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Results from Ghana

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Abstract

This paper uses the roll-out of the national health insurance in Ghana to assess the cushioning effect of coverage on the financial consequences of health shocks and resulting changes in coping behaviors. The analysis finds a strong reduction in medical expenditures, preventing households from cutting non-food consumption and causing a decrease in the volume of received remittances as well as the labor supply of healthy adult household members. Moreover, the paper presents evidence that the insurance scheme reduced the likelihood that households experiencing a health shock pulled their children out of school to put them to work. Avoidance of such costly coping mechanisms is potentially an important part of the social value of formal health insurance.

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1 Introduction

Large and unpredictable medical expenditures expose uninsured households to substantial financial risk. Households may sell financial assets or incur debt to pay for health care without compromising consumption levels. In poorer economies, however, limited wealth and access to financial instruments may make it difficult to smooth consumption over shocks. Accordingly, a key objective of public health insurance schemes in those settings has been to secure the subsistence of vulnerable populations. Consistent with this, the prevention of negative shocks to food consumption has been traditionally considered the social value of insurance (Townsend, 1994).

This may be an overly narrow perspective because uninsured households facing health shocks in developing countries may employ strategies to protect minimum food consumption levels that can be similarly detrimental in the short term and even more so in the medium to long term (Chetty & Looney, 2006). For instance, in order to generate additional income that allows (partially) covering catastrophic medical expenditures, households may take children out of school and put them to work with likely negative effects on their well-being later in life.[1] In fact, two studies of health insurance schemes have documented effects on child labor (Landmann & Frölich, 2015; Chakrabarty, 2012) and, thus, arguably illustrated their social value. However, the studied schemes were small programs run by non-profit organizations. Policy makers are now interested in whether the same result applies to large-scale public insurance schemes. This is of particular importance as there are numerous examples of small programs that were scaled up by governments, but the size of the measured treatment effect diminished substantially (Al-Ubaydli, List, LoRe, & Suskind, 2017).[2]

This paper adds to the literature by presenting the first causal estimates of the effects of a country-wide health insurance scheme on the incidence of child labor.[3] As a secondary contribution, we examine the effects on additional costly risk coping strategies such as the

[1] See Heady (2003), Borga (2019), and Mussa et al. (2018) for the effects of child labor on skill and human capital formation. Dumas (2012) and Keane et al. (2018) show detrimental effects of child labor on cognitive development. Also, children who work early on were found to have lower wages (Emerson & Souza, 2011; Posso, 2017) and a worse health status (Beegle et al., 2009; Sturrock & Hodes, 2016) later in life. Of course, it is not to question that food consumption profiles of children can be important determinants of their future health and education (e.g., Christian & Dillon, 2018).

[2] This is explained by various factors including increased administrative complexity, different implementation of the program at scale, and altered composition of program participants (Al-Ubaydli, List, & Suskind, 2017).

[3] Evidence from country-wide insurance schemes was highlighted as an important knowledge gap in the field of research on child labor (Dammert et al., 2018). The public scheme examined in this paper is indeed remarkably different from the schemes that were studied by Landmann & Frölich (2015) and Chakrabarty (2012). For example, it was not restricted to micro-finance clients but available to the general public, the number of insured people is several thousand times larger, and children as well as the elderly were automatically covered.

reduction of consumption of food and selling productive assets or land. We also analyze whether the scheme caused changes in the use of less costly coping mechanisms. Specifically, we estimate effects on the consumption of non-food goods, utilization of informal loans, receipt of remittances, and supply of labor by non-sick adult household members.

Our analysis exploits the introduction of the national health insurance scheme (NHIS) in Ghana, which was the first in Sub-Saharan Africa and served as a precedent for many countries of the continent (Alhassan et al., 2016). The main identification problem when analyzing the effects of a national policy is finding a suitable comparison group. In this paper, we exploit the fact that the staggered implementation of the NHIS overlapped with the roll-out of the 5fth round of the Ghanaian Living Standards Survey. We are able to observe, within the same district, subdistricts interviewed right before the NHIS introduction, and subdistricts interviewed right thereafter. This allows us to use a regression discontinuity design with time and district fixed effects, where the running variable is months to the NHIS implementation at the district level. Because the timing of interviews of households was external to the timing of the NHIS adoption, we argue that, within the same district, whether a household was observed before or after NHIS implementation is as good as random. We indeed find no systematic discontinuities in the characteristics of households that were interviewed around the time of the NHIS introduction.

Our first finding is that the NHIS resulted in a reduction of out-of-pocket (OOP) payments for medical care of about 20 percent. To gauge the extent to which this effect is driven by NHIS cushioning households from the financial consequences of more severe health shocks, we split the sample by the length of a sickness episode. We show that this measure was not affected by the introduction of the NHIS in the period studied. Estimation results reveal that the drop in OOP payments is larger for households experiencing higher sickness intensity (35 percent).

Our second finding is that households experiencing higher sickness intensity increased food consumption by roughly 1 percent as a result of the insurance. This effect only accounts for a tiny fraction of the estimated OOP savings. Thus, for the most part, food consumption was already protected against expenditure shocks in the absence of health insurance. According to the overly narrow traditional perspective, this implies that the scheme had limited social value. Yet, the third and central result of this study is that the incidence of child labor decreased by 8 percentage points as a consequence of insurance coverage among households with higher sickness intensity. Also, class attendance of children increased by 2.5 percent for the same group of households. Considering that disinvestment in the human capital of children is associated with detrimental long term effects according to previous literature (e.g., Beegle et al., 2009), this rather suggests that the scheme had a large social value.

Further findings include significant effects of the NHIS on non-food consumption and remittances, which suggests that in the absence of the insurance, households first resorted to risk coping strategies that arguably have less detrimental long term consequences. Yet, in monetary terms, these effects are only a fraction of the estimated savings in OOP. In particular, non-food consumption increased by a fifth and remittances fell by another fifth of the estimated drop in medical expenditures. Thus, households were not able to fully cope with the expenditure shock on the basis of these measures alone in the absence of insurance and had to use additional measures, which is consistent with our findings of child labor effects of the NHIS. We also find a significant negative effect of the NHIS on employment participation and hours worked of healthy adult household members.

This paper adds to a small evidence base on the deployment of informal and self-insurance strategies to cope with health shocks and the extent to which formal health insurance substitutes for welfare-decreasing coping mechanisms such as child labor. Chakrabarty (2012) uses data from Bangladesh and finds that micro health insurance in combination with credit can reduce child labor for poor households. Landmann & Frölich (2015) use a randomized control trial that paired micro-credit with a micro health and accident insurance in Pakistan. Their findings suggest a lower incidence of child labor and reduced child labor earnings. To the best of our knowledge, Liu (2016) is the sole other study that examines a wider range of coping mechanisms in the context of a rural health insurance program in China. The author reports that households insure income and consumption against health shocks through increased levels of labor supply in the absence of access to health insurance. The health insurance reduced the use of these smoothing mechanisms. Also, rural health insurance increased investments in school attendance and reduced the employment of children aged between 7 and 18 years. A limitation of the study is that it does not show to what extent the employment of children constitutes child labor according to the international definition of child labor (ILO convention). The specific measure that the author uses mostly captures whether a child is reported as working or not (excluding household work) without taking into account the number of hours worked per week even though this is an important criterion for child labor. For instance, according to the ILO convention, children between 14 and 17 years old are only child laborers if they work more than 43 hours per week. Thus, the number of employed children is likely to exceed the number of child laborers in the study sample.[4] It is well possible that the effects of the health insurance are much smaller on child labor than

[4]He (2016) argues that child labor rates in China are generally very low due to compulsory education consisting of six years of primary school and three years of junior middle school. Official statistics are not available.

on the utilized employment measure.

Our paper further connects to three broader strands of literature. First, there exist many empirical papers that examine the effects of health insurance on health care utilization and health outcomes. This applies especially to the developed world, where more generous insurance coverage tends to result in increased health care utilization (for a review of the literature, see Cutler & Zeckhauser, 2000). In low and middle income countries, the available evidence also suggests greater health care utilization as a result of health insurance. On the contrary, results for the impacts on OOP spending are mixed, with a few studies even finding increased household OOP spending particularly at high levels (Wagstaff & Lindelow, 2008; Wagstaff et al., 2009).[5] Nonetheless, our finding of negative effects on OOP payments does not represent an outlier in the literature (Wagstaff 2010; Bauhoff et al. 2011; Limwattananon et al. 2015; Strupat & Klohn 2018).

Second, several studies examine strategies used by households both in developed and developing countries to deal with health and other shocks (e.g., Sauerborn et al., 1996; Wagstaff, 2007). Genoni (2012), for instance, shows that remittances from relatives are an important source of consumption smoothing of Indonesian households in the face of health shocks. Sparrow et al. (2014), on the other hand, find that borrowing and drawing from the family network are the main coping strategies for the poor in Indonesia. Mitra et al. (2016) find that Vietnamese households use an array of coping mechanisms that include decisions with potentially detrimental long term consequences, such as asset sales and decreased education expenditures. The third broader strand of literature examines the nexus between public policies and child labor (for a review see Basu, 1999). The examined polices range from education programs (e.g., Kondylis & Manacorda, 2012) to legal age restrictions for entry into the labor force (e.g., Piza, 2017).

The remainder of the paper is organized as follows. First, we describe the roll-out of the NHIS in Ghana. Then, we describe the data used. In Section 4, we develop our research design, and Section 5 presents our results. Section 6 concludes.

2 Institutional Background

The Ghanaian NHIS. In August 2003, the Ghanaian government passed the National Health Insurance Act 650 (HI Act), establishing the terms of the NHIS. Its primary goal was to improve access and quality of primary health care services in Ghana through the establishment of district-wide insurance schemes (Gajate-Garrido, 2013). The HI Act guided

[5] The authors argue that by expanding health services coverage the health insurance also increased contact with medical services.

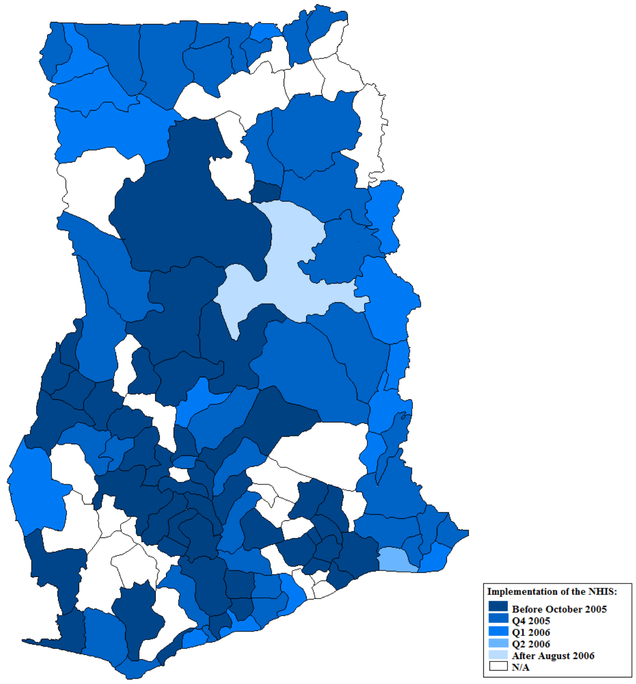
the structure of the district-wide insurance schemes and set up the legal framework for the establishment of a regulatory body, the National Health Insurance Council (NHIC). The role of the NHIC was to register, license, and regulate health insurance schemes and to accredit and monitor health care providers operating under these schemes. It also played a crucial role in managing the National Health Insurance Fund.

