitution effects between private and public providers can be investigated directly. This is difficult in health care demand studies since information on the price menu offered by alternative health care providers is often unavailable. As an alternative to exogenous price data many models estimate the demand for medical care based on proxy variables derived from (endogenous) household expenditure data (Gertler, Locay, and Sanderson 1987; Lavy and Quigley 1993; Ching 1995; Mocan, Tekin, and Zax 2000) or variations in indirect cost measures, such as opportunity costs due to loss of work or travel time to the nearest provider (Gertler and van der Gaag 1990; Dow 1999). However, opportunity costs do not vary by public or private provider. Studies that do manage to identify price variation across provider types generally find substantial substitution effects between public and private providers as a result of public price policy (Mwabu, Ainsworth, and Nyamete 1993; Sahn, Younger, and Genicot 2003).

The third contribution of this article is that it evaluates the impact of the public spending component of the program and compares its magnitude to that of the targeted price subsidy. Empirical evidence is inconclusive on the causality between spending and health outcomes (World Bank 2004; Filmer and Pritchett 1999). There are empirical studies using provider or community data that show a positive effect of supply and quality of care (especially drug availability) on use (Lavy and Quigley 1993; Mwabu, Ainsworth, and Nyamete 1993; Lavy and Germain 1994; Akin, Guilkey, and Denton 1995; Akin and others 1998; Sahn, Younger, and Genicot 2003). The problem with these quality and supply variables is that they are often endogenous due to government policy, and the measured effects are likely to capture both supply and demand effects. Although some studies manage to control for the endogeneity problem, it is much harder to control for the second effect.

This article identifies effects of both the price subsidy and of the budgetary support on health care use and shows that the largest share of the program's effect is due to increased public spending. The effects of the price subsidy and the supply impulse differ by income group. For low-income groups there is both a substitution from private to public care and an increase in total use because of the health card, but little effect from increased spending. For the more wealthy groups the substitution effect dominates, and the supply-induced effect of the budget increase is larger, possibly since the rich typically face fewer barriers to access to medical care than the poor do. Overall, the nonpoor captured most of the benefit, despite the propoor targeting of the health cards, because of the weak link between financing and use of health cards.

The next section gives an overview of the data. Section II describes the health card program in more detail. Section III focuses on the evaluation problem and the strategy for estimating the impact of the health card on use of

health care services. The results are discussed in section IV, and section V highlights some caveats and examines the sensitivity of the results to the main assumptions of the study.

## I. THE DATA

The study is based on data from Indonesia's nationwide socioeconomic household survey (*susenas*) conducted by the Indonesian Bureau of Statistics. The 1999 survey round contained a module on the use of SSN interventions, including the health card program. The health card program started in September 1998, and the survey was fielded in February 1999. The survey data therefore reflect the experience of the first months of operation. For this reason, and other data limitations, the analysis here is limited to the impact of the program on access to medical care (in terms of use), and no effort is made to estimate the effect on health. The survey sampled 205,747 households and collected a wide range of socioeconomic indicators along with a measure of consumption. In the area of health the survey collected information on self-reported illness, use of health care services, user fees, and ownership and use of the health card. Data from the 1998 household survey were used to provide pre-intervention data for the analysis. This round, also fielded in February, includes 207,645 households and covers the same questionnaire and variables as the 1999 survey, except for the SSN module.

A 1996 village-level census (*podes*) provides pre-intervention information on accessibility and supply of health services and on various other community characteristics. The 1996 *podes* includes 66,486 villages and urban precincts and can be merged with the national household survey.

Administrative data concerning the 1998/99 budget for the SSN program were also used. These data include the budget allocated to 293 districts to implement the health card program and to compensate the public health clinics and village midwives for the expected extra demand for health services resulting from the health card program. The largest share of this budget was transferred directly to public health care providers. The transfers were made in two to four phases, depending on the province, starting in the last quarter of 1998. By the time of the survey SSN budgets had arrived at the health centers.

## II. USE OF HEALTH CARE SERVICES AND THE HEALTH CARD PROGRAM

The economic crisis hit Indonesia in the fall of 1997, exacerbated by social and political unrest in 1998. Real GDP dropped roughly 15 percent in 1998 causing poverty to rise sharply. Suryahadi, Sumarto, and Pritchett (2003) estimate an increase in the poverty headcount ratio from 15 percent in May 1997 to 33 percent at the end of 1998. The consumer price index rose 78 percent in 1998. The price of food doubled, with rice and staple foods experiencing the

most severe increase. There is little evidence of rising overall unemployment during the crisis. Instead, real wages dropped about 40 percent in the formal sector during the first year of the crisis, whereas agriculture seems to have absorbed part of the displaced labor from other sectors (Cameron 1999; Smith and others 2002; Frankenberg, Smith, and Thomas 2003).

The severity of the crisis undoubtedly affected households' health care use and expenditures. Frankenberg, Smith, and Thomas (2003) find that household consumption declined by 20 percent in 1998, with investment in human capital (health care and education) decreasing 37 percent. Data from household surveys on use of modern health care in February of each year for several years before and during the crisis indicate a sharp decrease in the use of modern health care from 1997 to 1998, due largely to declining use of public sector providers (table 1).<sup>3</sup> Waters, Saadah, and Pradhan (2003) attribute the decline to a worsening in the quality of public sector providers. The deterioration was due mainly to the growing shortage of drugs and supplies at public facilities during the crisis, especially in rural areas (Frankenberg, Thomas, and Beegle 1999; Knowles, Pernia, and Racelis 1999). From 1998 to 1999 total use of modern health care providers remained the same, but the share of the public sector increased. One possible explanation for the change is the introduction of the health card program.<sup>4</sup>

Under the SSN health program the allocation of health cards and funds is delegated to lower administrative levels. The amount of subsidy for public health care providers to be distributed across districts, along with the number of health cards to be issued, is determined by a pre-intervention poverty estimate. This poverty measure is constructed by the national family planning board [Badan Koordinasi Keluarga Berencana Nasional (BKKBN)] and counts the number of poor households per district based on "prosperity status." Under this definition a household is deemed poor when it has insufficient funds for any one of the following: to worship according to its faith, to eat basic food twice a day, to have different clothing for school or work and home, to have an improved floor (not made of earth), or to have access to modern medical care for children or access to modern contraceptives. The BKKBN regularly collects this information on a census basis.<sup>5</sup>

3. Modern health care is defined as public health care providers—hospitals, health clinics (*puskesmas*), village maternity posts (*polindes*), and integrated health posts (*posyandu*)—and private providers—hospitals, doctors, clinics, and paramedical services. Traditional health care is not included.

