# Cognitive Droughts[‡]

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**ABSTRACT:** This paper tests whether uncertainty about future rainfall affects farmers' decision-making through cognitive load. Behavioral theories predict that rainfall risk could impose a psychological tax on farmers, leading to material consequences at all times and across all states of nature, even within decisions unrelated to consumption smoothing, and even when negative rainfall shocks do not materialize down the line. Using a novel technology to run lab experiments in the field, we combine recent rainfall shocks and survey experiments to test the effects of rainfall risk on farmers' cognition, and find that it decreases farmers' attention, memory and impulse control, and increases their susceptibility to a variety of behavioral biases. In theory, insurance could mitigate those effects by alleviating the material consequences of rainfall risk. To test this hypothesis, we randomly assign offers of an index insurance product, and find that it does not affect farmers' cognitive load. These results suggest that farmers' anxiety might be relatively difficult to alleviate.

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"The hunger, the moldy saline water from the shrinking reservoirs, the skeleton vegetation, the dying livestock, and the forced migration are vivid realities (…) [T]he presence of rain prophets and the many natural 'signs of rain' to which rural people attribute great significance are testimonies to the *psychological anxiety* that the threat of drought engenders."

– T. Finan (2001, p. 6; emphasis added)

# 1 Introduction

Rainfall variation is a central dimension of the lives of the poor in the developing world.[1] In fact, it is the canonical example of risk in development economics, for both its unpredictability and its substantial effects on wages, income, and consumption. While rational responses to rainfall risk have been extensively studied, its psychological consequences have been overlooked. This paper tests whether rainfall risk decreases farmer's attention, memory and impulse control, and increases their susceptibility to behavioral biases.

Rainfall risk may affect decision-making through a variety of mechanisms. In conventional economic theory, uncertainty about future rainfall affects individuals through risk aversion, potentially leading to precautionary savings, insurance take-up, or investment in risk-coping technologies, such as irrigation. In contrast, behavioral theories predict that rainfall risk could affect individuals through mechanisms other than risk aversion. Farmers in drought-prone regions such as Northeast Brazil (scenario for the opening quote) depend so fundamentally on rainfall that rainfall risk could bring about psychological costs. Such costs may take the form of cognitive load.[2, 3]

The mental bandwidth/cognitive load theory (Mullainathan and Shafir, 2013) predicts that the prospect of scarcity increases the opportunity cost of allocating mental bandwidth to tasks that do not involve scarce resources.[4] The increase in the *relative price* of setting bandwidth to decisions involving non-scarce

[1] Over half a billion people worldwide live in arid regions without access to irrigation. Strikingly, a substantial share of this population is made of farmers, and the rural poor living in fragile areas outnumber those living in favored areas by a factor of two (Barbier, 2010). In Africa only, droughts affect between 40 and 70 million people every 5 years. The economic costs of these events are high, and they raise almost one-to-one with the GDP share of agriculture (Benson and Clay, 1998).

[2] The predictions of the cognitive load theory with respect to the effects of risk might be interpreted as a specialization of the "risk as feelings" hypothesis (Loeweinstein et al, 2001); see section 3.3.

[3] Rainfall risk may also affect decision-making through anticipation or dread (Elster and Lowenstein, 1992; Caplin and Leahy, 2001) or through subjective perceptions of the probabilities of future states (the affect heuristic; Finucane et al., 2000). We do not study these mechanisms in this paper.

[4] Even though this theory is about the effects of facing low *levels*, it speculates that facing *variance* could bring about the same effects.

resources leads to an income effect (*cognitive load*): by reducing mental bandwidth available for all tasks, it may decrease attention, memory, and impulse control, and increase susceptibility to biases. On the other hand, such relative price change also leads to a substitution effect (*focus*): as setting bandwidth to decisions involving scarce resources becomes relatively cheaper, it may partially reverse the negative income effect. As a result, decision-making under uncertainty should become worse overall, but less so in what comes to decisions involving the resources at risk.

This paper tests whether rainfall risk increases farmers' cognitive load, and whether it enhances their focus on scarce resources. Such mechanism does not operate through utility, but rather through the *foundations* of decision-making, affecting choices in present and future periods, even if these choices are unrelated to consumption smoothing. Worse decisions driven by rainfall risk, in turn, generate economic consequences at *all* times and across *all* states of nature, even if negative rainfall shocks do not materialize down the line.

In order to test these hypotheses, we conduct two sets of experiments. The first set tests the effects of rainfall risk on cognitive load and focus. The second set considers the effects of insurance on the same outcomes since, by alleviating the material consequences of rainfall risk, it could mitigate its effects on cognitive function.

Within the first set of experiments, we explore two sources of variation that cause psychological anxiety linked to rainfall risk. First, we exploit natural variation from recent rainfall shocks (in the previous month). Such shocks provide farmers with signals about the rainy season and future harvest, and so should affect worries about rainfall risk. Second, we conduct survey experiments, randomly making some farmers worried – but not others – by means of making them think about the consequences of a drought in their municipality (what the cognitive psychology literature calls *priming*). Combining these two sources of variation allows us to overcome the disadvantages of each. We can benchmark the effects of priming to those of the actual shocks, while the survey experiments give us higher statistical power to detect those effects and can help us rule out alternative explanations for the effects of rainfall shocks.

Measuring the cognitive effects of rainfall risk in the field is, however, a challenging task, for two reasons. First, while rainfall shocks provide exogenous variation in worries about rainfall, this approach would only yield enough statistical power to detect the effects of interest if outcomes are tracked across a large number of locations and times. To address this challenge, we follow 2,822 farmers scattered across 47 municipalities in Ceará, Northeast Brazil, over the course of four months during the rainy season. Second, while it would be ideal to measure psychological outcomes in a controlled lab environment, achieving this ideal would be prohibitively costly. Research infrastructure is often spatially concentrated, while subjects are scattered across many locations – some of them more than 5 hours away from the state capital.

To address the latter challenge, we developed a methodology to run lab experiments in the field. We are able to reach a large pool of farmers at high frequency using phone surveys, taking advantage of the fact that most households in the state have cell phones. Cell phones are prevalent in most parts of the world; Internet and smartphone apps, however, are not. To tackle this issue, we rely on a simple but innovative technology: interactive voice response (IVR) surveys, through which farmers receive automated voice calls (computer-managed surveys narrated by a human voice), and answer to incentivized numerical and categorical questions through keystrokes on their cell phones. Running lab experiments over the phone allows us to measure the outcomes of interest, but also entails additional hurdles. Many known psychological tests used as measures of cognitive functions, such as stroop or word search, involve visual elements that must be adapted in a way suitable to be conducted over the phone. Another contribution of this paper is to develop audio versions of these tests.

Cognitive outcomes are organized in two categories, which capture the theoretical predictions about the negative and positive cognitive effects of rainfall risk. The first is cognitive load, comprising tasks aimed at assessing working memory, attention and impulse control (what cognitive psychologists call *executive functions*), and outcomes that measure subjects' sensitivity to anchoring (a cognitive bias defined as the tendency to rely on an irrelevant initial piece of information to make subsequent judgements). The second category is focus, comprising tasks involving scarce resources (water and money) – when relevant, in comparison to tasks that do not involve these resources. Such tasks include (i) relative valuation of scarce resources in simple trade-offs, (ii) performance in word search games, and (iii) sensitivity to framing in trade-offs between scarce resources and time (a cognitive bias defined as inconsistency across decisions that involve the same relative price between resources and time but that have different framings for baseline values/amounts).

We find that both actual rainfall shocks and priming increase farmers' cognitive load.[5] The loss in cognitive performance coming from rainfall risk is equivalent to over 40% of the effect of actual harvest losses on cognitive load, by the end of the rainy season, or to downgrading a farmer from high school back to elementary school (in a cross-sectional comparison). We find that priming significantly increases focus, whereas rainfall shocks have no effect on this measure, suggesting that living under endemic worries may lead the income effect to dominate the substitution effect. Priming particularly affects farmers at

[5] Since we conduct a multiplicity of tests within each category, estimating separate regressions for each outcome would substantially inflate the probability of false positives above stated significance levels. For this reason, we build summary measures for cognitive load and focus, following Kling, Liebman and Katz (2007).

intermediate quantiles of the distributions of cognitive load and focus. This pattern is consistent with the claim that those who are extremely worried with rainfall (with the highest cognitive load and focus) would already have it top of mind, while for those not worried at all (with the lowest cognitive load and focus) priming would not be enough to make it top of mind. Lastly, we offer farmers the opportunity to listen to real credit and insurance offers. We find suggestive evidence that the psychological tax imposed by rainfall risk may lead to a poverty trap: worries cause farmers to demand less credit for irrigation and crop insurance (relative to credit for consumption and funeral insurance). This keeps farmers vulnerable to rainfall risk, potentially making cognitive effects persistent.

Having documented that rainfall risk does indeed affects farmers' cognitive function, we move on to study whether insurance mitigates those effects. In theory, insurance could mitigate the effects of rainfall risk on cognitive function by alleviating its material consequences. While previous research has pointed out that insurance may provide "peace of mind" (Krantz and Kunrether, 2007), this is the first paper to test this hypothesis.

We randomize offers of an index insurance product that is typical in the developing world. This insurance alleviates the material consequences of rainfall risk, paying farmers the equivalent of their household average income in case municipal harvest losses are 70% or higher. We exploit the fact that farmers face liquidity constraints before government insurance pays out in August.[6] The insurance product we randomize pays out already in June, when marginal utility should be very high if harvest losses are that high.[7] Moreover, we assign insurance in February, when many crucial production decisions – in particular, planting area and crop choice – have already been undertaken and cannot be changed. That way, we shut down rational responses that operate through the risk-aversion mechanism. Most of the 1,192 farmers who were offered the new product took it up.[8]

Surprisingly, we find that index insurance does not systematically affect farmers' worries about rainfall or cognitive load. Why does insurance have no effects? It is not a matter of statistical power: calculations indicate that we would be able to detect effects of insurance of similar magnitude to those of the survey experiments. Most importantly, point estimates of its effects on worries are actually positive (and, hence, not a matter of precision), and its effect on cognitive load is a very precise zero. The null result is also not due to heterogeneity: we do not find systematically different effects of insurance for subsets of participants presumably more worried about rainfall – those without irrigation, those otherwise uninsured, those living

[6] From monthly face-to-face interviews, about 80% of the sample has no credit intake over the course of this period, and less than 50% has a checking account.
[7] About 30% of the sample faced municipal harvest losses of 70% or higher in 2015.
[8] Take up rate was 76.5%. The main reason provided for rejecting the offer was the concern that taking up this product would make farmers ineligible for government insurance, even though the script was explicit about this not being the case. See Appendix C for the call center scripts.

in the most drought-prone regions of each municipality, and those facing negative rainfall shocks or randomly primed about droughts.

These results suggest that farmers' anxiety might be relatively difficult to alleviate. One possibility is that shortcomings of index insurance, such as distrust and basis risk (Dercon, Gunning and Zeitlin, 2015; Clarke, 2011), may preclude it from reducing farmers' worries, and even make them worry even more. Index insurance is very prevalent in the developing world, mainly because other forms of insurance involve very high verification costs. Some of its features – from the structure of payouts to the choice of the index – may affect not only insurance take-up, but also its effectiveness. Insurance products that pay out infrequently, that are based on indices computed over long periods, or that are too complex to understand (such as yield insurance, based on random audits to a sample of plots in each municipality) may not help mitigate worries about droughts and their detrimental effects on farmers' decision-making.

The remainder of this paper is organized as follows. Section 2 describes the setting, the timeline and the lab-in-the-field technology we rely upon. Section 3 describes our empirical strategy: the design of the experiments, the conceptual framework for the psychological effects of rainfall risk and our main hypotheses, the outcomes we track, and our procedure for dealing with multiple testing. Section 4 presents the results for the effects of recent rainfall shocks and priming on cognitive load and focus. Section 5 describes the insurance policy, its assignment, and take-up, and presents the results for the effects of insurance on cognitive load and focus. Section 6 concludes the paper with a discussion of the implications of our results for the design of financial solutions for the poor.

## 2 Setting, timeline, and lab-in-the-field technology

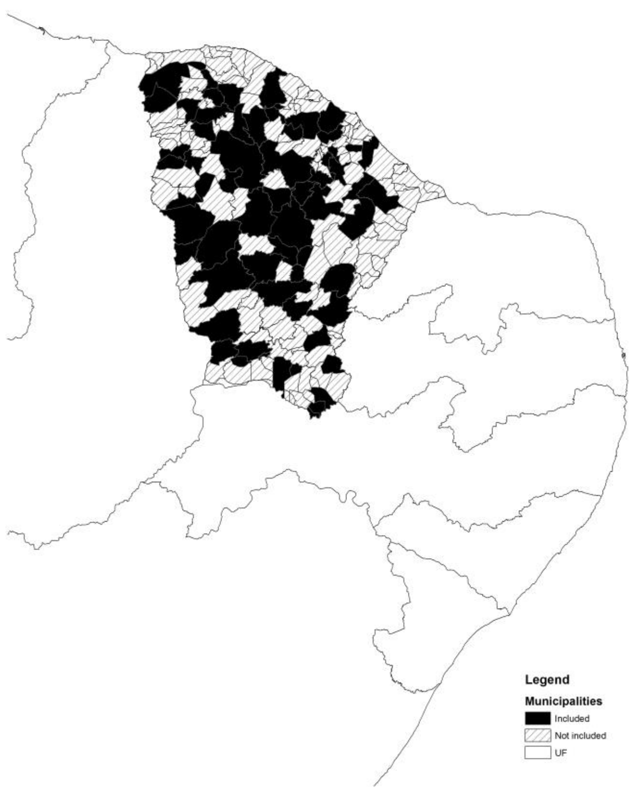
This section first presents the setting in which our experiments take place, providing some background about Ceará in subsection 2.1 and describing the enrollment process and the main characteristics of our sample in subsection 2.2. Second, it presents in subsection 2.3 a detailed timeline of enrollment, data collection and the most important milestones in terms of the rainy season, government insurance and production decisions in Ceará. Last, it provides details about the technology we used to run lab experiments in the field in subsection 2.4.

### 2.1 **The State of Ceará**

Ceará is a poor and drought-prone state in Northeast Brazil. Over 80% of its territory lies in the semiarid region, and about 60% of its municipalities were faced with below-normal rainfall levels (among the bottom 1/3 rainfall levels out of the previous 30 years) every year over the previous 4 years. In an extreme year such as 2013, all municipalities except the state capital, Fortaleza, declared emergency state in order to receive emergency funds from the federal government to support the estimated 1.8 million family farmers living in the state. Irrigation and modern agriculture techniques such as drip irrigation are rare in the state, and most farmers have to rely solely on rainfall in order to harvest anything. This setting generates a great deal of anxiety and mysticism linked to rainfall forecasts (see Taddei, 2013, for a detailed anthropolical account), making it a promising environment in which to study the psychological effects of worries about rainfall risk.

## 2.2 **Enrollment and descriptive statistics**

In partnership with Ceará's Rural Development Secretariat, we enrolled 4,084 farmers across 47 municipalities of the hinterlands of the state, over January 2015. Extension workers in each municipality received 100 consent forms to be handed to the farmers they oversee, and through which farmers who consented could inform their mobile phone number. Within each municipality, we directed half of the forms to farmers living in the most drought-prone region in the municipality, and half for those living in the least drought-prone region. Due to the high heterogeneity in microclimate within-municipality, we use this information for stratifying treatment assignments.



Despite enrolling that many farmers, 1,262 of them never answered our surveys. We cannot tell if they did not because the phone number provided was wrong or no longer active at the time of the surveys, if the telecommunications' tower coverage in some regions is bad enough that they never have signal when we placed the calls, or if they changed their minds and were no longer interested in participating. Appendix D presents detailed analysis on attrition and balance. Table D2 displays the distribution of respondents per number of calls among those 2,822 farmers that took at least one call over the course of the 4 waves; about 50% of the sample tool, at most, 8 calls.

[Table D2]

Table D3 analyzes the marginal effect of all covariates collected at baseline on the average probability of completing any of the 24 calls over the course of the research.

[Table D3]

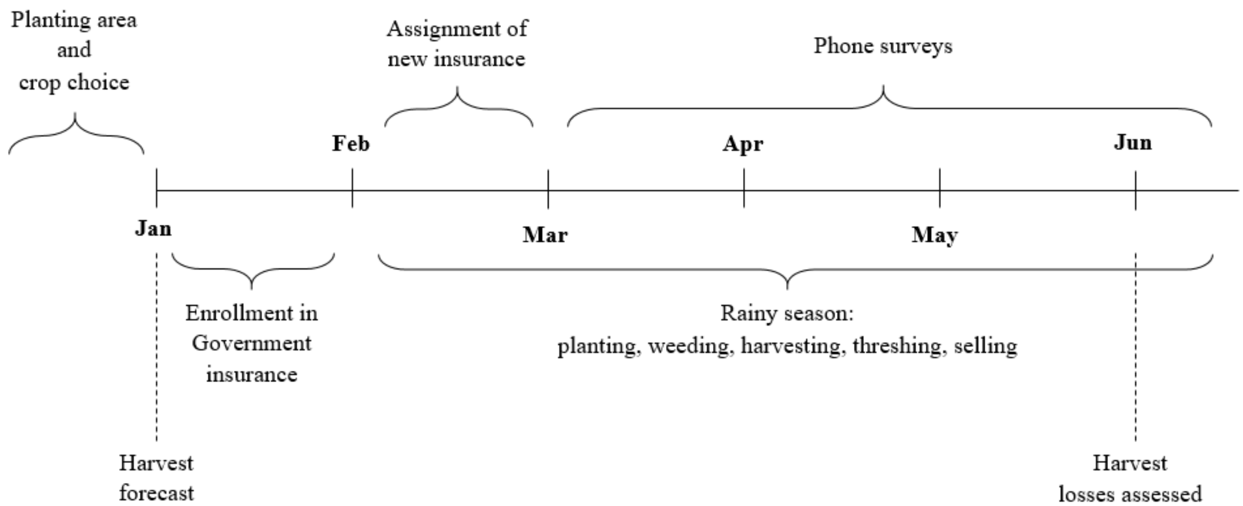
Participation seems to increase in need (higher for those living in the most drought-prone region within the municipality, those without irrigation, and those not enrolled in Brazil's flagship conditional cash transfer, *Bolsa Família*) but also in schooling. Most importantly, however, table D1 shows that the sources of variation we explore for identifying the effects of interest do not significantly affect the probability of completing calls.

