Effects of Interventions to Raise Voluntary Enrollment in a Social Health Insurance Scheme

A Cluster Randomized Trial

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Abstract

A cluster randomized controlled trial was undertaken, testing two sets of interventions to encourage enrollment in the Philippines' Individual Payer Program. Of 243 municipalities, 179 were randomly assigned as intervention sites and 64 as controls. In early 2011, 2,950 families were interviewed; unenrolled Individual Payer Program-eligible families in intervention sites were given an information kit and a 50 percent premium subsidy until the end of 2011. In February 2012, the "non-compliers" had their voucher extended, were re-sent the enrollment kit, and received Short Message Service (SMS) reminders. Half were told that in the upcoming end-line interview the enumerator could help complete

the enrollment form, deliver it to the insurer, and have identification cards mailed. The control and intervention sites were balanced at baseline. In the control sites, 9.9 percent (32/323) of eligible individuals had enrolled by January 2012, compared with 14.9 percent (119/801) in intervention sites. In the sub-experiment, enrollment was 3.4 percent (10/290) among eligible non-compliers and who did not receive assistance but 39.7 percent (124/312) among those who did. A premium subsidy combined with information can increase voluntary enrollment in a social health insurance program, but less than an intervention that reduces the enrollment burden; even that leaves enrollment below 50 percent.

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by

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Introduction

Much of the world is in the midst of a push toward universal health coverage (UHC) (World Health Organization 2010). One of the biggest challenges facing developing countries is extending coverage to informal-sector workers (Tangcharoensathien et al. 2011; Lagomarsino et al. 2012). This group is very mobile, and since they are also typically self-employed, these workers do not have employers who can be mandated to deduct premium contributions from their salaries. Since they are not officially poor or marginalized, and because they represent a sizeable fraction of the population, many countries are reluctant to fully subsidize their coverage, although some have done so (Thailand is an example (Pannarunothai et al. 2004)), albeit with somewhat less generous coverage than that enjoyed by the previously covered. Many countries have instead used partial subsidies to encourage enrollment either into a new government-run scheme, as in China (Wagstaff et al. 2009a; Wagstaff et al. 2009b; Yip et al. 2012) and Mexico (Frenk et al. 2003; Frenk et al. 2006), or into the government's existing social health insurance (SHI) program, as in the Philippines (Obermann et al. 2006; Jowett and Hsiao 2007) and Vietnam (Ekman et al. 2008; Lieberman and Wagstaff 2009; Wagstaff 2010). Some countries (including China and Mexico) have achieved high coverage levels, but they have ended up almost fully subsidizing coverage. Countries like the Philippines and Vietnam have not, and these are better tests of the voluntary enrollment model; unhappily for the governments concerned, growth in enrollment among the informal sector in these countries has been sluggish.

The question naturally arises as to what additional steps governments might take to raise voluntary enrollment rates and hence move closer towards UHC. Surprisingly little is known about the relative cost-effectiveness of alternatives measures. In this paper, we present evidence from two randomized experiments conducted in the Philippines, where the

informal sector remains the population group with the lowest coverage rate, despite numerous initiatives (see ‘Social health insurance in the Philippines’ panel). Our main experiment tested the effectiveness, in terms of its impact on enrollment in the voluntary government-run Individual Payer Program (IPP), of a combination of information and premium subsidy; our sub-experiment tested – on the non-compliers in the main experiment – a package of measures that drastically reduce the amount of effort a family has to make to enroll.

Literature review

Observational studies have highlighted a variety of factors influencing the demand for health insurance in developing countries. Knowledge about enrollment options and procedures were found to be important among informal sector workers in Kenya (Mathauer et al. 2008). In Vietnam, voluntary enrollment has been found to increase with expected health benefits of health insurance, as indicated by proximity to public hospital and the number beneficiaries within a family (Nguyen and Knowles 2010). While distance to and the quality of health facility were also found to be significant motivating factors in Rwanda and Guinea, the affordability of the insurance premium was also found to be an issue (Schneider and Diop 2001; Criel and Waelkens 2003).

