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The Impact of Price Subsidies on Child Health Care Use:

Evaluation of the Indonesian Healthcard

Aparnaa Somanathan

The World Bank
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Abstract

Financial barriers to seeking care are frequently cited as one of the main causes of underutilization of child health care services. This paper estimates the impact of Indonesia's healthcard on health care use by children.

Evaluation of the healthcard effect is complicated by the fact that card allocation was non-random. The analysis uses propensity score matching to control for systematic differences between treatment and control groups. A second potential source of bias is related to contemporaneous, exogenous influences on health care use unrelated to the healthcard itself. Using panel data collected prior to and after the introduction of the healthcard, a difference-in-differences estimator is constructed to eliminate the effects of exogenous changes over time.

The author finds that although health care use declined for all children during the crisis years of 1997-2000, use of public sector outpatient services declined much less for children with healthcards. The protective effect of the healthcard on public sector use was concentrated among children aged 0-5 years. The healthcard had no significant impact on use of private sector services. The results highlight the need to provide adequate protection against the financial burden of health care costs, particularly during economic crises.

This paper—a product of the Health, Nutrition and Population Unit, Human Development Sector Department of East Asia and Pacific Region—is part of a larger effort in the department to disseminate working papers by staff members on issues related to human development. This paper was written as part of the author's doctoral thesis submitted to Harvard University in June 2006. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at asomanathan@worldbank.org.

THE IMPACT OF PRICE SUBSIDIES ON CHILD HEALTH CARE USE:

EVALUATION OF THE INDONESIAN HEALTHCARD

Aparnaa Somanathan

Email: asomanathan@worldbank.org

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Introduction

Demand-side subsidies are considered an effective means of tackling financial barriers to access and expanding the demand for health services, especially among the poorest and most vulnerable groups in the population [1, 2]. Yet, little empirical evidence exists on how utilization of child health care services responds to price subsidies, particularly in the demand constrained settings that exist in developing countries. An important underlying question is whether reducing financial barriers to care is indeed the magic pill needed to expand the demand for child health services, or whether physical barriers to access and a lack of information and knowledge about services would continue to limit the expansion of demand, despite the availability of price subsidies.

This paper examines the impact of the Indonesian healthcard on health care use by children. The healthcard was the key component of a targeted price subsidy program in the public health sector in Indonesia. It was introduced in a context where, not only was utilization of child health services already low by regional standards, but utilization had been depressed as a consequence of the economic crisis that affected South-East Asia in the 1990s. This study goes beyond a previous evaluation of the Indonesian healthcard by Pradhan et al [3] in several ways. It is concerned with the impact of the healthcard on children's use of services, whereas the earlier study examined outpatient care use for the population as a whole, making no distinction between adults and children. In addition to evaluating outpatient care utilization, this study looks at the effects of the healthcard on self-medication among children which has not been considered previously. Finally, Pradhan et al's analysis reflects the experiences of the first five months of the healthcard program while data used for this paper reflect a longer time interval of 2-3 years. Healthcard ownership nearly doubled after the first year of program implementation, making it quite critical to evaluate its effects beyond the initial time period.

I find that, although health care use declined for all children during the crisis years of 1997-2000, the use of public sector outpatient services declined less for children with healthcards. The protective effect of the healthcard on public sector use was concentrated among children aged 0-5. These findings are consistent with those of Pradhan et al's results for the general population. Contrary to Pradhan et al, I find that the healthcard had no significant impact on use of private sector services. This suggests that with respect to children at least, the healthcard program was not associated with any substitution from private to public health care. My findings also suggest that there was less recourse to self-medication among healthcard owning children, although these results are not statistically significant.

Evaluation of the healthcard effect is complicated by the fact that the allocation of the healthcards to households was non-random. I use propensity score matching to control for systematic differences between treatment and control groups that could potentially bias the estimation of the program effect. A key advantage of propensity score matching over regression methods is that it does not require a parametric model of health care use involving assumptions about functional forms and error distributions. A second potential source of bias is related to contemporaneous, exogenous influences on health care use unrelated to the healthcard. The availability of panel data collected just prior to the introduction of the healthcard in 1997 and again in 2000 makes it possible to use a difference-in-differences estimator. The combination of propensity score matching with difference-in-differences eliminates selection bias as well as bias caused by time-invariant unobservables from the estimated healthcard effect. This represents another source of deviation from Pradhan et al’s study, which used a post-intervention treatment-control group design. They evaluated the healthcard impact as the difference between utilization outcomes of healthcard owners and observationally similar non-healthcard owners in 1999. I find that failure to control for these sources of bias significantly underestimates the program effect.

I begin by describing health care utilization trends in the aftermath of the economic crisis and provide an overview of the Indonesian healthcard program. This is followed by a review of the literature. I then lay out the hypotheses and analytical framework, explain the empirical strategy and describe the data. Thereafter, I present and discuss the results.

Background

Low levels of use of modern health services in Indonesia are largely attributed to financial barriers to access. The large household out-of-pocket share in overall health financing provides justification for such claims. Households accounted for 60-70% of total health expenditures during 1995-2000 [4]. Out-of-pocket payments are incurred when seeking care from private providers as well as public providers, where official and unofficial user fees are widespread. User fee exemptions exist at public facilities but are rarely effective. Two insurance schemes exist for civil servants and private sector employees but account for less than 5% of total health financing. Recent work on the distribution of household health expenditures has shown that out-of-pocket payments for health care impose a considerable burden on household budgets, especially for low-income groups[5, 6].

The Indonesian economic crisis began in the second half of 1997 when the rupiah depreciated rapidly and plunged the country into economic recession through 1998. Food prices increased by an estimated 80% during the same year. The devaluation of the rupiah increased levels of debt faced by private companies resulting in bankruptcies. This led to a reduction in labor demand, rising unemployment and a corresponding loss of social security coverage[7]. Much of the impact was felt in the manufacturing, construction and finance sectors, which were concentrated in the provinces of Java and Bali. Rural provinces experienced declines in real agricultural wages. Real GDP decreased by roughly 15% in 1998 [8]. Poverty rates, estimated at 11.3% prior to the crisis rose to around 18 to 20%.

The severity of the crisis inevitably affected households' health care utilization and expenditures. Frankenberg et al. [9] found that household consumption fell by 20% in 1998, with investments in human capital (health and education) decreasing by 37%.

Utilization of modern health care services fell sharply during 1997-98 and remained constant during 1998-99. Much of this decline was due to a significant decrease in the use of outpatient services, particularly public sector services as shown in Figure 1 [7, 9, 10]. Trends in child health care use mirrored overall trends, with public sector outpatient care experiencing the greatest decline [11].

Figure 1: Changes in contact rate by type of provider

The price of treatment at Indonesian government health centers, where the bulk of public sector outpatient care is provided, increased by an estimated 67% during the crisis due to higher input prices of drugs [7]. To households, already faced with diminishing purchasing power, this was a significant increase in health care costs. The price increase, combined with supply outages experienced by cash-strapped government facilities, may explain why public sector utilization rates fell in Indonesia.   Yet in neighboring Thailand, public sector use increased during the same period despite virtually identical macroeconomic conditions and comparable increases in costs of inputs and services. Different levels of social protection in the two countries underlie this different performance. In Thailand, insurance coverage was already around 77% when the crisis began and public assistance programs were expanded during the crisis. By contrast, a large majority of Indonesians remained uninsured during the first year of the crisis.

The Indonesian health subsidy program - JPS-BK - examined in this paper, was part of a series of measures introduced by the Indonesian government in 1998 to improve social protection for the population. The Social Safety Net (SSN) programs included workfare, subsidized rice sales, targeted scholarships and subsidized health services. Starting in 1998, the JPS-BK initiative financed a range of reproductive and child health services, supplementary feeding for young children and pregnant mothers, other basic health services, and outpatient and inpatient care for poor patients who were referred to hospitals [12]. Utilization of public sector outpatient care services, which saw the greatest decline immediately after the crisis, rebounded faster after 1998 than other types of services. This improvement in the use of public sector services has been attributed in part to JPS-BK [10, 11].

The Indonesian healthcard

Under JPS-BK, the most vulnerable households in each community were allocated healthcards, which entitled them to the price subsidy. Government health facilities that provided subsidized care received extra budgetary support to compensate for the increased volume of services they provided. The scheme was financed through the central government using both GOI and donor sources of funding.

The size of the subsidy distributed to health care providers and the number of healthcards issued were determined by the estimated number of poor households per district. The poverty measure was constructed by the national family planning board (BKKBN) and was based on the ‘prosperity status’ of the households. According to this definition, households were considered poor if they had insufficient funds to (i) worship according to their faith, (ii) eat basic food twice a day, (iii) have different clothing for school/work/home, (iv) have a floor not made out of earth, and (v) have access to modern medical care for children or access to modern contraceptive methods. This prosperity measure, which was developed in the context of a family planning program designed to promote modern contraception, was not entirely appropriate for administering SSN programm. Given the non-availability of survey data with up-to-date information on household socio-economic status at below the district level, there was no alternative but to use it. The BKKBN collects this information on a census basis at regular intervals [13].

