

Chapter 3

Intra-Household Allocations of Micro Health Insurance: No Adverse Selection After All?¹

This chapter analyzes adverse selection in micro health insurance. Health insurance is typically family-based to limit adverse selection within households. I investigate whether such selection occurs empirically, using the case of an individual-based micro health insurance program in rural Nigeria. The empirical analysis applies two-sample two-stage least squares and uses household fixed effects to control for confounding heterogeneity at the household level. In the first stage, estimated in an uninsured control sample, both self-reported and objectively measured baseline health are significant risk factors for follow-up health expenditures. Nevertheless, in the second stage, predicted expenditures do not affect intra-household insurance allocations in a treatment sample. I conclude that adverse selection within households is limited, and that individual-based enrollment potentially solves limited insurance take-up among the poor.

3.1 Introduction

Illnesses and injuries can have severe economic consequences for households without formal health insurance. Informal coping mechanisms like social risk-sharing or savings provide incomplete insurance against health shocks (Gertler and Gruber, 2002; Dercon and Krishnan, 2003). This induces households to borrow or sell their productive assets (Leive and Xu, 2008). Several low-income countries are hence piloting ‘micro health insurance’ - affordable health insurance targeted at the poor. The long-run aspiration is to create sustainable insurance markets for the uninsured informal sector.²

Although these products are often heavily subsidized and appear good value at first glance,

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²For an overview of micro health insurance schemes, see the systematic literature review of impact evaluations in Acharya et al. (2012).

take-up remains low and is very sensitive to price (Thornton et al., 2010; Dercon et al., 2012; Polimeni and Levine, 2012). One explanation for this stylized fact is that households cannot afford paying the premium in one go for all family members, especially when they have limited access to credit. Households might then still be able to enroll a few members of the household. Individual- instead of family-based enrollment hence potentially increases take-up. But when households offered individual insurance select only high-risk members, adverse selection poses a threat to the sustainability of micro health insurance (Akerlof, 1970; Rothschild and Stiglitz, 1976). Not surprisingly, voluntary private health insurance schemes indeed mostly use family- or group-based enrollment.³

The question, then, is whether adverse selection within households really occurs. Earlier studies do not always find evidence of selection on health risk, but focus on developed insurance markets with strict measures against adverse selection (see Cutler and Zeckhauser, 2000, for a review). Further, most studies do not aim at identifying determinants of intra-household insurance allocations and do not necessarily control for confounding unobserved heterogeneity at the household level. Wealth and risk aversion, for instance, correlate negatively with health risk but positively with insurance coverage (e.g. Finkelstein and McGarry, 2006; Doiron et al., 2008). Unconditional on these household characteristics, we might fail to observe adverse selection, even when the household enrolls members on the basis of their health risks.

This study tests for adverse selection in intra-household allocations of micro health insurance, controlling for inter-household heterogeneity. I study the case of a formerly uninsured farming population in rural Nigeria that was recently offered micro health insurance. The scheme does not take strict measures to prevent adverse selection. Participation in the scheme is voluntary and households can enroll members on an individual basis. As a result, a large share of households enroll only a few members and insurance status varies within households. This variation provides a unique opportunity to investigate whether adverse selection occurs conditional on unobserved household-level characteristics.

To test for adverse selection, this study applies two-sample two-stage least squares (TS2SLS), instrumenting intra-household differences in follow-up health expenditures by self-reported and objectively measured baseline health. The first stage is estimated in an uninsured control sample and hence unbiased by price or incentive effects of insurance. This yields a counterfactual prediction of health expenditures for the treatment sample with access to insurance. The second stage tests whether predicted expenditures are higher among insured or uninsured household members. Both stages include household fixed effects to control for unobserved heterogeneity between households.

³This holds for instance for all micro health insurance programs cited in this chapter (Wang et al., 2006; Zhang and Wang, 2008; Thornton et al., 2010; Polimeni and Levine, 2012; Dercon et al., 2012). I am not aware of programs that do not cover the family of the principal enrollee.

The findings suggest that adverse selection within households is limited. Although both self-reported and measured health indicators are significant risk factors for follow-up health expenditures, these factors do not determine insurance take-up. The TS2SLS estimates of adverse selection are relatively small especially conditional on age and gender - factors that the insurance provider can price. Further, program officers appear to play an important role; adverse selection is in particular limited among households nearby a community-based health care provider that actively enrolls clients. These findings are robust to several specifications of the first stage and apply to different subsamples of the population.

This study differs from the existing literature on adverse selection in three respects: the estimation method, the type of health indicators used in the first stage of the analyses, and the unit of enrollment in the program.

To start, most studies on adverse selection in health insurance are unable to estimate counterfactual health expenditures, i.e. what the household would have spent without insurance. Health insurance affects health-seeking behavior and expenditures. Therefore, a positive correlation between health expenditures and take-up does not necessarily imply adverse selection. The few studies on micro health insurance with panel data hence test whether *past* health expenditures and self-reported health are correlated with *future* insurance take-up. For the case of rural China, Wang et al. (2006) and Zhang and Wang (2008) find that households with sick members are more likely to purchase insurance. Parmar et al. (2012) and Dercon et al. (2012) however do not replicate this finding in Burkina Faso and Kenya, respectively.

This correlation test has two limitations. First, being ill is not a perfect proxy of health expenditures. Past illnesses will measure the health risk with error, attenuating observed levels of adverse selection (Polimeni and Levine, 2012). Second, the correlation test says nothing about the magnitude of adverse selection in terms of health expenditures, only in terms of past illnesses. Without subjective expectations of future health expenditures, it is unclear whether future health expenses drive the insurance decision. Sick individuals might for instance enroll because the risk becomes more salient, not because they expect higher health expenditures. The TS2SLS estimator circumvents these issues by predicting counterfactual health expenditures using baseline health and follow-up expenditures in a control sample.

A second contribution is the prediction of health expenditures by multiple health indicators, both self-reported and objectively measured. This allows me to separately estimate selection on objective versus subjective health measures, with the hypothesis that selection on subjective health will be stronger than selection on objective health. In addition, a large number of studies on labor market outcomes demonstrate substantial differences between subjective and objective health measures (for a review, see Currie and Madrian, 1999). Reporting error can bias the estimates of interest when the first stage uses self-reported health (Baker et al., 2004). Even if the

measurement error in self-reported health is non-systematic, the first stage for follow-up health expenditures will suffer from attenuation bias. Multiple health measures and in particular the medical examinations will help reduce this bias.

A final contribution to the literature is the analysis of intra-household variation in enrollment. This allows me to use household fixed effects that control for confounding household-level characteristics such as wealth and risk aversion. The non-random risk factors used as instruments might of course still be correlated with unobserved sources of individual heterogeneity. I condition on individual characteristics to focus as much as possible on differences in health risk. Moreover, unlike earlier research, I at least control for household-level fixed effects, so that the selection identified in this study is truly due to intra-household variation and not related unobserved household characteristics.⁴

To summarize, this study finds no evidence of adverse selection within households. Since households might not be able to pay the bulky insurance premium for their entire family in one go, individual-based enrollment potentially increases take-up, without inducing adverse selection. It is hence worthwhile experimenting with individual-based enrollment to increase access to health insurance at the bottom of the income distribution.

The remainder of this chapter is structured as follows. The next section presents the study context, describes the data, and descriptive enrollment patterns. Section 3.3 introduces the empirical strategy to test for adverse selection within households. Section 3.4 presents the main findings on intra-household insurance allocations. Section 3.5 discusses policy implications. Section 3.6 concludes.

3.2 Study context

3.2.1 Background and intervention

This study analyzes adverse selection in the ‘Hygeia Community Health Care’ (HCHC) program, a micro health insurance scheme in Kwara State, Nigeria. Kwara State is the fourth poorest state in Nigeria in terms of consumption, with a large share of the population relying on subsistence farming. Of its 2.5 million people, a majority - 61.8 percent - lives below the poverty line of one dollar a day.⁵ The health system in this rural state is not exceptional for sub-Saharan Africa with limited awareness, low health care utilization and a high share of health expenditures paid out of pocket.

To strengthen the health system, the Dutch PharmAccess Foundation and Nigerian HMO

⁴Zhang and Wang (2008) and Parmar et al. (2012) use individual fixed effects in their panel data analyses. These capture time-invariant heterogeneity between households, but also eliminate time-invariant differences in members’ health risk. These papers hence investigate the effect of changes in household members’ health risk on changes in insurance decisions, conditional on time-invariant but not on time-variant household characteristics.

⁵Source: National Bureau of Statistics, Nigeria Living Standard Survey 2009/2010, using PPP exchange rates.

Hygeia Ltd. launched the community-based HCHC program. This micro health insurance pilot scheme is funded jointly by the Health Insurance Fund and Kwara State government. Health insurance was new to Kwara State and thus unavailable outside the pilot district. Anecdotal evidence suggests that the program did not induce an influx of migrants from outside the study area.

The program partners with two health facilities located in different towns, and individuals enroll in the program facility of their nearest town. Cross-overs from one town or facility to the other are limited due to high transportation costs. The two facilities differ considerably in their enrollment practices. One facility is public and does not send out enrollment officers to recruit potential clients. The other facility is private and actively involved in enrollment, with its own enrollment team and the option to pay for insurance on credit.

The program did not dictate these distinct enrollment practices. Differences between the two facilities arose spontaneously and are most likely related to differences in incentive structure. The medical director of the private partner facility receives a uniform capitation fee from the insurance provider for every individual enrolled and thus has a strong incentive to enroll a large and healthy risk pool. For the public facility, the Kwara State Ministry of Health administers and controls these capitation fees. Doctors in this facility have no private incentive to enroll clients.

Enrollment in the program is voluntary, subsidized, and at the individual level instead of family-based. There is no community rating, deductible, co-payment or waiting period to use health care - not even for pre-existing conditions. In the first two years, the total yearly premium was 4,000 Naira per person (US \$26.67, which was based on the per capita costs of the existing National Health Insurance Scheme available to formal and public sector employees). Households themselves pay 300 Naira (US \$2) per year for every insured member. This is 7.5 percent of the total premium, and 23.1 percent of yearly per capita health expenditures before the program was introduced.⁶

Subsidies and discounts helped increase take-up of previous micro health insurance schemes (Thornton et al., 2010; Dercon et al., 2012; Parmar et al., 2012; Polimeni and Levine, 2012). Potentially, discounts raise demand by relaxing liquidity constraints. These constraints however do not fully explain the low take-up puzzle. Another important barrier to take-up is a lack of trust in the insurer or health care provider (Dercon et al., 2011). To increase trust, the HCHC program mobilizes local communities for marketing and offers comprehensive benefits including outpatient and inpatient care as well as the treatment of chronic disease.⁷

Finally, improving the quality of supplied health care is an integral component of the program.