4. Another explanation for the dip in 1998 would be that households postponed preventive care, in anticipation of introduction of the health card. But this is unlikely because the program had not been announced at the time of the 1998 survey.

5. Use of the BKKBN prosperity measure results in a higher poverty headcount (42 percent of households in December 1997) than use of consumption-based measures [24 percent of population in February 1998, according to estimates by Suryahadi, Sumarto, and Pritchett (2003) based on the *susenas* consumption module for 1998]. The BKKBN measure has been criticized for being an unsuitable allocation criterion for the SSN, since its components are fairly inflexible and inappropriate for measuring economic shocks or the impact of a crisis. However, at the time of implementation it was the only up-to-date welfare measure at hand.

TABLE 1. Changes in Outpatient Contact Rates for Public and Private Care with SSN Program, 1995–99 (percentage of population that visited provider at least once in previous month)

Provider	1995	1997	1998	1999	1999 without SSN program
Public	7.00 (0.083)	6.65 (0.085)	5.03 (0.062)	5.34 (0.071)	4.87
Private	6.48 (0.073)	6.71 (0.079)	6.11 (0.070)	5.80 (0.078)	5.67
Overall outpatient care (public or private) <sup>a</sup>	12.83 (0.111)	12.83 (0.118)	10.48 (0.098)	10.53 (0.110)	9.98
Number of observations	873,647	887,266	880,040	864,580	

Note: Numbers in parentheses are standard errors.

<sup>a</sup>The contact rate for all modern care is smaller than the sum of the contact rates for public and private care since individuals who sought both public and private care are counted only once in the aggregate.

Source: Authors' analysis based on data from Indonesia's annual *susenas* household survey; see description in text.

At the district level committees were formed to deal with the allocation of funds to the health clinics and village midwives. The allocation was based on the BKKBN estimate of poor households eligible for a health card in the village or subdistrict served by each public provider rather than on actual services provided to health card owners. The district committee allocated health cards to villages, again based on the BKKBN measure, for distribution by village committees headed by local leaders. Along with the health cards the village committees received guidelines on which criterion to use when selecting households for the health card program. Besides households that were classified as poor by the BKKBN, the village committees were to consider households that were severely affected by the crisis. Local leaders maintained considerable discretion to distribute health cards according to their own insights, however. Health cards were usually distributed through local health centers and village midwives.

The health card entitled the owner and family members to free services at public health care providers consisting of outpatient and inpatient care, contraceptives for women of child-bearing age, prenatal care, and assistance at birth. A health card was not transferable to other households. The analysis here looks only at the impact of the health card program on outpatient health care use.

By February 1999 the health card program was already of substantial size, with 10.6 percent of Indonesians reporting that their household had been allocated a health card. The share rose to 18.5 percent among individuals from the

TABLE 2. Distribution of Health Card Allocation and Use, by per Capita Consumption Quintile, Gender, and Urban or Rural Residence (percent)

Characteristic	Coverage	Share in allocation	Share in use
Quintile 1 (poor)	18.5	33.7	31.3
Quintile 2	13.7	25.7	24.4
Quintile 3	10.6	20.1	20.4
Quintile 4	7.1	13.4	14.9
Quintile 5 (rich)	3.7	7.1	9.0
Male	10.5	49.3	43.8
Female	10.8	50.7	56.2
Urban	7.2	26.8	29.5
Rural	12.8	73.2	70.5
Indonesia	10.6	100.0	100.0

Note: Number of observations is 822,607.

Source: Authors' analysis based on data from Indonesia's annual *susenas* household survey; see description in text.

poorest 20 percent of the population (table 2) and 13.7 percent among those in the second poorest quintile (about half of whom were estimated to live below the poverty line). There was considerable leakage to more wealthy households, however. Whereas the poorest 20 percent of the population own 34 percent of the health cards, households from the wealthiest 60 percent of the population possess about 40 percent of the health cards. Use of health cards is also propoor but slightly less so. Those who received benefits were on average wealthier than those who received the card.

Use of outpatient care is higher among households that own a health card, especially, use of public services (table 3). Overall, 12 percent of health card owners visited an outpatient provider in the month before the survey compared with 10 percent of those without a health card.

TABLE 3. Use of Health Card (percentage that sought care at least once in previous month)

Use characteristic	Head of household reports having received a health card	Head of household reports not having received a health card
Received outpatient care	12.4	10.4
Went to public provider	8.2	5.0
Went to private provider	5.0	5.9
Number of observations	81,126	741,481

Source: Authors' analysis based on data from Indonesia's annual *susenas* household survey; see description in text.

### III. IMPACT OF HEALTH CARD PROGRAM ON USE OF HEALTH CARE SERVICES

What would the use of outpatient health care services have been if the health card program had not existed? This question incorporates two effects: the effect of the health card price subsidy and the effect of the additional budgetary resources made available to public sector services through the SSN program. Because of the weak link between these two components of the program, the two effects are treated as separate interventions.

#### *Disentangling Two Interventions*

The assumption is that the first intervention—the distribution of health cards—benefits only those who own a health card, whereas the second intervention can potentially benefit the whole population, depending on the size of the grant to the health care provider. This assumption rules out external or general equilibrium effects. Because only short-term impacts are considered, health-related general equilibrium effects are assumed not to be substantial since they are likely to take longer to materialize. However, externalities through congestion or crowding out induced by the program may also compromise the independence assumption. Sensitivity to these effects is examined in section V.

Under the independence assumption the combined average impact of the two interventions can be written as the sum of the two impacts separately. Let  $Y_i(h_i, q_j)$  denote the outcome for individual  $i$  living in district  $j$  as a function of the two interventions, with  $h_i = 1$  if a person lives in a household that has received a health card and 0 if not. The amount of SSN budgetary support to public health care providers in the area where the person lives (indicated by  $SSN_j$ ) is reflected by  $q_j$ .