Table 1 presents the descriptive statistics for our sample. Only 15.2% of the farmers in our sample have access to irrigation, and only 21.4% seed cassava, a higher-return but higher-risk crop. Most of the sample is poor: about 80% of the households live with under USD 100 a month, and the average family size in rural areas is 3.6, according to the Brazilian Institute for Geography and Statistics' 2010 Census.

[Table 1]

### 2.3 **Timeline**

The rainy season in most of Ceará spans February through May. In good years, the southern part of the state has a pre-season, in December and January, and the state as a whole has a post-season in June and July. According to the local extension workers, most productive decisions – in particular, land preparation and crop choice – are undertaken before January, in time for the pre-season. Enrollment in government insurance generally takes place until the end of January. Over the course of the rainy season, most of the margins that farmers can adjust involve labor. If rainfall allows farmers plant (mostly corn and beans), weed, harvest, thresh, and sell.




The new index insurance product was assigned over first two weeks of February. At the same time, we collected baseline information for as many farmers as we could reach over the course of this month. Data collection resumed in the first two weeks of each of the following four months.

### 2.4 **Lab-in-the-field technology**

While it would be ideal to measure psychological outcomes in a controlled lab environment, achieving this ideal would be prohibitively costly. Research infrastructure is often spatially concentrated, while subjects are scattered across many locations – some of them more than 5 hours away from the state capital.

To address this challenge, we developed a methodology to run lab experiments in the field. We are able to reach a large pool of farmers at high frequency using phone surveys, taking advantage of the fact that over 87% of households in the state have cell phones. However, while cell phones are found in most areas, the same is not ture for Internet and smartphone apps.[9] To tackle this issue, we rely on a simple but innovative technology: interactive voice response (IVR) surveys, through which farmers receive automated

[9] In Brazil, 76.5% of the active lines are pre-paid, and less than 27% of households have access to internet in Ceará.

voice calls (computer-managed surveys narrated by a human voice), and answer to numerical and categorical questions through keystrokes on their cell phones.[10]

Running lab experiments over the phone allows us to measure the outcomes of interest, but it also entails three challenges. First, while we have to measure a number of outcomes in order to estimate the effects of each treatment on both cognitive load and focus, attrition for phone surveys can be high, particularly for calls longer than 5 minutes. To deal with that issue, we divide our lab experiments into 6 calls of at most 5 minutes each, spread over the course of 2 weeks within each wave. Second, many known psychological tests used to measure cognitive functions, such as stroop or word search, involve visual elements which must be adapted in a way suitable to be conducted over the phone. To deal with that issue, we design audio versions of stroop and word search (to our knowledge, this is the first paper to perform audio versions of these tests). Third, farmers might have no interest in taking those psychological tests seriously, a possibility that could greatly limit the statistical power of the tests we undertake. To deal with that issue, we incentivize performance in cognitive tests, offering an extra top-up in airtime credit of USD 0.50 for the 25% top-performers in each wave.[11]

# 3 Empirical strategy

This section discusses the design of our experiments in subsection 3.1, and then describes the definition of rainfall shocks and priming in subsection 3.2 (a discussion of the insurance treatment is deferred until section 5). Next, subsection 3.3 further details the conceptual framework we rely upon, and is followed in subsection 3.4 by the description of the main outcomes. Last, subsection 3.5 gives details on how we estimate the effects of interest, deal with standard errors and correct for multiple testing.

## 3.1 **Design of the experiments**

The ideal experiment would independently randomize exposure to future rainfall risk on one hand, and insurance on the other. That way, we could compare farmers in high-risk plots to those in low-risk plots, as well as those with and without insurance within the subset of farmers in high-risk plots. What is crucial is that, on average, such farmers be otherwise identical, especially in regard to harvest losses. This is because we want to capture the effects of exposure to risk alone, not those of risk materialization.

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[10] All calls are reverse billed, so that farmers do not need airtime credit to respond. We also incentivize completing each call with airtime credit top-ups (about USD 0.25, equivalent to 10 SMS or 2-min in airtime).
[11] The expected hourly wage from taking all surveys is USD 3.25, about four-fold the average hourly wage reported by our sample.

Even though it is impossible to randomly assign farmers to different risk of a drought, in particular holding harvest losses constant, it is possible to randomize *worries* about droughts, in the spirit of mechanism experiments (Ludwig, Kling and Mullainathan, 2012). We approximate the ideal experiment by conducting two sets of experiments. The first set tests the effects of worries about rainfall on cognitive function, while the second considers the effects of insurance on the same outcomes.

Within the first set of experiments, we explore two sources of variation that cause psychological anxiety linked to future rainfall variation. First, we conduct survey experiments, randomly making some farmers (but not others) worries about the consequences of a drought in their municipality (a technique that the cognitive psychology literature calls *priming*). The advantage of this approach is control: the variation is randomly assigned, and precisely linked to the mechanism of interest. Its disadvantage is external validity: it is unclear to what extent we should expect findings from priming experiments to hold more generally, in particular with respect to the shocks that we actually care about.

Second, we exploit natural variation from recent rainfall shocks (in the previous month). Such shocks provide farmers with signals about the rainy season and future harvest, and so should affect worries about future rainfall. The advantage of this approach is external validity: negative rainfall shocks, compounded over the season, are exactly what characterize a drought. Its disadvantages are twofold: this source of variation is more coarse, as rainfall data varies only across municipalities and over time (whereas priming can be randomized at the individual level), and it includes variation that is unrelated to the mechanism of interest.

Combining these two sources of variation allows us to overcome the disadvantages of each. We can benchmark the effects of priming to those of the actual shocks, while the survey experiments give us higher statistical power to detect those effects and can help us rule out alternative explanations for the effects of rainfall shocks.

Our second set of experiments concerns the hypothesis about the effects of insurance. We randomize offers of an index insurance product that is typical in the developing world. This insurance alleviates the material consequences of the risk of future rainfall variation, paying farmers the equivalent of their household average income in case municipal harvest losses are 70% or higher.

| | **Offered insurance** | **Not offered** |
|---|---|---|
| **Higher worries** | Treatment 1 x Treatment 2 | Treatment 1 |
| **Baseline worries** | Treatment 2 | Control group |

The above table summarizes our empirical strategy. Our design allows us to estimate the effects of worries about future rainfall by comparing Treatment 1 to the Control group, and the effects of insurance (ITT) by comparing Treatment 2 to the Control group. The advantage of cross-randomizing the treatment arms is that we can also compare Treatment 1 x Treatment 2 to Treatment 1, in order to both better understand how the two treatments interplay and to pin down the underlying mechanisms.

### 3.2 **Rainfall shocks and priming**

We use two measures of recent negative rainfall shocks, both drawing on Ceará's official monthly rainfall data, collected by local meteorological stations for each municipality over the past 30 years.[12] The first measure is an indicator variable, equal to 1 if the municipality faced a below-normal rainfall shock in the previous month (i.e., if the month is amongst the 30% worst in municipality's 30-year distribution), and 0 otherwise. The second is a continuous variable, equal to the difference between municipality's average rainfall level over the past 30 years and the previous month's rainfall level.

Taking advantage of the IVR technology, we prime subjects at the beginning of each survey. Upon consenting to take a call, each farmer is randomly assigned to answer a question, either about droughts or about soap operas. The idea is that soap operas are interesting enough that people do not hang up, but that they should not make one systematically worry about rainfall. Other than the theme, questions have the exact same structure for the treatment and control groups, and we vary positive and negative framings across surveys in order to avoid systematically inducing a particular emotional state in the control group (Lerner et al., 2014).

### 3.3 **Conceptual framework**

Psychological theories consider a variety of mechanisms other than risk aversion for how rainfall may affect decision-making. In order to be precise about how different theories deviate from the rational-choice benchmark, we resort to a simple framework, as follows.

A risk-averse farmer chooses how much to consume at present ($x$) out of her present wealth ($w$), how much to consume in the future if the yield is high ($c_{+1}^{H}$) out of her wealth in this scenario ($w_{+1}^{H}$, what happens with exogenous probability $p \in [0,1]$), and how much to consume if the future yield is low ($c_{+1}^{L}$) out of her wealth in this scenario ($w_{+1}^{L}$), in order to maximize lifetime expected utility, discounting future payoffs

[12] When there is more than one meteorological station within a municipality, the state also reports the average rainfall level for the municipality as a whole. Since we do not have the GPS location of the farmers in our sample, not even for the least and most drought-prone regions within each municipality, we cannot explore information at finer aggregation levels.

with factor $\beta \in [0,1]$. Besides consumption, the farmer may choose to invest in a risk-coping technology such as insurance or irrigation ($y$), which affects the wealth distribution across future states by trading-off present consumption with future income. For simplicity, we assume there is no savings technology available.[13] Farmers' problem is summarized by equation (1):

$$(x^*, y^*, c^*) = \underset{x,y,\{c_{+1}\}}{\operatorname{argmax}}\ u(x, w-y) + \beta\left[pu\left(c_{+1}^H, w_{+1}^H(y)\right) + (1-p)u\left(c_{+1}^L, w_{+1}^L(y)\right)\right] \quad (1)$$

While there are many models for how decision-making might deviate from the rational-choice benchmark (e.g.: prospect theory), we restrict attention to deviations that are fundamentally connected to role of risk. One such mechanism is anticipation and dread (Elster and Lowenstein, 1992), formalized by Caplin and Leahy (2001). According to this theory, an anxiety parameter directly enters the utility function, penalizing present consumption experiences – on top of expected utility – from exposure to future risk. In equation (2), utility at the current period is indexed by the parameter $a\left(w_{+1}^H(y) - w_{+1}^L(y)\right)$, which is a function of the difference in wealth levels across future states (for simplicity). This theory predicts time inconsistency: as information about risk realization unravels, anxiety disappears, and present selves would like to revise "over-cautious" past decisions.

$$(\tilde{x}, \tilde{y}, \tilde{c}) = \underset{x,y,\{c_{+1}\}}{\operatorname{argmax}}\ u\left(x, w-y\,; a\left(w_{+1}^H(y) - w_{+1}^L(y)\right)\right) + \beta\left[pu\left(c_{+1}^H, w_{+1}^H(y)\right) + (1-p)u\left(c_{+1}^L, w_{+1}^L(y)\right)\right] \quad (2)$$

An alternative mechanism is the affect heuristic (Finucane et. al, 2000). According to this theory, feelings would mediate how individuals perceive the probability distribution of future states and the outcomes of such lottery. For instance, a previous negative experience might lead the individual to perceive that probability of the bad state as higher than it actually is. Related to this mechanism, there is a literature on the effects of trauma (Callen et al., 2014; Malmendier and Nagel, 2011) which links the effects of past shocks to those of future risk through emotional states (Lerner et al., 2014). Equation (3) captures this hypothesis in very simple form, simply stating that the decision maker considers a subjective probability

[13] This is without loss of generality, as savings could be subsumed by $y$.

instated of the objective one in her expected utility maximization problem, which is a function of the actual probability but also of the difference in wealth levels across future states (for simplicity).

$$(\widetilde{x}\ ,\widetilde{y}\ ,\widetilde{c}\ ) = \underset{x,y,\{c_{+1}\}}{\operatorname{argmax}}\ u(x, w-y) + \beta\left[\hat{p}\ u\left(c_{+1}^{H}, w_{+1}^{H}(y)\right) + (1-\hat{p})u\left(c_{+1}^{L}, w_{+1}^{L}(y)\right)\right] \quad (3)$$

where, for simplicity, $\hat{p} = f\left(p, w_{+1}^{H}(y) - w_{+1}^{L}(y)\right)$.

Yet another mechanism is "risk as feelings" (Loewenstein et al., 2001), which posits that exposure to risk may lead individuals to deviate from the maximization problem entirely, with decision-making dominated by the emotional states elicited by the presence of risk. An interesting prediction from this model is that risk could deteriorate the quality of *all decisions,* not only those related to consumption smoothing. This mechanism is summarized in equation (4):

$$(\widetilde{x}\ ,\widetilde{y}\ ,\widetilde{c}\ ) = F\left(p, w_{+1}^{H}(y) - w_{+1}^{L}(y)\right) \quad (4)$$

where, for simplicity, $F\left(p, w_{+1}^{H}(y) - w_{+1}^{L}(y)\right)$ is not an *argmax*, but rather a rule linking the probability distribution of future states and the difference in wealth levels across those states to choices for current and future consumption and for the risk-coping technology.

Related to this mechanism, the stress and negative affect hypothesis (Haushofer and Fehr, 2014) predicts that (exposure to future) shocks induce higher cortisol levels and anxiety, diverting attention from goals to habitual behavior, and increasing the influence of external stimulus (Eysenck et al., 2007). Even if through a different mechanism, most predictions from this model also operate through risk aversion.

The final mechanism we discuss is the cognitive load/bandwidth theory (Mullainathan and Shafir, 2013), which posits that individuals worrying about (future) scarcity suffer consequences of two sorts. First, a negative effect: worries act as a distraction or as cognitive load. This effect predicts lower attention and memory, and increased susceptibility to biases. Second, a positive effect: by making scarce resources top of mind, worries enhance focus. This effect predicts better performance in tasks involving scarce resources, and lower susceptibility to biases in trade-offs involving those resources.

While this mechanism if essentially cognitive (while Loewenstein et al., 2001, emphasize the non-cognitive effects of risk), it can be represented as a specialization of the "risk as feelings" hypothesis. Formally, it can be summarized by equation (4), where $F\left(p, w_{+1}^{H}(y) - w_{+1}^{L}(y)\right)$ is not an *argmax,* with the additional condition stated in equation (5): decisions that involve the resources at risk should be closer to the rational-choice benchmark than other decisions (the *enhanced focus* effect).

$$\|\tilde{y} - y^*\| \leq \|\tilde{x} - x^*\| \tag{5}$$

In this paper, we restrict attention to the mental bandwidth/cognitive load theory for two main reasons. First, in the same vein of the "risk as feelings" hypothesis, it predicts that risk could deteriorate the quality of *all decisions*, not only those related to consumption smoothing. Second, it provides a much sharper test of the mechanism given its prediction for focus enhancement.

With that in mind, our first hypothesis is that worries about future rainfall worsen cognitive function. Distinguishing between the positive and negative effects of worries, we expect a negative effect on cognitive load, and a positive effect on focus (in particular, lower sensitivity to framing in trade-offs between scarce resources and time; Shah, Shafir, and Mullainathan, 2015). Our second hypothesis is that, to the extent that insurance alleviates the material consequences of future rainfall variation, it should mitigate its effects on cognitive function.

### 3.4 **Outcomes**

Cognitive outcomes are organized into two categories, which capture the negative and positive effects of worries about rainfall that we previously outlined.[14] The first is cognitive load, comprising tasks aimed at assessing working memory, attention and impulse control (executive functions; Diamond, 2013), and outcomes that measure subjects' sensitivity to anchoring (a cognitive bias defined as the tendency to rely on an irrelevant initial piece of information to make subsequent judgements; Kahneman, 2011).

The motivation for looking at executive functions is that those are the foundations of decision-making; effects on attention, memory and impulse control should be pervasive across the different domains of farmers' choices. The motivation for looking at anchoring is that this bias is supposed to be prevalent at the time farmers are making production decisions, trying to anticipate future prices with past prices as reference; in fact, our pilot study has documented systematic evidence of anchoring in this setting.

We measure working memory through digit span tests, in which subjects must remember as many digits as they can from the numbers they hear (the more digits accurately recalled, the higher the score). We measure attention and impulse control through stroop tests, in which subjects must answer the number of

[14] We have pre-registered the study at AEA RCT Registry. Even though we did not list the regressions we would run, we specified the outcomes' categories and outlined how we would look at the effects of priming experiments and insurance on both cognitive load and focus.

times they heard a particular digit repeated in a sequence. While it is tempting to press the digit that he or she just heard repeated multiple times, the correct answer is never the digit itself.

For sensitivity to anchoring, subjects are initially primed with a high number (the price per kg of a live goat in the previous year, which was R$ 4), and are then asked to choose a price band for another price (either the future price of beans in their municipality, or the price of a subway ticket at a different state). We define anchoring as the tendency to choose higher price bands.[15]

The second category is focus, comprising tasks involving scarce resources (water and money) – when relevant, in comparison to tasks that do not involve these resources. Such tasks include (i) relative valuation of scarce resources in simple trade-offs, (ii) performance in word search games, and (iii) sensitivity to framing in trade-offs between scarce resources and time (a cognitive bias defined as decisions being influenced by whether monetary values or water amounts are presented as high or low; following and expanding on Shah, Shafir and Mullainathan, 2015).