⁶The program is planning to reduce the subsidy over time and the Kwara State government has the intention to take over the remaining subsidies in the long run.

⁷After collecting the data for this chapter, the benefit package became more restricted. Comprehensive benefits were offered during the time frame of interest.

In Dong et al. (2009) and Platteau and Ugarte (2013), low perceived quality of health care provided within the program is associated with lower take-up. A key innovation of the HCHC approach is that it upgrades and closely monitors partnering health facilities. Both insured and uninsured can access the refurbished program facilities, but the latter group pays for health care out-of-pocket. Thus, households select into a program that pre-pays their health care in upgraded facilities. The control group has no access to upgraded facilities. This should be kept in mind when interpreting the results.

A short-term impact evaluation (Gustafsson-Wright et al., 2013) shows that the program is worth paying the premium, assuming that households have actuarially unbiased expectations of future health expenditures. Households in the program area are significantly more likely to access health care, which will be the combined impact of facility upgrades and price effects. Despite increased utilization, households save on average 1,030 Naira on health expenditures - more than three times 300 Naira, the premium paid out-of-pocket by enrollees. The average treatment effect on the treated, i.e. the impact for enrolled individuals, is not homogenous across the population. Access to health care increased more among adults and females than among children and males, and people in towns benefit more than those in nearby villages.

3.2.2 Description of the data

The analysis uses a rich panel dataset with 5,526 individuals from 1,175 multi-person households.⁸ The data include a baseline survey completed before the launch of the HCHC, and a follow-up round two years later. Enumerators collected socioeconomic and biomedical details about all household members, as well as anthropometric measurements, blood pressure, and a blood sample. The blood tests do not significantly affect the outcomes studied here and are available only for a subsample. Therefore, I do not use them in the analyses.

Table 3.1 breaks down the sample by district. The survey includes the program (treatment) district and a nearby control district with a comparable baseline health infrastructure but no access to the program. The three main towns in these districts are a control town and the two program towns with a public versus private partner facility. The sample includes randomly selected households within ten kilometers from these towns, clustered by census Enumeration Area ('EA') and stratified on town versus village. Households are sampled proportional to EA size such that the sample is representative of the population within strata. 40 and 60 EAs were sampled in the control and treatment districts, respectively, half of which are located in a town.

Panel A in Table 3.1 summarizes the number of households ('HH') and individuals ('N') surveyed. In total, 2,265 members from 489 control households participated in the baseline

⁸The number of interviewed households, including single-person households, is 1,450. The data presented here exclude single-person households and will therefore differ from descriptive statistics reported in Gustafsson-Wright et al. (2013).

Table 3.1: Sample size at baseline

	Control		Treatment		Treatment by Area			
	Total sample		Total sample		Near public		Near private	
	HH	<i>N</i>	HH	<i>N</i>	HH	<i>N</i>	HH	<i>N</i>
A. Full sample								
Total at baseline	489	2265	686	3261	363	1715	323	1546
- <i>Town</i>	256	1106	325	1564	203	976	122	588
- <i>Rural</i>	233	1159	361	1697	160	739	201	958
Traced at follow-up	373	1587	599	2630	318	1419	281	1211
- <i>Town</i>	210	846	285	1264	181	816	104	448
- <i>Rural</i>	163	741	314	1366	137	603	177	763
B. Adults only								
Total at baseline	374	905	600	1506	325	805	275	701
- <i>Town</i>	188	454	282	709	181	455	101	254
- <i>Rural</i>	186	451	318	797	144	350	174	447
Traced at follow-up	259	600	509	1243	285	698	224	545
- <i>Town</i>	141	327	239	587	162	402	77	185
- <i>Rural</i>	118	273	270	656	123	296	147	360

Control sample: households from district without access to health insurance. Treatment sample: households from district where program was launched after baseline. HH: Number of households. *N*: Number of respondents. Sample sizes exclude households with data available for at most one household member at baseline. 'Traced at follow-up' drops households with follow-up data for at most 1 household member in Panel A and adult in Panel B.

survey, versus 3,261 members from 686 treatment households (see the first row). The second row drops households with follow-up survey data for at most one member. The remaining subsample, used in the main analysis, consists of 1,587 members from 373 control households and 2,630 members from 599 treatment households. This sample is henceforth referred to as the ‘panel’. For completeness, Panel B summarizes these statistics for the subsample of adults, excluding households with at most one adult.

Table 3.2 summarizes baseline statistics. The first four columns describe the panel, corresponding to the second row in Table 3.1. The first column indicates the total number of observations with a non-missing value and the second column the sample average. The third column presents the overall standard deviation for continuous variables and the fourth column the standard deviation within households, i.e. after subtracting household averages. The remaining columns describe the non-panel subsample, with at least two members interviewed at baseline but not at follow-up.

Panel A describes baseline household characteristics. Half of the sample lives in one of the three towns, and 61.4 percent in the treatment district. Households have up to sixteen - and on average almost five - members. Yearly consumption per capita is on average 84,800 Naira (US \$ 565, or \$ 1.55 per day). Wealth is the first principal component from a factor analysis of housing characteristics, household assets and livestock, standardized with mean 0 and variance 1. Less than half of all household heads can read and write and the same number completed at least primary education. Subsistence farming is the main economic activity in the area, followed by trading, which is mostly done by women.

Panel B presents individual demographic characteristics. At baseline, individuals are on average 24.7 years old. About half of them are female and 82.4 percent are Yoruba, the ethnic majority in Kwara State. Among the Yoruba, the relation to the household head determines an individual’s seniority within the household (Oyewumi, 2002). All else equal, birth members of the family are more senior than the 44.9 percent of adult respondents who join the household in other ways, for instance through marriage. They are called ‘Iyawo’.⁹ Among adult respondents, 81.5 percent worked for salary, profits or family gain. Ethnic majority is the only variable that hardly varies within households.

Panel C summarizes health expenditures reported at baseline and follow-up. In the 12 months before baseline, 28.9 percent of all individuals have been ill or injured at least once. When ill, households spend on average 3,380 Naira (US \$22.50, or more than 10 times the subsidized insurance premium of 300 Naira) on treatment. The prevalence of chronic disease is lower, but treatment more expensive. Among adults, 11.1 percent self-report having a chronic disease, on which they spent on average 17,100 Naira (US \$114) in total over the last year. Note that

⁹The common interpretation of this word is ‘wife’, but also male household members can be considered ‘Iyawo’.

Table 3.2: Description of the study population

	In Panel				Not in Panel		<i>t</i> -stat
	<i>N</i>	Mean	st.dev.	within	<i>N</i>	Mean	
A. Household-level characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	976	61.4			199	43.7	3.70**
Town	976	50.7			199	43.2	1.50
HH size	976	4.89	(2.26)		199	3.82	5.62**
Yearly consumption (1,000 NGN)	976	84.8	(66.7)		199	86.3	-0.30
Wealth (standardized)	976	0.05	(1.02)		199	-0.16	2.35*
Literate HH head	973	47.0			197	43.6	0.84
<i>Highest level of completed education household head</i>							
- Primary	968	19.8			197	19.3	0.16
- Secondary	968	16.8			197	16.8	0.03
- Tertiary	968	10.6			197	8.63	0.97
<i>Main source of income household head</i>							
- Farming	914	46.1			179	45.3	0.15
- Trading	914	12.5			179	14.0	-0.48
B. Individual demographic characteristics							
Age	4766	24.7	(22.0)	(19.2)	759	24.9	-0.21
Female	4755	52.3		(46.2)	757	51.8	0.33
Ethnic majority	4763	82.4		(4.56)	759	68.4	2.62*
Iyawo	2243	44.9		(45.9)	367	54.2	-5.36**
Worked last year	2242	81.5		(29.6)	367	85.3	-1.74 ⁺
C. Health expenditures							
<i>Baseline</i>							
Was ill/injured	4735	28.9		(28.4)	757	28.0	0.26
- Acute expenditures (1,000 NGN)	1362	3.38	(15.4)	(9.81)	211	1.66	3.93**
Chronic disease	2224	11.1		(21.3)	365	10.7	0.22
- Chronic expenditures (1,000 NGN)	239	17.1	(58.6)	(23.0)	39.0	7.21	2.0*
<i>Follow-up</i>							
Was ill/injured	4162	28.5		(32.4)			
- Acute expenditures (1,000 NGN)	1162	2.49	(12.6)	(5.52)			
Chronic disease	1998	16.6		(24.8)			
- Chronic expenditures (1,000 NGN)	316	3.84	(9.61)	(3.29)			
D. Self-reported and measured health status							
Low on 10-step health ladder	4737	13.7		(25.1)	756	11.4	1.44
Disability level	2224	0.17	(0.46)	(0.31)	365	0.13	1.29
BMI category							
- Underweight	4181	6.29		(20.2)	665	7.07	-0.70
- Overweight	4181	10.7		(26.8)	665	11.3	-0.43
- Obese	4181	5.09		(18.7)	665	5.11	-0.02
Blood pressure							
- High (BP ≥ 130/85 mmHg)	2177	29.2		(33.5)	354	27.7	0.58
- Grade 1 HT (BP ≥ 140/90 mmHg)	2177	10.7		(22.3)	354	11.6	-0.48
- Grade 2 HT (BP ≥ 160/100 mmHg)	2177	8.04		(19.4)	354	9.04	-0.59
Observations	4766				760		

'Panel': Surveyed at baseline and follow-up, and at least one other household member as well; otherwise 'Not in panel'.

Binary variables in %, 1,000 Naira is USD 6.67. *t*-statistic clustered by EA. Columns (1) and (5) give number of responses;

Columns (2) and (6) averages; Column (3) the standard deviation; Column (4) the within-household variance and

Column (8) tests for equal means. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

follow-up expenditures are much lower; a potential impact of introducing insurance.