The analysis seeks to establish to what extent the observed development in use from 1998 to 1999 is due to these two interventions. The overall impact of the program can be expressed as a weighted mean of the impact on people with a health card ( $h_i = 1, q_j = SSN_j$ ) and on people who did not receive a health card but who benefited only from the budget increase ( $h_i = 0, q_j = SSN_j$ ). Under the independence assumption, the overall impact can be written as

$$\begin{aligned}
 & p\{E[Y_i(1, SSN_j)|h_i = 1, q_j = SSN_j] \\
 & \quad - E[Y_i(0, 0)|h_i = 1, q_j = SSN_j]\} \\
 & + (1 - p)\{E[Y_i(0, SSN_j)|h_i = 0, q_j = SSN_j] \\
 & \quad - E[Y_i(0, 0)|h_i = 0, q_j = SSN_j]\}
 \end{aligned} \tag{1}$$

where  $p = \Pr(h_i = 1)$ . The observed average outcome for people with a health card is  $E[Y_i(1, SSN_j) | h_i = 1, q_j = SSN_j]$ , whereas  $E[Y_i(0, SSN_j) | h_i = 0,$

$q_j = \text{SSN}_j]$  reflects the observed average outcome for people who did not receive a health card. The other two terms reflect the expected counterfactual outcomes for the two groups: what would have happened if the programs had not been implemented. Equation 1 can be rewritten by adding and subtracting  $pE[Y_i(0, \text{SSN}_j) | h_i = 1, q_j = \text{SSN}_j]$ , as

$$\begin{aligned} & p\{E[Y_i(1, \text{SSN}_j) | h_i = 1, q_j = \text{SSN}_j] \\ & \quad - E[Y_i(0, \text{SSN}_j) | h_i = 1, q_j = \text{SSN}_j]\} \\ & \quad + E[Y_i(0, \text{SSN}_j) | q_j = \text{SSN}_j] \\ & \quad - E[Y_i(0, 0) | q_j = \text{SSN}_j]. \end{aligned} \quad (2)$$

Here the first two terms (weighted by  $p$ ) give the impact of the pure health card program, conditional on the budget increase, for those who own a health card. This is referred to as the direct effect of the program. The last two terms reflect the effect of the budget increase for the whole population, referred to as the indirect effect of the SSN program.

#### *Estimation Strategy*

Both the direct health card effect and the overall effect are estimated. The indirect effect of the program cannot be identified directly. Instead, the impact of the general increase in funding to public services is derived by subtracting the direct effect estimate from the total effect estimate.

For estimating the direct effect of the health card intervention, a control group is formed from the population that did not receive a health card. Since both those with and those without health cards benefited from the transfer of funds to health care providers, this measures the differential effect of owning a health card conditional on the transfer program. Since selection was not random, a direct comparison of those with and those without health cards after introduction of the program does not yield a valid impact estimate. The health card was distributed to poor households, and even without a health card use of health care services by these households would have been different from that of wealthier households without health cards. It is also possible that health cards were allocated based on need. In that case health card recipients would, on average, use more health care, even without the health card.

Propensity score matching is used to correct for nonrandom placement of the program, relying on matching on observables and the assumption of conditional independence<sup>6</sup> (that is, conditional on a set of observed characteristics

6. We experimented with instrumental variables but abandoned this approach because we were not convinced that we could construct adequate instruments. We used variables that measure the perception of fairness of the distribution of health cards in the district. But the results were very sensitive to specification and choice of instrument. We also experimented with 1997 district BKKBN estimates. However, when using 1998 data we found that these variables appear to be correlated with the level of use (but not with changes).

selection into the program can be treated as random<sup>7</sup>). The unit of analysis is the household, as health cards were distributed at this level. Households in the treatment group are then matched to households in the potential control group.<sup>8</sup>

The extent to which propensity score matching will reduce the bias depends on the specification of the propensity score model and the quality of the control variables (Heckman, Ichimura, and Todd 1997). It is therefore crucial to understand the program design and to include sufficient information about the selection procedure (at all allocation levels) in the model. There are two main sources of bias. The first is the endogenous placement of health cards with households. The second relates to systematic differences in regional program intensity between the control and the treatment groups. District-fixed effects are included to control for these regional differences. They capture any between-district variation in the allocation of health cards and SSN funding. BKKBN poverty estimates for subdistricts control for the allocation of subsidies within districts and the number of health cards issued in the areas covered by the public health facilities. Thus, matched households live in areas that enjoy similar program intensity in terms of health card coverage and SSN budget.

Endogenous program placement is caused by purposive targeting at different stages in the decentralized allocation process. To control for endogenous program placement at the village level, variables from the village-level census are included that reflect pre-program access to health care: number of public health clinics, auxiliary health clinics, and maternity facilities in the village; dummy variables indicating whether the majority of village traffic is by land; and a dummy variable reflecting village leaders' opinions about the accessibility of health clinics. Because health cards are distributed by local facility staff, the number of doctors and village midwives living in the village (per 1,000 inhabitants) is included as a proxy for informal networks within the village. Finally, the level of education of the village leader is included, as well as dummy variables indicating eligibility for *Inpres Desa Tertinggal* (IDT), an antipoverty program for economically less developed villages, and whether the village is located in a rural area.

For households the five criteria of the BKKBN prosperity status are included as dummy variables. Other household welfare variables include housing characteristics (type of house occupied; type of roof, walls, and floor; sewage, sanitation, and drinking water facilities; and source of light), sector of main source of household income, and employment status of the head of household. Other controls include household composition (gender and age), household size, and characteristics of the head of household (gender and education level). Per capita consumption is endogenous (a health card reduces health care expenses) and is

7. Following Rosenbaum and Rubin (1983). Smith and Todd (2005) provide an insightful discussion on the application and pitfalls of propensity score matching in the recent literature.

8. As a result, the sample sizes of the treatment and matched control group differ because households vary in number of people.

therefore omitted. A household with a health card would, on average, report a lower consumption level than it would if it had not received a health card.<sup>9</sup> If household expenditure were added to the propensity score function, the control group would be less wealthy than the intervention group. Consequently, the health card effect would be overestimated.

