Effects of a subsidized voluntary health insurance on insured and uninsured in Nigeria

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Abstract

Interventions aiming to simultaneously improve financial protection and quality of care might provide an important avenue towards universal health coverage (UHC). In this study we exploit panel data collected in 2009 and 2011 among 3509 randomly selected respondents in Kwara, Nigeria to estimate the effects of the Kwara State Health Insurance program on both the insured and uninsured. Within this program a subsidized voluntary low cost health insurance was offered by a private insurer and activities were undertaken to upgrade quality in selected health care facilities. Using propensity score matching we find that for the insured the program increased health care utilization and reduced out of pocket (OOP) expenditure. These improvements seem largely driven by the insurance. However, among the uninsured in the area with upgraded facilities, formal health care utilization decreased, informal health care utilization increased and OOP expenditures went up. These results suggest crowding-out of the uninsured from formal care facilities, which is problematic given that 67 percent of our sample did not take up the insurance in the initial two years of implementation. We conclude that implementing voluntary health insurance as a means towards UHC, warrants careful design of simultaneous supply side interventions to limit potential negative effects on those who do not enrol in the insurance. Further research is necessary to identify the processes driving the crowding-out of the uninsured.

Keywords

Voluntary health insurance, quality upgrade, impact evaluation, health care utilization, health care expenditure, negative effects.

Word count

5935

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[†] Prof. Dr. Joep Lange passed away on the 17th of July 2014 on board the MH17 flight to Melbourne.

Introduction

Interventions aiming to simultaneously improve financial protection and quality of care are scarce in low and middle income countries (LMIC). However, such combined interventions might provide an important avenue towards the achievement of Universal Health Coverage (UHC): provision of good quality care to everyone who needs it, without causing financial hardship (Open Working Group of the General Assembly, 2014). Many health care financing interventions in Sub-Saharan Africa (SSA) focus solely on the demand side, i.e. individuals or households, by providing insurance coverage (Giedion and Diaz, 2010) or on the supply side, i.e. health care providers, by trying to improve the quality of care through for example performance based financing (Witter et al., 2012). Such one-sided interventions run the risk of yielding limited results towards reaching UHC because they either provide the poor with access to low quality care or only the better off with access to good quality care.

The aim of this study is to estimate for both the insured and the uninsured the effects on health care utilization and expenditure from a combined demand and supply side program in Kwara State, Nigeria. The Kwara State Health Insurance (KSHI) program provides access to a heavily subsidized voluntary health insurance scheme offered by a private insurer and initiates quality upgrades in health care facilities. We first estimate the effects of the combined intervention on the insured (*total effect*). Subsequently we attempt to estimate the effect of the insurance independent of the quality upgrade on the insured (*insurance effect*) and we estimate the effect of the quality improvements on the *un*insured (*quality effect*). The latter two estimates provide insights into i) the relative impact of offering insurance compared to upgrading facilities and ii) the effects of the intervention on the uninsured. This is particularly relevant for recent discussions (see for example Van Doorslaer et al. (2014)) about how the poor can be reached most successfully through demand and/or supply side interventions.

This study adds to the existing body of knowledge from LMIC about the effect of programs combining demand and supply side interventions in the health care sector and more specifically about the effect of voluntary health insurance schemes on utilization and financial protection among both insured and uninsured. Without suggesting that we provide a complete overview, below we present a selection of relevant existing work.

Powell-Jackson et al. (2014) study in Ningxia province, China, the effects of a redesign of the rural insurance benefit package combined with the introduction of performance based financing among primary care providers. They find that the insurance intervention, in isolation, led to a 47 percent increase in the use of outpatient care and greater intensity of treatment. However, the two interventions in combination showed no effect on health care use above that generated by the redesign of the insurance benefit package. A second relevant study was performed in Thailand which implemented a major reform extending health insurance coverage and changing the organization and payment of public health care providers with the intention of raising cost-effectiveness. Limwattananon et al. (2015) show that this reform reduced out of pocket (OOP) expenditure by 28 percent and raised utilization of both inpatient and ambulatory care. King et al. (2009) found that Seguro Popular, a voluntary subsidized health insurance scheme in Mexico, reduced catastrophic expenditure but had no effect on health care utilization or health outcomes. Wagstaff et al. (2009) find increased health care utilization but also increased OOP spending after extension of insurance to the poor in China. The latter seemingly paradoxical finding can according to the authors be explained by the initiation of health care utilization because of the insurance, resulting in additional expenditures which are not covered by the insurance. Giedion and Diaz (2010) conclude based on a literature review that health insurance improves access and utilization and seems to improve financial protection. However, De Allegri et al. (2009) conclude, also based on a literature review, that health insurance schemes suffer from low enrolment i.e. rates between one and ten percent, apart from a few isolated successes.

The intervention in Kwara state, Nigeria

In 2006, the Dutch Health Insurance Fund (HIF) received a 100 million euro grant from the Dutch Ministry of Foreign affairs to develop, implement and evaluate, together with its implementing partner PharmAccess Foundation (PAF), health insurance programs in four African countries, including Nigeria (Health Insurance Fund, 2007). In 2009 the HIF and PAF introduced the KSHI program (formerly known as Hygeia Community Health Care) in the research area in Kwara state, together with the private partner Hygeia Nigeria Limited. Nigeria is a lower middle income country with a population of 179 million inhabitants, of which 46 percent live below the poverty line. Life expectancy lies at 52 years and the gross national income per capita is 2710 US dollars (World Bank, 2015).

