The Demand for Health Insurance in a Poor Economy: Evidence from Burkina Faso

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I. Introduction

Illness is one of the most frequent economic shocks to households in low- and middle-income countries. Apart from an immediate deprivation in well-being, health shocks cause indirect costs by preventing individuals from engaging in income-earning activities and triggering high out-of-pocket expenditures for medical care at the same time. Therefore, health shocks constitute a severe, unpredictable economic risk that threatens households' short- and long-term consumption. Relative to informal arrangements, formal insurance schemes have the potential to offer better financial protection by providing more efficient risk pooling and by avoiding the enforcement and commitment problems of informal risk-sharing networks. Through less healthy working and living conditions, the poor are especially exposed to the risk of ill health while having little access to formal insurance. One popular way to provide access to health insurance in low- and middle-income countries has been voluntary social health insurance. This model is similar to private health insurance but typically features subsidization by the state and simpler procedures, often with a uniform premium independent of an insuree's observable characteristics. As for other forms of microinsurance, such as index-based crop insurance for farmers,

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take-up rates consistently fall short of policy targets (Eling, Pradhan, and Schmit 2014).

In this paper, we study the demand for voluntary health insurance in the context of a pilot health insurance program introduced in one administrative department of Burkina Faso. While all households were offered individual-level health insurance policies, a 50% subsidy on the individual premium was offered to the poorest quintile of households in each village and urban neighborhood. A household's poverty status was determined through community wealth rankings. Exploiting the community wealth rankings, we develop an original fuzzy regression discontinuity design, which elicits insurance demand elasticities for households at the poverty threshold. The richness of our data, which includes a demographic and an economic census, allows us to identify heterogeneous effects of pricing across subpopulations and to distinguish between household-level and individual-level enrollment. Moreover, by correlating the heterogeneous treatment effects with economic and demographic characteristics across population subgroups, we identify some key predictors of demand levels and elasticities in a poor economy. Finally, with data on insurance claims and health-facility utilization, we test for adverse selection into health insurance by assessing whether insurees' unobserved risk, which we measure by the value of insurance claims, correlates with the exogenously varied insurance price.

Our findings are as follows. Subsidization has moderate absolute but large relative effects on enrollment, with important differences between rural and urban sectors. Urban households around the eligibility threshold, which are moderately poor by international standards, more than triple enrollment, from initially 10% to 38%, when offered a 50% discount, and individual enrollment increases from 7% to 23%. For the urban areas, the targeted vouchers of our intervention increase equity in access to health insurance along the wealth distribution as enrollment in the poorest quintile ties with enrollment in the upper four quintiles. In contrast, subsidization is ineffective for rural households at the eligibility threshold, which are ultrapoor by international standards. Regarding selection into insurance, we find that subsidization disproportionately attracts households whose head is widowed and male. There are no gender disparities caused by subsidization. Instead, individuals who get additionally enrolled are primarily elderly. Coverage among individuals aged above 55 increases drastically, from 7% to almost 50%, while the increase is only 14% for prime-aged adults and close to zero for children. This pattern is driven by the heterogeneous extensive margin effects, as widower-headed households have fewer children and more elderly in our study context.

In contrast to these important enrollment effects, we find only small and statistically insignificant negative differences in health-care facility utilization and indemnification payments between unsubsidized and subsidized insured individuals. While the signs of our point estimates are opposite to classical adverse selection—that is, a lower premium attracts somewhat greater risks—these effects are small and statistically far from significant.

While our research design contains no experimental variation in dimensions other than insurance pricing, we attempt to identify the causes of the lack of demand as well as the channels driving heterogeneity in demand elasticities across different types of households. An important driver of the lack of demand that our data suggest is a mismatch between the price of the health insurance policy, on the one hand, and poor households' desired health budgets, on the other. According to household survey data collected prior to our subsidization campaign, urban households around the voucher eligibility threshold have medical expenses equal to only about one-half the regular insurance premium, while this figure equals only one-quarter among rural households. As a consequence, the subsidized insurance premium roughly matches poor urban households' desired medical expenses, while rural households' revealed preference for medical budgets falls far short of even the subsidized premium. Second, the survey data suggest that informal risk sharing between households is intense. Informal receipts from friends and relatives are a multiple of households' health expenditures in rural and urban sectors alike. Given that health shocks are largely idiosyncratic, actuarially fair health insurance has little scope in this environment (as in Arnott and Stiglitz 1991). Instead, subsidization primarily serves to overcome the mismatch between households' desired health budgets and the actuarially fair price of the insurance policy's generous benefits package.

Our results regarding heterogeneous demand elasticities are consistent with these two observations. According to our survey data, households headed by a widower, whose price elasticity is a multiple of that of other types of households, have considerably larger health budgets prior to our intervention and are less well informally insured. For rural households, we also explore additional economic and noneconomic factors, such as exposure to health shocks, demographic structure, education, and remoteness. However, we discard these as predictors of the lack of rural demand.

Our study contributes to a recent literature on microinsurance in developing countries, in which two themes have been most prominent: first, the properties of insurance demand and, second, the effects of formal insurance on production and health outcomes. The two types of insurance that have by far received most attention are index-based crop insurance and health insurance. Regarding the former, there have been important recent contributions from Giné, Townsend, and Vickery (2008), Cole et al. (2013), and Karlan et al. (2014), who find take-up rates of less than 20% for crop insurance at actuarially fair prices in India and Ghana, respectively, but sizable price elasticities of demand. Consistent with the pattern that idiosyncratic health shocks are better insured by informal risk-sharing networks than covariate weather shocks to agricultural production, health insurance take-up and the potential of subsidization have been found to be even lower in several middle-income countries (Thornton et al. 2010; Capuno et al. 2016; Levine, Polimeni, and Ramage 2016; Wagstaff et al. 2016).

We make the following contributions to this literature. First, we study health insurance demand specifically among the poorest villagers and urban dwellers, who notoriously fail to be reached by social protection schemes in sub-Saharan Africa and elsewhere. We thereby fill a gap between the current state of academic research on the demand for micro health insurance, which is almost exclusively concerned with average demand elasticities, and demands by development practitioners and policy makers, who prioritize providing social protection to the poorest.[1] Of course, this focus of our research design is also a limitation, as it does not identify demand elasticities among wealthier households. Second, the design of our insurance policy, which is sold to individuals, combined with a detailed census of the study population allows us to identify distributional effects of insurance pricing on enrollment. In particular, we are able to disentangle household and individual enrollment as well as insurance coverage within households across different age and sex groups. Our third innovation is that we identify heterogeneous demand elasticities among different subgroups of households as well as predictors of health insurance demand from extensive additional survey data. Finally, to the best of our knowledge, all existing studies of micro insurance pricing are located in middle-income countries of Asia and Latin America and limited to specific subpopulations, most frequently informal workers. In contrast, Burkina Faso is one of the world's poorest countries, and the insurance product is offered to the universe of households in the study area. Hence, our results give a comprehensive picture of the potential of voluntary health insurance in a poor sub-Saharan African context, including the issue of adverse selection.

[1] To our knowledge, Capuno et al. (2016) provide the only exception to the literature, reporting demand effects by wealth quintile in response to a combined information and subsidization intervention in the Philippines.

A recent Africa Social Protection Policy Brief by the World Bank (2012) points out that "achieving high rates of insurance coverage among poor populations will require subsidizing or even entirely covering the cost of premiums for these households in the short term. Determining which groups to subsidize, to what extent, and for how long is ultimately a political question but needs to be informed by a sound understanding of poor populations, including their location and employment status."

