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EVIDENCE OF A CAUSAL LINK BETWEEN HEALTH OUTCOMES, INSURANCE COVERAGE, AND A POLICY TO EXPAND ACCESS: EXPERIMENTAL DATA FROM CHILDREN IN THE PHILIPPINES

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SUMMARY

In this paper, we present evidence on the health effects of a health insurance intervention targeted to poor children using data from a randomized policy experiment known as the Quality Improvement Demonstration Study. Among study participants, using a difference-in-difference regression model, we estimated a 9–12 and 4–9 percentage point reduction in the likelihood of wasting and having an infection, respectively, as measured by a common biomarker C-reactive Protein. Interestingly, these benefits were not apparent at the time of discharge; the beneficial health effects were manifest several weeks after release from the hospital. Copyright © 2010 John Wiley & Sons, Ltd.

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1. INTRODUCTION

Increased utilization of health services through policy is believed to be a major pathway to better health. Remarkably, there is little empiric evidence that changing policy can increase access and in turn results in better health (Peabody *et al*., 1999). Other investigators have pointed to the many observational studies showing a direct association between health insurance and health status; they also point out that these studies have not been able to establish causal links (Levy and Meltzer, 2004). To date, the only randomized study on health insurance and health status (and thus, can properly identify causality) is the RAND Health Insurance Experiment. This ran from 1974 to 1982 and asked among other things whether free medical care resulted in better health than health insurance plans requiring patients to shoulder part of the cost. Interestingly, the reported evidence of health benefits from the experiment were limited only to individuals with myopia and hypertension (Brook *et al*., 1984). For the average patient, free care relative to cost-sharing had small effects on five health measures. Thus, although the RAND experiment provides evidence of a causal link between financial protection and health outcomes, the magnitude and selectiveness of the estimated effects limit the applicability of the results.

With nonexperimental data, causality between health insurance and health is difficult to establish because of the complex behavioral responses from both the patient and health-care provider. For

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example, with the adverse selection problem where sicker individuals are more likely to enroll in health insurance programs, health improvements among the insured may be unnecessarily smaller, perhaps because adverse selection would lead to insufficient insurance coverage for those who are most in need (Belli, 2001). Thus, the absence of nonrandom variations in health insurance coverage can bias health effects of insurance. Another behavioral response confounding health insurance effects is provider-initiated moral hazard, where increased insurance coverage may only translate into higher mark-ups, not additional services or higher quality of care needed to secure health improvements (Gertler and Solon, 2002). In addition, when insurance premiums are highly subsidized and the cost to an individual of enrolling into an insurance program is marginal, there could be little incentive for the insured to gather the information needed to optimize use of insurance benefits (Quimbo *et al*., 2008). All of these point to the possibility that health insurance could affect health (e.g. through the traditional utilization channel); health could affect health insurance (e.g. the case of adverse selection); or health could be affected by a variable that is correlated with health insurance (e.g. higher prices that in turn impedes utilization or better awareness) (Levy and Meltzer, 2004). Identifying these various causality channels can be extremely difficult with nonexperimental data. The complex relationships surrounding health insurance, utilization of services, and health outcomes require careful controls for confounding factors in properly identifying the health effects of expanded insurance. A review by Hadley (2003) of 94 studies, none involving randomized trials, showed that while a majority of the studies provide evidence of a positive association between health outcomes and having health insurance or used medical services, estimates of the magnitudes of the potential health benefits vary considerably by disease and population.

Despite the lack of solid evidence, for more than two decades, policymakers globally have advanced health insurance programs that reduce the financial barriers and increase access to care.

As such, an increasing number of people all over the world are being covered by health insurance programs. Expanded insurance policies are part of the developed and developing world's landscape. For example, in the United States, the State Children's Health Insurance Program was created in 1997 and provided $40 million for states to expand health insurance coverage for children (Trafton *et al*., 2000). Expanding health insurance is also a policy priority in emerging countries such as Korea and Taiwan (Cheng, 2003; Gertler, 1998) and in developing countries, such as the Philippines (Hsiao and Shaw, 2007).