There is a dearth of rigorous evaluation of efforts to encourage voluntary SHI participation. Just two studies are included in a recent systematic review (Acharya 2013). In one, randomly-selected communities were subjected to a campaign to increase enrollment in Mexico’s Seguro Popular program and received increased medical personnel and drug supplies; enrollment was 37 percentage points higher in the intervention areas (King et al. 2009). In the other informal-sector workers in open-air markets in Managua,

Nicaragua, were randomly assigned between a group receiving a voucher entitling them to six months free coverage in the SHI program, a group receiving an information pack, and a control group; those receiving the subsidy voucher were found to be 30 percent more likely to enroll than the control group, but those receiving the information pack were actually five percentage points less likely to enroll than the control group (Thornton et al. 2010).

Two studies were missing from the aforementioned systematic review. One (Das and Leino 2011) was a RCT investigating the effects of information on enrollment in India's Rashtriya Swasthya Bima Yojana (RSBY) insurance scheme for the poor; the authors found the information package by itself had negligible effects, but that those given the information package and included in a sample survey were slightly more likely to enroll. The other missing study is of a quasi-experimental study in Bangladesh where some localities offered an education intervention about health insurance; the treatment group reported a 34 percent higher willingness-to-pay for health insurance (Khan and Ahmed 2013).

Study setting

The setting is Philippines, a country which has made a strong commitment to UHC (Jowett and Hsiao 2007; Chakraborty 2013). SHI in the Philippines dates back to 1969, although mandatory coverage of formal-sector workers did not begin until 1972 (Jowett and Hsiao 2007). Up to 1997 SHI coverage remained concentrated among formal-sector workers and their family members, with coverage reaching around 37 percent of the population in 1997. Coverage picked up toward the end of the 1990s as the result of three initiatives: the creation in 1995 of PhilHealth, a government corporation tasked with administering the national health insurance program; the launch in 1997 of an indigent program aimed at

poor households; and the launch in 1999 of the Individually Paying Program (IPP) aimed at informal-sector workers and self-employed professionals and their families. Two initiatives in the 2000s helped increase coverage still further: the launch in 2001 of Plan 500, aimed at mass enrollment of poor households; and the setting up in 2003 of the Partnership with Organized Groups initiative, aimed at the informal sector.

By 2010 PhilHealth reported a coverage of 70 million beneficiaries, equivalent to 75 percent of the population (Manasan 2011). Of these, 29 million (42 percent) were formal-sector workers and their families; 22 million (32 percent) were poor families; and only 11 million (16 percent) were IPP-enrolled informal-sector workers and their families (Manasan 2011). This third group had the lowest coverage rate by far: in 2004 only 23 percent of eligible persons were actually IPP members; by 2010, this figure had increased to 33 percent but no other target group accounts for more un-enrolled individuals (Jowett and Hsiao 2007; Manasan 2011).

In contrast to formal-sector workers for whom enrollment is mandatory and whose contribution is linked to earnings and split between the employee and employer, and the poor who are supposed to be identified and enrolled at the tax-payer's expense by local governments, informal-sector workers decide whether to enroll themselves and their families (i.e., spouse and all children below 21 years old). If they choose to enroll, they pay PhP 2,400 per annum if they are a professional earning at least PhP 300,000 per annum or PhP 1,200 per annum otherwise (Manasan 2011). The regular IPP premium of PhP 1,200 is equivalent to \$US27.32, 6.4 percent of the official Philippines poverty line in 2012, and 1 percent of mean per capita consumption; IPP enrollees can pay the premium on a quarterly, semi-annual or annual basis, and should have paid at least three months within the

immediately preceding six months to stay covered. Irregular payments have been common among IPP enrollees.

A variety of initiatives have been launched to increase coverage among the informal sector, most recently a facility for paying premium through mobile phones launched in 2009, and a series of nationwide, municipal-level registration and advocacy fairs held on selected Saturdays in 2011. The overall impact of these initiatives appears to have been limited.

Research design and intervention

Study sites and participants

We sought to obtain a nationally-representative random sample of households eligible for IPP. To achieve this we sampled five randomly-selected households in 590 ‘barangays’, distributed across most of the Philippines and selected using a multi-stage cluster design with five stages: the broad region (5: the National Capital Region, North-Central Luzon, South Luzon, Visayas, and Mindanao); the administrative region (15); the province (63); the municipality or city (hereafter ‘municipality’) (243); and the barangay (590).