Village and municipality level committees were responsible for allocating the healthcards to the eligible population in each community. Eligibility was determined on the basis of the prosperity measure described above, although local leaders used their discretion in identifying who was poor and vulnerable. Healthcards were usually distributed through local health centers or village midwives. The healthcard entitled the owner and family members to free services at public health care providers for outpatient and inpatient care, contraceptives for women in child bearing age, pre-natal care and assistance at birth [3].

Ownership of the healthcard among Indonesian households grew to 10.6% in 1999 and to 20% in 2000 [14, 15]. The healthcard was not very well targeted, especially initially when less than 5% of the poorest quintile was covered. Pritchett et al. [14] note that as the program went from being a "regular" program to a crisis program, it moved from the typical pattern of middle class capture to being quite pro-poor in the early phase of expansion. However, as the program expanded further it became less targeted once again (Table 1). On average, however, healthcard owning households were more likely to be poor and employed in agriculture, and have a head of household who was less educated and was female [3].

Table 1 about here

Literature review

A large body of literature exists on the impact of prices on the demand for health care. Studies from the developed world, which have examined the impact of insurance on medical care demand have reported price elasticities ranging from -0.2 to as high as -2.1 [16-19]. Studies from developing countries have focused largely on the price response of health service utilization to user fees. One strand of this literature suggests that prices are not important determinants of health care utilization [20-26]. Another strand demonstrates that prices have important negative effects on utilization [27-31]. Demand studies have also noted that the price elasticity of demand is greater for poorer households than richer households [32, 33]; Sahn et al. 2003), and that price effects are positive when they are accompanied by quality improvements [34]. Of these studies, Ching [32], Hallman [23] and Levin et al [25] examined the price responsiveness of demand specifically for child health services, with varying results as to the impact of prices. In his estimation of demand for health care in Indonesia, Deolalikar [35] corrected for methodological problems that may have caused the mixed results with regard to price effects in the earlier literature. He found that the demand for both traditional and modern health services in Indonesia was highly responsive to price, when it was not conditioned on reporting sick.

As Pradhan et al [3] point out, much of the existing literature draws on simulation-based health care demand models, which make ex-ante predictions of possible price scenarios given other estimated parameters. A drawback of simulation-based models is that the underlying estimates reflect the effects of marginal changes in prices, while the sensitivity of health care demand to price changes may be different when it involves discrete jumps, as in the case of a fee waiver or price subsidy. The simulations rely on out of sample predictions, where the forecasted interventions lie outside the range of observed price data (Gertler and Hammer 1997 as quoted in Pradhan et al).

Ex post studies, which examine the impact of price-related policy interventions avoid having to rely on out-of-sample predictions to measure the impact on health care use and outcomes. However, a key challenge for these studies is the need to control for selection bias inherent to many public interventions such as health insurance and fee waivers. It is well-established in the literature that individuals do not always take up the public assistance or benefits that they are eligible [36]. Individuals who are poorer and less well informed may be less likely to enroll in public assistance programs, while sicker individuals may be more likely to enroll. To the extent that these variables also influence utilization rates and outcomes, failure to control for selection effects would produce a biased estimate of the program effect. Currie and Gruber exploit variations in the extent to which Medicaid eligibility was increased in different states to identify the impact of Medicaid expansions. They find a significant positive impact on child health utilization and mortality.

In the developing country literature, only a limited amount of ex-post evaluations of health care utilization has controlled for selection bias. Yip and Berman [37] analyzed the impact of a school-based insurance scheme in Egypt on child health care use, treating participation as selection on observables. They found that school health insurance significantly increased visit rates among children and reduced differentials in utilization between low and high income socioeconomic groups. Wagstaff and Pradhan examined the impact of the introduction of Vietnam's health insurance (VHI) program using a combination of propensity scores and panel data to control for selection effects[38]. They found that among young children, VHI increased use of primary care facilities. It led to substitution away from the use of pharmacists as a source of non-prescribed medicine towards the use of pharmacists as a supplier of medicines prescribed by a health professional. Pradhan et al examined the impact of the Indonesian healthcard on outpatient care utilization, also using propensity score matching to control for non-random healthcard allocation. A key finding of their work is that the price subsidy resulted in a higher level of utilization of outpatient services among poor beneficiaries, and a substitution from private to public providers for non-poor beneficiaries.

Thus, much of the literature on the price responsiveness of health care utilization has focused on the sensitivity of demand to marginal changes in prices or user fees. Existing studies on the impact of health insurance or price subsidies on health care use suggest that child health care use generally increases in response to reductions in price. The magnitude of the price effect and the reliability of the estimate remain open to question because of a range of other factors that can be argued to have limited or strengthened the price effect. Contemporaneous changes in economic conditions and physical and information barriers to access are examples of such variables. Both are relevant in the context of the healthcard in Indonesia. Improvements in macroeconomic conditions during the second year, after the introduction of the healthcard, may be argued to have had a positive effect on utilization independently of the healthcard. On the other hand, the impact of the healthcard may have been restrained due to supply side barriers such as inadequate levels of staff and supplies, or a lack of knowledge about how to access health services. This paper attempts to control for the first set of potential confounders in estimating the impact of the healthcard.

4 Hypotheses

The following hypotheses are tested with regard to changes in the use of health services between 1997 and 2000, the period before and after the introduction of the healthcard.

(1) Following the economic crisis, the probability of contacting a public provider for inpatient or outpatient care declined less for children in healthcard owning

households than for others, so that the net impact of the healthcard on use of government health services was positive.

(2) The contact rate and number of visits for public sector outpatient care declined less for children in healthcard owning households than for others, so that the net impact of the healthcard on use of public sector outpatient care services was positive.

(3) The impact of the healthcard on private sector outpatient care contacts was negative, as healthcard households increased their use of government services.

(4) Children in healthcard owning households were less likely than others to self-treat in 2000 relative to 1997, so that the net impact of the healthcard on self-treatment was negative.

(5) The poorest recipients of the healthcard and healthcard owners in rural areas were less likely to experience large gains in use of government health services relative to more affluent recipients and those in urban areas.

5 Analytical framework

This section sets out the estimation problem, discusses potential sources of bias and proposes ways of controlling for them.

The estimation problem

The impact of the healthcard for a given individual, i on utilization, Y is the difference between the value of Y after the individual was exposed to the healthcard and the value of

Y that would have been observed, had the individual not been exposed to the healthcard. More formally, the mean impact (β) on healthcard owners is:

$$\beta = E(Y^{1}_{i,t+p} - Y^{0}_{i,t-r} \mid h_i = 1) - E(Y^{0}_{i,t+p} - Y^{0}_{i,t-r} \mid h_i = 1) \tag{1}$$

where

Y^0_{it} = the level of utilization when <u>not</u> exposed to the healthcard

Y^1_{it} = the level of utilization when exposed to the healthcard

h_i = 1 if the individual is allocated a healthcard; 0 otherwise

t = time of the intervention (healthcard introduced in 1998)

p = number of years after the intervention, when the outcome is measured

r = number of years before the intervention, when the outcome is measured

There are two potential sources of bias in estimating β: (a) the non-random nature of the process used to allocate healthcards and (b) the influence of exogenous factors that were present during the time of the intervention that may have had an impact on utilization independently of the intervention.

Non-random healthcard allocation

The Indonesian healthcard was not distributed randomly within communities. Allocation was based on a range of socioeconomic and demographic attributes of the household. Many of these attributes also influence health care use. A direct comparison of health care use by children with healthcards and those without would produce a biased estimate of the program effect. It cannot be assumed that, absent exposure to the healthcard the outcome would have been the same for both healthcard and non-healthcard owners. In short, the following cannot be assumed:

$$[E(Y^0{}_{i,t+p} \mid h_i = 1) - E(Y^0{}_{i,t+p} \mid h_i = 0)] = 0 \tag{2}$$

It is reasonable to expect that had there been no healthcard program, healthcard owners who are poorer and less educated would have used less health care, relative to non-healthcard owners. To obtain unbiased estimates of the healthcard effect, it is, therefore, important to adequately control for systematic differences between healthcard and non-healthcard owners, so that the two groups are comparable in terms of their pre-intervention characteristics.

This paper uses propensity score matching (PSM) methods to address the problem of non-random healthcard allocation. PSM relies on matching each healthcard owner with a non-healthcard owner with the same observed characteristics in order to reduce the differences between the treatment and control groups. Critical to this method is the assumption of conditional independence, which implies that, conditional on a set of observed characteristics, allocation of the card can be treated as random [39]. The more variables used to match households, the more likely it is that systematic differences between treatment and control groups will be reduced. Rosenbaum and Rubin [40] have shown that if it is valid to match on all of the selected variables separately, it is equally valid to match on a score estimated from those variables called the propensity score. The probability that an individual is allocated a healthcard is estimated as a function of all relevant pre-intervention socioeconomic and demographic characteristics as follows:

$$P(h_i = 1) = \Phi(X_i \gamma) \tag{3}$$

The predicted value $H^*{}_i = \Phi(Xi\gamma*)$ from equation (3), referred to as the propensity score can be interpreted as the probability an individual *i* is allocated a healthcard.

