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Can health-insurance help prevent child labor? An impact evaluation from Pakistan☆

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ABSTRACT

Child labor is a common consequence of economic shocks in developing countries. We show that reducing vulnerability can affect child labor outcomes. We exploit the extension of a health and accident insurance scheme by a Pakistani microfinance institution that was set up as a randomized controlled trial and accompanied by household panel surveys. Together with increased coverage the microfinance institution offered assistance with claim procedures in treatment branches. We find lower incidence of child labor, hazardous occupations and child labor earnings caused by the innovation. Boys are more often engaged in child labor in our sample, but also seem to profit more from the insurance innovation.

1. Introduction

Poor households in developing countries are especially vulnerable to economic shocks. As a consequence of adverse events such as accidents, they might have to sell productive assets, reduce consumption below critical values, take children out of school to save school fees, or send children to work as an additional income source. The economic literature on child labor (see Edmonds, 2008 for an excellent review) confirms that economic shocks are an important determinant of child labor for low-income households (e.g. Beegle et al., 2006; Dillon, 2013; Duryea et al., 2007). Insurance, on the other hand, is supposed to decrease vulnerability to shocks by smoothing its financial consequences. In this paper we estimate the effects of extending the availability of a health insurance product in Pakistan to additional household members on child labor.

The policy relevance of analyzing this research question is straightforward. Child labor is the focus of development initiatives around the world. Many studies show substantial negative effects of child labor, such as lower human capital accumulation (e.g. Heady, 2003; Rosati and Rossi, 2003; Gunnarsson et al., 2006), lower wages in adult life (Emerson and Souza, 2011) and potentially even negative long-term health outcomes (Kassouf et al., 2001). Evidence on an innovative potential tool in combating child labor therefore should be of substantial interest. Surprisingly, there exists little rigorous research on the effect of microinsurance on child labor so far.[1]

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[1] To the best of our knowledge, there is only one working paper comparing individuals without microcredit, microcredit clients and microcredit clients who

This paper analyzes the extension of an accident and health insurance scheme offered by the National Rural Support Program (NRSP), a large microfinance institution in Pakistan. It is a mandatory insurance for all clients, their spouses and their children below 18 years. In 2009, the program was extended to include supplementary household members (adult children of the client and other household members) on a voluntary basis. In addition, clients were assisted with claim procedures. This package of two innovations was implemented as a randomized controlled trial (RCT) in nine out of thirteen branch offices in urban Hyderabad.

We find robust evidence for less child labor as a result of the innovation package. There is strong evidence for households to rely less on child labor earnings and to reduce hazardous occupations. Effects tend to be larger for boys, which is not surprising as they are more affected by child labor in our sample. In supplementary analyses we find suggestive evidence that the dominant effect might be coming through the extension of insurance.

Two caveats should be pointed out: First, the study covers only 13 branches, which were randomly assigned to treatment and control. While a larger sample size was not possible for this pilot intervention due to operational constraints, a much larger number of branches would be preferable in further studies in order to obtain more precise estimates. We thus rely on a low-powered RCT. Yet, at least the availability of baseline data permits us to assess baseline balance. Second, household data was collected by staff members of the microfinance institution. Although the institution placed great emphasis on ensuring neutral data collection, one might still be concerned that knowledge about the treatment status might have influenced (unconsciously) the household surveying and data collection approach by the interviewers.

2. The health insurance innovation and its background

Pakistan is a poor country: 22.3% of the population live below the poverty line of 1.25 US$ per day and another 20.5% are classified as vulnerable (World Bank, 2012, p. 19). According to the Pakistan Ministry of Health (2009, p. 6) public health expenditures are about 0.6% of GDP which is much lower than in comparable countries, and 75% of health expenditures are paid by patients out of pocket. The quality of health service providers corresponds to this low level of public spending. While some companies and insurers have contracts with hospitals or run their own hospitals (with varying quality), the options for the poor are limited. There are public health facilities that are supposed to be for free, but they often offer poor quality and many elements such as drugs must be paid privately as they are not covered.[2] The Pakistan Ministry of Health (2009, pp. 5–6) describes the situation for low-income households as follows:

> *"Poor are not benefiting from the health system whereas they bear major burden of diseases. Expanded infrastructure is poorly located, inadequately equipped and maintained resulting in inadequate coverage and access to essential basic services. Private health sector continues to expand unregulated mainly in urban areas. Factors contributing to inadequate performance of health sector are deep rooted including weak management and governance, partially functional logistics and supply systems; poorly motivated and inadequately compensated staff, lack of adequate supportive supervision, lack of evidence based planning and decision making, low levels of public sector expenditures and its inequitable distribution."*

Due to the limited capacity and availability of public providers, patients in some situations are forced to seek expensive private medical care. This makes health shocks a substantial economic risk for poor households. Consequently, illness and health are ranked as the top priority by potential microinsurance clients when it comes to unpredictable risk events in Pakistan (World Bank, 2012, p. 28). Moreover, in this country with a majority of informal employment contracts there is no universal health insurance system. Instead, several arrangements coexist at a time. Social security (for police officers, soldiers, civil servants, etc.) only covers a tiny part of the population.[3] There are various alternative health insurance schemes on the provincial level or offered by a multitude of private insurers; however, they are often packaged with other insurance, restricted to formal sector corporate clients and have no national outreach (World Bank, 2012, p. 11). In any case, only 1.9% of households are estimated to use any kind of formal insurance product (World Bank, 2012, p. 21), and the most vulnerable households are generally not the target group. Only microfinance institutions currently provide insurance for the low-income population, but here mainly schemes combining credit with life insurance are prevalent. According to the World Bank (2012, p. 50), only NRSP is offering health microinsurance with significant outreach.

