WPS7035

Policy Research Working Paper 7035

Dynamics of Demand for Rainfall Index Insurance

Evidence from a Commercial Product in India

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September 2014

Policy Research Working Paper 7035

Abstract

This paper analyzes the dynamic nature of rainfall insurance purchasing decisions, specifically looking at whether and why receiving an insurance payout induces a greater chance of purchasing insurance again the next year. This analysis uses customer data from the Indian micro-finance institution BASIX, and finds that receiving an insurance payout is associated with a 9 to 22 percentage points increased probability of purchasing insurance the following year. This affect appears to be driven by behavioral effects of receiving a payout, and cannot be explained by trust, learning, or direct effects of weather. Overall, low repurchasing rates even after payouts suggest that current rainfall index insurance products are likely to continue struggling to achieve significant sales at market prices.

This paper is a product of the Impact Evaluation Team, Development Research Group. It is part of a larger effort by the World Bank to provide open access to its research and make a contribution to development policy discussions around the world. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at dstein@worldbank.org.

Dynamics of Demand for Rainfall Index Insurance: Evidence from a Commercial Product in India

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JEL: D12, D14, D81

Keywords: Micro-Insurance, Rainfall Index Insurance, Demand for Insurance

[1] dstein@worldbank.org. This work would not have been possible without the assistance of the insurance staff at BASIX, especially Sridhar Reddy, for help in obtaining and understanding their customer database. Thanks to Tim Besley, Greg Fischer, Gani Aldashev, and Xavier Gine for giving helpful advice and commenting on earlier drafts. Thanks to participants at the Munich Re 6th Annual Microinsurance Conference, the Midwestern International Economic Development Conference, seminars at the London School of Economics, University of Namur, and KU Leuven for helpful comments. All remaining errors are my own.

1 Introduction

Roughly 60% of India's population is employed in agriculture, and over 50% of agricultural land is dependent on rainfall to nurture the crops.[1] But the Indian monsoon is notoriously unpredictable, prone to droughts and floods that can have devastating effects on the livelihood of rural Indians. While Townsend (1994) argues that Indian villages do an effective job of providing informal consumption insurance against idiosyncratic shocks, a poor monsoon will hit whole villages and districts at once, likely rendering intra-village transfers ineffective. Beginning in the early 2000s, rainfall index insurance was introduced in India as a potentially important tool to help poor farmers deal with rainfall risk (Hess, 2004), but has struggled to reach a critical mass of customers, especially when unsubsidized.[2]

Unlike physical goods (or even credit), it is difficult for customers to evaluate the benefits of insurance since its main benfits only occur when a payment is received. If customers are unfamiliar with how insurance works, they may be influenced by their recent experiences with insurance and also by the experiences of their friends and neighbors. Evidence in the developed world (Kunreuther et al., 1985; Gallagher, Forthcoming) shows that purchases of flood and earthquake insurance in the US are greatly influenced by recent experiences with disasters and insurance payouts, that peoples' insurance decisions are influenced by their friends and neighbors' experiences with insurance. Reacting to low rates of rainfall insurance uptake in Andhra Pradesh, India, Giné et al. (2008) suggest that "over time, lessons learned by insurance 'early adopters' will filter through to other households, generating higher penetration rates among poor households."

This paper seeks to understand how previous insurance payouts can affect future insurance purchasing decisions, and what mechanisms can explain this behavior. Using data on three years of insurance purchasers from the Indian micro-finance institution BASIX, I find that customers who received an insurance payout are 9-22% more likely to repurchase in the following year as compared to customers who did not receive any insurance payments. I argue that this effect is caused by the behavioral effects of receiving an insurance payout, and cannot be accounted for by other explanations such as wealth effects, changing expectations about weather, or trust in the insurance company.

I test two main alternative hypotheses as to why receiving payouts could increase insurance demand the following year. First, it is possible that weather shocks themselves could have an effect on insurance demand, such as Kunreuther et al. (1985) observe in the US. This could happen because weather shocks change customers' beliefs about future shocks, change their wealth, or simply increase the salience of shocks. I look for direct effects of weather by testing how rainfall in the year before insurance was introduced affected insurance purchases, and find evidence that previous rainfall shocks tend to *decrease* insurance purchasing. This provides evidence against the argument that it is weather shocks

[1] CIA World Factbook: India; Indiastat.com

[2] There have been some areas with a large number of insured, but this is generally due to heavy government subsidies or requiring insurance to obtain government loans. Exposing official uncertainty about the prospects of rainfall index insurance, the government of India decided to delay expansion of its rainfall index insurance pilot (WBCIS) to all of India, and in 2010 piloted a modified version of its area-yield index product (NAIS).

as opposed to insurance payouts that are driving insurance purchases.

Next I test whether receiving insurance payouts could induce trust in the insurance company or learning about the insurance product. Bryan (2010) suggests that index insurance take-up is low due to ambiguity aversion, which should decrease as customers learn more about the product. I assume that if trust and learning are driving purchases, we should be able to witness spillover effects on other people in the village. This is because people in a village who witnessed payouts but did not receive them should also have been able to learn about insurance and gain trust in the insurance company. I do not find convincing evidence that these spillover effects are present, and argue that this is evidence that insurance repurchasing is not being driven by trust or learning.

I instead argue that insurance re-purchases are being driven by the psychological effects of receving an insurance payout. Re-purchase behavior is consistent with a number of behavioral explanations such as recency bias, shifting reference points, and viewing insurance as a system of balanced reciprocity.

This paper is related to a few separate lines of research. First, it contributes to a growing list of empirical studies that attempt to determine demand for weather index insurance (Mobarak and Rosenzweig, 2013; Cole et al., 2013; Giné et al., 2008). One overarching conclusion from previous studies on index insurance is that demand for index products is low when provided at market rates[3], and only increases when prices are slashed significantly. However, most of these studies look at insurance as a static purchasing decision, seeing what factors lead people to become first time customers.