Health status is the one important unobserved variable that is missing from this specification. Soelaksono and others (1999) provide some evidence that health cards were allocated based on illness. This is reinforced by the fact that self-reported illness is higher among health card owners. This would suggest that the positive bias due to health status outweighs the negative bias due to propoor targeting. The *susenas* survey records self-reported illness, but this is prone to reporting bias and may be endogenous to health card allocation.<sup>10</sup> Health status is therefore omitted from the propensity score function. Many of the individual characteristics included will reduce the health bias (for example, age, housing and sanitary conditions, BKKBN criteria), but some may remain. However, it is shown later that the potential bias from omitting health status is small and that the estimation results are robust and within reasonable bounds.

The propensity score function was estimated as a logit, separately for each of the five main regions in Indonesia.<sup>11</sup> This restricted the match to households in the same region. A household with a health card living in Java, for instance, will not be matched with a household without a health card living in Sumatra. The pseudo  $R^2$  for the regional models ranged from 0.12 to 0.26. Nearest neighbor matching, the simplest matching procedure, was applied to households within the range of common support.<sup>12</sup>

The matched households are very similar in the individual observed characteristics that entered into the matching function (table 4). From table 4 it appears that, before the match, households that owned a health card perform worse on the BKKBN criteria, are slightly larger, and work more often in agriculture compared with households that do not own a health card. Heads of households with a health card have less education on average and are more likely to be female. After the match the control and intervention groups are well balanced across the observed characteristics.

The second panel of table 4 shows variables that were not included in the matching function. Both program intensity variables are balanced for the

9. See van de Walle (2003) for a discussion on assumptions about behavioral responses regarding the effect of public policy on household consumption.

10. Using an experiment with increases in user fees in Indonesia, Dow and others (2000) provide an illustration of the problem of reporting bias and measurement error in self-reported health status. Whereas objective measures of health show that increasing user fees leads to a deterioration in health status, self-reported measures suggest an improvement in health. Dow and others (2000) argue that this reporting bias is correlated with exposure to the health system, which is affected by the user fee increase. See also Strauss and Thomas (1998) for a more general discussion.

11. The five regions are defined as Java and Bali, Sumatra, Sulawesi, Kalimantan, and Other Islands.

12. The estimation results for the propensity score function and details on the matching procedure are reported in the supplemental appendix, available at <http://wber.oxfordjournals.org/>.

TABLE 4. Descriptive Statistics for Households with and without a Health Card and for Matched Pairs

Variable	All households		Matched pairs		Difference in means	Standard error
	No health card	Health card	No health card <sup>a</sup>	Health card		
<i>Included in matching</i>						
Propensity score	0.0823	0.2488	0.2433	0.2433	-0.0000	0.0018
Female head of household	0.1268	0.1608	0.1618	0.1601	-0.0017	0.0038
Education head of household						
No education completed	0.3641	0.5087	0.5090	0.5073	-0.0017	0.0052
Primary	0.2985	0.3324	0.3289	0.3327	0.0038	0.0049
Junior secondary	0.1220	0.0814	0.0818	0.0818	0.0000	0.0028
Senior secondary	0.1689	0.0667	0.0693	0.0674	-0.0019	0.0026
Higher	0.0465	0.0107	0.0111	0.0108	-0.0003	0.0011
Head of household unemployed	0.0079	0.0074	0.0075	0.0074	-0.0001	0.0009
Household size	4.2043	4.2576	4.2211	4.2449	0.0238	0.0189
BKKBN household prosperity criteria						
Worship	0.9343	0.8894	0.8911	0.8902	-0.0010	0.0032
Food	0.9835	0.9778	0.9785	0.9790	0.0004	0.0015
Clothing	0.9645	0.9473	0.9487	0.9487	0.0000	0.0023
Floor	0.8193	0.5935	0.5962	0.5954	-0.0008	0.0051
Health	0.8899	0.9061	0.9056	0.9057	0.0001	0.0030
Main source of household income						
Agriculture, farming	0.4551	0.5568	0.5526	0.5546	0.0020	0.0051
Mining, quarrying	0.0097	0.0089	0.0089	0.0089	-0.0001	0.0010

Processing industry	0.0687	0.0685	0.0655	0.0682	0.0027	0.0026
Electricity, gas, water	0.0022	0.0007	0.0009	0.0007	-0.0002	0.0003
Construction	0.0400	0.0494	0.0507	0.0496	-0.0011	0.0023
Trade	0.1482	0.1180	0.1206	0.1193	-0.0013	0.0034
Transport, storage, communications	0.0510	0.0519	0.0522	0.0521	-0.0002	0.0023
Finance, insurance, real estate	0.0091	0.0031	0.0026	0.0031	0.0005	0.0006
Services	0.1462	0.0931	0.0957	0.0936	-0.0021	0.0030
Other	0.0028	0.0037	0.0033	0.0036	0.0004	0.0006
Income recipient	0.0672	0.0459	0.0470	0.0464	-0.0006	0.0022
Rural area	0.6792	0.7880	0.7856	0.7862	0.0006	0.0042
IDT village <sup>b</sup>	0.2822	0.3495	0.3476	0.3444	-0.0032	0.0049
BKKBN rate per subdistrict	0.3088	0.4407	0.4417	0.4390	-0.0028	0.0026
<i>Not included in matching</i>						
Program intensity at district level						
SSN budget per capita	1.6164	1.8178	1.8154	1.8147	-0.0007	0.0099
Health card coverage	0.0886	0.1885	0.1865	0.1870	0.0004	0.0012
Weight for age Z-score, children under five <sup>c</sup>	-1.2116	-1.2943	-1.2924	-1.2987	-0.0063	0.0244
Member of household ill	0.3110	0.3620	0.3293	0.3605	0.0312	0.0049
Number of observations	173,366	18,993	18,727	18,727		

<sup>a</sup>Includes 406 households that are matched more than once.

<sup>b</sup>Villages in the IDT antipoverty program.

<sup>c</sup>*Susenas* nutrition module, 1999. Total sample is 7,902 children with a health card and 64,946 children without a health card. Matched sample is 7,502 children with a health card and 6,891 children without a health card.