The remainder of this paper is structured as follows. Section II describes the insurance product and subsidization intervention. We introduce our empirical methodology in section III. Results are presented in section IV, and section V concludes.

II. The Intervention: Targeted Subsidized Health Insurance

Our study area is the administrative department of Nouna in northwestern Burkina Faso, a part of Kossi Province bordering Mali. Its main town is Nouna town, our study site's only urban area. Health shocks are a major threat to livelihoods in the region, while the average distance to the closest health-care facility is about 10 km, more than the national average of 7.2 km (Robyn et al. 2012).

With the objective of developing a nationwide health insurance scheme, the Burkinabé Ministry of Health decided to explore the potential of voluntary health insurance during the early 2000s. The department of Nouna was chosen as the pilot site because it features a Health Research Center, which has been administering a Health and Demographic Surveillance System since 1993. In 2004, the local insurance corporation Assurance Maladie à Base Communautaire was founded, and the scheme was rolled out within 3 years, so that by 2006 every household in the department had access to health insurance. To enroll, a household pays a one-time membership fee of CFA 200 and an up-front premium of CFA 1,500 and CFA 500 (approximately US$3 and US$1 in 2009, not purchasing power parity adjusted, respectively) per insured adult and child, up to the age of 16, respectively.[2] Payment can take place in installments, and health insurance coverage starts from the day of the last installment, lasting for 12 months. Enrollment campaigns were carried out around the turn of the calendar year, after the harvest of the main crops when the liquidity of farmers is at a peak, with the objective of enrolling households before the onset of the monsoon in May, a time when the health environment becomes especially critical. A household could purchase health insurance for only a subset of its members. To obtain benefits, each insured individual obtains an insurance card, which entitles the holder to consultations and drugs free of charge at all health centers and a hospital, per the benefits package.[3] As a social health insurance

[2] According to our calculations, the adult premium corresponds to about 1.15% of median annual per capita consumption expenditures in the Nouna Department, which in turn are roughly equal to the national annual poverty line.

[3] The benefit package includes comprehensive prenatal care, laboratory tests, inpatient hospital stays, X-rays, emergency surgery, and transport by ambulance as well as generic and essential brand-name drugs. It does not include dental and ophthalmologic treatment, conditions of addiction, HIV-AIDS, and other chronic diseases.

scheme, economic self-sufficiency has not been an objective, and subsidies have come from the Burkinabé Ministry of Health as well as international donors.

Despite the seemingly affordable insurance premium and the favorable benefits-to-cost ratio, by 2006, enrollment rates remained far below expectations and were especially low among poor households. Consequently, in 2007, we decided to offer a 50% discount on the insurance premium to the poorest quintile of households in each village and urban neighborhood. To identify beneficiary households for two subsequent years, we carried out community-based targeting (CBT) exercises (Alatas, Banerjee, and Hanna 2012) in all villages and Nouna town during the first quarter of each of the years 2007, 2009, and 2011. The focus of this paper is on the 2009 campaign.[4] Regarding the urban area, guided by local informants, we partitioned Nouna town into 22 similar-sized neighborhoods of around 90 households. In the sequel, we will refer to both villages and urban neighborhoods as “communities.”

III. The Fuzzy Regression Discontinuity Design

In each community, the CBT started with a publicly convened assembly open to everyone interested, where the facilitator first discussed the purpose of the meeting. Detailed transcripts of these assemblies confirm that the objective of identifying the poorest households was successfully communicated on the ground. The facilitator initiated a focus group discussion to elicit criteria for poverty and wealth.[5] The assembly was then instructed to combine these criteria and define three or four wealth brackets. Subsequently, the assembly elected three local key informants by acclamation. Physically separated from the assembly and each other, each key informant first assigned each household to one of the previously defined wealth brackets and, second, ordered all households within each bracket.[6] While the number of households eligible in a community, for example, Q, was fixed in advance by us and set equal to 0.2 times the number of ranked households, neither the community nor the key informants knew Q before the completion of the rankings.

[4] In 2007, the CBT exercises faced some implementation challenges that negatively impact the experimental design we are exploiting for the identification of causal effects of insurance pricing (Oberländer et al. 2014). Moreover, we carried out an economic census, which we use in some of our analyses, only in 2009. More details on the 2009 CBT exercise, including a comparison with statistical targeting procedures, can be found in Hillebrecht et al. (2020).

[5] The two most often stated criteria were “has insufficient food” and “has nothing.”

[6] For rural and urban communities alike, there is a high correlation of key informants’ ranks with objective wealth ranks obtained from an asset-based principal component, with Spearman correlation coefficients of 0.63 for rural communities and 0.62 for urban communities. This gives us confidence that, first, our subsequent estimates are also externally valid for objectively poor households and, second, that heterogeneous demand elasticities in rural and urban sectors are not driven by differences in key informants’ objectivity regarding poverty assessments.

Once all key informants had completed the ranking, the set of beneficiary households was determined by the facilitator according to the following algorithm:

1. All households ranked in the poorest quintile by at least two informants are initially selected (18% of all households, on average; the sum of the rows "Targeted by all three informants" and "Targeted by exactly two informants" in table A1).
2. Denoting the number of households identified in step 1 by S,

   a) if $S = Q$, the CBT ends, and the set of beneficiaries comprises the households identified in step 1;
   b) if $S < Q$, in a subsequent consultation among the three informants, the remaining $Q - S$ beneficiary households are selected from the set of households ranked in the poorest quintile by exactly one informant (see the row "Targeted by exactly one informant" in table A1);
   c) if $S > Q$, in a subsequent consultation among the three informants, $S - Q$ households ranked in the poorest quintile by exactly two informants are removed from the initial beneficiary set.

Case 2*a*, which occurred in seven communities, is the most straightforward configuration to introduce our regression discontinuity design (RDD). Denoting the rank of household i, from poorest to wealthiest, assigned by informant k by rk_{ik}, we define the household's median rank, $\widetilde{rk}_i$, as the median of the three informants' assessments, $\widetilde{rk}_i = \text{median}(rk_{i1}, rk_{i2}, rk_{i3})$. According to the algorithm, a household obtains a voucher if at least two informants have assigned a rank not greater than Q, which is equivalent to the condition that the household's median rank is no greater than Q. In other words, using the median rank as forcing variable, the probability of obtaining a voucher drops sharply, from one to zero, at the threshold Q.

In the 37 communities to which case 2*b* applies, all households with a median rank no greater than Q continue to obtain a voucher automatically, but in addition a consultation for selecting the remaining beneficiaries took place. The set of households discussed in this consultation are ranked in the poorest quintile by only one informant and hence have a median rank greater than the threshold Q. Applying the RDD outlined in the preceding paragraph to these communities gives a fuzzy RDD because some households to the right of the threshold, those selected in the consultation, are also beneficiaries.

In the remaining six communities, to which case 2*c* applies, no household with a median rank greater than Q obtains a voucher, while not all households

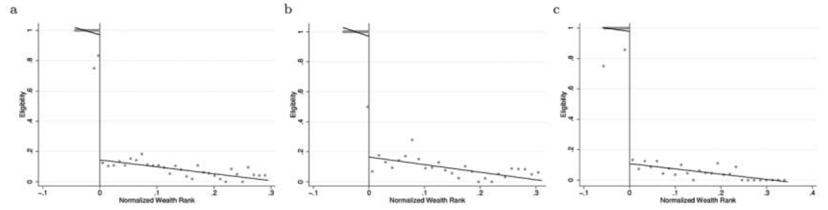

Figure 1. Normalized wealth rank and eligibility. *a*, Pooled. *b*, Rural. *c*, Urban. Circles represent outcome means by bin. Choices of bandwidths and the number of bins in evenly spaced partitions are fully data driven, according to Calonico et al. (2015).

with a median rank smaller than Q are beneficiaries. Applying our RDD to these communities again gives a fuzzy RDD because some households to the left of the threshold, those removed in the consultation, fail to obtain a voucher.