There are a few of studies that do look at dynamic insurance decisions. Hill and Robles (2010) provide rainfall insurance for free as part of an experimental game in Ethiopia, and then return the next year to sell the same insurance. Despite the fact that two-thirds of the people who were granted insurance during the experiment received payouts, this group had a low take-up rate of 11% the next season, which was a lower rate than those who had not participated in the experiment. Karlan et al. (2012) study a multi-year rainfall insurance program in Ghana, and find that people who received insurance payouts in the first year of the program are more likely to purchase in the second year. Also, they find that these effects spill over into customers' social networks. Cai and Song (2012) show that experiences in hypothetical insurance games affect future real-world insurance purchase decisions.

This study differs from these previous works in a number of ways. Most importantly, the two previously-cited papers were small-scale experiments with closely-controlled marketing and pricing strategies. This paper instead studies a large-scale commercial insurance operation, providing a much larger dataset and real-world conditions. Although the previous studies have very strong internal validity due to their closely controlled experimental setting, this study complements them by studying a setting that is more likely to mirror commecial-level insurance operations, therefore providing a higher level of external validity.

The paper will proceed as follows. Section 2 explains the insurance policies and data that will be studied in the empirical section. Section 3 provides the main empirical evidence, and shows that recip-

[3]Market rates tend to be around 2-6 times actuarially fair rates.

ients of insurance payouts are more likely to purchase insurance the following year. Section 4 searches for evidence of a number mechanisms by which this could take place. Section 5 discusses a number of behavioral explanations for the results. Section 6 concludes and offers policy recommendations.

2 Index Insurance and Customer Data

2.1 Context: BASIX Policies

In this analysis I study monsoon rainfall index insurance policies underwritten by the insurance company ICICI-LOMBARD and sold by BASIX, a microfinance institution based in Hyderabad. The policies insure against excess or deficit rainfall, and are calculated based on rainfall measured at a stated weather station. By basing payoffs on just rainfall, the policies should have low monitoring and verification costs, and also should be free of adverse selection and moral hazard. These attributes make policies inexpensive to create and administer, which allows them to be sold in small quantities and priced at levels affordable for poor farmers. BASIX's policies are designed to pay out in situations where adverse rainfall would cause a farmer to experience crop loss, and are therefore calibrated to the water needs of local crops.

BASIX policies are divided into three phases, which are meant to roughly capture the three phases of the growing season: planting, budding/flowering, and harvesting. If cumulative rainfall is too low or high in any of these phases, the crop's output is potentially damaged and the farmer could suffer a loss. The policies are designed to start when farmers first start planting, which depends itself on rainfall. Therefore, the policies have a dynamic start date which means that Phase 1 begins on the day that cumulative rainfall since June 1 reaches 50mm or on July 1, whichever comes first. Each phase generally lasts 35-40 days. During this time, rainfall data is collected daily at a designated weather station, and payouts are calculated using the cumulative rainfall over the phase.

A phase of coverage is defined by three parameters: "Strike", "Exit", and "Notional". Deficit policies begin to pay out when the rainfall drops below the level of the Strike, and gives its full payout when it falls below the Exit. In between, it pays the Notional amount of rupees for each millimeter below the Strike.

In 2006 and 2007, all rainfall insurance contracts sold by BASIX included three phases, with the first two protecting against deficit rainfall, and the third protecting against excess rainfall. In 2005 the policies all had three phases, but each phase protected only against deficit rainfall. Table 1 presents a sample contract, from Nizamabad district in the state of Andhra Pradesh.

Given the policy parameters we can see how the payouts will evolve according to rainfall. Figure 1 shows the payout schedule for phase II of the above policy. There is no payout when rainfall is above the strike, which is 125mm. Then as rainfall decreases the payout increases linearly until rainfall reaches the exit of 40mm, then jumps to the policy limit of Rs 1000 once rainfall falls below 40mm.

BASIX insurance policies are sold in April and May, which are the months that precede the monsoon in India. Insurance policies cover only one season, so customers must purchase insurance again if they want coverage for the following year.

Table 2 presents summary statistics for the insurance policies studied.

2.2 Data

The data set consists of the entire set of BASIX's purchasers of rainfall index insurance from 2005-2007, which covers six states.[4] Though it ran small pilots in 2003 and 2004, BASIX began to mass-market rainfall insurance starting in 2005. The data contains limited personal information about each customer including their location, how many policies they purchased, and what payouts they received during that season. The BASIX data covers 42 weather stations, and includes a total of 19,882 customers from 2005-2007.[5] After numerous rainfall shocks in 2006, BASIX realized that many customers who had purchased only a small amount of insurance were disappointed that they received small payouts. In response, BASIX instituted a rule in 2007 that required all customers to purchase insurance coverage with a maximum payout of at least Rs 3000. This was meant to encourage people to buy a level of coverage that would actually provide meaningful payouts in the event of a shock, but resulted in a sharp decrease in the number of customers in 2007. A summary of characteristics of BASIX customers is given in Table 3.

For rainfall data, I use a historical daily grid of rainfall, which is interpolated based on readings from thousands of rainfall stations throughout India. The data is provided by the Asian Precipitation Highly Resolved Observational Data Integration Towards Evaluation of water resources.[6] This data set has daily readings of rainfall from 1961-2004, at a precision of .25°[7]. For each .25° x .25° block, the data contains the amount of rainfall in millimeters and the number of stations within the grid that contributed to the data. This data is used to evaluate how the insurance policies would have paid out historically, which can be used as a proxy for past rainfall shocks.

The three individual years of BASIX customer data were converted into a panel by manually matching individual customers using available identifying data. Errors in matching customers from year to year create the possibility of introducing non-classical measurement error into the analysis. In

[4]The states are, in descending order of number of buyers: Andhra Pradesh, Maharashtra, Jharkand, Karnataka, Madhya Pradesh, Orissa.