Panel D presents a number of medical indicators measured during the baseline survey. When self-rating health on a ten-step ladder, ranging from very poor (first step) to very high (tenth step), a mere 13.7 percent of respondents reports to be on a 'low' step, i.e. the eighth step or lower. The disability level measures the extent to which adult respondents face difficulties carrying out daily activities, given their current health. This subjective variable is scaled from zero (no disability, 85.5 percent) to three (unable to do daily activities).

The remaining variables are objectively measured. Based on body-mass indices, 6.29 percent of all individuals are underweight and 15.79 percent are overweight or obese.¹⁰ Even in this relatively rural population, cardiovascular risk is a real concern for adult health. Measured blood pressure at baseline is at increased levels among 29.2 percent of all adults, another 10.7 percent has levels associated with grade 1 hypertension, and 8.04 percent is diagnosed with grade 2 hypertension. Also these medical characteristics vary substantially within households.

The panel contains observations for 86.2 percent of all individuals surveyed at baseline, including a few migrants who were interviewed in their new location. Among non-panel individuals, the vast majority, 75.4 percent, migrated away from their baseline community and could not be traced at follow-up. Another 5.03 percent died between the baseline and follow-up surveys, and 6.53 percent of attrition is due to other reasons, for instance refusals to participate. The remaining 11.1 percent drops from the panel because none of their baseline household members were interviewed at follow-up.

Attrition potentially creates a selection bias, since it is correlated with observed and most likely also unobserved characteristics. Attrition is lowest in large households from the treatment district with higher wealth. Panel individuals are more likely to be Yoruba, and less likely to be Iyawo or work at baseline. Finally, conditional on being ill or injured, they report higher health expenditures at baseline (Panel C). Chapter 3.4.5 discusses to what extent selective attrition biases the main findings.

3.2.3 Enrollment

Despite the subsidy and quality improvements, 58.6 percent of panel households did not have an insured member during the follow-up survey. Most uninsured households attribute this to problems associated with the registration and renewal process, for instance not being at home when the enrollment officer is around, followed by a lack of money. Another puzzle is that insured households do not enroll all members. Among insured panel households nearby the public partner facility, only 34.4 percent enrolls the entire household. One hypothesis is that households cannot

¹⁰For children, BMI is recoded according to the Childhood Obesity Working Group of the International Obesity Taskforce cutoff values. For adults, a BMI lower than 18.5 indicates that the person is underweight, a BMI between 18.5 and 25 is normal, a BMI between 25 and 30 is associated with overweight adults and a BMI above 30 means that the person is obese. BMI is calculated as weight (in kilograms) divided by squared height (in meters).

afford paying the insurance premium for their entire family in one go. Consistent with this hypothesis, family enrollment is somewhat higher at 53.0 percent in the area with a private facility, where the loans provided by the medical director will have reduced the cost of credit.

Table 3.3 investigates whether liquidity constraints can explain limited family enrollment. Each column estimates a logit model for insurance status as dependent variable. To explain insurance status, the model includes dummy variables to indicate whether the household lives in a program town and household size; a continuous variable for wealth; and interactions between wealth and household size terciles. Columns (1), (4) and (7) also include a variable to indicate households living near the private partner facility. Every panel household in the treatment sample represents one observation. The table presents marginal effects and standard errors are clustered by Enumeration Area.

Column (1) estimates the model for the probability that at least one household member enrolls in insurance. Enrollment is 20.9 percentage points higher in towns with program facilities than in rural villages. The private partner facility, actively trying to increase enrollment through credits, did not significantly increase the share of households enrolling at least one member. In general, higher wealth is associated with higher enrollment rates, especially in small households. Given the large subsidies, the premium for one member will have been affordable. Columns (2) and (3) estimate the same model, restricting the sample to households living nearby the public and private partner facility, respectively.

The remaining columns analyze full household enrollment. Column (4) estimates a logit model for the probability that all household members have insurance. Family enrollment is 11.1 percentage points higher in towns than in villages, and 10.5 percentage points higher near the private partner facility than near the public facility. Further, in Columns (5) and (6), full household enrollment is reducing significantly in household size only near the public partner facility.

Columns (7) to (9) finally repeat Columns (4)-(6) for the subsample of households with at least one insured member. Among small households and households nearby the private partner facility, household size does not significantly determine enrollment, and there is no longer a significant wealth effect. Only in the area with the public facility, larger households with lower wealth are significantly less likely to fully enroll. Notably, enrollment patterns within the same program differ substantially between the two program areas.

To summarize, the vast majority of households does not enroll their entire family despite the heavily subsidized insurance premium and the positive impacts that the program achieved over the first two years (see Section 3.2.1). This is true in particular among larger households with lower wealth in the area with a public partner facility, where they could not pay the premium on credit. These descriptive findings suggest that the opportunity cost of money is a barrier to

Table 3.3: Enrollment by household characteristics

	At least one member		All members			All members At least 1			
	(1) All	(2) Public	(3) Private	(4) All	(5) Public	(6) Private	(7) All	(8) Public	(9) Private
A. Location									
Town	0.209** (0.053)	0.230** (0.068)	0.181** (0.087)	0.111** (0.037)	0.048 (0.039)	0.180** (0.064)	0.043 (0.062)	-0.120 (0.080)	0.176** (0.089)
Private	0.056 (0.053)			0.105** (0.036)			0.184** (0.054)		
B. Household size									
- Four to six members	-0.018 (0.046)	-0.044 (0.067)	0.016 (0.057)	-0.050 (0.040)	-0.066 (0.042)	-0.035 (0.070)	-0.103 (0.075)	-0.098 (0.101)	-0.071 (0.124)
- Seven members or more	-0.040 (0.055)	-0.016 (0.069)	-0.066 (0.078)	-0.066 (0.045)	-0.112** (0.045)	-0.030 (0.076)	-0.106 (0.082)	-0.236** (0.103)	0.057 (0.134)
C. Wealth									
- At HH 2 to 3 members	0.150** (0.062)	0.084 (0.068)	0.236** (0.053)	0.069** (0.024)	0.059** (0.026)	0.093** (0.040)	0.045 (0.043)	0.100+ (0.055)	-0.057 (0.101)
- At HH 4 to 6 members	0.039 (0.024)	0.046 (0.030)	0.035 (0.041)	0.008 (0.021)	0.016 (0.030)	-0.001 (0.029)	-0.008 (0.042)	0.004 (0.062)	-0.033 (0.054)
- At HH 7+ members	0.062+ (0.038)	0.029 (0.050)	0.109** (0.055)	0.055** (0.022)	0.048** (0.020)	0.048 (0.041)	0.087 (0.056)	0.115** (0.040)	-0.021 (0.105)
Observations	599	318	281	599	318	281	248	131	117
Clusters	60	26	34	60	26	34	56	26	30
Pseudo R-squared	0.074	0.069	0.095	0.069	0.060	0.070	0.044	0.061	0.028
Mean dependent variable	0.414	0.412	0.416	0.179	0.142	0.221	0.431	0.344	0.530

Marginal effects: (d) for discrete change of dummy variable from 0 to 1. Standard errors in parentheses clustered by Enumeration Area.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.05$.

enrollment. Households have selectively enrolled household members into the program. The main question is whether the intra-household insurance allocation is driven by differences in health risk, resulting in adverse selection.

3.3 Econometric strategy

3.3.1 Empirical specification

The identification strategy uses two-sample two-stage least squares, henceforth TS2SLS. A key advantage of TS2SLS is that it can estimate the 2SLS moment conditions in two different samples (Angrist and Krueger, 1992). This is useful when one dataset contains an outcome variable (insurance status) and exogenous instruments (health at baseline), whereas another dataset includes the endogenous regressor (health expenditures without having access to insurance) and the exogenous instruments.

Following the exposition in Angrist and Krueger (1995), consider the following two-equation model for the dependent variable - insurance demand $d_{ih|T}$ - and the endogenous regressor, expected health expenditures if not offered health insurance, $e_{ih|T}^*$:

$$d_{ih|T} = \beta_0' w_{0ih|T} + \beta_1 e_{ih|T}^* + \eta_{h|T} + u_{ih|T} \equiv x_{ih|T} \beta + \eta_{h|T} + u_{ih|T} \quad (3.1)$$

and

$$x_{ih|T} = \Pi_0' w_{0ih|T} + \Pi_1' w_{1ih|T} + \zeta_{h|T} + v_{ih|T} \equiv z_{ih|T} \Pi + \zeta_{h|T} + v_{ih|T} \quad (3.2)$$

for $i = 1, \dots, N$ observations and $h = 1, \dots, H$ households, where $T = 0$ and $T = 1$ indicate the control and treatment sample, respectively, and $z_{ih|T}$ is a $(p+k) \times 1$ vector of regressors, including (i) $w_{0ih|T}$ - the p exogenous variables appearing in Equation (3.1) - and (ii) $w_{1ih|T}$ - the k variables used to instrument health expenditures. Thus there are k excluded instruments and $k-1$ over identifying restrictions. Household fixed effects $\eta_{h|T}$ and $\zeta_{h|T}$ reflect unobserved heterogeneity in household characteristics, for instance location, wealth, or the household head's risk preferences. These can be correlated with the regressors in vector $x_{ih|T}$ and with the instruments $z_{ih|T}$.¹¹

Adverse selection occurs when the probability of enrolling is increasing in health expenditures, $\beta_1 > 0$. The main challenge is to identify this parameter without observing expected health expenditures. Actual health expenditures are endogenous since the program decreases health expenditures by covering enrollees' health expenditures, and increases expenditures through incentive effects. These observed expenditures are hence correlated with the disturbances in the structural equation, $u_{ih|T}$, creating either a positive or negative bias in the OLS estimate of β_1 in

¹¹These fixed effects are not included in Angrist and Krueger (1995)'s original model.

Equation (3.1).

To identify the intra-household effect of expected health expenditures on insurance take-up, we need an instrument that is both exogenous and varies within households. Health indicators satisfy the latter criterion. However, if adverse selection occurs, these risk factors are endogenous in a first-stage regression for actual health expenditures. This is because they explain variation in expected expenditures, correlated with insurance take-up, which in turn affects actual expenditures through the first-stage residual $v_{ih|T}$.¹²

I therefore adopt an alternative two-sample approach that includes the quasi-experimental control district where health insurance was unavailable. The first stage is estimated on this control sample so that insurance has no impact on actual health expenditures. The cross-sample prediction of expenditures then proxies expected expenditures assuming that individuals in the treatment sample would not have been able to access the program. The second stage estimates the difference in these counterfactual expenditures between the insured and uninsured in the treatment sample.