*Source:* Authors' analysis based on data described in text.

matched households, while they differ strongly for the nonmatched households. This confirms that the district dummy variables in the matching function managed to control for variation in the size of the grants and health card coverage in the district. The match has also balanced the weight for age  $Z$  score of children under age five.<sup>13</sup> Weight is indicative of the health of children over a period of time, which, in this case, will reflect mostly the period before the launch of the health card. In the absence of panel data it is thus the best proxy for balance in pre-intervention outcome variables.<sup>14</sup> Section V further investigates the robustness of the impact estimate to health status.

Comparing means of the matched treatment and control group yields the average direct effect of the health card intervention on the use of outpatient services by health card owners. This is obtained by estimating the regression:

$$Y_i = \delta + \beta HC_i + \varepsilon_i \quad (3)$$

on the matched sample, applying sample weights. The term  $\hat{\beta}$  is an unbiased estimate of the treatment effect for those who are selected into the program:

$$\hat{\beta} = E[Y_i(1, SSN_j) - Y_i(0, SSN_j) | h_i = 1, q_j = SSN_j]. \quad (4)$$

Weighting this by the probability of selection into the program,  $\hat{p} = \Pr(h_i = 1)$ , gives the average direct health card effect,  $\hat{p}\hat{\beta}$ , defined in equation (2).

The overall impact of the program, as defined in equation (1), is obtained by exploiting regional variation in the financial compensation of public health care providers for the health card program and the fact that the allocation to districts was based on pre-intervention poverty estimates. Pre-health card use rates—based on the 1998 *susenas* survey—are compared with health care use rates right after introduction of the health card program. The impact estimate is a result of the two interventions acting simultaneously. The robustness of this approach is evaluated later in the article.

Administrative data on the 1998/99 budget allocated for transfers to public health facilities were used to measure the variation in SSN compensation. The variation was substantial. For example, Sulawesi's allocation was (weighted by district population) 29 percent higher than Sumatra's and 34 percent higher than Java and Bali's, but about half that allocated to the smaller islands. The total SSN budget allocated in block grants to public providers amounted to 159 billion rupiah, or 1,432 rupiah per capita.

The effect of the general increase in funding is modeled as a linear function of the budget allocation. For district  $j$  in time period  $t$ , use of health services is

13. The weight for age  $Z$  score is based on a 1999 *susenas* nutrition module covering 72,848 children under age five.

14. Waters, Saadah, and Pradhan (2003) find that the crisis had no observable effect on the weight for age  $Z$  score. Effects of the health card program on the score are unlikely as it did not cover nutritional programs.

written as

$$Y_{jt} = \alpha_j + \theta_0 d_t + \sum_{r=2}^5 \theta_r d_r d_t + \gamma \frac{\text{SSN}_{jt}}{N_{jt}} + \phi' W_{jt} + \varepsilon_{jt} \quad (5)$$

where  $\text{SSN}_j$  is the amount of compensation for public health clinics allocated to district  $j$ , and  $N_j$  denotes the district population size. The time subscript,  $t$ , refers to either the time period before the intervention (1998) or the time period after the intervention (1999). The time dummy variable,  $d_t$ , takes a value of zero if the period is 1998 or one if it is 1999 and is interacted with five region-specific fixed effects,  $d_r$ , to allow for some flexibility in capturing the time effect.<sup>15</sup> In the pre-intervention year  $\text{SSN}_j$  equals zero for all districts. A set of regional welfare and demographic characteristics,  $W_{jt}$ , are also added to the model. These include the poverty rate,  $P_0$ , and poverty gap,  $P_1$ , for the districts, the average age and household size, the district population size, and the fraction of the population living in a rural area. Frankenberg, Smith, and Thomas (2003) show evidence of changes in household size and migration between urban and rural areas as households restructured their composition in response to the crisis. Although the average household size increased in (lower cost) rural areas, the number of working age family members increased in urban households.

The nonrandom allocation of the SSN budget is accounted for by a district-fixed effect,  $\alpha_j$ . This removes any bias due to unobserved time-invariant factors that affect geographic allocation and are also correlated with health care use. The fact that the SSN budget allocation was determined by static pre-program poverty estimates, and not on the basis of dynamic changes in poverty, legitimizes the fixed-effects approach.

Taking differences across districts over time gives

$$\Delta Y_{jt} = \theta_0 + \sum_{r=2}^5 \theta_r d_r + \gamma \frac{\text{SSN}_{j99}}{N_{j99}} + \phi' \Delta W_{jt} + \Delta \varepsilon_{jt}. \quad (6)$$

Estimating equation (6) by ordinary least squares (OLS) yields unbiased estimates under the assumption that the allocation of SSN funds is not correlated with time-variant unobservables. If the geographic allocation is correlated with important district-level trends that are not captured by the time dummies or  $\Delta W_{jt}$ , then OLS estimates may still be biased. This is not very likely, given that the BKKBN indices are badly suited for capturing changes in welfare. Further reassurance is given by the fact that no correlation is found between SSN allocation (per capita) and pre-program changes in use from 1997 to 1998.

15. Java and Bali (region 1) are used as the reference group.

The overall impact of the program is then obtained by taking a population-weighted average of the effects for the districts

$$\sum_{j=1}^J \hat{\gamma} \frac{SSN_j N_j}{N_j N} = \hat{\gamma} \overline{SSN} \quad (7)$$

where  $\overline{SSN}$  is the average financial compensation for the health card program per person across the country, and  $J$  is the number of districts.

The estimated impact of the supply impulse on the use of outpatient services (the indirect effect) is given by the difference between the estimate of the average total effect and the average direct health card effect. Inserting equation (7) and the estimate of  $\beta$  into equations (3) and (2) yields an expression for the impact of the general budget increase for public service providers

$$\begin{aligned} E[Y_i(0, \overline{SSN}_j) | q_j = \overline{SSN}_j] \\ - E[Y_i(0, 0) | q_j = \overline{SSN}_j] = \hat{\gamma} \overline{SSN} - \hat{p} \hat{\beta} \end{aligned} \quad (8)$$

#### IV. RESULTS

The estimation results of the direct health card effect on outpatient use for health card owners,  $\hat{\beta}$ , and the average direct effect,  $\hat{p} \hat{\beta}$ , are summarized in table 5. The estimate of  $\hat{p}$  is simply the fraction of individuals (for a specific population group) living in a household that owns a health card. The table also shows the percentage change relative to the counterfactual.