[5]Note that BASIX also sold many policies in the district of Deogarh in Jarkhand, and those buyers are omitted from this analysis. The reason for this is that the policy for Deogarh is heavily subsidized, resulting in a policy that is completely different from all the others. For instance, the Deogarh policy for 2005 has an expected payout of Rs 1140 compared to an average of Rs 149, although the policy does not cost more than average. Because of its incredibly generous terms, the Deogarh policy has huge payouts for all years of the study, and therefore does not seem to be 'normal' enough to warrant inclusion in the main dataset. All the analysis below is performed excluding all buyers in Deogarh, though most results do not change substantially when it is included.

[6]APHRODITE's water resources project; http://www.chikyu.ac.jp/precip.

[7].25° Latitude equals about 27.5km, while .25° longitude varies by latitude. Over the range of latitudes in this survey it equals roughly 26km.

Section 3 I discuss the possible consequences of such errors and how they affect interpretation of the estimates.

3 Results: The Effect of Payouts on Take-up

In this section I address the central question: is receiving an insurance payout correlated with repurchasing insurance the following year? To do this I examine BASIX's customers in 2005 and 2006, and regress repurchasing on payout reception and a year dummy. The basic econometric specification is as follows:

$$y_{i,t+1} = \alpha + \beta_1 P_{i,t} + B_2 D_{2006} + \epsilon_{t,i} \tag{1}$$

Here $y_{i,t+1}$represents whether subject i purchases insurance at time $t+1$, and $P_{i,t}$ is a dummy variable that takes a value of 1 if person i receives an insurance payout at time t.[8] The sample is all buyers of insurance from 2005 and 2006, and I include a dummy (D_{2006}) that takes a value of 1 for purchasers in the year 2006 to control for time effects. Also, I only include purchasers who have weather insurance contracts available in their area in the following year.[9] These results are presented in Table 4, and Column 1 reports the baseline OLS results. It shows that receiving a payout is associated with a 9% increased chance of repurchasing insurance the following year, which means that those who receive an insurance payout are more than twice as likely to purchase insurance the following year as those who did not receive a payout. The dummy for 2006 is negative and significant, which is expected due to the minimum sum insured rules imposed in 2007.

One point of concern with these results is that there are many cases where there are multiple purchasers of insurance in a certain village in one year, and then zero in the next year. While this could be the result of people simply being unsatisfied with insurance, the large amount of villages that suddenly drop to zero purchasers is suspicious, especially since the BASIX data does not contain information about whether marketing activities took place in a given village in a given year. For all the villages that had purchasers in one year and then none in the next year, it is quite likely that no BASIX representative visited the village, and therefore the customer did not really have a chance to purchase the insurance. If this was the case it would make sense to exclude these villages from the

[8]It makes sense to assume that the error $\epsilon_{t,i}$ is correlated for the same person across time, as well as across people in a given year. Ideally, we would like to include individual fixed effects to account for individual heterogeneity. However, in order to exploit this variation we would need to look at customers who purchased insurance in both 2005 and 2006, and received payouts in only one of those years. Unfortunately, due to the very low repurchase rate, this results in very little variation and is therefore an unsuitable method of analysis.

[9]Basix's insurance coverage area varied somewhat from year to year. Results do not change significantly if all areas are included in the regression.

analysis, as the previous year's payout would have no way to possibly influence a customer's purchase decision.

In Column 2 I exclude villages that had no purchasers the following year from the analysis, creating what I call the 'Marketing Restricted Sample'. For instance, say village A had 10 purchasers in 2005, 13 purchasers in 2006, and 0 in 2007. In this case, the buyers from Village A in 2005 would be included in the sample since they had the opportunity to purchase the next year. However, the 2006 buyers would be excluded because I make the assumption that they didn't have the opportunity to buy in 2007. Restricting the sample this way results in a drop of the number of observations from 11,002 to 4,202, and causes the coefficient on receiving a payout to more than double to .22. This provides evidence that the omitted information about whether a village received marketing was downward biasing the results of the original specificiation.

The coefficients generated in this restricted sample may be incorrect, as the decision to market to certain villages and not others is most likely not exogenous. If the marketing teams decided whether or not to market to certain villages based on the previous year's rainfall or experience with insurance then the results could be biased. For instance, assume that there were a number of villages that experienced a rainfall shock but received very low payouts, making them unhappy with insurance. If the marketing team knew this they may have decided to not market to as many of these villages, therefore censoring villages that received a payout but were likely to have few repeat buyers. Regressions that use previous years' payout characteristics to try to predict whether insurance is sold in a village the following year do not reveal any patterns that would suggest selection bias, but they may miss more subtle selection patterns. It is possible that the coefficient for the marketing restricted sample is upward biased and it therefore would be reasonable to regard the coefficients in Columns 1 and 2 as lower and upper bounds respectively.

The effect on repurchasing may depend on the size of the payout as well. In Columns 3 and 4 I add two new continuous variables to the regression: the ratio of the payout received to the premium paid (which I will call the "payout ratio") and the payout ratio squared. The payout ratio has a positive and strongly significant effect, while the squared term is smaller and negative. This suggests that higher insurance payouts result in greater propensity to purchase the following year, but that the marginal effects flatten out for larger payouts. Also, the simple dummy of receiving a payout flips to negative, suggesting that small payouts have a negative effect on purchasing. In fact, payouts have a positive effect only when the payout ratio nears 1. This is consistent with an interpretation that the effects of an insurance payout are being driven by customers experiencing a net gain on their insurance transaction the previous year.

One may be concerned that the linear probability model may give biased estimates, especially since such a small percentage of the sample were repeat buyers. For a robustness check I also ran the regressions in Columns 3 and 4 using probit and logit specifications, and the results are very consistent with those obtained from OLS.[10] Also, clustering standard errors at the weather station level still yields highly significant coefficients.