In this study, we have the unique opportunity to evaluate possible health benefits of expanded health insurance coverage for the poor using experimental data gathered over the past 5 years in the Quality Improvement Demonstration Study (QIDS) being conducted in the Philippines. QIDS is an ongoing social experiment jointly undertaken by the Philippine Health Insurance Corporation (PhilHealth), the Department of Health, the UPEcon Foundation, and the University of California, San Francisco. By the design of the experiment – in particular, randomization of interventions – we are able to address possible biases arising from the nonrandomness of insurance status. Using data from patients that were collected in two survey rounds as well as two points in time – upon discharge and four to six weeks later – we compared health effects of expanded insurance. Lastly, QIDS has anthropometric measures of health (wasting) and a biomarker (C-reactive protein (CRP)) that are obtained from blood samples drawn from individual patients. These disparate measures capture different aspects of health and thus allow a richer analysis of health effects of insurance. In the following section, we describe in more detail the study's intervention – a PhilHealth policy that intensified enrollment efforts and expanded insurance benefits for children under 5 years old toward zero copayments for most illnesses.

2. BACKGROUND AND STUDY DESIGN

In the Philippines, improving access to care has been a priority policy concern for some time. Based on data from the 2003 National Demographic and Health Survey (NDHS, 2003), only about 45% of the

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poorest children with acute respiratory illness receive medical treatment, while about 73% of the poorest children with symptoms of diarrhea get oral rehydration therapy. Moreover, QIDS baseline data indicate that PhilHealth support rates in the study hospitals average only 30%. And out-of-pocket expenditures remains the most dominant form of health-care financing, accounting for 49% of total health-care expenditures in 2005 (Philippine National Health Accounts, 2005).

With only a 30% of poor, sick individuals able to afford care when ill, new policy initiatives have been part of a fundamental Health Sector Reform movement launched early in 2000 (DOH-PIDS, 1993 in DOH, 1999). PhilHealth – the country's social health insurance program and the largest insurance carrier – reportedly covers 86% of the population, although the actual coverage rate could be substantially smaller based on other national surveys (e.g. the 2003 NDHS). PhilHealth coverage is most expansive for those employed in the formal sector, although in recent years a series of electorally related programs have offered temporary and long-term coverage to indigent households. This complex array of policies and exogenous factors underscores how difficult policy impact assessment can be.

PhilHealth policy is to eventually provide universal coverage by 2010 (RA 7975, National Health Insurance Act of 1995). The enrollment strategy has been to do this by starting with selected population groups, i.e. the employed, indigent, retirees, and overseas workers. Premiums naturally vary across these population groups. The formally employed are charged premiums equivalent to 3% of their monthly incomes but subject to a ceiling. Indigent households are a 'sponsored group' and their premium payments, amounting to 1200 pesos (about 25 USD) per household per year, are subsidized by the national and local governments. The basic benefit package provides insurance coverage for inpatient care and benefits themselves are subject to peso ceilings, which are determined by three levels of service (i.e. ordinary, intensive, and catastrophic). When charges exceed the benefit ceilings, the patient shoulders the difference in the form of out-of-pocket payments. On an average, the QIDS surveys show that PhilHealth covers about 30% of total charges among insured children.

2.1. QIDS experiment design

QIDS is currently being implemented in 11 provinces in the central regions in the Philippines. We chose 30 public hospitals for inclusion in the policy experiment. These study hospitals have an estimated one million households in their catchment areas. The patients cared for in these hospitals are the sample population under consideration.

QIDS organized the 30 participating hospitals into matched blocks on the basis of demand- and supply characteristics such as average household income, number of beds, average case load, PhilHealth accreditation status of the hospital and PhilHealth coverage of households. The matched blocks of three hospitals were randomized to a control and two interventions. The first intervention is referred to as Intervention A (for access) and consists of zero copayments and increased enrollment. The second intervention is a provider focused quality measurement and pay-for-performance scheme and is not considered further in this paper as it was not intended to directly impact on access. All hospitals participated on a voluntary basis.