An appropriate comparison group is created by matching households that own a healthcard to households without a healthcard, based on the propensity score. The treatment and control groups will yield unbiased estimates of the healthcard impact if the following assumption holds:

$$[E(Y^0{}_{i,t+p} \mid H^*{}_i, h_i = 1) - E(Y^0{}_{i,t+p} \mid H^*{}_i, h_i = 0)] = 0 \tag{4}$$

That is, conditional on the propensity score, treatment and control groups would have had the same outcomes in the absence of the healthcard. The extent to which propensity score matching reduces the bias in estimation depends on the specification of the propensity score model and the quality of the control variables [41]. It is thus critical to include sufficient information about the allocation procedure in the estimation of the propensity score.

An alternative method is to run a regression of the health care use variable on dummy variables, indicating ownership of the healthcard. Observable covariates enter in the regression as linear controls. In the health economics literature, program participation of this nature is generally regarded as endogenous, leading to the use of instrumental variable (IV) estimators within a regression framework. Typically, a subset of variables is found that is highly correlated with program allocation, but does not have a direct impact on the likelihood of using health services. The set of variables forms the IV and is used to predict program participation. An alternative IV technique is the Heckman "two-stage" estimator. In the first stage, the probability of participation is estimated. In the second stage, the first stage results are used to statistically adjust the disturbance term in the outcome regression so that the impact estimate will be unbiased. Typically, only one variable that both predicts participation and is uncorrelated with the outcome is needed to construct a good instrument.

Regression methods require the same, un-testable assumptions about conditional independence, which underpin PSM methods. In IV regressions, this assumption is implicit in the exclusion restriction that the IV is independent of the outcomes, given participation in the program [42]. The IV estimator is consistent, provided the independence assumption holds and the IV is highly correlated with program participation. While there are many good examples of IV based identification strategies from the developed world [43], evaluation work in the developing world has generally failed to produce convincing IV's that satisfy these assumptions. It is particularly difficult to find a suitable IV in the context of the evaluation of the healthcard because almost all of the criteria used to assess healthcard eligibility (low income, inadequate access to health care) also affect health care utilization.

An advantage of PSM over regression methods is that the former does not require a parametric model linking program allocation to outcomes. It thus allows estimation of mean impacts (including impacts conditional on income or area of residence) without arbitrary assumptions about functional forms and error distributions [42]. Both OLS and IV regression methods impose functional form assumptions about the treatment effects and control variables.

PSM allows for the inclusion of a wider range of control variables than regression methods. Jalan and Ravallion [42] note that, in the regression context, there is a bias towards control variables that are exogenous predictors of the outcome variable. Rubin and Thomas' [44] analysis and simulations showed that variables with weak predictive ability for outcomes can still help reduce bias in estimating causal effects using PSM. In the evaluation of the healthcard for instance, variables such as the number of rooms in the house which is a poor predictor of health care use, or health care use by other family members which may be endogenous, would be excluded from the regression analysis.

They are, however, instrumental in reducing systematic differences between treatment and control groups and, when included in the PSM model, contribute to reducing bias in the estimated impact of the healthcard.

A further advantage of PSM over regression methods is to do with the sample used for the analysis. Regression methods use the full sample of households or individuals for the analysis. Estimation based on PSM is restricted to the matched group of treatment and controls only; unmatched comparison households are dropped. Rubin and Thomas [44] have shown that impact estimates based on full (unmatched) samples are generally more biased, and less robust to mis-specification of the regression function, than those based on matched samples.

Exogenous influences on utilization

Estimation of the healthcard effect may be biased, even after propensity score matching, if exogenous changes in macroeconomic conditions taking place over the same time period as the intervention independently affect outcomes and confound the impact of the healthcard. For instance, utilization of modern health care providers fell in the immediate aftermath of the crisis in 1998-99 but picked up again in 1999-2000 as shown in Figure 1; this rebound in utilization could be due to the healthcard as well as to improvements in living standards during 1999-2000. Failure to control for such changes would result in erroneously attributing all improvement in utilization to the healthcard.

I propose to use a difference-in-difference estimator to eliminate time-invariant exogenous influences on the outcome that may confound the healthcard effect. This involves comparing changes between 1997 and 2000 (pre and post intervention) in outcomes of healthcard owners (ΔY_i^H) with those of matched non-healthcard owners (ΔY_i^M). Using the notation developed above, changes in outcomes for matched intervention and control groups may be defined as:

$$\Delta Y_i^H \equiv E(Y^1{}_{i,t+p} - Y^0{}_{i,t-r} \mid H^*{}_i, h_i = 1) \tag{5}$$

$$\Delta Y_i^M \equiv E(Y^0{}_{i,t+p} - Y^0{}_{i,t-r} \mid H^*{}_i, h_i = 0) \tag{6}$$

Following Wagstaff and Pradhan [38], the change in outcomes pre and post intervention can be decomposed as follows:

$$\Delta Y_{it}{}^H = \Delta Y_{it}{}^* + \Delta HC_{it} + \Delta\theta_t + \Delta\varepsilon_{it}, \tag{7}$$

where Δ denotes the change between 1997 and 2000, Y^*_{it} is the counterfactual outcome for healthcard owners, HC_{it} is the impact of the healthcard program, θ_t is the unobserved effect specific to time t, and ε_{it} is a white noise measurement error term associated with the outcome variable. The data are observed for N individuals in households owning healthcards. For N matched control individuals, change in outcomes may be written:

$$\Delta Y_{it}^{\ M} = \Delta Y_{it}^{\ *} + \Delta\theta_t + \Delta\mu_{it}, \tag{8}$$

where μ_{it} is the white noise error term. Difference-in-differences in outcomes are obtained by subtracting (8) from (7):

$$\Delta Y_{it}^{\ H} - \Delta Y_{it}^{\ M} = \Delta HC_{it} + \Delta\varepsilon_{it} - \Delta\mu_{it}. \tag{9}$$

As the program effect is only relevant for the post-intervention period, $\Delta HC_{it} = HC_{i,2000}$ and Equation (9) may be re-written as:

$$\Delta Y_{it}^{\ H} - \Delta Y_{it}^{\ M} = HC_{i2000} + \Delta\varepsilon_{it} - \Delta\mu_{it}. \tag{10}$$

Taking expectations of Equation (10) results in:

$$E[\Delta Y_{it}^{\ H} - \Delta Y_{it}^{\ M}] = E[HC_{i,2000}], \tag{11}$$

assuming ε and μ distributed with zero mean.

Differencing out the change in outcomes experienced by the control group purges the treatment effect of any exogenous changes in economic conditions that took place during the same time interval. The above formulation allows for a common unobserved growth component in the outcome growth equations. Under this set of assumptions the only driver of differential change between the matched intervention and control households is ownership of the healthcard itself. This would not be the case if the unobserved time trends are not independent of healthcard ownership, as it is assumed above. For instance, if changes in living standards caused healthcard owners' expectations with respect to health care to increase at a faster rate relative to the control group, the time trends would be different. Under this framework, changes in outcomes that are attributable to different unobserved time trends would be erroneously attributed to the healthcard.

Examining pre and post observations for intervention and control groups has one further advantage. The assumption in (4) is most likely to hold when selection bias is largely a result of observable differences, which can be controlled for using propensity scores. Systematic unobservable differences between intervention and control groups, if present, could still bias the impact estimate. To the extent that unobservable differences are time invariant, using multiple observations for each participant will difference out the unobservable factors, thus controlling at least partially for this source of bias.

The average treatment effect of the healthcard on healthcard owners is estimated by comparing the change in utilization for the intervention (ΔY_{it}^{H}) and control groups (ΔY_{it}^{M}). The advantage of this estimation approach is that it is not necessary to specify a model for the outcome variable. It is necessary, however, to specify a model for the allocation of the healthcard itself, which is described in the next section.

6 Empirical strategy

Data

This paper is based on data from the Indonesian Family Life Survey (IFLS), an ongoing, longitudinal survey of individuals, households and communities. Fieldwork for the second wave of the survey was completed by December 1997, nine months prior to the introduction of the healthcard program. The third wave of the survey was fielded in 2000. The analysis presented here, therefore, reflects the experience of at least fourteen months of the program. The 1997 survey (IFLS-2) covered 7,619 households, which included 10,429 children. In the 2000 round (IFLS-3), a total of 10,435 households consisting of 11,307 children were interviewed. 7,704 children were interviewed in both IFLS-2 and IFLS-3. The analysis in this paper is restricted to panel respondents aged 0 to 15 years old. Table 2 provides information on the number of children in each survey and the number of panel children. Attrition is limited in IFLS surveys as a lot of attention is paid to ensuring high re-contact rates. Households that moved between one wave and the next are followed up even when they move to a different province. Of the original IFLS-1 households, around 95% were re-interviewed in IFLS-2 and IFLS-3.