As mentioned earlier, the dependent variable in this regression was generated by manually matching customers from one year to another, and therefore is likely measured with some error. While there is no reason to believe that this measurement error is correlated with any independent variables in the regression, since the dependent variable in the regression is a dummy variable this can lead to downward bias on the estimated coefficients. In order to get a feel for the potential magnitude of this error I run simulations where I assume that the BASIX data has been matched completely correctly, and then induce 'measurement error' by randomly changing the dependent variable of whether people purchased the following year or not. With the introduction of 10% matching errors (with an equal probability of a mismatch for buyer or non-buyers), the coefficient on receiving a payout in the full sample (Column 1) falls from .090 to an average of .072 over 1,000 simulations. For the marketing restricted sample in Column 2, it drops from .222 to .178. In other words, if we assume 10% matching errors, then the estimated coefficients are likely to be underestimated by around 20%. It also may be possible that most of the error came from being unable to identify positive matches, possibly due to different members of a household signing the insurance contract from year to year. Repeating the above simulation but assuming that only people who were found not to have bought the next year could have been errors, the coefficients become underestimated by around 10%. While the exact form and structure of the matching errors cannot be known, it is possible that the reported coefficients are somewhat lower (in absolute value) than the true coefficients.

Overall, the results indicate that receiving an insurance payout correlates with a roughly 9-22% higher chance of repurchasing the next year compared with someone who purchased insurance but did not receive a payout. They also suggest that higher payouts lead to a greater chance of repurchasing, and that very low payouts may actually have a negative effect. The next section will explore some possible mechanisms for this result.

4 Explanations for Main Result

The results of the previous section show a simple correlation that people who receive insurance payouts are more likely to purchase insurance the following year. There are a number of reasons consistent with a neo-classical frameworkwhy this may be the case, and in this section search for support of these explanations in the data.

I first consider the hypothesis that a rainfall shock as opposed to the insurance payout may cause people to be more likely to purchase the following year. This may be the case due to people learning about the effects or shocks, or experiencing changes in wealth or liquidity. To do this I look at villages in the first year they were offered insurance, and see if a rainfall shock the previous year correlates with

[10] Results not shown.

greater insurance take-up. On the contrary, I find that villages that had a rainfall shock the previous year were actually less likely to purchase insurance the following year, which provides strong evidence against the argument that weather as opposed to payouts are driving the main result.

I next consider the widely hypothesized suggestion that receiving insurance payouts would cause people to gain trust in the insurance company and learn about insurance, therefore making them more likely to purchase insurance in the future. To do this I assume that in order to gain trust in the insurance company or learn how insurance works, one would not have to receive a payout themselves; witnessing a neighbor receive a payout should also have the same effect. I therefore look for evidence of spillovers within a village and do not find evidence that witnessing a payout without actually receiving it yourself has a significant effect on the propensity to purchase the following year.

I also consider the possibility that payouts cause increased take-up due to direct wealth and/or liquidity effects as opposed to psychological effects. While I do not have data to empirically separate these possible mechanisms, I argue that due to the timing and circumstances of rainfall insurance payouts, wealth and liquidity are unlikely to play an important role.

Finally, I address the concern of unobserved marketing variation. While the effect of this omitted variable is admittedly difficult to measure, I argue it is unlikely to be driving the central results.

Given that I fail to find support for all of these explanations, I argue in the next section that re-purchases are being driven by behavioral responses to receiving insurance payments.

4.1 Direct Effects of Rainfall

Since most rainfall insurance payouts come at the same time as a rainfall shock, it is possible that the rainfall shocks themselves as opposed to the insurance payouts are what is driving increased take-up the following year. There is some evidence for this happening in developed markets, as Kunreuther et al. (1985) note that purchases of flood and earthquake insurance in the US spike after a recent event, even if people were not insurance customers before.

There are a number of theories that could explain this behavior. First, recent experiences with rainfall could change subjects' beliefs about the probability of a rainfall shock the following year (this is proposed as "recency bias" in Karlan et al. (2012)). If there is actual autocorrelation of rainfall events or if the subject has limited knowledge about the effects of rainfall shocks, people may update their beliefs about shocks and therefore have more desire for insurance the following year. Alternatively, recently experiencing a rainfall shock could make shocks more salient, increasing the chance they will buy insurance the following year. Also, rainfall shocks may affect the wealth of the farmers. If farmers become poorer due to bad rainfall, CRRA utility would suggest that they would be even more risk averse the next year as a second shock would cause greater disutility.

I start by examining whether there is actual autocorrelation in the rainfall data. To test for autocorrelation, I create a panel of various rainfall indicators from 1961-2004 for each weather station.

For each indicator, I run a regression of six lags of the variable on the current value, including weather station fixed effects. These results are presented in Column 1 of Table 5, with just the coefficient on the first lag shown. While a fixed effects regression with a lagged dependent variable is not generally consistent, it will converge to the true value as $T \to \infty$. As T is relatively large (38), these estimates are likely to suffer from little bias. I also run a regression of the first lag using previous lags as instruments, using the methodology proposed by Arellano and Bond (1991), with results presented in Column 2. The results from both specifications are similar, and show a negative first-order autocorrelation in rainfall that appears to be driven by rains early in the season. The bottom two rows test for autocorrelation of rainfall shocks using the parameters of the 2005 insurance policy to determine shocks. "Would Have Been Payout" is a dummy variable that takes a value of 1 if the insurance policy of 2005 would have given a payout, while "Total Insurance Payout" is the size of this payout. By these measures, shocks do not appear to exhibit significant positive first-order autocorrelation.

This evidence casts doubt on the hypothesis that positive autocorrelation of weather events is driving increased insurance purchasing. It appears that total rainfall is actually negatively autocorrelated, while shocks (which are proxied by the insurance contract giving a payout) do not appear to be correlated at all.