In our main experiment, three quarters of the sampled municipalities within each broad region were assigned via randomization (see below for details) as treatment sites; the rest became control sites. The municipality (rather than the barangay or household) was selected as the unit for the experimental cluster to reduce the risk of contamination between the treatment and control groups: treatment households could have more easily

passed on the details of the information leaflets to households in the control group. We interviewed, on average, 12 households per cluster.

All sampled households in the intervention sites who were eligible for the IPP program (Group I) were given the first intervention (see Figures 1 and 2); sampled households in the control sites, including those who are IPP-eligible (Group II), were not. We considered households eligible for IPP if the household head claims not to be covered, or claims to be covered but has not paid a premium in the preceding six months (the PhilHealth criterion to be considered an active member). The household's insurance coverage status and eligibility were determined using a protocol administered by survey enumerators, and only after the household head, spouse or an adult member had been interviewed during the baseline survey.

Households in Group I who had still not enrolled by January 2012 (9-11 months after the baseline survey – see Figure 2) became the sample for our sub-experiment (Group III in Figures 1 and 2). Half were given only Intervention II. The rest were also given Intervention III. This sub-experiment was at the household level, not the municipality level.

Timing of data-collection and interventions

Families in the main experiment were recruited through the baseline survey which was conducted February-April 2011 (see Figure 2). IPP-eligible non-complier families in the treatment sites as of January 2012 were recruited into the sub-experiment in February 2012.

Interventions

Intervention I comprises an insurance voucher, an information kit, and several SMS messages (see also Panel ‘Interventions’). Valid for eleven months initially (1 February to 31 December 2011) and worth 600 pesos (US\$13.66), the voucher pays for the initial 50 percent (or 25 percent if the individual’s average monthly family income is greater than 25,000 pesos) of the annual insurance premium. The kit includes a PhilHealth membership application form, a membership data record form, and leaflets covering enrollment, insurance claims and various frequently-asked questions. During the voucher’s validity period, SMS reminders were sent by the study team at regular intervals to these households reminding them to submit their completed application forms and vouchers to a local PhilHealth office, to pay the balance of their premiums, and to take advantage of their benefit entitlements as SHI beneficiaries. Households in the control sites did not receive Intervention 1 or any other intervention.

Intervention II involved re-sending (by mail) the membership application forms, along with letters and SMS messages from the study team saying that the voucher’s validity had been extended up to the end of February 2012.

Intervention III involved the enumerator who visited the family for the endline survey offering to help the family complete the application form. The form was then sent to PhilHealth, and the family’s IPP ID card was sent by mail to the family (see Figure 3).

Idea behind the interventions

The objective of the main experiment is to see how far voluntary enrollment in the government’s SHI program responds to an intervention that combines information and an

enrollment subsidy. The general objective of the sub-experiment was to see the effects on enrollment among non-compliers (families who had been given the information and the subsidy but chose not to enroll) of measures aimed at drastically simplifying the enrollment process.

The rationale for the subsidy is that an unenrolled family's willingness-to-pay (WTP) for coverage must be less than the overall costs of enrollment, including time and travel costs. The direct monetary cost of enrollment is actually not very high relative to average consumption, but the time and travel costs associated with the rather cumbersome enrollment process may well be a deterrent. On the other side of the balance sheet, WTP for coverage could be low for a number of reasons. Families may not be sufficiently risk-averse. Some informal-sector workers may perceive the probability of falling sick and needing care to be too low to be worth enrolling, i.e. the SHI scheme may suffer from 'adverse selection'. Families may also think that the benefit conditional on falling sick is too small, either because insurance makes an insufficiently large dent in out-of-pocket payments, or because insurance is perceived to be associated with lower quality care; the latter could be due to enrollees being limited to accredited providers, or to a belief that providers may deliver worse quality care to insured patients (Centrángolo et al. 2013).