Implementation. After obtaining a license from the NHIC, local district assemblies were responsible for initiating District Mutual Health Insurance Schemes (DMHIS), identify human resources to provide technical support for the establishment of the schemes, and carry out social mobilization (Gajate-Garrido, 2013). The ability to set up DMHIS varied across local district assemblies, in part as it depended on whether (partial) health insurance coverage was already in place. Specifically, existing government-sponsored Mutual Health Organizations (MHOs), which were organized at the district-level and offered coverage to informal sector workers, were automatically converted to DMHIS (Agyepong & Adjei, 2008). As a result, districts that had a better health care provision system and a higher number of MHOs were able to more easily obtain licenses for DMHIS than districts that needed to build their health care organizations from scratch. Due to such capacity differences, districts adopted the NHIS on different dates, with districts with more developed health care systems implementing the NHIS earlier.

Figure 1 shows the map of Ghana's 110 districts, colored by the implementation date of the NHIS. Some districts adopted the NHIS as early as January 2003 as part of a pilot. By January 2005 it became a national policy, and local authorities consistently started adopting the NHIS. By October 2007, all local authorities had successfully introduced the NHIS in their districts. Over a third of all districts adopted the NHIS in our period of analysis (October 2005 to September 2006), which allows us to observe a sizable number of subdistricts within each district before and after NHIS implementation.

Coverage. The principal objective of the implementation of the NHIS was to ensure equitable and universal access to health care by removing the financial barriers imposed by user fees. The NHIS extended the coverage offered by MHOs, which was restricted to inpatient expenditures. The NHIS health care package was expanded to basic health care services, including outpatient consultations, essential drugs, maternity care (normal and cesarean delivery), eye care, dental care, and emergency care. The NHIS package was particularly generous in the range and extent of medicines covered. DMHIS were required to adhere to the defined benefit package (Gajate-Garrido, 2013).

Figure 1: Spatial Roll-out of the NHIS



Notes: Own data collection on NHIS implementation date

Premiums. Covered health services were financed through a Health Insurance Levy (a 2.5 percent tax on specific goods and services made in or imported to Ghana), and the payment of insurance premiums. NHIS annual premiums were income-related, set at a minimum of 72,000 Ghana cedis (GHS) per adult (US$ 7.5), and a maximum of 480,000 GHS (US$ 50).[6] In a typical two-parent family with three children, the entire family would have been covered for 144,000 GHS per year (US$ 15). Children (up to age 18), the elderly (age 70 and older), and the indigent were exempt from payment. Satisfaction surveys during the first months of its implementation raised the issue of the affordability of its annual premium. This concern was backed up by moderate initial take-up rates of insurance (Sulzbach et al., 2005). However, a comparison of the NHIS premium with MHOs shows the advantages of the NHIS for its enrollees (Sulzbach et al., 2005). For a typical family, the premiums paid under MHOs for inpatient coverage were comparable to those paid under the NHIS for the full coverage package.

Health Care Use. Sulzbach et al. (2005) show that insured individuals sought formal health care sooner after the onset of illness than their uninsured counterparts. Greater coverage from the NHIS also induced behavior consistent with moral hazard. Debpuur et al.

[6] In 2007, the exchange rate was 9,600 GHS to US$1.

(2015) highlight numerous ways in which the insured took advantage of the NHIS, such as frequent and unnecessary visits to health facilities, impersonation, or even feigning sickness to collect drugs for non-insured persons.

3 Data and Research Design

3.1 Data

To analyze the effects of NHIS implementation on risk coping mechanisms with a focus on child labor impacts, we use household data from the fifth round of the Ghana Living Standards Survey (GLSS), collected from October 2005 to September 2006. The fifth round of the GLSS interviewed 37,128 individuals in 8,687 households. We define a child as aged 5 to 17 years and limit the sample to those households composed by at least one adult and one child (30,060 individuals in 7,982 households), i.e., households with at least one potential child laborer.

Because the timing of implementation of the NHIS is non-random,[7] our empirical strategy relies on within district variation as opposed to cross-district variation. Correctly identifying geographic information is therefore central to our analysis. The finest within-district level reported in the survey is the enumeration area (EA), which roughly corresponds to villages (in rural areas) and neighborhoods (in urban areas). EAs within the same district were surveyed at different times, with no systematic order. Importantly, the date of the survey of each EA is orthogonal to key defining characteristics, such as population, economy, infrastructure, provision of education, and health care facilities.[8] This orthogonality is key for the identification of the treatment effect of insurance, as we will explain below.

We link the survey data to information on the exact implementation date of the NHIS at the district level. We collected these data from several sources, ranging from district level NHIS web-pages to newspaper articles and phone calls to district level NHIS authorities.[9] During the period of the survey, 53 districts adopted the NHIS. Of these, 52 districts had at least one EA surveyed before and one after the adoption of the NHIS. We only keep households in such districts, resulting in a final sample of 16,556 individuals in 3,493 households.

[7]The adoption date of the NHIS was negatively correlated with the level of district-level health care provision.

[8]Regressing the date of the interview on a wide range of EA characteristics yields mostly insignificant point estimates. The correlation between EA characteristics and the date of interview is always below 0.15.

[9]We were unable to find information on the exact implementation date for 18 districts of the 110. We drop observations from these districts.

3.2 Research Design

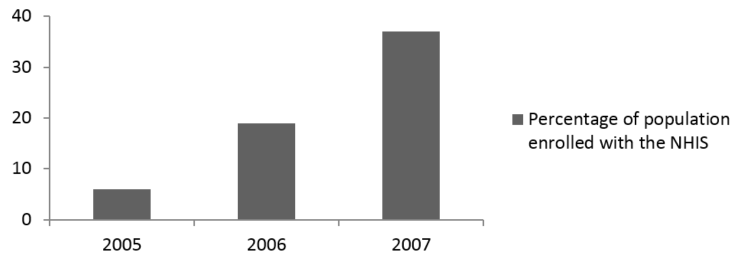
The purpose of this paper is to identify the impact of public health insurance on households' OOP spending for medicines and the incidence of informal insurance mechanisms used to cope with these expenditures and other financial consequences of health shocks.

In an ideal experiment, one would randomly grant NHIS access to districts and compare household responses to health shocks in districts with access to the NHIS to the counterfactual behavior of ex-ante similar households in districts without access to the NHIS. Absent this, we exploit the fact that the NHIS was rolled out simultaneously to the GLSS survey in some districts. We construct d, a measure of the months between the date of interview and the NHIS implementation date in the district of residence. Normalizing the date the district adopted the NHIS to zero, the survey contains households from subdistricts interviewed at $d < 0$, and households from subdistricts interviewed at $d \geq 0$.

By keeping only these districts having both negative and positive (or zero) values of d, we can exploit the within-district variation in the timing of the survey. If within each district, timing is truly orthogonal to EA characteristics, within-district variation in exposure to the NHIS at the time of the survey is as good as random. In this case, EAs interviewed before the NHIS implementation in the district represent suitable controls for EAs interviewed after the NHIS was available in the district. This measure d is the running variable of our regression discontinuity (RD) design, which is presented in detail in the subsequent section. We model the effect of NHIS implementation on household outcomes that would be potentially affected by a health shock.

Our research design captures the reduced-form short-term impact estimates of the reform, where the first stage corresponds to the impact of the NHIS rollout on the health insurance take-up of households. Our data do not allow us to correctly identify changes in health insurance coverage due to the implementation of the NHIS. We can, however, rely on reported national enrollment rates to get an impression of the extent of NHIS coverage. Figure 2 shows that national enrollment in 2006 was around 20 percent, and increased to almost 40 percent in 2007. These figures, however, are low bounds of the real coverage rate of the NHIS, as households were frequently allowed to enroll under the NHIS after the occurrence of a health shock (Gajate-Garrido, 2013). Our results need to be interpreted keeping in mind that at the time of observation, about 20 percent of Ghanaians, situated higher on the health risk spectrum, were enrolled under the NHIS.

Figure 2: NHIS Enrollment Rate



Notes: Source NHIA 2010

3.3 Variables, Sample Averages, and Their Evolution

Table 1 reports a basic summary of the outcome variables used in the analysis. The table compares households observed up to six months before NHIS implementation to those observed up to six months after NHIS implementation.[10] Figures 3 and 4 present the change in the outcome variables around the date of the introduction of the NHIS. The x-axis measures the months to NHIS implementation, with the cutoff at month 0 indicated with a vertical line. Each observation in the graph is an average for a monthly bin. The figure includes a linear fit for the outcomes six months before and after the introduction of the NHIS (in black) and its 95 percent confidence interval (blue dashed line). It also includes a kernel-weighted polynomial fit.

In what follows, we describe in detail the construction of the outcome variables and compare their means before and after the implementation of the NHIS.[11]

Medical Expenditures. The GLSS contains information on total medical expenditures over the two weeks before the interview for households reporting any illness or injury during that same period. Panel A1 of Table 1 reports the sample average medical expenditures before and after NHIS implementation. Households observed before NHIS implementation reported an average of 24,070 GHS ($2.5), whereas those observed after NHIS implementation reported an average of 18,330 GHS ($1.9). The reported OOP medical expenditures are highly skewed, as seen by the large standard deviation, and thus are used in our empirical analysis after applying a logarithmic transformation.[12]

[10] We select the bandwidth to the left and right of the discontinuity of d using optimal bandwidth selector methods provided by Calonico et al. (2016). The bandwidth is found to be optimally six months before and after of the NHIS implementation.

[11] For a summary on the construction of all variables used in this paper, we refer to Tables A1 and A2 in Appendix A.

[12] We deal with the large number of zeros in the medical expenditures variables by mapping the zeros to zero, that is, using $ln(x+1)$.