In principle, worse performance in psychological tests could accrue entirely to factors as stress or undernutrition (in the case of negative rainfall shocks). Focus has the potential to help us understand whether the cognitive function mechanism is at play. If susceptibility to biases changes differentially for tasks and decisions "inside the scarcity tunnel", that would provide evidence that at least part of the effects are driven by the psychology of scarcity, through reallocation of mental bandwidth.[16]

We measure tunneling (Mullainathan and Shafir, 2013) through the relative valuation of the scarce resources in simple trade-offs – between money and cashews, or between water and cashews – relative to the valuation of a non-scarce resource in the same trade-off – between oranges and cashews. Tunneling is defined as the tendency to report higher rates of substitution (offering less money or water in exchange for cashews than what one offered in oranges in exchange for the same cashews). Another way we measure tunneling is through word search games, in which subjects must correctly identify whether or not they heard specific words in a sequence of words narrated with audio distortion. Scores compare subjects' performances in instances involving resources (*money* or *water*) to those involving neutral words (*husband* or *brother*). The higher the differential performance within subject, the higher our measure of tunneling.

For sensitivity to framing, we use subjects' answers in trade-offs between resources and time as building blocks. These trade-offs address decisions between buying an item at the baseline price, or purchasing it at a discount price at a store located 40 minutes away, and between getting a baseline quantity of water gallons

[15] Price bands were: "below R$ 3.40", "between R$ 3.40 and 3.80", "between R$ 3.80 and 4.20 " and "above R$ 4.20" (see Appendix A).

[16] For instance, Shah, Shafir and Mullainathan (2015) document that worries with scarcity (induce through priming) lead to *lower* sensitivity to framing in decisions involving the scarce resource.

from a water truck at the current location, or getting an extra gallon at a different truck located 1 hour away. Such trade-offs are presented under different scenarios for the baseline price or quantity of water (high or low). We define sensitivity to framing as disagreement between subject's decisions to go to the different location, in each case, when the baseline value/quantity is high relative to when it is low. When a subject decides to buy the good at the current location regardless of the baseline value, or to go to the other location for water regardless of the baseline quantity, then there is no framing effect. Conversely, when subjects decide differently conditionally on baseline value/quantity, then there is a framing effect. The analysis of this variable is restricted to subjects that (i) answered both questions that offered these trade-offs, which were spread across different calls within each wave of the survey; and (ii) were equally primed (or not primed) in both calls. For this reason, we have less observations in this case.

### 3.5 **Estimation and summary measures**

For each outcome, we estimate the empirical counterparts of $\beta_j$ in equations (6), (7), and (8), where each outcome $Y^j$ is indexed by municipality $m$, individual $i$ and survey $t$:

$$\begin{cases} Y^j_{mit} = \alpha + \theta_m + \theta_t + \beta^1_j Rainfall_{m,t-1} + u_{mit} & (6) \\ Y^j_{mit} = \alpha + \theta_{mt} + \beta^2_j Priming_{mit} + u_{mit} & (7) \\ Y^j_{mit} = \alpha + \theta_{mt} + \beta^3_j ITT_{mi} + u_{mit} & (8) \end{cases}$$

In equations (6) to (8), $\alpha$ is a constant term; $\theta_m$, $\theta_t$ and $\theta_{mt}$ are municipality fixed-effects, survey fixed-effects, and municipality-survey fixed-effects respectively; $Rainfall_{m,t-1}$ is either measure of rainfall shock in the month immediately before the survey; $Priming_{mit}$ equals 1 if individual $i$ was primed at survey $t$, and 0 otherwise; $ITT_{mi}$ equals 1 if individual $i$ was offered insurance, and 0 otherwise; and $u_{mit}$ is an error term. We cluster standard errors at the municipality level in equation (6), and at the individual level in equations (7) and (8), in order to account for potential serial correlation in residuals.

Some remarks are in order. First, due to Ceará's high microclimate heterogeneity, in practice we define $\theta_m$ as municipality-region fixed-effects (and $\theta_{mt}$ as municipality-region-survey fixed-effects). As such, we explore variation within individuals living in the least/most drought-prone in each municipality. Second, equation (7) could include individual fixed-effects or even individual-survey fixed-effects; however, our panel is very unbalanced, such that many subjects do not respond to the same call at different waves (see

Table D2 in Appendix D). In the Supplementary Appendix we show that including individual fixed-effects basically does not affect point estimates, but substantially decreases the precision of estimated coefficients.

Since we conduct a multiplicity of tests within each category, estimating separate regressions for each outcome would substantially inflate the probability of false positives above stated significance levels. For this reason, we build summary measures for each set of outcomes and for cognitive load, following Kling, Liebman and Katz (2007). To do that, first we normalize all outcomes to z-scores. Second, following Kling and Liebman (2004), we run seemingly unrelated regressions (SUR) to compute an effect size $\hat{\beta}$ for each summary measure, given by equation (9):

$$\hat{\beta} = \frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}} \tag{9}$$

In equation (9), $\hat{\beta}_j$ are the point estimates obtained for ordinary least squares (OLS) regressions of $Y^j$ on a particular treatment variable, $\hat{\sigma}_{j_c}$ is the variance of that outcome for the control group, and $K$ is the number of outcomes in that category. We use bootstrapping to obtain standard errors for $\hat{\beta}$.

# 4 Does rainfall risk increase farmers' cognitive load?

This section considers the hypothesis about the effects of rainfall risk on farmers' cognitive function and decision-making. We start by analyzing the effects of actual harvest losses on cognitive load in subsection 4.1, using them to motivate the analysis of our survey experiments and natural experiments on the following subsections. Next, subsection 4.2 discusses some basic threats to identification of causal effects in our experiments. Subsection 4.3 presents the effects of these experiments on worries about rainfall, followed by their effects on cognitive load in subsection 4.4, and on focus in subsection 4.5. Next, subsection 4.6 presents the effects of these experiments on a real economic decision: the relative demand for production-related credit and insurance. Subsection 4.7 summarizes robustness checks, while subsection 4.8 discusses how our findings relate to the previous results from the cognitive load/bandwidth literature.

## 4.1 **Harvest losses**

To motivate the effects of interest, we start by considering the relationship between harvest losses and farmers' cognitive load. Harvest losses are measured by Government as the difference between estimated harvest – based on projections for planting area and yield in January (pre-season) – and actual harvest – verified in late May (post-season) through audits in randomly selected plots in each municipality. Since the January predictions account for all information available before the rainy season (including planting area and crop choices), harvest losses can be considered randomly assigned. Cognitive load, which we introduced in section 4, encompasses performance in test that executive functions – attention, memory and impulse control –, and susceptibility to anchoring. Using the average of the summary measures of the outcomes under these categories to illustrate the effect of harvest losses on cognitive load, we find the following.

[Figure 1]

Restricting attention to Figure 1's Panel 1, cognitive load increases with harvest losses by May (the last month of the rainy season, when uncertainty is resolved but before payout eligibility has been announced). Even if suggestive, there is not enough statistical power to estimate this effect as statistically significant.

What is more, we are interested in the psychological effects of exposure to risk, separate from the consequences of the materialization of risk. For those reasons, we move on to study the effects of our experiments on worries, on cognitive load and focus. Nevertheless, we can use the correlations between harvest losses by May and the measures of cognitive load and focus to benchmark the effects of the treatments on those measures to those of actual losses by the end of the rainy season.

### 4.2 **Balance and attrition**

Both below-normal rainfall shocks and rainfall deviations from municipality's historical average are plausibly randomly assigned across municipalities at any given time. In particular, previous rainfall realizations do not help predict future rainfall: neither measure displays systematic patterns of serial correlation (auto-correlation coefficients are not significant up to two lags, and there are insufficient observations to estimate higher-order lags). In our survey experiments, priming is randomly assigned at the beginning of each call, ensuring that the treatment and control groups are balanced in expectation in what comes to observable and unobservable characteristics.

One might be still concerned that treatment (either rainfall shocks or priming) interacts with other attributes to determine which individuals self-select into completing the phone surveys. Even in the case of priming, this might work by participants selectively hanging up, for instance, after being primed about droughts at the beginning of a call. If that happens selectively (either based on observed characteristics or on unobserved ones, like cognitive load), then our estimates would confound the effect of treatment with that of those attributes.

Table D1 presents the results of ordinary least squares (OLS) regressions with an indicator variable of whether or not each call was completed as dependent variable, and with each of our treatments as independent variables in columns (1) through (3). The marginal effects of facing a negative rainfall shock, or of being primed on the probability of completing a call, are not only not statistically significant, but also very small in magnitude (below 1% for both variables).[17]

[Table D1]

Table D4 displays differences in baseline covariates across municipalities that did and did not face below-normal rainfall shocks at the previous month. Out of 13 baseline covariates, the difference between treatment and control is significant for only 1, a ratio that one would expect to happen by mere chance.

[Table D4]

Table D5 displays the differences across the primed and not-primed subsamples considering all baseline covariates. Most differences are not significant, and the few that are – number of rooms and schooling – are of tiny magnitude, of about 1.5% of the average of the control group in both cases.

[Table D5]

### 4.3 **Worries about rainfall**

Worries about rainfall are measured through a survey question about the extent to which someone in the household worried about rainfall in the previous week (“not at all”, “a little”, or “a lot”; see Appendix A). We normalize this variable to a z-score and estimate ordinary least squares (OLS) regressions of worries on both measures of rainfall shocks, on priming, and on their interaction.

[17] Our previous discussion about attrition indicated that some characteristics significantly affect the average probability of completing the surveys (see Table D3). This would only matter if we were interested in heterogeneous treatment effects. If that were the case, we could re-weight observations in order test the sensitivity of the estimates to selective attrition.

Columns (1) and (2) present the effect of below-normal rainfall shocks. Column (1) includes municipality fixed-effects, while column (2) does not, yet controls flexibly (with a third-order polynomial) for municipal-level harvest losses. Those specifications explore different sources of variations (within-municipality in the first column, and across municipalities in the second), both of which should affect worries about future rainfall while holding actual losses constant. In both cases, we find that below-normal rainfall shocks increase worries by about 60% of the effect of losing access to irrigation (in a cross-sectional comparison), but the effect is not statistically significant.

[Table 2]

Along these lines, column (3) presents the effect of the continuous measure of negative rainfall shocks, with municipality fixed-effects. Its effect is also sizable but not statistically significant. For all rainfall variables, standard errors are clustered at the municipality level. With only 47 groups, we have limited statistical power to detect the effects on any particular outcome. The survey experiments do not suffer from this limitation. In fact, the effect of priming on worries, presented in column (4), is positive (about twice that of lack of irrigation) and significant at the 10% level. Most importantly, speaking to the mechanism of interest, that effect comes entirely from the subsample that faced negative rainfall shocks in the previous month: column (5) presents the result for the interaction of priming with below-normal rainfall shocks, a very sizable and statistically significant effect (at the 1% level).

### 4.4 **Cognitive load**

Moving on to analyze the effects of worries on cognitive load, we present the results for the effect sizes of rainfall shocks and priming on this summary measure in Table 3. Columns (1) through (4) present the effects of within- and across-municipality variation in rainfall, considering the two measures of recent rainfall shocks. In 3 out of 4 cases the coefficient is statistically significant at the 10% level and, most importantly, effect sizes across different sources of variation are strikingly similar. The loss in cognitive performance coming from higher worries with future rainfall is massive: tantamount to that which would arise from moving a farmer from high school back to elementary school.

[Table 3]

The effect of priming, presented in column (5), is also negative and statistically significant (at the 1% level, since the survey experiments have higher statistical power to detect the effects of interest), remarkably similar to those of recent rainfall shocks.

To shed further light on this psychological mechanism, we examine how the effects of priming vary with the distribution of cognitive load using quantile regressions. For these regressions, we average all z-scores within the summary measure.[18] One difficulty is that we cannot include fixed-effects even with generalized quantile regressions, since we have a large number of municipalities and few time periods (Powell, 2013). However, because of stratified randomization, including municipality-survey fixed-effects increases precision but mostly does not affect point estimates. For this reason, we run quantile regressions without including fixed-effects. Figure 2 presents quantile regression estimates with confidence intervals for quantiles 0.1 to 0.9.

[Figure 2]

We find that the highest effects are concentrated in the middle of the distribution. This is consistent with the claim that those who are too worried with rainfall (with the highest cognitive load) would already have it top of mind, while for those not worried at all (with the lowest cognitive load), priming would not be enough to make it top of mind.

Last, Figures 6 through 8 present the components of the cognitive load summary measures. Together, they show that the results for indices are consistent with negative results for both executive functions and sensitivity to anchoring.

[Figure 5]

[Figure 6]

[Figure 7]

[18] Doing so has little effect on point estimates from the SUR estimation. See Supplementary Appendix.

## 4.5 **Focus**

Table 4 presents the results for the effect sizes of rainfall shocks and priming on the summary measure of focus. Once again, columns (1) through (4) present the effects of within- and across-municipality variation in rainfall, considering the two measure of recent rainfall shocks. In all cases the coefficient is not statistically significant. For priming, however, the effect size is positive, sizable (about 5 times that of being relocated to municipality's most drought-prone region) and statistically significant at the 10% level.

[Table 4]

One way to reconcile these different effects is to think of worries as changing the relative price of allocating mental bandwidth to tasks involving non-scarce resources. As worries increase, there is both a substitution effect (focus enhancement) and an income effect (cognitive load). For extreme shocks, the income effect dominates. Another possibility is that negative rainfall shocks affect other margins, like nutrition, which work in the direction of worsening performance in cognitive functions across all tasks.

The effects of the different shocks on the components of the summary measure of focus, illustrated in Figures 6 through 8, might be consistent with the overlay of income and substitution effects discussed in the previous paragraph, particularly in what comes to sensitivity to framing.

Last, we also run quantile regressions (without fixed-effects) to examine how the effects of priming vary with the distribution of focus. Figure 3 presents quantile regression estimates with confidence intervals for quantiles 0.1 to 0.9.

[Figure 3]

Once again, we find that the highest effects are concentrated in the middle of the distribution. Together with the results for cognitive load, the focus enhancement effect of priming provides further evidence that the effects of worries about future rainfall operate through the cognitive load/bandwidth mechanism.

## 4.6 **Economic decisions**

Finally, we document that this mechanism translates into economic decisions. While cognitive function lies at the foundation of every decision (Burks et al., 2009; Dohmen et al., 2010; Benjamin et al., 2013), it is challenging to link it explicitly to decision-making outside the lab. To explore the sources of variation that we combine, we must be able to track farmers' decisions that are possibly influenced by our survey experiments; the effects of priming, however, are short-lived.[19] To overcome this challenge, we include real economic decisions *directly* in our phone surveys, by giving farmers the opportunity to listen to real credit and insurance offers. Farmers do not have to pay to listen to an offer, but choosing to do so makes the call take about 30 seconds longer. We alternate across waves whether offers relate to production (credit for irrigation and crop insurance) or to consumption (credit for consumption and funeral insurance). [20]

We track farmers' decisions of whether or not to listen to those offers, and define *relative demand* as the differential demand for listening to a production-related offer relative to that for listening to a consumption-related one. That variable equals 0 if the subject listened to both or to neither production- and consumption-related offers of credit/insurance; 1 if he/she listened to the production-related, but not to the consumption-related one; and -1 if the other way around. Hence, the analysis of this variable is restricted to subjects that (i) took both calls that contained these offers, which were spread across different waves of the survey; and (ii) were equally primed (or not primed) in both calls. For this reason, we have substantially less observations in this case.

Before we come to the results, there are three issues about the analysis worth highlighting. First, we analyze the demand for credit/insurance linked to production *relatively* to that linked to consumption for the following reason: even if, for instance, priming or rainfall shocks increased the likelihood of listening to an offer about credit for irrigation, farmers have a fixed budget set. Hence, what matters is the *budget share* allocated to production-related credit and insurance. The relative demand measure aims to capture this concept.

Second, since there are many other goods and services for which budget shares may be affected by worrying about rainfall risk, we restrict attention to how demand changes within two well-defined financial products, which can be tied to either consumption or production, in order to illustrate the mechanism through which these effects may play out.

Third, the particular choices we make for production-related credit and insurance – credit irrigation and crop insurance – are no accident. Even if, from the farmers' perspective, it may be optimal to demand relatively more credit for present consumption than for irrigation (the same rationale applies to insurance), it still means that this decision has the potential to make cognitive effects persistent, by keeping farmers vulnerable to rainfall risk.

[19] See Supplementary Appendix.

[20] We alternate content such that there is new information available every wave. All information conveyed through these offers is real; see Appendix A for the full script of the credit and insurance offers.

Table 5 presents the results for ordinary least squares (OLS) regressions of relative demand as dependent variable. All exogenous sources of variation in worries negatively affect the demand for production-related credit and insurance, relative to consumption-related offers. The effect of below-normal rainfall shocks is statistically significant at the 5% level.

[Table 5]

The fact that both negative rainfall shocks and priming decrease the demand for these products (relative to credit for consumption and funeral insurance) suggests how the psychological tax imposed by worries might lead to a poverty trap. Rainfall risk not only increases farmers' cognitive load, but may also decrease the demand for irrigation and crop insurance, keeping farmers vulnerable to uncertainty and making cognitive effects persistent.