The TS2SLS estimator requires a cross-sample prediction of health expenditures but not of the estimated household fixed effects. To see this, denote individual i 's deviation from mean demand $\tilde{d}_{ih|T} = d_{ih|T} - \bar{d}_{h|T}$, where $\bar{d}_{h|T}$ is the proportion of insured household members. Using a similar notation for the regressors, and - without loss of generalization - assuming that $\bar{v}_{h|T} = \bar{u}_{h|T} = 0$, the estimating equations based on (3.1) and (3.2) are:

$$\tilde{x}_{ih|0} = \tilde{z}_{ih|0}\Pi + v_{ih|0} \quad (3.3)$$

and

$$\tilde{d}_{ih|1} = \tilde{x}_{ih|0}\beta + u_{ih|1} \quad (3.4)$$

where $\hat{x}_{ih|01}$ is the cross-sample prediction from the first stage. This vector includes the first-stage prediction of health expenditures $\hat{e}_{ih|01}$, and the exogenous regressors, $w_{0ih|1}$. $\tilde{x}_{ih|01}$ is the deviation from household averages.

This model with transformed regressors eliminates unobserved heterogeneity at the household level and resembles any standard TS2SLS framework. TS2SLS is hence consistent under the regular exclusion restriction that \tilde{z}_{ih} is uncorrelated with v_{ih} and u_{ih} , and the condition that \tilde{z}_{ih} is correlated with \tilde{x}_{ih} (see Angrist and Krueger, 1995; Inoue and Solon, 2010). A third assumption

¹²Polimeni and Levine (2012) instrument the insurance decision through randomly assigned discounts to get around this problem, but such random discounts typically do not vary within the household and can hence not be used to estimate intra-household adverse selection.

provides the basis for combining the two samples:

$$E(\tilde{z}'_{ih|0}\tilde{x}_{ih|0}) = c E(\tilde{z}'_{ih|1}\tilde{x}_{ih|1}) \text{ and } E(\tilde{z}'_{ih|0}\tilde{z}_{ih|0}) = c E(\tilde{z}'_{ih|1}\tilde{z}_{ih|1}) \quad (3.5)$$

for some c (Inoue and Solon, 2010). The next subsection discusses the validity of this assumption. Note that although consistent, the TS2SLS estimator is biased towards zero in finite samples. The findings thus provide a lower bound of adverse selection.¹³

The results section presents estimates of (3.3) and (3.4) for three types of expenditures reported in the follow-up survey: (i) acute health expenditures in the total sample, (ii) acute expenditures in the adult subsample, and (iii) chronic expenditures in the adult subsample. Selection on chronic expenditures is estimated only for the adult subsample, because the prevalence of chronic disease is low among children. The following log transformation is used to deal with outliers and zero health expenditures among a large number of respondents: $\log(e_{ihT} + 1)$. Tobit models for expenditure levels, e_{ihT} , yield qualitatively similar results but are less robust and have a weaker first stage.

Instruments include both subjective and objective health indicators described in Section 3.2.2. Acute health expenditures in the total sample are instrumented by binary variables for low health on the ten-step health ladder ('Low on ladder') and for being overweight or obese ('Overweight'). Instruments for the adult subsample are the lag dependent variable - log acute baseline health expenditures - and BMI z -scores.¹⁴ To instrument chronic expenditures, I use grade 2 hypertension ('Grade 2 HT'), self-reported disability levels ('Disability'), and the lag dependent variable - log chronic baseline expenditures.

The first and second stage control nonlinearly for age, interacted with gender. The analyses on the adult subsample also include variables that indicate working household members and the Iyawo, i.e. members who joined the family not through birth but for instance marriage. These controls proxy intra-household heterogeneity in income-generating capacity and bargaining power, potentially correlated with both health and insurance allocations. Conditional on these controls, the health indicators are likely interpreted as exogenous, affecting intra-household insurance allocations only via expected expenditures.

On a final note, the econometric procedure estimates both the first and the second stage with fixed effects. Naive standard errors will ignore the noise in the first-stage prediction of health

¹³A practical question is whether to use 2SLS or instrumental variables (IV). When using two samples, instrumental variable (IV) and 2SLS estimators do not produce equal results, not even in the case of a just-identified model. Inoue and Solon (2010) show that TS2SLS is more robust than two-sample IV, which requires $c = 1$ in Equation (3.5). TS2SLS is also asymptotically more efficient than the IV equivalent.

¹⁴Standardized BMI scores depend on a child's age, which are measured with error, but do not depend on age among adults. In the specification with the total sample, I use a dummy for being overweight to reduce measurement error in z -scores.

expenditures and will therefore underestimate true standard errors (Murphy and Topel, 2002). Standard errors are hence calculated by bootstrapping the full model. Bootstrap replications sample all households within an Enumeration Area to yield clustered standard errors.

3.3.2 Identifying assumptions

A necessary condition for consistency of $\hat{\beta}$ is the validity of Equation (3.5). This condition implies that the true first-stage parameters, Π_0^* and Π_1^* , are equal across the two samples:

$$\Pi_0^* = (\tilde{Z}'_0 \tilde{Z}_0)^{-1} \tilde{Z}'_0 \tilde{X}_0^* = (\tilde{Z}'_1 \tilde{Z}_1)^{-1} \tilde{Z}'_1 \tilde{X}_1^* = \Pi_1^*$$

where Z_0 and Z_1 are the $N_0 \times (p + k)$ and $N_1 \times (p + k)$ matrices with instrumental variables for the control and treatment sample, respectively, and X_0^* and X_1^* the $N_0 \times (p + 1)$ and $N_1 \times (p + 1)$ matrices with p exogenous regressors and expected health expenditures for individuals without access to insurance, e^* .

I indirectly test the condition above using lagged health expenditures - as reported at baseline - since insurance was unavailable at that stage. Table 3.4 estimates the following fixed-effects equation for baseline expenditures x_{ih}^0 :

$$x_{ih|T}^0 = z_{ih|T} \delta_1 + z_{ih|T} T_h \delta_2 + \eta_{h|T} + \varepsilon_{ih|T}$$

A necessary condition to combine the two samples is that the k risk factors in $z_{ih|T}$ have the same coefficient in both treatment, $T = 1$, and control, $T = 0$. This is rejected if the interaction term for treatment T and risk factors in z_{ih} is significant, $\delta_2 \neq 0$.

Column (1) in Table 3.4 estimates the equation above for log acute health expenditures, using the full panel sample, including household fixed effects. Column (2) adds age and gender as covariates. The remaining columns restrict the sample to households with at least two adult members and drop children from the analyses. Columns (3) and (4) estimate the equation above for log acute health expenditures. Columns (5) and (6) focus on chronic health expenditures. For both expenditure types, the second column - (4) and (6) - controls for age, gender, work and Iyawa.

None of the interactions for treatment and health risk factors differ significantly from zero, independent of whether the estimates condition on the exogenous covariates. The F -statistic testing for jointly significant treatment-risk interactions is also low in each column and associated with high p -values (not reported). This suggests that the relation between risk factors and baseline health expenditures is consistent across the two samples, validating the TS2SLS approach.

Table 3.4: Balancing of treatment and control districts

	Full sample		Adults only		Adults only	
	Acute expenditures		Acute expenditures		Chronic expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log Exp.	Log Exp.	Log Exp.	Log Exp.	Log Exp.	Log Exp.
A. Health risk factors						
Low on ladder	0.934** (0.222)	0.464* (0.229)				
- Treatment	0.182 (0.290)	0.183 (0.299)				
Overweight	0.522** (0.168)	0.105 (0.182)				
- Treatment	-0.214 (0.227)	-0.044 (0.241)				
BMI (std.)			0.019 (0.087)	0.027 (0.095)		
- Treatment			-0.026 (0.114)	0.012 (0.126)		
Disability					1.910** (0.252)	1.808** (0.289)
- Treatment					0.184 (0.332)	0.145 (0.372)
Grade 2 HT					1.060** (0.348)	0.832* (0.369)
- Treatment					-0.078 (0.472)	-0.007 (0.491)
B. Control variables						
Age, gender		✓		✓		✓
x Adult		✓				
Work, Iyawo				✓		✓
Observations	4303	4299	2202	2202	2270	2266
Mean dep. var.	1.93	1.94	2.36	2.36	0.80	0.80
F Health x Treat	0.61	0.21	0.05	0.01	0.16	0.08

Estimated with household fixed effects and baseline health expenditures. Low on ladder: < step 8 on 10-step health ladder. Grade 2 HT: Grade 2 hypertension (blood pressure \geq 160/100 mmHg). 'Age, gender': Female, Age divided by 10, its square, both interacted with female. 'x Adult': Every age/gender variable interacted with a dummy variable indicating adults. Log expenditures: $\text{Log}(X + 1)$, where X are self-reported expenses on the treatment of acute/chronic disease. Columns (1)-(2) and (3)-(6) drop households with at most one member and one adult, resp. Std. errors in parentheses clustered by EA. $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$

3.4 Results

This section first estimates the first stage, Equation (3.3), and the correlation between insurance take-up and risk factors; then presents estimates of the second stage, Equation (3.4); and concludes by a discussion of selective attrition.

3.4.1 First stage and reduced form

Table 3.5 presents estimates of reduced-form Equation (3.3) for intra-household differences in health expenditures and insurance status as measured during the follow-up survey. Columns (1) to (4) include all households with data available for at least two members. Columns (5) to (7) restrict the sample to adults only, dropping all households with one adult from the analyses.

The first two columns focus on acute health expenditures. Column (1) regresses these expenditures on baseline health, controlling for unobserved heterogeneity between households through household fixed effects. Both subjective and objective health indicators are associated with significantly higher health expenditures at follow-up. Self-rated low health increases log expenditures by 0.942. This is 59.9 percent of average log expenditures. Overweight and obese household members' log health expenditures are 0.855 points or 54.3 percent higher than the sample average. These estimates remain significant when controlling for age and gender in Column (2).