NRSP is a Pakistani non-profit organization committed to support poor and vulnerable households all over the country. It is part of the Rural Support Programs Network consisting of 12 rural support programs that are all active in distinct regions of Pakistan. NRSP is the largest of these support programs and serves more than two million households by offering different microfinance services (mainly credit) and client training.[4]

2.1. NRSP's microinsurance innovation

Given the need to cover health shocks of poor households, NRSP in 2005 started to bundle health insurance to their microcredit product. Before the start of the research project, the insurance was built into the credit and was mandatory for loan clients, for their spouses and all children of the client below 18 years.[5] The product covers hospital stays of more than 24 h with a cost ceiling of 15,000 rupees (approximately 175 US$). Covered expenses range from room charges, doctor's visits, drugs, operations, and pregnancy care to transportation costs. Also accidents leading to death or permanent disability are covered up to 15,000 rupees. Costs of hospitalization are reimbursed after contacting the MFI field officer and submitting bills along with other relevant documents. Similarly, claims after death or disability can be submitted to the MFI field officer. NRSP aims at settling all claims within 15 days.[6]

are covered by additional insurance with respect to their child labor outcomes (Chakrabarty, 2012). Most research focused on impacts of insurance on access to medical services, e.g. Wagstaff (2010), Wagstaff et al. (2009), Dror et al. (2006), Dekker and Wilms (2010), Jütting (2004). Some other work has been done on agricultural investment decisions with insurance (Giné and Yang, 2009) and crowding out effects on informal risk-sharing (Landmann et al., 2012).

[2] This information was gathered through multiple country-specific reports (Asian Development Bank, 2004; Asian Development Bank, 2005; Qamar et al., 2007). They describe the status of the Pakistani health system prior to the innovation that took place in 2009.

[3] Asian Development Bank (2005, p. 2) estimates that ". . .less than 3% of the total employed labor force" are covered under this formal scheme.

[4] See Rural Support Programmes Network (2012) for more detailed information.

[5] The insurance product gradually changed over time. It initially covered loan clients and their spouses and was expanded in 2009 (i.e. before the baseline data used in this paper was collected) to include minor children. Also other details changed, but the basic design is what we describe in the following. For a detailed description of early product characteristics and developments we refer to Qamar et al. (2007).

[6] The appendix provides a more detailed description of the insurance package and reimbursement practices.

Fig. 1. Location of treatment (dark) and control (bright) branches within Hyderabad, Pakistan.

Source: Google Earth with GPS coordinates of branch locations.

Table 1
Observations per branch at baseline.

Branch name	Treatment status	Number of households	Number of individuals
Garhi Khata	Treated	138	841
Gulshan e Hali	Control	258	1512
Hussainabad	Treated	96	587
Islamabad	Treated	153	908
Islamia Colony	Control	192	1145
Kotri-1	Treated	198	1218
Kotri-2	Control	141	874
Latifabad-12	Control	186	1222
Latifabad-5	Treated	147	897
Liaqat Colony	Treated	120	723
Pathan Colony	Treated	204	1322
Phuleli	Treated	105	564
Pretabad	Treated	159	1122
All		2097	12,935

However, it seems that not all clients and credit officers were aware of the new product, resulting in very low claim ratios (World Bank, 2012, p. 50; Qamar et al., 2007). In an effort to increase the social impact of its activities, NRSP in 2008 conducted a diagnostic survey in the area of Hyderabad. In this district in the south of Pakistan an estimated 9.3% of all households are organized through NRSP according to Rural Support Programmes Network, 2012. The survey indicated high prevalence of child labor especially in the hazardous glass bangle industry and still a high vulnerability to health costs, often caused by accident, surgery or illness.

Responding to the vulnerability of their clients, NRSP in 2009 introduced two components additional to the mandatory insurance as part of an experiment.[7] In randomly selected treatment branch offices only, additional household members (adult children of the client and other minor or adult household members e.g. aunts, cousins, parents) are offered a *voluntary* insurance for a premium of 100 rupees (approximately 1 US$) per adult and year.[8] Second, clients are visited monthly and asked whether they had incurred any medical costs and whether they needed assistance with claims. With increased coverage of individuals and easier filing of claims, NRSP deliberately targets child labor through a better protection of poor households.[9] These two components are introduced in the nine treatment, but not in the four control branches. The clients in the control branches are not aware of the treatment. Before the introduction of the modifications of the insurance, household baseline data is collected in all treatment and control branches at the same time. The geographic distribution of branches in urban Hyderabad (Sindh province) is shown in Fig. 1.

3. Data collection

The sample consists of all clients of the 13 branch offices whose credit appraisals have been conducted in September/October 2009. Thus, the complete client cohort of 2 months and their households are included in the study: 777 households in four control and 1320 in nine treatment branches. Table 1 provides the names and number of observations for each branch. The number of households interviewed varies between 96 and 258 per branch.