### 4.7 **Robustness checks**

This subsection summarizes the robustness checks presented in the Supplementary Appendix. First, one might worry that including individual-fixed effects might change the effects of priming on worries, cognitive load and focus, or decision-making. Because we have a very unbalanced panel (see Appendix D), we would lose many observations from exploiting within-individual variation in outcomes only; for this reason, we do not include individual-fixed effects in the main paper. In principle, doing so should not affect our results: survey experiments rely on randomization, such that fixed-effects should only increase the precision of the estimates if the panel data were balanced. The Supplementary Appendix shows that including individual fixed-effects has little effect on point estimates, although effects are much less precisely estimated, with substantially less observations.

Second, one might worry that controlling for baseline covariates might change the effects of negative rainfall shocks or priming on worries, cognitive load and focus, or decision-making. Since covariates are balanced across treatment and control groups for both rainfall shocks and survey experiments, controlling for covariates should only increase the precision of our estimates. However, because we do not have information on baseline controls for all subjects, we end up having lower precision due to somewhat smaller sample sizes. The Supplementary Appendix shows results, first without controls but restricting the sample to the subset of individuals for which we have data on baseline covariates, and then including those variables in the regressions. We find that estimates are not affected by controlling for baseline covariates.

Last, we analyze the robustness of our results for sensitivity to framing. One might wonder to what extent (the risk of) scarcity only decreases sensitivity to framing in decisions of the sort that Shah, Shafir and Mullainathan (2015) entertain, which we adapt for trade-offs between money or water, on one hand, and time on the other; or if it extends to all decisions involving resources more broadly. In particular, it would be interesting to know if it extends to time preferences, since the consequences of time-inconsistency among the poor is an active topic in economics (e.g.: Gruber and Köszegi, 2004; Ashraf, Karlan and Yin, 2006; Schilbach, 2015).

To measure time inconsistency, we use subjects' responses in intertemporal trade-offs as building blocks. Such trade-offs present decisions between receiving a monetary transfer / free irrigation of part of one's plot at a given time period, on one hand, and receiving a higher sum / higher share of the plot if the subject waits one additional week, on the other. Trade-offs are presented under different scenarios for the baseline horizon for receiving the transfer / irrigation (a week or a month from today). We define time-inconsistency as discrepancies across decisions to receive money / irrigation at the baseline period or one week later when the baseline horizon changes. When a subject decides to receive the transfer at the baseline period, regardless if it is today or in a month from now, or to have only ¼ of his or her plot irrigated at the baseline period, irrespective if it is today or in a month from now, then there is no time inconsistency (no sensitivity to framing). Conversely, when subjects decide differently conditionally on the baseline horizon, then there is time-inconsistency (sensitivity to framing).

The Supplementary Appendix shows that priming significantly reduces time-inconsistency, in line with our previous finding for focus enhancement. Interestingly, higher consistency plays out through higher – rather than lower – patience. Furthermore, this finding falsifies the prediction from the anticipation/dread theory (Caplin and Leahy, 2001) that worries about future risk should lead to more inconsistencies.

### 4.8 **Relation to the literature**

While the mental bandwidth/cognitive load theory concerns the effects of facing low *levels* of resources, it conjectures that those effects could also extend to facing *variance*. This paper is the first to provide evidence that the predictions of the theory also apply to risk.

Our results are also in line with previous findings for the effects of scarcity on psychological outcomes. Mani et al. (2013) find that poverty significantly decreases IQ. Through a variety of survey experiments priming subjects about expenses, it finds that priming adversely affects poor subjects' performance in cognitive tests. The paper connects these experiments with the finding of sugarcane farmers' differential performance in Raven matrices' tests, before and after harvest, taking advantage of (assumed randomly) staggered harvesting dates induced by a monopsonist sugar mill. Shah, Shafir and Mullainathan (2015) find that priming decreases subjects' susceptibility to framing through a variety of survey experiments, using tests very similar to the ones used in this paper. More closely associated with droughts, Haushofer and Fehr (2014) explore the effects of negative rainfall shocks on stress, measured by cortisol levels.

Relative to Mani et al. (2013) and in Haushofer and Fehr (2014), we are better able to rule out alternative explanations for our empirical findings, for four reasons. First, we combine natural variation with survey experiments, which are based on randomization and are tightly linked to the mechanism of interest. Second, our psychological tests are undertaken within, at most, 5 minutes from the priming, discarding alternative mechanisms that could confound the effects of worries – in particular, differential nutrition. Third, by relying on an automated technology to run our lab experiments, our findings are not subject to recent criticism about experimenter bias (Doyen et al., 2012), which posits that interviewers' awareness of the objective of priming experiments creates a tendency to find significant effects. Fourth, we are able to assess both cognitive load and focus enhancement, providing crisp evidence for the cognitive load/bandwidth mechanism.

In contrast to previous results, Carvalho, Meier and Wang (2015) find that changes in economic circumstances do not significantly affect cognitive performance or the quality of decision-making. Randomly assigning subjects to be surveyed right before or right after payday in a sample of poor US individuals, they find that those surveyed after payday have higher expenses and are somewhat less concerned about making ends meet. Nevertheless, they do not perform differentially in psychological tests that are in the same spirit of the ones we perform in this paper, nor exhibit different risk aversion or present-bias than those surveyed before payday. While the experiment is internally valid, it is unclear to what extent its findings are generalizable: worries may not vary substantially with payday in this setting. Having said that, the fact that payday is *certain* in the experiment may suggest that it is risk – rather than predictable changes in economic circumstances – which plays a central role in the previous findings of Mani et al. (2013).

The evidence that negative rainfall shocks increase cognitive load even holding harvest losses fixed also has implications for other areas of development research. It suggests that rainfall shocks may not satisfy the exclusion restriction necessary for a valid instrumental variable in uncovering the relationship between poverty and stress (Haushofer and Fehr, 2014), and the relationship between poverty and conflict (Miguel, Satyanath and Sergentin, 2004).[21]

[21] Higher cognitive load leads to positive affect and higher fairness; Schulz et al. (2014).

## 5 Does insurance mitigate the effects of rainfall risk?

This section starts by describing the new index insurance product in subsection 5.1, and by providing details about treatment assignment, insurance distribution, and take-up in subsection 5.2. We revisit harvest losses in subsection 5.3 to motivate the effects of interest. Next, subsection 5.4 discusses some basic threats to identification of causal effects in our experiment. Subsection 5.5 presents the effects of the experiment on worries about rainfall, followed by their effects on cognitive load and focus in subsection 5.6. Subsection 5.7 presents results for heterogeneous treatment effects and summarizes robustness checks. Subsection 5.8 discusses some reasons for why index insurance might have no effects in this setting, and presents results for heterogeneous treatment effects by municipality's average trust. Subsection 5.9 discusses how our findings relate to the other research about insurance.

### 5.1 **New index insurance product**

We randomly assigned offers of a new index insurance product to 1,192 farmers within our pool of enrolled subjects. This insurance alleviates the material consequences of the risk of future rainfall variation, paying farmers the equivalent of their household average income (about USD 47.5) in case municipal harvest losses are 70% or higher. The design of the insurance policy combines the two features that we discussed might affect worries. It alleviates the material consequences of future rainfall variation, possibly decreasing farmers' worries about droughts , and it pays out based on a municipal-level index, possibly making farmers worry more.

There are two challenges to identifying the effects of insurance on worries and cognitive function in this setting. First, most farmers in our sample also have access to government insurance, which pays out 5 times the amount our insurance does, for 50% or higher municipal harvest losses. This might mean that farmers do not worry about future rainfall, regardless of the assignment of our product, or that, even if they do, the marginal decrease in exposure from additional coverage is not enough to make them worry less. Second, insurance induces rational responses through the risk-aversion mechanism, which might affect cognitive function through variation unrelated to the mechanism of interest.

We deal with the first challenge by exploiting the fact that farmers face liquidity constraints before government insurance pays out in August. The insurance product we randomize pays out already in June, when marginal utility should be very high if harvest losses are that high. We deal with the second challenge by assigning insurance in February, when many crucial production decisions – in particular, planting area and crop choice – have already been undertaken and cannot be changed. That way, we shut down rational responses that operate through the risk-aversion mechanism.

### 5.2 Assignment and take-up

We distributed the new index insurance free of charge through a call center, completely separate from the surveys, so as not to induce any sort of reciprocity bias (individuals offered insurance exerting higher effort at cognitive tests, for instance). Out of the 1,192 farmers offered the new product free of charge, 912 (76.5%) accepted it. The main reason provided for rejecting the offer was the concern that taking up this product would make farmers ineligible for government insurance, even though the script was explicit about this not being the case.[22]

All estimates presented are intention-to-treat (ITT) effects, comparing those farmers offered insurance to those who were not offered, within each municipality and wave. We present estimates for treatment effects on the treated (ToT) in the Supplementary Appendix.

### 5.3 Harvest losses revisited

To motivate the effects of interest, we go back to the relationship between harvest losses and farmers' cognitive load, in Panel 2 of Figure 1. By June, after harvest losses are made public, cognitive load decreases right after 50% harvest losses, the threshold at which government insurance pays out, and right after 70% harvest losses, the threshold at which the new insurance product pays out (the effects shown are lower bounds, since it considers municipal averages without accounting for assignment status).

Even though we do not have sufficient statistical power to detect those effects as statistically significant, these findings are strongly suggestive that insurance has the potential to mitigate the adverse effects of worries about rainfall risk on the cognitive function. This is because the effects are driven by eligibility, rather than actual payout – which is processed only in August for the government policy, and in late June for the new policy.

Since our interest is on whether insurance might mitigate the effects of worries, earlier on, before uncertainty is resolved, we move on to study the effects of our field experiment on cognitive load and focus. Nevertheless, we can use the correlations between payout eligibility for Garantia Safra and the summary measures of cognitive load and focus by June (restricting attention to the municipalities between 40% and 60% harvest losses, the narrowest bandwidth for which these correlations can be computed) to benchmark the effects of insurance on those measures.

[22] See Appendix C for the call center scripts.

## 5.4 **Balance and attrition**

Insurance offers are assigned ensuring that the treatment and control groups are balanced in what comes to baseline covariates. However, not all enrolled subjects participate, as we have previously discussed, and one might be concerned that treatment assignment interacts with other attributes to determine which individuals self-select into participating. If that is the case, then our estimates would confound the effect of treatment with that of those attributes.

Column (4) of table D1 presents the result of an ordinary least squares (OLS) regression with an indicator variable of whether each call was completed as dependent variable, and with the intention-to-treat indicator as independent variable. The marginal effect of having been offered insurance on the probability of completing a call is not statistically significant, and quite small in magnitude (1.3%).

Table D6 displays the differences across the subsamples offered insurance and not offered, considering all baseline covariates. Differences are significant for only 1 out of 13 variables, as one would expect to happen out of mere chance.

[Table D6]

## 5.5 **Worries about rainfall**

Using the same working definition of worries about future rainfall as before, Table 6 presents results of Ordinary Least Squares (OLS) regressions of worries on insurance (ITT). Column (1) presents the effect of insurance itself, and columns (2) through (5) also include its interaction with the variables we previously documented to increase farmers' worries. In none of the specifications does insurance have significant effects on worries about rainfall. What is more, point estimates are positive in all but one case, the opposite of what one would expect if farmers thought of insurance as alleviating the material consequences of future rainfall variation.

[Table 6]

## 5.6 **Cognitive load and focus**

Table 7 presents the mean effect sizes of insurance on the summary measures of cognitive load and focus. Given the null effects of insurance on worries, discussed in the previous subsection, it is not surprising that insurance (ITT) also has no effects on either cognitive load or focus. In fact, the effect of insurance on cognitive load is a precisely estimated zero.

[Table 7]

Figure 9 shows that the absence of insurance effects on cognitive load is consistent across both executive functions and sensitivity to anchoring.

[Figure 8]

## 5.7 **Heterogeneity and robustness checks**

One possibility is that even though index insurance does not mitigate the effects of worries on cognitive function on average, it might do so for specific subsets of farmers. Insofar as priming seems to affect farmers who neither perform too well nor too poorly on cognitive tests, it might also be that only part of the distribution is *marginal* in what comes to the effects of insurance. To investigate this hypothesis, we start by running quantile regressions (without fixed-effects) to examine how the effects of insurance (ITT) vary with the distribution of cognitive load.

Figure 4 presents quantile regression estimates with confidence intervals for quantiles 0.1 to 0.9. Insurance does have positive effects on cognitive performance among farmers who perform the worst with respect to executive functions and sensitivity to behavioral biases. Extending the rationale used for the case of priming, insurance has positive effects only among farmers who worry the most about future rainfall. While this is testimony to the potential benefits of insurance, in this setting such benefits accrue to a very narrow subset of farmers.

[Figure 4]

Another strategy to address potential heterogeneity is to estimate the effects of insurance on cognitive load and focus for subsets of the sample that, in principle, should be more worried about future rainfall. We do so by interacting the insurance intention-to-treat indicator with several proxies for higher worries, and testing whether each interactions positively affect cognitive function. We consider five different proxies for worries about rainfall. The first two are below-normal rainfall shocks and priming, which we have previously shown to increase farmers' worries. The other proxies are absence of irrigation, absence of government insurance, and an indicator of whether the farmers lives in the most drought-prone region within his/her municipality.

Columns (1) through (5) in Tables 8 and 9 display, respectively, the results for cognitive load and focus. Neither insurance nor its interaction with either proxy for higher worries about future rainfall have positive and significant effects on cognitive function. If anything, in some instances, insurance significantly decreases cognitive performance amongst the most worried, according to our proxies.

[Table 8]

[Table 9]

Taking stock, there is no systematic evidence that the null effect of insurance on cognitive function accrues to heterogeneity.

In terms of robustness checks, one might worry that controlling for baseline covariates might change the effects of insurance on worries, cognitive load, or focus. Since covariates are balanced across treatment and control groups insurance (ITT), controlling for covariates should only increase the precision of our estimates. However, because we do not have information on baseline controls for all subjects, we end up having lower precision due to somewhat smaller sample sizes. The Supplementary Appendix shows results, first without controls but restricting the sample to the subset of individuals for which we have data on baseline covariates, and then including those variables in the regressions. We find that estimates are not affected by controlling for baseline covariates.

### 5.8 **Why does insurance have no effects?**

Why does insurance have no effects on cognitive function? It is not a matter of statistical power: calculations indicate that we would be able to detect effects of insurance of similar magnitude to those of the survey experiments. Most importantly, point estimates of its effects on worries are actually positive (hence not a matter of precision), and its effect on cognitive load is a very precise zero. The null result is also not due to heterogeneity: we do not find systematically different effects of insurance for subsets of participants that should be more concerned with rainfall – those without irrigation, those otherwise uninsured, those living in the most drought-prone region in each municipality, and those facing negative rainfall shocks or randomly primed about droughts.

We speculate that this null result relates to the recent literature on the potential shortcomings of index insurance, ranging from distrust to basis risk. To test the hypothesis about distrust in index insurance, we compute municipality's average trust level, based on individual performance in trust games in which subjects decide how much money to send to the other player (Berg at al., 1995). In this game, player 1 decides how much out of R$200 to send to player 2 (50, 100, 150, or 200). The amount sent forward is multiplied by 3. Player 2 then decides how much to send back: any integer up to 150 if player 1 sent 50; any integer up to 300 if player 1 sent 100; any integer up to 450 if player 1 sent 150; and any integer up to 600 if player 1 sent 200 (see Appendix A). We define trust as the share of R$200 that subjects decide to send forward when playing as player 1.

In computing municipal averages, we only consider subjects at the first wave (March), who do not have insurance, and who were not primed at that survey, since we do not want this measure to be influenced by our treatments nor by previous participation in the experiments.

Table 10 presents the results for the effects of the interaction of insurance with municipal average trust. We find that insurance decreases worries about rainfall within municipalities with higher levels of trust (although the effect is not statistically significant). What is more, it improves cognitive function (significant at the 10% level), and reduces focus on scarce resources (not statistically significant).

[Table 10]

Although not always precisely estimated, results are consistent with the mental bandwidth mechanism. Even if trust is not randomly assigned across municipalities, this finding is suggestive that the distrust on payout could explain why insurance did not mitigate worries or cognitive load in this setting. In fact, the results from quantile regressions for the effects of insurance could be interpreted as consistent with the trust mechanism, as evidence from lab experiments suggests that individuals under high cognitive load might also be more trusting.[23]

Given that our product was advertised as an emergency top-up of a popular and well-established government product with such high take-up, we consider this a striking result.

[23] Higher cognitive load leads to positive affect and non-strategic behavior; Schulz et al. (2014).

### 5.9 **Relation to the literature**

There has been active research on how to bring insurance take-up to higher levels in the developing world (e.g.: Casaburi and Willis, 2015; Cole, Stein and Tobacman, 2014; Cole, Gine, Tobacman, Topalova, Townsend and Vickery, 2013; Gaurav, Cole and Tobacman, 2011). There is now systematic evidence that behavioral biases (in particular, accessibility biases and present bias) decrease the demand for insurance. This paper provides evidence that there might be additional channels through which biases constrain higher take-up: we have shown that rainfall risk decreases the relative demand for crop insurance relative to funeral insurance, through worse cognitive function.

There is also recent work on the positive effects of insurance operating through the risk-aversion mechanism. Karlan et al. (2014) document that risk constraints are first-order among poor farmers in developing countries: once granted insurance, farmers find their way through credit constraints and substantially increase investment. However, while investment substantially increases, profits do not. Given this puzzling finding, we believe it is timely to think more broadly about the effects of insurance on decision-making. The evidence in this paper that rainfall risk increases farmers' cognitive load while index insurance does not mitigate these effects might partly explain this puzzle.