In our empirical analysis, we pool the data from all 50 communities and normalize the forcing variable community-wise to account for different community sizes. For each community, we make its distribution uniform through a probability integral transformation of the median rank and center it at the community's RD threshold. We will call this transformed forcing variable "normalized wealth rank" in the sequel.[7] Figure 1 plots households' average beneficiary status over the normalized wealth rank and confirms that the sign of a household's normalized wealth rank is a strong predictor of beneficiary status: at the RD threshold, the incidence of beneficiaries drops by 81 and 86 percentage points in rural and urban communities, respectively. These large drops are consistent with the figures in table A1, according to which 16.5% of beneficiaries are selected in consultations (the incidence of "never-takers" in table A1 divided by the share of households selected in step 1 of the algorithm), while only 0.4% of households that are automatically selected by the algorithm (the incidence of "always-takers" in table A1 divided by the targeting rate) are removed in a subsequent consultation.

Turning to the econometric implementation, we perform nonparametric local linear regressions with automatic bandwidth selection (Calonico, Cattaneo, and Titiunik 2014) and estimate the intent-to-treat (ITT) effect of subsidization on enrollment using the following equation:

$$y_{cit} = \alpha_c + \beta \cdot \mathbb{1}\{0 \leq wr_{ci}\} + \delta_1 \cdot wr_{ci} + \delta_2 \cdot \mathbb{1}\{0 \leq wr_{ci}\} \cdot wr_{ci} + u_{cit}, \quad (1)$$

[7] To be precise, for a community with households indexed by i, where $i = 1, \ldots, n$, we define the normalized wealth rank by $wr_i = (\#\{j : \widetilde{rk}_j \leq \widetilde{rk}_i\} - \#\{j : \widetilde{rk}_j \leq Q\} + 0.5)/n$, where $\#\{A\}$ denotes the cardinality of set A. The first bracket gives what may be called a community wealth rank when households are ordered according to their median rank. It ranges from 0 (poorest) to $n - 1$ (most affluent). The second bracket gives the number of households ranked in the lowest quintile by at least two informants, which is the RD threshold applying to the community wealth rank.

where wr_{ci} denotes the normalized wealth rank, and y_{cit} is a dummy variable equal to one if household or individual i in community c enrolled in year t. For household demand, y_{cit} equals one if any member of the household identified by ci enrolled, and an observation is a household in a given year.[8] For individual demand, y_{cit} equals one if the individual identified by ci enrolled, and an observation is an individual in a given year.[9] In this case, wr_{ci} is the normalized wealth rank of the household to which individual ci belongs. The subscript t indexes the years 2009 and 2010, for which the vouchers that were allocated during the first quarter of 2009 were valid. The coefficient β captures the effect of a price increase, from the subsidized to the full premium, on enrollment. Following Pop-Eleches and Urquiola (2013), we include a community fixed effect, α_c, and cluster standard errors at the level of the community.

IV. Empirical Analysis

A. Data

We merge the following three data sources by household identities: wealth rankings and beneficiary lists from the targeting exercise; administrative data on insurance enrollment and benefits from the health insurance provider; and demographic characteristics of households and individuals from a demographic census, which is updated every 3 months. For further analyses we use, in addition, information on household wealth and occupations from a household economic census canvassed in 2009, contemporaneous with the community targeting exercise.

Table A2 contains descriptive statistics for the merged data set at the individual and household levels.[10] For enrollment, we pool the years 2009 and 2010. We report mean values for the entire study area and for rural and urban sectors separately. In addition, we report means for the subgroup of cutoff households, whose normalized wealth rank is within a 5 percentile range (one-sided) around the eligibility cutoff.[11] Education levels are low, and households

[8] This corresponds to the dependent variable reported by Thornton et al. (2010), Capuno et al. (2016), and Levine, Polimeni, and Ramage (2016).

[9] This is the dependent variable reported by Wagstaff et al. (2016).

[10] The wealth ranking data comprise the universe of 5,350 households that were present in the 28 study villages and in Nouna town in early 2009. From the community wealth ranking lists, we were able to identify 4,820 households with a total of 38,705 individuals in the demographic census, which is essential for our analyses. This implies an attrition rate of 8.9%. While we cannot track insurance enrollment for these households, we find that 17% of them (14% and 20% in urban and rural communities, respectively) were targeted by at least two informants, which compares to 18% among all ranked households. This figure gives us confidence that the households in our data are representative of the entire population.

[11] We choose this window because 0.05 corresponds roughly to the average of the smaller (the left) bandwidth chosen by the data-driven bandwidth choice algorithm used in the subsequent estimations.

primarily rely on agricultural activities for income generation, even in the urban sector. With an average of 5.8 members, cutoff households are significantly smaller, by 1.7 individuals, or 23%, than the average household. In the urban sector, they also have a significantly higher share of elderly and fewer children. Close to 50% of urban cutoff households are headed by a widow or widower.

Household enrollment rates in 2009 and 2010 average 6% and 15% in rural and urban areas, respectively, while individual enrollment has stood at 3% and 9%. The average insurance benefit for individuals in cutoff (all) households equals CFA 2,715 (CFA 3,525), a little less than twice the regular adult insurance premium of CFA 1,500. These figures illustrate that the insurance scheme is heavily subsidized even at the policy's regular price, which falls far short of an actuarially fair price.

We close this section with a brief discussion of the living conditions of households at the RD threshold. According to our calculations involving data from various sources, urban households' consumption at the threshold is close to Burkina's national poverty line. This assessment is also broadly consistent with the national urban poverty rate of 25% for 2009 (INSD 2015). The consumption of a rural household at the threshold, on the other hand, falls short of the national poverty line by around 30% on average, and hence the rural households to which our estimates apply are ultrapoor by international standards. This assessment is also broadly consistent with Burkina's national rural poverty rate of 53% (INSD 2015).

B. Validation

In this section, we discuss the fundamental identifying assumptions of our RDD and perform some econometric tests of these assumptions. First, no individual involved in the community targeting exercise—key informants as well as ranked households—must be able to manipulate the eligibility threshold of the forcing variable or the value of the forcing variable in a way that leads to "precise sorting" (Lee and Lemieux 2010) of households around the eligibility threshold. Regarding the first item, no manipulation of the eligibility threshold results from our CBT design, where the beneficiary contingent Q—and hence the threshold value for the median rank—was fixed exogenously by us before the wealth ranking and disclosed neither to the community nor to the informants before each informant had turned in his wealth ranking. Regarding the second item, no precise sorting around the threshold follows from our procedural rules by which, first, informants carried out the wealth rankings in seclusion and separately from each other and, second, the informants were unaware of the cutoff wealth rank Q. Hence, neither a household nor an informant has any opportunity to sort a particular household precisely around the cutoff.[12]

Another potential concern with our RDD is its fuzziness, in particular to the right of the threshold, where 15%–20% of households are beneficiaries (see fig. 1). These are the result of the trilateral consultations characterized under case 2*b* of the voucher allocation algorithm. In more technical language, these households are selected into treatment based on unobservables; but figure 1 also clearly illustrates that the jump in the treatment probability (at the discontinuity) is large, greater than 80 percentage points. Hence, the ITT effects from our RDD and the true but unobserved average treatment effects of subsidization at the threshold will be very similar. Moreover, in section OA.1 of the online appendix, we conduct additional econometric analyses recently introduced by Bertanha and Imbens (2020) providing evidence in support of the hypothesis that ITT estimates are closer to the average treatment effects (ATEs) in our context than local average treatment effects obtained from instrumental variable estimations of the fuzzy RDD.