Table 2 about here

The household survey contains a wide range of information regarding household members' incomes and assets, education and consumption, which are vital for matching healthcard owners with suitable control households. Information about the quality of living conditions and housing were also collected. A separate section of the survey asked household members about their knowledge of and distance to different types of service providers. A new question added in the 2000 survey asked about healthcard ownership, the treatment variable for this study.

The Child Information (CI) book was administered to the parent or guardian of all children aged 15 or less. Areas covered by modules in the CI book include self-treatment, outpatient and inpatient treatment for children and vaccination histories. Questions about health care use for children were not conditioned on the child reporting illness. Data were also collected on the type of provider sought, frequency of visits and in most instances, the cost of each visit. The instrument used in all three waves followed a similar structure and repeated the same questions, in order to allow comparison across different waves.

In addition, I obtained data on BKKBN sub-district poverty rates, which are used by the government to distribute healthcards across districts and sub-districts.

Specification of the propensity score function

The first step is to estimate the propensity scores which will be used as a basis for matching each child in a healthcard household with a child in a household without the healthcard. The propensity score function in Equation (3) above was specified with a binary outcome variable indicating healthcard ownership. Its purpose is to model the likelihood of an individual belonging to a household that was allocated a healthcard on the basis of observable characteristics of the individual, household and community.

Understanding the design of the healthcard program and ensuring that the propensity score specification reflects all aspects of the card allocation procedure are vital for reducing the bias in estimation. The following aspects of program design are pertinent in this respect:

- *Criteria for allocating subsidies*: the amount of health subsidy and the number of healthcards allocated by the central government to districts and then to sub-districts were based on the pre-intervention (1999) poverty rate in each district or sub-district.
- *Criteria for allocating healthcards to households*: at the sub-district level, village committees distributed healthcards to households on the basis of the prosperity measure described above, additional criteria they received from the government and their own village-specific information. Special attention was paid to households that were severely affected by the crisis. Although village committees were expected to identify eligible households based on information available to them, in practice they faced significant informational barriers. The information and criteria used to identify eligible households were not well defined. This meant that households that were better able to signal their socio-economic situation and needs to the village committees faced a higher probability of receiving a card.
- *Healthcard distribution mechanism*: as healthcards were distributed through village midwives and health centers, households' familiarity with and proximity to health providers would have influenced card ownership.

The propensity score function, therefore, includes a range of household and community level variables that would have influenced the allocation criteria above. Pre-intervention characteristics as reported in the IFLS 1997 survey are used throughout. Use of household and community level information reported in IFLS 2000, the post intervention period may cause endogeneity in the model. For instance, households' familiarity with health care providers in 2000 may be a consequence of owning a healthcard. Similarly, household consumption reported in 2000 may be higher for households with healthcards than comparable households without cards, because of the latter's reduced spending on health. Estimating the propensity score on the basis of pre-intervention attributes alone ensures that there is no endogeneity in the analysis.

For households, a wide range of variables were included that would not only indicate households' eligibility for a healthcard to village committees but would also capture the households' propensity to signal its needs to the committee. Socio-demographic characteristics of the head of household, such as sex, education level and employment status were included because they would indicate the level of social and economic deprivation faced by the household and influence what actions it took to obtain a healthcard. Households headed by females or by individuals who worked in sectors such as manufacturing and construction that were particularly affected by the crisis would have been regarded as more vulnerable. On other hand, heads of households with primary or a higher level of schooling would have placed greater value on access to health care, and consequently, made a conscious effort to obtain a healthcard. Household size and composition were also controlled for, because larger households with more children would have been considered more vulnerable.

Another set of household level covariates corresponded to housing characteristics, such as materials the house floor and walls were made of, sources of drinking water and types of toilet facilities available. Easily observed, tangible measures of deprivation such as these would have played a significant role in the village committees' evaluation of relative poverty, given the absence of more reliable measures of socioeconomic status at the community level. Other variables included to capture household socioeconomic status were ownership of selected assets and annual household consumption per capita. Also included was a binary variable indicating whether a household purchased food or other goods at subsidized prices. Being registered for social assistance provided through existing poverty alleviation programs would have certainly increased a household's propensity to receive a healthcard.

Households' familiarity with health care providers in the area was captured using a set of binary variables indicating whether or not the head of household knew where the local hospital, health centre and midwife were located. A second set of variables was included, reflecting children's use of health services in the pre-intervention period. It controlled for the household's pre-intervention level of familiarity with health care providers, which would have significantly improved their chances of being selected for a healthcard. It partially ensured that children with similar pre-intervention levels of utilization would be matched together. A child in the treatment group, who had experienced a fairly high level of use of health services in 1997 for instance, is not likely to be paired with a child who had had no utilization in 1997.

The propensity score model includes a dummy variable indicating households' ownership of the Kartu Sehat (healthcard), the precursor to the post-crisis healthcard program. The Kartu Sehat was given to poor families and entitled them to obtain free services from public providers (midwife, health centre or hospital). Initiated in 1994, the card had not yet been widely implemented by 1997, and was not very successful. It is, however, a relevant explanatory variable in the propensity score model because it was distributed through the village head, who was also charged with distributing the post-crisis cards. To the extent village heads used the same set of criteria to evaluate economic deprivation in their villages, ownership of the Kartu Sehat in the pre-intervention period would be a strong predictor of healthcard ownership during the economic crisis.

Healthcard allocation at the sub-district level was controlled for by including the sub-district poverty rate in 1999 when the program started, as well as a dummy indicating whether the area was urban or rural. The former captured both the allocation of government subsidies within districts, as well as the number of healthcards allocated.

As Pradhan et al. [3] noted, there may be unobserved characteristics which vary by region, which influence the effect that other variables have on the probability of receiving a healthcard. For instance, it is likely that the distribution of the healthcard was quite different in the more urbanized and densely populated Java and Bali provinces, compared to the Eastern Islands or Sulawesi province. Two region dummies were included to control for this. Within each region, province dummies were included to control for unobserved variation in how the subsidies were allocated from the central government to the next tier of government. District dummies would have been more appropriate, given that the health subsidy program was largely administered through district and sub-district governments. They were not included, however, because the number of treated observations within each district was not large enough to allow for sufficient variation.

Only one of the five BKKBN criteria officially used to allocate healthcards -“have a floor made out of earth” - was measured in IFLS and, therefore, included in the model. Absence of the other four variables from the model does not represent a critical gap in the specification of the propensity score for two reasons. The range of household level predictors included in the model are rich and varied enough to capture the extent of social and economic deprivation that would otherwise have been reflected in the BKKBN indicators. Secondly, it is fairly clear that village leaders did not rely exclusively on the BKKBN indicators to identify eligible households, using instead the criteria they received from their district and sub-district governments, as well as their own discretion and information. I am confident that the household and community level variables included in the model, along with the province and region dummies, are sufficient to capture the various factors used to allocate healthcards, within limits set by the data available for this paper.

Estimation of propensity scores

The propensity score was estimated using a probit model. When the explanatory variables are not balanced between treatment and control groups, the balancing property fails and the model is mis-specified. I tested the necessary conditions for the balancing property using an algorithm developed by Becker and Ichino [45] and re-calibrated the model to ensure the balancing property was satisfied. With the exception of some of the province dummies, the explanatory variables were balanced between treatment and control groups in the final specification for each of the two regions.

Creation of matching control groups

The propensity score is a continuous variable. No two observations in the sample are likely to have the same propensity score. Several matching methods exist for creating a control group with similar propensity scores to the treatment group [46, 47]. Rubin's [48] nearest neighbor matching involves choosing the most similar household based on the propensity score for each household in the treatment group. The potential control group is re-weighted so that households that are not matched receive a weight of zero, those that are matched receive a weight of one, and those matched more than once receive a weight greater than one. Inevitably, some of the matches may be quite poor because for some treated units, the nearest neighbor may have a very different propensity score; s/he would still contribute to the estimation of the treatment effect independently of the difference. Using the alternative radius matching method, each treated unit is matched only with control units whose propensity score falls within a predefined neighborhood of the treated unit [45]. The smaller the size of the radius, the higher the quality of the match but the higher the probability of some treated units not being matched because the specified neighborhood does not contain any control units. I have chosen to use nearest neighbor matching with replacement for this analysis after having experimented with both the nearest neighbor and radius matching methods.

In addition, the common support restriction was imposed in order to ensure high quality matches. The estimator was calculated only where the propensity score overlapped for treatment and control groups. The drawback of imposing this restriction was that high quality matches were lost at the boundaries of the common support and the sample was reduced as a consequence.