All households were interviewed prior to the innovation. This baseline survey took place in September and October 2009: During the social appraisal, households were interviewed with respect to their socio-demographic situation with an extended survey section on child outcomes and child labor. The technical appraisal for credit then took place usually within less than 2 weeks during which the additional insurance product was also offered. Together with the loan, health insurance is provided and for 'client, spouse and her children' the premium is automatically deducted from the loan. However, for any insurance of any additional family members, the households paid premiums in cash and receipts were issued. Health insurance hence starts immediately at loan disbursal and thus soon after baseline data collection and applies in treatment as well as control branches. The only difference between treatment and control branches are the contents of the insurance package.

Four follow-up surveys are conducted afterwards every 6 months: March/April 2010, October/November 2010, May/June 2011 and October/November 2011. In each survey data was collected on various outcome variables with a detailed section dedicated to child labor.

3.1. Definition of child labor

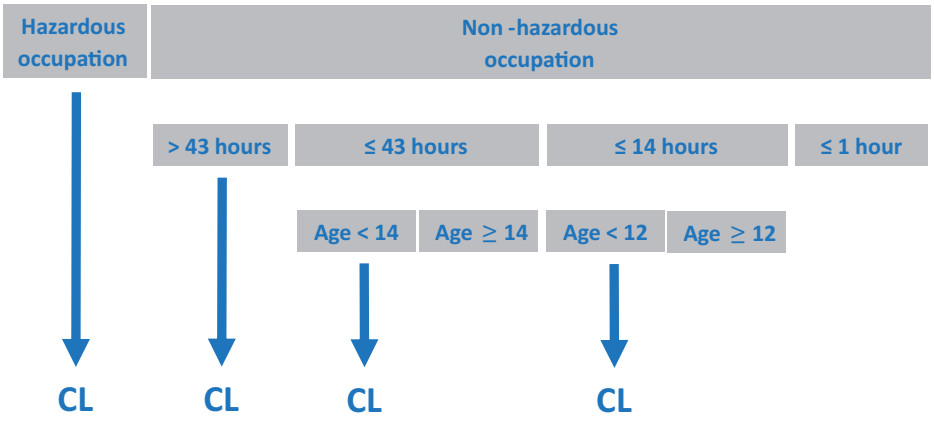
In our empirical analysis we measure child labor in various ways. Our main specification follows the ILO definition of child labor, but results are robust to alternative definitions. The definition of child labor is sketched in the following figure. It is mainly based on the ILO Conventions C138 from 1973 and C182 from 1999. According to the convention, child labor occurs if different conditions are met. First, all children working in hazardous occupations are automatically classified as child laborers. In our case these are mainly jobs in the dangerous production of glass bangles. But also welding and mechanics work belong to the hazardous occupations. If the occupation is in a non-hazardous occupation, child labor depends on age and hours worked. Young children below 12 years who work more than 1 h per week, children between 12 and 13 who work more than 14 h per week and juveniles between 14 and 17 who work more than 43 h are defined as child laborers. Our questionnaire also captures hours worked at home, hence we also include non-labor-force work which is especially important for girls. Note that in our sample only children who are 5 years or older are considered as potential child laborers.

[7] The experimental introduction of the innovation was financially and methodologically supported by the ILO Social Finance Program "Microfinance for Decent Work (MF4DW)" project.

[8] The average monthly per capita income in our sample is around 3000 rupees (30–35 US$).

[9] Consequently, questions about child labor and schooling formed the core of the household questionnaire. The other sections of the questionnaire were very short in order to avoid annoying clients with long and repetitive surveys and to reduce administrative effort.

Child labor definition related to ILO Conventions C138 (1973) and C182 (1999). *Note*: CL = classified as child labor, hours are per week.



The above definition is arbitrary to some extent. Especially the age categories are important for classification as child labor, and a slight transition from age 11 to 12 or from 13 to 14 might change child labor status from one to zero even if working hours are increased. We thus also examined various alternative definitions of child labor and results remain robust to those specifications.

4. Econometric approach

The econometric methodology used is based on the cluster experimental design of the innovation. The insurance innovation was randomly assigned at the branch level, and we thus pursue regressions at the level where the randomization took place. This basically reduces the sample to 13 observations, observed at different points in time. This very small sample size does not permit us to extensively control for household and branch characteristics as there would be too few degrees of freedom. We therefore first assess balance in baseline covariates in the next section, and thereafter conduct regressions without further covariates.

Our dataset contains data collected at baseline as well as four follow-up waves. In the main paper, we average the four follow-up waves into a single "post-treatment" observation to ease the presentation of the results.[10] With the pooled follow-up data, our econometric analysis is thus based on $N = 13$ branch-level observations observed once before and once after treatment. In the next sections we thus show simple OLS regressions of ΔY on treatment status, i.e. the change in Y over time regressed on the binary treatment dummy and a constant.

We provide OLS standard errors and classical inference, as well as randomization inference. Randomization inference is based on the sharp null hypothesis of zero treatment effect for everyone, and provides exact finite sample inference. We conduct randomization inference by forming all possible permutations of the randomization vector for the 13 branches and calculating the regression estimates. This provides the finite sample distribution of the estimated treatment effect under the null of zero effect, upon which we base our test of zero effect. This approach has recently been applied e.g. in Bloom et al. (2013).