Following Lee and Lemieux (2010), we now present some econometric internal validity tests for RDDs. First, we conduct McCrary tests and examine the continuity of the density of the forcing variable at the cutoff. According to figure 2, the local polynomial density estimator by Cattaneo, Jansson, and Ma (2018) exhibits no statistically significant discontinuity at the threshold, in line with the RDD's identifying assumptions.

Second, we assess the assumption underlying an RDD that the expected value of the outcome of interest absent the treatment is continuous in the forcing variable (Lee and Lemieux 2010). We test this assumption in two ways. First, we conduct placebo experiments by employing enrollment data from years preceding our 2009 intervention. Second, we carry out balancing tests with several covariates surveyed contemporaneously to the CBT. For the placebo experiment, we estimate (1) with the modification that the dependent variable

[12] Relatedly, informants could favor their peers and rank coethnic households systematically poorer (Alatas et al. 2019). While we cannot rule out such manipulation and indeed find some, albeit very limited, ethnic favoritism in a companion paper (Schleicher 2017), this will not jeopardize the internal validity of our estimates as long as the resulting sorting is not precise around the eligibility threshold. Suppose, e.g., that the majority ethnicity of a community dominates the council of informants, who rank their coethnic households systematically lower than they would based on other objective criteria. This will merely result in an estimate that is consistent for the causal ATE in a population that is a mixture of the objectively poorest minority households and somewhat more affluent ethnic majority households. Phrased in the terminology of Bertanha and Imbens (2020), favoritism can only threaten the external validity (bias relative to ATEs obtained at the 20th percentile of the objectively poorest households) but not the internal validity of our treatment effect estimates, which are causal for households positioned between the lowest two quintiles in a community wealth ranking.

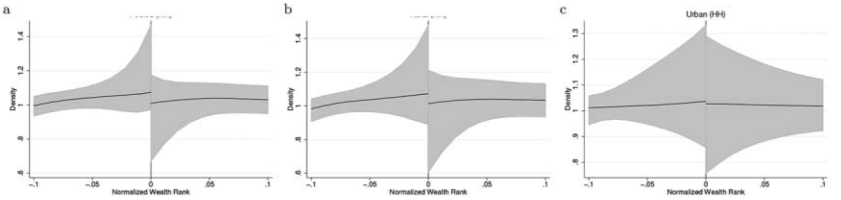

Figure 2. Distribution of the forcing variable. *a*, Pooled. *b*, Rural. *c*, Urban. Plots depict local polynomial density estimates of the normalized wealth rank together with 95% confidence intervals, separately for positive and negative values of the forcing variable. Plots are generated with the Stata package rddensity (Cattaneo, Jansson, and Ma 2018) with the options plot plot_range(−.1 .1) fitselect(restricted).

is lagged by 2 years; that is, we regress household enrollment in 2007 and 2008 on the household's normalized wealth rank in 2009. According to the results, which are plotted in figure 3 and set out in table 1, there are only very small and statistically insignificant placebo effects.

For the balancing tests, we estimate (1) with several dependent variables characterizing the demographic, educational, and occupational structure of a household, which we invoke in further analyses in sections IV.C and IV.D. According to the results set out in table 2 and plotted in figure OA1 (figs. OA1–OA7 are available online), we find no discontinuity that is significant at conventional levels and conclude that these covariates are distributed as good as randomly around the threshold.

C. Main Results

1. Health Insurance Enrollment

Columns 1–3 of table 3 contain the estimated ITT effects of subsidization on insurance enrollment at the household level, β in (1), for different subsamples. In addition, figure 4 plots health insurance enrollment at the household level over the normalized wealth rank. According to column 1 of table 3, in the pooled sample, there is a statistically significant increase in the fraction of households that enroll at least one member of 10.2 percentage points. Columns 2 and 3

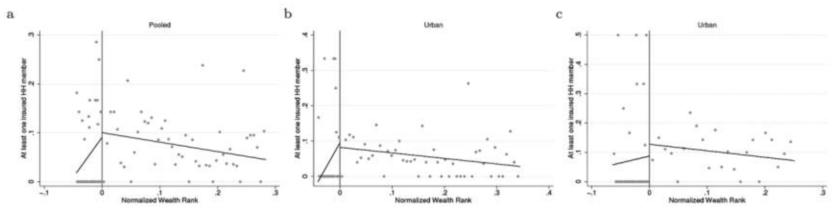

Figure 3. Subsidization and household enrollment: placebo experiment. *a*, Pooled. *b*, Rural. *c*, Urban. See figure 1.

TABLE 1

INTERNAL VALIDITY TEST: PLACEBO EXPERIMENT (DEPENDENT VARIABLE: ANY HOUSEHOLD MEMBER INSURED)

	Pooled (1)	Rural (2)	Urban (3)
RD estimate	−.004	−.028	.029
	(.034)	(.041)	(.053)
Outcome mean[a]	.076	.059	.103
Bandwidth left	.038	.036	.059
Bandwidth right	.253	.276	.204
Unit of observation	HH	HH	HH
Estimation sample	All HHs	All HHs	All HHs
Observations (left)	422	242	228
Observations (right)	2,457	1,681	740
No. of communities	50	28	22
Community FE	Yes	Yes	Yes

Note. Robust standard errors (clustered at the community level) are in parentheses. RD = regression discontinuity; HH = household; FE = fixed effects.
[a] Mean is calculated for the subsample of observations located above the cutoff and below the right-hand boundary of the bandwidth.

reveal that this effect is almost exclusively driven by urban households' demand, where the share of enrolled households increases greatly, from 10.3% to 34.3%. In the urban sector, our intervention in fact increases enrollment in the lowest wealth quintile (as measured by a household's median CBT wealth

TABLE 2

INTERNAL VALIDITY: REGRESSION DISCONTINUITY DESIGN BALANCING TESTS

					Dummy for Being Headed by a			
	HH Size (1)	Share of Female HH Members (2)	Any Educated HH Member (3)	Nonagricultural Employed Head of HH (4)	Widowed Male (5)	Widowed Female (6)	Elderly Male (7)	Elderly Female (8)
RD estimate	−.098	−.011	.040	−.044	.013	−.027	−.061	−.012
	(.437)	(.024)	(.054)	(.052)	(.038)	(.035)	(.054)	(.029)
Outcome mean[a]	7.222	.474	.562	.248	.121	.108	.312	.059
Bandwidth left	.060	.067	.049	.054	.040	.068	.056	.064
Bandwidth right	.201	.158	.226	.234	.126	.272	.193	.207
Observations (left)	312	354	261	288	225	354	295	332
Observations (right)	976	770	1,107	1,148	610	1,334	941	1,009
Community FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. Robust standard errors (clustered at the community level) are in parentheses. HH = household; RD = regression discontinuity; FE = fixed effects.
[a] Mean is calculated for the subsample of observations located above the cutoff and below the right-hand boundary of the bandwidth.