Our findings about heterogeneous treatment effects of insurance with respect to trust are consistent with a recent literature on the shortcomings of index insurance. There is increasing evidence that farmers do not trust index insurance will pay out, even when they are eligible (Dercon, Gunning and Zeitlin, 2015; Karlan et al., 2014). This problem is amplified by basis risk, as farmers worry about states in which they lose their harvest due to idiosyncratic shocks and yet insurance does not pay out (Elabed and Carter, 2015; Clarke, 2011). While this literature has noticed that these shortcomings further contribute to the low demand for index insurance, they may affect not only product take-up, but also its effectiveness, by failing to mitigate worries about rainfall risk and their detrimental effects on farmers' decision-making.

## 6 Discussion and concluding remarks

Using a combination of survey experiments and natural variation in recent rainfall shocks, this paper has documented that rainfall risk increases farmers' cognitive load and their susceptibility to a variety of behavioral biases. The fact that survey experiments also improve farmers' performance in tasks involving scarce resources further supports the interpretation that these effects are driven by the bandwidth/cognitive load mechanism (Mullainathan and Shafir, 2013). This paper is the first to provide evidence that the predictions from this theory carry over from *actually* having too little to the *risk* of having too little, as well.

Such a mechanism is fundamentally different from the conventional rational responses to future rainfall variation, working through risk aversion. The latter predicts that the risk of a drought might impact current choice variables that affect the distribution of outcomes across states of nature in the future, trading off payoffs across states. The mechanism we study in this paper predicts that *all* current choice variables might be impacted by anticipation and anxiety, possibly leading to lower payoffs in *every* state of nature in the future, regardless of the occurrence of a drought.

There are two reasons to believe that these effects could be first-order. First, the impact of worries on cognitive function that we find in this setting are sizable. The gap in cognitive performance across farmers differentially affected by rainfall risk is equivalent to that between farmers in municipalities with no harvest losses and those in municipalities with over 40% losses at the end of the rainy season (in a cross sectional-comparison). Second, in any given year, only some farmers are actually hit by a drought (in Ceará, for instance, 1/3 of municipalities are affected each year on average), whereas all of them are *always* at risk.

Cognitive function lies at the foundation of every decision, and we have illustrated its link with decision-making through the demand for production-related credit and insurance. The fact that both negative rainfall shocks and priming decrease the demand for these products (relatively to credit for consumption and funeral insurance) suggests how the psychological tax imposed by worries might lead to a poverty trap. Exposure to droughts increase worries, which not only lead to worse-quality decisions but also decrease the demand for crop insurance, keeping farmers exposed and making these effects persistent.

While we expected insurance to at least partly mitigate worries about rainfall and their effects on cognitive function, we found no effects on either dimension. It seems that distrust in index insurance (which has been the subject of active research) is also an important factor in this setting. In municipalities with higher average levels of trust, insurance does reduce worries about rainfall (even if the effect is not precisely estimated) and significantly alleviates cognitive load. Moreover, when information about payout eligibility at last becomes public, it seems to drive cognitive load down (even though we do not have sufficient statistical power to take advantage of the discontinuity design). Last, the fact that the most common reason provided by those who did not accept the insurance offer was the fear of losing coverage from government insurance is testimony to the generalized distrust in that product. Strikingly, that is the case even though *Garantia-Safra* has been around since 2002, and despite the fact that it has paid out for at least 1/3 of the State every year, and for as many as 2/3 of its municipalities only two years before our experiments.

At the same time, we do acknowledge that given the long distances and high transportation costs in the developing world, there is yet no feasible alternative to index insurance for protecting poor farmers from rainfall risk. Nevertheless, this paper suggests that the limited credibility of index insurance limits not only higher take-up, but also the potentially positive psychological benefits of insurance among those who take it up.

One conjecture is that insurance policies that pay out more frequently might help with both dimensions. More frequent payouts might not only increase take-up by making its benefits more salient to farmers (Karlan et al, 2014), but might also decrease worries and cognitive load, since there is evidence that payments increase cognitive function (Mani et al, 2013). What features of insurance design matter – the choice of the index, the structure of payouts, the provision of complementary information which might reduce anxiety about future rainfall variation – are interesting questions to be explored in future research.

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## Appendix A – Definition of dependent variables

**WORRIES**

"How much did you and your family worry last week about how much it will rain in the next month? If not at all, press 0, if a little, press 1, if a lot, press 2"

**COGNITIVE LOAD**

**- Executive Functions**

Digit span:

"Please type the sequence of numbers as you hear it. 4 8 2 0 5 / 5 2 9 1 7 / 0 3 6 4 8 / 9 1 9 2 1"

Stroop:

"How many times is number '9' repeated in the following? 9 9 9 9 / 6 6 6 6 6/ 0 0 0 / 5 5 5 5"

**- Anchoring:**

Price of beans:

"Last year's average price per kg of a live goat was R$ 4 in Ceará. Which of the following price bands best characterizes what the selling prices of beans in May will be in your municipality? If between 3 and 3.40 reais, press 1; if between 3.40 and 3.80, press 2; if between 3.80 and 4.20, press 3; or, if above 4.20 reais, press 4"

Price of subway ticket:

"Last year's average price per kg of a live goat was R$ 4 in Ceará. Which of the following price bands best characterizes what the price of a subway ticket in São Paulo is? If between 3 and 3.40 reais, press 1; if between 3.40 and 3.80, press 2; if between 3.80 and 4.20, press 3; or, if above 4.20 reais, press 4"

**FOCUS**

**- Tunneling:**

Word search (water):

"If you hear WATER or HUSBAND among the following scrambled words, please press 1 at the end of each set; otherwise press 0: ÁLCOOL ; ALTO ; ÁGUA ; ARCO / PAI ; FILHO ; ESPOSA ; IRMÃO / LAGO ; NUVEM ; CHUVA ; SECA / QUERIDO ; PALITO ; MARIDO ; FERIDO"

Word search (water) = score[water] – score[neutral]

Word search (money):

"If you hear MONEY or BROTHER among the following scrambled words, please press 1 at the end of each set; otherwise press 0: CHIQUEIRO ; DINHEIRO ; MARINHEIRO ; PINHEIRO / IRLANDA ; SERMÃO ; LIMÃO ; SALMÃO / CHEQUE ; CARTÃO ; BANCO ; DÍVIDA / MARIDO ; PRIMO ; IRMÃO ; ESPOSA"

Word search (money) = score[money] – score[neutral]

Trade-off oranges vs. cashews:

"How many oranges would you offer to trade in 2 kg of cashews? If less than 1 liter, press 1, if between 1 and 4 liters, press 2, if between 4 and 7 liters, press 3, if between 7 and 10 liters, press 4, or if more than 10 liters, press 5."

Trade-off money vs. cashews:

"How much money would you offer to trade in 2 kg of cashews? If less than 2 reais, press 1; if between 2 and 5 reais, press 2; if between 5 and 8 reais, press 3; if between 8 and 11 reais, press 4; or, if more than 11 reais, press 5."

Tunneling (money) = [Trade-off oranges vs. cashews] – [Trade-off money vs. cashews]

Trade-off water vs. cashews:

"How many liters of water would you offer to trade in 2 kg of cashews? If less than 1 liter, press 1; if between 1 and 4 liters, press 2; if between 4 and 7 liters, press 3; if between 7 and 10 liters, press 4; or, if more than 10 liters, press 5."

Tunneling (water) = [Trade-off oranges vs. cashews] – [Trade-off water vs. cashews]

**- Framing:**

Trade-off money vs. time – low value:

"Consider the following scenario: Let's imagine you walk into a store to buy batteries which costs R$ 10. The seller tells you there is a store 40 minutes away which sells the same batteries for R$ 5. If you would buy them for R$ 10 anyway, press 1; if you would rather go to the other store to buy them for R$ 5, press 2"

Trade-off money vs. time – high value:

"Consider the following scenario: Let's imagine you walk into a store to buy an iron which costs R$90. The seller tells you there is a store 40 minutes away which sells the same iron for R$40. If you would buy it for R$90 anyway, press 1; if you would rather go to the other store to buy it, press 2"

Sensitivity to framing (money): money[high] *vs.* money[low], for the subset of individuals either primed in all calls, or not primed in any calls involving these outcomes.

Trade-off water vs. time – low amount:

"Consider the following scenario: Let's imagine you walk downtown to get 1 gallon of water from a water truck. A neighbor tells you there is another municipality, which takes 30 extra minutes to reach (and 30 extra to come back), where you could get 2 gallons of water from a water truck. If you would get water in your own town, press 1; if you would get water from the other municipality, press 2."

Trade-off water vs. time – high amount:

"Consider the following scenario: Let's imagine you walk downtown to get 5 gallon of water from a water truck. A neighbor tells you there is another municipality, which tales 30 extra minutes to reach (and 30 extra to come back), where you could get 6 gallons of water from a water truck. If you would get water in your own town, press 1; if you would get water from the other municipality, press 2."

Sensitivity to framing (water): water[high] *vs.* water[low], for the subset of individuals either primed in all calls, or not primed in any calls involving these outcomes.

**RELATIVE DEMAND**

Credit related to production (waves 1 and 3):

"If you would like to listen to information about credit for irrigation, press 1; otherwise, press 0"

*If subject presses 1*: "*Pronaf Mais Alimentos* finances equipment for irrigation with discount up to 15% of its market price. Irrigation systems financed by the program are: surface irrigation, overhead irrigation, micro-aspersion, and drip irrigation. To the find out which irrigation system best suits your needs, reach out to EMATERCE to prepare the irrigation technical project including: technical specification, layout, and list of materials."

Credit unrelated to production (waves 1 and 3):

"If you would like to listen to information about credit for consumption, press 1; otherwise, press 0"

*If subject presses 1*: "If there are any retirees in your household, that person can file for payroll lending at any bank or financial institution. Payroll lending is a type of loan in which installments are automatically deducted from the retirement payroll, as long as the retiree authorizes. Reach out to your bank or financial institution. If that does not work, you can directly contact *Central do INSS* by calling 135, by contacting *Procon* of Ceará, or through the National Consumer Secretariat's website, www.consumidor.gov.br."

Relative demand for production-related credit: credit[production] – credit[consumption], for the subset of individuals either primed in all calls, or not primed in any calls involving these outcomes.

Insurance related to production (waves 2 and 4):

"If you would like to listen to information about insurance for crop disease, press 1; otherwise, press 0"

*If subject presses 1*: "*Proagro Mais* is a government insurance tailored to small farmers associated with *Pronaf*, covering their investment and working capital operations, either financed with external credit or out-of-pocket. Reach out to the nearest branch of *Banco do Brasil* for more information or to enroll in this insurance."

Insurance unrelated to production (waves 2 and 4):

"If you would like to listen to information about funeral insurance, press 1; otherwise, press 0"

> *If subject presses 1*: "Ceará's electric utility, *Coelce*, offers the Family Funeral Insurance, which includes life insurance in case of death of the primary account holder, food support, electricity bill support, weekly lottery tickets and funeral assistance for all members of the household. For more information, call 0800 707 44 90 or reach out to *Coelce*'s customer service."
>
> Relative demand for production-related insurance: insurance[production] – insurance[consumption], for the subset of individuals either primed in all calls, or not primed in any calls involving these outcomes.

**TRUST**

Trust:

"You and your neighbor are invited to play a game. You receive R$ 200 and can transfer to him either R$ 50, R$ 100, R$ 150, or R$ 200. Whatever you transfer to him is multiplied by 3, and then he can decide how much to give back and how much to keep. How much do you transfer him? If R$ 50, press 1; if R$ 100, press 2; if R$ 150, press 3; or, if R$ 200, press 4."

Trustworthiness / Reciprocity:

"You and your neighbor are invited to play a game. He receives R$ 200 and can transfer to you either R$ 50, R$ 100, R$ 150, or R$ 200. Whatever he transferred to you is multiplied by 3, and then you can decide how much to give back and how much to keep. If you receive R$ 150, how much do you send back? / If you receive R$ 300, how much do you send back? / If you receive R$ 450, how much do you send back? / If you receive R$ 600, how much do you send back?"

**TIME INCONSISTENCY**

Patience (money, week):

"Imagine that someone in your family sends you some amount of money regularly. He/she calls you today and says that he/she can send you R$ 100 today or if you can wait for a week they can send you R$ 150. If you want R$ 100 to be sent today, press 1; if you want R$ 150 to be sent in a week, press 2"

Patience (money, month):

"Imagine that someone in your family sends you some amount of money regularly. He/she calls you today and says that he/she can send you R$ 100 in 1 month or, if you can wait 1 month and 1 week, they can send you R$ 150. If you want R$ 100 to be sent in a month, press 1; if you want R$ 150 to be sent in a month and a week, press 2"

> Time-inconsistency (money): patience[money,week] *vs.* patience[money,month], for the subset of individuals either primed in all calls, or not primed in any calls involving these outcomes.

Patience (water, week):

"Suppose that you get a call on February from an irrigation company that is trying to promote their services offering to irrigate ¼ of your plot for free this week. Alternatively, if you wait 1 week, then they could irrigate ½ your plot. If you want ¼ of your plot irrigated now, press 1; if you want ½ your plot irrigated in a week, press 2"

Patience (water, month):

"Suppose that you get a call on February from an irrigation company that is trying to promote their services offering to irrigate ¼ of your plot for free in a month. Alternatively, if you wait 1 month and 1 week, then they could irrigate ½ your plot. If you want ¼ of your plot irrigated in a month, press 1; if you want ½ your plot irrigated in a month and a week, press 2"

> Time-inconsistency (water): patience[water,week] *vs.* patience[water,month], for the subset of individuals either primed in all calls, or not primed in any calls involving these outcomes.

**OTHER OUTCOMES (Not shown)**

**- Credibility:**

"If you are enrolled in other rainfall insurance, different from Garantia-Safra, press 1; otherwise, press 0."

**- Production decisions:**

Weeding:

"If you have undertaken weeding last week, press 1; otherwise, press 0"

Water re-usage:

"If you have re-used shower water or water from other sources for irrigating your plot last week, press 1; otherwise, press 0"

**- Locus of control**

"For each of the following questions, press 1 if you strongly disagree, press 2 if you disagree a little, press 3 if you agree a little, or press 4 if you strongly agree. 'It's not always wise for me to plan too far ahead, because many things turn out to be a matter of good or bad fortune.' / 'When I get what I want, it's usually because I worked hard for it.' / 'My life is determined by my own actions.'"

**- Aspirations**

Children can succeed:

"If you think a child of yours could succeed outside of farmer's life, press 1; otherwise, press 0."

Educational investment:

"If you would you sell a cow to pay for your child to go to Fortaleza to take university's admission exam, press 1; otherwise, press 0."

**Appendix B – Priming: treatment and control messages**

**- Call #1:**

Treatment: “Please tell us after the BIP what you would do in case your municipality is faced with a drought this year.”

Control: “Please tell us after the BIP what you would do in case the next prime-time soap opera is not good.”

**- Call #2:**

Treatment: “Please tell us to what extent you think your income this year will be determined by rainfall.”

Control: “Please tell us to what extent you think your sleep time will be determined by what is on TV.”

**- Call #3:**

Treatment: “Please tell us to what extent you have been following the rainfall forecast this year and tell us why.”

Control: “Please tell us to what extent you have been following the prime-time soap opera this year and tell us why.”

**- Call #4:**

Treatment: “Please tell us what do you think determines whether the rainy season in your municipality will be good.”

Control: “Please tell us what do you think determines whether the next prime-time soap opera in your municipality will be good.”

**- Call #5:**

Treatment: “Please tell us to what extent rainfall matters for farmers in Ceará.”

Control: “Please tell us to what extent soap operas matter for farmers in Ceará.”

**- Call #6:**

Treatment: “Please tell us what you think the impacts of a drought are on family farmers.”

Control: “Please tell us what you think the impacts of soap operas are on viewers.”

## Appendix C – Insurance: call center script

**- Script call #1:**

Good morning, I need to speak with the farmer responsible for the household. A foundation is testing a new insurance product called São José, an emergency rainfall insurance similar to Garantia-Safra. This insurance has no costs to the farmer, and pays out to enrolled farmers R$ 170 by the end of June in case harvest losses are 70% or higher at his/her municipality, according to EMATERCE. There are no costs because it is being piloted this year, only in Ceará's communities most severely affected by droughts. Your farmer's ID was drawn, from EMATERCE's dataset, to receive this insurance free of charge. I would like to know if you are interested. I will contact you again within a week. Until then, feel free to think about it and discuss it with your family. Do you have any questions?

<u>FAQ</u>

- Won't I have any costs? Not even in case I am eligible for the payout?

The insurance has no cost to the producer – neither at the time of consent, nor at the time of payout – because it is being piloted in 2015.

- Which foundation?

This insurance product is being piloted by the Bill and Melinda Gates Foundation.

- Can I consent now?

I will register your interest, but the foundation requests me to contact you again within a week to make sure you have enough time to think and discuss it with your family.

- What do I have to do to consent?

You just have to confirm your interest when I return this call within a week.

- Do I need to be homologated or enrolled in Garantia-Safra to participate?

No.

- What do I have to do to receive the payout?

In case you consent and are eligible to receive the payout by the end of June, you will receive another call at this same phone number asking for complementary information so that the foundation can transfer you the payout through *vale-postal* [a transfer which does not require a checking or savings account], which can be withdrawn at any post office agency.

