Dynamic Field Experiments in Development Economics: Risk Valuation in Morocco, Kenya, and Peru

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The effective design and implementation of interventions that reduce vulnerability and poverty require a solid understanding of underlying poverty dynamics and associated behavioral responses. Stochastic and dynamic benefit streams can make it difficult for the poor to learn the value of such interventions to them. We explore how dynamic field experiments can help (i) intended beneficiaries to learn and understand these complicated benefit streams, and (ii) researchers to better understand how the poor respond to risk when faced with nonlinear welfare dynamics. We discuss and analyze dynamic risk valuation experiments in Morocco, Peru, and Kenya.

Key Words: poverty, risk and uncertainty, dynamics, experiments, Kenya, Morocco, Peru

Recent research in development economics has improved our theoretical conception of poverty and empirical methods for measuring it. As one significant improvement, economists now pay much greater attention to dynamic dimensions of poverty and vulnerability. This appreciation for asset and poverty dynamics and for the crucial intertemporal dimensions of poverty in rural agro-pastoral settings is manifest in both theoretical and empirical advances and is starting to influence policy in some settings.

The effective design and implementation of interventions that reduce vulnerability and poverty require a solid understanding of underlying poverty dynamics and associated behavioral responses. When introducing insurance products or risk-reducing crops, for example, understanding how the target beneficiaries assess and value risk in the context of these underlying dynamics is critical. Of course, the success of these interventions hinges not only on our understanding as researchers of the underlying dynamics and behavioral responses but turns critically on how well

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the target beneficiaries understand the potential benefits associated with a new product, policy, or technology. Interventions aimed at reducing vulnerability often confer benefits that are both stochastic and dynamic, which complicates this learning process. To illustrate, the benefits of an insurance product to a household depend on the dynamic wealth or asset forces it faces (e.g., its asset position relative to a dynamic asset threshold). These benefits are also stochastic since payouts are a function of a stochastic outcome such as rainfall. Stochastic and dynamic benefit streams can make it difficult for the poor to learn the value of such an insurance product to them—even after they fully understand how the product works.

Field experiments can help intended beneficiaries to learn and understand these complicated benefit streams. Moreover, these experiments can help us as researchers to better understand how the poor respond to risk when faced with nonlinear welfare dynamics. We describe in this paper three recent field experiments motivated by these objectives that explicitly incorporate dynamic elements and incentives. Each of these experiments is part of a separate research effort to reduce the vulnerability of the rural poor. Each is also the subject of more detailed ongoing analyses, so our intent in this paper is not to provide a comprehensive analysis and comparison of these three experiments. Rather, we focus here on how these three experiments introduce dynamic features and how subjects respond to these features. For each experiment, we provide some essential details about the larger research projects into which they fit, but intentionally stop well short of a complete description of the project, the experiment, or the broader results.[1]

The first project aims to assess the welfare impacts of drought risk among rainfed cereal farmers in Morocco and to evaluate farmers' valuation of drought-tolerant cereal varieties in this context. As part of this project, a field experiment was designed to simulate drought tolerance and elicit farmers' valuation of this trait. The experiment elicited their valuation of drought tolerance without and with land accumulation dynamics, and our analysis highlights the effect these dynamics have on subjects' risk valuation. The second and third projects aim to introduce index insurance products among Peruvian farmers and Kenyan pastoralists, respectively. The experiments associated with these projects elicit subjects' valuation of these insurance products and explicitly incorporate dynamic incentives. Since these experiments are part of an effort to roll out new insurance products, a primary objective of these experiments is to build comprehension among subjects.

In the next section, we offer a brief background to experimental development economics and to dynamic experiments in economics more broadly. We then describe and present the Moroccan field experiment and results related to the dynamic element of this experiment. Then, in the subsequent two sections, we describe and present the field experiments from Peru and Kenya, respectively, along with pertinent results. For each field experiment, we present analysis of the effect of the dynamic structure on subject behavior. We conclude with a comparison and discussion of these three experiments, which we use as a platform for assessing the potential value and limitations of dynamic experiments in development economics.

Background

After Binswanger conducted risk experiments in India (Binswanger 1980), development economists did very little subsequent work with experiments for nearly two decades. The past decade, however, has seen an explosion of experiments in development economics [see Cardenas and Carpenter (2008) for an overview]. Most of these recent experiments fit into standard categories such as risk, public goods, and social norms related to fairness and equality. Others are tailored to topics that are fairly unique to development economics. For example, some sophisticated experiments have simulated various lender-borrower and borrower-borrower interactions in microfinance arrangements that involve group lending (e.g., Cassar, Crowley, and Wydick 2007, Gine et al. 2009). The experimental design mindset now permeates nearly every aspect of development economics, including the randomized evaluation of programs. Many of these experiments have generated insights that could not have come via standard research methods.

[1] Interested readers are referred to working papers—available upon request—for these details. The full protocols used to administer the experiments are also available from the authors upon request.

Several recent field experiments in development economics have built on Binswanger's experimental elicitation of risk preferences. Many of these offer subjects a choice of gambles much like Binswanger's original design (e.g., Humphrey and Verschoor 2004a, Wik and Holden 1998). Others elicit certainty equivalents directly through open-ended bidding for various gambles (e.g., Henrich and McElreath 2002, Lybbert 2006). Whereas risk experiments in development economics initially aimed to provide estimates of coefficients of risk aversion, the more recent round of risk experiments aim to test prospect theory and other alternatives to expected utility theory (e.g., Humphrey and Verschoor 2004b), to elicit time and risk preferences jointly (Tanaka, Camerer, and Nguyen, forthcoming), or to evaluate risky decision making in specific contexts as implied by decisions related to specific risk-related products such as new seed varieties that reduce risk by conferring pest or drought tolerance (Lybbert 2006). The experiments we discuss in this paper are of the latter ilk and assess risky decision making by offering subjects gambles representing new risk-reducing seeds and insurance products that protect them from bad covariate shocks.

Both in and (mostly) out of development economics, economists have recently started designing dynamic experiments in which subjects' decision problem is linked across repeated rounds—often via randomized endowments or cumulative earnings and a discrete change in key parameters in the experiment at a known point in endowment or earnings space. In developing countries, dynamic designs have appeared in microfinance experiments in which default on a loan jeopardizes future credit (Abbink et al. 2006, Gine et al. 2009). The dynamic incentive created by the threat of cutting off future credit importantly shapes repayment rates. Indeed, judging by experimental evidence, these dynamic incentives seem to affect repayment rates more than group lending arrangements (Abbink et al. 2006, Gine et al. 2009). More relevant for our purposes, Gine et al. (2009) use their experimental data from individual loans without dynamic incentives to categorize individuals into three (static) risk-aversion types, then condition individuals' subsequent response to dynamic incentives on this indicator of static risk aversion. They find that the risk response (i.e., the reduction in propensity to select the risky project) to adding dynamic incentives is significant for the high and medium risk averse types, but not for low risk averse types. Throughout this paper, we explore this relationship between static risk aversion and dynamic risk responses in experimental settings in greater detail.