There is, in fact, evidence that SHI coverage in the Philippines provides limited financial protection. Out-of-pocket payments are high among the insured, not just among the uninsured. These large out-of-pocket payments reflect coverage ceilings (expenses beyond the ceiling are not covered) and a policy whereby providers are allowed to set their own prices leaving the patient to pay out-of-pocket the difference between the price and PhilHealth's ceiling. Providers can – and apparently do – raise their price for insured patients who could end up paying a similar amount out-of-pocket to what they would have

paid without insurance (Gertler and Solon 2002). Enrollees also face a high degree of uncertainty over both the amount PhilHealth will reimburse and the amount the provider will charge; hence the degree of financial protection afforded by SHI membership is hard to establish ex ante.

Outcome

The outcome measure of interest is whether the family is enrolled in PhilHealth's IPP scheme. (The IPP, despite its name, is designed as family scheme.) In the main experiment, pre-intervention and post-intervention enrollment status was self-reported through the baseline and endline surveys; the endline survey asked when a family enrolled, and enrollment status is as of January 2012. In the sub-experiment, pre-intervention enrollment status was established in January 2012 from PhilHealth records and verified subsequently in the endline survey in March-May 2012. (Nine families reported by PhilHealth not to be enrolled as of January 2012 turned out in the endline survey to have been enrolled at that date.) Post-intervention enrollment status among those receiving only Intervention II was self-reported through the endline survey in March-May 2013 and subsequently validated by PhilHealth records. For those receiving Intervention III, enumerators knew enrollment status of each family because the enumerator either helped complete and then deliver the enrollment form or not as the family wished.

Sample size and statistical power

We fixed the sampling of municipalities and households within municipalities to achieve at least 80 percent power at the five percent level to detect enrollment rate increases (compared to the control group rate of 10 percent) of 7.5 percentage points. This target effect was because it was felt to be the minimum impact that might be considered

meaningful from a policy perspective; a 50 percent subsidy combined with an information packet is a serious policy effort, and an effect of less than 7.5 percentage points would raise serious questions about their usefulness as a mechanism to help achieve UHC. In our calculations, we assumed a standard deviation in the enrollment rate of 0.25, an intra-cluster correlation coefficient (ICC) of 0.16 (the municipality is our cluster), and a ratio of intervention to control households of just under 3 (we have 243 municipalities in total of which 179 were assigned to the intervention group).¹ On these assumptions, we could, in fact, have sampled fewer municipalities and fewer households and still achieved 80 percent power to detect a 7.5 percentage point effect. Or equivalently with our sample we could detect a somewhat smaller effect with 80 percent power: we could reduce the target effect to 7.25 percentage points and still have 80 percent power; below 7.25, the number of municipalities in the intervention group becomes too small. We chose to sample so many municipalities and so many households per municipality in part because the sample survey was used for other studies as well as this RCT study.

Randomization

The sample municipalities were randomly assigned as intervention or control sites via block randomization using the broad region as the block. Within each broad region, each municipality was assigned a computer-generated random number; municipalities were then ranked in descending order of their random number; starting at the top of the list, municipalities continued to be classified as intervention sites up to the point where 75 percent of sampled barangays had been accounted for; the rest of the sampled

¹ In the event, our intra-cluster correlation assumption proved overly pessimistic (it was only 0.07 in our data), but our assumption about the standard deviation for the enrollment rate proved overly-optimistic (it was 0.3 in the treatment group and 0.35 in the control group).

municipalities – which account for 25 percent of the sampled barangays – were classified as control sites.

Statistical methods

The effect of Intervention I was estimated by comparing the enrollment odds ratios of (a) eligible families in the intervention sites (Group I) and (b) eligible families in the control sites (Group II) (see Figures 1 and 2). Estimation was via a logistic regression of enrollment status as of January 2012 on a dummy variable capturing whether the family lived in an intervention site; only eligible families were included in the regression. Standard errors were adjusted for clustering at the municipality level. Adjusted effects were obtained by re-running the logistic regression including – in addition to the treatment dummy variable – the baseline values of the covariates listed in Table 1.