TABLE 3
SUBSIDIZATION AND ENROLLMENT

	Any HH Member Insured (Extensive Margin)			Individual Insured				
				Population			Intensive Margin (Urban)	HH Size (Urban)
	Pooled (1)	Rural (2)	Urban (3)	Pooled (4)	Rural (5)	Urban (6)	(7)	(8)
RD estimate	−.102**	−.022	−.240***	−.087*	−.043	−.183*	−.022	.483
	(.048)	(.039)	(.086)	(.044)	(.039)	(.096)	(.131)	(1.904)
Outcome mean[a]	.061	.035	.103	.032	.014	.058	.428	8.588
Bandwidth left	.063	.089	.064	.062	.093	.048	.077	.080
Bandwidth right	.224	.192	.177	.232	.192	.167	.163	.133
Unit of observation	HH	HH	HH	Individual	Individual	Individual	Individual	HH
Estimation sample	All HHs	All HHs	All HHs	All individuals	All individuals	All individuals	All individuals in insured HHs	Insured HHs
Observations (left)	653	568	249	3504	3,070	1,038	303	71
Observations (right)	2,178	1,155	642	15,543	7,710	4,246	572	51
No. of communities	50	28	22	50	28	22	22	22

Note. Robust standard errors (clustered at the community level) in parentheses. HH = household; RD = regression discontinuity.
[a] Mean is calculated for the subsample of observations located above the cutoff and below the right-hand boundary of the bandwidth.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

rank) to the average encountered in the four upper wealth quintiles, about 25%.[13]

Columns 4–6 of table 3 contain ITT estimates for individual enrollment. Analogous to household-level demand, the fraction of individuals who enroll increases from 5.8% to 24.1% in urban areas, while there is only a small and insignificant increase among rural residents (see also fig. 5).[14] In general, the price elasticities of individual- and household-level demand are identical only if the incidence of insurance within households that enroll as well as the size of

[13] To put these figures into perspective, for a self-selected subpopulation of villagers in rural Cambodia, Levine, Polimeni, and Ramage (2016) find an increase in household enrollment from 7% to about 50%, with an 80% discount, implying a price elasticity that is close to our urban one. Thornton et al. (2010), who offer a 50% discount to households of Nicaraguan urban informal workers, find only modest increases in take-up, by around 11 percentage points. Capuno et al. (2016) find only a small increase in enrollment among rural households in the Philippines, from 8.4% to 11.4%, in response to a 50% subsidy combined with an information leaflet.

[14] For comparison, Wagstaff et al. (2016) finds only a small and insignificant increase in individual enrollment in response to a 25% subsidy in rural Vietnam.

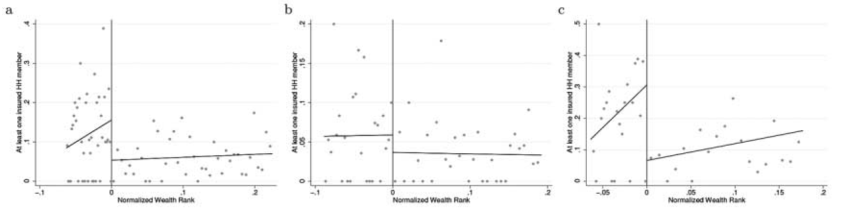

Figure 4. Subsidization and household enrollment. *a*, Pooled. *b*, Rural. *c*, Urban. See figure 1.

households that enroll are price inelastic (we show this formally in sec. OA.2 of the online appendix). Column 7—where the dependent variable is individual enrollment and the estimation sample consists of all individuals in households with at least one enrolled member—substantiates the former, and column 8—where the dependent variable is household size and the estimation sample consists of all households with at least one enrolled member—substantiates the latter.[15] Combining the enrollment figures for urban households and individuals around the RD threshold in table A2 (16% and 9%, respectively) with the point estimates in columns 3 and 6 of table 3 (24.0 and 18.3 percentage points), we find similar household- and individual-level demand elasticities, of 1.5 and 2.0, respectively.

2. Selection into Health Insurance

Information from the detailed demographic census allows us to explore heterogeneous demand elasticities by household characteristics. For example, female-headed households are typically more vulnerable to idiosyncratic shocks and could, in this case, benefit from formal insurance disproportionately. Moreover, at the individual level, we can explore differences in coverage across age groups and sex caused by subsidization. We restrict these analyses to the urban sector, where insurance demand is sufficiently price elastic.

Table 4 reports heterogeneous treatment effects of subsidization by household head characteristics. In particular, we distinguish households headed by a married male ("modal households," for short), which account for 54% of cutoff households (see table A2), from households headed by a widowed male

[15] These two estimations, as well as those reported in tables 4 and 6, do not contain community fixed effects (CFEs) because, with community fixed effects, the automatic bandwidth choice procedure of Calonico, Cattaneo, and Titiunik (2014) fails to converge. This is due to the low density of household-level observations around the threshold in some communities. Omission of control variables does not affect consistent estimation of RDDs in general (Lee and Lemieux 2010), however, and omission of CFEs in the estimations reported in cols. 1–6 of table 3 gives virtually identical point estimates, which are reported in table OA1. The same applies to enrollment effects among subgroups of individuals reported in table 5 (with CFEs) and table OA4 (without CFEs).

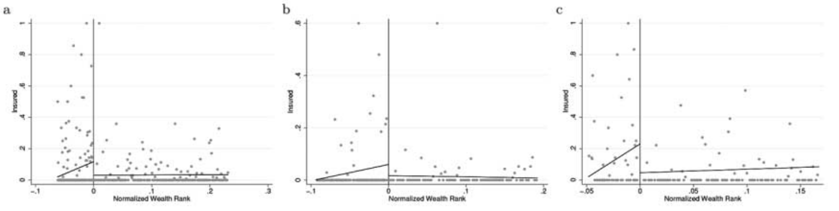

Figure 5. Subsidization and individual enrollment. *a*, Pooled. *b*, Rural. *c*, Urban. See figure 1.

(17%) or female (28%).[16] Columns 1–4 of table OA2 (tables OA1–OA4 are available online) establish that none of these household head characteristics exhibits a discontinuity at the threshold in the urban subsample. Although precision in these subsample analyses is sometimes more than halved relative to our main results due to limited sample sizes and the omission of community fixed effects (see n. 15), the point estimates set out in columns 1–3 feature a striking pattern: while insurance demand at the regular price is low and very similar across the three groups of households (according to the means reported in the third row), the price elasticity of widower-headed households is more than three times that of the other two groups of households. The demand increase among widower-headed households of 60 percentage points is more than two times the average effect reported in column 3 of table 3, while the price responsiveness of female-headed as well as modal households falls short of the average effect by one-fifth and one-third, respectively. Hence, subsidization gets close to universal coverage among widower-headed households: our estimates imply a coverage rate (at the extensive margin) of close to 75% among subsidized households of this type. Moreover, while the nonparametric estimation procedure we employ does not allow us to formally test for heterogeneous effects, the difference between the respective point estimates is around 2 standard errors or more.

In columns 4–7 of table 4, we explore heterogeneous effects by the household head's age. We find a large difference, of 24 percentage points, between elderly and prime-aged male household heads. Overall, these results suggest that both widowerhood and the household head's age—which are, of course, positively correlated—are important predictors of price responsiveness in the urban area.

The selection into insurance by marital status and age of the household head has important consequences for the distribution of insurance coverage in the population. Figure OA3 demonstrates that, compared to modal households,

[16] In our study context, the latter group and the set of female-headed households are almost identical as there are only two households headed by a married female.