Outside of developing country contexts, many more experiments have incorporated dynamic incentives. These include public goods experiments with repeated rounds that can create dynamic incentives to build reputation over rounds, natural experiments that involve inherently dynamic choices (Andersen et al. 2006), experiments aimed at testing choice behavior with asset integration and the formation of natural reference points (Andersen, Harrison, and Rutström 2006), and dynamic saving experiments used to test bounded rationality and learning (Brown, Chua, and Camerer 2009). One recent dynamic experiment conducted among U.S. undergraduates in lab settings assesses the impact of voting and communication on growth in experimental economies that are subject to poverty traps (Capra et al. 2009). Although topically related to the experiments we describe below, this experiment has quite a different objective, is far more stylized, and is not conducted in a field setting.

Morocco: Drought Risk and Dynamic Thresholds

Droughts evoke a double response from the rural poor engaged in rainfed agro-pastoralism (see Elbers, Gunning, and Kinsey 2007). First, drought events directly impact these households and often force them to modify their livelihood strategies as a matter of survival. Second, the anticipation of drought can dramatically shape the livelihood strategies a household chooses. These responses have important welfare implications for poor and vulnerable households.

Morocco has become especially drought prone in the last 30 years. In the late 1990s, the World Bank launched a rigorous effort to create a rainfall index insurance product in Morocco, but this project stalled just before the insurance product was to be marketed, in part because the recent downward trend in rainfall hinted at a troubling non-stationarity in precipitation data. While other insurance interventions to improve drought risk management are still on the table, agricultural research and development in drought tolerance as a cereal trait remains a priority.

Experiment Design

As part of a multi-year project aimed at characterizing drought risk in Morocco and better understanding household drought-coping strategies, we conducted a framed field experiment[2] with rainfed cereal farmers in the Meknes region. The goal of these experiments was to assess farmers' valuation of drought tolerance as a seed trait. In these experiments, farmers were offered three distinct payoff distributions—each representing a crop return distribution associated with a "seed." The payoff distributions presented payoffs as a function of "rainfall." A "rainfall" chip was drawn at the end of each round to determine the crop return for that round. These distributions include a baseline "seed" A, a drought-tolerant "seed" B, which was less sensitive to low rainfall, and a risky, high return "seed" C that was much more responsive to the rainfall draw.

After building comprehension through practice rounds, we elicited farmers' valuation of these "seeds" in isolation using the Becker-DeGroot-Marschak (1964) mechanism. Next, we offered these farmers all three experimental "seeds" and asked them to choose between them. Then, in the final segment of the experiment—the focus of the current paper—we continued to offer them a choice among the three experimental "seeds" but linked repeated rounds via cumulative earnings and introduced two discrete dynamic thresholds. In this dynamic experiment, farmers started with one plot, but they could lose this plot if their cumulative earnings dropped below the first threshold, set at cumulative earnings less than or equal to zero (i.e., bankruptcy). They would acquire a second plot if their cumulative earnings surpassed 140 Moroccan Dirhams (Dh). We conducted three sets of seven dynamically linked rounds in order to allow for learning.

[2] Framed field experiments are framed using a specific context and conducted with subjects who are familiar with this context. See Harrison and List (2004).

Effects of Dynamic Design

Our experimental design allows us to use farmers' static seed valuation and choices as control variables and thereby isolate the behavioral response introduced by the dynamic thresholds. Specifically, we use two measures of static risk aversion that do not require the specification of a utility function. First, we use individuals' willingness to pay for the stand-alone gambles in the static round to compute their implied risk premium averaged over the three gambles:

$$RP_i = \sum_{j=1}^{3} \left(EV_j - WTP_{ij} \right) \Big/ EV_j \ .$$

Second, we use individuals' dichotomous choices between the three gambles to compute their average choice. This is another indicator of risk aversion since gamble C is riskier than A, which is riskier than B. We use this same natural ordering as the basis of an ordered probit model to estimate the behavioral response to the dynamic thresholds in the set of dynamic rounds. In addition to these risk-aversion measures, we use a household wealth index as a time-invariant subject trait and subjects' earnings in the previous round as a time-variant control variable.[3] To estimate the effect of the dynamic thresholds, we measure individuals' proximity to the 0Dh cumulative earnings threshold (bankruptcy and loss of first plot for one round) from above and the 140Dh cumulative earnings threshold (acquisition of second plot) from both below and above. Figure 1 depicts these proximity functions. In the full specification, we include these proximity measures alone and interacted with risk premium (RP) and with an index of household wealth in order to allow behavioral responses to these dynamic thresholds to be conditional on risk preferences and wealth.

Table 1 contains the estimation results of three ordered probit models with random effects. Model 1 omits independent variables constructed as interactions between the proximity variables depicted in Figure 1 and wealth and risk-aversion variables. Specification 2 includes these interac-

[3] In multiple round experiments, subjects are often sensitive to previous round experiences. While these intra-experiment dynamics are not our focus, it is nevertheless necessary to control for them in order to avoid omitted variable bias.