For households reporting positive medical expenditures, values can be dis-aggregated into three categories of care: inpatient, outpatient and medicine purchases. Panel A.1. of Table 1 also reports descriptive statistics of these outcomes. Medicine purchases were lower after NHIS introduction, and represent 80% of the total medical expenditures among households pre-NHIS and 55% post-NHIS. Outpatient expenditures appear to be larger among households observed after the introduction of the NHIS, possibly as a result of the higher level of care. This difference is small and statistically non-significant. Inpatient expenditures are minimal, as the reported incidence of inpatient care is low in the sample. We opt to omit this variable from our analysis for the same reason.[13]

Panel A of Figure 3 displays the OOP medical expenditures (in logarithms) in the months around the introduction of the NHIS. The figure reveals a noticeable drop in OOP payments at the time of NHIS implementation, of similar magnitude when evaluated with a linear fit compared to a kernel-weighted polynomial fit.[14]

Our empirical strategy could be underestimating the impact of the introduction of the NHIS if households delay their medical expenditures in expectation to the introduction of the NHIS. If there was anticipation, we might see a dip in the average OOP payments in the months right before the NHIS. Panel A indicates that this does not seem to be the case.

Health Status. The GLSS also contains self-reported information on the number of days of illness or injury over the past two weeks. Panel A.2. of Table 1 displays that 53% of the households reported any illness or injury before the NHIS. This share increases to 64% among households observed after the introduction of the NHIS. This may be due to specific reporting behavior because it has been documented that respondents in low- and middle income country settings tend to report their use of medical care instead of their health status (Murray 1996; Bago d'Uva et al. 2008).[15] We use this variable to construct measures of health shock intensity that are not significantly affected by the NHIS and use them to assess effect heterogeneity of the scheme (see next section).

[13] We expect that omitting inpatient expenditures from our analysis induces small consequences for our understanding of the reform, as the NHIS did not offer additional coverage than MHOs on inpatient care.

[14] In our sample, seven households were interviewed during the first two weeks of the month when their month of interview corresponded to the time of NHIS implementation. This implies that they could have had medical expenditures before the NHIS while being classified as under the NHIS. Our results are fully robust to their exclusion.

[15] As we will discuss in further detail below, this difference could represent an issue for our analysis, as it may be the result of a change in the composition of households reporting medical expenditures before and after the NHIS. We implement several methods that circumvent this issue in our analysis, and acknowledge that the observed medical expenditures only render low bound effects of the impact of the NHIS on OOP spending.

With respect to risk coping measures, we use data on household consumption, loans and remittances, and adult labor supply.

Consumption. The GLSS reports households' frequent consumption (excluding medical expenditures) during the six days before the interview. Panel B of Table 1 shows its sample average. Households observed before the implementation of the NHIS reported an average weekly consumption of 104,420 GHS ($11). Households observed after the NHIS reported an average of 88,530 GHS ($9). As with reported medical expenditures, reported consumption is highly skewed. We, therefore, use a logarithmic transformation of consumption in our analysis. Panel B in Figure 3 shows the respective RD graph. While there is no change at the discontinuity, we observe a significant drop in the average household consumption two months after NHIS implementation, which will not be picked up in the regression discontinuity analysis. Overall, the average consumption was higher during the six months after NHIS implementation than in the six months before, as seen by the fitted lines.

Loans and Remittances. Households were also asked about formal loan take up, the quantity borrowed, and remittances received by the household that were used for health purposes. The variables are reported over the year before the interview. Panel C of Table 1 displays that about 5% of households in our sample observed after the introduction of the NHIS borrowed a loan, compared to 7% among households observed before the NHIS. The latter had a higher average borrowing than the former. This difference is not statistically significant.

Remittances received in households observed before the implementation of the NHIS amounted to 96,650 GHS ($10), compared to 77,170 GHS ($8) among households observed after. This is a sizeable difference. The low value of remittances is because only about 2 percent of households reported receiving any remittances. To deal with the large number of zeros in the loans and remittances amounts, we use a logarithmic transformation of the quantity that households borrowed and received through remittances in our analysis.

Panel C in Figure 3 shows the RD graphs for the amount borrowed through formal loans (in logs) and remittances received (also in logs). In line with the sample averages, we do not find any discontinuity in the amount borrowed around the date the NHIS was introduced. We no longer observe any differences in remittances when examining the data around the discontinuity.

Adult Labor Supply. To analyze intra-household labor supply, we use the employment participation rate and hours worked over the week before the interview. Panel D of Table

1 reports descriptive statistics for ill (D.1) and healthy adults (D.2) in households in which at least one adult member experienced an illness or injury. The average employment participation and hours worked in the week before the interview for adults reporting having experienced an episode of illness or injury are low and appear to be lower in the sample of households observed after the introduction of the NHIS. Panel D.1 in Figure 3 illustrates the drop in their probability of being employed from crossing the discontinuity, which disappears by the second month after the implementation of the NHIS.

Healthy adults in households experiencing a health shock, on the contrary, participated in employment at a close to full employment rate before the introduction of the NHIS (Panel D.2 of the table). This figure is smaller after the NHIS was introduced.[16] Panel D.2. of Figure 3 displays a small drop in the employment participation of healthy adults at the discontinuity. This effect disappeared by the third month after the implementation of the NHIS. As with ill adults, there was a significant dispersion of the employment rate of healthy adults over time.

We also analyze costly risk coping measures, specifically assets and investments into the human capital of children.

Assets. We construct two binary variables measuring whether the household sold land or productive assets (livestock and equipment) in the 12 months before the interview. Panel E.1 in Table 1 reports the sample means of these variables before and after the introduction of the NHIS. While we observe no difference in the fraction of households selling livestock before and after the NHIS, we note a significantly smaller fraction of households selling land after the introduction of the NHIS.

Panel A.1 of Figure 4 shows the RD graphs for the sale of productive assets. We observe a breakpoint in the fraction of households selling productive assets at the time of the introduction of the NHIS. Panel A.2 reveals an even more pronounced drop in the fraction of households selling land at the time of introduction of the NHIS.

Children's Outcomes. The first row in Panel E.2 of Table 1 reports children's enrollment in school at the time of the survey. We observe a significantly higher school enrollment in the sample of households observed after the introduction of the NHIS. The second row reports the hours of class attended in the two weeks before the interview. Children observed during the six months before the implementation of the NHIS attended an average of 34 hours of school, whereas children observed during the six months after the NHIS introduction attended an average of 45 hours of school. Panel B.1. of Figure 4 illustrates the evolution of the hours

[16]We note the same pattern across samples when evaluating the weekly hours worked.

of class attended in the two weeks before the interview. Upon crossing the discontinuity, we observe a statistically insignificant jump in the hours of class attended.

Households observed before the NHIS was implemented spent an average of 124,000 GHS (US$13) per year on school fees and school-related expenditures (uniforms, books, transportation, food and boarding, and other contributions). Households observed after NHIS implementation spent more, 138,000 GHS (US$14), but this difference is not statistically significant.

The third row in Panel E.2 reports the incidence of child labor, which we define using the ILO convention. Specifically, child laborers are defined as children (1) below 12 years old working more than one hour per week; (2) between 12 and 13 years old working 14 hours or more per week; and (3) between 14 and 17 years old and working more than 43 hours per week. The rate of child labor in the sample of households observed after the NHIS is almost half the rate of the sample of households observed before the NHIS (14% versus 8%). This large difference is also found in the hours that children spend doing household chores in the two weeks before the interview (11 hours compared to 6 hours). Panel B.2. of Figure 4 shows the evolution of child labor rates. At the time of the NHIS introduction, we observe a significant drop in the incidence of child labor.

4 Empirical Specification

For each outcome variable, we estimate the following regression:

$$Y_{hts} = \beta_0 + \beta_1 f(d_{hts}) + \theta NHIS_{hts} + \beta_2 h(d_{hts}) NHIS_{hts} + \delta X_{hts} + \mu_s + \gamma_t + \varepsilon_{hts} \quad (1)$$

where d is the months separating the date of the interview t for a given household h and the date of NHIS implementation in the district s. $NHIS_{hts}$ is a variable taking value 1 if $d \geq 0$, and 0 if $d < 0$. X is a vector of pre-determined characteristics, μ_s and γ_t are district and time fixed effects, and ε is the error term. We allow the polynomial to differ from the left to the right of the month of NHIS implementation. θ is the coefficient of interest, and it captures the effect of NHIS implementation at the district level on household responses. $f(.)$ and $h(.)$ are some polynomial expansions in d. Because of the small number of observations, there is not enough variation to allow both functions to vary non-linearly. We impose in what follows that $f(.)$ and $h(.)$ are two different linear trends before and after NHIS implementation, acknowledging that this may represent a limitation for some of the outcome variables.

We select the optimal bandwidth to the left and right of the discontinuity of d using the

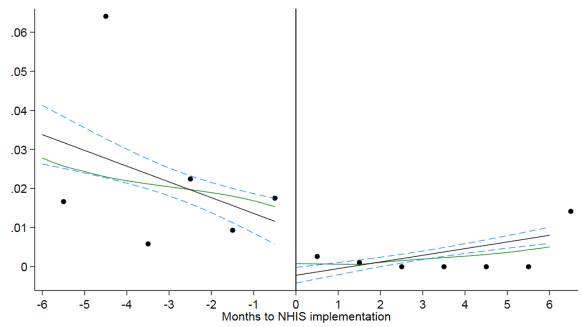
Table 1: Descriptive Statistics

	Sample of analysis		Test of equal means
	Before NHIS (1)	After NHIS (2)	p-value (3)
A. Health Outcomes (×10,000)			
A.1. Medical expenditures (×10,000)			
Total OOP payments	24.07	18.34	< 0.01
	(40.75)	(41.38)	
Inpatient expenditures	0.04	0.03	0.89
	(0.21)	(0.28)	
Outpatient expenditures	6.49	7.50	0.56
	(26.32)	(37.17)	
Medicine expenditures	17.55	10.80	< 0.01
	(28.74)	(17.30)	
A.2. Reported health status			
Illness or injury (0/1)	0.53	0.64	< 0.01
	(0.50)	(0.48)	
B. Consumption (×10,000)			
Frequent consumption	104.42	88.53	< 0.01
	(109.72)	(84.85)	
C. Loans and remittances			
Loan (0/1)	0.05	0.07	0.46
	(0.21)	(0.24)	
Amount loan (×10,000)	24.92	23.04	0.74
	(137.78)	(105.26)	
Remittances (0/1)	0.02	0.02	0.79
	(0.14)	(0.13)	
Amount remittances (×10,000)	96.65	77.17	0.37
	(496.57)	(419.02)	
D. Adult labor supply			
D.1. Ill Adult			
Employment (0/1)	0.14	0.10	0.04
	(0.34)	(0.30)	
Hours worked	6.36	4.49	0.03
	(19.30)	(16.80)	
D.2. Healthy Adult			
Employment (0/1)	0.90	0.87	0.08
	(0.30)	(0.33)	
Hours worked	78.66	64.15	< 0.01
	(76.05)	(54.82)	
E. Costly coping mechanisms			
E.1. Assets sold			
Livestock sold (0/1)	0.12	0.13	0.68
	(0.33)	(0.33)	
Land sold (0/1)	0.55	0.44	0.01
	(0.89)	(0.83)	
E.2. Children outcomes			
Enrolled in school	0.72	0.76	0.06
	(0.32)	(0.37)	
Class attendance	33.60	44.65	< 0.01
	(46.10)	(51.94)	
Total school costs (× 10,000)	12.39	13.84	0.27
	(22.36)	(27.77)	
Child work (0/1)	0.14	0.08	< 0.01
	(0.29)	(0.23)	
Hours HH chores	11.34	5.62	< 0.01
	(49.54)	(24.88)	

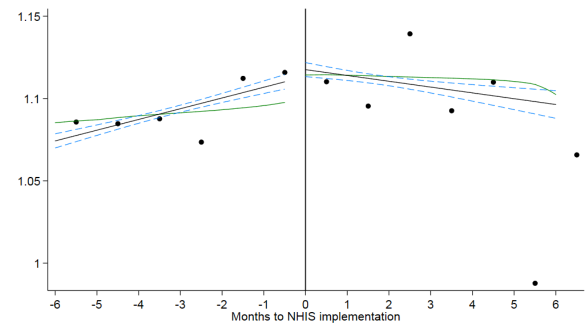
Notes: Sample of analysis spans these households observed up to and including 6 months before NHIS implementation ("Before NHIS") and up to and including 6 months after NHIS implementation ("After NHIS").