The impact of Intervention III (relative to Intervention II) was estimated by comparing the odds ratios of (a) families receiving both interventions II and III and (b) families receiving only Intervention II (see Figures 1 and 2). Estimation was via a bivariate regression of enrollment status as of April/May 2012 on a dummy variable indicating whether the family had received Intervention III, estimated on the subsample of IPP-eligible non-complier families (Group III). Although the assignment of Intervention III is at the household level, to be cautious we still adjusted standard errors values for clustering at the city level, since the assignment in the main experiment was at that level. Adjusted effects were obtained by re-running the logistic regression including – in addition to the treatment dummy variable – the baseline values of the covariates listed in Table 1.

Data

Our data were largely from baseline and endline household surveys designed by us with a view to the data requirements of the present RCT study but also other studies. However, as mentioned above, in the sub-experiment, pre-intervention enrollment status was established from PhilHealth records and verified subsequently in the endline survey. Post-intervention enrollment status among those receiving only Intervention II was self-reported through the endline survey and subsequently validated by PhilHealth records. For those receiving Intervention III, enrollment status was established by the enumerator because they either helped complete and then deliver the enrollment form or not as the family wished.

Results

Participant flow, eligibility and attrition

The full sample is 2,950 households (Figure 1 shows the participant flow in the study). By design three quarters of these live in intervention municipalities. Of these, 1,419 (64 percent) proved ineligible for the experiment, already being an IPP member or in another PhilHealth scheme. (The fraction of the sample with some form of PhilHealth coverage already was somewhat lower in the control group: 56 percent.) The 801 eligible families in the intervention sites (Group I) were offered Intervention I; the IPP-eligible families in the control sites (Group II) were not. Of the 682 families in Group I who – as of January 2012 – had not enrolled in the IPP scheme, 602 were still eligible for it (Group III). These were roughly evenly split into two groups, with 290 receiving Intervention II only, and 312 receiving both Interventions II and III.

Baseline values and inter-group differences

The differences at baseline are shown in the Table 1. By selection, neither Group I nor Group II contains any IPP enrollees at baseline and the outcome binary variable (enrollment status) is zero. In the main experiment, families in the intervention and control sites differ significantly at baseline at the five percent level on only two variables: families in the intervention sites are more likely to be living in an urban area and have a less educated household head. In the sub-experiment, there are also only two variables where differences that are significant at the five percent level are observed between the groups receiving both Interventions II and III (the treatment group for the sub-experiment) and those receiving only Intervention II (the control group): families in the treatment group were less likely to have had an adverse health event in the previous 12 months, and had fewer children.

Numbers analyzed

Only families with complete information on the covariates used in computing the adjusted results were included in the analysis. The numbers of families in the treatment and control groups in the two experiments are shown in Figure 1.

Intervention impacts

The effects of the two sets of interventions for the full sample are reported in Table 2. The combined information and subsidy intervention in the main experiment (Intervention I) raises enrollment by five percentage points; the effect is significant at the five percent level. Given the counterfactual (the rate among eligible families in the control sites) is 10 percent, this represents an approximately 50 percent increase (the relative odds

ratio is 1.59; 95% CI: 1.00-2.52)). This is a large percentage effect; moreover, given the 50-percent reduction in the annual premium for at least some of the IPP-eligible households, the results imply that the demand for insurance is price-elastic, i.e. the elasticity is greater than one. If the treatment effect is converted to population projections, the 50-percent increase in IPP enrollment is equivalent to an extra 335 thousand households in the IPP. Nonetheless, the fact remains that even after the information and subsidy, the IPP enrollment rate is just 13 percent.

Adding the home visit and the delivery of completed form to the PhilHealth office (Intervention III) to the re-mailing of the forms, the extension of the voucher validity and the SMS reminders (Intervention II) has a dramatically larger effect than Intervention I has (relative to no intervention) (relative odds ratio 18.47; 95% CI: 9.03-37.76). Projecting this effect to the entire population, we estimate that this intervention would add an additional 2.36 million families into the IPP. This result suggests that the insurance take-up is likely to be more sensitive to reductions in indirect costs than to reductions in premium. Again, it is important, however, to note how many families remain unenrolled after the intervention: 60 percent of families chose not to enroll despite receiving an information kit, a 50 percent subsidy, and help at home completing the form; and despite not having to go to the PhilHealth office to deliver the completed form.