Health card ownership resulted in a 1 percentage point increase in the use of outpatient services, a 9.1 percent increase relative to the base counterfactual. This increase was due to an increase in use among the poorest quintiles. Only a substitution effect is observed among the richest quintile, from private to public health care providers. For all income groups health card ownership resulted in an increase in the use of public sector services and a decrease in the use of private sector services. For the richest quintile the two effects cancelled out each other. There was a small but statistically insignificant increase in overall use. The shift from private to public care seems to have occurred in both urban and rural areas. The health card program affected use more among women than among men, possibly because maternity services were covered under the program. Both the overall increase in outpatient visits and the substitution effect from private to public services were larger for women.

Table 5 also presents the estimates of  $\gamma$  from equation (6), and estimates of the overall effect of the program ( $\hat{\gamma} \overline{SSN}$ ), defined in equation (7). The results indicate an absolute increase in the use of outpatient services of 0.5 percentage point, which stems mainly from an increase in the use of public services, as the

TABLE 5. Impact of Health Card Ownership and Overall Effect of SSN Interventions on Use of Outpatient Services (one month reference period)

	Intervention group	Control group	Direct effect of health card <sup>a</sup>		Overall effect of SSN <sup>b</sup>		Indirect effect <sup>c</sup> (percentage share of overall effect) <i>p</i>	
			Difference ( $\hat{\beta}$ )	Change (%)	Direct effect ( $\hat{p}\hat{\beta}$ )	Coefficient ( $\hat{\gamma}$ )		Overall effect ( $\hat{\gamma} \overline{SSN}$ )
All outpatient visits								
Quintile 1 (poor)	0.0993	0.0869	0.0123*	14.2	0.0023*	0.0039	0.0056	(58.93)
Quintile 5 (rich)	0.1510	0.1451	0.0059	4.0	0.0002	0.0075**	0.0108**	98.15
Male	0.1158	0.1069	0.0089*	8.3	0.0009*	0.0037***	0.0053***	83.02
Female	0.1270	0.1157	0.0113*	9.8	0.0012*	0.0040***	0.0058***	79.31
Urban	0.1392	0.1281	0.0110*	8.6	0.0008*	0.0045	0.0064	(87.50)
Rural	0.1149	0.1061	0.0088*	8.3	0.0011*	0.0060**	0.0086**	87.21
All	0.1215	0.1113	0.0101*	9.1	0.0011*	0.0039***	0.0055***	80.00
Outpatient public								
Quintile 1 (poor)	0.0729	0.0542	0.0187*	34.6	0.0035*	0.0035	0.0049	(28.57)
Quintile 5 (rich)	0.0841	0.0590	0.0251*	42.5	0.0009*	0.0094*	0.0134*	93.28
Male	0.0734	0.0537	0.0197*	36.8	0.0021*	0.0026***	0.0037***	43.24
Female	0.0871	0.0622	0.0249*	40.1	0.0027*	0.0040**	0.0057**	52.63
Urban	0.0869	0.0628	0.0241*	38.3	0.0017*	0.0016	0.0023	(26.09)
Rural	0.0779	0.0565	0.0214*	38.0	0.0027*	0.0053*	0.0076***	64.47
All	0.0804	0.0580	0.0224*	38.6	0.0024*	0.0033**	0.0047**	48.94
Outpatient private								
Quintile 1 (poor)	0.0305	0.0371	-0.0066*	-17.7	-0.0012*	0.0020	0.0028	
Quintile 5 (rich)	0.0803	0.0983	-0.0179*	-18.2	-0.0007*	-0.0016	-0.0023	
Male	0.0501	0.0601	-0.0100*	-16.6	-0.0010*	0.0012	0.0017	

(Continued)

TABLE 5. Continued

	Intervention group	Control group	Direct effect of health card <sup>a</sup>			Overall effect of SSN <sup>b</sup>		Indirect effect <sup>c</sup> (percentage share of overall effect) <i>p</i>
			Difference ( $\hat{\beta}$ )	Change (%)	Direct effect ( $\hat{p}\hat{\beta}$ )	Coefficient ( $\hat{\gamma}$ )	Overall effect ( $\hat{\gamma} \overline{SSN}$ )	
Female	0.0477	0.0606	-0.0129*	-21.3	-0.0014*	0.0005	0.0008	
Urban	0.0613	0.0726	-0.0113*	-15.5	-0.0008*	0.0031	0.0045	
Rural	0.0442	0.0565	-0.0123*	-21.7	-0.0016*	0.0013	0.0019	
All	0.0489	0.0604	-0.0115*	-19.0	-0.0012*	0.0009	0.0013	

\*Significant at the 1 percent level; \*\*significant at the 5 percent level; \*\*\*significant at the 10 percent level.

*Note:* Detailed estimation results are available in the supplemental appendix (tables S.7, S.9–S.11).

<sup>a</sup>Number of observations: 76,903 individuals in treatment group, 73,986 in control group. Bootstrapped standard errors with 500 replications.

<sup>b</sup>Number of observations: 293 districts.

<sup>c</sup>Numbers in parentheses are indirect effect estimates that are based on imprecise estimates of the total effect.

*Source:* Authors' analysis based on data described in text.

program does not seem to affect the private sector. The effect is larger for wealthier households. For poor households the estimates are smaller and imprecise. As with the direct health card effect, the overall effect of the program on public services is larger for females than for males. The program had the largest impact on the use of public care in rural areas; in urban areas the estimates are imprecise. Since private care seems unaffected, the results are similar for the overall effect on use.

The indirect effect that could be attributed to an overall supply or quality impulse as a result of the extra budget support in the public sector seems to have been a main contributor to the increase in the use of public health care services. Combining the estimates of the direct health card effect with the overall effect of the SSN permits investigation of what share of the increase in the use of public sector services is due to the indirect effect (as defined in equation (8)). The share of the indirect effect in the total effect is given by  $1 - [(\hat{\beta}) / (\hat{\gamma} \overline{\text{SSN}})]$ . The indirect effect accounts for about 80 percent of the overall increase in use. In the public sector about half of the total increase can be attributed to the indirect effect of the budget increase. The results also suggest that the indirect benefits of the program increase with income. For the richest quintile only 7 percent of the increased use of public care can be attributed to the health card itself whereas for the poor there is less clear evidence of an indirect effect. The indirect effect for the poor is smaller, but based on an imprecise estimate. Finally, the supply impulse had an above average effect in rural areas, emphasizing the shortage of resources of rural public health care providers. The overall effects for the private sector are not significant, so the indirect effects are not calculated.