The policy objective of Intervention A is to increase existing PhilHealth coverage (measured in terms of coverage or proportion of households covered by PhilHealth), which has two levers: (i) zero copayments, which signify a large expansion of benefits and (ii) expanded enrollment, done by local government units for poor families into the PhilHealth program. PhilHealth-insured patients who utilized hospitals under Intervention A were entitled to increased peso ceilings, which was sufficient to eliminate copayment for hospitalization. As Intervention A' enrollment arm targeted indigent households, those who gained insurance coverage through the QIDS intervention had fully subsidized premiums. In addition, we used a novel method for expanding enrollment deploying personnel, referred to as Policy Navigators, who were uniquely dedicated to promoting enrollment through regular one-on-one meetings with heads of local government units (e.g. mayors and governors)

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wherein they explained the merits of insurance, encourage enrollment, facilitate administrative requirements for enrollment, and monitored enrollment trends. Shimkhada *et al*. (2008) present in more detail the QIDS experiment while Solon *et al*. (2009) describe and examine enrolment attributed to Policy Navigators in the QIDS sites.

2.2. Sampling design

To properly evaluate the impact of the QIDS interventions, we collected data at baseline or 'round 1' and after the interventions were implemented in 'round 2'. By the time round 2 commenced, interventions had been in place for close to 2 years. Identical baseline and round 2 data were collected from the intervention and control groups.

We administered patient exit surveys among parents of children under 5 years old admitted to the study hospitals during the survey rounds. At the time of discharge, all children with the diagnosis of pneumonia or diarrhea (roughly one half to two-thirds of all the discharges) were eligible. We requested parents to provide information on the child's symptoms in a variety of categories including prior to admission, health seeking behavior for the reference illness, diagnosis, services provided, drugs prescribed and purchased, payments and financing arrangements (including PhilHealth membership status). We directly measured weight and height measurements and drew blood samples from the children and to test for C-reactive protein, among others.

Pneumonia and diarrhea patients included in the exit surveys were then visited at their homes 4–6 weeks after discharge, where our field staff conducted a more comprehensive household ('follow home') survey. We collected other pertinent information such as on the socioeconomic profile as well as illness and health seeking patterns of the household. We asked a more comprehensive block of questions on the insurance status of all household members.

2.3. Study hypotheses

In this analysis, we test two hypotheses by making three comparisons (Figure 1). The first hypothesis is that as compared to controls, children who enroll and have expanded benefits (i.e. children aged 5 and under in the QIDS Intervention A group) will have better health as measured at the time of discharge. We base this on the assumption that health insurance promotes access to health services and thus better health through a variety of pathways: increased enrollment means households gain access to PhilHealth benefits, which provide partial financial support for inpatient services where they receive expanded services, and thus these children are more likely to use hospital care or get more care and thus could have better health outcomes.

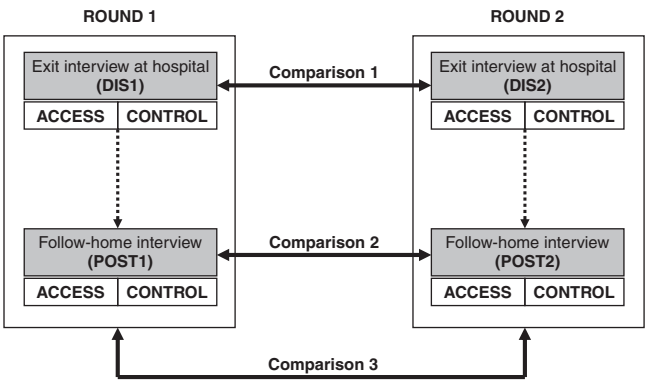

Figure 1. Survey samples and comparisons

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We know from other studies that with increased enrollment, one's chances of seeking facility-based care increases (Dubay and Davidoff, 2004; Kasper *et al.*, 2000; Newacheck *et al.*, 2000, 1998). Recovery, however, is not always assured and will be the outcome of the quality of hospital care received (Udvarhelyi *et al.*, 1992; Valderas *et al.*, 2008). With expanded benefits, quality of care may improve because of a greater likelihood of completing treatment (e.g. medication). In the Philippines, like other places, the failure to fill prescriptions is an important barrier to recovery among Filipino patients (Auer *et al.*, 2000). To test our first hypothesis, we compare objective health status measures taken upon discharge at Round 1 (DIS1) and Round 2 (DIS2) (shown as 'comparison 1' in Figure 1) against controls.