TABLE 4
SUBSIDIZATION AND ENROLLMENT BY HOUSEHOLD (HH) HEAD CHARACTERISTICS, URBAN SECTOR

	Married	Widowed HH Head		Elderly HH Head		Prime-Aged HH Head	
	HH Head	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD estimate	−.172	−.600***	−.228	−.404**	−.201	−.161*	−.118
	(.111)	(.208)	(.171)	(.170)	(.205)	(.084)	(.166)
Outcome mean[a]	.098	.139	.117	.125	.130	.076	.101
Bandwidth left	.092	.063	.046	.077	.055	.093	.092
Bandwidth right	.216	.117	.321	.245	.297	.107	.317
Observations (left)	175	60	57	99	46	131	33
Observations (right)	508	72	206	304	123	170	79
Community FE	No	No	No	No	No	No	No

Note. Dependent variable is any household member insured in 2009 or 2010. Robust standard errors (clustered at the community level) are in parentheses. RD = regression discontinuity; FE = fixed effects.
[a] Mean is calculated for the subsample of observations located above the cutoff and below the right-hand boundary of the bandwidth.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

widower-headed households have substantially higher shares of elderly and smaller shares of children and adolescents (aged 16 and younger), while the fraction of prime-aged adults, whom we define as those 17–55 years old, is similar.[17] In line with these patterns, the results set out in table 5 show that individuals of older ages benefit disproportionately from subsidization, with enrollment increases of 41 percentage points. Consistent with the extensive margin selection, the principal beneficiaries are elderly individuals in widower-headed households, for whom enrollment increases by 82 percentage points, while elderly in female-headed households enjoy increases of only 32 percentage points.[18] Enrollment rates among children and adolescents increase only insignificantly, while prime-aged adults enjoy significant enrollment gains of around 15 percentage points (cols. 3 and 4 of table 5), similar to the average individual enrollment effect of 18.3 percentage points (col. 6 of table 3). Across all age groups, there are no gender biases, irrespective of the insurance price. These heterogeneous individual-level effects of subsidization for different age groups are consistent with the extensive-margin selection effects rather than age-specific selection on the intensive margin, that is, within households.

Selection into insurance according to unobserved individual risk has stimulated a large body of theoretical and empirical research. In the context of a single health insurance policy that gives customers no choice except to take it or

[17] Columns 5–10 of table OA2 establish that the population shares of these sex and age groups are as good as randomly distributed around the threshold in the urban subsample.
[18] These two figures are not displayed in the tables.

TABLE 5

SUBSIDIZATION AND INDIVIDUAL ENROLLMENT BY DEMOGRAPHIC CHARACTERISTICS, URBAN SECTOR

	Children		Prime-Aged		Elderly	
	Female (1)	Male (2)	Female (3)	Male (4)	Female (5)	Male (6)
RD estimate	−.071	−.024	−.132**	−.156**	−.411***	−.414***
	(.071)	(.038)	(.057)	(.064)	(.080)	(.130)
Outcome mean[a]	.061	.056	.062	.059	.059	.075
Bandwidth left	.036	.050	.035	.040	.048	.048
Bandwidth right	.180	.210	.165	.134	.219	.191
Observations (left)	182	228	184	188	91	66
Observations (right)	1,024	1,255	871	717	339	239
Community FE	Yes	Yes	Yes	Yes	Yes	Yes

Note. Dependent variable is individual insured. Robust standard errors (clustered at the community level) are in parentheses. RD = regression discontinuity; FE = fixed effects.

[a] Mean is calculated for the subsample of observations located above the cutoff and below the right-hand boundary of the bandwidth.

** $p < .05$.

*** $p < .01$.

leave it, adverse selection means that the risk in the pool of individuals who enroll increases with the offer price. Following Polimeni and Levine (2012), we conduct a price test to explore the possibility of adverse selection in health insurance demand by comparing average ex post insurance claims across different insurance offer prices. To be precise, we use annual individual insurance claims as the dependent variable in (1), and our estimation sample contains only insured individuals. We conduct this analysis with and without the basic demographic characteristics age group and sex. When the insurer observes precisely these characteristics, the estimation with controls will identify the extent of pure adverse selection stemming from information asymmetries in the market. With no controls, the estimate of β in (1) will yield the compound effect of selection into insurance on observable and unobservable risk. In addition to the value of insurance claims, which we calculate as the cost of prescriptions and a flat consultation fee, we also consider the number of health-care facility visits per year.

For urban households, the results are set out in table 6 and illustrated in figure 6. According to columns 2 and 4, where demographic controls are included, we find no evidence for selection into insurance according to unobserved risk. In fact, visits and claims are estimated to increase by about 8% and 10%, respectively, under the subsidization regime. While these effects are imprecisely estimated, their sign is opposite to adverse selection, where a higher price attracts higher risks. When no controls are included, both point estimates increase, suggesting that selection on observables goes into the same direction as selection on unobserved risk. The implied direction of the selection on

TABLE 6
ADVERSE SELECTION, URBAN SECTOR

	Health-Care Facility Visits		Insurance Claims	
	(1)	(2)	(3)	(4)
RD estimate	−.362	−.156	−1,051.507	−358.856
	(.642)	(.655)	(1,103.354)	(1,210.354)
Outcome mean[a]	1.950	1.950	3,574.887	3,574.887
Bandwidth left	.077	.078	.068	.067
Bandwidth right	.158	.158	.141	.140
Controls[b]	No	Yes	No	Yes
Observations (left)	219	244	210	210
Observations (right)	282	282	265	265
Community FE	No	No	No	No

Note. Robust standard errors (clustered at the community level) are in parentheses. The estimation sample contains all insured individuals from 22 urban communities in 2009 and 2010. Columns 2 and 4 include demographic controls. RD = regression discontinuity; FE = fixed effects.
[a] Mean is calculated for the subsample of observations located above the cutoff and below the right-hand boundary of the bandwidth.
[b] Control dummy variables are female, prime-aged, and elderly.

observable characteristics is in line with our previous finding that disproportionately more elderly, who face greater health risks, buy insurance when offered a discount.[19]

D. Determinants of Health Insurance Demand

Our results thus far show that subsidization of health insurance has only a limited effect on demand among poor households, with the exception of one specific subgroup. Most notably, demand is minimal in rural areas, even with a 50% subsidy. While our research design does not feature experimental variation beyond the offer price, in this section, we seek to explore factors that are likely responsible for this lack of demand and price sensitivity. Guided by our findings regarding heterogeneous effects, we will focus on two types of comparisons: first, between rural and urban households and, second, across urban households by sex and marital status of the household head. In these analyses, we employ data from the 2009 economic census mentioned earlier as well as from a household panel data set covering 340 rural and 225 urban households interviewed annually between 2006 and 2009. Importantly, the panel data include information on illness, health-related expenditures, and informal transfers.

[19] Using a different empirical approach, adverse selection in Nouna's health insurance scheme between 2004 and 2007 is the subject of Parmar et al. (2012). In a sample survey data set, they find heterogeneous demand responses to subsidization, 14 (9) percentage points for individuals who had (not) reported a medical condition in the survey.

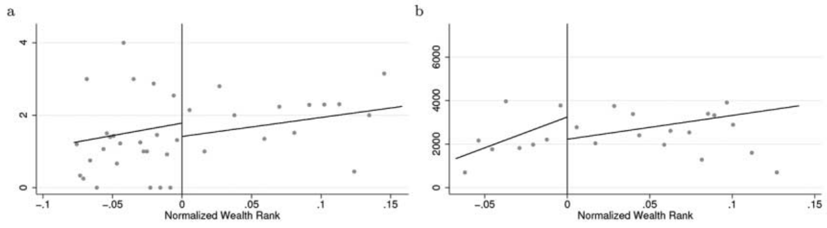

Figure 6. Adverse selection. *a*, Total number of visits. *b*, Total value of insurance claims. Demographic controls are included; see figure 1.