So, can the revival in use of public sector health services be attributed to the SSN program? The answer appears to be yes. The results reported in table 5 can be used to estimate use had the health card program not existed. Table 1, which reported trends in health care use, included the counterfactual for public and private health care use in the absence of the health card program. From 1998 to 1999 the contact rate for public health services increased from 5.0 percent to 5.3 percent, whereas the contact rate for all modern health care providers remained stable at 10.5 percent. The estimates suggest that without the health card program public health care use would have dropped further to 4.9 percent and the overall contact rate would have dropped to 10.0 percent.

## V. CAVEATS AND SENSITIVITY ANALYSIS

This section discusses some caveats to the empirical analysis and examines the robustness of the results with respect to specification and the main assumptions.

*Crowding Out, Congestion, and Interaction Effects*

The main assumption underlying the study is that use of health care services by households with health cards is independent of that by households without health cards. This implies that the number of health card recipients (program intensity) in the region does not affect use of care for nonrecipients and that both groups enjoy similar benefits from the SSN budget. However, if health care supply were inelastic, then distributing health cards could lead to congestion and crowding out. For example, if services were delivered to health card owners according to set standards, resources would be redistributed from nonrecipients to health card recipients. In this case the estimated direct effect of the health card would be biased upward. The difference in use would consist of the “true” health card effect and the crowding out effect. Alternatively, externalities can manifest themselves if the direct benefits of the health cards do not follow set standards but are contingent on available resources. The quality of care provided to health card owners will then increase with the SSN budget.

One might argue that the external effects of health card allocation are likely to be small. Since health card coverage is 11 percent and concentrated among the poor, whose health care demand is typically low, it is unlikely that the program would strain the capacity of health care facilities. For example, doubling the use of public health services for health card owners would result in 16 percent more outpatient visits for a typical public health care facility. The district dummy variables included in the matching functions do capture program intensity and the supply shock induced by the SSN program (Table 4). Moreover, the estimation method allows for effect heterogeneity due to regional variation in program intensity, since the estimated impact for all the households with a health card is simply averaged.

Nevertheless, it is possible to test for the presence of externalities by controlling for program intensity when estimating the direct effect, and including interaction effects of health card ownership with the average number of health cards distributed in the district and the per capita amount of the SSN subsidy. Crowding out or congestion would imply that the interaction effects for program intensity are statistically significant. If crowding out or congestion effects are important, they would be expected to be stronger in areas where the program is underfunded. These are areas where a large number of health cards are distributed compared with the budget that is received. This can be tested by including the amount of the SSN subsidy per allocated health card as regressor and interacting this with the health card dummy variable. Statistically significant interaction effects would indicate the presence of general equilibrium and external effects.

The top panel of table 6 shows estimates of the direct effect given different specifications. The results suggest that the estimated direct effect is not biased due to externalities. Specification 1 gives the initial estimates. Specification 2 controls for the fraction of the population with a health card and the SSN

TABLE 6. Sensitivity of Impact Estimates to Different Specifications and Assumptions

	Overall outpatient care	Public	Private
Direct effect of health card			
1. Original estimate table 5	0.0101* (0.0020)	0.0224* (0.0015)	-0.0115* (0.0015)
2. Program intensity control variables <sup>a</sup>	0.0109* (0.0020)	0.0235* (0.0015)	-0.0113* (0.0015)
3. Interaction effects <sup>a</sup>			
a. SSN per capita and health card allocation per capita	0.0106** (0.0052)	0.0272* (0.0038)	-0.0140* (0.0038)
b. SSN per health card in district	0.0114* (0.0021)	0.0239* (0.0016)	-0.0109* (0.0016)
4. Selection on needs <sup>b</sup>	0.0081* (0.0020)	0.0201* (0.0016)	-0.0117* (0.0015)
Total effect of SSN program			
5. Original estimate table 5	0.0039*** (0.0022)	0.0033** (0.0015)	0.0009 (0.0013)
6. Health card coverage in district <sup>c</sup>	0.0042 (0.0028)	0.0037 (0.0019)	0.0007 (0.0017)
7. Interaction effects <sup>d</sup>	0.0044 (0.0032)	0.0039 (0.0022)	0.0007 (0.0020)

\*Significant at the 1 percent level; \*\*significant at the 5 percent level; \*\*\*significant at the 10 percent level.

*Note:* The coefficients of the interaction terms and other covariates are omitted for convenience. Detailed estimation results are available in the supplemental appendix (tables S.12–S.16).

<sup>a</sup>Probit marginal effects. Specification 2 includes SSN budget per capita and health card coverage in districts. Specification 3 adds interaction terms of these two variables with the treatment dummy variable. Both specifications include age, gender, characteristics of head of household (gender, education), household size, BKKBN prosperity status, main source of income (agriculture/no agriculture), village status (rural, IDT antipoverty program), availability of health providers in the village or township, subdistrict BKKBN index, and district poverty profile ( $P_0$ ,  $P_1$ ). The sample concerns the same set of individuals from matched households as in table 5. Numbers in parentheses are robust standard errors.

<sup>b</sup>The propensity score function includes a dummy variable that indicates whether a health complaint has disrupted work, school, or the daily activities of a household member.  $N$  treated = 76,956,  $N$  control = 74,263. Numbers in parentheses are bootstrapped standard errors (with 500 replications).

<sup>c</sup>Similar to specification 5, with health card coverage in districts added.

<sup>d</sup>Similar to specification 5, with interaction term (health card coverage in districts)  $\times$  (SSN per capita in district) added.