Another effect of expanded health insurance coverage is that households are better protected from the adverse effects of catastrophic health expenses on future consumption of nonhealth goods, in particular, food (Wagstaff and Pradhan, 2005; Nichter, 1994). Among poor households, confinement can cause future consumption to be reduced by the extent of loan payments. Because of the substantial out-of-pocket burden of households, poor households typically rely on loans from family and friends (Solon *et al.*, 1996). After a confinement, indebted households would now have to begin making loan payments, thus possibly affecting nonhealth consumption. Thus, our second hypothesis is that some of the health effects of insurance will appear with a lag – not manifest at the time of discharge but only later after a recovery period. To test this second hypothesis, we compared measures taken at postdischarge follow up in round 1 (POST1) and round 2 (POST2), shown as 'comparison 2' in Figure 1. In addition, we compared across survey rounds the change in health improvements between the time of discharge and 4–6 weeks post-discharge, e.g. POST1-DIS1 versus POST2-DIS2 ('comparison 3').

3. MODEL

We use a difference-in-difference specification that compares the change (before and after intervention) in health in intervention sites with the corresponding change in control sites. By looking at the change over time, we control for (a) all characteristics that do not change over time within control and intervention sites and (b) characteristics that change over time but are common to both control and treatment areas. Thus, the difference-in-difference estimator controls for individual and area-specific characteristics and secular trends that might confound the estimated impact of the insurance intervention on health status.

For comparisons 1 and 2, the difference-in-difference model can be specified as

$$\ln\left(\frac{\Pr(Y_{it}=1)}{1-\Pr(Y_{it}=1)}\right)=\alpha_0+\alpha_1 N_i+\beta_0 T_i+\beta_1 N_i T_i+\sum_j \phi_j X_{jit}+u_{it}$$

The variable, Y_{it}, in this model is a health status measure of individual patients in survey round T. We use two alternative dummy variable indicators for poor health: wasting and CRP-positive. Wasting is defined as having less than 0.90 ratio of actual weight of a child to his/her ideal weight for actual height (Del Mundo, 1999). CRP indicates the presence of an acute infection or other types of inflammation (Jaye and Waites, 1997). Although we initially considered other biomarkers such as hemoglobin and folate levels in blood (measures of nutrition status), we argue that CRP and wasting, being short-term measures of infection, are the most appropriate health status measures for this analysis. By the nature of our dependent variables, we used logistic regressions to estimate all our regression models.

The parameter $\hat{\beta}_1$ measures the difference in health status across survey rounds and across intervention and control sites. This difference-in-difference measure is our central parameter of interest.

The right-hand-side variables include the following: (i) an indicator variable for the intervention site (N); (ii) an indicator variable for the postintervention period (T), (iii) an interaction term for intervention site and postintervention period, and (iv) a vector (X) of j patient and household

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characteristics that vary across individuals and which may also vary over time, such as patient age, illness severity measures, PhilHealth coverage, and household income. The coefficient β_1 is the difference-in-difference estimate of the impact of the QIDS intervention on health.