Kwara state is located in the North Central geopolitical zone of Nigeria, bordering Benin and has a population of 2.5 million (Kwara State, 2014a). Similar to the rest of the country, Kwara State has a weak health system with inadequate government funding for health, weak governance and legislation, inadequate health infrastructure and poor service quality (Hendriks et al., 2014). The

KSHI program has received international recognition through among others the OECD Health Innovation prize, commendation from the United Nations Secretary General and the Bill & Melinda Gates Foundation for its creative approach to pro-poor health care delivery (Kwara State, 2014b). The voluntary subsidised health insurance is provided through a the private insurer Hygeia and a range of activities are undertaken to upgrade the quality in the two largest health care facilities in the research area, located in the Afon and Aboto Oja districts in Central Kwara (Hendriks et al., 2014).

Voluntary subsidised health insurance

The voluntary subsidised health insurance offered to individuals, covers a range of health care services, as shown in Table 1 (Gustafsson-Wright et al., 2013). The insurance generally covers preventive care as well as in- and outpatient care, in the intervention facilities. If necessary, referral to two tertiary care facilities in the state capital Ilorin is possible. The insurance does not cover high technology investigations (for example magnetic resonance imaging), major surgeries and complex eye surgeries, family planning commodities, treatment for substance abuse/addiction, cancer care requiring chemotherapy and radiation therapy, provision of spectacles, contact lenses and hearing aids, dental care, management of acute cardiovascular events other than admission to a hospital intensive care treatment and dialyses (Hendriks et al., 2014).

Table 1 Services covered by KSHI

Annual check-ups
Antenatal care and delivery
Eye examination and care
Health education
Hospital care and admissions (unlimited)
Inpatient care
Laboratory investigations and diagnostic tests
Minor and intermediate surgeries
Neonatal care
Outpatient care
Preventive care including immunization
Provision of prescribed drugs and pharmaceutical care
Radiological investigations
Screening for diseases including malaria and tuberculosis
Screening for sexually transmitted diseases
Specialist consultation
Testing and counseling for HIV

The insurance is offered to the population by trained agents and community leaders such as Emirs and village heads are involved in the rollout. The agents go door-to-door to explain the insurance scheme and offer those interested the opportunity to enrol. In addition large-scale communication activities are implemented in the target communities. This includes marketing for the program via billboards, comics, brochures, flyers and elaborate announcements and information sharing on the radio. Enrolment is possible with the agents and at dedicated kiosks in several urban centres and during several events organised for the community. After signing up there is a waiting time of minimal six days and maximal 36 days. It is not possible to sign up and immediately obtain coverage for any type of health care utilization. All households living in the intervention area are eligible for enrolment, without any pre-enrolment screening for chronic diseases (Hendriks et al., 2014). Table 2 displays information about the duration between enrolment in the health insurance and the first use of a health care service covered by the insurance. Hygeia & PharmAccess program data showed that the average duration between enrolment and first contact with a health care provider is at least half a year and 10 to 16 percent of the enrolees have their first visit within a month after enrolment, suggesting that most enrolees typically sign up several months before they actually use health care. The latter is to some extent re-assuring because the effect estimates in this study would be biased if enrolment in the insurance at the point of use would occur frequently.

The insurance policy covers a period of one year, after which enrolees need to re-enrol. Because the program is heavily subsidized by the Kwara State government (about 60 percent of the total subsidy) and the HIF, enrolees pay seven percent of the total premium which results in an annual self-paid premium of 300 Naira or approximately 2 US dollars per person per year (Gustafsson-Wright et al., 2013). This translates to 0.96 to 0.16 percent of the average annual per capita consumption for respectively the poorest and richest twenty percent of the target population at baseline (Hendriks et al., 2014). Enrolees do not incur OOP payments when using covered health care services at the upgraded health care facilities because these facilities receive direct payments from Hygeia (Gustafsson-Wright and Schellekens, 2013).

Table 2 Duration between enrolment and first use of health care services

Enrollment year	Average # days between enrolment and first use of covered health care	Percentage of enrollees with first use after enrolment within one month
2009	191	16
2010	230	10
2011	159	13

Calculations based on Hygeia - PharmAccess program data

Payment and quality upgrade of health care facilities

In addition to providing the health insurance, the two largest health care facilities in the intervention area are upgraded and a new payment mechanism is implemented. For the insured using primary care, payment from the insurer to the health care provider is based on a capitation fee per enrolee. Payment for other types of care is based on fee-for-service. The insurer has no say about the facilities' prices for the services used by the uninsured.

The quality improvement program consists of three components. First, grants are provided to upgrade the equipment in an intervention facility. Second, a baseline assessment in the facility is conducted and a quality improvement plan is formulated and follow-up visits are planned. Examples of quality improvement interventions include implementation of treatment guidelines, upgrading of laboratory equipment, assurance of continuous essential drug supplies, adequate medical file keeping, waste management protocols and hospital infection control protocols. Third, health care staff at the intervention facility receive relevant training.

Methods

Data

Data were collected among a randomly selected sample of households in the intervention area (Afon and Aboto Oja districts, Kwara state) and in the control area (Ajasse Ipo district, Kwara state) (Hendriks et al., 2014). The control district was selected on the basis of its similarity with the intervention area characteristics in terms of language, main economic activities, income levels, urban/rural composition and population size. At baseline, limited availability and quality of health care services was found in all districts. In both the intervention and control area, there were few functional health care facilities before the implementation of the combined program. An assessment was performed across all facilities in both areas showing that most were poorly maintained, essential equipment was lacking and patient numbers were low. The intervention area included three public and three private facilities from which the private Ilera Layo clinic in Aboto Oja and the public General Hospital Afon in Afon were selected to participate in the program. The control area included three public and two private facilities (Hendriks et al., 2014).