We examine six different outcome variables: child labor incidence, hours worked by children, work in hazardous occupation, monthly earnings generated through child labor, school attendance, and monthly days missed at school.[11] We observe these outcomes at baseline and follow-up and compare changes in these outcomes for treatment and control branches. Hence for these outcome variables, the data available allows for the comparison of treatment and control branches before and after the innovation took place. We compute branch averages as the average child outcome over all children of households belonging to one of the 13 branches in our study at a particular point in time, i.e. we exploit the entire unbalanced panel data. (In our simplified econometric analysis in Section 6 we further average these branch means across the four follow-up waves.)[12] For the four child labor outcomes, only children age 5–17 are included. For the school outcomes, only children age 5–14 are included. Note that we define the outcome school attendance as the fraction of children who attended school. The outcome monthly days missed at school, on the other hand, is defined conditional on school attendance. Hence, it is the average number of days missed at school in a branch where the average is only taken over those individuals who have attended school at least once.

5. Design aspects – insurance take-up, balance and attrition

5.1. Insurance take-up

In the following, we describe coverage rates and uptake decisions of the innovation. Remember that the sample consists of all clients whose credit appraisals are conducted in September/October 2009. They take up their loans after the baseline is conducted. Fig. 2(a) shows self-assessed insurance coverage of clients across waves, where each client represents a household. In the first follow-up wave, i.e. at month 6, we observe that 100% of all clients are insured. The reason for this is that insurance is mandatory for all clients (as well as spouses and minor children, as discussed before). Even though 100% of clients are insured in the treatment *and* also in the control branches, there are two main differences between treatment and control branches: In the treatment branches the insurance package includes claim assistance,

[10] It is also possible to assess treatment effects over time. In the appendix, we accordingly analyze all post-treatment waves separately. The results in the appendix support our main conclusions; yet the estimates are not sufficiently precise to permit drawing conclusions on the timing of the effects.

[11] Note that the child labor outcomes are linked to each other: The child labor definition depends on hours worked and hazardous occupation and thus any effect on those two variables should also propagate through child labor incidence and earnings.

[12] Because of attrition, we are thus examining differences in averages and not averages of differences.

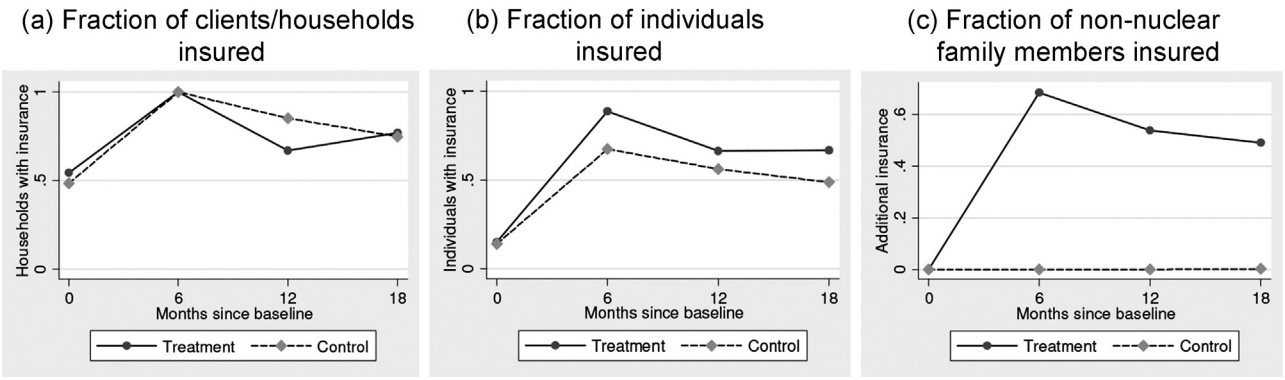


Fig. 2. Insurance coverage in treatment and control branches. *Note*: Insurance coverage is self-assessed (cross-checked with MFI's information system in follow-up surveys). In graph (a), the percentage of clients (= households) insured is given, at different points in time. In graph (b), the percentage of all individuals in the sample (i.e. all members of all households) insured is given. In graph (c), only the individuals who are extended family members (adult children, aunts, parents) are included. The graph (c) shows the percentage of these extended family members who (voluntarily) bought insurance.

whereas insurance in the control branches does not. Second, in the treatment branches, clients can voluntarily also include additional household members in the insurance, whereas insurance in the control branches covers only the nuclear family (client, spouse and minor children). Hence, although 100% of clients (and thus households) are covered in both treatment and control branches, the number of household members covered within a household may differ, as we explore below.