For comparison 3, we added a third difference to the model, in order to account for the health status differences from the time of discharge (upon exit interview) and postdischarge (upon follow home interview):

$$\ln\left(\frac{\Pr(Y_{it}=1)}{1-\Pr(Y_{it}=1)}\right) = \alpha_0+\alpha_1 N+\beta_1 \mathrm{SS}_1 T_1+\beta_2 \mathrm{SS}_1 T_1 N+\beta_3 \mathrm{SS}_2 T_2 + \beta_4 \mathrm{SS}_2 T_2 N+\beta_5 \mathrm{SS}_1 T_2+\beta_6 \mathrm{SS}_1 T_2 N+\sum \beta_j X_{jit}+u_{it}$$

where SS_1 and SS_2 are indicator variables for follow home and patient exit, respectively. The expression $\hat{\beta}_6-\hat{\beta}_2-\hat{\beta}_4$ captures the difference across survey rounds, intervention and control sites, and from discharge to postdischarge.

We consider the possibility that our control and intervention groups differ in the X variables at baseline. While we strived to organize the study hospitals in matched blocks on the basis of basic demand and supply characteristics, data limitations prevented us from considering other matching variables such as health status at discharge. We thus conduct simple t-tests on the baseline data to evaluate differences in means of our independent variables. Rejecting the hypothesis that control and intervention groups are the same at baseline could lead to a modification of the basic difference-in-difference model to account for baseline differences.

We also recognize one possible unintended effect of the intervention on health seeking behavior that needed to be controlled for in the analysis, namely, hospitals randomized to the intervention would attract sicker patients with the prospect of better financial protection. The analysis, therefore, initially looks into the possibility that health status could vary across intervention sites. We used simple tests of differences in means of health status measures for this.

4. DATA

Our analysis is based on a sample of close to 1100 patients each in the intervention and control sites. There were about 500 patients in round 1 and 600 patients in round 2. Table I shows that at round 1, the mean age of patients was about 20 months. Average household income was about 60,000 pesos (about 1260 USD), which was roughly 60% below the national average. Mothers of the interviewed patients, on the average, had close to 9 years of schooling about 2 years short of completion of secondary education. Less than one-third of the patients had PhilHealth coverage. All variables except for length

Table I. Means of dependent and independent variables; full sample, baseline, by intervention sites

	Round 1		
Variable	Intervention	Control	p-Value[a]
Wasting (percent)	39.9	36.9	0.34
CRP (percent positive)	23.2	25.5	0.39
Age (in months)	20.4	20.2	0.79
Household income (annual, in pesos)	59 435	52 367	0.11
PhilHealth coverage (percent)	29.1	26.7	0.40
Mother's education (years of schooling)	9.0	8.6	0.05
Length of stay (number of days)	3.4	3.2	0.15
Number of observations[b]	596–608	575–578	

[a]p-Value for the test in difference in the means across access and control groups.
[b]A range is reported as the number of missing variables differs by variable.

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Table II. Health status at round 1

	Intervention sites		Control sites		Intervention sites	Control sites
	Uninsured	Insured[a]	Uninsured	Insured[a]	All[b]	All[b]
Wasting (percent, upon discharge)	42.2%	33.6% (0.09)	37.2%	36.2% (0.84)	39.9%	36.9% (0.34)
CRP (percent positive, upon discharge)	22.9%	24.0% (0.79)	24.5	28.1 (0.41)	23.2%	25.5% (0.39)
Length of stay (number of days)	3.4	3.3 (0.44)	3.2	3.3 (0.38)	3.4	3.2 (0.15)
Number of observations[c]	344–354	125–129	331–355	141–146	469–483	472–501

[a]*p*-Values for the test of differences in the means between uninsured and insured are enclosed in the parentheses.
[b]*p*-Values for test of difference in the means across intervention and control groups are enclosed in the parentheses.
[c]A range is reported as number of missing variables differs by variable.

of stay have no significant differences across the intervention and control sites, although the difference in length of stay is negligible (0.2 days).

Table II shows that at baseline, over one-third of the patients were wasted and about one-fourth were CRP-positive upon discharge. There were no significant differences across intervention and control sites in health status at discharge, based on wasting, CRP, and length of stay expected from our random matched block design. Moreover, we found no such health differences across the uninsured and insured subsamples of patients within sites. Thus, there are no apparent selection issues to address, as the insured are not necessarily sicklier than the uninsured in the intervention sites.