1. Lack of Demand among Rural Households

We first deal with the sluggish demand among rural households and explore whether rural-urban differences in the burden of disease may be driving the demand patterns we observe. According to panel *a* of figure OA4, which visualizes data from the household survey for all households with a normalized wealth rank of less than 0.2, adults in rural households face a higher burden of disease as measured by the incidence of severe illness: more than 3% of rural adults experienced a severe illness during the 30 days preceding the interview, while this figure stands at only 2% in the urban sector. With a p-value of .03, this difference across the two sectors is statistically significant. Hence, we can safely rule out a differential exposure to illness as a driver of the lack of rural demand. On the other hand, there is a large difference in health-related out-of-pocket expenditures across the two sectors, with rural and urban cutoff households spending CFA 360 and CFA 720 per adult and month, respectively. As a consequence, the subsidized insurance premium of CFA 750 per adult roughly matches urban households' health budgets, while it vastly exceeds rural dwellers' ordinary health expenditures.

We now investigate the role of wealth in more detail. In this connection, a limitation of our research design is that we do not observe the price elasticity of demand along the entire wealth distribution in each community. Hence, we cannot hold wealth constant and interact our treatment effect with a rural dummy. Instead, our estimates are local in the sense that they refer only to cutoff households. We exploit, however, the fact that the cutoff is at the same relative position in each community and that there is natural variation in wealth across communities. Along these lines, figure 7 plots RD estimates community by community over a normalized community-wise household asset index obtained from principal component analysis, $\tilde{w}_c$, where c indexes communities. The index is calculated such that it proxies the wealth of a cutoff household in community c.[20] According to the plot, rural cutoff households command substantially

[20] To measure household wealth, we first calculate a wealth index for each household in the study area as the first principal component of 28 indicators of dwelling characteristics and asset possessions, surveyed

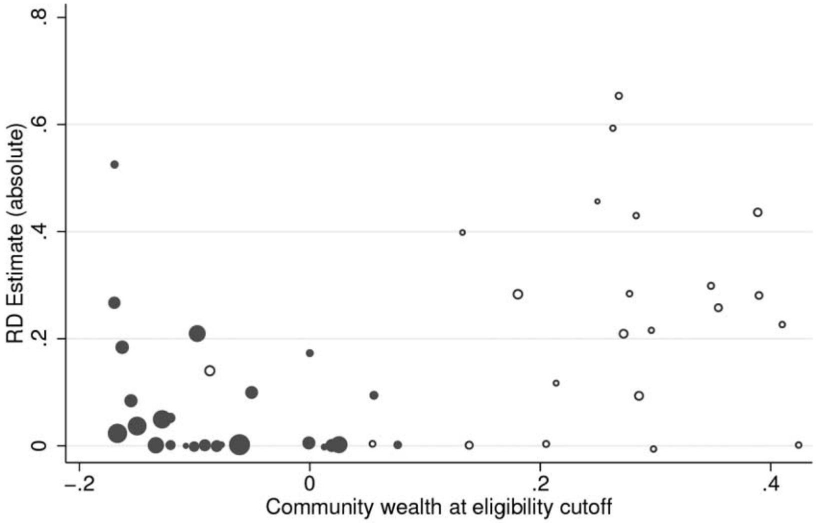

Figure 7. Regression discontinuity (RD) estimates by community. Each circle represents a community from the rural (filled) or urban (open) sector. The circle perimeter corresponds to community size. Communities are horizontally aligned with respect to community wealth around the cutoff, expressed in percentiles of the aggregate asset wealth distribution. For each community, the vertical scale gives the absolute value of the RD estimate for a community-wise local linear regression with a triangular kernel and an asymmetric manual bandwidth of 0.1 and 0.2, left and right of the eligibility cutoff, respectively.

less wealth than their urban counterparts. And while there is a small positive correlation between wealth and the size of the RD estimate in urban areas, such a correlation is completely absent in the villages.

We also estimate heterogeneous treatment effects of subsidization by wealth status as measured by $\tilde{w}_c$. To accomplish this, we have to give up the nonparametric estimation approach employed thus far and instead conduct a parametric estimation of the discontinuity and its interactions (as in, e.g., Dell 2015) to assess whether wealth differences alone can explain the low demand elasticity in rural areas or whether there are rural demand impediments beyond wealth.

According to the results, which are set out in column 1 of table 7, almost the entire difference in the price elasticity of demand between rural and urban areas is accounted for by the rural interaction, which is statistically significant at the 10% level, while the point estimate of the wealth interaction is very close to zero. Given the limited overlap in wealth at the threshold between rural and

in the second half of 2009. We then sort all households by this index to obtain the wealth percentile for each household. We then sort households community by community, again by the asset index, and identify the household at the top of the first (i.e., poorest) quintile. We take that household's aggregate wealth percentile as a proxy of household wealth at the cutoff in community *c*.

TABLE 7

INSURANCE DEMAND IN RURAL AND URBAN SECTORS: ROLE OF STRUCTURAL DIFFERENCES (ANY HOUSEHOLD MEMBER INSURED)

	(1)	(2)	(3)	(4)
RD estimate	−.179**	−.276**	−.279**	−.176***
	(.068)	(.114)	(.122)	(.062)
Rural	−.154**	−.173***	−.201***	−.214***
	(.066)	(.052)	(.052)	(.050)
Rural × RD estimate	.125*	.111**	.145***	.139***
	(.067)	(.049)	(.052)	(.050)
Wealth	.105			
	(.189)			
Wealth × RD estimate	.001			
	(.184)			
HH size		−.029		
		(.023)		
HH size × RD estimate		.024		
		(.020)		
No. of literate HH members			−.016	
			(.031)	
Literacy × RD estimate			.027	
			(.027)	
Distance to next CSPS (km)				.006
				(.004)
Distance × RD estimate				−.003
				(.005)
Observations (left)	730	730	730	730
Observations (right)	2,140	2,140	2,140	2,140
No. of communities	50	50	50	50
Community FE	No	No	No	No

Note. Robust standard errors (clustered at the community level) are in parentheses. The estimation sample contains observations from 22 urban and 28 rural communities. Regression discontinuity design specification is a parametric local linear regression with triangular kernels and specified asymmetric bandwidths of −0.07 and 0.22. All additional covariates vary at the community level. Specifically, wealth, household (HH) size, and literacy are the estimated asset-index rank, HH size, and number of literate HH members for each community's HH located at the top of the first wealth rank quintile. RD = regression discontinuity; CSPS = primary health care center; FE = fixed effects.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

urban communities, however, we take these findings as not more than suggestive. With an analogous methodology, we explore differences in household size, which we take as a proxy for the within-household diversification of risk, literacy, and remoteness, as measured by the distance from the next medical facility, as predictors of the demand elasticity. According to columns 2–4, however, none of these factors successfully explains differences in enrollment across communities. Instead the estimate of the rural interaction becomes even more significant—probably because of a lower correlation of these factors with rural residency than wealth.

We also explore the possibility of better risk sharing in rural than in urban communities. Toward this, panels *c* and *d* of figure OA4 plot various measures of transfers received by adult members of the household in the 5 months preceding the interview. According to these figures, by all measures, urban cutoff households are more active regarding informal transfers, which suggests that informal risk sharing is no better for rural than for urban households. To conclude, this somewhat informal analysis suggests that there are important non-economic obstacles to the dissemination of health insurance in rural areas beyond wealth, remoteness, and literacy. A lack of trust in or experience with formal economic and health-care institutions is an obvious candidate reason in this context (Giné, Townsend, and Vickery 2008; Cole et al. 2013).