Figure 3: RD Graphs of Medical Expenditures, Consumption, Borrowing, and Adult Employment

A. Out of pocket payments (Log)

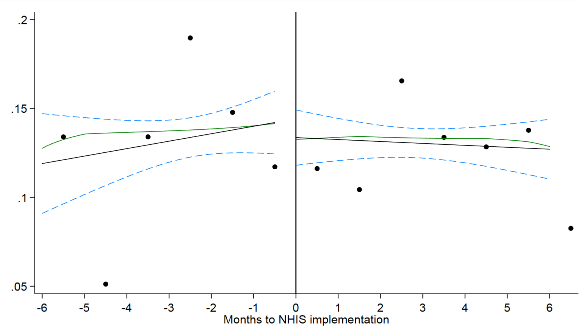


B. Consumption (Log)

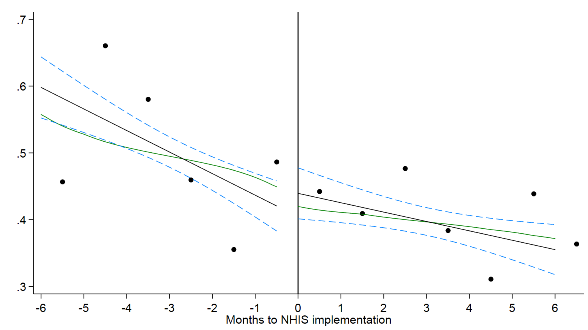


C. Loans and remittances

C.1. Loans (Log)

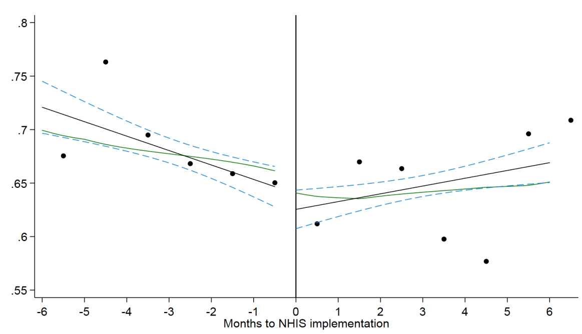


C.2. Remittances

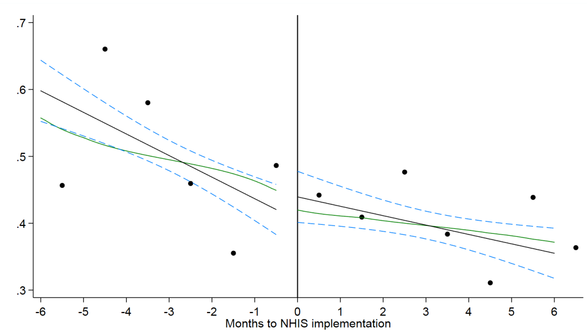


D. Employment of adults by health status in households with positive days of illness

D.1. Ill adult



D.2. Healthy adult

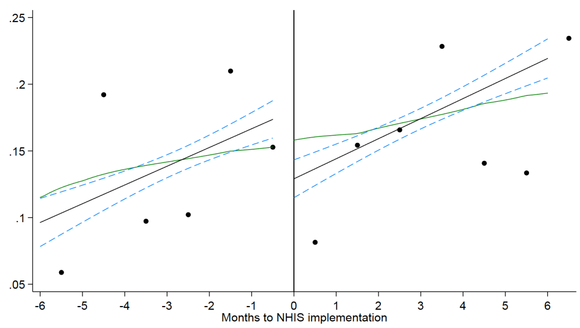


Notes: Each observation represents average health insurance enrollment by monthly bins of the distance between the interview date and NHIS implementation date. The vertical line denotes the distance cutoff of 0 and is given for households interviewed during the same month that the NHIS was implemented in their district of residence. The solid black trend lines are based on regressions using unbinned data. Confidence intervals are reported with the dashed blue line. Green line reports a local linear polynomial fit. The sample size is 2,228 households. In Panel C, we consider only these households in which at least one adult member reported an illness or injury. Ill adult refers to adult members suffering from a health shock, and healthy adult refers to adult members not suffering from a health shock.

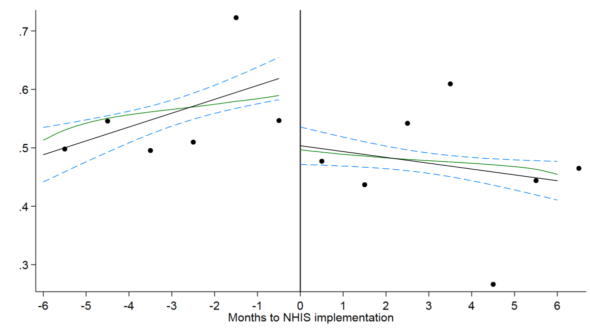
Figure 4: RD Graphs on Costly Risk Coping Measures

A. Assets

A.1. Assets sold

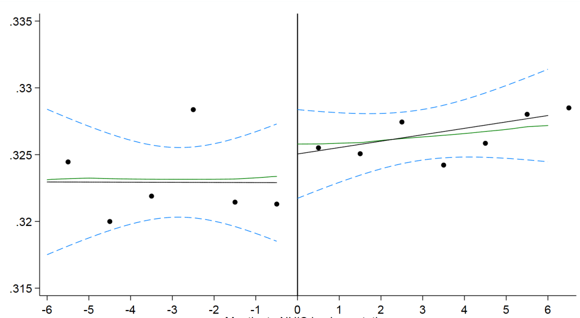


A.2. Land sold

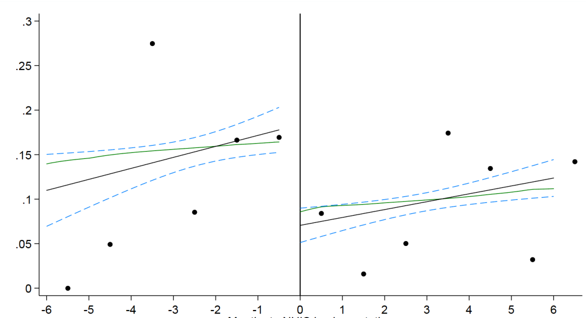


B. Children outcomes

B.1. Hours school attended (Log)



D.2. Child labor



Notes: See Table 3

bandwidth selector method provided by Calonico et al. (2016). The bandwidth is found to be optimally six months before and after of the NHIS implementation.[17]

Threats to Identification. The validity of our research design requires the counterfactual outcome being orthogonal to the date of implementation relative to the interview date. That is, in the absence of the NHIS, households at the left and right of the discontinuity in the assignment variable would have fared the same.

To present support for the assumption of no underlying selection on either side of the discontinuity, we perform several empirical balancing tests.[18] First, we estimate specification (1) using pre-determined household characteristics as dependent variables. We select characteristics that, a priori, should not be affected by the NHIS introduction such as household composition, the main source of household income, and education of adult household members. Column (1) of Table 2 reports no statistically significant discontinuities around the cutoff, except for the fraction of female household members without any education.

To further test the balancing of characteristics on both sides of the discontinuity, we assess whether the sample averages of household characteristics are similar in the samples before and after NHIS implementation. Columns (2), (3) and (4) in Table 2 demonstrate that household characteristics are similar in both samples. Only the household size and the share of the head of the households born in the village are significantly smaller for the sample observed after the introduction of the NHIS.[19]

It is possible that we do not find significant differences between samples around the cutoff because of our small sample size. To overcome this, we present the standardized differences along with 95 percent confidence intervals. We now observe that some of the characteristics are significantly different before and after NHIS implementation. These are the fraction of rural households, the fraction of individuals living below the poverty line, the fraction of traditional households, the fraction of households in which the father lives in the household, as well as the size of the household and number of children in the household. After NHIS implementation, we observe larger values of these variables, which tend to be associated with greater vulnerability to expenditure shocks. This suggests that estimates presented in the subsequent section may be low bounds of the full effect of the NHIS.

[17]Limiting the analysis to households within a six-month bandwidth before and after NHIS implementation reduces our sample to 2,235 households.

[18]Although manipulation of the running variable is not a potential issue in this setup, we confirm it by running a McCrary (2008) density test. We do not reject the null hypothesis of a smooth density around the time of exposure to the NHIS (p-value=0.18).

[19]Yet, data six months away from NHIS introduction are systematically different from data just around the date of the introduction, highlighting the need for inclusion of $f(.)$ and $h(.)$ in the regression equation.