Adjusting for differences in covariates across groups makes relatively little difference to the relative odds ratios or their p-values (Table 2); this reflects the high degree of balance on observables at baseline in Table 1.

Table 2 also shows the effect of restricting the analysis to families reporting either an adverse health event or having a household head with poor health. Our hypothesis here

is that these groups ought to be more responsive to the two interventions, since insurance has a greater value for them; in fact, we see this ‘adverse selection’ in the baseline data where those reporting an adverse health event in the previous year have a higher willingness to pay for health insurance. By contrast, in the main experiment, we see no significant effect of Intervention I among families with health problems, and in the sub-experiment, we see a smaller – albeit still significant – effect of Intervention III relative to Intervention II. Table 2 also shows the effect of restricting the sample to families whose head had at most some secondary education. We see somewhat larger effects of both the main experiment and sub-experiment among this group than among the sample as a whole, consistent with the cost of insurance and the enrollment process being a greater burden on poorer and less well educated families.

Discussion

Our results suggest that subsidies to the informal sector – the target population of the Philippines Individual-Payer Program – can expand voluntary participation in a SHI program, but that even a generous subsidy leaves the bulk of the target population unenrolled. In this case, a 50 percent premium subsidy, bundled with general and personalized information (i.e. interactive SMS reminders), led to a five percentage point (50 percent) increase in enrollment among those who initially had chosen not to enroll; however, even among the group receiving the subsidy, enrollment reached only 13 percent.²

² The government has since expanded the number of fully subsidized enrollees in the program. As of January 1 2014, the state fully subsidizes the health insurance premiums of around 45 million of the poorest Filipinos, defined as lying below a defined threshold on the National Household Targeting System for Poverty Reduction (NHTS-PR) list, approximately located at the upper end of the second quintile. This was enabled by the passage of a “sin tax” law, effective January 1 2013, which earmarked a share of its revenues for health insurance coverage among the poor and the near-poor. No doubt, some share of this expansion includes coverage of those whom this paper defined as the informal sector.

More promising than a premium subsidy seems to be a combination of measures aimed at simplifying the enrollment process, in our case involving someone coming to help complete the forms in the home, taking the forms to the SHI agency's office, and having the ID card mailed to the family. This intervention led to a massive 36 percentage point (1068 percent) increase in enrollment compared to a reminder and extension of the subsidy voucher validity.³ This dramatic difference points to the importance of the transport and time costs associated with enrollment – costs that were highlighted by one participant who texted in reply to one of our SMS reminders that she had missed the boat and therefore could not go the local PhilHealth office that day. It is important to realize, however, that even with all this assistance, 60 percent of the sub-sample chose not to enroll.

Since our main intervention bundled a subsidy intervention and an information intervention, it is not possible to separate out the effects of subsidies and information. It is possible that the information had a negligible effect and all the effect was driven by the premium subsidy, which would be consistent with the Nicaragua study by Thornton et al. (2010). Moreover, in our sub-experiment we bundled our enrollment process simplification intervention with the subsidy and information interventions, so we do not know how effective the enrollment process simplification intervention would be by itself. Nor do we know how effective it would be on the general population of unenrolled families – our sub-experiment was conducted among the non-compliers in the main experiment, a particularly hard group to entice into the scheme. And of course, one would also want to know the costs of the various interventions, not just their effects. The enrollment process simplification intervention, as Figure 3 illustrates, involves several steps, and entailed a substantial amount of time for both the family and the study team. Automatic enrollment would

³ The government has since simplified the enrollment process: <http://www.gov.ph/2014/01/28/philhealth-registration-made-easier-for-every-juan/>.

eliminate such costs, of course, and is being considered by PhilHealth for at least some groups, such as the near-poor.