A stratified two-stage random sample was drawn in 2009 in the intervention and control areas. All households located in the study areas were eligible for inclusion. The first stage consisted of a random selection of 100 out of 300 enumeration areas (EAs) from the 2005 National Population Census. These 300 EAs were located within 15 kilometres distance from the towns Afon, Aboto Oja and Ajasse Ipo, the capitals of the intervention and control districts. Subsequently a local census was performed to list all households in these 100 EAs. In the second stage, households were randomly sampled from this list to take part in the survey. A number of replacement households were sampled within each EA, in anticipation of household migration in the period between the local census and the baseline survey. The baseline data were collected in May and June 2009 and the second wave was collected in the same months in 2011, limiting a potential seasonal bias. The survey was extensively piloted and local staff was recruited and trained to administer the survey[1] (Hendriks et al., 2014). The KSHI program was launched shortly after the baseline survey was finished.

The resulting balanced panel, for which observations from both 2009 and 2011 were available, contains 2191 observations from the intervention and 1318 from the control area. Due to migration, refusal, death and other reasons the number of observations decreased between the

[1] Ethical clearance was obtained from the Ethical Review Committee of the University of Ilorin Teaching Hospital. Informed consent was obtained from all participants by signature or by fingerprint.

baseline and the second wave, resulting in an attrition rate of 4.9 percent in the intervention group and 3.0 percent in the control group.

Statistical analysis

Using these panel data and the differences in exposure between intervention and control groups, we estimate three effects: i) the *total effect* of the combined intervention on the population in the intervention area, ii) the *insurance effect* on the population in the intervention area and iii) the *quality effect* on the uninsured in the intervention area. Because enrolment in the health insurance is voluntary, a simple comparison in outcomes between enrolees and non-enrolees provides biased estimates because the decision to enrol is likely to be driven by factors that also correlate with health and health care use. We therefore apply propensity score matching (PSM) to construct the relevant samples of control observations that are similar to the treated in terms of observable characteristics (Rosenbaum and Rubin, 1983).

Table 3 identifies the treatment and control group for the estimation of the three different effects. Those living in the control area, where no insurance or facility upgrade was offered, serve as a potential match for selected observations from the intervention area. Being "insured" is defined as an individual (or the most knowledgeable household member on his/her behalf) reporting to have a health insurance at the moment of the second wave of the survey (May-June 2011). The data does not allow us to determine when an insured individual took up this insurance with the KSHI program.

First, the total effect is estimated by comparing the insured in the intervention area to the matched controls with a high probability of taking up the insurance had it been offered in the control area. Second, the insurance effect is based on a comparison between the insured and the uninsured in the intervention area, which both profit from the facility upgrades but differ in their enrolment status. This allows us to estimate the insurance effect for those who also experienced the facility upgrades and givens an idea of the relative importance of the insurance effect in the total effect. Finally, the quality effect is estimated by comparing the uninsured in the intervention area, who profit from the facility upgrades but not from the insurance, to the matched controls with a low probability of taking up the insurance had it been offered. This allows us to estimate the effects of the program on those who did not take up the insurance. While these three estimates are highly policy relevant, we cannot disentangle the total effect into a pure insurance uptake or facility upgrade effect as would have been the case when both parts of the intervention had been implemented in different areas.

Table 3 Identification of treatment and control groups

	Intervention area		Control area	
	Insured	Uninsured	Matched to insured	Matched to uninsured
Total effect	T		C	
Insurance effect	T	C		
Quality effect		T		C

Note: T = treatment group, C = control group

The outcome measures used in this study are shown in Table 4. We first estimate the total, insurance and quality effect on the use of *any* health care in the past year and subsequently differentiate between the use of any formal and informal health care. Formal care refers to care provided in a hospital, clinic, (primary) health centre or by a private doctor, nurse, midwife or paramedic. Informal care includes care provided by a traditional healer, pharmacist, patent medicine vendor, alternative medicine provider or religious person. We then use a different part of the questionnaire where for every respondent with self-reported need, the use of health care is differentiated between chronic, acute, hospital and other health care. Finally we study the effects on per capita health expenditure in the past year in Naira divided by one thousand (1000 Naira is about 5.30 US $).

To obtain propensity scores for each respondent, we estimate a probit model for the decision to enrol in the insurance on all baseline covariates shown in Table 5. The covariates in this model include the standard age & gender, education, marital status, urbanicity, wealth, employment and some (self-assessed) health variables. We also include covariates reflecting the distance between the household and the nearest upgraded facility. In the control area this is the “to be upgraded facility” which is a comparable facility which would be upgraded if the program would be rolled out to the control area in the future. A variable reflecting whether a respondent reported an acute illness or injury between the two waves was included to control for some of the potential bias arising from respondents who took up the insurance because of health problems arising after the baseline survey. We also include variables reflecting determinants of the willingness to enrol in a health insurance which often remain unobserved: whether someone states to be interested in a health insurance and the four personality dimensions i) extraversion, ii) conscientiousness, iii) emotionally stability and iv) openness to experience, ranging from zero to fifty (Norman, 1963). Finally we include variables reflecting the outcome measures at baseline to control for different starting levels of health care utilization and health care expenditure.

This probit model is estimated on the observations in the intervention area, reflecting the fact that they can make an actual decision to enrol, as opposed to the control area where the insurance is not offered. The parameters are subsequently extrapolated to those from the control area to estimate propensity scores for all respondents. The probit model underlying the estimation of the quality effect has to reflect the decision to *not* enrol because this effect is estimated on the *un*insured. This is the same probit model but with all coefficient signs reversed. For ease of interpretation, we report average marginal effects as opposed to coefficients.

The average treatment effect on the treated can be written as (Khandker et al., 2010):

$$ATT_{PSM} = \frac{1}{N_T}\left[\sum_{t \in T} Y_t - \sum_{m \in M} w(t,m) Y_m\right] \qquad (1)$$

where N_T is the number of treated t and $w(t,m)$ is the weight used for control observation m when comparing with treated observation t. Effects are estimated based on the observed values for the outcome measures from the second wave while baseline values of these outcomes are included in the decision to enrol probit model[2].