Estimation of average treatment effect

Average treatment effect on the treated (ATT) was estimated for changes in health care utilization, ΔY_{it} between 1997 and 2000. The following utilization outcomes were examined.

Child received any public sector care (admissions or outpatient visits) during past 12 months.

Child received any outpatient care during the past 4 weeks.

Child received public sector outpatient care from a hospital, health centre or sub-health centre during the past 4 weeks.

Child received private sector outpatient care from a hospital or doctor during past 4 weeks.

Number of public sector outpatient visits for child during past 4 weeks.

Number of private sector outpatient visits for child during past 4 weeks.

Any modern medicines purchased from pharmacy or shop for child during past 4 weeks.

Estimation of ATT was carried out for the entire sample and separately for the following sub-groups:

age-groups: all children were grouped into three groups based on age in 1997 - 0-5 years, 6-10 years or 11-15 years.

consumption-based groups: all children were grouped into three groups based on household consumption per capita in 2000.

region and type of community: based on area of residence in 2000, all children were grouped into two regions (Java and Bali provinces, Eastern Islands and Sumatera province) and within each region, into urban or rural depending on the area which they lived in.

Table 3 provides unconditional means of change (i.e. prior to matching using propensity scores) in medical care use between 1997 and 2000 for children in households with and without healthcards. Utilization of public and private health care services fell for all children during the period, regardless of whether or not they owned a healthcard. The fall in utilization was, however, larger and more likely to be statistically significant for children in households without healthcards. The use of over-the-counter (OTC) medicines increased for children without healthcards, but decreased for children with healthcards.

Table 3 about here

7 Results

Healthcard allocation

A propensity score function was estimated first. The full propensity score models are provided in Annex Table A1.

Household socio-economic status is an important predictor of healthcard ownership. Physical characteristics of the house in which the child lives prove to be as important as more conventional measures of socio-economic status. Having good quality floors, water supply inside the house and own toilets was associated with a lower probability of healthcard ownership, while dirt floors and poor quality walls had a positive effect. Higher rent or house values and the number of rooms also had a negative impact. It is quite likely that local authorities considered the physical features of each family's living environment when evaluating social and economic deprivation in villages. The low levels of statistical significance may be due to multicollinearity between the variables. Ownership of assets such as fridges and gas or electric stoves had a negative effect and was statistically significant. Household consumption also had a negative impact. It is worth noting that interpretation of the consumption and asset coefficients is complicated by the fact that the former may reflect expenditures on the latter. Finally, the coefficient on the dummy for purchasing goods at subsidized prices is strongly significant in both models, confirming that households which participated in existed poverty alleviation programs stood a greater chance of being selected for a healthcard.

Other important household level predictors include education of the head of household and the proportion of children in the household. A head of household with primary schooling was more likely than one without any education to secure a healthcard for his or her family. Households with a large number of children stood a greater chance of getting a healthcard, which is consistent with targeting rules used to allocate the cards. Head of household's employment is not statistically significant, but it is notable that the coefficients for manufacturing, finance and retail are positive, while the coefficient for agriculture and mining are negative. Not an unexpected finding given that the former were the worst affected sectors of employment during the crisis. As it was pointed out earlier, village committees were instructed to target households that were severely affected by the crisis, which would, no doubt, have included households involved in these three sectors.

Prior access to and familiarity with the local health system helped households gain healthcards. Ownership of the *kartu sehat* (pre-crisis healthcard) in 1997 had a quantitatively important, and statistically significant, impact on the probability of receiving a healthcard after the economic crisis. Given that both healthcards were distributed through the same network of health centers and midwives, it is not surprising that ownership of one enhanced the household's chances of getting the other. Similarly, households that knew where the nearest hospital, health centre and midwife were located in the 1997 survey were more likely to receive healthcards: coefficients for all three variables pertaining to knowledge about health facilities in the area are positive and statistically significant.

The urban dummy is positive and statistically significant in the propensity score model. Urban areas were worse affected by the economic crisis than rural areas because the sectors of employment that were directly affected were more likely to be in urban areas. To the extent that the healthcard program was introduced as a social safety net to help households cope with the crisis, a higher probability of healthcard ownership among worse affected urban households is not surprising. On the other hand, it may be a consequence of the increase in the number of health centers and midwives - the main vehicle for distributing healthcards – being more accessible in urban or semi-urban areas.

The sub-district poverty rate coefficient is positive in both models, indicating that households living in sub-districts with higher poverty rates were more likely to receive healthcards.

Province dummies were jointly significant in both models. The pseudo R-squared was 0.0889 for the model as a whole.

Table 4 provides a description of the estimated propensity scores for the two regions. The mean propensity score is 0.30, with little variability between treatment and control groups. The propensity score model was calibrated so that the balancing property was satisfied at significance level of $p < 0.005$ but without loss of the key individual, household and community level explanatory factors described above. The region of common support between treated and control groups is relatively high, covering 90.7% of all children. Children with propensity scores outside the common support were not considered for the analysis.

Table 4 about here

The quality of the match is best illustrated by the distributions of the propensity scores for treatment and control groups, before and after matching (Figure 2-3). As Figure 2-3 shows, the distributions are almost identical for treatment and control groups after matching on propensity scores. There are few matches at high values of the propensity score, or high probabilities of being allocated a healthcard. Examining average values of observed variables for matched treatment and control groups provides further evidence of the degree of comparability between the two groups (Table 5). The first two columns show descriptive statistics for healthcard owners and all others, while the third and fourth columns present the same information for the matched pairs. Differences in covariate means are considerably smaller for the matched pairs. As the last two columns show, the differences in means are not significantly different from zero in most cases.

Figure 2: Histograms of propensity scores for healthcard owners and others before matching

Figure 3: Histograms of propensity scores for treatment and control groups after matching

Table 5 about here

To summarize, the matched groups are generally well balanced across observed characteristics. The results of the propensity score estimations suggest a high degree of overlap between treatment and control groups, which should eliminate most of the bias due to the non-random allocation of the healthcards and enable reliable estimation of average treatment effects.

Impact on health care use

Table 6 reports both the average treatment effect on the treated (ATT) estimated by comparing healthcard owners and their matched controls, and the unmatched difference-in-difference effect obtained by comparing healthcard owners and all potential controls.

Use of public sector services

Household survey data indicate that public sector utilization rates declined substantially for children during 1997-2000 [11, 49]. The healthcard, which was introduced during this period, entitled household members to free outpatient and inpatient care services at public facilities. I tested the hypothesis that the net impact of the healthcard on the change in public sector use rates was positive from 1997 to 2000. I find that the healthcard resulted in a net increase of 3.6% in public sector utilization for healthcard children relative to others. The difference-in-difference estimates for the unmatched groups are not statistically significant and smaller in magnitude than estimates of ATT.

Results presented earlier in Table 3 point to a significant decline in public sector use rates for children between 1997 and 2000, a finding consistent with earlier work. The unmatched difference-in-difference results presented in Table 6 suggest that the decline in public sector use rates was less dramatic for children in healthcard owning households compared to others, although the estimates are not significant. ATT estimates obtained by comparing healthcard children with similar children without healthcards confirm that use of government health facilities fell significantly less for the former. In the aftermath of the economic crisis, the healthcard evidently helped maintain public sector utilization rates among those who received it.

Table 6 about here

Use of public sector outpatient care services

Inpatient care utilization rates, historically quite low in Indonesia, remained relatively unchanged through the crisis years while outpatient care fell substantially both in the public and private sectors. I examine impact of the healthcard on changes in outpatient care use between 1997 and 2000. As before, both ATT and the unmatched difference-in-difference results are presented.

Results in Table 6 suggest that the healthcard had a positive and statistically significant effect on the contact rate, or probability of receiving outpatient care from government facilities. The unmatched difference-in-difference estimate for healthcard owners and all others is not significantly different from zero, showing once again that the failure to match healthcard children with a comparable set of controls substantially underestimates program impact.

Estimates of the healthcard's impact on the number of public sector outpatient contacts shown in Table 6 largely mirror those for outpatient contact rates above. The total number of visits declined for healthcard children and others between 1997 and 2000, but less so for the former as indicated by the positive ATT estimates.

The results confirm earlier findings that public sector outpatient use generally fell between 1997 and 2000. They also show that the healthcard "protected" public sector outpatient care use in the post-crisis period. These findings are in line with Pradhan et al's [3] results that the healthcard had a positive and statistically significant effect on public sector outpatient contact rates in Indonesia. It is not possible to directly compare the magnitude of the impacts as the previous study is a one period analysis and ATT results reported for adults and children are combined.

Use of private sector outpatient care services

It was shown earlier that the probability of receiving private outpatient care declined in both regions between 1997 and 2000 (Table 3). In contrast to public sector outpatient care, the magnitude of the decline in contact rates was not statistically different for children with healthcards and a group of observationally identical children without healthcards. As Table 6 shows, estimates of the treatment effect are not significantly different from zero. While the lack of statistical significance means it is not possible to draw any firm conclusions, these results do point to substitution away from private to public outpatient care use among healthcard owners.