Table 2: Balancing Test of Means Around the Months of Exposure to the NHIS

	RD test	Sample of analysis		Test of equal means	Standard diff.	C.I.
		$d \in [-6;0)$	$d \in [0;6]$	p-value		(95%)
	(1)	(2)	(3)	(4)	(5)	(6)
Rural household	0.039	0.698	0.712	0.536	0.101	[0.012; 0.189]
	(0.060)	(0.460)	(0.453)			
Poor	0.046	0.361	0.399	0.113	0.124	[0.035; 0.213]
	(0.047)	(0.481)	(0.490)			
Farmers	0.033	0.590	0.599	0.690	0.070	[-0.019; 0.159]
	(0.046)	(0.492)	(0.490)			
Traditional household	−0.047	0.205	0.193	0.546	−0.100	[-0.188; -0.011]
	(0.044)	(0.404)	(0.395)			
Household size	−0.343	6.293	5.987	0.021	−0.176	[-0.265; -0.087]
	(0.380)	(2.907)	(2.585)			
Father in household	−0.007	0.725	0.716	0.688	−0.096	[-0.185; -0.007]
	(0.039)	(0.447)	(0.451)			
Married or cohabiting	−0.041	0.853	0.829	0.178	−0.095	[-0.184; -0.006]
	(0.032)	(0.354)	(0.377)			
Age head	1.208	47.579	48.133	0.394	< 0.001	[-0.089; 0.088]
	(0.903)	(12.714)	(13.183)			
Number children in household	−0.092	2.330	2.243	0.200	−0.092	[-0.181; -0.003]
	(0.181)	(1.476)	(1.312)			
Formal sector (paid worker)	−0.001	0.040	0.037	0.747	−0.025	[-0.114; 0.063]
	(0.009)	(0.197)	(0.189)			
Informal sector (paid worker)	−0.022	0.044	0.049	0.607	−0.003	[-0.092; 0.086]
	(0.019)	(0.205)	(0.217)			
Own business	−0.020	0.231	0.252	0.317	0.055	[-0.033; 0.144]
	(0.035)	(0.422)	(0.434)			
Not working	−0.002	0.013	0.015	0.666	−0.035	[-0.124; 0.054]
	(0.007)	(0.113)	(0.123)			
Born in village	−0.017	0.659	0.611	0.046	0.042	[-0.047; 0.131]
	(0.054)	(0.474)	(0.488)			
No education (father)	−0.039	0.504	0.524	0.404	0.075	[-0.013; 0.164]
	(0.041)	(0.500)	(0.500)			
Some school (father)	0.010	0.207	0.193	0.487	−0.057	[-0.146; 0.032]
	(0.068)	(0.405)	(0.395)			
No education (mother)	−0.067**	0.275	0.304	0.194	0.057	[-0.031; 0.146]
	(0.030)	(0.447)	(0.460)			
Some school (mother)	0.022	0.183	0.177	0.727	−0.053	[-0.142; 0.036]
	(0.066)	(0.387)	(0.381)			
Households	2,235	717	1,518			

Notes: The first column reports the θ coefficients estimation of specification (1) with demographic characteristics as the dependent variable. Standard errors are in parenthesis. Columns (2) and (3) report the sample averages, and standard deviation in parentheses, 6 months before and 6 months after the NHIS implementation, respectively. Column (4) reports the p-value of a test of equal means.

Analysis by Health Shock. To gain intuition on the extent to which the effect of the NHIS varies with health, we split the sample using a measure of health shock that we construct based on the self-reported number of days of sickness over the two weeks before the interview. Specifically, we define our health shock measure as the ratio of the weighted sum of the days that every adult in the household has reported being ill over the two weeks before the interview, by the period of reference (14 days). That is:

$$HS_{hts} = \frac{\sum_{i=1}^{I} a_{ihts}}{14n_h},$$

where a are the days individual i from household h has reported being ill or injured over the two weeks before the interview, and n_h is the number of individuals in household h.

By distinguishing households according to the extent of a health shock as opposed to whether a sickness episode is reported at all, we aim to solve the endogeneity problem that arises when households remember better an episode of sickness if they sought health care (i.e., reporting behavior is positively affected by the introduction of the NHIS). The rationale is that intensity of sickness is unrelated to the introduction of the NHIS if households correctly report the number of sickness days once they remember a sickness episode. To verify that, we estimate specification (1) using HS as dependent variable. By construction of this variable, the estimation sample only includes households that report any illness episode. The first column in Table 3 reports the results. We observe no statistically significant effect of the introduction of the NHIS. The same applies even when examining the (standardized) sample means in the six months before and after the NHIS implementation.

We use HS to divide households with at least one day of sickness into low and high intensity of sickness, according to whether HS is below or above the median of the full sample (i.e., the sample not bounded by the months to NHIS introduction). Among households reporting an illness or injury, the median of this sickness intensity measure is 0.1. That is, for the median household, 10 percent of the days in the last two weeks were lost to illness. Low sickness households, therefore, are households in which less than 10 percent of the days in the last two weeks were lost to illness.

As a comparison to our main health shock measure, we construct a second variable by taking the ratio of the weighted sum of the days that every adult in the household reported having their activities of daily living (ADL) limited by an illness or injury. Analogous to the previous measure, we construct:

$$ADL_{hts} = \frac{\sum_{i=1}^{I} adl_{ihts}}{14n_h},$$

where adl are the days individual i from household h has reported a limitation on their ADL due to sickness, over the two weeks before the interview. The second row of Table 3 displays the sample means of this alternative health shock measure, suggesting a higher duration of illness and higher ADL scores among households observed after the introduction of the NHIS. However, these differences, are small and not statistically significant, giving confidence that splitting the sample by the median duration of ADL limitations from ill-health will not result in selecting significantly different samples before and after the introduction of the NHIS.

The median limitation ratio of illnesses over all household members is 0.07. Accordingly, we classify households in which less than 7 percent of the days in the last two weeks were affected by an ADL limiting illness as low ADL limited households.

Table 3: Validity Test of Health Shock Measures

	RD test	Sample of analysis		Test of equal means	Standard diff.	C.I.
		$d \in [-6; 0)$	$d \in [0; 6]$	p-value		(95%)
	(1)	(2)	(3)	(4)	(5)	(6)
HS ratio	-0.007	0.133	0.131	0.729	0.001	[-0.115; 0.118]
	(0.011)	(0.097)	(0.113)			
ADL ratio	-0.002	0.097	0.099	0.711	0.035	[-0.102; 0.172]
	(0.009)	(0.075)	(0.090)			
Households	2,235	717	1,518			

Notes: See Table 2.

5 Estimation Results

5.1 Medical Expenditures

The first row of Table 4 presents the RD estimates of health care expenditures. To estimate specification (1) we include control variables measured at the time of the survey (age and gender of the head of the household, educational attainment of parents, household composition, and employment sector of the head), district and time fixed effects.

Column (1) reports the effects of the NHIS on the full sample, which we estimate to be 21 percent. The estimated effect of the NHIS on medical expenditures is comparable to other estimates of the effect of universal health coverage on OOP payments, which find savings on medical expenditures ranging from 30 to 50 percent (Wagstaff, 2010; Bauhoff et al., 2011; Limwattananon et al., 2015; Strupat & Klohn, 2018). To explore the source of OOP savings from the introduction of the NHIS, the subsequent rows of Table 4 decompose the OOP payments between outpatient care and medicine purchases. We estimate small and non-statistically significant effects of the introduction of the NHIS on the expenditures in outpatient care. Despite obtaining a large point estimate of the effect of the NHIS on drug expenditures, the effect is not statistically significant.

An issue with interpreting the effects from the NHIS on medical expenditures on the full sample comes from the fact that the GLSS only records medical expenditures of individuals reporting an illness or injury, and thus the effect is estimated from comparing the responses between these households reporting an illness or injury, and households that did not. Since households reporting an illness or injury are possibly different before and after NHIS implementation (see Table 1), we split the sample of households reporting some illness or injury by the previously defined measures of health shock.

Columns (2) and (3) limit the sample by days of sickness of adults in the household, and Columns (4) and (5) by days in which ADL was limited due to the sickness of adults in the household. The results yield an estimated effect of the NHIS on OOP payments of -35 percent among households with more sickness intensity. In contrast, we do not find statistically significant effects among households with less sickness intensity. This drop was driven by large savings in medicine purchases, which we estimate to be 66% among households with more sickness.

We reach similar conclusions when splitting the sample by days of ADL limitations. Households with less ADL limitations experienced lower savings in their OOP payments than households with more incapacitating sickness. Again, savings in OOP were driven by a drop in drugs expenditures, of 63%. Because of the closeness in the estimates for both health shock measures, we will present in what follows the results only by the measure of intensity of sickness. Results by ADL limitation are available upon request.

5.2 Risk Coping Strategies

Consumption. The first row in Table 5 shows the RD coefficients on the logarithm of frequent consumption. We find a small statistically significant increase of 3% in frequent consumption from the NHIS implementation for the full sample. This effect was driven by households experiencing more sickness, for whom we estimate an 8% increase in consumption.

When disaggregating the measure of frequent consumption by its components, we find non-statistically significant effects of the NHIS on food consumption for the full sample and a statistically significant increase of 0.6% for the sample of households with more sickness intensity. In comparison, non-food consumption increased by 1.2 percent for the full sample after NHIS implementation. The point estimate of this effect was again larger for households with more sickness intensity (2.5 percent). These results suggest that non-food expenditures

Table 4: RD Estimates for Out of Pocket Payments, Disaggregated (Logarithm)

	Full Sample	Less Sickness	More Sickness	Less Limited ADL	More Limited ADL
	(1)	(2)	(3)	(4)	(5)
Total OOP payments	−0.208**	−0.067	−0.353**	−0.221	−0.417***
	(0.074)	(0.081)	(0.164)	(0.199)	(0.134)
Outpatient expenditures	−0.053	0.006	−0.118	−0.129	−0.133
	(0.087)	(0.083)	(0.162)	(0.176)	(0.123)
Medicine purchases	−0.336	0.173	−0.659***	−0.222	−0.632***
	(0.261)	(0.126)	(0.297)	(0.236)	(0.226)
Number of households					
Before Cutoff	1,518	245	356	246	284
After Cutoff	717	356	248	155	173
Total	2,228	601	604	439	419

*Notes: Column (1) reports estimates on the full sample of households in districts that implemented the NHIS during the time of the survey, and for which we observe subdistricts interviewed within nine months before or after the implementation of the NHIS at the district level. Columns (2)/(3) limit the analysis to households in which none/at least of the members reported hospitalization in the previous two weeks, respectively. Medical expenditures measure the self-reported expenditures within the two weeks before the interview. All coefficients are estimated using an RD model with separate linear trends on each side of the cutoff and triangular weights. All specifications include district and time fixed-effects, as well as control variables measured at the time of the survey (age and gender of the head of the household, educational attainment of parents of the parents, household composition, and sector of the head). Standard errors in parentheses are clustered at the enumeration area (EA) level. **p<0.05, *p<0.10*

have absorbed more of the health shock as a way of insulating food consumption (Skoufias & Quisumbing, 2005).

Our results indicate that about a fifth of the estimated savings in medical expenditures were used for higher household consumption. In what follows, we will evaluate how the remaining savings in medical expenditures shaped household's behavior.

Table 5: RD Estimates for Frequent Consumption (Logarithms)

	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)
Frequent consumption	0.034**	0.044	0.082***
	(0.016)	(0.027)	(0.031)
Food	0.002	0.004	0.006**
	(0.002)	(0.003)	(0.003)
Non-food	0.012***	0.008	0.025***
	(0.003)	(0.005)	(0.007)
Number of households			
Before Cutoff	1,518	245	356
After Cutoff	717	356	248
Total	2,228	601	604

Notes: See notes in Table 4.