[2] As a sensitivity check we also estimated the three different effects using a difference in differences approach. We excluded all baseline values of the outcome variables from the decision to enrol probit model and studied changes in outcomes between 2009 and 2011 as opposed to outcome levels in 2011. This led to qualitatively the same conclusions.

Table 4 Means for outcome measures

	Insured 2009	Uninsured 2009	Unmatched controls 2009	Insured 2011	Uninsured 2011	Unmatched controls 2011
Any care in past year	0.257	0.233	0.362	0.465	0.307	0.322
Any formal care in past year	0.214	0.164	0.300	0.422	0.201	0.212
Any informal care in past year	0.042	0.070	0.061	0.044	0.105	0.110
Any chronic care in past year	0.068	0.056	0.069	0.155	0.080	0.070
Any acute care in past year	0.154	0.157	0.236	0.317	0.235	0.247
Any hospital care in past year	0.018	0.020	0.025	0.056	0.032	0.033
Any other care in past year	0.102	0.087	0.178	0.073	0.022	0.049
Per capita health expenditure in past year (naira/1000)	2.088	1.780	1.820	1.165	1.181	2.323

Table 5 Means for covariates and estimates decision-to-enroll probit model

	Insured 2009	Uninsured 2009	Unmatched controls 2009	Av. marg. effect	Coeff.	p-value
Male 0-18 years	0.205	0.215	0.248	0.016	0.047	0.624
Male >18 years	0.274	0.309	0.241	-0.022	-0.064	0.693
Female >18 years	0.362	0.298	0.295	0.061	0.174	0.267
Primary education	0.109	0.119	0.112	-0.070**	-0.198	0.040
Married	0.544	0.491	0.440	0.078**	0.222	0.027
Urban	0.636	0.429	0.524	-0.123***	-0.348	0.009
0 to 1 km to (to be) upgraded facility	0.302	0.142	0.131	0.0146***	0.414	0.000
2 to 3 km to (to be) upgraded facility	0.007	0.003	0.001	0.165	0.470	0.293
3 to 4 km to (to be) upgraded facility	0.062	0.091	0.068	-0.213***	-0.603	0.000
4 or more km to (to be) upgraded facility	0.224	0.433	0.417	-0.245***	-0.694	0.000
Lowest wealth tertile	0.255	0.422	0.212	-0.176***	-0.499	0.000
Middle wealth tertile	0.389	0.363	0.382	-0.096***	-0.273	0.000
Household has savings	0.186	0.159	0.150	-0.028	-0.078	0.375
Job in services	0.134	0.122	0.158	-0.046	-0.129	0.342
Job in trade	0.219	0.171	0.149	-0.018	-0.050	0.711
Job in agriculture	0.190	0.229	0.149	0.014	0.040	0.756
Not working	0.293	0.321	0.386	-0.028	-0.080	0.384
Good self-assessed health	0.937	0.936	0.925	0.036	0.102	0.419
Normal BMI (20-25)	0.285	0.283	0.266	-0.004	-0.011	0.876
Got acute illness or injury between waves	0.398	0.316	0.331	0.081***	0.231	0.000
Interested in health insurance	0.553	0.525	0.467	-0.034	-0.097	0.352
Extravert personality	25.530	25.499	24.951	-0.010***	-0.030	0.001
Conscientious personality	37.553	37.088	36.807	0.001	0.003	0.693
Emotionally stable personality	29.188	28.675	28.533	0.006**	0.017	0.040
Personality open to experience	33.805	33.622	33.597	-0.006*	-0.016	0.078
Any care in past year (baseline)	0.257	0.233	0.362	-0.036	-0.103	0.597
Any formal care in past year (baseline)	0.214	0.164	0.300	0.189***	0.537	0.001
Any chronic care in past year (baseline)	0.068	0.056	0.069	-0.007	-0.021	0.900
Any acute care in past year (baseline)	0.154	0.157	0.236	-0.074	-0.209	0.165
Any hospital care in past year (baseline)	0.018	0.020	0.025	-0.076	-0.217	0.331
Any other care in past year (baseline)	0.102	0.087	0.178	-0.096*	-0.274	0.061
Per capita health exp (baseline)	2.088	1.780	1.820	0.004	0.010	0.191
N				*2191*		

Notes: The probit model uses the listed covariates as collected in 2009, so means for 2011 are not reported. Some covariates were not collected in 2011.
* p<0.1; ** p<0.05; *** p<0.01

Having obtained the propensity score for all treatment and control observations, we define the three relevant samples to estimate the total, insurance and quality effect as shown in Table 3. For each of the three samples we apply Kernel matching[3] to match the treated to comparable controls on the basis of the propensity scores. Kernel matching uses a weighted average of all controls to construct a hypothetical match for each enrolee. The weights are determined by the distance to the propensity score of the enrolee: closer controls obtain a larger weight (Khandker et al., 2010). The precise nature of the weighting is determined by the form of the kernel and the bandwidth, which we set at 0.06 (Mensah et al., 2010). To check sensitivity of the results we also apply the three other often used matching methods: nearest neighbour with replacement (NN w. rep.), nearest neighbour without replacement (NN w/o rep.) and radius matching (Khandker et al., 2010). The first and second method match each treated individual to the control observation with the closest propensity score, which means that the difference in propensity scores between the

[3] We use the routine “psmatch2” in STATA 13 by Leuven and Sianesi (2014).

treated and the matched control can still be very large. To avoid this, radius matching applies a maximum propensity score distance (caliper), which we set at 0.02, in line with Mensah et al. (2010). For the sake of parsimony we present results from Kernel matching only when the other matching methods lead to qualitatively the same conclusions. The results from the other matching methods are available upon request from the authors.