2. Demand Heterogeneity among Urban Households

We now explore differences between widower-headed and other households for understanding the stark differences in their responsiveness to health insurance pricing. According to figures OA3, OA5, and OA6, widower-headed households are relatively small, wealthy (regarding assets), old (with respect to the age of their members), and literate. According to panels *a* and *b* of figure OA7, they face a high burden of disease and, with about CFA 960 per adult, have by far the largest health budgets. Moreover, according to panel *d*, they have somewhat less access to informal risk sharing than other households. While these factors have been found to be positive predictors of health insurance demand in other contexts previously (Eling, Pradhan, and Schmit 2014), a novel finding that our results suggest is that a household's health budget and access to informal risk sharing are important predictors of the price sensitivity of demand rather than its level at the regular price. Consistent with our finding on the lack of demand in rural areas, a household's health budget appears to be a particularly important predictor for the price responsiveness of demand: in both comparisons that we have considered here, the subgroups with the largest health budgets are far more likely to respond to vouchers. An implication of this pattern is that the generous benefits included in the insurance package appear to be of limited importance to households' enrollment choices. Notice that, according to table A2, individual insurance benefits average around CFA 3,000, which is more than three times urban adults' average out-of-pocket expenditures. Instead, our findings suggest that insurance pricing has to take into account households' previous health budgets. Insurance premiums exceeding households' health expenditures in the absence of formal insurance appear to have little potential to expand insurance outreach noticeably—even if highly subsidized.

V. Conclusion

Around the globe, micro health insurance programs face the challenge of low enrollment rates, especially among the poor. With an original fuzzy regression discontinuity design, we have evaluated the impact of a targeted 50% subsidy on the take-up of voluntary health insurance in Burkina Faso. We have found that halving the insurance premium more than triples enrollment among poor urban households, while this intervention has been largely ineffective for rural households. A similar but less stark pattern has been found by Capuno et al. (2016) in the Philippines. While they attribute the lack of rural demand to higher transactions costs due to remoteness, we find no evidence for this channel in our setting. Among urban households, we have found that subsidization attracts almost exclusively households headed by widowers, while insurance penetration within households remains unchanged. Correlating these heterogeneous effects with background information on household health budgets prior to the intervention, as well as informal transfers, the patterns we find are consistent with two important and thus far little explored challenges that micro health insurance faces in low-income environments. First, a one-size-fits-all insurance policy with a defined benefits package and a uniform price appears to be unable to meet households' varying health expenditure goals. Second, informal risk sharing between households appears to reduce the demand for actuarially fair insurance to close to nil. When informal risk-sharing networks succeed in insuring idiosyncratic risks effectively, fairly priced formal insurance has little scope for improving financial protection. In contrast, all households with average health budgets smaller than the insurance premium will find health insurance unattractive. In such a case, subsidization will merely compensate for the mismatch between desired health budgets and the insurance premium. This challenge does not exist for the other, thus far most popular form of microinsurance, index-based crop insurance. First, the amount of insurance that can be purchased by a farmer is variable and can be adjusted to farm size. Second, informal risk sharing is largely ineffective for aggregate weather shocks. On the downside, unlike health insurance, index insurance features basis risk, a mismatch between crop losses and insurance payouts, albeit informal risk-sharing networks appear to mitigate such basis risk to some extent (Mobarak and Rosenzweig 2013; Dercon et al. 2014). Substantiating the patterns that our data suggest within a fully experimental research design, for example, by offering policies with different benefit packages, seems a promising direction for future research.

We end this paper with a brief remark on the direction social health insurance has taken in Burkina Faso since 2010. Due to the persistence of low enrollment rates, a lack of government funding, and administrative challenges, the pilot scheme investigated here was abandoned in 2012, after another round of community targeting exercises and vouchers in 2011. In 2015, after the national parliament had passed a bill toward introducing universal health insurance in the entire country, the Nouna pilot scheme has been revived and even expanded in territorial coverage, albeit under a different implementing agency, the Association Songui Manégré—Aide au Développement Endogène. At the same time, under the Programme Gratuité, the Ministry of Health has generally abolished fees for maternal and child health-care services and is currently experimenting with a performance-based financing scheme for public health centers and hospitals to improve health-care provision.

Appendix

TABLE A1
COMMUNITY-BASED TARGETING INTERVENTION

	Pooled (1)	Rural (2)	Urban (3)
Ranked HHs per community	107	117	93
	(74)	(92)	(39)
Targeted HHs per community	21	23	18
	(15)	(19)	(7)
Targeted HHs per community (share)	.20	.20	.20
	(.01)	(.01)	(.01)
Targeted by all three informants	.07	.07	.08
	(.03)	(.03)	(.02)
Targeted by exactly two informants	.11	.11	.12
	(.31)	(.31)	(.32)
Targeted by exactly one informant	.22	.23	.20
	(.41)	(.42)	(.40)
Rank correlation between three informants	.66	.66	.66
	(.13)	(.15)	(.09)
Noncompliance rates:			
Always-taker	.033	.039	.023
	(.179)	(.193)	(.148)
Never-taker	.008	.005	.017
	(.088)	(.067)	(.130)
No. of communities	50	28	22
Communities with always-takers	37	21	16
Communities with never-takers	6	3	3

Notes. Standard deviations are in parentheses. Always-takers = eligible households not selected by step 1 of the beneficiary selection algorithm; never-takers = ineligible households selected by step 1 of the beneficiary selection algorithm.

TABLE A2
DESCRIPTIVE STATISTICS

	Pooled (*N* = 50)		Rural (*N* = 22)		Urban (*N* = 28)	
	All (1)	Cutoff (2)	All (3)	Cutoff (4)	All (5)	Cutoff (6)
	A. Individual Level					
No. of individuals	38,705	3,089	24,141	1,955	14,564	1,134
Enrollment rate	.05	.05	.03	.03	.09	.09
Total no. of health-care facility visits	2.3	1.8	2.4	1.9	2.2	1.7
Total value of insurance claims (CFA)	3,525	2,715	3,491	2,760	3,546	2,689
Literate individual	.17	.16	.10	.09	.29	.29
	B. Household Level					
No. of HHs	4,820	497	2,993	310	1,827	187
Share of targeted HHs	.20	.58	.20	.58	.21	.58
Normalized wealth rank	.32	.00	.32	.00	.31	−.00
Enrollment rate	.10	.09	.06	.05	.15	.16
HH size	7.50	5.80	7.53	5.89	7.44	5.64
Any educated HH member	.19	.20	.12	.13	.32	.32
Nonagricultural employed head of HH	.30	.26	.16	.14	.54	.47
Share of female HH members	.49	.50	.48	.48	.49	.53
Share of HH members by age:						
Child (<16)	.45	.42	.48	.45	.41	.37
Prime-aged (16–55)	.40	.40	.38	.39	.44	.42
Elderly (>55)	.15	.18	.14	.16	.16	.21
HH head:						
Widowed	.21	.30	.15	.20	.31	.46
Male	.10	.13	.08	.11	.14	.17
Female	.12	.16	.08	.09	.18	.28
Elderly	.41	.45	.39	.39	.45	.54
Male	.34	.36	.35	.36	.33	.36
Female	.07	.09	.04	.03	.12	.18
Prime-aged	.59	.55	.62	.61	.56	.46
Male	.54	.48	.58	.55	.49	.36
Female	.05	.08	.04	.06	.07	.10

Notes. Values are sample means for 2009 and 2010. Cutoff refers to only observations with a normalized wealth rank smaller than 0.05 in absolute value. HH = household.

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