- I want to know more about the payout.

In case harvest losses are 70% or higher at your municipality, according to EMATERCE, and in case you have consented to participate, you will receive a single installment of R$ 170 until the end of June. You will receive another call at this same phone number asking for complementary information so that the foundation can transfer you the payout through vale-postal [a transfer which does not require a checking or savings account], which can be withdrawn at any post office agency.

- How I am told that I am eligible for receiving the payout?

The foundation will communicate with you over SMS, and you will receive another call at this same phone number asking for complementary information so that the Foundation can transfer you the resources

- How did you have access to my information (phone number, DAP)?

The foundation has a partnership with the State Secretary of Rural Development, EMATERCE and FUNCEME.

Do you have interest: yes or no? In any case I will contact you again within a week. Thank you for your time and talk to you then.

**- Script call #2:**

Good morning, I need to speak with the farmer responsible for the household. I am following-up on last week's call about Seguro São José. Do you remember our conversation?

- If yes: Do you consent to enrolling in the insurance free of charge? Yes or no?

    - If yes: You will receive a text message at this same phone number confirming your enrollment and with contact information in case you have any questions. Thank you for your time.

    - If no: Thank you for your time.

- If no: As I explained last week, A foundation is testing a new insurance product called São José, an emergency rainfall insurance similar to Garantia-Safra. This insurance has no costs to the farmer, and pays out to enrolled farmers R$ 170 by the end of June in case harvest losses are 70% or higher at his/her municipality, according to EMATERCE. There are no costs because it is being piloted this year, only in Ceará's communities most severely affected by droughts. Your farmer's ID was drawn, from EMATERCE's dataset, to receive this insurance free of charge. I would like to know if you are interested. I will contact you again within a week. Until then, feel free to think about it and discuss it with your family. Do you consent to enrolling on the insurance free of charge? Yes or no?

    - If yes: You will receive a text message at this same phone number confirming your enrollment and with contact information in case you have any questions. Thank you for your time.

    - If no: Thank you for your time.

**Appendix D – Balance and attrition tests**

**Table D1 –** Selective attrition tests

| | (1)<br>complete call | (2)<br>complete call | (3)<br>complete call | (4)<br>complete call |
|---|---|---|---|---|
| Below-normal rainfall | 0.004 | | | |
| | [0.0065] | | | |
| Negative rainfall shocks | | 0.005 | | |
| | | [0.0068] | | |
| Priming | | | 0.006 | |
| | | | [0.00678] | |
| Insurance (ITT) | | | | 0.013 |
| | | | | [0.0100] |
| Municipality fixed-effects | Yes | | No | No |
| Survey fixed-effects | Yes | | No | No |
| Municipality-survey fixed-effects | No | | Yes | Yes |
| Observations | 14,711 | | 22,687 | 22,687 |
| Number of clusters | 31 | | 2,682 | 2,682 |
| R-squared | 0.087 | | 0.153 | 0.153 |

Notes on Table D1:

1. All columns are Ordinary Least Squares (OLS) regressions, with an indicator variable as dependent variable, equal to 1 if the survey (call) was completed by the subject, and 0 otherwise;
2. Columns (1) to (3) are Ordinary Least Squares (OLS) regressions;
3. Robust standard errors in brackets, clustered at the municipality level for rainfall variables and at the individual level for priming and insurance;
4. For rainfall variables, due to the low number of clusters, p-values computed using t-statistics with G – 2 degrees of freedom, where G is the number of clusters (Angrist and Pischke, 2008);
5. *** p<0.01, ** p<0.05, * p<0.1.

**Table D2 –** Number and percentage of subjects per number of surveys completed

| No. of Surveys | Subjects | % |
|---|---|---|
| 1 | 300 | 10.6 |
| 2 | 268 | 9.5 |
| 3 | 225 | 8.0 |
| 4 | 188 | 6.7 |
| 5 | 150 | 5.3 |
| 6 | 167 | 5.9 |
| 7 | 131 | 4.6 |
| 8 | 113 | 4.0 |
| 9 | 115 | 4.1 |
| 10 | 101 | 3.6 |
| 11 | 100 | 3.5 |
| 12 | 105 | 3.7 |
| 13 | 88 | 3.1 |
| 14 | 87 | 3.1 |
| 15 | 93 | 3.3 |
| 16 | 82 | 2.9 |
| 17 | 83 | 2.9 |
| 18 | 65 | 2.3 |
| 19 | 57 | 2.0 |
| 20 | 55 | 1.9 |
| 21 | 52 | 1.8 |
| 22 | 48 | 1.7 |
| 23 | 80 | 2.8 |
| 24 | 69 | 2.4 |

Notes on Table D2:

1. Distribution of the number of surveys (calls) completed by the 2,822 subjects reached by at least one call.

**Table D3 –** Marginal effects of baseline characteristics on the probability of completing a call

| Variable | Marginal effect on probability of completing a call |
|---|---|
| Respondent lives in municipality's most drought-prone region | 0.02** |
| Respondent is male | -0.01 |
| Respondent's age | -0.00** |
| Respondent believes that rainy season will be good if it rains on March 19th | 0.02 |
| Respondent's plot is at least partly irrigated | -0.05*** |
| Respondent owns their property | -0.01 |
| Respondent seeds cassava | 0.00 |
| Number of rooms in respondent's household | 0.00 |
| Respondent's average household income | -0.01 |
| Respondent's schooling | 0.02** |
| Respondent's household is a beneficiary of *Bolsa-Família* | 0.02 |
| Respondent enrolled in Government insurance (*Garantia Safra*) | -0.02* |

Notes on Table D3:

1. All rows are coefficients from Ordinary Least Squares (OLS) regressions, with an indicator variable as dependent variable, equal to 1 if the survey (call) was completed by the subject, and 0 otherwise, and with municipality-survey fixed effects;
2. The sample includes all individuals for which there is information on baseline characteristics;
3. P-values computed from robust standard errors, clustered at the individual level;
4. *** p<0.01, ** p<0.05, * p<0.1.

**Table D4 –** Balance tests: Below-normal rainfall shocks

| | Below-normal = 0 | Below-normal =1 | Difference [1 - 0] | Difference [1 - 0] (municipality fixed-effects) |
|---|---|---|---|---|
| Most drought-prone | 0.535 | 0.541 | 0.007 | 0.002 |
| | [0.0256] | [0.0314] | [0.0261] | [0.0117] |
| Male | 0.323 | 0.349 | 0.0256* | 0.0273** |
| | [0.0222] | [0.0182] | [0.0148] | [0.0133] |
| Age | 35.070 | 34.540 | -0.531 | -0.040 |
| | [0.785] | [0.759] | [0.624] | [0.493] |
| Believes in RoT | 0.677 | 0.638 | -0.0392*** | -0.004 |
| | [0.0175] | [0.0181] | [0.0125] | [0.0119] |
| Irrigation | 0.144 | 0.131 | -0.013 | -0.001 |
| | [0.0131] | [0.0142] | [0.0114] | [0.00932] |
| Owns property | 0.302 | 0.316 | 0.014 | 0.014 |
| | [0.0213] | [0.0180] | [0.0172] | [0.0144] |
| Plot size | 1.235 | 1.349 | 0.114 | 0.022 |
| | [0.230] | [0.313] | [0.178] | [0.223] |
| Cassava | 0.224 | 0.211 | -0.013 | 0.000 |
| | [0.0353] | [0.0334] | [0.0314] | [0.0129] |
| Number of rooms | 5.171 | 5.159 | -0.012 | 0.016 |
| | [0.0871] | [0.0872] | [0.0462] | [0.0340] |
| Household income | 1.647 | 1.663 | 0.016 | 0.019 |
| | [0.0413] | [0.0411] | [0.0319] | [0.0237] |
| Schooling | 2.146 | 2.132 | -0.014 | 0.000 |
| | [0.0297] | [0.0311] | [0.0267] | [0.0222] |
| Bolsa-Família | 0.780 | 0.773 | -0.007 | 0.003 |
| | [0.0180] | [0.0163] | [0.0173] | [0.0171] |
| Government insurance | 0.814 | 0.799 | -0.015 | -0.008 |
| | [0.0249] | [0.0241] | [0.0188] | [0.0138] |

Notes on Table D4:

1. Columns (1) and (2) present the averages for each variable collected at baseline (February) for the control group and for the treatment group, respectively;
2. Column (3) presents the unconditional difference between the treatment and control groups for each variable collected at baseline, and column (4) presents the within municipality difference between the treatment and control groups for each variable collected at baseline;
3. Weighted averages and p-values computed from the sample of all individuals with information for baseline characteristics, and treatment assignment across all 24 surveys.

**Table D5 –** Balance tests: Priming

| | Priming = 0 | Priming = 1 | Difference [1 - 0] | Difference [1 - 0] (municipality-survey fixed-effects) |
|---|---|---|---|---|
| Most drought-prone | 0.542 | 0.537 | -0.00436 | - |
| | [0.0125] | [0.0125] | [0.00758] | |
| Male | 0.338 | 0.338 | -5.99E-05 | 0.00195 |
| | [0.0139] | [0.0139] | [0.0110] | [0.0109] |
| Age | 35.54 | 35.18 | -0.368 | -0.366 |
| | [0.622] | [0.608] | [0.372] | [0.424] |
| Believes in RoT | 0.659 | 0.670 | 0.0115 | 0.00531 |
| | [0.0143] | [0.0140] | [0.0112] | [0.0114] |
| Irrigation | 0.138 | 0.134 | -0.00321 | -0.00333 |
| | [0.0115] | [0.0112] | [0.00668] | [0.00718] |
| Owns property | 0.318 | 0.316 | -0.00174 | 0.00221 |
| | [0.0165] | [0.0168] | [0.0133] | [0.0139] |
| Plot size | 7.142 | 6.583 | -0.559 | -0.148 |
| | [1.193] | [0.944] | [0.472] | [0.409] |
| Cassava | 0.208 | 0.216 | 0.00794 | 0.00791 |
| | [0.0139] | [0.0144] | [0.00789] | [0.00754] |
| Number of rooms | 5.200 | 5.122 | -0.0778** | -0.0797** |
| | [0.0545] | [0.0551] | [0.0337] | [0.0332] |
| Household income | 1.657 | 1.651 | -0.0062 | 0.000677 |
| | [0.0262] | [0.0261] | [0.0148] | [0.0153] |
| Schooling | 2.158 | 2.127 | -0.0313* | -0.0294* |
| | [0.0292] | [0.0296] | [0.0161] | [0.0177] |
| Bolsa-Família | 0.769 | 0.782 | 0.013 | 0.012 |
| | [0.0153] | [0.0150] | [0.00863] | [0.00914] |
| Government insurance | 0.795 | 0.789 | -0.00655 | -0.00749 |
| | [0.0110] | [0.0113] | [0.00763] | [0.00796] |

Notes on Table D5:

1. Columns (1) and (2) present the averages for each variable collected at baseline (February) for the control group and for the treatment group, respectively;
2. Column (3) presents the unconditional difference between the treatment and control groups for each variable collected at baseline, and column (4) presents the within municipality-survey difference between the treatment and control groups for each variable collected at baseline;
3. Averages and p-values computed from the sample of all individuals with information for baseline characteristics, and treatment assignment across all 24 surveys.

**Table D6 –** Balance tests: Insurance

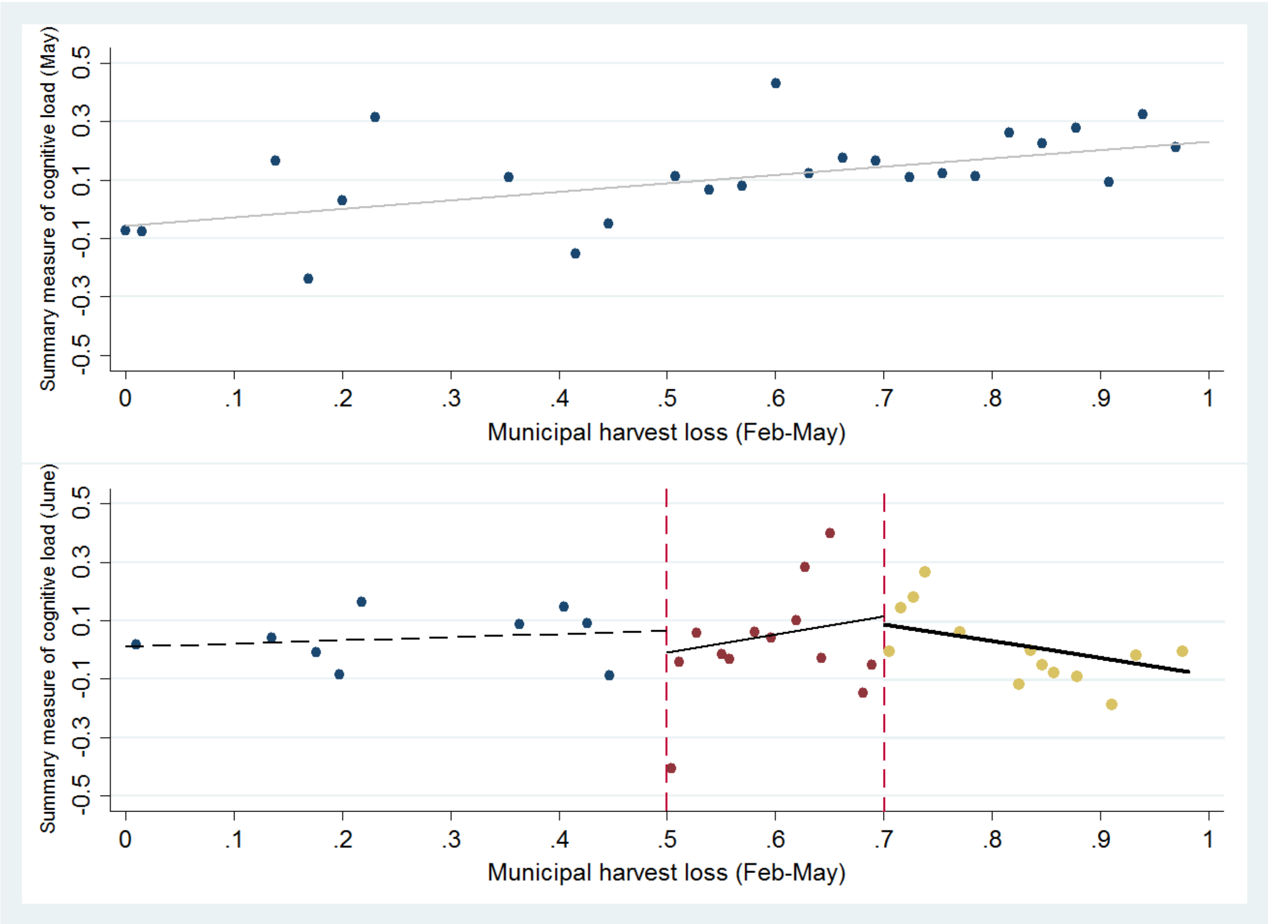
| | ITT = 0 | ITT = 1 | Difference [1 - 0] | Difference [1 - 0] (municipality fixed-effects) |
|---|---|---|---|---|
| Most drought-prone | 0.520 | 0.505 | -0.0152 | - |
| | [0.00935] | [0.0146] | [0.0173] | |
| Male | 0.371 | 0.332 | -0.0387* | -0.0332 |
| | [0.0134] | [0.0191] | [0.0233] | [0.0240] |
| Age | 34.06 | 33.83 | -0.23 | -0.448 |
| | [0.596] | [0.912] | [1.089] | [1.187] |
| Believes in RoT | 0.673 | 0.619 | -0.0541** | -0.0479* |
| | [0.0133] | [0.0202] | [0.0242] | [0.0252] |
| Irrigation | 0.150 | 0.156 | 0.00612 | 0.0175 |
| | [0.0121] | [0.0179] | [0.0216] | [0.0226] |
| Owns property | 0.319 | 0.313 | -0.00698 | 0.00991 |
| | [0.0161] | [0.0232] | [0.0282] | [0.0296] |
| Plot size | 1.434 | 1.363 | -0.0708 | -0.185 |
| | [0.307] | [0.323] | [0.446] | [0.522] |
| Cassava | 0.214 | 0.213 | -0.00112 | -0.0138 |
| | [0.0144] | [0.0207] | [0.0252] | [0.0250] |
| Number of rooms | 5.156 | 5.139 | -0.0165 | -0.0278 |
| | [0.0566] | [0.0794] | [0.0976] | [0.0990] |
| Household income | 1.693 | 1.644 | -0.0489 | -0.0648 |
| | [0.0281] | [0.0394] | [0.0484] | [0.0520] |
| Schooling | 2.195 | 2.148 | -0.0476 | -0.051 |
| | [0.0301] | [0.0413] | [0.0511] | [0.0548] |
| Bolsa-Família | 0.776 | 0.782 | 0.00584 | 0.00434 |
| | [0.0153] | [0.0219] | [0.0267] | [0.0284] |
| Government insurance | 0.792 | 0.781 | -0.0109 | -0.00717 |
| | [0.0150] | [0.0221] | [0.0267] | [0.0269] |

Notes on Table D6:

1. Columns (1) and (2) present the averages for each variable collected at baseline (February) for the control group and for the treatment group, respectively;
2. Column (3) presents the unconditional difference between the treatment and control groups for each variable collected at baseline, and column (4) presents the within municipality-survey difference between the treatment and control groups for each variable collected at baseline;
3. Averages and p-values computed from the sample of all individuals with information for baseline characteristics.