*Source:* Authors' analysis based on data described in text.

budget per capita allocated to the districts, the subdistrict BKKBN index, and district poverty indicators  $P_0$  and  $P_1$ . It further includes a set of individual and household characteristics, IDT village and rural area dummies, and the availability of health facilities in the village. Specification 3 includes the interaction terms. The interaction effects are not statistically significant for public and overall outpatient care. For private care, there is a small positive and weakly significant effect only for the SSN subsidy interaction term. This is an interesting finding, because doctors working at public facilities in Indonesia often maintain private practices. This could suggest that in districts with relative SSN budget abundance, some doctors have used the SSN subsidy to treat health card recipients in their private practice. The impact estimate is robust to different specifications. The point estimate for the direct health card effect on overall use is slightly larger, but still within one standard deviation, whereas the substitution effect between public and private is also slightly larger.

#### *Selection on Health Status*

A potentially more serious problem is the failure to take into account the possibility that households may have been selected based on health status. Those with poor health may have received a health card because of their higher anticipated need while otherwise similar individuals did not receive one. Officially, health cards should have been distributed based on BKKBN criteria, but health status could well have played a role in actual distribution. If so, failing to include a measure of health status in the matching function will result in an intervention group with a worse health status than the control group. Poor health will, other things being equal, increase the demand for health care. The resulting impact estimate will be larger or equal to the true effect.

The only measure of health status that the *susenas* survey collects is self-reported illness. However, including self-reported illness in the matching function would likely have resulted in an underestimate of the true health card effect. Evidence indicates that self-reported illness depends on the affordability of care. The rich report illness more often than the poor, which is surely not a result of the rich having a worse health status than the poor. If self-reported illness depends on the affordability of health care, and health care is more affordable for those who own a health card, then matching on self-reported illness will result in a control group with worse health status than the intervention group. Better health will, other things being equal, decrease the demand for primary health care. Thus the impact estimate would have been an underestimate.

Two impact estimates, one obtained without and one with self-reported illness included in the matching function, can provide some notion of the extent of the bias. The health card effect should lie between the estimate that controls for self-reported illness (lower bound) and the one that does not (upper bound). The results suggest that the estimates presented earlier are not sensitive to systematic differences in health status, since the estimated bounds lie close to each other. A dummy variable was included in the matching

function that indicated whether a health complaint had disrupted work, school, or daily activities for any member of the household during the last month. Specification 4 in table 6 gives the results for a one-month reference period. The impact estimate for all outpatient care decreases slightly, from 0.0101 to 0.0081. The point estimates are within one standard deviation. This leads to an upper and a lower bound for the direct effect of 0.11 to 0.09 percentage point. The difference comes from the change in demand for public care. The estimated effect for private care remains unchanged.

### *Total Effect*

Is the combined effect of the SSN funding and the allocation of health cards, as defined in equation (1), identified if general equilibrium effects compromise the independence assumption? It could be, for example, that the indirect effect of the subsidy decreases if health card allocation is relatively high. Alternatively, there could be districts with a high SSN allocation but with a delay in health card distribution at the time of the survey. Does the variation in SSN budget then adequately capture the total effect, and does this allow clear interpretation of the indirect effect? To investigate, health card coverage was added to the model, as well as an interaction term with the SSN variable. If the budget allocation does not identify the total effect, the results would be expected to be sensitive to the new variables.

Note that health card allocation data are likely to be endogenous. Unlike the SSN budget, these data are not driven by pre-program welfare indicators. The data reflect the actual allocation of health cards, which depend on district-specific infrastructure, organization, and welfare characteristics, and are likely to be correlated with the heterogeneous effects of the crisis. Therefore, the BKKBN indices from December 1997 are used as instruments for health card allocation.<sup>16</sup> The results are given in the bottom panel of table 6 and suggest that the original estimates are fairly robust and capture the combined effect of the program. When health card coverage or the interaction term is added to the regression, the coefficients for the SSN grants are slightly larger and a little less precise.

## VI. CONCLUSION

This article presented an impact evaluation of the health card program as it operated under the SSN program in its first months. It found that in many ways the program was a success. It is also the case that the program may have worked in ways that were not the objective at the outset. The health card

16. The indices for the two poorest BKKBN classifications are used (pre-prosperous and KS1). Households ranked in one of these groups are eligible for a health card. The instruments are not correlated with the pre-crisis trend. An overidentifying restrictions test further validates the instruments. Detailed results are available in the supplemental appendix (tables S.16 and S.17).

program has a weak link between the delivery of services to health card owners and the financial compensation of health care providers. Service providers are reimbursed using a lump sum transfer based on the number of health cards distributed to their area of influence. As a result, serving a health card owner did not result in a direct financial reward to the service provider. This makes the health card program a rather particular case of a targeted price subsidy scheme.

There is clear evidence that the health card program was propoor. The poor had a higher probability of receiving a health card, and those with a health card had increased use of health services, presumably making them healthier. However, there was considerable leakage of benefits to the richer quintiles, and use of services is less propoor than is ownership. Conditional on ownership, the rich have a higher propensity to use their health card.

Returning to the questions posed initially, for all households health card ownership was found to result in a large substitution effect away from private providers to public providers, with a net increase in the overall use of outpatient medical services. A dynamic analysis further indicates that the combined SSN program resulted in an increase in the outpatient contact rate at modern health care providers of 0.55 percentage point. Without the program use of outpatient facilities would have fallen further in 1999. However, the direct health card effect contributed only about 20 percent to the increased use. A considerable proportion of the impact of the program seems to have been through the budgetary support for public health services. If this is true, the revival of public health services can be attributed in large part to the supply impulse induced by the increased spending under the SSN health program.

However, the effects of both the health card and the supply impulse show a strong heterogeneous pattern across subgroups of the population. While the targeting and impact of the health cards were propoor, the total effect was not. The poor responded to a price subsidy but not to the supply impulse. The health card increased use and led to a substitution effect from private to subsidized public care. For the nonpoor, however, use seems to be mainly supply driven, as the health card affected only their choice of health care provider without increasing use.

These results suggest that in the absence of clear incentive mechanisms for health care providers, general increases in public spending are relatively ineffective in reaching the poor. A stronger link between provision of services to health card owners and budget support would likely have improved targeting to the poor. Health card distribution was propoor, and use of modern care among health card owners increased. A stronger link could have been established, for example, by tying compensation for providers to services delivered to health card owners, on a fee for service or capitation basis. An alternative would be to establish contracts with providers, with budgets dependent on monitorable target indicators in the communities they serve. The empirical evidence worldwide on alternative mechanisms to stimulate demand for health services among the poor is still scarce and merits further research.

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