We also observe that about 50% of clients are insured at baseline, and this number is about the same for the treated and control branches. Note that at baseline only the standard mandatory insurance was available (i.e. client, spouse, minor children), but not the two innovation components, i.e. the help with claims and the extended coverage of other household members, that were introduced only later. Note further that at baseline clients are insured if and only if they had NRSP loans already before.[13]

As mentioned, soon after the baseline all clients have a loan and 100% of clients are covered by insurance. Afterwards, coverage rates decrease due to clients repaying their loans. (After loan repayment insurance cannot be extended unless a new loan is taken.) The coverage rates are very similar in treatment and control branches, except for 12 months after the baseline. (The difference in month 12 is, in fact, only driven by a single control branch. Without that control branch, there would be virtually no differences.)[14]

While we see little difference between treated and control group in Fig. 2(a) with respect to the insurance status of the client (usually the household head or the spouse), larger differences are visible in Fig. 2(b) with respect to the *number* of individuals in a household insured: Only the households in the treatment branches had the option to voluntarily insure those additional household members who were not mandatorily insured. Fig. 2(b) thus shows insurance coverage rates at the individual level. Take-up is substantially higher in treatment than in control branches from 6 months until 18 months after the baseline. This is the result of considerable voluntary take up in the treatment branches, which is examined in Fig. 2(c). There we show the number of household members who are voluntarily insured, i.e. who are extended family members (adult children, aunts, parents) and voluntarily bought insurance. Around 70% of those without mandatory insurance are covered in the treatment areas. The figure gradually decreases to about 50% at 18 months. In the control branches, these figures are zero since voluntary insurance of additional household members was not offered there.[15]

The (self-assessed) take-up rates for all household members illustrated in Fig. 2(b) and (c) are also shown in Table 2. Besides comparing coverage in treatment versus control branches (column 1), we also separate individuals into two categories: those with mandatory insurance (client, spouse or child < 18) and those eligible for voluntary insurance (children ≥18 years of age and non-nuclear family members, e.g. aunts, cousins, parents). Since information on family structure was collected in all branches, we define these potentially eligible groups in the same way in treatment and control branches. (i.e. In Table 2 we define the group "voluntarily insurable" in the control branches as those individuals who would have had access to voluntary insurance if they had lived in a treatment area.)

At baseline not all clients were yet aware of the coverage, so there the variable was measured with error. Nevertheless, the baseline values are very similar between treatment and control branches. After the baseline nearly all individuals with mandatory insurance also report coverage, see follow-up at month 6. This number thereafter declines as in Fig. 2 as households repay their loans and thereafter are no longer eligible for insurance unless they take up a new loan.

Consistent with the controlled design, additional voluntary insurance is taken up *only* in treatment branches. While virtually none in the control areas are voluntarily insured (as they had not been offered this option), nearly 70% in the treatment areas are voluntarily insured after 6 months (see last column of Table 2). This number declines to about 50% until month 18, partly also because of early repayment of loans which makes them no longer eligible.

Note that the innovation affects households differently depending on their family structure. Households without voluntarily insurable members could not extend their coverage because all household members are already mandatorily insured. They were thus affected only by the technical assistance. On the other hand, households with additional voluntarily insurable members could additionally also benefit from the offer of optional additional coverage. Our impact estimates thus provide some average of the two effects.

[13] If they had an NRSP loan in the previous year, they had been obliged to buy insurance cover for a year and are thus insured at baseline. On the other hand, insurance could not be obtained as a stand-alone product but only in combination with a loan.

[14] There is one of the four control branches with 100% clients having a loan and consequently insurance. Without this branch, rates would be very similar at 12 months as well.

[15] Note that due to data problems the insurance coverage information is not available for month 24. In the last survey wave at month 24, insurance coverage was no longer cross-checked with the register data and reliable information on individual insurance coverage is thus missing.

Table 2
Insurance take-up (percent of household members), by control vs. treatment branches.

Fraction of individuals insured	Take-up among all observations		Take-up in subsample of type 'mandatorily insured'[a]		Take-up in subsample of 'voluntarily insurable'[b]	
	Control	Treated	Control	Treated	Control	Treated
At baseline[c]	14.1%	15.3%	20.6%	22.3%	0.0%	0.0%
N	4742	8182	3250	5594	1492	2588
6 months	67.5%	88.6%	99.2%	99.4%	0.0%	68.5%
N	4781	8051	3252	5238	1529	2813
12 months	56.2%	66.3%	84.5%	73.1%	0.0%	53.9%
N	4666	7926	3105	5125	1561	2801
18 months	48.8%	66.7%	74.3%	77.4%	0.2%	49.0%
N	4592	7809	3014	4877	1578	2932

[a] These are all clients, their spouses and all children below 18 years of age (nuclear family).
[b] These are all children ≥18 and non-nuclear family members (aunts, cousins, parents).
[c] At baseline, individuals were not always fully aware of their coverage.

5.2. Balance of baseline covariates

In this section we discuss the balancing of the baseline covariates across branches. The study covered 13 branch offices of the city of Hyderabad: 9 treatment and 4 control branch offices. Despite being randomly assigned, this corresponds to a low-powered RCT with a sample size of only 13 at the level where the randomization took place. It would have been desirable for statistical power to have had a much larger sample of branches. A larger sample size was not possible for this intervention though because the branch offices needed to be sufficiently distant from each other to minimize possible spill-over effects. In further projects it would be advised to pilot such interventions in several cities to have a larger sample size at the cluster level.

Although randomization implies that baseline covariates should be uncorrelated with the treatment status, this is only guaranteed if sample size is sufficiently large. In Table 3 we examine differences in baseline covariates and find that most important baseline covariates are indeed actually very similar, despite the small sample size.