The healthcard did not have any significant impact on the number of private outpatient care contacts, which declined for both healthcard children and the control group during the crisis. The hypothesis that the healthcard had a dampening effect on private outpatient care is not confirmed by these results, contrary to Pradhan et al who found a substitution effect between public and private outpatient care.

Purchase of over-the-counter medicines

Households increasingly resorted to self-treatment as prices at health facilities rose sharply following the economic crisis. This trend was reported for children as well as for adults[11, 49]. If the healthcard made public sector care more affordable, it would have reduced households' reliance on medicines purchased at pharmacies and shops. I tested the hypothesis that the healthcard had a negative impact on the use of over-the-counter (OTC) medicines between 1997 and 2000.

The last row in Table 6 shows the change in the probability of consuming OTC medicines during 1997-2000. It was shown in Table 3 that use of OTC medicines increased for children without healthcards but decreased for healthcard owners. After matching based on propensity scores, ATT estimates are consistently negative but not statistically significant for any of the groups examined. The ATT estimates are also smaller in magnitude than the simple difference-in-difference estimates. The results point to some differentials in OTC use between healthcard owning children and observationally similar children without healthcards. Children in households without healthcards were more likely to have experienced a net increase in OTC use between the two years. It is not possible, however, to reject the null hypothesis of no-effect based on these results. The healthcard did not have a significant negative effect on OTC use during the crisis.

Distribution of the healthcard impact by age-group

As discussed above, use of public sector services declined less for children in households with healthcards than for observationally similar children without healthcards. This effect was driven largely by a positive and statistically significant ATT effect of 7.0% on children aged 0-5 years (Table 7). Treatment effects on older children are not significantly different from zero. The finding that the protective effect of the healthcard was limited to very young children is not surprising, given that the program targeted mothers and young children. The absence of any statistically significant effect, particularly for the 10-15 age-group may be due in part to small sample sizes. With less than 10% of all children reporting any contact with public sector providers on average, there was insufficient variation within small sub-groups, each consisting of 100-400 treated and control individuals to reliably estimate treatment effects.

Table 7 about here

Treatment effects by age-group for public sector outpatient care use point to a similar story, as Table 8 shows. The impact of the healthcard on public outpatient contact rates for children aged 0-5 is statistically significant and almost twice the size of the ATT effect on all children combined. Effects on older children are not significantly different from zero. Similarly, the impact on the number of public sector outpatient contacts children had was driven largely by children in the 0-5 age-group. Owning a healthcard had little impact on the number of public sector outpatient care contacts by older children. In short, the protective effect the healthcard had on use of government outpatient care was large and statistically significant for very young children only.

Table 8 about here

No such trends are evident for outpatient care in the private sector (Table 9) or use of OTC medicines (Table 10). It is worth noting that the net impact on private care use by children aged 0-5 years was negative, implying that the fall in utilization was much larger for this age group. This result is not statistically significant. However, when examined in conjunction with the results for public sector outpatient care, it does suggest substitution from private to public sectors for the 0-5 age-group.

Tables 9 and 10 about here

Distribution of the healthcard impact by socio-economic status and geography

The healthcard was a targeted price subsidy program, designed to benefit poor and vulnerable households during the economic crisis. If the poor are more sensitive to changes in prices as the health care demand literature suggests, the healthcard should have had a larger impact on poor children relative to others. This section examines whether the price response varied by household consumption level and urban or rural setting.

Table 11 presents healthcard ATT results by consumption group. None of the estimates of within-group ATT are statistically significant. The estimates suggest, however, that the impact of the healthcard was positive and increased with income for public sector utilization in general. The net impact of the healthcard on overall public sector use was 0.2% for the poorest children, compared to 2.0% for the richest ones. No particular trend was observed for the use of private sector services. The impact on OTC use was negative for all three groups, consistent with patterns described earlier.

Pradhan et al. found that in the first year after the program, healthcard owners in all income groups experienced a higher level of use public outpatient services and lower level of use private outpatient services relative to those without healthcards; for the richest quintile, the two effects cancelled out. They also found that the highest increases relative to the base for public outpatient care use were for the third, fourth and fifth (richest) quintiles. Their results cannot be directly compared with those presented above for reasons already mentioned. Differences in the distributional results are worth highlighting nevertheless. It is clear that the size of the pure healthcard impact on public outpatient care was generally higher for better-off individuals in Indonesia. The distribution of the impact on private outpatient care use is more ambiguous. Pradhan et al's (2003)'s one period analysis showed that private outpatient care use was lower for healthcard owners, particularly poor healthcard owners. In my analysis, there is little evidence of the healthcard effect being concentrated among the rich or the poor. It is worth noting that Pradhan et al estimate the "pure" impact of the healthcard, as well as the overall effect of the program intervention, taking into account the impact of the subsidies received by health facilities. This paper is concerned with the former only.

Table 11 about here.

ATT estimates for children living in rural and urban areas are presented in Table 12. Results for urban areas in Eastern-Sumatera region are not reliable, as they are based on a small sample of 185 treated children and 144 controls. With average outpatient care utilization rates of less than 10%, the number of children who experienced any type of care were too few to produce meaningful estimates. I discuss results for the other three groups for which there were 250-500 observations in each treated and control group.

The impact of the healthcard on use of public sector care generally and public outpatient care was positive, but not significant in rural areas in both regions and urban Java-Bali. As described above, this reflects the protective effect the healthcard had on public sector care use during the crisis. Within Java-Bali, the magnitude of the health care effect was greater in urban areas compared to rural areas. ATT estimates for private outpatient care are negative in urban Java-Bali and rural areas of the Eastern-Sumatera region, implying that private use fell more for children with healthcards than for the control group. The healthcard also had a negative impact on use of OTC medicines for children in the same two regions, although it was not statistically significant.

The findings for urban and rural areas are consistent with Pradhan et al for public sector outpatient care use. They found that the healthcard had a positive impact in both rural and urban areas, with the latter impact relatively larger. The impact on private outpatient care use, however, was negative and significant for rural and urban areas. In my analysis, small sample sizes and a lack of statistical significance mean that it is not possible to draw any firm conclusions about the regional and urban/rural distribution of the healthcard effect.

Table 12 about here

8 Discussion

This paper set out to examine the impact of a targeted price subsidy, the Indonesian healthcard program on child health care use. The availability of panel data collected before and after the introduction of the healthcard made it possible to eliminate any confounding effects associated with improvements to the macroeconomic environment that took place alongside the introduction of the healthcard. Combining propensity score matching with a double-difference estimator meant that the estimation was purged of any bias due to selection on observables as well as on time-invariant unobservables. However, other potential sources of bias remain that could not be controlled for. Time-varying unobservable differences between healthcard owners and their matched controls could bias the estimation of the treatment effect. This would be the case, for instance, if changes in living standards made healthcard owners' expectations with respect to health care utilization to increase at a faster rate, relative to the control group. I acknowledge the existence of this source of bias but argue that, in the absence of experimental data, the study design used here represents the best approach to the problem. The estimation method is also attractive because it avoided having to specify a regression model for the utilization outcomes, which could potentially be subject to incorrect specification of the functional form.

The first objective of this analysis was to determine the impact of the healthcard on children's use of public and private health services and OTC medicines. Preliminary tabulations of the data confirmed existing evidence that utilization of public sector services, particularly outpatient services, declined sharply and that use of OTC medicines increased between 1997 and 2000, mainly as a consequence of the economic crisis. Comparing healthcard owning children with all other children in the sample showed that the reduction in use rates and volumes was larger and statistically significant for the latter group. ATT estimates for matched treatment and control groups confirmed that use of public outpatient services and public sector services generally declined far more for children without healthcards, so that the net impact of the healthcard was positive for children who owned healthcards. In short, the healthcard helped maintain utilization of public sector outpatient care in a context where utilization of all types of care was falling dramatically. This is consistent with Pradhan et al's one period analysis which also showed that healthcard owners experienced higher rates of use of public sector outpatient services than the matched control group.

Analysis of the treatment effects by age-group showed that the protective effect of the healthcard was limited to children aged 0-5 years in 1997. Among this group of children, healthcard owners experienced significantly smaller declines in utilization than the control group. Treatment effects on older children were not significant. The healthcard, therefore, seems to have achieved one of its main goals, which was to ensure that young children continue to receive adequate levels of health care during the crisis.

With regard to private outpatient care use, I find that the healthcard had no significant impact on either the probability of seeing a private outpatient care provider or the number of private outpatient contacts. This result should not be too surprising given that the healthcard only entitled households to public sector services. It is, however, inconsistent with Pradhan et al's results that private sector outpatient care use was lower for healthcard owners relative to the matched control group. I attribute the difference in results to the different study design. Pradhan et al's one period analysis would have picked up one-period differences in utilization patterns. Under my estimation strategy, any changes in utilization over time that were not due to the healthcard may have been differenced out resulting in the null effect described above.