Even if there is no positive autocorrelation of rainfall, there may be other aspects about experiencing a shock that result in people having a higher propensity to purchase insurance. In order to look at the results of weather separately from the effects of insurance, I analyze how previous weather events affected insurance purchase decisions in the first year that insurance was offered to BASIX customers, which was 2005. To accomplish this, I first aggregate the purchasing data to the village level and then test to see whether villages that experienced a rainfall shock in 2004 had more insurance purchasers in 2005 than villages who did not experience a rainfall shock. A shock is defined using each location's insurance policies in 2005: If insurance would have paid out in 2004 based on the structure of the 2005 weather policy, this is deemed a rainfall shock. As the quality of the rainfall data is related to the amount of nearby weatherstations, I weight the observations based on the number of nearby rainfall stations.[11] Also, I create a hypothetical payout ratio, similar to the "Ratio of Payout to Premium" variable presented in Table 4. This is the ratio of the amount that the 2005 policy would have paid out in 2004 divided by the premium of the policy.

The results of this regression are presented in Table 6.[12] Column 1 presents the baseline regression,

[11]The APHRODITE weather data provides information about how many local weatherstations contributed to a certain rainfall reading. Since some of the rainfall observations are likely to be more accurate than others, I weight them according to accuracy. If there are no rainfall stations contributing to the APHRODITE data within a $.75^o$x$.75^o$ grid around the desired BASIX weather station, the observation is given a weight of 1. If there is a least one weather station in this $.75^o$x$.75^o$ grid, the observation is given a weight of 1.5. If there is a rainfall station within the $.25^o$x$.25^o$ grid, the observation is given a weight of 2. The weighted results to not differ significantly from the unweighted results.

[12]Note that while it is reasonable to think that village-specific characteristics (such as village size) may have an effect on village-level insurance take-up, village-level co-variates are not included in the regression. When the regressions are run with the village characteristics from the 2005 Indian census, the coefficients of interest do not change significantly. Also, most village-level characteristics had insignificant coefficients, with the exception that a more literate population

which shows that villages that experienced a rainfall shock in 2004 actually had an average of 3.8 *fewer* purchasers in 2005. One worry with this regression may be that since the insurance policies and rainfall patterns of each location are different, the definition of a shock may vary from one place to another. Therefore, the estimates may be improved with the inclusion of location and policy-specific covariates, which I title "Weather Station Constants". In Column 2 I add controls for the historical average rainfall, historical rainfall standard deviation, the policy premium in 2005, historical average payout of the policy, and the percentage of historical years there would have been a payout. Note that all the "historical" data is calculated from 1961-2000. With the addition of these controls, the coefficients on having a rainfall shock in 2004 remains negative, and even decreases slightly.

Following previous results that suggest that the size of the insurance payout is important, in Columns 3 and 4 I include variables for the severity of the shock in 2004 using the ratio of the hypothetical payout to the premium (the payout ratio) and the payout ratio squared. In both specifications these variables are insignificant, suggesting that most of the variation in purchasing in 2005 is explained by the binary shock variable.

The main conclusion to be drawn from these regressions is that the data does not support the hypothesis that bad weather induces people to purchase insurance in the following season. If anything, bad weather seems to decrease insurance purchases. I can only speculate on the reasons for this; it may be due to the fact that people recognize the actual negative autocorrelation of rainfall, or it may be that the rainfall shocks decrease the available liquidity to purchase insurance the following year. Regardless, this data provides relatively convincing evidence that the direct effect of weather is not causing people who receive insurance payments to purchase again the following year.

4.2 Trust, Learning, and Spillover Effects

It is also possible that the propensity to purchase insurance after receiving a payout results from learning about insurance and trusting the insurance company, as opposed to being a direct result of the payout. In order to separate the effects of trust and learning from that of receiving the payout, I make the assumption that if trust and learning are playing an important role in causing people to purchase insurance after they have received a payout, then we should be able to see a positive spillover effect of payouts within the village.[13] This is because one shouldn't need to actually receive a payout to gain the effects of trust and learning, as someone who witnesses a payout gains all the same information as someone who receives a payout.

To perform this analysis I aggregate all buyers to the village level, but divide them into two types: repeat buyers and new buyers, where repeat buyers are people who purchased insurance the year

was correlated with higher takeup. Since village-level coefficients were only available for around 50% of the villages, these variables are not included in the main specifications.

[13]If people can only gain trust and learning by actually receiving a payout themselves, then then data gives us no way to separate trust and learning from other possible mechanisms of receiving a payout.

before. I then regress the number of each type of buyer on payout statistics and the total number of buyers in the previous year. When there is an insurance payout in the previous year, most of the repeat buyers the following year received money from the insurance company, while new buyers did not receive anything.[14] These results are presented in Table 7.

In order to compare results with the main specification in Table 4, I again provide a dummy for whether there was a payout in the village along with a quadratic effect of the ratio of payouts to the premium. When aggregating the village data I used the mean of the payout ratios in the village to create a payout ratio for the village.[15] The overall results of the table tell a consistent story: payouts drive repeat buyers but not new purchasers, showing few spillover effects. Column 3 shows how payouts affect the number of repeat buyers the next year, and the results are very consistent with the baseline results from Table 4. A dummy for whether there was any payout is negative and significant, but the payout size has a positive effect. This suggests that low payouts have a marginally negative effect on the number of repeat purchasers, but this effect flips to positive as the size of the payout ratio increases above 1. Column 2 shows the effect of payouts on new buyers in a village. Here all the payout coefficients are insignificant, but due to large standard errors I cannot reject that they are the same as the effects on repeat buyers.

In Panel B I restrict the analysis to villages that had at least one buyer the year after insurance outcomes, creating a sample analogous to the 'Marketing Restricted Sample' in Table 4. The logic behind this is that if a village had zero buyers it is likely that insurance was not marketed in the village in that year, and therefore customers did not have an opportunity to purchase insurance. Restricting the data set in this way gives a much clearer pattern. Column 6 now shows much stronger effects of payouts on repeat buying, though the pattern is the same as in Column 3. Small payouts have a negative effect, while increasing the payout ratio increases repeat buying. The squared term on the payout ratio is now negative and significant, indicating that high payout ratios have diminishing effects.