We conduct a balancing test to check whether after matching, the mean for each explanatory variable across treatment and control group, does not significantly differ. This ensures that the treated and the matched controls are balanced in that similar propensity scores are based on similar explanatory variables (Becker and Ichino, 2002). Using the STATA user-written command –pscore- we find that the balancing property is satisfied when studying the insurance effect (see Table 3) on the treated and controls in the intervention area. While for the estimation of the total effect balance is achieved on the majority of the explanatory variables, this is not the case for "3 to 4 km to (to be) upgraded facility", "Married" and "Emotionally stable personality". For the quality effect balance is also achieved for most explanatory variables, apart from "Good self-assessed health", "Household has savings", "Not working" and "Lowest wealth tertile". This partial imbalance relates to the fact that the probit model reflecting the decision to enrol can only be estimated on the sample where the decision was actually made by respondents (the intervention area i.e. the sample used to estimate the insurance effect) and not on a sample including respondents that could not choose for an insurance (control area, used to estimate the total and quality effect).

The validity of this PSM approach depends on two conditions: i) conditional independence and ii) sizeable common support or overlap in propensity scores across the treated and the matched control sample (Khandker et al., 2010). The first, requiring no unobserved characteristics to affect the decision to enrol cannot be tested, but matching on demographics, (changes in) health status, distance to health care facilities and personality traits should eliminate most of the important drivers of selection bias, though some bias due to unobserved differences is likely to remain. However, given that we match at baseline outcomes, this bias should only result from time varying unobservables. We ensure validity of PSM in relation to the second condition by only considering observations on the common support of the propensity scores across the treated and the matched controls.[4] Figures 1-3 show the density graph for the samples underlying the estimates of the total, insurance and quality effect.

Results

In our sample, 33 percent of the respondents was enrolled in the KSHI program. The probability to enrol in the insurance scheme in our sample is smaller for those with primary education, reporting to live in an urban area, living further away from a (to be) upgraded facility (>3 km), in a lower wealth tertile, with an extravert personality and who used other care, for example preventive care, in the past year at baseline (Table 5). The probability to enrol correlates positively with being married, a reported acute illness or injury between the two waves of data collection (2009-2011), having an emotionally stable personality and the use of any formal care in the past year at baseline. This suggests some over representation of less healthy individuals in the program.

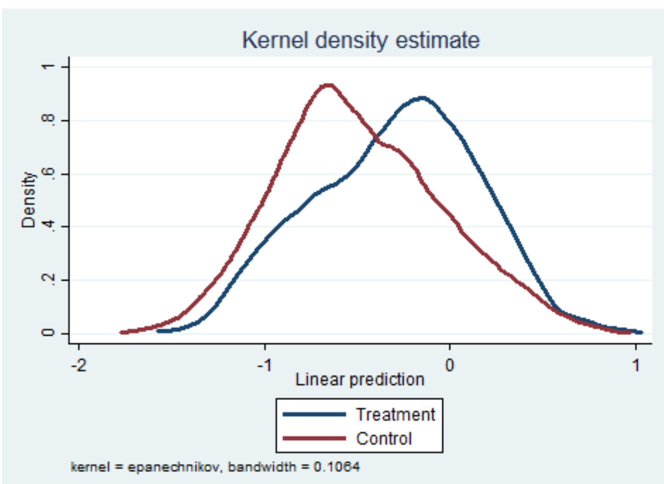

Figure 1 Total effect, kernel density estimate for treatment and control observations

[4] Excluding observations off the common support is possible given our relatively large number of observations. Less than 0.5 percent of our samples is off support. Following Leuven and Sianesi (2014), any observations with a propensity score higher than the maximum or lower than the minimum score of the controls are dropped.

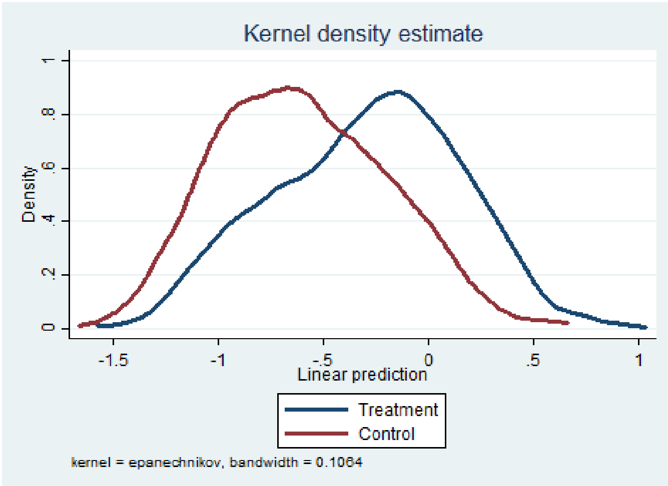

Figure 2 Insurance effect, kernel density estimate for treatment and control observations

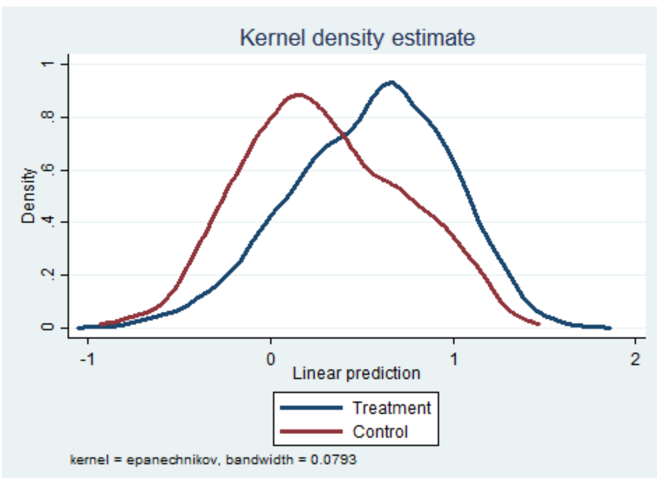

Figure 3 Quality effect, kernel density estimate for treatment and control observations