My analysis has also shown that failure to control for the non-random allocation of the healthcard could lead to substantial underestimation of the program impact. Estimates obtained by comparing healthcard children with all other children in the sample were found to be consistently lower than the ATT estimates for matched treatment and control pairs. The change in utilization for the matched control group is essentially the counterfactual change in utilization – that is, the change in utilization that would have been experienced by healthcard owners had they not received the healthcard. The fact that the actual program impact (ATT) was much larger than the simple difference in means implies the following: while healthcard owners experienced a smaller decline in utilization relative to the rest of the sample, their reduction in use is considerably smaller than what it would have been had they not received a healthcard.

The second objective of this analysis was to examine the distribution of the impact of the healthcard. The healthcard was a targeted subsidy program and there is evidence that the allocation of the healthcard was somewhat pro-poor. Conditional on being allocated a healthcard, however, richer children were more likely to benefit from the healthcard. In effect, the distribution of the impact of the healthcard favours the economically privileged. While ATT results by consumption groups are mostly statistically insignificant, the magnitude of the impact is found to be larger for the richest group. ATT results by urban and rural groups are not statistically significant and the sample sizes within each regional group were too small to draw any meaningful conclusions about the geographic impact of the healthcard.

In conclusion, the healthcard played an important role in protecting health care utilization in the aftermath of the economic crisis, particularly among very young children, but its impact was captured by relatively better-off households. Understanding this limitation of the healthcard requires examination of other non-price barriers to access which are significant in Indonesia. Firstly, physical barriers to access and opportunity costs associated with seeking care may have prevented households from obtaining health care, despite owning a healthcard. Indonesia has one of the poorest distributions of health facilities and medical staff in the Asian region, with much of the health service infrastructure concentrated in urban and semi-urban areas. Second, a lack of knowledge and information about healthcard related entitlements may have hindered use, particularly among poor, uneducated households. There is little evidence that those responsible for distributing the healthcard made a conscious effort to inform recipients of what their entitlements were. For instance, in IFLS-3 healthcard owners did not always report using their healthcard when obtaining cards for their children. In some cases, parents report using a healthcard for their child's outpatient visit, but the head of household does not report owning one. Moreover, since there was no direct link between health care use and the subsidy received by providers, providers also had little incentive to encourage use of the healthcard. Third, healthcard owners may have been discouraged by the attitudes of government providers, particularly if they were very poor. An earlier, pre-crisis version of the healthcard (kartu sehat) was characterized by low take-up rates and had little impact on health care use. Kartu sehat owners complained of providers being rude to them and providing poor quality care, which acted as disincentives to use the card [13]. The evaluation of the healthcard carried out in this paper suggests that the program could potentially be used to significantly expand children's use of modern health services. However, to improve the distribution of the healthcard impact, the government would have to address significant non-price barriers to care that exist in Indonesia.

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Table 1: Targeting of the healthcard – coverage and distribution across quintiles (%)

	Q1 coverage	Ratio of coverage ratios		Targeting ratio	Average coverage
		Q3/Q1	Q5/Q1		
December 1998	3.80	1.44	1.52	1.07	5.22
May 1999	15.49	0.65	0.49	0.91	11.23
October 1999	18.20	0.84	0.72	0.95	15.13

Source: (Pritchett, Sumarto, and Suryahadi 2002)
Notes: Q1 – Q5 represent quintiles 1 through to 5; the poorest quintile is Q1.

Table 2: Children in IFLS sample

	IFLS-2 (1997)	IFLS-3 (2000)
0-4 years	3,734	3,717
5-9 years	3,528	3,732
10-14 years	3,167	3,858
Total children	10,429	11,307
Panel respondents (present in IFLS-2 and IFLS-3)	7,704	

Table 3: Utilisation of health services by healthcard ownership, 1997 and 2000

	No healthcard			Healthcard		
	1997	**2000**	**Difference**	**1997**	**2000**	**Difference**
Probability of contacting a public provider						
All	0.085	0.063	-0.022	0.104	0.094	-0.010
			0.005**			0.011
0-5 years	0.107	0.077	-0.030	0.129	0.124	-0.005
			0.008**			0.017
6-15 years	0.067	0.052	-0.015	0.083	0.069	-0.014
			0.006**			0.014
Public outpatient contact rate						
All	0.077	0.059	-0.018	0.095	0.091	-0.004
			0.005**			0.011
0-5 years	0.096	0.071	-0.024	0.122	0.122	0.001
			0.008**			0.017
6-15 years	0.062	0.049	-0.013	0.073	0.064	-0.009
			0.006*			0.013
Private outpatient contact rate						
All	0.058	0.049	-0.009	0.039	0.034	-0.005
			0.004*			0.007
0-5 years	0.073	0.061	-0.012^{+}	0.051	0.038	-0.014
			0.007+			0.010
6-15 years	0.044	0.038	-0.007	0.029	0.031	0.002
			0.005			0.009
Public outpatient visits						
All	0.094	0.072	-0.022	0.120	0.110	-0.009
			0.006**			0.014**
0-5 years	0.121	0.089	-0.032	0.156	0.149	-0.007
			0.011**			0.024**

6-15 years	0.072	0.059	-0.013	0.089	0.078	-0.011
			0.008+			0.018+
Private outpatient visits						
All	0.070	0.058	-0.013	0.050	0.044	-0.006
			0.006*			0.009
0-5 years	0.091	0.074	-0.017	0.066	0.049	-0.017
			0.010+			0.014
6-15 years	0.054	0.044	-0.010	0.037	0.039	0.002
			0.007			0.013
Probability of using OTC medicines						
All	0.439	0.476	0.037	0.494	0.483	-0.011
			0.009**			0.018
0-5 years	0.463	0.497	0.033	0.517	0.498	-0.019
			0.014*			0.027
6-15 years	0.418	0.459	0.041	0.475	0.471	-0.005
			0.013**			0.024

Notes: standard errors in parentheses; + significant at 10%; *significant at 5%; **significant at 1%

Table 4: Estimated propensity scores

Mean propensity score	0.2208
Standard deviation	0.1264
Region of common support	[0.0329, 0.7718]
Significance of balancing property	0.005
Number of blocks	10

Distribution of samples used for analysis		
	Treatment	**Controls**
All children	1,530	1,160
0-5 years	686	512
6-10 years	681	530
11-15 years	163	123
Socioeconomic groups[(a)]		
Poorest	639	477
Middle	538	418
Richest	348	284
Java-Bali region		
Urban	409	316
Rural	580	409
Eastern islands and Sumatera region		
Urban	185	144
Rural	356	289

Notes:
(a) Based on household consumption per capita

Table 5: Descriptive statistics for healthcard owners and matched pairs

	All children		***Matched pairs***		**Difference between matched pairs**	**s.e**
	No health card	**Health card**	**No health card**	**Health card**		
Female head of household	0.100	0.122	0.112	0.122	-0.009	0.014
Education of head of household						
No education	0.126	0.118	0.125	0.118	0.008	0.014
Primary school	0.524	0.646	0.615	0.646	-0.030	0.020
Junior high school	0.120	0.091	0.096	0.091	0.006	0.012
Senior high school	0.230	0.146	0.163	0.146	0.017	0.015
Sector of employment of head of household						
Agriculture or mining sector	0.356	0.377	0.372	0.377	-0.005	0.020
Manufacturing or construction	0.143	0.184	0.165	0.184	-0.019	0.016
Finance / retail	0191	0.190	0.190	0.190	0.000	-0.02
Log of household size	1.765	1.742	1.743	1.742	0.001	0.015
Share of children in household	0.455	0.466	0.461	0.466	-0.005	0.006
Characteristics of the house						
Floor made of marble/tiles/terrazzo	0.357	0.253	0.291	0.253	0.038	0.019
Dirt floor	0.177	0.284	0.246	0.284	-0.038	0.019
Wall made of board or bamboo	0.390	0.482	0.432	0.482	-0.049	0.021
Log of number of rooms	1.530	1.492	1.492	1.492	0.000	0.016
Drinking water from tap or well at home	0.446	0.350	0.376	0.350	0.026	0.020
Own toilet	0.580	0.456	0.488	0.456	0.031	0.021
No proper drainage for sewage	0.418	0.478	0.484	0.478	0.005	0.021
Household asset ownership and socioeconomic status						
Electric/gas/ kerosene stove	0.545	0.416	0.450	0.416	0.034	0.021
Owned TV	0.605	0.500	0.513	0.500	0.013	0.021
Log of annual household consumption per	14.516	14.350	14.371	14.350	0.021	0.029

capita						
Received assistance from govt/NGO during past year	0.035	0.042	0.045	0.042	0.003	0.008
		Health care variables				
Household owned pre-crisis healthcard	0.117	0.139	0.141	0.139	0.002	0.015
Know where the nearest health centre is located	0.921	0.951	0.944	0.951	-0.007	0.009
Know where local midwife is located	0.747	0.800	0.783	0.800	-0.017	0.017
Urban area	0.407	0.365	0.394	0.365	0.029	0.021
Sub-district poverty rate in Jan 1999	0.417	0.435	0.426	0.435	-0.009	0.009
Number of children	6,061	1,637	1,160	1,530		