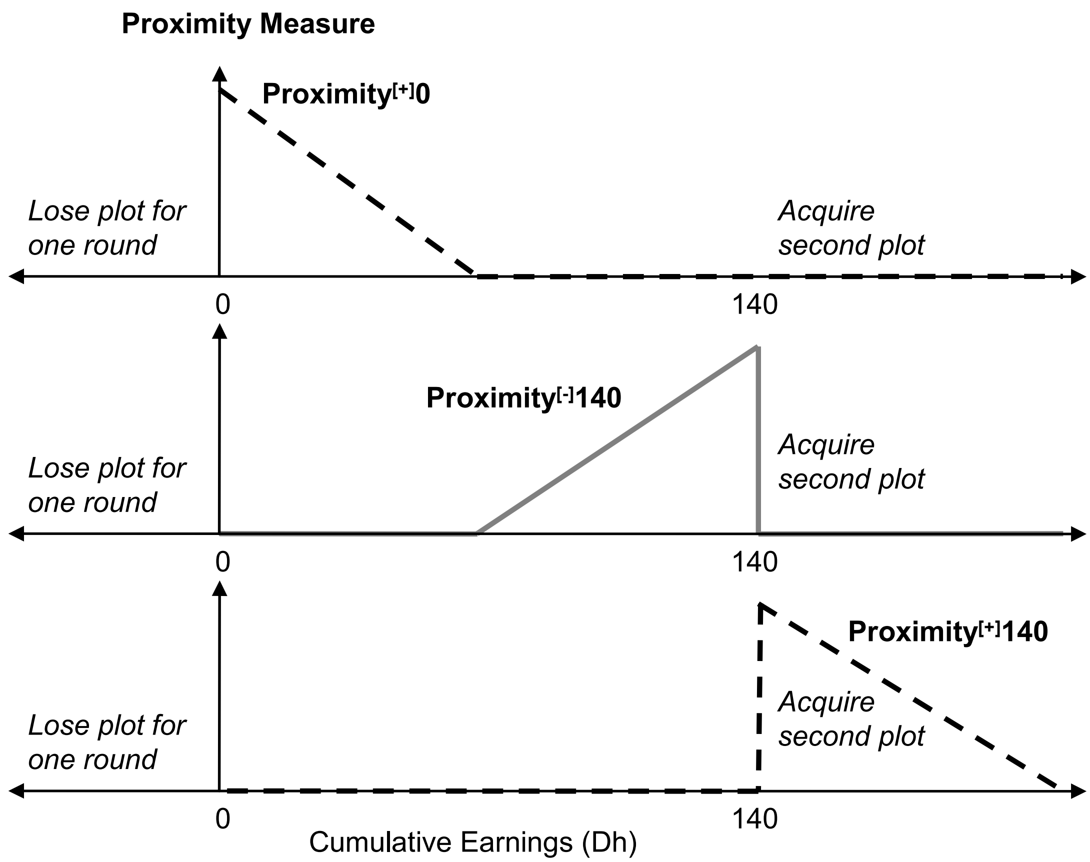

Figure 1. Proximity Functions Used to Measure Distance from Dynamic Loss (dashed lines) and Gain (solid lines) Thresholds at 0 Dh and 140 Dh of Cumulative Earnings

tion terms. Specification 3 includes these interaction terms and uses a restricted sample that excludes rounds for which cumulative earnings carried over from the prior round were miscalculated or the dynamic thresholds were mistakenly applied. As apparent from the number of observations, very few rounds are excluded by this criteria (<1 percent), but this serves as a robustness check nonetheless.

Consider first model 1 Table 1. Since the gamble choices used as dependent variables are ordered from low relative risk (seed B) to high relative risk (seed C), a positive coefficient indicates a higher probability of choosing a risky gamble. Thus, the estimated coefficients on the stand-alone proximity measures suggest that subjects tend to react most to the looming bankruptcy threshold at 0Dh. The quadratic shape of this relationship implies that once a subject's cumulative earnings slip below 40Dh, he starts shifting to less risky gambles. Approaching the 140Dh threshold from below causes subjects to shift toward riskier gambles, presumably in the hopes of clearing the threshold and acquiring a second plot. This is consistent with the dynamic risk response described by Lybbert and Barrett (forthcoming).

The plot 1 and 2 dummies—which indicate the main plot and second plot, respectively, in rounds when a farmer has two plots—indicate that farmers opt predominantly for the risky gamble on the second plot and that proximity to the 140Dh threshold does not shape this response. The coefficient on Plot 1 of 2, which is 1 for the farmer's first plot only if he had a second that round, suggests that the mere presence of a second plot does not affect the seed choice for the first plot. The

Table 1. Ordered Probit Results for "Seed" Choice as Function of Distance from Dynamic Thresholds

Model	1	2	3
Proximity [+] 0	0.014*** (0.0056)	0.014*** (0.0056)	0.013*** (0.0059)
Proximity [+] 0^2	-0.00020*** (0.000071)	-0.00018*** (0.000072)	-0.00017*** (0.00076)
Proximity [-] 140	0.014 (0.0079)**	0.0029 (0.0085)	0.0038 (0.0087)
Proximity [-] 140^2	-0.00017 (0.00013)	-0.000014 (0.00014)	-0.000034 (0.00015)
Proximity [+] 140	-0.0071 (0.013)	-0.0078 (0.013)	-0.0084 (0.014)
Proximity [+] 140^2	0.00015 (0.00022)	0.00011 (0.00022)	0.000082 (0.00023)
Plot 1 of 2 {0,1}	-0.041 (0.15)	-0.047 (0.15)	0.039 (0.17)
Plot 2 of 2 {0,1}	0.31** (0.16)	0.30** (0.16)	0.21 (0.17)
Plot 2 × prox [+] 140	0.021 (0.018)	0.021 (0.018)	0.025 (0.019)
Plot 2 × prox [+] 140^2	-0.00040 (0.00030)	-0.00041 (0.00030)	-0.00042 (0.00030)
Avg risk premium × prox [+] 0		-0.0019 (0.0049)	-0.0014 (0.0050)
Avg risk premium × prox [+] 0^2		-0.00013 (0.00014)	-0.00013 (0.00014)
Avg risk premium × prox [-] 140		0.035*** (0.015)	0.035*** (0.015)
Avg risk premium × prox [-] 140^2		0.0033*** (0.00094)	0.0033*** (0.00095)
Avg risk premium × prox [+] 140		-0.0031 (0.0081)	-0.00061 (0.0081)
Avg risk premium × prox [+] 140^2		0.00019 (0.00034)	0.00026 (0.00035)
Wealth × prox [+] 0		-0.00022 (0.0013)	-0.00013 (0.0013)
Wealth × prox [+] 0^2		-3.3e-06 (9.5e-06)	-5.6e-06 (9.9e-06)
Wealth × prox [-] 140		0.0045* (0.0031)	0.0046* (0.0031)
Wealth × prox [-] 140^2		-0.000041 (0.000040)	-0.000041 (0.000041)
Wealth × prox [+] 140		0.0022 (0.0021)	0.0022 (0.0021)
Wealth × prox [+] 140^2		0.000041* (0.000026)	0.000044** (0.000026)

cont'd.

Table 1 (cont'd.)

Model	1	2	3
Previous round earnings	-0.00063 (0.00062)	-0.00070 (0.00062)	-0.00083 (0.00063)
Wealth index	-0.085** (0.051)	-0.11 (0.077)	-0.13* (0.078)
Avg risk premium	-0.030 (0.19)	-0.0051 (0.28)	-0.040 (0.28)
Avg static "seed" choice	0.52*** (0.082)	0.53*** (0.082)	0.53*** (0.083)
Random effects	Yes	Yes	Yes
Excluding errors	No	No	Yes
N	2236	2236	2198

Note: ***, **, and * denote significance at the 5 percent, 10 percent, and 15 percent levels, respectively. Standard errors are in parentheses.

estimated coefficients on the other control variables suggest that relatively wealthier farmers tend to choose less risky gambles, and that risk aversion measured by the average (static) risk premium has no statistically significant effect on seed choice. The average static seed choice, however, is highly significant. Seed choice in the static rounds is indeed a good predictor of seed choice in the dynamic rounds. Our interest is not in this coefficient per se, but in these static risk-aversion measures as control variables.