Figure 1: Participant flow

Figure 2: Study timeline

	Feb-Apr 2011		Dec 2011	Jan 2012	Feb 2012	Mar-May 2012
Group I	Baseline survey	Intervention I	Intervention I voucher ceases to be valid at end of the month	Voucher use data obtained from PhilHealth		Endline survey; Intervention III voucher is valid up to date of endline survey interview.
Group III (non-compliers among Group I)					All Group III get Intervention II. Half also get Intervention III; For the half who did not get Intervention III voucher ceases to be valid at end of the month.	
Group II		No intervention				

Figure 3: Workflow in Intervention III

Table 1: Baseline values*

Main Experiment	Treatment [@]	Control [@]	<i>p</i> -value
Average per capita household expenditure	24663	23726	0.568
Head had at least college education	0.080	0.124	0.021
Head is working	0.889	0.854	0.110
Head has poor health	0.021	0.031	0.335
Experienced adverse health outcome in the last year	0.276	0.238	0.198
Number of Children under 21	1.829	1.731	0.394
Has other insurance	0.021	0.031	0.335
Has available health facility within 15 minutes	0.717	0.703	0.644
Urban	0.534	0.437	0.003
National Capital Region	0.205	0.161	0.092
Rest of Luzon	0.412	0.464	0.108
Visayas	0.211	0.204	0.804
Mindanao	0.172	0.170	0.936
N	801	323	
Sub-experiment			
Average per capita household expenditure	33900	30912	0.703
Head had at least college education	0.081	0.059	0.306
Head is working	0.837	0.867	0.332
Head has poor health	0.019	0.012	0.525
Experienced adverse health outcome in the last year	0.119	0.188	0.028
Number of Children under 21	1.763	2.070	0.048
Has other insurance	0.015	0.020	0.677
Has available health facility within 15 minutes	0.741	0.707	0.388
Urban	0.544	0.508	0.401
National Capital Region	0.248	0.180	0.056
Res of Luzon	0.370	0.418	0.265
Visayas	0.211	0.250	0.290
Mindanao	0.170	0.152	0.575
N	270	256	

*The covariates are baseline survey values in the main experiment and endline survey values in the sub-experiment. #Only cases with non-missing values on all covariates and enrollment status were included in the analysis. @The treatment group in the sub-experiment comprises the families who received Intervention III as well as Intervention II; the control group comprises the families who received only Intervention II.

Table 2: Effects of interventions I and III

	Enrollment rate	Odds ratio and 95% CI	p-value	Adjusted odds ratio and 95% CI	p-value	ICC [@]
<i>Full sample</i>						
Main experiment: Control	32/323 (9.9%)					0.039
Main experiment: Treatment ^{\$}	119/801 (14.9%)	1.59 (1.00-2.52)	0.050	1.62 (1.02-2.58)	0.040	0.073
Sub-experiment: Control [§]	10/290 (3.4%)					
Sub-experiment: Treatment [#]	124/312 (39.7%)	18.47 (9.03-37.76)	0.000	20.09 (9.34-43.20)	0.000	0.131
<i>Subsample with health shock in last year, or HH head has “not good” health</i>						
Main experiment: Control	13/81 (16%)					
Main experiment: Treatment ^{\$}	40/229 (17.5%)	1.11 (0.55-2.25)	0.779	1.19 (0.57-2.49)	0.645	
Sub-experiment: Control [§]	5/79 (6.3%)					
Sub-experiment: Treatment [#]	33/86 (38.4%)	9.22 (3.33-25.47)	0.000	13.73 (4.22-44.66)	0.000	
<i>Subsample with HH head with at best some secondary education</i>						
Main experiment: Control	18/236 (7.6%)					
Main experiment: Treatment ^{\$}	90/651 (13.8%)	1.94 (1.06-3.55)	0.031	1.87 (1.04-3.36)	0.038	
Sub-experiment: Control [§]	7/247 (2.8%)					
Sub-experiment: Treatment [#]	99/248 (39.9%)	22.78 (10.35-50.14)	0.000	26.25 (11.69-58.98)	0.000	

^{\$}Intervention I. [§]Intervention II only. [#]Interventions II and III. *Standard errors are adjusted for clustering at the municipality level. [#]Adjusted effects control for differences between the two groups in baseline covariates listed in Table 1. [@]Intraclass correlation coefficient (ICC) calculated at municipality level.

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