5. REGRESSION RESULTS

Tables III and IV report the regression results for the three comparisons. Table V summarizes the difference-in-difference estimates of the intervention effects on health status.

We find that postdischarge health outcomes (comparison 2) are better in intervention sites, in terms of both being CRP-positive and being wasted. Being confined in an intervention hospital decreases the likelihood of being CRP-positive and wasted by 4 and 9 percentage points, respectively. Our results for the first comparison, however, indicate that the intervention had no immediate impact on the discharge outcomes. That is, patients – whether in the intervention or control sites – had the same health conditions upon discharge and benefits were only observable in the early postdischarge period.

The lagged effect is consistent with the hypothesis that increased insurance or better financial protection confers better health and nonhealth (e.g. food) consumption of the patient in the future. We might expect insurance effects at the time of discharge, for example, from provider-initiated moral hazard wherein insured patients are kept longer in the hospitals, thus potentially allowing them to achieve better health status before discharge. However, our model addresses this possibility by controlling for length of stay. Thus, the absence of an insurance effect upon discharge seems to suggest that the discharge decision is based on a physician's clinical assessment and thus would not be expected to vary across intervention and control sites. Doctors base discharge decisions on the observation that a patient has attained the same minimum level of health status when they are discharged. Postdischarge, however, patients with expanded insurance would not have to borrow or borrow as much to pay for hospital bills, which in turn implies an ability to protect outpatient medical care, parental support, or more food consumption. This suggests that expanded insurance ensures the patient of being on a more stable long-term trajectory of health improvement.

These estimated health effects are large considering that the proportion of wasted children in the Philippines was about 27% in 2003 (Food and Nutrition Research Institute, 2003) and that a

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Table III. Logistic regression results (marginal effects) for comparisons 1 and 2

	Comparison 1 DIS 1 versus DIS 2				Comparison 2 POST1 versus POST2			
	Wasted		CRP+		Wasted		CRP+	
	dy/dx	p-Value	dy/dx	p-Value	dy/dx	p-Value	dy/dx	p-Value
Access	0.035	0.29	−0.025	0.36	0.024	0.42	0.025	0.17
Access* Round 2	0.000	1.00	0.024	0.55	−0.090	0.00	−0.041	0.00
Round 2	−0.011	0.73	0.045	0.10	0.112	0.00	0.000	0.99
PhilHealth member	−0.038	0.10	0.010	0.64	−0.006	0.78	−0.014	0.24
Mother's education (years of schooling)	−0.015	0.00	−0.007	0.02	−0.011	0.00	−0.003	0.14
Household income (annual, PhP)	−3.62E-07	0.04	−1.24E-07	0.35	−1.78E-07	0.27	1.09E-07	0.09
Length of stay (number of days in the hospital)	0.004	0.51	−0.006	0.22	−0.004	0.40	0.003	0.37
Age (in months)	0.003	0.00	0.003	0.00	0.001	0.05	−0.001	0.23

Table IV. Logistic regression results (marginal effects) for comparison 3

	(POST1-DIS1) versus (POST2-DIS2)			
	Wasted		CRP+	
	dy/dx	p-Value	dy/dx	p-Value
Access	0.035	0.29	−0.026	0.35
Follow home*Round 1	−0.113	0.00	−0.196	0.00
Follow home*Round 1*Access	−0.006	0.90	0.111	0.11
Exit*Round 2	−0.010	0.73	0.042	0.13
Exit*Round 2* Access	0.000	0.99	0.025	0.54
Follow home*Round 2	0.001	0.99	−0.191	0.00
Follow home*Round 2*Access	−0.122	0.00	−0.090	0.04
PhilHealth member	−0.024	0.17	−0.001	0.95
Mother's education (years of schooling)	−0.014	0.00	−0.008	0.00
Household income (annual, PhP)	−3.00E-07	0.02	−5.20E-09	0.96
Length of stay (number of days in the hospital)	0.000	0.92	−0.003	0.48
Age in months	0.002	0.00	0.001	0.01

Table V. Summary of difference-in-difference estimates (in percentage points)[a]

Type of comparison	Wasted	CRP+
Comparison 1 (DIS1 versus DIS2)	−0.01%	2.4%
Comparison 2 (POST1 versus POST2)	−9.0%[b]	−4.1%[b]
Comparison 3 (POST1-DIS1) versus (POST2-DIS2)	−12.2%[c]	−9.0%[c]

[a]A negative (positive) number indicates improvement (deterioration) in health.
[b]Significant at 5%.
[c]Significant at 10%.