In models 2 and 3, the coefficients on interaction terms—*conditioned* on these static measures—provide the key results related to the behavioral effect of the dynamic thresholds of the experiment. The average risk premium significantly affects seed choice when interacted with the below 140Dh proximity measure. Farmers that are more statically risk averse are much more likely to shift to the risky gamble as they close in on this threshold. Whereas Gine et al. (2009)—who did not include a gain threshold in their design—find that static risk aversion is positively correlated with a cautious dynamic risk response just above a loss threshold (cutting access to future credit), we find that static risk aversion is positively correlated with risk-seeking behavior just below a gain threshold. Like Gine et al. (2009), we find evidence of a cautious dynamic risk response just above a loss threshold, but this response is unconditioned on static risk aversion. Turning to the coefficients on wealth interactions, relatively wealthy farmers appear more likely to take additional risk when they are just below the gain threshold. In general, farmers seem to be less responsive to this threshold once they are above it, although there is some evidence that poorer farmers shift toward safer seed gambles just above this gain threshold.

Peru: Area Yield Index Insurance

Whereas the Moroccan experiment elicited farmers' valuation of drought risk and drought tolerance in general, the experiments in Kenya and Peru were built around specific insurance products that had already been designed. Soon after these experiments were conducted, these index insurance products were released to pastoralists and farmers, including subjects from the experiments. The experiments in Kenya and Peru aimed not only to understand subjects' valuation of risk and insurance but to build their comprehension of a specific insurance product, including the concept of basis risk. These experiments were therefore calibrated to accurately reflect features of the underlying index insurance product.

The Peruvian experiment was motivated by an area yield index insurance product, which aims to reduce the costs of uninsured risk for poor agricultural households. Risk makes people poor when it leads them to shy away from higher-return but riskier activities. Risk keeps people poor when it leads them to pursue defensive savings strategies that cut off pathways from poverty that they could traverse via sustained accumulation of productive assets. Risk impedes the development of agricultural finance markets in a region that can be important to the growth and development of the small-farm sector.

While insurance is a potential solution to these problems of pervasive and costly risk, it is widely absent in low-income rural areas. Furthermore, only if potential buyers understand its main properties can insurance remedy these risk impediments. Motivated by this premise, we designed and conducted a framed field experiment in the Pisco valley of southern Peru to build the familiarity with insurance that is required to sustain an insurance market. This experiment was calibrated to reflect an area yield insurance product that was designed as part of the broader research project. It was designed to assess the impact of insurance on credit uptake and reproduced the dynamic incentives underlying insurance and loan contracts. We conducted experiments with a random sample of about 400 small-scale cotton producers, who have limited formal education (6 years on average), extensive farming experience (with an average age of 55 years, the typical farmer spent 24 of them managing a farm), and existing access to working capital loans (60 percent with access).

Experiment Design

Crop yield insurance is designed to help farmers to smooth rough spots. Payments received in bad years substitute for lost income, allowing individuals to smooth their consumption over time. In many situations, insurance offers a second important advantage. Uninsured farmers, who borrow to pursue a commercial strategy, risk losing their land if a drought or another weather-related negative shock leaves them unable to repay a loan backed up by their land. Empirical work carried out in rural Peru reveals that about 20 percent of small farmers may refuse to take out loans precisely because they fear losing the assets on which their future livelihoods depend (Boucher, Carter, and Guirkinger 2008). In this context, insurance can allow rural producers to preserve their asset base. The experimental design challenge we faced was capturing and conveying this nuanced insurance benefit in a way that is transparent and accessible to farmers with limited literacy.

The experiment in Peru involved two sets of rounds. First, in a baseline set subjects chose between project 1, a low risk but low return self-financed project, and project 2, a high risk and high return project financed with an uninsured loan that used subjects' land as collateral.[4] Second, in a treatment set of rounds, subjects chose between these two options plus project 3, a high risk and high return project financed with an insured loan. Throughout the experiment, the yield on each project depended linearly on individual and covariate shocks. The covariate shock determined the average yield in a valley, and the individual shock, which we called "luck," captured individual-specific factors that added to or subtracted from the average valley yield to produce an individual's actual yield. The distributions of both covariate and individual shocks were estimated using yield data from the Pisco valley. We discretized these shocks to create five covariate shocks (very low, low, normal, high, and very high average valley yield) and three individual "luck" shocks (good, normal, and bad, with "normal" representing the center of the respective densities. In addition to earnings from the random yield on projects, subjects earned money based on the value of their land at the conclusion of each set of rounds. Dynamic incentives were introduced in this experiment via the possibility of defaulting on the loan, which reduced both access to credit and the value of land (collateral) in future rounds.

In baseline rounds, subjects chose between projects 1 and 2 and then learned the average yield prevailing in their valley and their individual luck for that round. The randomizing devices used to simulate the realizations of these discretized shocks were poker chips (valley yield) and ping-pong balls (individual luck). In each round, representing a single farming season, one participant from each valley drew a poker chip from the "valley sack" containing ten colored chips: 1 green

[4] In these experiments, subjects were endowed with a hectare of land at the beginning of the experiment. Those in default had their land depreciated.

(very high), 2 blue, 4 white, 2 red, and 1 black (very low). Each subject then individually drew a colored ball from the "luck sack," which contained 1 yellow ball (very good luck), 2 white (normal), and 1 purple (bad luck). Payoffs to the uninsured loan project 2 were negative under some realizations of those shocks, thus making it impossible to repay the loan and forcing an individual into default. Once in default, individuals lost access to future credit (i.e., they were stuck with the self-financed project 1) and saw their land—used as collateral—depreciate in value. This default effect on earnings makes this experiment dynamic.

In insurance treatment rounds, subjects were offered a third project in addition to projects 1 and 2. Project 3 involved simultaneously taking a loan and buying area-based yield (ABY) insurance. This ABY insurance contract was designed so that indemnity payouts occurred when either a low (red chip) or very low (black chip) covariate shock was drawn. Thus, with this insured loan, subjects could reduce the likelihood of default in rounds with an average valley yield that was low or very low. Since the ABY insurance is triggered by the covariate shock alone, however, subjects choosing the insured loan might still default due to a bad individual luck draw with normal average valley yields. Indeed, helping farmers to understand this basis risk was a primary objective of the experiment. By evaluating how subjects respond to this insured loan project (i.e., comparing the baseline and treatment sets), we are able to assess the extent to which ABY insurance reduces the fear of default and thereby encourages people to take loans in order to undertake riskier but more profitable activities (i.e., the "de-rationing" effect).