Columns (3) and (4) use the same variables to explain intra-household health insurance allocations in the treatment sample. Household members with low health are 8.2 percentage points more likely to enroll than members in good health. This increases enrollment by 29.3 percent and is statistically significant ($p < 0.10$). Overweight and obese individuals, on the other hand, are not significantly more likely to enroll. In Column (4), conditional on age and gender, also self-rated health is no longer a predictor of insurance take-up.

The remaining columns focus on the adult subsample and control for age, gender, work and Iyawo. Estimates without these control variables are qualitatively similar and not presented here. Column (5) explains log acute follow-up expenditures as a function of log acute baseline expenditures and standardized BMI. Both instruments are significant risk factors. Column (6) presents the first stage for log chronic health expenditures. The respondent's disability level, whether the respondent has grade 2 hypertension, and log chronic expenditures are significant risk factors. Joint with the other covariates, they explain 21.4 percent of the intra-household variation in chronic expenditures.¹⁵

Column (7) finally estimates the effect of health risk factors at baseline on intra-household

¹⁵To avoid overfitting, the analysis of chronic health expenditures includes only these three risk factors, not the risk factors used in the analysis of acute health expenditures. Results for the full model including the two acute health risk factors are qualitatively similar and available upon request.

Table 3.5: First stage and reduced form: Health, expenditures and take-up

	Full sample		Full sample		Adults only		Adults
	Log expenditures		Has insurance		Log expenditures		Insured
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Acute	Acute	Insured	Insured	Acute	Chronic	Insured
A. Health risk factors							
Low on ladder	0.942** (0.301)	0.851* (0.337)	0.082** (0.023)	0.042 (0.025)			
Overweight	0.855** (0.247)	0.739* (0.299)	0.027 (0.023)	-0.002 (0.022)			
BMI (std.)					0.347** (0.125)		0.006 (0.009)
Log acute					0.144* (0.067)		-0.005 (0.004)
Disability level						1.444** (0.406)	0.014 (0.038)
Grade 2 HT						1.369* (0.647)	0.117** (0.042)
Log chronic						0.124* (0.055)	-0.000 (0.006)
B. Other individual characteristics							
Worked last year					-0.409 (0.710)	0.276 (0.481)	-0.001 (0.036)
Iyawo					-0.824 (0.684)	-0.063 (0.388)	0.188** (0.056)
C. Demographic controls							
Age, gender		✓		✓	✓	✓	✓
x Adult		✓		✓			
Observations	1408	1406	2264	2264	556	583	1146
R-squared within	0.024	0.037	0.010	0.044	0.044	0.214	0.080
R-squared overall	0.011	0.017	0.001	0.006	0.019	0.179	0.009
F risk factors	12.1	4.74	6.40	4.29	2.92	11.57	4.79
Mean dep. var.	1.57	1.58	0.280	0.280	1.88	0.865	0.315

Estimated with household fixed effects. 'Age, gender': Age divided by 10, its square, both interacted with female. 'x Adult': Every age/gender variable interacted with a variable indicating adults. Log expenses: $\text{Log}(X + 1)$, X are self-reported expenses on the treatment of acute/chronic disease. Columns (1)-(4) and (5)-(7) drop households with at most one member and one adult, respectively. Standard errors in parentheses clustered by EA.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

insurance take-up at follow-up. Despite a significant correlation between baseline health and follow-up expenditures, baseline health is not correlated with the insurance allocation. The one exception is measured blood pressure, which increases insurance take-up by 11.7 percentage points ($p < 0.01$). This is more than one third of the sample average. None of the other risk factors are significantly correlated with take-up though. Contrarily to our priors, selection occurs on one specific objectively measured risk factor rather than on self-reported health.

The table also demonstrates that households select members on non-health characteristics. Iyawa members are likely to have less bargaining power within the household. However, they are more likely to enroll in Column (7), even conditional on age and gender, and even though they do not have higher follow-up health expenditures. The scheme might have targeted women who married into a household since they are at reproductive age. Because only few observations in the control sample report pregnancy-related expenditures, I cannot estimate selection on this type of health expenditure.

Having work at baseline is not a significant predictor for either health expenditures or insurance status. Assuming that employment status proxies a household members' productivity, Columns (5) to (7) do not yield evidence of health-productivity linkages in the insurance allocation. Thus, neither bargaining models nor productivity differences appear to explain why the correlation between baseline health and follow-up insurance status is small. For this reason, this study will control for these two characteristics but does not focus on the mechanisms in more detail.

Figures 3.1 and 3.2 show how the cross-sample prediction of expenditures and insurance status varies by age and gender. The dots indicate the linear combination of the age, gender and adult coefficients estimated using household fixed effects in Columns (2), (4), and (5) to (7). For tractability, the figures do not draw confidence intervals. These are discussed in the text.

Figure 3.1 plots the prediction from Columns (2) and (4), log acute health expenditures and insurance status, for the full treatment sample. In Panel (a), adults have higher health expenditures than children, and among children, expenditures are linearly decreasing in age. None of the coefficients on variables for age, gender or adult are however statistically significant. In Panel (b), adults are significantly more likely to have insurance ($p < 0.10$). This is mostly driven by women enrolling at reproductive age and older men having insurance.

Figure 3.2 plots the prediction from Columns (5) to (7), log acute and chronic health expenditures, as well as insurance status, for the adult treatment sample. In Panel (a), acute expenditures are higher among females than males, but the differences are statistically not significant. In Panel (b), chronic expenditures are increasing in age for men but not for women. The interaction terms between linear age and female, and quadratic age and female, are indeed statistically significant ($p < 0.05$). In Panel (c), the observed differences are not significant.

Finally, note that most first-stage regressions do not meet the "rule of thumb" that the F -

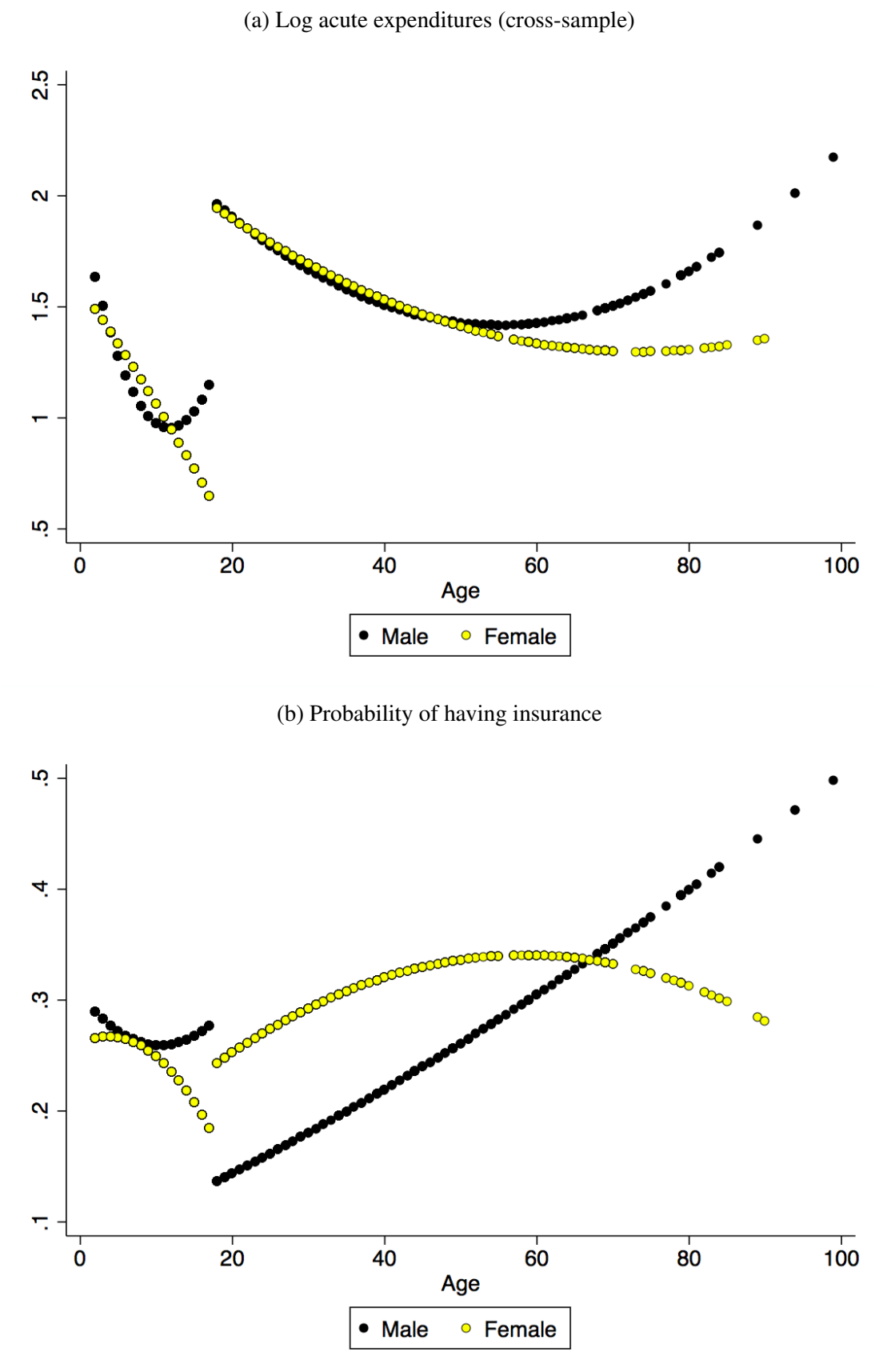


Figure 3.1: Predicted health expenditures by age and gender (all)