Loans and Remittances. The first row of Table 6 shows the effect of NHIS access on loan take up. We find neither a statistically significant effect of loan take up for the full sample, nor for the sample of households divided by sickness intensity. In contrast, the amount borrowed decreased significantly in the full sample and in the sample of households with less sickness. One explanation is that the NHIS rendered it more difficult for households to justify borrowing to cover health expenditures, especially when they faced a mild episode of illness or injury.

The third row of Table 6 reports whether families received remittances for health purposes. The reduced form estimates show 3.1 percentage points drop in the remittances received for the full sample. This drop amounts to 2.2 percentage points among households with more sickness (statistically significant at the 10 percent level), and is non-statistically significant for households with less sickness intensity. The estimates on the log of the amount of remittances is consistent with the results at the extensive margin. We estimate a drop in 4% of remittances received for the full sample and a drop in 2.8% for households with more sickness intensity. In relative terms, the drop in remittances represents a 20% of the drop in medical expenditures for the full sample, and a 8% of the drop for households with more sickness.

Table 6: RD Estimates for Loans and Remittances

	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)
Loan (0/1)	−0.007	−0.026	−0.006
	(0.013)	(0.067)	(0.019)
Amount borrowed (Log)	−0.154***	−0.390***	−0.049
	(0.068)	(0.161)	(0.089)
Remittances (0/1)	−0.031***	−0.022	−0.022*
	(0.010)	(0.020)	(0.013)
Amount received (Log)	−0.040***	−0.027	−0.028*
	(0.013)	(0.025)	(0.017)
Number of households			
Before Cutoff	1,518	245	356
After Cutoff	717	356	248
Total	2,228	601	604

Notes: See notes in Table 4.

Labor Supply of Adults. The first column in Table 7 reports the RD estimates of the labor supply of ill and healthy adults in households reporting any illness or injury. We find small and non-statistically significant positive effects on the employment participation of ill adults in households with some sickness and less sickness. The point estimates are larger in households with more sickness, although this difference is not statistically significant. We do not find statistically significant effects of the introduction of the NHIS at the intensive margin.

Panel B reports the RD estimates for healthy adults. For healthy adults in households with some sickness, we estimate a sizeable significant drop in employment participation of 8% to 11%. At the intensive margin, healthy adults reduced their weekly working hours by 4% and 5% for households with some and more sickness, respectively. This result suggests that the NHIS crowded out labor supply of healthy adult household members as a source of self-insurance against health shocks. The reduction in employment participation at the extensive and intensive margins among healthy adults, however, did not translate into lower labor earning. This is possibly due to the fact that remuneration is in-kind in many cases.

Table 7: RD Estimates of Adult Labor Supply

	Some Sickness	Less Sickness	More Sickness
	(1)	(2)	(3)
***A.** Ill Adult*			
Employment participation	0.020	0.025	0.047
	(0.026)	(0.042)	(0.038)
Labor earnings *(Log)*	0.059	0.020	0.078
	(0.053)	(0.052)	(0.070)
Hours worked *(Log)*	−0.027	−0.034	0.050
	(0.071)	(0.070)	(0.060)
***B.** Healthy Adult*			
Employment participation	−0.082***	0.017	−0.108**
	(0.033)	(0.035)	(0.048)
Labor earnings *(Log)*	−0.010	−0.002	−0.012
	(0.014)	(0.012)	(0.017)
Hours worked *(Log)*	−0.039***	0.011	−0.053***
	(0.014)	(0.012)	(0.020)
Number of households			
Before Cutoff	1,130	245	356
After Cutoff	452	356	248
Total	1,578	601	604

Notes: See notes in Table 6. Only households experiencing a health shock reported. Column (1) reports the labor market outcomes of the adults in the household experiencing the health shock, and column (2) reports the outcomes of the healthy adults in these households.

5.3 Costly Risk Coping Measures

Our results suggest that three-fifths of the savings in medical expenditures were neither used to increase consumption nor resulted in lower use of formal and informal borrowing. The question that remains unanswered, thus, is to what extent were households under-insured against expenditures associated with health shocks? To further explore this question, we now evaluate the impact of the NHIS on costly insurance mechanisms to cope with health shocks.

Sales of Productive Assets. Table 8 reports the estimated effect of the introduction of the NHIS on the sale of productive assets and land. Our results suggest negative, although non-statistically significant, point estimates on the probabilities of selling assets and land. This finding stands in contrast to results presented in Mitra et al. (2016) and may be explained by the required time to liquidate assets in the study context that makes them less suitable to cover instantaneous health expenditures.

Table 8: RD Estimates for Assets

	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)
Sold asset (0/1)	−0.064	−0.102	−0.046
	(0.079)	(0.140)	0.091
Sold land (0/1)	−0.074	0.130	−0.170
	(0.194)	(0.224)	(0.197)
Number of households			
Before Cutoff	1,518	245	356
After Cutoff	717	356	248
Total	2,228	601	604

Notes: See notes in Table 4.

Children's Outcomes. We now move on to explore the potential effect of public health insurance in preventing the disinvestment in the human capital of children. First, we analyze the effects for the full sample to then examine effect heterogeneity across gender.

Table 9 reports the RD estimates on schooling (Panel A) and child labor outcomes (Panel B). Class attendance and school enrollment significantly increased by 2% and 5 percentage points as a result of NHIS introduction, respectively. We estimate small and statistically non-significant effects on the hours children spent doing their homework. School costs were neither affected by the NHIS. One interpretation of these opposing results is that, along the lines of our argumentation in case of the measure of health status, households were more likely to remember that a child was enrolled in school when he or she currently was attending classes. Thus, effects on school enrollment may be due to the effect of NHIS on reporting behavior.

Panel B in Table 9 explores whether the NHIS had an impact on the incidence of child labor. We find that child labor was negatively affected by the NHIS, but the point estimate is statistically insignificant. However, for households with more sickness, we find that the NHIS reduced the incidence of child labor by eight percentage points (about 22%).

Despite finding a significant drop in the incidence of child labor in households experiencing higher sickness intensity, we do not find that the NHIS significantly reduced children's labor earnings. The reason behind this may lie in the low fraction of reported labor earnings for children: in fact, only about a fourth of the children identified as child laborers report positive payments for their work. Possibly, child laborers often receive in-kind compensation for their work rather than a formal wage. This is in line with the fact that child laborers in our sample work mostly in the agricultural sector. In addition to working outside their homes, children appear to be spending more time on household chores in the face of a health shock in the household. We estimate that the introduction of the NHIS reduced the weekly hours spent doing household chores by 1.8%. This reduction, in absolute terms, increases to 2.8% among households experiencing high sickness intensity.

Our results are suggestive that, when faced with lengthy ailments, households resort to limiting human capital investments on children, by reducing their class attendance and increasing their employment. When the NHIS offered coverage of medical expenditures, households were able to cope with health shocks without reducing class attendance and requiring further involvement of children in the productive activities of the household.

Table 9: RD Estimates for Schooling and Child Labor

	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)
A. Schooling outcomes			
School enrollment (0/1)	0.052***	0.028	0.039
	(0.019)	(0.024)	(0.026)
Class attendance (Log)	0.020***	0.015*	0.026*
	(0.008)	(0.009)	(0.014)
Hours homework	0.275	−0.045	0.795
	(0.234)	(0.247)	(0.576)
School costs (Log)	−0.088	0.030	−0.069
	(0.086)	(0.112)	(0.120)
B. Labor			
Child work (0/1)	−0.010	−0.003	−0.080***
	(0.060)	(0.088)	(0.024)
Labor earnings (Log)	−0.013	−0.026	−0.038
	(0.022)	(0.041)	(0.047)
Household Chores (Log)	−0.016***	−0.018***	−0.028***
	(0.005)	(0.004)	(0.010)
Number of households			
Before Cutoff	1,518	245	356
After Cutoff	717	356	248
Total	2,228	601	604

Notes: See notes in Table 6.

Boys and girls may play different roles within the household in coping with health shocks. Table 10 reports descriptive statistics for boys and girls. Boys and girls have similar rates of school attendance and child work. The costs of schooling of boys and girls were similar in the pre-NHIS sample. In the post-NHIS sample, however, school spending on boys is statistically larger than in the pre-NHIS sample, while school spending on girls does not vary across periods. Incidence in child labor among boys and girls was smaller in the post-NHIS sample than in the pre-NHIS sample, although, in absolute terms, this fall was larger among girls. There was a sizeable difference in the hours boys and girls spent on household chores: before the NHIS, girls spent an average of 8 hours per week doing household chores, while boys spent around 4 hours. Although both boys and girls spent fewer hours on household chores in the post-NHIS sample, the difference across gender remained high after the introduction of the NHIS, with girls spending significantly more time on them.

We note that there was a small change in the age composition of girls in the samples before and after the NHIS was introduced: girls were younger on average in the sample observed after the NHIS. This difference across samples is statistically significant but, at the same time, small in absolute terms (less than four months). Hence, we do not expect that it could drive gender dis-aggregated results.

Table 10: Descriptive Statistics by Gender of Child

	Boys			**Girls**		
	Sample of analysis		Test of equal means	Sample of analysis		Test of equal means
	Before NHIS (1)	After NHIS (2)	p-value (3)	Before NHIS (4)	After NHIS (5)	p-value (6)
Age	11.52	11.75	0.14	11.90	11.62	0.08
	(2.64)	(2.70)		(2.84)	(2.73)	
Schooling						
School enrollment	0.39	0.40	0.62	0.31	0.32	0.75
	(0.31)	(0.32)		(0.27)	(0.27)	
Hours attendance	17.24	23.27	< 0.01	16.35	21.38	< 0.01
	(29.82)	(33.76)		(27.67)	(30.50)	
Hours homework	0.63	0.57	0.50	0.61	0.52	0.29
	(1.82)	(1.71)		(1.88)	(1.65)	
School costs ($\times 10,000$)	5.60	7.13	0.08	5.90	5.64	0.73
	(11.74)	(19.32)		(14.34)	(14.59)	
Labor						
Child work	0.15	0.09	< 0.01	0.15	0.07	< 0.01
	(0.31)	(0.27)		(0.31)	(0.24)	
Hours household chores	3.66	1.87	0.03	7.67	3.75	< 0.01
	(21.72)	(14.04)		(36.38)	(19.18)	

Notes: See Notes in Table 1.

Table 11 reports the estimated effects of the NHIS on children outcomes, by gender of the child. On average, class attendance of boys increased by roughly 1% as a result of the introduction of the NHIS. This increase comes from boys in households experiencing more sickness. We find no effects of the NHIS on the incidence of child labor among boys. We estimate a modest and statistically significant decrease in the amount of hours that boys living in households with more sickness spent doing household chores (last row of Column 3).