To enable subjects to learn the covariate and individual nature of risk in this experiment, as well as the implications of choosing a particular project, subjects played a sequence of six low stakes or learning rounds in both the baseline and treatment sets. Low stakes rounds were followed by a sequence of six high stakes rounds in both games, where subjects were paid twice as much for each payoff unit they earned. Learning how insurance works was further facilitated by the fact that participants experienced several rounds in rapid succession, each with a different covariate and individual shock.

Effects of the Dynamic Design

Before turning to the data to assess the effect of the dynamic default feature of this experiment, it is worth noting that the design of these dynamic incentives involved some important pre-testing—precisely because they strongly affect subjects' behavior. In our original design, we initially gave each player two hectares for cotton production, each with a land title certificate. One hectare of land was required as collateral on any loan. Any player unable to repay a loan therefore had to forfeit one hectare of land and its corresponding land title certificate. Such a player was thereafter in the precarious position of holding only a single hectare of land. Based on pre-testing of the experiment, farmers readily understood this default mechanism, but it proved to be too powerful an incentive. It also dramatically increased competition among participants, who enjoyed teasing fellow players who lost their land. While it is true that Peruvian lenders threaten to seize land in the event of default, such threats are rarely implemented in our area of study. Thus, in order to better mimic naturally occurring dynamic incentives, default was softened so that defaulting individuals lost access to the credit system for future rounds and were paid a lower value at the end of the game for land against which a credit lien was still held.

Table 2 summarizes the results of the baseline rounds and the insurance treatment. In the baseline rounds, 24 percent of subjects chose the self-finance project (i.e., were risk-rationed) and 76 percent chose the uninsured loan project. In the insurance treatment, the majority of risk-rationed subjects responded to the possibility of dodging the dynamic default penalty by choosing the insured loan instead of the other two projects. In particular, 57 percent of those risk-rationed in the baseline chose the insured loan project, a result that suggests that introducing insurance may effectively increase the reach of credit markets through de-rationing. Overall, almost 60 percent of subjects chose the insured loan project in the insurance treatment.

To examine how the dynamic incentives affected project choices, we estimated an ordered logit model with project choice in the insurance treatment as dependent variable, where the ordering is given by the riskiness implied by the

Table 2. Choices Made in the Baseline and Insurance Games, Peru

		Insurance Game				
		Uninsured Loan (1)	Self-Finance (2)	Insured Loan (3)	Total	%
Baseline Game	Uninsured Loan (1)	109	14	164	287	75.9
	%	*38.0*	*4.9*	*57.0*	*100.0*	
	Self-Finance (2)	20	19	52	91	24.1
	%	*22.0*	*20.9*	*57.1*	*100.0*	
	Total	129	33	216	378	100.0
	%	*34.1*	*8.7*	*57.1*	*100.0*	

projects from uninsured loan (most risk) to insured loan (less risk) to self-finance (least risk). As control variables, we use a set of variables from within the experiment, including the probability of being risk-rationed in the high stakes baseline rounds, the subject's earnings in previous rounds, whether two consecutive "very low" covariate shocks (average valley yields) were drawn in the two final low stakes rounds before the high stakes rounds of the insurance treatment began, an index that indicates comprehension of the experiment, and risk indicators based on a Holt and Laury (2002) lottery experiment conducted at the conclusion of the insurance treatment.[5] We constructed the comprehension index using a series of questions asked at the conclusion of the experiment that gauged farmers' knowledge about the consequences of not repaying a loan, how insurance payouts for the products contained in the experiment were triggered, and the basic procedures of the experiment. We also include two variables collected outside the experiment using detailed household surveys: wealth and the number of peers in their agricultural information networks.[6] In this setup, we would expect the probability of being risk-rationed in the baseline rounds to be positively correlated with the choice of a safe project. We also expect the comprehension index and wealth to be positively correlated with the probability of choosing a risky (but profitable) project since more financially literate subjects and wealthier subjects should have a better assessment of risk-return tradeoffs and an enhanced capacity to handle risks, respectively. It is less clear *a priori* how bigger agricultural networks and greater the prior earnings should affect project choice. Finally, we control for a specific form of "hot-hand" bias in these ordered logit models by including a dummy variable for consecutive negative draws in the preceding rounds. Drawing two consecutive black chips in the two final low stakes rounds of the insurance treatment may lead subjects to mistakenly over-estimate (under-estimate) the autocorrelation in the series of "very bad" years, which may then drive them to rely on a safer (riskier) project.

Table 3 reports the coefficients of these correlates on project choice in the insurance treatment for three logit models with session fixed effects. Model 1 includes those correlates without any interaction or higher order terms; model 2 includes the squared risk aversion variable; and model 3 adds the interaction term between the risk aversion variable and the comprehension variable to specification 2. Given that the dependent variable (project choice) is ordered from a low risk (self-finance) to high risk project (uninsured loan), a positive (negative) coefficient indicates a higher (lower) probability of choosing a risky project.

In model 1, the coefficient estimates indicate that being risk-rationed (i.e., choosing the self-finance project in the baseline rounds) is negatively correlated with the probability of choosing less risky projects. Contrary to our priors, drawing very low covariate shocks (i.e., black chips) in the last two low stakes rounds in the insurance

[5] We assumed Constant Relative Risk Aversion (CRRA) preferences under expected utility to estimate these risk aversion coefficients as the curvature parameter, which we further assumed to be a function of age, education, and gender. On average, subjects exhibit moderate risk aversion—the estimated CRRA coefficient is 0.45 (Galarza 2009).

[6] An agricultural information network is defined as the number of subjects in a given valley with whom a person shares information about farming activities.