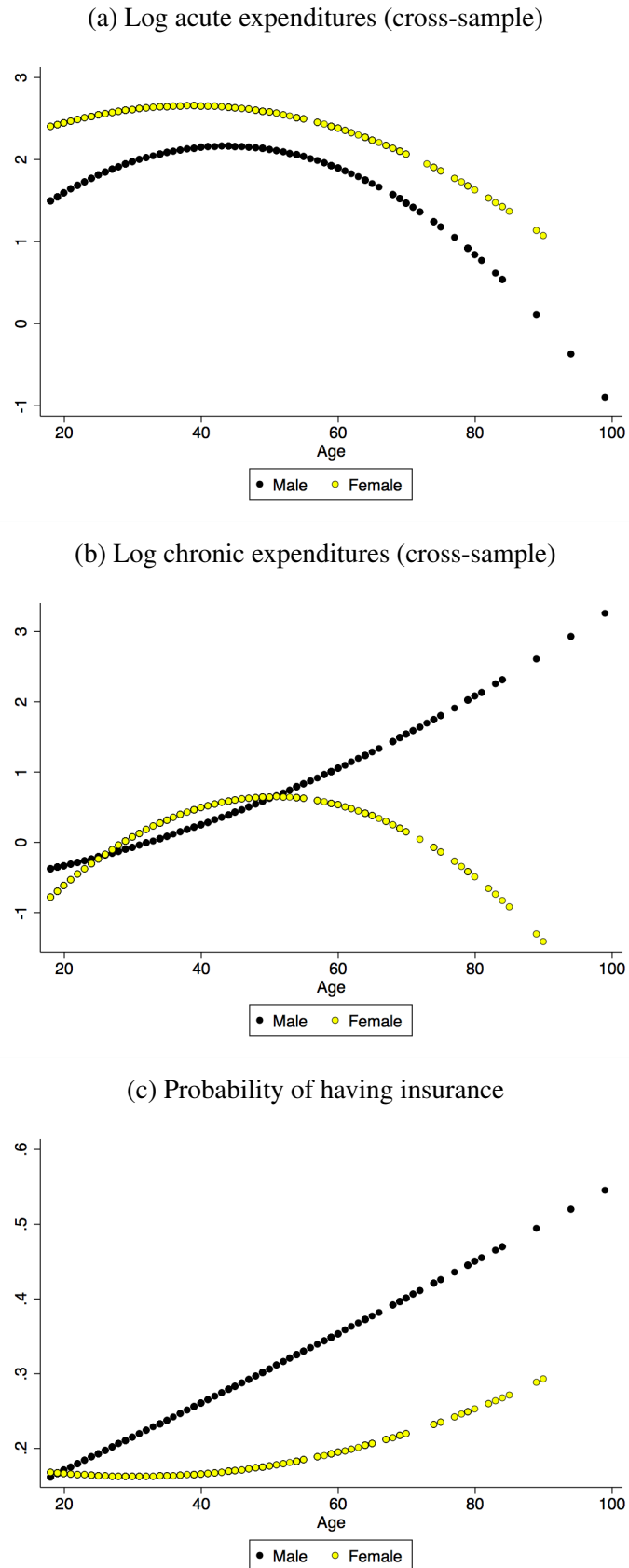


Figure 3.2: Predicted health expenditures by age and gender (adults)

statistic should be at least 10 to avoid a weak instrument problem (Staiger and Stock, 1997). Weak instruments are however more problematic when the first and second stages are estimated on one sample. In that case, 2SLS is biased because we do not know the true first-stage parameters Π^* and estimate $\hat{\Pi}$ instead. The first-stage prediction is correlated with the error in the structural equation, u_i , biasing results in the direction of OLS (Bound et al., 1995). TS2SLS estimates the two stages in distinct samples. This breaks the link between u_i and v_i in Equations (3.3) and (3.4) and eliminates the weak instrument bias in finite-samples (Angrist and Krueger, 1995). A high F -statistic would nevertheless be desirable as it will increase precision of the estimates.

3.4.2 Second stage

Table 3.6 estimates Equation (3.4), relating intra-household insurance allocations to differences in predicted health expenditures. The first row presents the coefficient on the cross-sample prediction of health expenditures, $\hat{\beta}$, for the aggregate treatment sample. Figure 3.3 plots the distribution of the regressors with controls, net of household averages. These cross-sample predictions from the first stage vary substantially within households.

The first two columns in Table 3.6 estimate the effect of log acute health expenditures on insurance take-up, using self-rated low health and measured overweight or obesity as instruments. The first column does not include covariates. A unit increase in predicted log expenditures - the equivalent of three standard deviations - raises the probability of having insurance by 5.7 percentage points ($p < 0.05$). Although statistically significant, this is a minor effect from an economic perspective, as a unit increase in predicted log expenditures is relatively large compared to the shift in enrollment rates.

The second column includes a flexible, nonlinear specification of age and gender. The resulting conditional estimate is small and statistically insignificant. Note that the first-stage F -statistic is lower than in the first column. The point estimate in Column (1) does however not change when using the weaker first stage from Column (2), and remains statistically significant ($p < 0.05$; results available upon request).

The third and fourth column estimate the second stage for adults' acute expenditures. Because households with at most one panel adult are dropped, the sample in Columns (3)-(4) is a subset of the adult sample in Columns (1)-(2). In both samples, predicted log acute expenditures do not significantly affect the intra-household insurance allocation. Unconditional on control variables, a unit increase in log expenditures (around 2 standard deviations) is associated with 3.9 percentage points higher enrollment rates. The conditional estimate in Column (4) reduces to 0.4 percentage points.

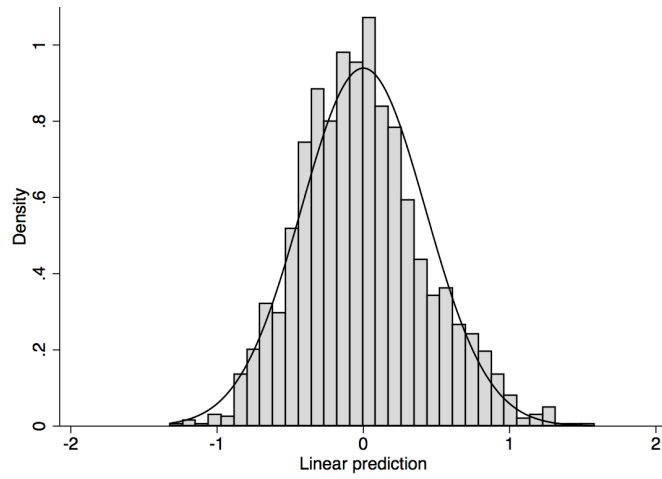
Chronic health expenditures - with higher serial correlation - should be easier for households to predict than acute health expenditures. The fifth and sixth column estimate selection on this expenditure type. The disability level, log chronic baseline expenditures and grade 2 hypertension

Table 3.6: Second Stage: Predicted expenditures and enrollment

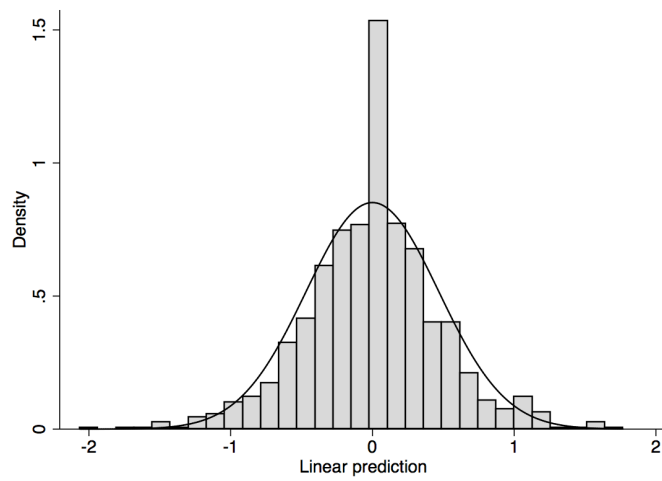
	Full sample		Adults only		Adults only	
	Acute expenditures	Acute expenditures	Acute expenditures	Chronic expenditures	Chronic expenditures	Chronic expenditures
	(1)	(2)	(3)	(4)	(5)	(6)
A. All panel households						
\hat{x}_{01}	0.057*	0.022	0.039	0.004	0.040+	0.023
	(0.027)	(0.030)	(0.032)	(0.023)	(0.021)	(0.020)
F health risk factors	12.08	5.53	5.36	8.12	8.31	5.80
B. Near public or private partner facility						
\hat{x}_{01} x Public	0.088*	0.046	0.046	-0.003	0.047	0.029
	(0.042)	(0.040)	(0.045)	(0.030)	(0.031)	(0.030)
\hat{x}_{01} x Private	0.019	-0.004	0.032	0.007	0.030	0.014
	(0.023)	(0.041)	(0.036)	(0.034)	(0.019)	(0.025)
P Public = Private	0.1314	0.3844	0.7900	0.8137	0.6128	0.6954
C. Above or below median wealth						
\hat{x}_{01} x Above	0.074	0.018	0.053	-0.000	0.029	-0.006
	(0.047)	(0.063)	(0.048)	(0.034)	(0.027)	(0.030)
\hat{x}_{01} x Below	0.043	0.020	0.031	0.004	0.049+	0.044+
	(0.030)	(0.072)	(0.055)	(0.116)	(0.026)	(0.024)
P Above = Below	0.5425	0.9833	0.7593	0.9680	0.6040	0.2089
F health - Above	4.61	2.91	4.38	7.26	16.60	6.72
F health - Below	9.97	3.83	0.87	1.14	5.22	5.90
Observations Panel A-C	2264	2264	1226	1226	1205	1205
D. Male or Female						
\hat{x}_{01} x Male	0.035	-0.006	-0.020	-0.020	0.020	-0.006
	(0.029)	(0.107)	(0.027)	(0.037)	(0.027)	(0.036)
\hat{x}_{01} x Female	0.046+	0.014	-0.001	-0.011	0.018	0.017
	(0.027)	(0.048)	(0.028)	(0.029)	(0.028)	(0.030)
P Male = Female	0.3203	0.8568	0.2874	0.8543	0.9486	0.6064
F health - Male	1.49	0.91	0.53	0.50	14.90	8.22
F health - Female	5.70	3.43	0.95	1.62	2.17	5.75
Observations	1868	1868	913	913	608	608
\hat{x}_{01} - Measured	0.028	-0.002	0.068	0.034	0.072	0.091
	(0.034)	(0.321)	(1.371)	(0.055)	(0.045)	(0.372)
F measured health	12.36	5.57	6.41	7.30	10.11	4.16
Observations	2184	2184	1177	1177	1150	1150
Controls		✓		✓		✓

Estimated with household fixed effects. Controls: 'Age, gender' (all columns); 'x Adult' in Columns (1)-(2); 'Work' and 'Iyawa' in Columns (3)-(6). Instruments in first stage: 'Low on ladder', 'Overweight' in (1)-(2); 'Log acute expenses' and BMI z-score in (3)-(4); and 'Log chronic expenses', disability level, 'Grade 2 HT' in (5)-(6). Excl. households with follow-up data for at most one member. Traced migrants included. Std. errors in parentheses bootstrapped with 499 replications, clustered by EA. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