Girls school enrollment and class attendance significantly increased for the full sample. We do not find any statistically significant effects for the more sickness sub-sample. This suggests that the results are driven by household that do not experience serious health shocks and is indication that induced change in economic uncertainty also has effects ex ante, i.e., before a serious shock actually takes place. Alternatively, this result may simply be due

Table 11: RD Estimates for Schooling and Labor by Gender of Child

	Boys			Girls		
	Full Sample	Less Sickness	More Sickness	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)	(4)	(5)	(6)
A. Schooling outcomes						
School enrollment (0/1)	0.015	0.004	0.004	0.036**	0.040	−0.021
	(0.026)	(0.056)	(0.036)	(0.016)	(0.034)	(0.023)
Class attendance (Log)	0.009**	0.005	0.011***	0.007**	0.007**	0.003
	(0.004)	(0.006)	(0.006)	(0.003)	(0.003)	(0.007)
Hours homework	0.098	−0.212	0.370	0.228	0.019	0.755
	(0.187)	(0.179)	(0.314)	(0.163)	(0.149)	(0.614)
School Fees (Log)	−0.036	0.014	0.017	−0.006	0.016	0.042
	(0.036)	(0.116)	(0.062)	(0.036)	(0.110)	(0.040)
B. Labor						
Child work (0/1)	−0.015	−0.002	−0.032	−0.031	−0.008	−0.108***
	(0.036)	(0.069)	(0.036)	(0.077)	(0.093)	(0.021)
Labor earnings (Log)	−0.011	−0.032	−0.032	−0.008	0.001	−0.024
	(0.021)	(0.035)	(0.046)	(0.009)	(0.013)	(0.022)
Household Chores (Log)	−0.003	−0.001	−0.008**	−0.015***	−0.017***	−0.025***
	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)	(0.010)
Number of households						
Before Cutoff	1,072	302	153	1,056	290	152
After Cutoff	536	151	282	553	146	284
Total	1,609	453	435	1,584	436	436

Notes: See notes in Table 6.

to a reduction in reporting error that arise if households better remember the enrollment status of children in case they regularly attend class. We further estimate that the NHIS reduced the incidence of child labor of girls living in households with more sickness by 10.8 percentage points, and their hours spent on household chores by 2.5% (Column 6).

None of the effect estimates statistically differ between boys and girls for the full sample even though, in absolute terms, the impacts seem to be larger for girls (compare Columns 1 and 4). On the contrary, we find that the effect of NHIS on class attendance is significantly greater for boys relative to girls among households experiencing more sickness. Girls, however, benefit more than boys from NHIS introduction in terms of the reduction in child labor and household chores in the same subsample. In addition, we observe a remarkable gender-related asymmetry in the relationship between child work and class attendance. Our results suggest that reduced household chores did translate into increased class attendance only in the case of boys, even though the NHIS freed up less time for boys relative to girls (Columns 3 and 6). This is possibly due to larger baseline school enrollment rates of boys (see Table 10), which allow them to more flexibly attend classes.

Table 12: RD Estimates for Medical Expenditures (Logarithm) Using Different Bandwidths

	Full Sample	Less Sickness	More Sickness	Less Limited ADL	More Limited ADL
	(1)	(2)	(3)	(4)	(5)
			Bandwidth 5 months		
Total OOP payments	−0.205***	−0.074	−0.337***	−0.230	−0.394***
	(0.074)	(0.077)	(0.172)	(0.203)	(0.134)
Outpatient expenditures	−0.043	0.011	−0.091	−0.129	−0.099
	(0.092)	(0.083)	(0.169)	(0.178)	(0.141)
Medicine purchases	−0.333	0.181	−0.691***	−0.239	−0.632***
	(0.267)	(0.127)	(0.291)	(0.240)	(0.240)
Number of households					
Before Cutoff	1,396	379	328	262	229
After Cutoff	708	189	248	155	173
Total households	2,097	567	576	417	402
			Bandwidth 7 months		
Total OOP payments	−0.220***	−0.037	−0.408***	−0.256	−0.431***
	(0.069)	(0.057)	(0.163)	(0.151)	(0.095)
Outpatient expenditures	−0.034	0.021	−0.028	0.003	−0.048
	(0.066)	(0.067)	(0.118)	(0.116)	(0.130)
Medicine purchases	−0.440*	0.076	−0.705***	−0.211	−0.843***
	(0.236)	(0.138)	(0.237)	(0.179)	(0.204)
Number of households					
Before Cutoff	1,799	502	414	343	292
After Cutoff	725	196	249	161	173
Total	2,517	697	663	504	465
			Bandwidth 8 months		
Total OOP payments	−0.217***	−0.043	−0.410***	−0.257	−0.426***
	(0.063)	(0.056)	(0.164)	(0.143)	(0.098)
Outpatient expenditures	−0.018	0.056	0.005	0.068	−0.052
	(0.060)	(0.070)	(0.126)	(0.140)	(0.131)
Medicine purchases	−0.511***	0.050	−0.704***	−0.213	−0.844***
	(0.212)	(0.126)	(0.236)	(0.172)	(0.190)
Number of households					
Before Cutoff	1,518	245	356		
After Cutoff	717	356	248		
Total	2,228	601	604		

*Notes: See Notes in Table 4. **p<0.05, *p<0.10*

5.4 Sensitivity Analysis

To test the sensitivity of our results, we reproduce all estimates using three different bandwidths: 5, 7 and 8 months before and after NHIS implementation. Table 12 reports the results on OOP spending. The direction and significance of our results are very robust to any changes in the bandwidth, with only minor differences in the point estimates. Decreasing the bandwidth from 6 to 5 months slightly decreases the estimated effects, although in most of the cases this change does not affect levels of statistical significance (Panel A). Increasing the bandwidth from 6 to 7 months (Panel B) and from 6 to 8 months (Panel B) increases the point estimates. An increase in the bandwidth from 7 to 8 months barely changes the estimated results, with the exception of the point estimate for medicine purchases in the full sample, which turns larger. For the other outcome variables, we find similar results of the sensitivity analysis (see Tables B4-B5 in the Appendix).[20]

6 Conclusion

In this paper, we exploit the fact that the implementation of the NHIS overlapped with the roll-out of the fifth round of the GLSS. We can observe, within the same district, subdistricts interviewed right before the NHIS introduction, and subdistricts interviewed right after. Because the timing of the interview was external to the timing of the NHIS adoption, whether a household was observed before or after the NHIS implementation was as good as random within the same district. We exploit this variation using a regression discontinuity design, where the running variable was the months' to NHIS implementation at the district level.

We find that the introduction of the country-wide health insurance induced savings in health care expenditures, particularly in medicines. Decreased OOP payments have translated into greater non-food frequent consumption, and a drop in remittances received. Our results suggest that these responses from NHIS implementation were larger for households that experienced lengthier and more incapacitating disabilities. Importantly, for these households, we find a drop in child labor and an increase in class attendance. This suggests that households experiencing severe health shocks did not have to resort to dis-investments in human capital of their children.

Our study closes an important knowledge gap in the field of research on child labor as evidence on the impacts of country-wide public health insurance schemes is currently missing

[20] While changing the bandwidth around the cutoff does not change the direction of the results for assets, the estimates seem to be somewhat sensitive and larger in absolute terms. We base our interpretation on the conservative results.

in the literature (Dammert et al., 2018). Our findings confirm previous results from micro insurance schemes (Landmann & Frölich, 2015; Chakrabarty, 2012) and are very encouraging with respect to the potential of the public health sector to contribute to the reduction of child labor at large scale. As child labor is in the long run arguably one of the most welfare-decreasing coping strategies due to negative implications for human capital formation, cognitive development, and economic activities later in life, public health insurance schemes seem to have greater social value than previously thought (e.g., Townsend, 1994). Policy makers should consider this value in their cost-benefit analysis when deciding on the implementation of public health insurance schemes.

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Appendices

A Variable Creation

Table A1: Data Sources and Variable Definitions

Variable	Survey question	Selection criteria	Timing and Units
I. Health outcomes			
Hospitalization	Were you hospitalized in the past two weeks?	=1 if at least one household member responded yes	0/1 dummy, over the past two weeks;
Severity of illness	Days of illness/injury over the past two weeks?	Sum of all household members days of illness/injury over the past two weeks, divided by 14	ratio, over the past two weeks;
Low severity of illness	−−	Severity of illness $\leq$ median severity sample (7 days)	binary, over the past two weeks;
High severity of illness	−−	Severity of illness $>$ median severity sample (7 days)	binary, over the past two weeks;
Total OOP payments	Total medical expenditures	Sum of household medical expenses related to illness/injury over the past two weeks	GHS, over the past two weeks;
Outpatient expenditures	Outpatient expenditures	Household outpatient expenses related to illness/injury over the past two weeks	GHS, over the past two weeks;
Inpatient expenditures	Inpatient expenditures	Household inpatient expenses related to illness/injury over the past two weeks	GHS, over the past two weeks;
Medicine purchases	Value of medicine purchased	Household medicine expenses related to illness/injury over the past two weeks	GHS, over the past two weeks;
II. Consumption			
Household food consumption	How much was spent on ... since last visit?	Amount spent on food items by household	GHS, over the past week;
Household non-food frequent consumption	How much was spent on ... since last visit?	Amount spent on non-food items by household	GHS, over the past week;
III. Loans and remittances			
Remittances received for health purposes	During the past 12 months has this household received or collected money or goods from any other individual? Use of the cash received.	=1 if remittances received used for health purposes.	Binary, 12 months;
Amount of remittances received for health purposes	What was the total amount of the cash this household received from this individual during the past 12 months? Use of the cash received.	Amount received in remittances that will be primarily used for health.	GHS, 12 months;
Loans for health purposes	Does any member of the household owe money or goods to another person, institution, or business? Purpose of the loan.	=1 if money borrowed for health purposes.	Binary, at the time of interview;
Amount of loans for health purposes	What was the total amount of the original loan? Purpose of the loan.	Amount of money borrowed for health purposes.	GHS, at the time of interview;

Table A2: Data Sources and Variable Definitions (cont.)