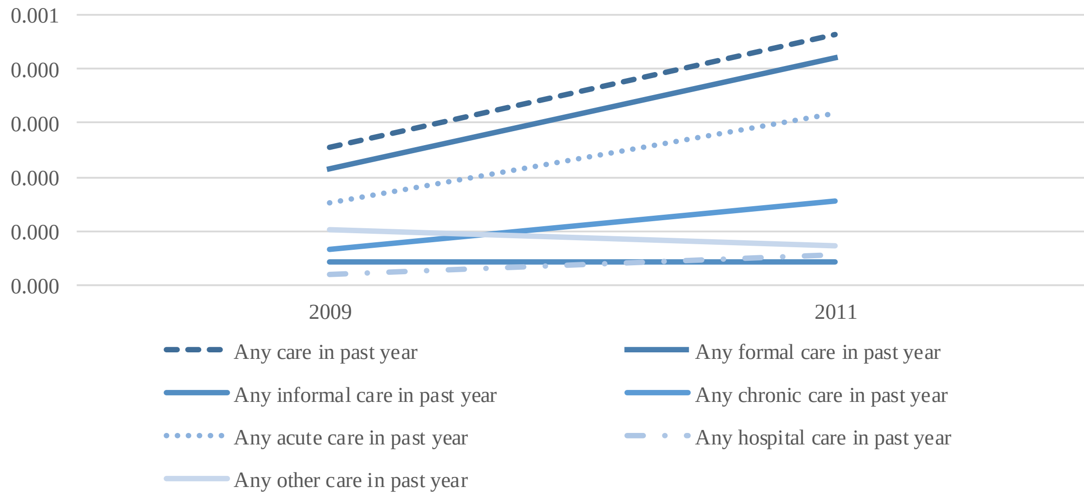

Figure 4 Changes in outcomes between baseline and second wave among insured

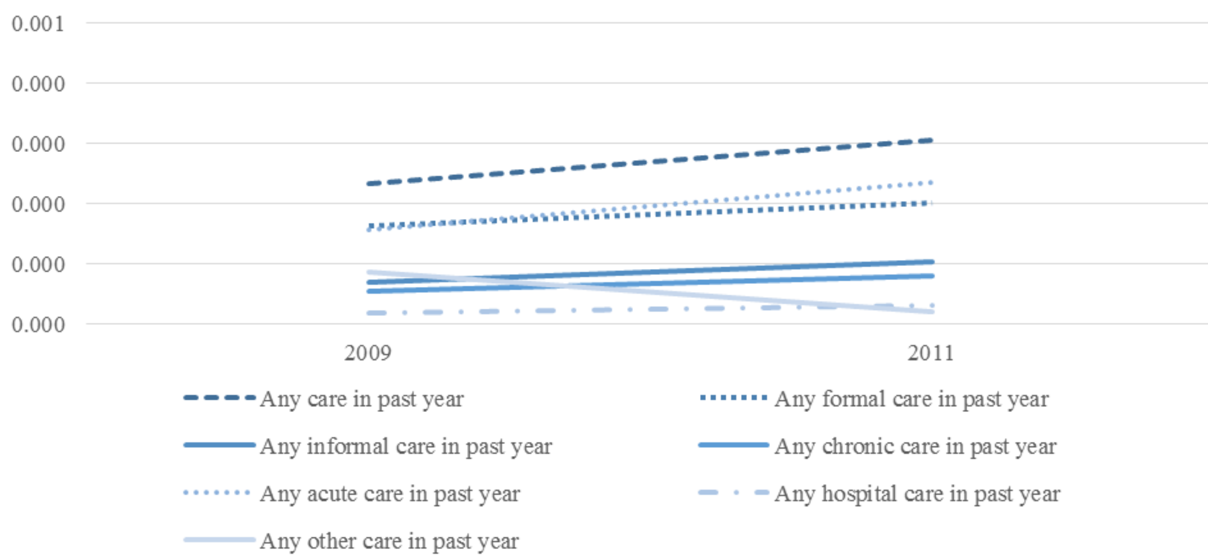

Figure 5 Changes in outcomes between baseline and second wave among uninsured

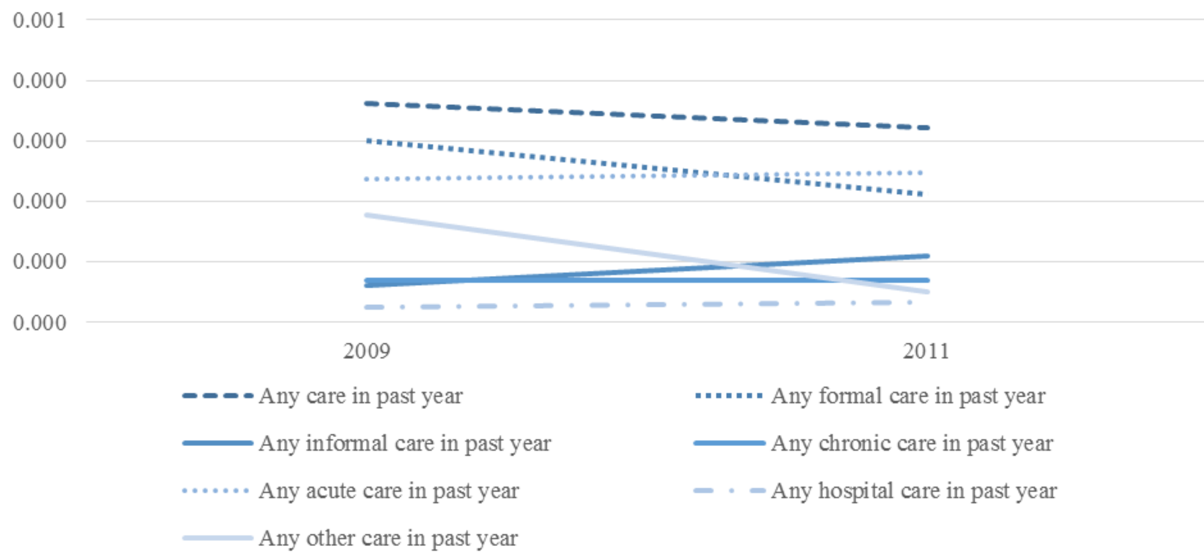

Figure 6 Changes in outcomes between baseline and second wave among unmatched controls