(a) Log acute expenditures (full sample)



(b) Log acute expenditures (adults only)



(c) Log chronic expenditures (adults only)

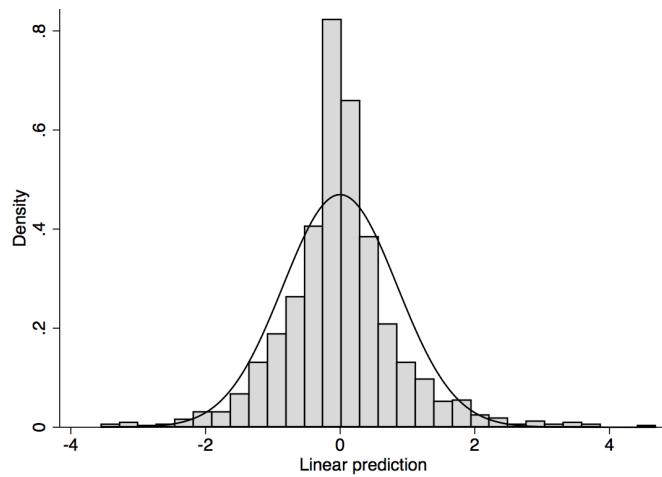


Figure 3.3: Histograms of predicted health expenditures

are used as instruments. A unit increase in predicted log expenditures (equal to 1.5 standard deviations) increases the probability of having insurance by 4.0 percentage points ($p < 0.10$). This is close to the point estimate in Column (3) for acute expenditures, but is estimated with higher precision. Column (4) estimates the effect conditional on a set of controls. The coefficient on expenditures is lower and no longer statistically significant.

Panel B estimates the second stage separately for households near the public and private partner facility, respectively. Only the private partner facility was actively engaged in enrollment. This may have resulted in qualitatively different enrollment patterns. Consistent with this hypothesis, intra-household adverse selection is stronger near the public partner facility. The gap is more pronounced in the full sample than in the adult subsample, but in both samples, the differences are statistically insignificant.

In Section 3.2.3, households with higher wealth are more likely to enroll their entire household. Adverse selection might hence be stronger among low-wealth households. To shed light on this, Panel C estimates the full model separately for households above and below the median wealth level. Selection on acute health expenditures remains limited and does not differ significantly across the lower and upper half of the wealth distribution. There is some evidence of selection on chronic expenditures only below the median wealth level ($p < 0.10$). The absolute size of selection however remains small.

3.4.3 Ruling out a potential bias due to facility upgrading

This raises the question why partially insured households do not select members on the basis of forecasted health expenditures. One explanation is that insurance impacts health-seeking more for some household members than for others, in particular since the introduction of health insurance was combined with the upgrading of health facilities. In case these facility upgrades had a heterogeneous impact, health expenditures in the control group are no longer a good proxy for health expenditures in the treatment district. Predicted health expenditures in the control sample reflect actual health risk only if the impact of health facility upgrades is homogeneous across the sample.

Panel D therefore estimates the two-stage model separately for males and females. The average treatment effect on the treated in terms of health care utilization is higher for insured females than for insured males (Gustafsson-Wright et al., 2013). At baseline, women were more likely to consult a public health provider, with lower fees, than their male household members, who were more likely to consult private health providers. After the introduction of the program, women gained access to quality health care in upgraded facilities, so that for them the predicted expenditures in the control group might not be a good proxy. Assuming that this is much less of a concern in the male subsample, we would expect to observe stronger selection in the male subsample.

The estimates for the two subsamples are however similar or even lower than the estimates in Panel A, and do not differ significantly between the male and female subsample.¹⁶ The facility upgrades could in theory have a stronger impact for women than for men, biasing the cross-sample prediction of health expenditures. But since the second-stage estimates are quite similar for men and women, I cannot attribute the lack of adverse selection to the facility upgrades.

3.4.4 The ability to predict future expenditures

An alternative explanation for limited selection on health expenditures is that decision-makers have limited information about health risk factors. Awareness of cardiovascular risk is for instance low. Among all respondents diagnosed with hypertension in the medical examination at baseline, 92 percent were unaware (Hendriks et al., 2012). Enumerators informed these respondents and urged them to seek health care. They did not give information about the potential costs of other risk factors such as a high BMI. This could explain selection on high blood pressure in Table 3.5, but not the limited selection on self-reported health.

Table 3.7: Selection on subjective versus objective health risk

	Full sample		Adults only		Adults only	
	Acute expenditures		Acute expenditures		Chronic expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Self-reported health as instrument						
\hat{x}_{01}	0.077 (0.061)	0.040 (0.062)	0.005 (0.079)	-0.014 (0.058)	0.029 (0.021)	0.015 (0.021)
<i>F</i> first stage	11.81	7.22	4.60	4.89	13.53	8.28
Observations	2620	2620	1323	1323	1322	1322
B. Measured health as instrument						
\hat{x}_{01}	0.034 (0.035)	-0.003 (0.244)	0.071 (1.385)	0.027 (0.053)	0.079 (0.049)	0.090 (0.364)
<i>F</i> first stage	12.36	5.57	6.41	7.30	10.11	4.16
Observations	2267	2267	1231	1231	1212	1212
Age, gender		✓		✓		✓
Age, gender x Adult		✓				
Work, Iyawo				✓		✓

Estimated with household fixed effects. Instruments first stage: 'Low on ladder' (self-reported), 'Overweight' (measured) in (1)-(2); 'Log acute expenses' (self-rep.), BMI z-score (measured) in (3)-(4); and 'Log chronic expenses', disability level (self-rep.), 'Grade 2 Hypertension' (measured) in (5)-(6). Excl. households with follow-up data for at most one member. Traced migrants included. Bootstrapped std. errors in parentheses are clustered by EA. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

¹⁶The analyses omit non-acute maternal health care because only 44 female respondents in the control sample reported expenditures on this type of health care. Further, pregnancy at baseline is omitted as risk factors since it does not significantly affect follow-up health expenditures in the control sample or insurance take-up in the treatment sample. One explanation is that the time span between baseline and follow-up is too long; women pregnant at baseline have to renew at least once after delivery in order to be covered at follow-up.

As a robustness check, Table 3.7 therefore presents results for two alternative specifications of the first stage: one using self-reported instruments, and the other using measured health. Both are significant determinants of follow-up health expenditures as shown in Table 3.5. Blood pressure is the measured health instrument in the last two columns. Adverse selection remains limited under both specifications of the first stage, with statistically insignificant estimates of the effect on health risk. In the full sample, selection on acute health expenditures is strongest using self-rated health in the first stage. Conversely, among adults, the prediction based on measured risk factors results in higher adverse selection.

In sum, I find no evidence of adverse selection within households. To the extent that it occurs, it is relatively modest and disappears when controlling for age and gender. Adverse selection unconditional on these observable characteristics only occurs among households near the public partner facility. When splitting the sample by wealth or gender, there is no differential selection. This suggests that pre-program barriers to use health care and the combination with facility upgrading cannot account for limited observed selection. Finally, using alternative specifications of the first stage, I find no support of the hypothesis that households have limited information about health risks so that they select on subjective but not on objective health.

3.4.5 Selective attrition

The panel includes 86.2 percent of the baseline sample. Selective attrition related to health characteristics potentially biases the estimates. As a final check, Table 3.8 therefore repeats the analyses presented in Panels A to C of Table 3.6, now including the non-panel treatment sample for whom follow-up insurance data is not available. Columns (1)-(6) and (7)-(12) assume that this sample is uninsured or insured, respectively. The first stage is the same as in Table 3.5, including all respondents from the control sample with data on follow-up health expenditures.

Overall, including migrants does not affect the results qualitatively. In Panel A, a unit increase in log expenditures - equal to 3 standard deviations of predicted acute expenditures in the full sample - raises insurance take-up by at most 5.7 percentage points. Adverse selection increases the probability of having insurance by at most 7.8 percentage points among households living near the public partner facility, which is generally stronger than near the private partner facility. The one exception is chronic expenses, where conditional on age and gender selection is significant only in the area with a private partner facility, but not in the area with a public partner facility.

In sum, independent of the specification of the first and second stage, the subsample and the type of health expenditures, adverse selection is limited. Selective attrition cannot account for the finding that households do not select members into insurance on their private health risks.

Table 3.8: Selective attrition: Assuming that individuals not interviewed at follow-up are either uninsured or insured

	Non-traced individuals: Uninsured			Non-traced individuals: Insured								
	Full sample Acute expenses	Adults only Acute expenses	Adults only Chronic expenses	Full sample Acute expenses	Adults only Acute expenses	Adults only Chronic expenses						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. All panel households												
$\hat{\alpha}_{01}$	0.057* (0.028)	0.011 (0.031)	0.033 (0.029)	-0.002 (0.021)	0.045* (0.018)	0.024 (0.016)	0.022 (0.024)	0.042 (0.035)	0.025 (0.040)	0.023 (0.027)	0.048* (0.022)	0.055* (0.026)
B. Near public versus private partner facility												
$\hat{\alpha}_{01}$ x Public	0.078* (0.038)	0.038 (0.038)	0.025 (0.037)	-0.012 (0.029)	0.053+ (0.028)	0.036 (0.028)	0.068+ (0.037)	0.068 (0.051)	0.061 (0.040)	0.036 (0.031)	0.047 (0.031)	0.032 (0.031)
$\hat{\alpha}_{01}$ x Private	0.034 (0.031)	-0.018 (0.048)	0.042 (0.041)	0.005 (0.030)	0.036* (0.018)	0.011 (0.017)	-0.030 (0.026)	0.009 (0.041)	-0.008 (0.067)	0.008 (0.040)	0.049+ (0.026)	0.091* (0.037)
P Private = Public	0.3074	0.3551	0.7405	0.6842	0.5643	0.4496	0.0397	0.3588	0.3850	0.5558	0.9654	0.1645
C. Above versus below median wealth												
$\hat{\alpha}_{01}$ x Above	0.062 (0.044)	-0.008 (0.084)	0.034 (0.037)	-0.014 (0.031)	0.035 (0.022)	0.004 (0.025)	0.044 (0.045)	0.029 (0.050)	0.037 (0.048)	0.009 (0.037)	0.027 (0.023)	0.015 (0.030)
$\hat{\alpha}_{01}$ x Below	0.053 (0.033)	0.021 (0.059)	0.040 (0.061)	0.007 (0.049)	0.048+ (0.025)	0.037+ (0.019)	0.007 (0.027)	0.043 (0.069)	0.018 (0.057)	0.019 (0.097)	0.043 (0.031)	0.061* (0.031)
P Above = Below	0.8717	0.7891	0.9408	0.7007	0.6961	0.3181	0.4277	0.8686	0.8048	0.9211	0.6645	0.2796
Observations	2795	2795	1466	1466	1458	1457	2795	2795	1466	1466	1458	1457
F Risk factors	12.08	5.53	5.36	8.12	8.31	5.80	12.08	5.53	5.36	8.12	8.31	5.80
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Estimated with household fixed effects. Controls: As in Table 3.6. First stage includes traced migrants only. Standard errors in parentheses are bootstrapped with 499 replications, clustered by Enumeration Area. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

3.5 Policy implication

The preceding analyses show that individual-based enrollment does not necessarily solicit adverse selection. In addition, since households who cannot afford insurance under family-based enrollment might enroll some household members in an individual-based program, individual enrollment provides a potential solution to low take-up. An important question from the perspective of an insurance provider is not only how this affects enrollment within households, but also overall adverse selection.