In Table 3 we show descriptive statistics for the 9 treatment and 4 control branches, that is branch averages of baseline covariates.[16] The households have on average three minor and three to four adult members (mean household size 6.5). Their mean monthly per capita income is around 3200 rupees (approx. 35 US$). Compared to the Pakistani average, client households seem to be relatively poor: According to World Bank (2012), the poverty rate for Pakistan was at 22.3% in 2010/2011. Even when including income from child labor, 51% of households (or 59% of individuals) in the sample report a per capita income below this poverty line (3100 rupees monthly or 1.25 US$ per day). Compared to other data from urban Sindh, the average NRSP client household in our sample is not extremely poor, but seems to be well below the median income. (See the appendix for further details.) There might be some measurement error in income, but the data nevertheless indicates that NRSP is successful in targeting low-income households. We also observe that three quarters of clients already had experience with NRSP. They have loans in the order of 15,000 rupees at baseline and only very few have difficulties repaying their loan. Regarding child outcomes, we find child labor incidence of around 20%, monthly earnings through child labor of about 300 rupees, about 12 h worked per week, hazardous work for 9% of children and school attendance of about 70%. Note that the income generated through child labor corresponds to roughly ten percent of monthly per capita income which is a non-negligible amount. From Table 3 we further observe that only three variables are significantly different at the 5% level and another variable significant at the 10% level. Overall, we conclude that most of the baseline covariates are balanced. Particularly, the child labor and schooling variables are very similar in magnitude. More details are given in the appendix.[17]

5.3. Attrition in follow-up waves

After the baseline survey in September/October 2009, four follow-up surveys were conducted every 6 months: March/April 2010, October/November 2010, May/June 2011 and October/November 2011. The attrition rate shown in Table 4 is between 0.9 and 4.0% for each wave, and similar in treatment versus control branches. In the follow-up surveys after 12, 18 and 24 months there are a few households 'dropping back in'. There is no evidence for differential non-response: a two-sample proportion test of the hypothesis that the fraction of households answering all survey waves are the same (90.2% in control versus 89.5% in treatment branches) is not rejected. Within-household compositions likewise are unaffected by the innovation: We checked for treatment effects on household size, number of adults and number of children in different age categories. None of the regressions resulted in any significant differences. It is also possible to calculate attrition on the individual level, and those figures are comparable as well across treatment and control groups. However, we consider this information to be less reliable. The main reason is that individual identifiers were less central for the survey logistics, and it is thus more likely that errors in those identifiers occurred. We analyze attrition figures on different levels in the appendix.

6. Empirical findings

Before presenting the econometric results, we first examine in Fig. 3 the health expenses claiming behavior between treatment and control branches. One important potential effect channel for the treatment is a better protection of households in case of a shock. Insured individuals can get their hospital costs reimbursed after submitting a claim that should be settled within 15 days, as explained in Section 2. In line with higher insurance coverage we would therefore expect more claims and ultimately more reimbursement for households in treatment branches. Fig. 3(a) shows

[16] The tests for equality are based on the $N = 13$ branch level observations.

[17] We also examined household level regressions where we controlled for the covariates given in Table 3. There we found that the treatment effect estimates that we report later are robust to controlling for the imbalances in Table 3.

Table 3
Descriptive statistics of baseline covariates aggregated to branch level; differences between treatment and control branches.

	Mean in control branches	Mean in treatment branches	Difference	*t*-Value	*p*-Value
Poverty score at baseline (PPI)[a]	30.7	32.2	1.5	1.81	**0.098**
Spouse in household? (yes/no)	0.79	0.76	−0.03	−2.24	**0.046**
Number of children age 0–4	0.58	0.59	0.01	0.16	0.878
Number of children age 5–13	1.48	1.43	−0.04	−0.50	0.624
Number of children age 14–17	0.78	0.88	0.10	1.33	0.212
Number of adults	3.65	3.49	−0.16	−1.05	0.315
Mean (female) client age	40.7	40.9	0.18	0.16	0.872
Mean (male) spouse age	44.9	45.1	0.13	0.19	0.855
Mean (female) client education (years)	2.4	3.3	0.9	2.28	**0.044**
Mean (male) spouse education (years)	4.0	4.9	0.8	1.50	0.162
Monthly income per capita[b]	3171	3153	−18.5	−0.08	0.935
Monthly expenses: total	13,757	12,529	−1228	−1.15	0.275
Monthly expenses: children	307.3	297.6	−9.6	−0.14	0.894
Monthly expenses: books	294.0	196.3	−97.6	−1.25	0.236
Monthly expenses: outpatient	452.8	357.7	−95.0	−0.69	0.507
Monthly expenses: hospital	107.0	54.6	−52.3	−1.55	0.149
Credit with NRSP before? (yes/no)	0.722	0.773	0.051	1.23	0.245
Credit amount	15,969	15,704	−264.9	−0.28	0.784
Difficulties repaying loan? (yes/no)	0.011	0.022	0.010	0.69	0.504
Age of child	11.2	11.6	0.3	2.39	**0.036**
Education (years)	3.23	3.44	0.21	0.64	0.536
Child labor? (yes/no)	0.19	0.18	−0.01	−0.26	0.803
Hours of work (weekly)	11.2	12.9	1.7	1.12	0.286
Hazardous occupation? (yes/no)	0.087	0.085	−0.002	−0.09	0.928
Monthly child labor earnings[c]	301.1	333.1	31.9	0.72	0.486
School attendance	0.680	0.707	0.026	0.60	0.563
Monthly school days missed	1.063	1.079	0.015	0.03	0.976

Notes: 9 Treatment branches, 4 control branches, total sample size 13.
Differences significant at the 10% level (i.e. *p*-value smaller than 0.1) are marked in bold.
[a] PPI refers to the progress out of poverty index.
[b] Adjusted for minor household members (factor 0.6) and excluding income from child labor, income in Pakistani rupees (1000 Rs = approx. 11 US$).
[c] Earnings per child (age 5–17), only earnings that are generated by work classified as child labor.