Table 4 shows the averages for the outcome measures in 2009 and 2011, before matching. For ease of interpretation this information has also been represented graphically in Figures 4-6. Table 5 shows means for the covariates at baseline before matching. Table 6 shows the effect estimates after using Kernel matching[5]. Note that the estimate of the insurance and the quality effect does not sum to the total effect because the first is calculated among the insured within the intervention area while the latter is calculated using the sample of uninsured and matched controls. Those who took up the insurance *and* benefited from the facility upgrades because they lived in the intervention area show a significant increase in the use of any care of 9 percentage points (pp) on average (Total effect, column 2), when compared to matched controls from the area where facilities were not upgraded. This increase was driven by an increase in formal health care use and a simultaneous smaller decrease in informal health care use. The use of any chronic care also significantly increased, with 8 pp. A small increase in hospital care use (2 pp) was also observed for the insured in the intervention area. Health care expenditure reduced significantly with on average 1315 Naira per capita (6.95 US $) at an annual basis, which is a relatively large (i.e. a 63 percent) reduction compared to 2088 Naira per capita at baseline.

When comparing those with insurance to those without, but all living in the area where facilities were upgraded, we estimate insurance effects similar to the total effect, though in most cases slightly smaller. However, contrary to the total effect, the insurance effect suggests a significant

[5] Results NN w. rep., NN w/o rep. and radius available upon request from the authors.

increase in the use of any other care (5 pp) but no significant reduction in health care expenditure. The latter is due to a decrease in expenditure in the treatment area for both insured and uninsured, though smaller for the latter (see Table 4).

The quality effect, assessed as the difference between the uninsured in the intervention area and the matched controls, shows a relatively large and significant average fall in almost all types of health care utilization[6]. Only the use of informal care increases significantly with 7 pp, suggesting that the uninsured in the intervention area moved away from formal care. Although we cannot directly compare the size of the effects, the reductions in formal health care utilization among the uninsured in the intervention area seem particularly large when compared to the increases resulting from the total and insurance effect. These negative effects on the uninsured are not in line with our initial expectations and suggest that the supply side intervention to improve the quality of facilities, without simultaneous take up of health insurance, has not raised but lowered formal health care utilization among the uninsured. We also observe a significant increase in health care expenditure in this group, which seems to be driven by an increase in expenditure among the matched controls (see Table 4). The latter suggests that the reduction in health care use among the uninsured is not so much driven by a possible price increase but by a crowding out effect of the uninsured. This negative effect is of importance given that 67 percent of our sample did not take up the insurance.

[6] We have performed several sensitivity checks and found in all cases negative effects on health care utilization when comparing uninsured to matched controls. We first checked the quality effect when limiting the sample to those living close to a health care facility (<3 km), we then trimmed the sample to only exclude the 10 percent observations with the least common support and we finally estimated the effect only on (would be) upgraded health care facilities as opposed to all health care facilities. All estimations provided qualitatively the same conclusion: the facility upgrades reduced health care utilization among the uninsured.

Table 6 Effect estimates using Kernel matching

	Total effect		Insurance effect		Quality effect	
	Kernel	p-value	Kernel	p-value	Kernel	p-value
Any care in past year	0.094***	0.000	0.089***	0.000	-0.113***	0.000
Any formal care in past year	0.170***	0.000	0.164***	0.000	-0.182***	0.000
Any informal care in past year	-0.076***	0.000	-0.075***	0.000	0.069***	0.000
Any chronic care in past year	0.078***	0.000	0.059***	0.000	-0.068***	0.000
Any acute care in past year	0.021	0.340	0.021	0.354	-0.034	0.135
Any hospital care in past year	0.018*	0.087	0.022**	0.035	-0.033***	0.003
Any other care in past year	0.015	0.209	0.048***	0.000	-0.042***	0.000
Per capita health expenditure in past year (naira/1000)	-1.315***	0.000	-0.133	0.418	0.660***	0.000

Note: * p<0.1; ** p<0.05; *** p<0.01

To check whether the above results were indeed driven by the studied intervention, we estimate the effects for health care utilization in the upgraded facilities only. These effects can be estimated for the outcome measures where specific information is available about the facility where the respondent used care (any chronic, acute, hospital or other care in the past year). Table 7 suggests that the earlier findings are indeed driven by the KSHI program because the earlier significant increases in health care utilization, when estimating the total and the insurance effect, as well as the significant decrease in utilization resulting from the quality effect are confirmed based on observations from the upgraded facilities only.

Table 7 Effect estimates for health care utilization in upgraded facilities using Kernel matching

	Total effect		Insurance effect		Quality effect	
	Kernel	p-value	Kernel	p-value	Kernel	p-value
Any chronic care in past year	0.125***	0.000	0.093***	0.000	-0.107***	0.000
Any acute care in past year	0.205***	0.000	0.150***	0.000	-0.155***	0.000
Any hospital care in past year	0.030***	0.000	0.024***	0.000	-0.032***	0.000
Any other care in past year	0.051***	0.000	0.050***	0.000	-0.050***	0.000

Note: * p<0.10; ** p<0.05; *** p<0.01

Discussion and limitations

The aim of this study is to estimate the effects of the Kwara State Health Insurance (KSHI) program on both the insured and the uninsured in Kwara State, Nigeria. Through this program the Health Insurance Fund and the private insurer Hygeia provide access to a subsidized voluntary health insurance scheme and the PharmAccess Foundation (PAF) initiates quality upgrades in selected health care facilities. These facilities receive a combination of fee-for-service and capitation based payments and a quality improvement program consisting of three components. First, grants are provided to upgrade the equipment in intervention facilities. Second, a baseline quality assessment in the facility is conducted, an improvement plan is formulated and follow-up visits are planned. Third, health care staff receive relevant training.