When moving from family-based to individual-based enrollment, the composition of the risk pool changes in two dimensions. First, at the intensive margin, households might decide to enroll fewer members. This study shows that along this dimension, individual-based enrollment is unlikely to worsen the risk pool, since adverse selection within households is limited. Health risks do not offer an explanation for low intra-household enrollment.

Second, at the extensive margin, the number of households with at least one insured member will be higher under individual enrollment. Households who join only individual and not family-based insurance are most likely large and less wealthy, since for these households, the family premium will be most bulky. If they expect higher health expenditures on average, increased enrollment at the extensive margin induces adverse selection. If the opposite is true, individual enrollment will create advantageous selection.

Table 3.9 presents between-estimates of risk factors and health expenditures as a function of household size and wealth, controlling for location. Each column regresses the household average of the stated dependent variable on dummies for location, household size and a continuous wealth variable interacted with the household size categories. The analyses do not control for other variables like age and gender.

Larger households - assumed to join with higher probability under individual enrollment - are significantly healthier in terms of self-rated health in Column (1); disability levels in Column (5); chronic baseline expenditures in Column (6); grade 2 hypertension in Column (7); and chronic follow-up expenditures for the control sample in Column (10). Wealth is associated with lower hypertension rates but only in small households.

Thus, the type of household for whom individual-based enrollment potentially enhances take-up is in better health than the average household. This suggests that individual-based enrollment does not induce adverse selection at either the intensive margin, within households, or at the extensive margin, between households. This stands in sharp contrast to the hypothesis that without program restrictions, selection on health risk poses a threat to the sustainability of health insurance.

Table 3.9: Health and expenditures by household characteristics

	Acute risk factors		Acute risk factors			Chronic risk factors			Follow-up exp. (control)		
	Ladder (1) All	Overweight (2) All	Std. BMI (3) Adults	Acute (4) Adults	Disability (5) Adults	Chron (6) Adults	Grade 2 HT (7) Adults	Acute (8) All	Acute (9) Adults	Chron (10) Adults	
A. Location											
Town	-0.027 (0.022)	0.056** (0.017)	0.502** (0.086)	-0.011 (0.273)	-0.071** (0.028)	0.013 (0.162)	-0.041** (0.016)	-0.012 (0.390)	0.206 (0.330)	0.012 (0.232)	
Private	-0.007 (0.025)	0.022 (0.020)	0.111 (0.088)	-0.641** (0.236)	-0.012 (0.039)	0.001 (0.209)	0.019 (0.018)				
Treatment	0.017 (0.028)	-0.006 (0.019)	-0.239** (0.091)	-0.802** (0.345)	0.105** (0.034)	-0.041 (0.175)	-0.047** (0.016)				
B. Household size											
- 4 to 6 members	-0.058** (0.021)	-0.017 (0.020)	0.008 (0.101)	0.132 (0.225)	-0.098** (0.034)	-0.458** (0.204)	-0.047** (0.018)	-0.105 (0.378)	-0.179 (0.302)	-0.326 (0.221)	
- 7+ members	-0.088** (0.023)	-0.016 (0.023)	0.033 (0.102)	-0.231 (0.233)	-0.069+ (0.038)	-0.572** (0.213)	-0.070** (0.018)	0.113 (0.373)	-0.205 (0.301)	-0.481+ (0.282)	
C. Wealth											
Wealth	-0.008 (0.015)	0.021 (0.016)	0.096 (0.090)	0.082 (0.172)	-0.022 (0.033)	-0.111 (0.146)	-0.042** (0.013)	0.091 (0.184)	0.209 (0.185)	0.057 (0.142)	
x 4 to 6 members	0.021 (0.018)	0.007 (0.018)	0.071 (0.102)	0.021 (0.221)	0.015 (0.038)	-0.015 (0.159)	0.045** (0.017)	0.278 (0.293)	0.055 (0.257)	0.021 (0.249)	
x 7+ members	0.020 (0.019)	0.000 (0.022)	0.098 (0.108)	0.036 (0.230)	0.027 (0.040)	0.186 (0.166)	0.059** (0.017)	-0.208 (0.306)	-0.114 (0.279)	0.002 (0.246)	
Observations	974	968	955	967	967	965	964	368	375	369	
Clusters	99	99	99	99	99	99	99	39	39	39	
R-squared	0.019	0.041	0.093	0.045	0.046	0.015	0.039	0.010	0.022	0.009	
Mean dep. var.	0.155	0.167	-0.260	2.408	0.175	0.852	0.089	1.889	1.700	0.839	

Linear estimates. Estimation sample: All HH with at least two members in panel. Std. errors in parentheses clustered by EA. + $p < .1$, * $p < .05$, ** $p < .05$

3.6 Conclusion

This study investigated intra-household adverse selection in micro health insurance, using an individual-based program in rural Nigeria. Two-sample two-stage least squares yielded little evidence of selection on health risk. Drawing on a control sample without access to health insurance, self-reported as well as measured health were significant risk factors for follow-up health expenditures. They did however not affect the intra-household insurance allocation in a treatment sample. The correlation between insurance take-up and cross-sample predictions of health expenditures was insignificant from both an economic and statistical point of view.

In this context, this finding contradicts our priors. Unlike insurance programs in mature insurance markets, this subsidized program in Nigeria did not limit adverse selection by requiring group insurance, co-payments, deductibles or by excluding pre-existing conditions. In addition, the analyses used fixed effects to control for unobserved heterogeneity in household characteristics. This should improve the power to detect adverse selection (Finkelstein and McGarry, 2006; Doiron et al., 2008). Notwithstanding these conditions, I failed to reject the null hypothesis of zero selection within households.

A question for future research is why households do not select members on individual health risks even though Section 3.2.3 finds suggestive evidence of price being a constraint to enrollment. I identify four potential explanations. The first one is that health risk factors - the non-random instruments - correlate with a second unobserved individual attribute that is related to both follow-up expenditures and insurance. Households might prefer enrolling the productive, low-risk breadwinner to avoid simultaneous occurrence of health expenditures and income losses. But inconsistent with demonstrated productivity-health-nutrition linkages (Dasgupta and Ray, 1986; Pitt et al., 1990; Dercon and Krishnan, 2000), working household members were less likely to have insurance than non-working members.

Second, the cross-sample of health expenditures from the first stage might not have been a good counterfactual if the upgrading of health facilities, which came with the insurance intervention, had a heterogeneous impact. Women and members of poor households in rural villages were most likely go to low-quality public health facilities at baseline and facility upgrades might have affected their health-seeking behavior most. Nevertheless, disaggregating the analyses by location, wealth and gender does not yield qualitatively different results. Also the reduced form, which does not depend on a cross-sample prediction for health expenditures, finds limited adverse selection. This suggests that the facility upgrades cannot explain the lack of observed adverse selection.

A third explanation is that the household did not have the information to predict future health expenditures. Limited information about private risk factors hamper the adoption of

health-prevention technologies such as bed nets or water purification tablets, and information about private risk factors helps increase adoption (Kremer and Glennerster, 2011). In a similar fashion, blood pressure - the one health indicator for which households received a diagnosis at baseline - was the sole risk factor affecting the insurance allocation. However, the associated increase in predicted expenditures is relatively modest. Further, households have information about self-reported health but did not select on these indicators either.

A final interpretation of the results is that households did not consider private health risks when purchasing insurance. Formerly uninsured households received access to insurance two years before follow-up and may not have understood the concept of insurance. Households may have failed to realize that insurance has higher benefits for high-risk members. Future research could test how insurance education and experience with health insurance - which will improve understanding - affect adverse selection.

Adverse selection was limited in particular conditional on age and gender, and when restricting the treatment sample to households living near the private partner facility. This yields two policy recommendations. First, to the extent that selection on health risk does occur, it is on observable characteristics and asymmetric information appears not to play a role. A commercial insurance provider will be tempted to screen or price-discriminate on these observable differences in actuarial costs (e.g. by discouraging women at reproductive age and the elderly to enroll). This would reduce enrollment among vulnerable populations that are more expensive to insure, whilst policy-makers aim to cover precisely these groups. Targeted subsidies or so-called risk adjustment may help maintain affordable insurance premiums (Van de Ven and Ellis, 2000).

Second, the private partner facility with higher enrollment rates and less adverse selection was actively engaged in the enrollment process, and offered households the option to pay the premium on credit. This may have helped attract low-risk members of the household. Program officers play an important role in targeting of development programs (Elbers and Gunning, 2012). In this case, the private versus public partner facility appeared to use distinct targeting strategies, underscoring the need to better understand these program officers' role. A direction for future research is to explore the optimal design of their incentives to limit adverse selection and maximize enrollment.

In conclusion, this study finds no adverse selection within households. This suggests that the fear for adverse selection in individual-based micro health insurance programs is exaggerated and that family-based enrollment is unnecessary to limit adverse selection. Given that individual-based enrollment offers the option to enroll a few household members only, making the insurance premium less bulky than in family-based schemes, it seems worthwhile experimenting with different types of enrollment schemes. Ultimately, individual-based insurance might be more effective at protecting households from catastrophic uninsured health expenditures.