These coefficients are now all significantly different from the coefficients for new buyers found in Column 5. In fact, the coefficients in Column 5 flip signs, suggesting that payouts have the opposite effect on people who did not receive payouts. These results suggest that low payouts actually induce more new buyers, but that these effects decrease and then turn negative as the payout in the village increases. The average mean village payout in this sample (for villages that received any payout) is 3.25, and the coefficients suggest that this level of payout will have roughly no effect on new purchasers compared to a village that did not experience a payout. The opposite effects of spillovers versus direct effects are somewhat perflexing, but are not consistent with the hypothesis that receiving payout increases trust and learning in the village where it occurs.

One important clarification of these results is that most of the potential "new buyers" living in a

[14] Some buyers may not have received money if they bought one phase of the insurance policy but one of the other phases paid out. This happened in 427 cases, and removing these individuals does not change there results.

[15] The results are not sensitive to using the mean, and are very similar using the median, maximum, and mode.

village that had experienced payouts would have also experienced uninsured rainfall shocks during the same season. Therefore it may be possible that there are effects of trust and learning, but they are outweighed by opposite effects of the weather. As we saw in the previous section, rainfall shocks tend to have a negative effect on insurance demand, so the (lack of) evidence of spillovers may be a result of a more complex interaction between trust/learning and direct effects of weather.

Overall, these results do not support the hypothesis that trust, learning, or any other effects of simply witnessing insurance payouts are driving increased purchasing. While it is possible that our measurements of spillovers are too crude and miss more subtle effects, the data simply does not provide evidence that here are strong spillover effects. Since we do not see these spillover effects, this casts doubt on the theory that repurchases are being driven by increased trust in the insurance company or learning about insurance payouts.

4.3 Direct Effects of Payouts on Wealth and Liquidity

The previous two sections discount the possibility that trust, learning, or weather effects are driving the result that receiving an insurance payout is correlated with purchasing insurance the following year. This points to the actual reception of money from the insurance company as being the driving force behind greater takeup. The most natural explanation for this phenomenon would be that recving an insurance payout could directly affect choices the next year due to its effects on wealth and liquidity. For instance, if insurance is a normal good then increased wealth would result in greater insurance demand.[16]

While the BASIX data set does not offer the opportunity to test the direct effects of a cash payment separately from an insurance payout, there are a number of reasons why it is unlikely that wealth or liquidity effects are driving the results. Most importantly, insurance payouts are given in the context of a rainfall shock, which would most likely result in a loss of income. It may help to recall that the empirical results are being driven by variation in rainfall across locations, not by levels of insurance within a village. Therefore, for wealth effects to be driving the results, one would need to think that experiencing an insurance payout in the context of a rainfall shock resulted in people becoming wealthier than those people who didn't experience a shock at all. Given the fact that most buyers bought a relatively low amount of insurance coverage relative to their incomes, experiencing a rainfall shock, even when insured, would likely decrease future wealth. Therefore, wealth effects seem like a poor explanation as to why receiving payouts spur future insurance sales.

If people who received insurance payouts had a decrease in wealth it is also unlikely that receiving the insurance payout would increase their liquidity the next season. Insurance payments were generally made in January, while people had the opportunity to purchase insurance for the next season only in May. It is doubtful that these payments would have a lasting enough liquidity effect to influence

[16] This is consistent with the empirical findings of Cole et al. (2013).

insurance buying decisions five months later.

While I cannot provide direct empirical evidence against the hypothesis that insurance payments drive increased take-up due to wealth or liquidity effects, given the structure and timing of insurance payments this explanation seems extremely unlikely.

4.4 Omitted Marketing Intensity

With the available data it is not possible to observe the level of marketing that each person received, making "marketing intensity" an important omitted variable. When BASIX markets rainfall insurance, it first calls a group meeting in a village, and shows the villagers a video about rainfall insurance (and other BASIX products). It then speaks with visitors and answers questions. The BASIX team then makes a follow-up visit where it goes door to door, trying to sell BASIX products including rainfall insurance. Unfortunately, the data contains no information on the specific marketing practices in each village.

If the intensity of marketing was correlated with both previous years' insurance payouts and current years' sales, this omitted variable could be biasing the results. For instance, assume that the marketing staff at BASIX think that people who have just received a payout are more likely to repurchase insurance. In this case, as the marketing team has limited resources, it may make sense for them to direct these resources towards the area of highest return, which would be people who have already received payouts. If this was the case, the increased take-up rates from people who received payouts could simply result from increased marketing attention from the BASIX team.

While the results could be picking up some of this effect, there are a couple of reasons I believe it is unlikely to be a significant factor. First, regressions of observable marketing factors (such as a dummy of whether there were any purchasers in the village) do not show any significant correlations with payouts. Next, the BASIX marketing staff claim to not give any special marketing treatment to previous payout recipients.[17] As they are trying to build long-term business, BASIX claims that they do not change their marketing practices for villages that have recently received a payout.[18] Finally, if BASIX targeted payout recipients and they did not really have a higher tendency to purchase, one would think that the marketing team would quickly learn that this strategy was not effective and would stop it. While I only observed two marketing cycles and erroneous beliefs could survive throughout this short time span, it is telling that the effect of payouts on take-up is greater in 2006 than 2005, suggesting that the effect is increasing over time.[19] If it was caused by erroneous expectations of the marketing team, we would expect the effect to decrease over time. Overall, while I must accept the possibility that increased marketing is driving the results, I regard it as unlikely.

[17]Conversation with Sridhar Reddy, Assistant Manager for Insurance at Basix, Jan 09.
[18]This claim is borne out by the fact that BASIX has continued to sell rainfall insurance through 2013.
[19]Results not shown.