Table 3. Ordered Logit for Project Choice as Function of Individual Characteristics with Ordering Given by Project Riskiness (i.e., low risk = self-finance, high risk = uninsured loan)

Model	1	2	3
Probability of being risk rationed in baseline {0,1}	-1.373***	-1.352***	-1.391***
	(0.406)	(0.406)	(0.408)
Earnings in low stakes insurance treatment (Soles)	0.118	0.102	0.070
	(0.193)	(0.191)	(0.187)
"Very low" covariate shocks in last two previous rounds {0,1}[a]	1.569***	1.669***	1.682***
	(0.603)	(0.562)	(0.542)
Comprehension index [b]	-0.075	-0.127	-1879
	(0.754)	(0.755)	(1.446)
Risk aversion estimate	0.599	-1.255	-4.353[‡]
	(0.558)	(1.306)	(2.689)
Risk aversion estimate squared		3.398*	4.297**
		(1.750)	(1.855)
Risk aversion estimate × comprehension index			4.242
			(2.975)
Household wealth (10,000 Soles)	-0.049	-0.039	-0.032
	(0.047)	(0.048)	(0.049)
Number of peers in agricultural network [c]	0.140	0.152[†]	0.159*
	(0.094)	(0.095)	(0.096)
Session fixed effects	Yes	Yes	Yes
Pseudo-R^2	0.122	0.129	0.134
N	350	350	350

[a] This dummy variable indicates that two consecutive black chips were drawn by the subject in the last two low stakes rounds (immediately before the high stakes) in the insurance treatment.
[b] Financial literacy measures the degree of comprehension of the insurance game (insured and uninsured loans), and ranges from 0 (did not understand at all) to 1 (fully understood). The average value of this variable is 0.54.
[c] An agricultural network is composed of a set of farmers who interchange information about farming activities.
[‡] *P*-value is 0.105.
[†] *P*-value is 0.108.

Notes: Clustered standard errors in parentheses are reported. ***, **, and * denote significance at the 1 percent, 5 percent, and 10 percent levels, respectively. Regressions are weighted by the inverse of the variance of the risk aversion estimate.

game increases the probability of choosing risky projects, but this is also a very low probability (1/100) event. The other control variables—prior earnings, agricultural information network, wealth, risk aversion, and the comprehension index—do not statistically affect project choice in these specifications. In model 2, we see that the agricultural information network variable becomes marginally significant: belonging to a bigger network appears to increase the propensity to opt for risky projects. Although the coefficient on the risk aversion variable alone does not have a significant effect on project choice, its quadratic term is significantly correlated with a higher probability of choosing a risky project. The joint effect of these risk aversion variables is statistically significant at the 5 percent level. A clearer picture of the relationship between risk aversion and project choice can be seen in model 3. In particular, we find that risk-averse subjects are more likely to choose a safe project. Together these results suggest that choices in the baseline rounds are correlated with those made in the insurance treatment, that static risk preferences are good predictors of project choice, and that judgment biases may also affect project choice.

Kenya: Index-Based Livestock Insurance

The Kenyan experiment—conducted in the Marsabit district of northern Kenya—was built around an index-based livestock (IBL) insurance product that uses NDVI data from satellite imagery as the basis of the index. One of its primary objectives was to help participants to learn how IBL insurance works and how it might benefit them in practice. Since potential insurance benefits are especially important in settings such as this that are characterized by nonlinear asset dynamics (Barrett et al. 2006, Lybbert et al. 2004), the experiment was designed to build subjects' comprehension of these potential dynamic benefits to IBL insurance. Thus, this experiment, like the Moroccan experiment, used a dynamic structure created by nonlinear asset dynamics that produce a poverty trap. Subjects decided whether or not to insure their herd in the presence of these dynamic forces and were able to see the impact of these decisions over repeated rounds. We conducted these experiments in five locations with a total of 207 pastoralists.

Experiment Design

Northern Kenya is characterized by bimodal rainfall, with two rainy seasons interrupted by two dry seasons of roughly equal length. The game was structured so that each round represented a rainy-season–dry-season pair. We played the game for ten rounds, equivalent to five years. Each player drew a random starting period herd size of six, eight, or ten head of cattle. To reflect the mixed herd of small and large livestock that are common in this region, they were told that ten goats and sheep were the equivalent of one head of cattle (or one tropical livestock unit, TLU). At the opening of each round, participants had to pay five "goats or sheep" (0.5 TLU) to feed their family. As explained below, this simple fixed consumption requirement creates a bifurcation in expected herd growth that produces nonlinear herd dynamics.

We explained that herd growth depended on two sources of luck. First was the idiosyncratic luck specific to each individual. Second was the covariate climate luck that affected everyone in the community. Each player first selected his or her idiosyncratic luck by drawing from a bag containing three bottle caps representing good, average, and bad luck (+10 percent, 0, or -10 percent adjustments to the covariate herd growth, respectively). The distribution of covariate herd growth was set to mirror the distribution of average herd growth rates observed over time in the area. Participants observed a ping pong ball drawn by one of them from a bag representing the covariate climate luck for the community. There were 16 balls in the bag that together implied an expected gross growth rate of 7.5 percent.[7] The fixed consumption constraint of 0.5 TLU per period, however, creates a bifurcated net growth rate: expected herd growth including this consumption constraint is negative up to a herd size of 6.6 TLU and positive above this threshold. Following the ball drawing, enumerators at the table went to work with calculators quickly figuring out the resulting herd growth and moving to the next season, which again opened with collection of the 0.5 TLU consumption requirement.

We played this game four times. In the first learning game, there was no idiosyncratic risk and each table of approximately four players had one herd to follow. This quickly illustrated the basic game dynamics and the meaning of the chips representing livestock wealth and balls representing the climate outcome. After learning the game from the first set of rounds, everyone then played the game a second time with their own herd with idiosyncratic risk, but no insurance. The third game recreated the idiosyncratic and covariate shocks from game two and introduced index insurance based on remotely sensed data on forage conditions. We explained climate as a function of rainfall and forage conditions and explained that these forage conditions were observed and reported by satellites. Satellites were explained as "the moving stars in the night sky," which participants had frequently seen. In this game, in order to buy insurance against covariate shocks, a player had to sell goats in order to pay the round-specific insurance premium of 1 percent of insured asset value (for example, 0.01 TLU insures 1 TLU) with a 10 percent strike rate, meaning that the insurance covered losses only in excess of 10 percent. Insurance payout in the event of -20 percent covariate growth rate (drought) thus returned a payment of $(20\% - 10\%) \times$ TLU in-

[7] There were five +20 percent growth rate balls representing a very good year. There were seven +10 percent growth rate balls reflecting a good year. Two balls were zero growth balls, reflecting a bad year, but not a drought. Then there were two final balls representing droughts of different magnitudes: one each of -20 percent and -30 percent growth rates.

sured = 0.1 TLU for each TLU insured, while a -30 percent drought returned twice as much.

After comparing the results with insurance and without insurance with the players, we played a final set of rounds in which they decided how much to insure, from zero to their full herd size (rounded down to an integer value). They were informed they would receive cash payouts based on their performance in this round of the game based on a randomly selected binding round in the game. After conducting these final rounds, we ended with a debriefing in which we discussed the results and how this game related to the actual IBL insurance product that would soon be available for purchase. We then concluded the experiment and paid subjects their earnings.