Using panel data collected in 2009 and 2011 among 3509 randomly selected respondents from the intervention area and a comparable control area, we estimate three different effects of this combined demand and supply side program. We use the differences in exposure between the intervention and control groups, with individuals in the first group exposed to facility upgrades and the offer to enrol in a subsidized voluntary health insurance and individuals in the control area for which both were not available. We estimate i) the *total effect* of the combined intervention on the population in the intervention area, ii) the *insurance effect* on the population in the intervention area and iii) the *quality effect* on the uninsured in the intervention area. To limit the bias arising from heterogeneity across those who do and do not decide to enrol in the insurance, we apply propensity score matching to construct three samples of control observations that are similar to the treated in terms of observable characteristics. The total effect is estimated by comparing the insured in the intervention area to the matched controls with a high probability of taking up the insurance had it been offered in the control area. The insurance effect is based on a comparison between the insured and the uninsured in the intervention area, which both profit from the facility upgrades but differ in their enrolment status. This allows us to estimate the insurance effect for those who also experienced the facility upgrades and givens an idea of the relative importance of the insurance effect in the total effect. Finally, the quality effect is estimated by comparing the uninsured in the intervention area, who profit from the facility upgrades but not from the insurance, to the matched individuals in the control group with a low probability of taking up the insurance had it been offered. This allows us to estimate the effects of the program on those who did not take up the insurance. Given the setup of this study we cannot disentangle the total effect into pure insurance and facility upgrade effects as would have been the case if both parts of the intervention had been implemented in different areas. Another limitation derives from the fact that we only know the reported enrolment status at the moment of the survey (May-June 2011). Enrolment into the KSHI program is for a period of twelve months and can be initiated throughout the year. This can lead to an over- or underestimation of our treatment effects depending on whether the enrolment at the time of survey over- or underestimates the enrolment in the period twelve months before the survey.

We found that the total intervention improved health care utilization and decreased out of pocket (OOP) expenditure among the insured. These improvements seem mainly driven by the health insurance, because the size and direction of the insurance effects are similar to the total effects. However, we can only estimate the insurance effect among those who also experienced the facility upgrades. This implies that we do not know what the effect of the insurance would have been without the facility upgrades. It is possible that the effect of the insurance alone would have been smaller had no facility upgrades been implemented. We subsequently estimated the effect of the quality improvements on the uninsured, which make up 67 percent of the study sample. We found that among the uninsured living in the intervention area, formal health care utilization decreased, informal care utilization increased and OOP expenditures went up. These effects were relatively large and not necessarily in line with initial expectations of such a program aiming to improve access to formal care and financial protection. Our findings suggest crowding-out of the uninsured from formal care facilities. Our data did not allow identification of the practical routes of crowding-out but we can provide some preliminary suggestions. First, given that the providers received fee-for-service payments for a range of services, providers might have had less financial incentives to care for to the uninsured. However, we do not know how the OOP payments received from uninsured compare to the fee for service payments received for the insured when providing these services, this requires further investigation. Also, further research in which a differentiation is made between the effects on primary care (payment through a capitation fee per enrolee) and on the other types of care (fee-for-service) would allow to draw additional conclusions about the effect of the payment method on the behaviour of health care providers and the associated potential crowding-out of uninsured. Second, the upgrade of the facilities together with the provision of health insurance may have created (longer) queues which were particularly discouraging to the uninsured. In fact, the increase in patients coming to the facilities was described as "enormous", although it was unclear whether only the insured or also the uninsured were in these queues (Hendriks, 2014). Third, the perception towards the upgraded facilities may have changed, reducing the use of these facilities by the uninsured. Prices may have increased for those paying out of pocket because health care facilities were free to set their prices. However, we do not have the necessary information about prices for health care services to test this latter hypothesis. A more methodological explanation for the negative estimates for the quality effect relates to fact that we could only study the effects of the facility upgrade among the uninsured. To the extent that our matching exercise does not rule out differences in unobservables that correlate with not taking up insurance and decreases in health care use over time, our estimated quality effects could be biased downward. However, the negative quality effects were particularly large compared to the total and insurance effect, suggestion that even with some downward bias due to unobservables our conclusion remain qualitatively the same. Further research - for example through interviews with potential health care users, especially those who did not take up the insurance - is necessary to fully understand the processes driving the crowding-out of the uninsured.

Hygeia – PharmAccess program data showed that in March 2015 the total number of current enrolees was 16,220 though the exact target population is not defined. The program evaluated in this study is currently being developed into a state-wide scheme, aiming to expand the program to cover 600,000 people within the next five years. The program would then reach 60 percent of the rural population in Kwara State. Currently the program has been expanded to 26 health care providers (April 2015) and the State's government has agreed to increasingly contribute to the payment of the premium subsidy for low-income individuals and to invest in health care infrastructure. Further research is necessary to determine whether the geographical expansion of the program, combined with the aim to further increase the enrolment rate, reduces the negative effects of the program on the uninsured.

This study showed that when implementing or potentially expanding voluntary health insurance as a means towards UHC, careful design of supply side interventions is warranted to limit potential negative effects on those who did not enrol in the insurance. Given that we found that the uninsured are those with on average lower levels of income, imperfectly designed interventions may increase utilization and financial protection among the insured but in turn widen overall socioeconomic inequalities.

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