5 Discussion

The previous section has spent time rejecting a number of theories about why recent insurance payouts could be driving insurance purchases. The data does not show support for a number of explanations (learning, trust, wealth, liquidity) that would be consistent with a neoclassical framework. However, there are a number of behavioral explanations that are consistent with the results.

Although we established that salience or recency bias related to the weather is unlikely to be driving the results, it is possible that recency bias unrelated to the weather is playing a role. People may simply believe that since the insurance paid out in the previous year, it is more likely to pay out in the follow year. This may be especially important if people consider insurance as an investment, as in Karni and Safra (1987). One might think that this type of bias would have spillover effects (which we do not observe), but may also be present at the individual level if people believe recent insurance payouts are a sign of individual luck.

Another explanation could be that insurance purchasers behave as if they are "gambling with house money", as in Thaler and Johnson (1990). If customers exhibit loss aversion, they may not view insurance premiums paid after receiving payouts as true losses, since they are still "in the red" in their relationship with the insurance company. If we assume that reference points adjust after receiving insurance payouts, the observed behavior is consistent with this theory.

A related explanation comes from an observational study on mutual insurance among fisherman in the Côte d'Ivoire by Platteau (1997). Platteau observes malfunctioning mutual insurance cooperatives and theorizes that they are failing because members view insurance as a system of balanced reciprocity, meaning that they expect to break even over the lifetime of the scheme. When members have not received the services (in this case sea rescue) of the mutual in a long time, they start to view the insurance as a bad deal and ask for their contributions back. The results in this paper are consistent with people viewing insurance as a system of balanced reciprocity, as they could see insurance purchases after receiving a payout as giving back to a system that has helped them previously.

The data used in this study does not allow me to distinguish between these competing explanations. They remain as areas for future research.

6 Conclusion

After receiving an insurance payout, customers of rainfall insurance in India are 9-22% more likely to purchase insurance again the next year. This behavior seems to be driven by actually receiving the money from the insurance company, as the data does not support alternative theories of re-purchase being driven by direct weather effects, trust, learning, wealth, or liquidity.

This study brings to light a number of questions that would be ripe for future research. First of all, it would be interesting to understand whether insurance payouts have long-term effects on future purchases, and also whether payouts continue to have similar effects for people who have years of experience with insurance. To answer these questions one would need a data set with a longer time frame. Also, a longer data set could shed further light onto the question of whether customers learn about insurance over time. It is possible that people need a few years of experience with insurance to really learn about the product and gain trust in it, which would explain why this paper fails to see any spillover effects.

These results point to a number of policy recommendations for the Indian rainfall insurance market, and possibly for insurance markets in general. One of the main arguments made for the slow adoption of insurance in India is that people do not understand insurance and do not trust the insurance companies. If trust and learning were the crucial determinant of insurance adoption, then incentives could be given to encourage early adoption and over time as people witnessed and experienced payouts we would expect insurance adoption to grow. This paper fails to find any evidence of increased trust and learning driving insurance decisions, which suggests that incentivizing early adopting is unlikely to quickly spur insurance take-up.

Historical evidence (as in Kunreuther et al. 1985) has suggested that an effective policy to spur insurance markets would be to target areas that have recently experienced a large shock. This paper does not support this notion in the case of rainfall index insurance in India, as places that recently experienced a shock were less likely to purchase insurance. Instead, it supports a policy of heavy premium subsidies in early year of the product, to ensure high take-up and therefore create a situation where a large number of people could experience payouts.

With relation to the future of rainfall index insurance in India, one stark result is that the raw numbers of continuing customers of insurance are very low, calling into question the sustainability of the product. Even among people who received payouts in excess of twice their premium in 2006, only 18% bought again in 2007. With the proportion of repeat buyers so low, one would have to assume that many people are not satisfied with their experience of insurance, which suggests that the product or marketplace will need to evolve in order to survive.

One factor to note is that this study looks at the early years of the first major scale-up of rainfall insurance in the world. Rainfall insurance is still a young product, and is still evolving to meet the needs of customers. One particular point of attention is the massive loading on most policies offered. As we saw in Table 2, many BASIX insurance policies had premiums of up to six times the actuarially fair rate. With premiums this high, it is unsurprising that people are not signing up. Also, one may argue that the correlation between insurance payouts and crop outcomes were less than ideal in these early products. Around the world, index insurance policies are constantly evolving to better correlate with crop outcomes and avoid basis risk. While this study predicts that rainfall insurance in the form of BASIX's policies from 2005-2007 are likely to fail, it is quite possible that innovations in products and pricing can create an insurance product that better meets the needs of small scale farmers.

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Table 1: Example Insurance Policy

Phase	I	II	III
Duration (Days)	35	35	35
Type	Deficit	Deficit	Excess
Strike (mm)	135	125	730
Exit (mm)	40	40	820
Notional (Rs/mm)	10	10	10
Policy Limit (Rs)	1000	1000	1000
Premium (Rs)	110	110	90

Figure 1: Example Payout Schedule

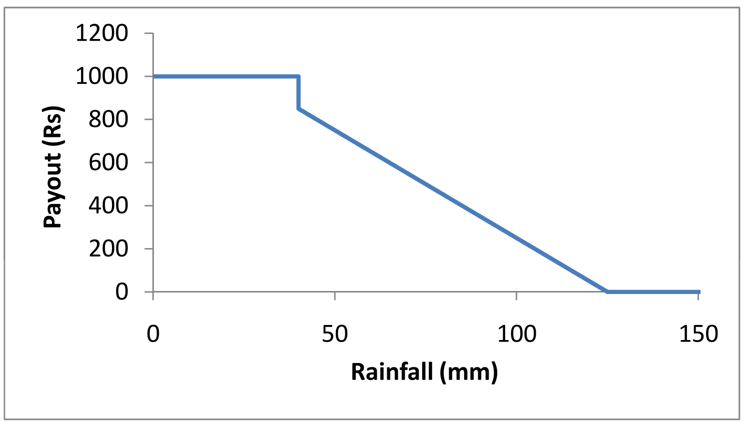

Table 2: Policy Summary Statistics

Year	2005	2006	2007
Number of Policies	34	42	28
Average Premium For Three Phases (Rs)	283	295	287
Expected Payout (Using rainfall from 1961-2004)	76	73	80
Mean Ratio of Premium to Expected Payout	5.76	5.94	5.6
Mean Percentage of Times Policy Paid out From 1961-2004	10.2	6.8	7