9 percentage point decrease among the general population of children is equivalent to a 30% improvement in the overall incidence of wasting. This result is important considering for example that Levy and Meltzer (2004) found, after overcoming measurement difficulties, small positive health insurance effects among the vulnerable population groups such as the elderly and children.

We did not find that there was a difference in health status at the time of discharge but only in the 4–10 week follow-up period. We interpret that this is because clinicians will not and do not differentially

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(prematurely) discharge uninsured patients. However, because the children in the intervention group are healthier in the postdischarge interval, it suggests that there is a better trajectory to full health recovery in the intervention group.

Our regression results for the third comparison provide more direct evidence that there are larger health status improvements among patients in the intervention compared to control hospitals. This difference in health improvements – expressed in terms of probabilities – is about 12 and 9 percentage points for not wasted and CRP-negative, respectively.

From a policy point of view, it would be useful to decompose our estimated health effects into enrollment and expanded benefits, the two policy levers of intervention. In this study, data limitations – in particular, the absence of information on how insurance status changed over time – prevented us from disentangling these two effects. Such information would make it possible to identify a subsample of patients who, if they were insured in both survey rounds, would allow an attribution as to a benefit versus the increased enrollment effects. Anecdotal evidence, however, leads us to believe that in our study setting the expanded benefit effects could be much smaller than the enrollment effects. One possible explanation is that prices of public hospital services tend to be modest, especially in secondary district hospitals such as those in our study sites. The inpatient benefit package of PhilHealth provides coverage up to peso ceilings, which are specified for every expense category (including room and board, professional fees, diagnostic procedures including laboratory exams, and prescription drugs). Since many of our intervention hospitals price their services well below the reimbursement ceilings, the upward adjustment of ceilings due to the intervention had little impact on the insurance support received by patients.

This has been a practice that dates back to the predevolution years, when government hospitals charged fixed fees that were thought to be generally below costs. In 1991, when health services including district hospitals were devolved to local government units, observers noted that a number of predevolution policies and practices were continued, similar to older evidence on price discrimination in public hospitals by insurance status (de la Paz-Kraft in 1997). QIDS claims data indicate that few patients benefited (i.e. received more services) from having higher insurance ceilings in QIDS hospitals. By contrast, enrollment effects, due to the use of Policy Navigators in QIDS, have been reported to be relatively large (Solon *et al.*, 2009).

6. CONCLUSION

In this paper, we present evidence from a randomized policy experiment on the health effects of a health insurance intervention targeted to poor children. Unlike the RAND experiment, though, we compare hospital users and see whether those who have better insurance coverage also have better outcomes. The health effects we found are relatively large and become manifest in the immediate post discharge period. These lagged health effects could be due to the impact of insurance coverage on household allocation, where financial protection possibly allows households to avoid having to use food budgetary allocations for health expenditures. However, as we were unable to identify via regression analysis whether it is increased enrollment or expanded insurance benefits that drive health status improvements nor the exact reasons why health effects of expanded insurance appear with a lag, future research should validate these with more comprehensive data. Longer term health effects of insurance, such as prevention of deaths, can also be observed with data on children followed for longer periods.

While the evidence from the RAND experiment continues to be important, it is now close to three decades old, showed benefit for only a limited number of health conditions, and is not applicable to the poorest parts of the world where most people live. Results from the QIDS experiment could be particularly more relevant to policymakers in developing countries where infectious diseases continue to be a public health concern and where children represent an important population group.

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