Table 4
Attrition across waves, control versus treatment branches.

	All	Control branches			Treatment branches		
	Households	House-holds	Drop-outs	Drop-ins	Households	Drop-outs	Drop-ins
Baseline	2097	777	–	–	1320	–	–
Follow up after							
6 months	2068	770	7 (0.9%)	–	1298	22 (1.7%)	–
12 months	2023	743	27 (3.5%)	0	1280	21 (1.6%)	3 (13.6%)
18 months	1972	733	21 (2.8%)	11 (32.4%)	1239	48 (3.8%)	7 (17.5%)
24 months	1943	728	22 (3.0%)	17 (38.6%)	1215	50 (4.0%)	26 (32.1%)

Note: The number of households in each wave, by treatment status, are given. The column "drop-outs" shows the number of households who have been available in the previous round but not in the current round. The column "drop-ins" shows the number of households who have *not* been available in the previous round but are available in the current round.
For the "drop-outs" the percentage in brackets indicates the number of drop-outs as a fraction of the previous wave's observations. For the "drop-ins" the percentage in brackets indicates the number of drop-ins as a fraction of the previous wave's missings.

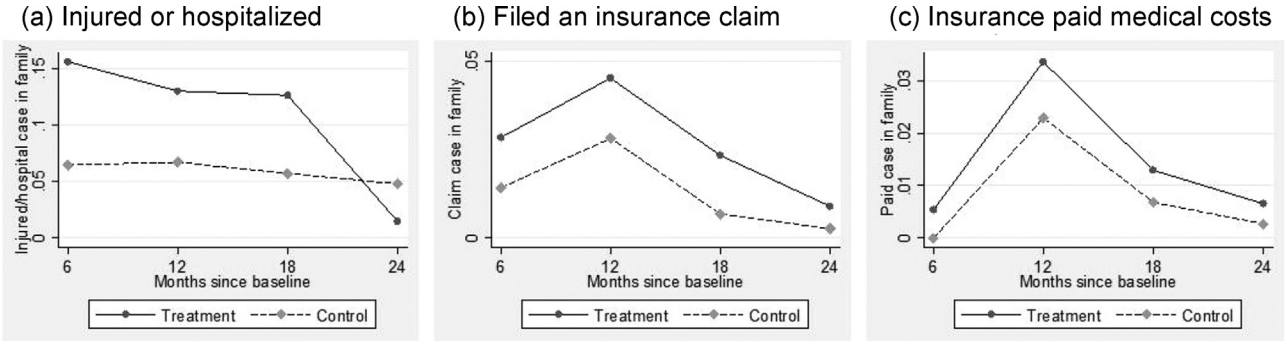


Fig. 3. Medical incidence, insurance claim and payment (share of households), control vs. treatment. *Note*: There is no information available on insurance related events at baseline (i.e. month 0). Panel (a) shows the percentage of households reporting an injury or hospitalization case since the last loan disbursement. Panel (b) shows the percentage of households who report that an insurance claim was filed. Panel (c) shows the percentage of households who had an insurance claim that was also approved, i.e. where medical expenses were indeed reimbursed.

Table 5
Regression analysis on branch level, $N = 13$.

Outcome variable	Child labor	Hours worked	Hazardous occupation	Child labor earnings	School attendance	Days missed at school
Treatment effect	**−0.034**$^{+}$	−1.80	**−0.046**$^{***,+++}$	**−142**$^{**,+++}$	0.012	−0.18
	(0.021)	(1.28)	(0.013)	(46.5)	(0.017)	(0.31)
N	13	13	13	13	13	12
R^2	0.187	0.152	0.544	0.458	0.049	0.033

OLS Regression of ΔY on treatment dummy and a constant. $N = 13$ observations. The dependent variable is the average outcome in follow-up periods minus average outcome at baseline.
The outcome monthly days missed at school is defined conditional on school attendance.
Estimates significant at the 10% level are marked in bold.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$ using OLS standard errors and two-sided t-test.
$^{+++}p < 0.01$, $^{++}p < 0.05$, $^{+}p < 0.1$, randomization inference, two-sided test.

Table 6
Regression analysis on branch level, $N = 13$, separately for boys and girls.