## Appendix E – Figures

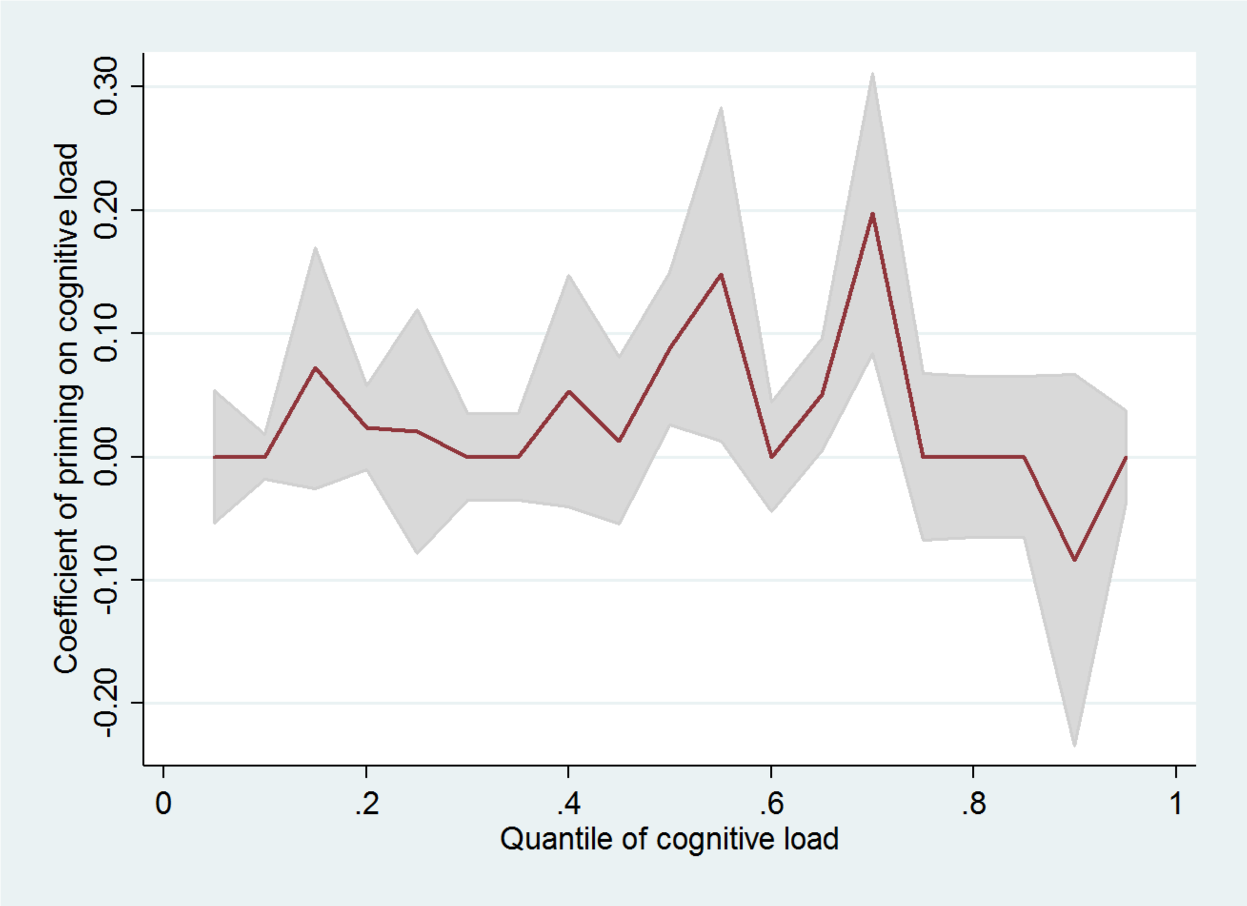
**Figure 1 –** Effects of harvest losses on executive functions among those not offered insurance




<u>Notes on Figure 1:</u>

1. Panel 1 (May) is a linear fit of the average of the standardized summary measure (z-score) for outcomes under the cognitive load category in that month, and the harvest loss between February and May. Cognitive load measure subjects' attention, memory, and impulse control, and susceptibility to anchoring. See Appendix B for the definition of all variables. Harvest losses are measured by government as the difference between estimated harvest – based on projections for planting area and yield in January (pre-season) – and actual harvest – verified in late May (post-season) through audits in randomly selected plots in each municipality;
2. Panel 2 displays three linear fits of the average of the standardized summary measure (z-score) for outcomes under the cognitive load category in that month, and the harvest loss between February and May, for municipalities below 50% harvest losses, between 50% and 70%, and above 70% harvest losses. *Garantia-Safra*, the government's index insurance, pays out for enrolled farmers in municipalities with harvest losses 50% or higher, and the insurance product we designed pays out for enrolled farmers in municipalities with harvest losses 70% or higher, both according to the government's end-of-May report.

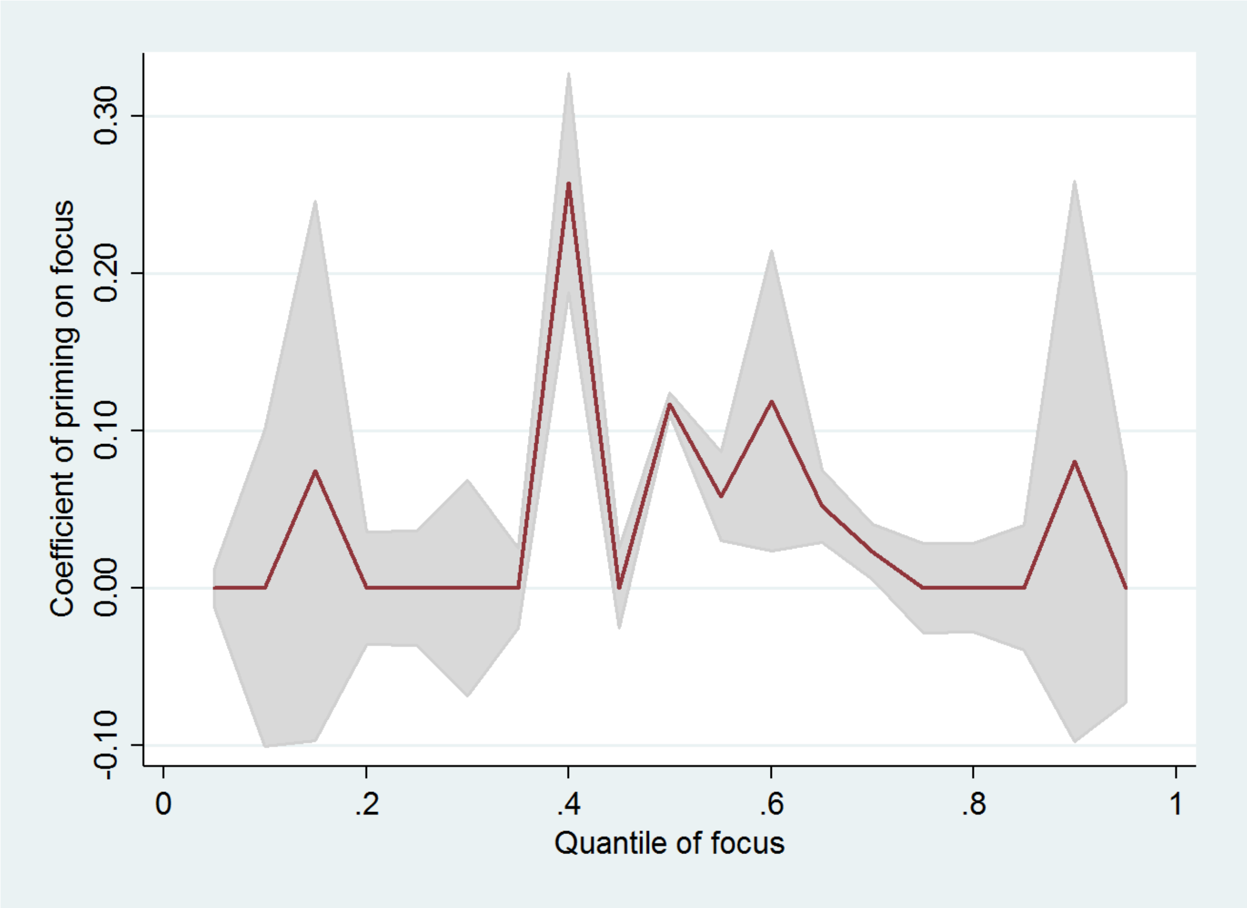
**Figure 2 –** Coefficients from quantile regression of cognitive load summary measure on priming




Notes on Figure 2:

1. Coefficients of quantile regressions of cognitive load summary measure on priming about droughts, not including fixed effects;
2. 90% confidence intervals, standard errors not clustered.

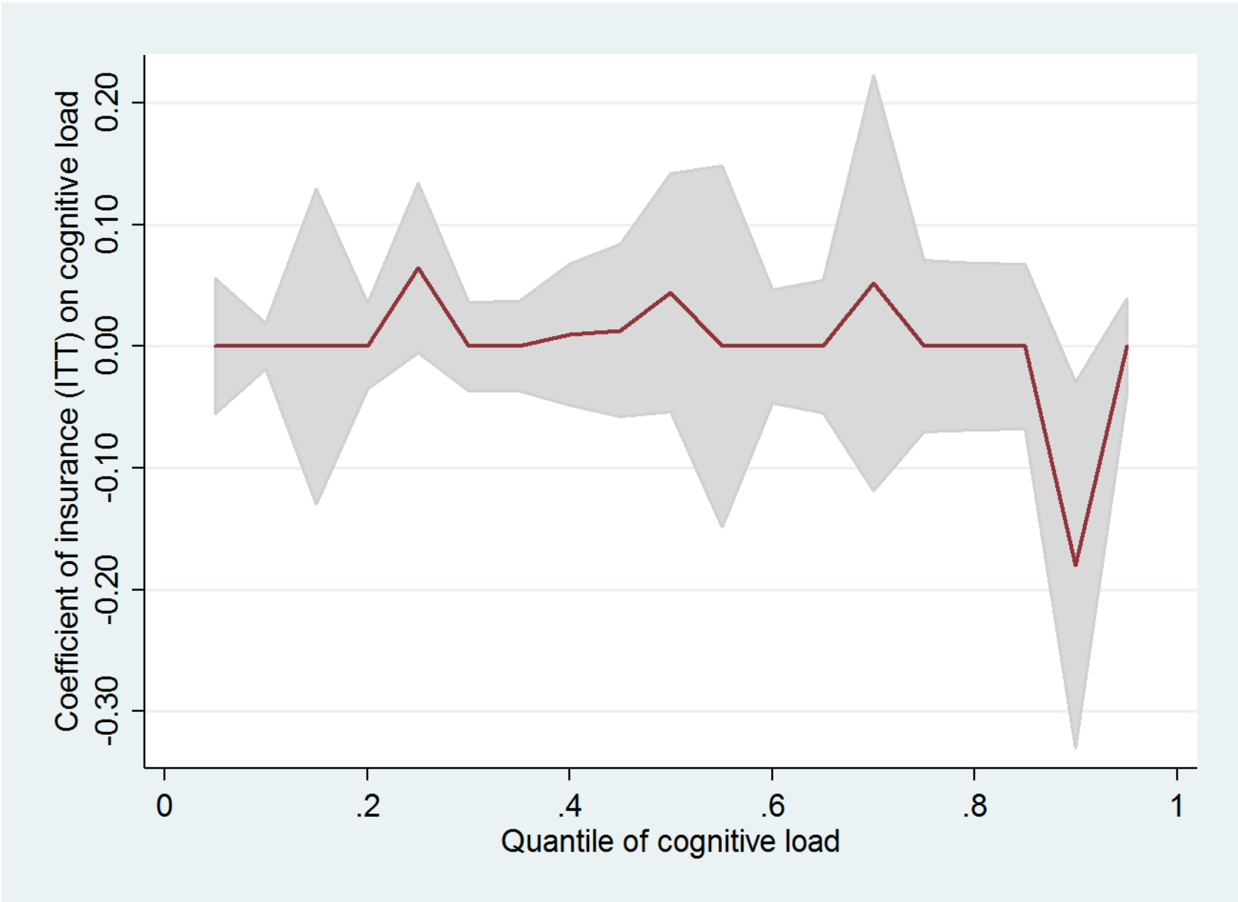
**Figure 3 –** Coefficients from quantile regression of focus summary measure on priming




<u>Notes on Figure 3:</u>

1. Coefficients of quantile regressions of focus summary measure on priming about droughts, not including fixed effects;
2. 90% confidence intervals, standard errors not clustered.

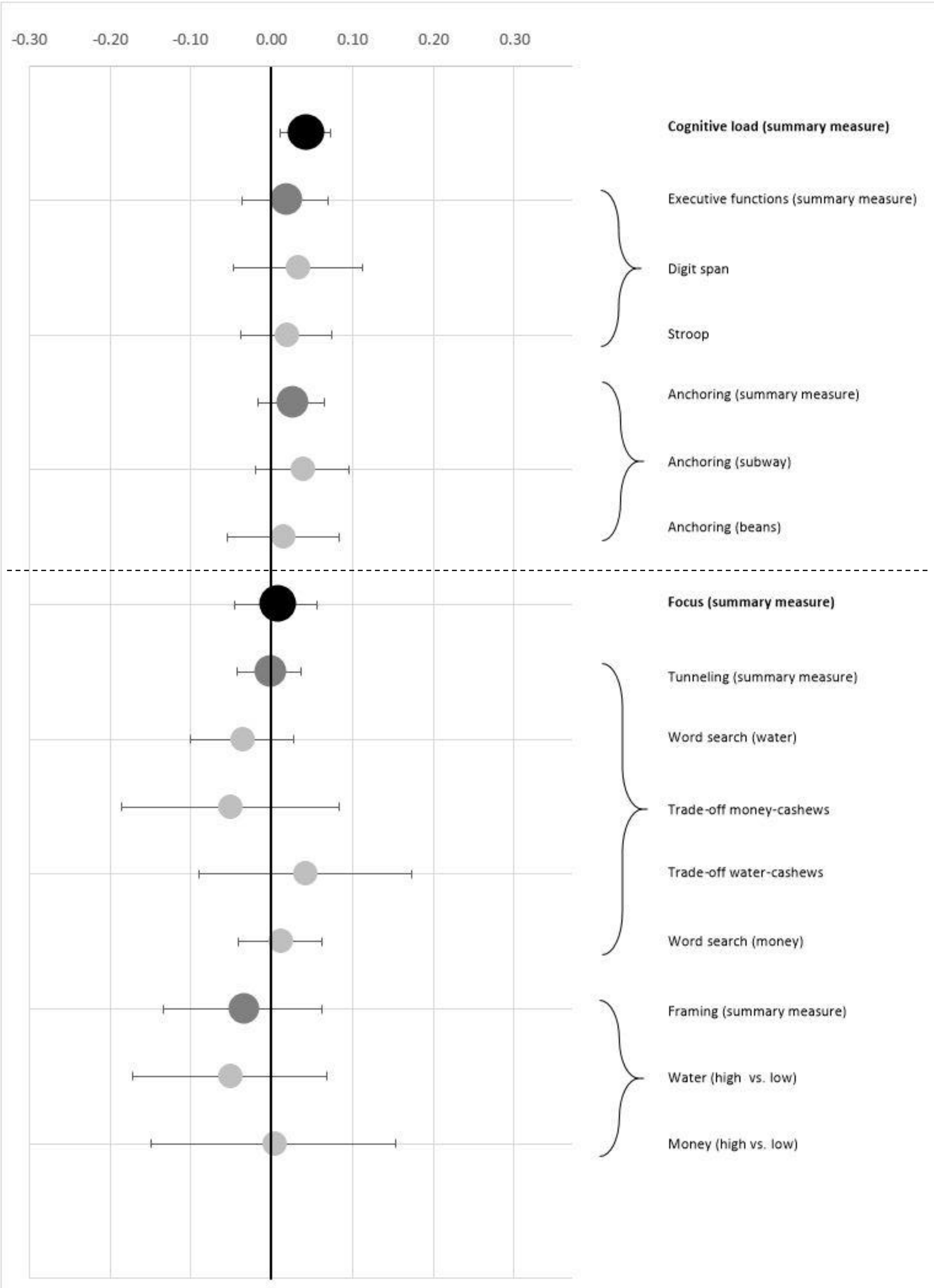
**Figure 4 –** Coefficients from quantile regression of cognitive load summary measure on insurance (ITT)




Notes on Figure 4:

1. Coefficients of quantile regressions of cognitive load summary measure on insurance (ITT), not including fixed effects;
2. 90% confidence intervals, standard errors not clustered.