Table 6: Treatment effect

	ATT	**Difference-in-difference effect[a]**
Probability of contacting a public provider	0.036	0.012
	(0.018)*	(0.012)
Public outpatient contact rate	0.040	0.014
	(0.015)**	(0.012)
Public outpatient visits	0.044	0.012
	(0.021)*	(0.016)
Private outpatient contact rate	-0.003	0.004
	(0.010)	(0.008)
Private outpatient visits	0.006	0.006
	(0.015)	(0.011)
Probability of using OTC medicines	-0.013	-0.049
	(0.032)	(0.020)**

Notes:
Standard errors in parentheses; ATT standard errors bootstrapped with 100 replications
+ significant at 10%; * significant at 5%; ** significant at 1%
(a) Simple difference-in-difference effect estimated by comparing treatment group with all potential controls, prior to matching based on propensity scores

Table 7: Treatment effects on contacting any public provider by age-group

Age groups	**ATT**	**Difference-in-difference effect[(a)]**
0-5 years	0.070	0.024
	(0.030)*	(0.019)
6-10 years	-0.009	-0.002
	(0.026)	(0.017)
11-15 years	0.000	0.013
	(0.042)	(0.032)

Notes:
Standard errors in parentheses; ATT standard errors bootstrapped with 100 replications
+ significant at 10%; * significant at 5%; ** significant at 1%
(a) Simple difference-in-difference effect estimated by comparing treatment group with all potential controls, prior to matching based on propensity scores

Table 8: Treatment effects on public sector outpatient care use by age-group

Age group	**ATT**	**Difference-in-difference effect[(a)]**
Contact rate		
0-5 years	0.075	0.025
	(0.026)**	(0.019)
6-10 years	-0.000	0.004
	(0.029)	(0.016)
11-15 years	-0.018	0.006
	(0.040)	(0.031)
Number of contacts		
0-5 years	0.073	0.025
	(0.038)+	(0.026)
6-10 years	0.002	-0.003
	(0.029)	(0.022)
11-15 years	-0.025	0.022
	(0.048)	(0.040)

Notes:
Standard errors in parentheses; ATT standard errors bootstrapped with 100 replications
+ significant at 10%; * significant at 5%; ** significant at 1%
(a) Simple difference-in-difference effect estimated by comparing treatment group with all potential controls, prior to matching based on propensity scores

Table 9: Treatment effects on private sector outpatient care use by age-group

Age-group	**ATT**	**Difference-in-difference effect[(a)]**
Contact rate		
0-5 years	-0.008	-0.002
	(0.021)	(0.012)
6-10 years	0.016	0.008
	(0.014)	(0.012)
11-15 years	0.031	0.011
	(0.034)	(0.020)
Number of contacts		
0-5 years	0.005	0.000
	(0.026)	(0.017)
6-10 years	0.026	0.014
	(0.021)	(0.016)
11-15 years	0.037	0.002
	(0.032)	(0.027)

Notes:
Standard errors in parentheses; ATT standard errors bootstrapped with 100 replications
+ significant at 10%; * significant at 5%; ** significant at 1%
(a) Simple difference-in-difference effect estimated by comparing treatment group with all potential controls, prior to matching based on propensity scores

Table 10: Treatment effects on use of OTC medicines

Age-group	**ATT**	**Difference-in-difference effect[(a)]**
0-5 years	-0.035	-0.052
	(0.044)	(0.030)+
6-10 years	-0.025	-0.049
	(0.043)	(0.030)+
11-15 years	0.058	-0.029
	0.101	(0.064)

Notes:
Standard errors in parentheses; ATT standard errors bootstrapped with 100 replications
+ significant at 10%; * significant at 5%; ** significant at 1%
(a) Simple difference-in-difference effect estimated by comparing treatment group with all potential controls, prior to matching based on propensity scores

Table 11: Treatment effects by consumption group

	Poorest	**Middle**	**Richest**
Probability of contacting a public provider	0.002	0.018	0.020
	(0.025)	(0.032)	(0.038)
Public outpatient contact rate	0.009	0.020	0.020
	(0.025)	(0.027)	(0.030)
Public outpatient visits	0.023	-0.012	-0.006
	(0.033)	(0.033)	(0.044)
Private outpatient contact rate	0.000	0.011	0.009
	(0.015)	(0.020)	(0.033)
Private outpatient visits	0.003	-0.001	0.057
	(0.018)	(0.024)	(0.050)
Probability of using OTC medicines	-0.018	-0.032	-0.011
	(0.043)	(0.050)	(0.053)

Notes:
Standard errors in parentheses; all standard errors bootstrapped with 100 replications
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 12: Treatment effects by urban or rural community

	Java-Bali		**Eastern-Sumatera**	
	Urban	Rural	Urban	Rural
Probability of contacting a public provider	0.031	0.030	-0.019	0.044
	(0.045)	(0.031)	(0.061)	(0.029)
Public outpatient contact rate	0.038	0.027	-0.016	0.041
	(0.038)	(0.029)	(0.065)	(0.030)
Public outpatient visits	0.056	0.034	-0.097	0.029
	(0.047)	(0.045)	(0.096)	(0.041)
Private outpatient contact rate	-0.009	0.017	-0.027	-0.006
	(0.030)	(0.015)	(0.022)	(0.013)
Private outpatient visits	0.021	0.019	-0.027	-0.006
	(0.037)	(0.020)	(0.039)	(0.018)
Probability of using OTC medicines	-0.083	0.029	-0.059	-0.015
	(0.056)	(0.050)	(0.075)	(0.059)

Notes:
Standard errors in parentheses; all standard errors bootstrapped with 100 replications
+ significant at 10%; * significant at 5%; ** significant at 1%

Annex Tables

Table A1: Propensity score estimations by region

	healthcard
Child's age	
6-10 years	-0.0051
	(0.0405)
11-15 years	-0.0769
	(0.0660)
Characteristics of head of household	
Female in 2000	0.1046
	(0.0729)
Education of head of household	
Ref: no education	
Senior high school or higher	0.1618
	(0.0878)+
Junior high school	0.1126
	(0.0911)
Primary school	0.2369
	(0.0651)**
Employment of head of household	
Agriculture or mining sector	-0.0061
	(0.0651)
Manufacturing or construction sector	0.1133
	(0.0736)
Finance or retail sector	0.0885
	(0.0702)
Social services sector	-0.0795
	(0.0771)
Worked during previous year	0.0391
	(0.0839)
Muslim head of household	-0.077
	(0.0873)
Household size and composition	
Share of children in household	0.3244
	(0.1369)*
Log of household size	-0.0372
	(0.0593)
Characteristics of house	
Annual rent (actual or imputed) for house	-0.0058
	(0.0039)

Floor made of marble/tiles/terrazzo	-0.124
	(0.0554)*
Dirt floor	0.0932
	(0.0596)
Wall made of board or bamboo	0.0946
	(0.0515)+
Log of number of rooms	-0.0073
	(0.0581)
Drinking water from tap or well at home	-0.0438
	(0.0477)
Main source of water inside house	-0.0044
	(0.0481)
Own toilet	-0.1599
	(0.0466)**
Shared or public toilet	0.1028
	(0.0681)
No proper drainage for sewage	-0.0491
	(0.0446)
Household asset ownership	
Owned TV	-0.0322
	(0.0470)
Owned fridge	-0.0892
	(0.0500)+
Owned gas or electric stove	-0.2005
	(0.0535)**
Know where the nearest hospital is located	0.1083
	(0.0442)*
Know where the nearest health centre is located	0.1886
	(0.0813)*
Know where local midwife is located	0.1579
	(0.0476)**
Used any outpatient care	-0.1816
	(0.0717)*
Used any public sector health care	0.1316
	(0.1212)
Number of visits to public health centre	0.1137
	(0.0860)
Household socioeconomic status	
Log of annual household consumption per capita	-0.0288
	(0.0337)
Household owned pre-crisis healthcard	0.2158
	(0.0602)**
Purchased food and other goods at subsidised prices	0.46
	(0.0447)**

Community level variables	
Urban community	0.1244
	(0.0525)*
Sub-district level variables	
Sub-district poverty rate in Jan 1999	0.1962
	(0.0969)*
Constant	-0.9431
	(0.5154)+
Observations	7,018
Pseudo R-squared	0.0889

Notes:
Robust z statistics in italics
+ significant at 10%; * significant at 5%; ** significant at 1%
Coefficients for province dummies are not shown but were jointly significant