Effects of the Dynamic Design

The main benefit of the dynamic specification of the game is that it allowed herd sizes to change over time, which is a major factor in the lives of the people in this area. As an extension of this benefit, the fixed per-period consumption constraint allowed this change over time to capture nonlinear herd dynamics that seemed well understood by participants—how hard it is to avoid becoming stockless once herd size falls below a critical threshold of about 7 TLU. During the game play, when herds began to diminish, people would joke with each other about climbing the trucks to head to Nairobi and seeking alternative employment. Overall, subjects readily understood the dynamic structure of the experiment because it paralleled the reality they face.

This dynamic structure allowed subjects to experience asset-dependent growth prospects in a familiar way. They sometimes expressed frustration at having to meet the subsistence requirement; one player jokingly pleaded that he would go hungry and sell only enough animals to buy food for the children so he could get by with selling only three goats instead of five. This request indicates how well they related to the dynamic forces in the experiment and highlights a critical survival strategy that is all too familiar to them: state-contingent consumption. While we did not attempt to formally involve state variables or stochastic shocks in determining consumption flows in the experiment, which were fixed each season at the subsistence 0.5 TLU level, the sense that the size of the household herd could influence consumption in this population—whose diet is disproportionately milk—is also important. Pastoralists do face the choice of reducing consumption now in order to "asset smooth" and thereby decrease the chances of facing eventual total asset loss, a behavior observed among this population (Barrett et al. 2006) that can undermine human capital through malnutrition (e.g., Hoddinott 2006).

Including this well-understood specification of herd dynamics allowed us to characterize the intertemporal impacts of asset risks and so to emphasize the intertemporal value of IBL insurance in this pastoral setting. This helped to further stimulate players to consider intertemporal costs and benefits when making insurance purchase decisions. Moreover, the bifurcating herd dynamics in the game implicitly suggested different potential of IBL insurance in altering herd and welfare dynamics conditional on a player's initial herd size. IBL insurance may be more valuable if it protects those vulnerable households with a herd size around the critical threshold from falling onto the negative herd growth path due to catastrophic covariate shock.

The sequential structure of the experiment enabled subjects to process the costs and benefits of IBL insurance by providing a direct comparison of the evolution of herd size with and without insurance. The first few columns of Table 4 summarize the comparison of "with insurance" to "without insurance," which is driven by the structure and calibration of the experiment. These results allowed us to demonstrate to participants that the main benefit of IBL insurance was that it reduced variability in herd size over time—particularly for relatively small herds near dynamic thresholds. From other research we have conducted in this area, we know that 63 percent of total income is from livestock and livestock products, the largest component (44 percent) being milk (McPeak 2004, McPeak and Doss 2006). This allowed us to stress the point that insurance that reduces variability in herd size over time enables herders to dramatically stabilize consumption over time. The final columns in Table 4 compare the mean share of the herd insured by subjects with different starting herd sizes. Based on this summary statistic, those with smaller starting herds—those at greater risk of falling below the critical 6.6 TLU threshold—insure more of their herd.

Table 4. Contrasting Outcomes With and Without Insurance in Kenya

	Mean Herd Size *With* Greater Than *Without*	Mean Variance in Herd Size Across Rounds *With* Compared to *Without*	Mean Share of Herd Insured Across Rounds	t-statistic on Test of Different Means[a] 6 – X	8 – 10
6 TLU start	+3%	-40%	81%	--	--
8 TLU start	+2%	-29%	78%	2.40	--
10 TLU start	+3%	-22%	68%	8.47	6.45

[a] This test does not assume equal variances.

More detailed analysis of insurance decisions across these final rounds reveals additional insights. Over the course of the last game played consisting of ten rounds, 49 percent of subjects insured half or more of their herds, with 12 percent insuring the full value of their herds in all ten rounds. To better understand insurance decisions made in each round, we estimate a tobit model with the fraction of the herd insured in a given round as the dependent variable (double-censored at 0 percent and 100 percent herd insured). We regressed this value on the number of the round, site dummies, starting herd size dummies, current round herd size, and the outcomes of the shock variables in the previous round. These results are shown in Table 5.

The dynamic nature of the game is reflected in these results in a variety of ways. First, the share of the herd insured increases as more rounds were played. This may reflect learning within the experiment, consistent with qualitative evidence that the experiment indeed improved herders' comprehension of insurance concepts. Second, the dynamic structure of the experiment allows a nuanced understanding of the wealth-dependent effects. On one hand, the coefficients for the initial herd size dummies illustrate that the higher the starting period herd size, the higher the share of the herd insured in subsequent rounds of the game. On the other hand, the beginning round herd size results indicate that the share of the herd insured is a decreasing function of herd size. This helps to explain the summary statistics in Table 4: subjects starting with 6 TLU insure a greater share of their herd on average not because of their starting point, but because they tend to have smaller herds across all rounds and the share insured is inversely related to herd size. Finally, subjects tend to insure more of their herd immediately after experiencing a negative shock. While this makes sense in general since the value of insurance is always clearer after getting hit with a bad shock, this response appears slightly stronger for the idiosyncratic shock even though the insurance covered only the covariate shock. In ongoing analysis, we will explore this effect further by linking the observed game play with a parallel contingent-valuation study of individuals' willingness to pay for insurance and underlying risk preferences.

Discussion

Table 6 summarizes the three field experiments we have discussed. Although these experiments have different objectives and designs, they share in common the use of repeated seasons to enable subjects to appreciate stochastic and dynamic benefits. They also share the objective of assessing subjects' valuation of risk reduction once they have learned about the nature of these benefits. The dynamic element of each experiment is quite distinct: whereas the Moroccan and Kenyan experiments explicitly incorporate asset dynamics (land and livestock, respectively), the Peruvian experiment introduces these dynamics implicitly via the default penalty. Obviously, standard experimental design tradeoffs apply so that incorporating the dynamic element required fewer other elements in the experiment. In the case of the Kenyan experiment, for example, incorporating herd dynamics made it difficult to include other choice variables. In making these tradeoffs in this case, we placed greater emphasis on illustrating how livestock insurance worked and how it could benefit pastoralists than on using the experiment