Outcome variable	Child labor	Hours worked	Hazardous occupation	Child labor earnings	School attendance	Days missed at school
Boys						
Treatment effect	**−0.083**$^{**,++}$	**−4.40**$^{**,++}$	**−0.044**$^{***,++}$	**−222**$^{**,++}$	**0.024**$^{*,+}$	−0.14
	(0.032)	(1.84)	(0.013)	(80.9)	(0.013)	(0.39)
N	13	13	13	13	13	12
R^2	0.376	0.343	0.514	0.407	0.228	0.012
Girls						
Treatment effect	0.010	0.62	**−0.048**$^{*,+}$	**−74.3**$^{***,++}$	0.00062	−0.22
	(0.028)	(1.49)	(0.022)	(22.2)	(0.033)	(0.36)
N	13	13	13	13	13	12
R^2	0.012	0.015	0.302	0.504	0.000	0.036

See note under Table 5. Separate regressions for boys and girls subsamples.
Estimates significant at the 10% level are marked in bold.

the percentage of households reporting an injury or hospitalization case since the last loan disbursement. Overall, a much higher percentage declares hospitalization in the treatment branches. While around 6% of control households report a medical case it is two to three times as often the case for treated households, except in the final survey wave. Also the claim frequency (Fig. 3(b)) is consistently about twice as high in treatment areas. Similarly, insurance payments are more frequent (Fig. 3(c)).[18] Unfortunately, we do not have baseline data for injury and hospitalization, but Fig. 3(a)–(c) are consistent with more individuals attending hospital in case of sickness if they are insured and thus do not bear the full costs of medical treatment. The higher frequencies are likely also influenced by the sensitivity and help offered by the credit officers in the treatment branches.

Now we turn to the regression analysis on child labor outcomes. Table 5 shows the regression results, as described in Section 4, on the six outcome variables. We find a highly significant (irrespective of the inference method) and sizable negative effect on *hazardous occupation* and *child labor earnings*. We also observe negative effects for *child labor* and *hours worked*, but they are too imprecisely estimated to permit drawing firm conclusions. (The effect on child labor is significant at the 10% level by randomization inference.) The estimates for schooling are imprecise and we cannot reject the null of no effect on schooling.

Note that the treatment effect estimates have to be interpreted as the combined effect of technical assistance with claims (i.e. the monthly visits of credit officers assisting with claim procedures) and the *offer* of additional insurance coverage. Hence, we compare households who have been *offered* additional coverage with those who did not have this option. (I.e. we do not directly compare households who have or have not purchased extended insurance, but according to the binary treatment status of their location of residence.)

In the appendix we estimate the same specification separately for each of the four follow-up periods. The signs of the estimates are the same as in Table 5: Negative effects for all child labor variables. As in Table 5, standard errors are rather large because of the small sample size, and thus only the results for hazardous occupation and child labor earnings are statistically significant. It also appears as if the effects tend to decrease in magnitude over time, with the largest effects observed at the 12 month follow-up period. While this could be due to the small sample size and the corresponding large standard errors (i.e. differences in effect sizes over time are not statistically significant), the observed pattern could also be due to the steady decline in the number of insured individuals over time, see Table 2. Households who repaid their loans cease to be covered by insurance once the insurance period runs out. This process, however, is likely to be endogenous and possibly affected by the treatment itself. Given the small sample size, we abstain from an extensive econometric analysis and abstain from drawing firm conclusions on the timing of the treatment effects, and merely note that the more flexible analysis in the appendix supports the main conclusions from Table 5.

In Table 6 we estimate our main regression separately for boys and girls. Interestingly, effects tend to be quite substantially larger for boys than for girls. For boys (but not for girls) we also find a modest and marginally significant positive effect on school attendance, whereas the decrease in number of missed school days is insignificant. The larger effects for boys are not surprising as they are most affected by child labor in our sample; at baseline they work on average about 20% more often in hazardous occupations, spend 40% more hours, are classified 60% more often as child laborers and earn eight times the amount through child labor as compared to girls.

[18] Simple two-sample proportion tests show significant differences 6 months after baseline in all three variables. However, some events are extremely rare and we should be careful in interpreting the differences. While 274 injuries or hospitalizations are reported, only 48 submitted claims and 7 claim payments can be found at $t = 6$ months.

7. Conclusion

Economic shocks play a large role for poor households. One of the undesired consequences might be that hardship forces parents to send children to work or take them out of school. This coping strategy is especially dangerous because it may harm long-term human capital accumulation or health for the next generation. Microinsurance is widely promoted as a tool to reduce vulnerability to shocks and hence potentially protects children from child labor, but so far there are almost no studies assessing the effect of formal insurance on child labor and schooling outcomes. It is straightforward to imagine that insurance protects children from being pushed into child labor once medical costs arise. Yet, a change in economic uncertainty might also have effects *ex ante*, before a shock actually takes place.

To estimate the actual effect of insurance we exploit a randomized controlled trial in the urban center of Hyderabad, Pakistan. An innovation package consisting of (a) the extension of voluntary health insurance coverage and (b) regular visits sensitizing microcredit clients regarding claim procedures was introduced in nine treatment branches. We make use of a baseline and four follow-up survey waves to estimate treatment effects. We find that the innovation package indeed helps to reduce child labor related outcomes. The combination of offering increased coverage and helping with claims decreased hazardous work and earnings through child labor. The effect is larger for boys which might be explained by the fact that child labor activities are more common amongst male children in our sample.

While we find statistically significant results we would like to point out that we in general recommend increasing the number of clusters used for randomization above the 13 branches available in our design. A second potential caveat is that staff members of the microfinance institution were involved in data collection. In our study we are rather confident that this did not affect our main results, because staff members were trained to gather objective measures of child labor and because baseline data shows no imbalances between treatment and control branches. Nevertheless, independent interviewers who are uninformed about treatment status are preferable for collecting survey data.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jhealeco.2014.10.003.

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