Table 3: Customer Summary Statistics

Year	2005	2006	2007
Number of Villages	954	1426	432
Number of Weather Stations	34	42	28
Number of Buyers	6428	10077	3377
Average Sum Insured (Rs)	3055	1612	3547
Buyers Receiving Payouts	351	1346	529
Average Payout	10.66	60.28	87.74
Average Payout (if Payout>0)	195.19	360.13	553.23
Buyers Who bought the Following Year	453	364	

Table 4: Insurance Repurchasing

	Dependent Variable is Customer Re-Purchasing Insurance			
	(1)	(2)	(3)	(4)
Received Insurance Payout	0.0897***	0.222***	-0.0877***	-0.195***
	(0.0242)	(0.0442)	(0.0201)	(0.0666)
Ratio of Payout to Premium			0.123***	0.246***
			(0.0237)	(0.0405)
Ratio of Payout to Premium ^2			-0.0120***	-0.0243***
			(0.00263)	(0.00409)
Year 2006 Dummy	-0.0250**	-0.0269	-0.0395***	-0.0336
	(0.0111)	(0.0274)	(0.0107)	(0.0274)
Constant	0.0704***	0.165***	0.0771***	0.168***
	(0.00901)	(0.0172)	(0.00898)	(0.0173)
Marketing Restricted Sample	NO	YES	NO	YES
Observations	10,997	4,201	10,997	4,201
R-squared	0.014	0.035	0.034	0.058

Robust standard errors in parentheses — State Level Fixed Effects Included

*** p<0.01, ** p<0.05, * p<0.1 — Errors Clustered at the Village Level

Table 5: First Order Autocorrelation of Weather Variables

	Fixed Effects	Arellano-Bond
	(1)	(2)
Total Rainfall	-0.106***	-.086***
	(.030)	(.021)
Phase 1 Rainfall	-.090***	-.075***
	(.030)	(.029)
Phase 2 Rainfall	-.018	-.026
	(.030)	(.028)
Phase 3 Rainfall	-.029	.007
	(.030)	(.028)
Would Have Been Payout	.023	.017
	(.030)	(.022)
Total Insurance Payout	-.0353	.004
	(.030)	(.028)
Weather Station Fixed Effects	YES	YES

Coefficients reported are from separate univariate regressions

Observation are years 1967-2004 for Fixed Effects Regression

Observation are years 1962-2004 for Arellano-Bond Regression

Fixed Effects regression contains six lags, Coefficient of First Lag Displayed

Arellano-Bond Regression contains one lag

Standard Errors are in Parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of Shocks on Purchasing

	Dependent variable is number of buyers in 2005			
	(1)	(2)	(3)	(4)
Would Have Been Payout in 2004	-3.843***	-4.592***	-5.045**	-3.788*
	(0.987)	(1.039)	(2.173)	(1.898)
Ratio of 2004 Payout to 2005 Premium			4.365	-0.755
			(4.610)	(5.543)
Payout Ratio Squared			-1.991	-0.279
			(1.814)	(2.064)
Constant	8.001***	0.651	7.985***	1.015
	(0.714)	(6.341)	(0.713)	(6.494)
Weather Station Constants	NO	YES	NO	YES
Observations	733	733	733	733
R-squared	0.073	0.094	0.075	0.097

Robust standard errors in parentheses | Obervations weighted by quality of rainfall data
*** p<0.01, ** p<0.05, * p<0.1 | Errors Clustered at Weather Station Level
All Regressions Include State Fixed Effects

Table 7: New Buyers In A Village

	Dependent Variable is the Number of Buyers in a Village the Following Year					
	Panel A: All Villages			Panel B: Villages With At Least 1 Repeat Buyer		
	Total Buyers	New Buyers	Repeat Buyers	Total Buyers	New Buyers	Repeat Buyers
	(1)	(2)	(3)	(4)	(5)	(6)
Was Payout in Village	-0.334	0.376	-0.709**	3.353	5.799*	-2.446***
	(2.252)	(2.121)	(0.338)	(3.275)	(3.413)	(0.433)
Mean Ratio of Payout to Premium	1.319	0.481	0.838*	0.522	-2.131*	2.653***
	(1.330)	(1.050)	(0.463)	(1.094)	(1.095)	(0.168)
Mean Payout Ratio Squared	-0.135	-0.0617	-0.0729	-0.0832	0.166	-0.249***
	(0.141)	(0.106)	(0.0539)	(0.120)	(0.114)	(0.0167)
Number of Buyers in Village	0.131***	0.0795*	0.0512***	0.197***	0.0959	0.101**
	(0.0480)	(0.0434)	(0.0178)	(0.0530)	(0.0596)	(0.0427)
Year 2006 Dummy	-2.994*	-2.738*	-0.256	-5.231	-5.241*	0.0105
	(1.661)	(1.448)	(0.236)	(3.284)	(2.929)	(0.514)
Constant	3.445***	3.301***	0.144	10.48***	10.00***	0.483*
	(1.140)	(1.065)	(0.136)	(1.599)	(1.614)	(0.249)
Observations	1534	1534	1534	459	459	459
R-squared	0.061	0.047	0.118	0.084	0.069	0.285

Robust standard errors in parentheses | Errors clustered at the weather station level. All regressions include state fixed effects
*** p<0.01, ** p<0.05, * p<0.1 | Data is aggregated to the Village Level
Includes all villages in 2005 and 2006 where there was insurance coverage the following year