Table 5. Tobit Estimation Results for Fraction of Herd Insured in Kenya

	Coefficients	Std. Errors
Round number (2,3,…,10)	0.012***	0.0046
Dirib Gumbo dummy	0.11***	0.0378
Karare dummy	0.13***	0.0346
Kargi dummy	0.14***	0.0354
North Horr dummy	0.09**	0.0365
Start 6 TLU dummy	1.25***	0.0771
Start 8 TLU dummy	1.38***	0.0938
Start 10 TLU dummy	1.39***	0.1022
Beginning round herd size	-0.0910***	0.0139
Beginning round herd size^2	0.0020***	0.0005
Covariate shock previous round	-0.0013*	0.0009
Idiosyncratic shock previous round	-0.0028**	0.0015
R^2	0.47	
N (207 subjects × 9 rounds each)	1863	

Notes: Logologo is omitted site dummy. All three start sizes included instead of an overall constant. Estimated as double censored tobit model [45 percent (2 percent) of observations censored at 100 percent (0 percent) of herd insured]. Subject fixed effects are not included so that start sizes can be. ***, **, and * denote significance at the 5 percent, 10 percent, and 15 percent levels, respectively.

to predict their livestock management and insurance uptake behavior in naturally occurring settings. In all three experiments, subjects' risk decisions respond to these dynamic elements. Some of these responses suggest interesting and subtle connections between static risk aversion and dynamic risk responses; others suggest that the evolution of a subject's asset holdings may shape his or her decision making with respect to risk. We view these dynamically induced behavioral wrinkles as suggestive of what might be learned from experiments with carefully calibrated wealth or asset dynamics. Although standard limitations of experimental economics apply, this type of dynamic experiment seems promising as a means of refining our understanding of risk responses in settings with important underlying dynamics.

Subject comprehension is an omnipresent concern when designing and administering field experiments—particularly among poor subjects with limited formal education and literacy. There are obvious tradeoffs that must be addressed when considering dynamic experiments, which often complicate the experimental design. While comprehension issues certainly loom large, one can frequently leverage the familiar context of framed field experiments to build comprehension of the dynamic design among other things and avoid some subject confusion. In the case of the two index insurance experiments we describe, the connection between an actual insurance product and a specific context seems to improve subjects' comprehension of the experiment, as well as their appreciation for the dynamic elements of these experiments. The comprehension gains of subjects in these two index insurance experiments—though not our focus here—are encouraging. In ongoing analyses and data collection, we aim to test whether these comprehension benefits spill over into uptake of the actual insurance product—which will provide an interesting external validity test. Anecdotally, we have some basis for believing these spillovers will exist: after participating in the Kenyan experiment, several herders repeatedly contacted our local collaborators to ask when the NDVI index based livestock insurance would be available to them.

While the advantages of using context to improve comprehension are real, so too are the comprehension challenges that can remain. In all three

Table 6. Summary of the Three Experiments, Their Dynamic Element, and the Effect of this Feature

	Morocco	Peru	Kenya
Research objectives	▪ Characterize drought risk and vulnerability among rainfed cereal farmers. ▪ Understand their likely valuation of drought tolerance and no-till techniques that reduce risk.	▪ Design and introduce an area yield insurance product. ▪ Evaluate its potential benefits to cotton farmers and assess their valuation and likely uptake of the product.	▪ Design and introduce an NDVI index based livestock insurance product. ▪ Evaluate its potential benefits to pastoralists and assess their valuation and likely uptake of the product.
Experiment objective	▪ Assess valuation of risk reduction.	▪ Build subjects' comprehension of index insurance product to insure covariate risk. ▪ Assess demand for insurance product and its impact on credit uptake.	▪ Build subjects' comprehension of index insurance product to insure covariate risk with nonlinear herd dynamics. ▪ Assess demand for insurance product.
Design of experiment	▪ Framed field experiment [a] in which subjects separately value three different gambles ("seeds") with payoffs determined by a random draw ("rainfall"), then choose between the three "seeds".	▪ Framed field experiment in which subjects chose between undertaking a project with covariate and idiosyncratic risk via self-financing or an uninsured loan. Insured loan then added as a third option.	▪ Framed field experiment in which subjects experience covariate and idiosyncratic risk with and without insurance, then choose how much of herd to insure.
Dynamic element of experiment	▪ Subjects choose between the three "seeds" for seven consecutive rounds with cumulative earnings. They lose their plot for one season (gain a second plot) if their cumulative earnings are below 0Dh (above 140Dh).	▪ Subjects with uninsured loan face dynamic risk of default, which eliminated their access to credit in future rounds and depreciated the value of their land.	▪ Subjects required to consume 0.5 livestock units each round, which creates positive (negative) expected herd growth above (below) 6.6 livestock units.
Effect of dynamic element	▪ Farmers are conservative just above the 0Dh threshold and aggressive just below the 140Dh. ▪ Statically risk averse farmers are especially aggressive just below 140Dh. ▪ Farmers take greater risks with the second plot once they have it.	▪ In pre-testing, losing land as default consequence too dominant as a dynamic incentive. ▪ 57 percent of risk rationed farmers opt for insured loan when available. ▪ Statically risk averse farmers tend to stick with self-financing.	▪ Herders clearly understood the nonlinearity introduced by the consumption requirement. ▪ Mean share of herd insured higher for those starting below 6.6 threshold. ▪ Share of herd insured increases with initial herd size but decreases as herds grow, which requires linked rounds.

[a] This terminology is from Harrison and List (2004).

experiments, building subjects' comprehension of the structure of the experiment took time. The dynamic elements of these experiments added to this time investment not only because of the added complexity, but more importantly because many repeated rounds are required for subjects to experience these dynamic forces and perceive long-run patterns. To be more specific, we faced several difficulties when explaining the Peruvian insurance experiment to participants, including the notion of average valley yield and the concept of index insurance that did not protect individual shocks. Finally, since conducting a dynamic experiment often demands several calculations each round for multiple subjects under time pressure, computational mistakes among enumerators can be a serious issue. In current research, we are therefore using a computer-based platform for conducting field experiments in these settings to reduce calculation time and human errors.

We conclude with a few observations about the external validity of the dynamic elements in our experiments. Here there are important differences between the Moroccan experiment and those in Kenya and Peru. In Morocco, the experiment—including the land accumulation dynamic—is not intended to have a direct naturally occurring analogue. Instead, its within-subject design is meant to enable the comparison of stylized decision making under risk in static and dynamic environments. In Kenya and Peru, however, the dynamic elements were intended to have some connection to naturally occurring analogues. Largely as a result of subjects' familiarity with these analogues, participants seemed to quickly catch on to this connection. While there is some hope for a degree of external validity of dynamic experiments, using experiments to mimic underlying poverty dynamics in order to directly inform policy design may be impossible. For more modest objectives, however, dynamic experiments may be a promising tool both for researchers seeking to understand the behavior of the poor and for the poor seeking to understand complex products such as index insurance.

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