Variable	Survey question	Selection criteria	Timing and Units
IV. Labor market of adults			
Ill adult employment	Did any work in the past seven days?	=1 if the adult hospitalized over the past two weeks of the household responded yes	0/1 dummy, over the past week;
Ill adult labor earnings	Will you receive any payment for this work? Amount received?	Logarithm of the monetary payment received for work by ill adult	GHS, over the past week;
Healthy adult employment	Did any work in the past seven days?	=1 if the adult not hospitalized responded yes	0/1 dummy, over the past week;
Healthy adult labor earnings	Will you receive any payment for this work? Amount received?	Logarithm of the monetary payment received for work by healthy adult	GHS, over the past week;
V. Child labor			
Child labor	Hours worked in the past 7 days?	=1 if children below 12 work more than one hour per week, children aged in between 12 and 13 work 14 hours or more, and teens between 14 and 17 work more than 43 hours	0/1 dummy, over the past week;
# of child laborers in the household	−−	Count of children per household falling in previous definition of child labor	categorical, over the past week;
VI. Schooling outcomes			
Hours of class attended	How many hours of class did Name attend last week?	Hours of school attend by child	Hours, weekly;
Hours of homework	How many hours of homework did Name do last week?	Hours of homework done by child	Hours, weekly;

B Sensitivity of Results

Table B1: RD Estimates for Consumption (Logarithm) Using Different Bandwidths

	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)
		Bandwidth 5 months	
Frequent consumption	0.027	0.032	0.079*
	(0.020)	(0.106)	(0.042)
Food	0.002	0.001	0.006
	(0.006)	(0.012)	(0.009)
Non-food	0.029	0.007	0.024
	(0.020)	(0.032)	(0.018)
Number of households			
Before Cutoff	1,396	379	328
After Cutoff	708	189	248
Total households	2,097	567	576
		Bandwidth 7 months	
Frequent consumption	0.041*	0.031	0.084**
	(0.024)	(0.022)	(0.035)
Food	0.006	0.006	0.009***
	(0.005)	(0.009)	(0.003)
Non-food	0.009***	0.013	0.025***
	(0.002)	(0.007)	(0.009)
Number of households			
Before Cutoff	1,799	502	414
After Cutoff	725	196	249
Total	2,517	697	663
		Bandwidth 8 months	
Frequent consumption	0.045***	0.040	0.081***
	(0.018)	(0.039)	(0.033)
Food	0.007	0.006	0.012**
	(0.004)	(0.008)	(0.006)
Non-food 0.009***		0.016	0.030***
	(0.002)	(0.009)	(0.003)
Number of households			
Before Cutoff	1,884	536	430
After Cutoff	754	202	263
Total	2,631	737	693

Notes: See Notes in Table 4. **p<0.05, *p<0.10

Table B2: RD Estimates for Loans and Remittances Using Different Bandwidths

	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)
		Bandwidth 5 months	
Loan (0/1)	−0.014	−0.039	−0.001
	(0.016)	(0.072)	(0.054)
Amount borrowed (Log)	−0.173***	−0.438***	−0.001
	(0.074)	(0.180)	(0.102)
Remittances (0/1)	−0.028***	−0.014	−0.019
	(0.010)	(0.019)	(0.012)
Amount received (Log)	−0.036***	−0.016	−0.025
	(0.013)	(0.025)	(0.016)
Number of households			
Before Cutoff	1,396	379	328
After Cutoff	708	189	248
Total households	2,097	567	576
		Bandwidth 7 months	
Loan (0/1)	−0.012	−0.037	−0.011
	(0.017)	(0.049)	(0.050)
Amount borrowed (Log)	−0.150***	−0.268***	−0.062
	(0.057)	(0.186)	(0.089)
Remittances (0/1)	−0.028***	−0.009	−0.026***
	(0.008)	(0.017)	(0.010)
Amount received (Log)	−0.036***	−0.009	−0.034***
	(0.010)	(0.021)	(0.013)
Number of households			
Before Cutoff	1,799	502	414
After Cutoff	725	196	249
Total	2,517	697	663
		Bandwidth 8 months	
Loan (0/1)	−0.016	−0.039	−0.015
	(0.016)	(0.041)	(0.048)
Amount borrowed (Log)	−0.098	−0.160	−0.071
	(0.077)	(0.231)	(0.082)
Remittances (0/1)	−0.028***	−0.009	−0.027***
	(0.007)	(0.015)	(0.010)
Amount received (Log)	−0.035***	−0.010	−0.035***
	(0.009)	(0.019)	(0.012)
Number of households			
Before Cutoff	1,884	536	430
After Cutoff	754	202	263
Total	2,631	737	693

*Notes: See Notes in Table 4. **p<0.05, *p<0.10*

Table B3: RD Estimates for Adult Labor Supply by Health Status Using Different Bandwidths

	Some Sickness	Less Sickness	More Sickness
	(1)	(2)	(3)
		Bandwidth 5 months	
Ill Adult			
Employment	0.035	0.038	0.064*
	(0.025)	(0.044)	(0.036)
Labor earnings (Log)	0.071	0.025	0.096
	(0.052)	(0.056)	(0.062)
Hours worked (Log)	−0.019	−0.039	0.077
	(0.072)	(0.072)	(0.056)
Healthy Adult			
Employment	−0.085***	0.013	−0.110***
	(0.034)	(0.038)	(0.051)
Labor earnings (Log)	−0.012	−0.004	−0.014
	(0.015)	(0.013)	(0.019)
Hours worked (Log)	−0.040***	0.011	−0.056***
	(0.015)	(0.014)	(0.021)
Number of households			
Before Cutoff	837	227	197
After Cutoff	452	113	149
Total households	1,252	340	348
		Bandwidth 7 months	
Ill Adult			
Employment	0.002	0.012	0.019
	(0.025)	(0.041)	(0.040)
Labor earnings (Log)	0.001	−0.034	0.019
	(0.044)	(0.051)	(0.061)
Hours worked (Log)	−0.045	−0.032	0.002
	(0.063)	(0.070)	(0.067)
Healthy Adult			
Employment	−0.091***	0.004	−0.109**
	(0.038)	(0.045)	(0.055)
Labor earnings (Log)	−0.007	−0.004	−0.004
	(0.015)	(0.012)	(0.023)
Hours worked (Log)	−0.044***	0.002	−0.054***
	(0.016)	(0.017)	(0.020)
Number of households			
Before Cutoff	1,079	301	248
After Cutoff	435	118	149
Total	1,517	418	398
		Bandwidth 8 months	
Ill Adult			
Employment	0.003	0.018	0.017
	(0.022)	(0.035)	(0.038)
Labor earnings (Log)	−0.013	−0.059	0.010
	(0.050)	(0.063)	(0.062)
Hours worked (Log)	−0.053	−0.048	−0.013
	(0.059)	(0.061)	(0.068)
Healthy Adult			
Employment	−0.083***	0.019	−0.100***
	(0.037)	(0.045)	(0.056)
Labor earnings (Log)	−0.006	0.004	0.001
	(0.013)	(0.014)	(0.023)
Hours worked (Log)	−0.042***	0.007	−0.050***
	(0.015)	(0.015)	(0.021)
Number of households			
Before Cutoff	1,330	322	258
After Cutoff	452	121	158
Total	1,579	442	416

*Notes: See Notes in Table 4. **p<0.05, *p<0.10*

Table B4: RD Estimates for Assets Using Different Bandwidths

	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)
		Bandwidth 5 months	
Sold asset (0/1)	−0.070	−0.048	−0.040
	(0.039)	(0.075)	(0.036)
Sold land (0/1)	−0.081	0.103	−0.238***
	(0.113)	(0.129)	(0.105)
Number of households			
Before Cutoff	1,396	379	328
After Cutoff	708	189	248
Total households	2,097	567	576
		Bandwidth 7 months	
Sold asset (0/1)	−0.060*	−0.070	−0.047
	(0.032)	(0.066)	(0.028)
Sold land (0/1)	−0.084	0.042	−0.145***
	(0.084)	(0.112)	(0.077)
Number of households			
Before Cutoff	1,799	502	414
After Cutoff	725	196	249
Total	2,517	697	663
		Bandwidth 8 months	
Sold asset (0/1)	−0.071***	−0.074	−0.049
	(0.032)	(0.059)	(0.027)
Sold land (0/1)	−0.082	0.029	−0.123
	(0.075)	(0.090)	(0.083)
Number of households			
Before Cutoff	1,884	536	430
After Cutoff	754	202	263
Total	2,631	737	693

*Notes: See Notes in Table 4. **p<0.05, *p<0.10*

Table B5: RD Estimates for Schooling and Child Labor Using Different Bandwidths

	Full Sample	Less Sickness	More Sickness
	(1)	(2)	(3)
		Bandwidth 5 months	
School enrollment (0/1)	0.053***	0.015	0.052**
	(0.026)	(0.039)	(0.027)
Class attendance (Log)	0.017	0.014	0.021
	(0.010)	(0.010)	(0.016)
Hours homework	0.347	−0.026	0.959
	(0.248)	(0.219)	(0.692)
School costs (Log)	−0.070	0.038	−0.015
	(0.083)	(0.119)	(0.159)
Child work (0/1)	−0.015	−0.020	−0.091***
	(0.065)	(0.101)	(0.029)
Labor earnings (Log)	0.012	−0.010	−0.038
	(0.030)	(0.048)	(0.052)
Household chores (Log)	−0.014***	−0.014***	−0.024***
	(0.005)	(0.004)	(0.010)
Number of households			
Before Cutoff	1,396	379	328
After Cutoff	708	189	248
Total households	2,097	567	576
		Bandwidth 7 months	
School enrollment (0/1)	0.050	−0.009	0.060***
	(0.037)	(0.050)	(0.028)
Class attendance (Log)	0.018	0.007	0.024***
	(0.010)	(0.013)	(0.012)
Hours homework	0.250	−0.028	0.621
	(0.245)	(0.270)	(0.516)
School costs (Log)	−0.061	0.013	−0.080
	(0.069)	(0.107)	(0.138)
Child work (0/1)	−0.006	0.020	−0.080***
	(0.060)	(0.096)	(0.026)
Labor earnings (Log)	−0.026	−0.020	−0.040***
	(0.025)	(0.044)	(0.019)
Household chores (Log)	−0.014***	−0.015***	−0.022***
	(0.004)	(0.005)	(0.008)
Number of households			
Before Cutoff	1,799	502	414
After Cutoff	725	196	249
Total	2,517	697	663
		Bandwidth 8 months	
School enrollment (0/1)	0.038	0.017	0.036
	(0.045)	(0.053)	(0.024)
Class attendance (Log)	0.016	0.005	0.014
	(0.012)	(0.013)	(0.012)
Hours homework	0.253	−0.327	0.743
	(0.208)	(0.245)	(0.457)
School costs (Log)	−0.076	0.009	−0.080
	(0.066)	(0.109)	(0.135)
Child work (0/1)	0.021	0.039	−0.076***
	(0.059)	(0.096)	(0.023)
Labor earnings (Log)	0.022	−0.001	−0.050***
	(0.023)	(0.039)	(0.020)
Household chores (Log)	−0.015***	−0.015***	−0.023***
	(0.004)	(0.005)	(0.008)
Number of households			
Before Cutoff	1,884	536	430
After Cutoff	754	202	263
Total	2,631	737	693

*Notes: See Notes in Table 4. **p<0.05, *p<0.10*