**Figure 5 –** Effects sizes of below-normal rainfall on cognitive load and focus (90% confidence interval)



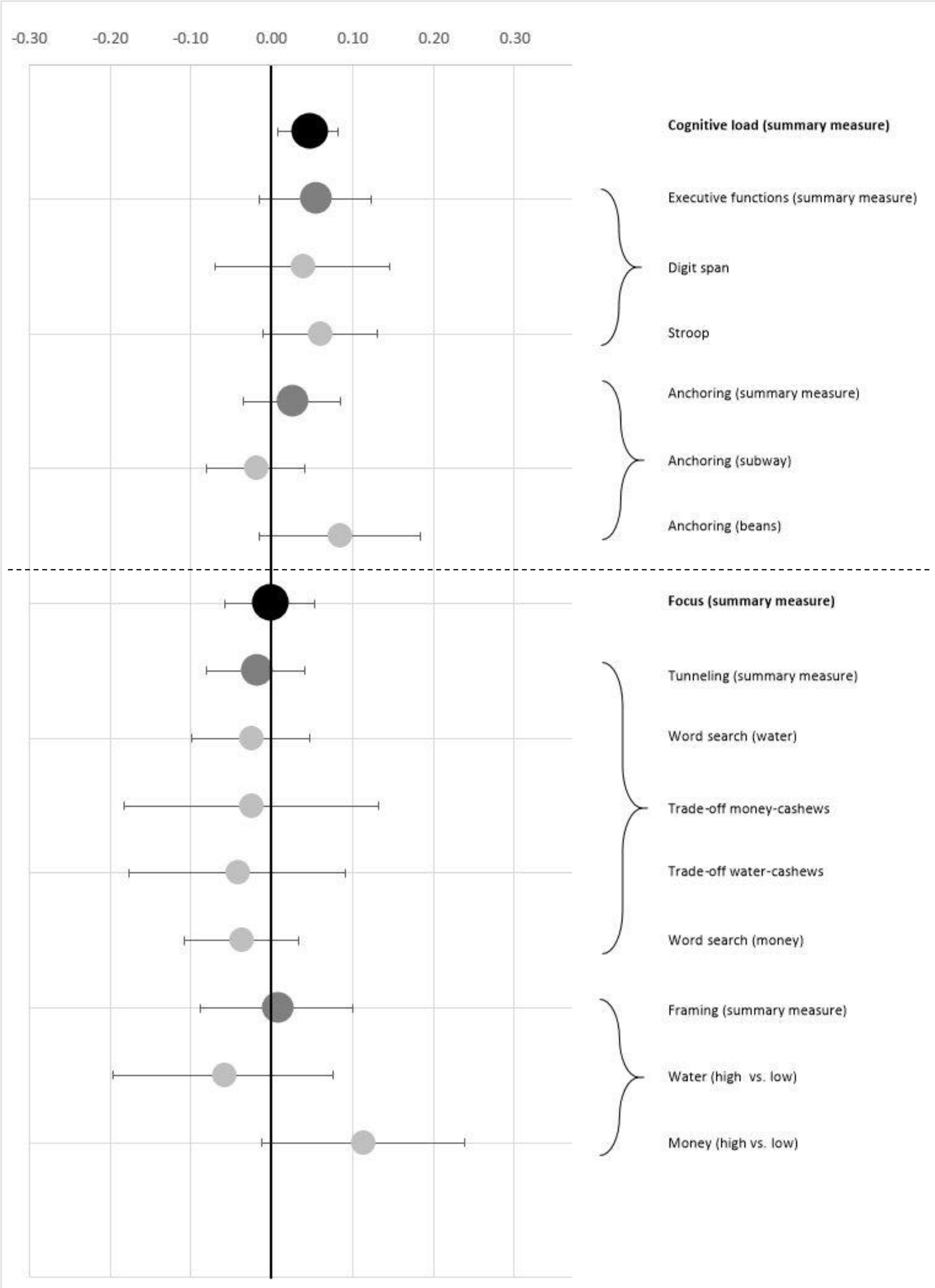

Notes on Figure 5:

1. The figure displays coefficients and 90% confidence intervals for summary measures and for all individual outcomes under each category;

2. Effect sizes are defined as $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) based on Ordinary Least Squares (OLS) regressions with standardized dependent variables (z-scores) for the components of each outcome category; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure;

3. All outcomes are normalized such that a positive coefficient means worse performance for components of the cognitive load summary measure and better performance for components of the focus summary measure.

**Figure 6 –** Effects sizes of rainfall shocks on cognitive load and focus (90% confidence interval)




Notes on Figure 6:

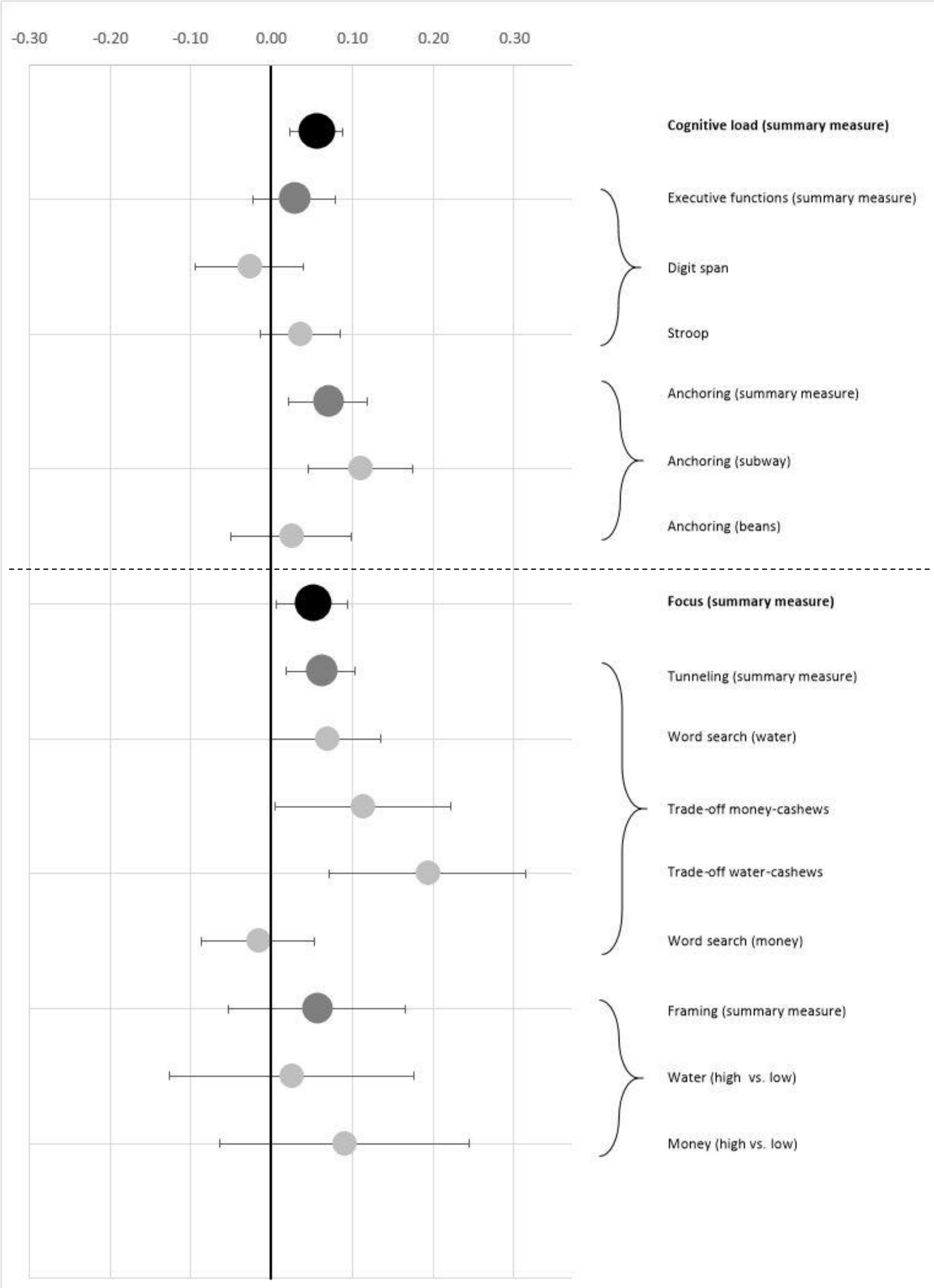
1. The figure displays coefficients and 90% confidence intervals for summary measures and for all individual outcomes under each category;

2. Effect sizes are defined as $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\widehat{\beta}_j}{\widehat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) based on Ordinary Least Squares (OLS) regressions with standardized dependent variables (z-scores) for the components of each outcome category; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure;

3. All outcomes are normalized such that a positive coefficient means worse performance for components of the cognitive load summary measure and better performance for components of the focus summary measure.

4. Since rainfall deviation from municipality's 30-year average is a continuous variable, we use the standard deviation of the control group for below-normal rainfall shocks to compute effect sizes for the summary indices.

**Figure 7 –** Effects sizes of priming on cognitive load and focus (90% confidence interval)



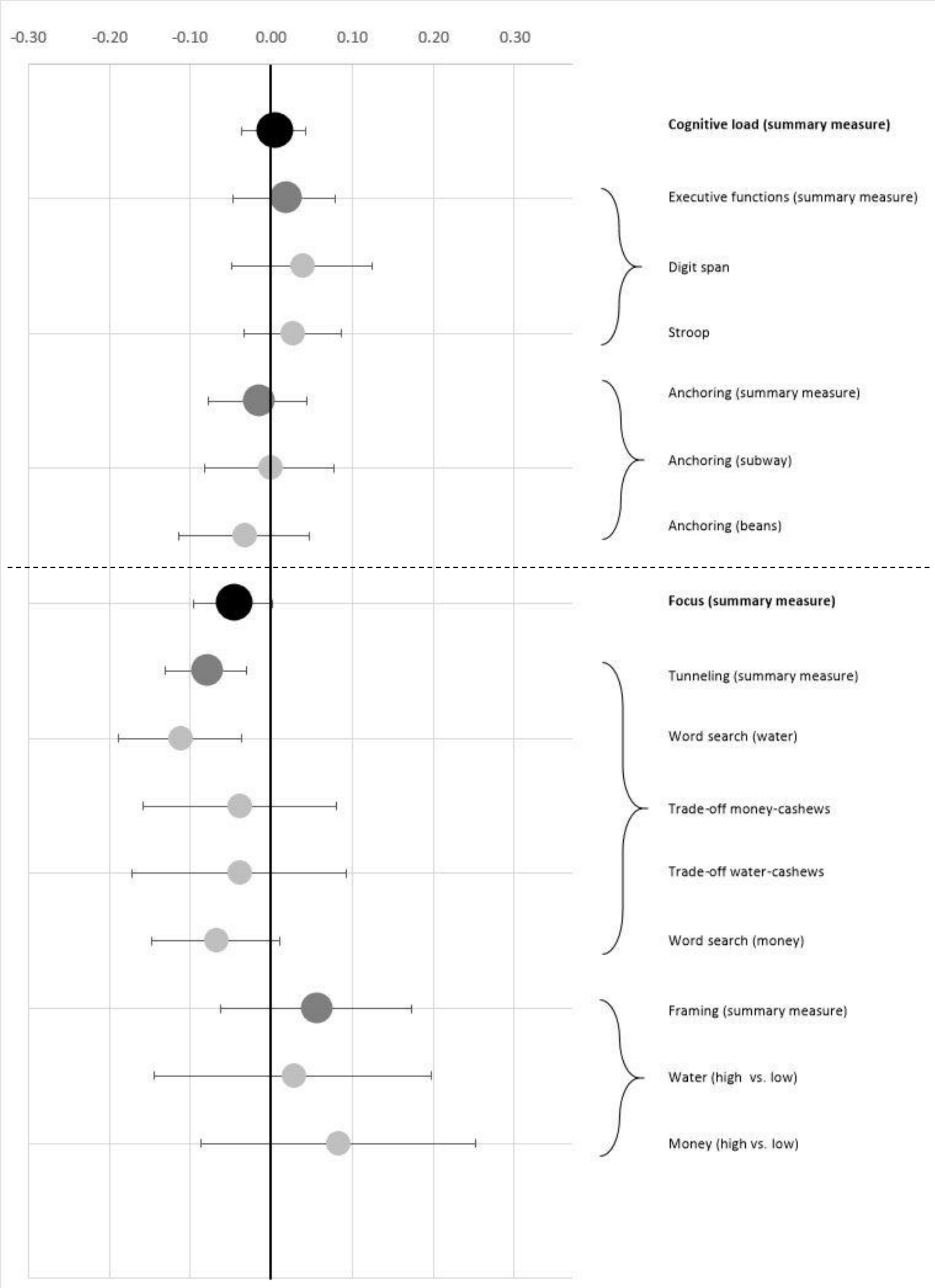

Notes on Figure 7:

1. The figure displays coefficients and 90% confidence intervals for summary measures and for all individual outcomes under each category;

2. Effect sizes are defined as $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) based on Ordinary Least Squares (OLS) regressions with standardized dependent variables (z-scores) for the components of each outcome category; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure;

3. All outcomes are normalized such that a positive coefficient means worse performance for components of the cognitive load summary measure and better performance for components of the focus summary measure.

**Figure 8 –** Effects sizes of insurance (ITT) on cognitive load and focus (90% confidence interval)




Notes on Figure 8:

1. The figure displays coefficients and 90% confidence intervals for summary measures and for all individual outcomes under each category;

2. Effect sizes are defined as $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) based on Ordinary Least Squares (OLS) regressions with standardized dependent variables (z-scores) for the components of each outcome category; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure;

3. All outcomes are normalized such that a positive coefficient means worse performance for components of the cognitive load summary measure and better performance for components of the focus summary measure.

**Appendix F – Tables**

**Table 1 – Descriptive statistics**

| Variable | | Sample mean |
|---|---|---|
| Respondent lives in municipality’s most drought-prone region | | 51.6% |
| Respondent is male | | 35.9% |
| Respondent’s age | | 34.0 |
| Respondent believes that rainy season will be good if it rains on March 19th | | 65.6% |
| Respondent’s plot is at least partly irrigated | | 15.2% |
| Respondent owns their property | | 31.7% |
| Respondent seeds cassava | | 21.4% |
| Number of rooms in respondent’s household | | 5.2 |
| Respondent’s household is a beneficiary of *Bolsa-Família* | | 77.8% |
| Respondent enrolled in Government insurance (*Garantia Safra*) | | 78.8% |
| Respondent’s average household income | | |
| | x < R$ 200 | 48.0% |
| | R$ 200 < x < R$ 400 | 38.7% |
| | R$ 400 < x < R$ 800 | 10.8% |
| | x > R$ 800 | 2.4% |
| Respondent’s schooling | | |
| | illiterate | 19.7% |
| | up to middle school | 46.5% |
| | high-school | 29.7% |
| | college | 4.0% |
| Respondent offered insurance (ITT) | | 29.5% |
| Respondent accepted insurance offer (treatment) | | 22.6% |

Notes on Table 1:

1. Summary statistics for variables collected at the baseline IVR (February).

**Table 2 – Effects of rainfall shocks and priming on worries about rainfall**

| | (1) worries (within cities) | (2) worries (across cities) | (3) worries (within cities) | (4) worries | (5) worries |
|---|---|---|---|---|---|
| Below-normal rainfall | 0.025 | 0.021 | | | - |
| | [0.0378] | [0.0428] | | | |
| Negative rainfall shocks | | | 0.0202 | | |
| | | | [0.0549] | | |
| Priming | | | | 0.0618* | -0.0306 |
| | | | | [0.0355] | [0.0490] |
| Below-normal rainfall x Priming | | | | | 0.182*** |
| | | | | | [0.0650] |
| Harvest loss (cubic polynomial) | No | Yes | No | No | No |
| Municipality fixed-effects | Yes | No | Yes | No | No |
| Wave fixed-effects | Yes | Yes | Yes | No | No |
| Municipality-wave fixed-effects | No | No | No | Yes | Yes |
| Observations | 3,781 | 3,781 | 3,781 | 3,871 | 2,529 |
| Number of clusters | 47 | 47 | 47 | 1,902 | 1,240 |
| R-squared | 0.016 | 0.043 | 0.029 | 0.119 | 0.117 |

Notes on Table 2:

1. All columns are regressions with standardized concern with rainfall (z-score) as dependent variable. See Appendix A for the definition of each variable;
2. Columns (1) to (5) are Ordinary Least Squares (OLS) regressions;
3. Robust standard errors in brackets, clustered at the municipality level for rainfall variables and at the individual level for priming and insurance;
4. For rainfall variables, due to the low number of clusters, p-values computed using t-statistics with G – 2 degrees of freedom, where G is the number of clusters (Angrist and Pischke, 2008);
5. *** p<0.01, ** p<0.05, * p<0.1;
6. $Corr(\text{worries, irrigation}) = -0.035$.

**Table 3 – Mean effect sizes of rainfall shocks and priming on summary measure of cognitive load**

| | (1) cognitive load | (2) cognitive load | (3) cognitive load | (4) cognitive load | (5) cognitive load |
|---|---|---|---|---|---|
| Below-normal rainfall | 0.022 | 0.041** | | | |
| | [0.0222] | [0.0190] | | | |
| Negative rainfall shocks | | | 0.046* | 0.045* | |
| | | | [0.0258] | [0.0228] | |
| Priming | | | | | 0.055*** |
| | | | | | [0.0198] |
| Harvest loss (cubic polynomial) | No | Yes | No | Yes | No |
| Municipality fixed-effects | Yes | No | Yes | No | No |
| Survey fixed-effects | Yes | Yes | Yes | Yes | No |
| Municipality-survey fixed-effects | No | No | No | No | Yes |
| Observations | 8,173 | 8,173 | 8,173 | 8,173 | 8,173 |
| Number of clusters | 47 | 47 | 47 | 47 | 1,950 |

Notes on Table 3:

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that positive values means worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring);
3. Columns (1) and (2) SUR are based Generalized Least Squares (GLS) municipality-level regressions with the number of respondents in each city, wave, and call as weights. Column (3) SUR is based on an Ordinary Least Squares (OLS) regression.
4. Bootstrapped standard errors in brackets, with 1,000 simulations, clustered at the municipality level for rainfall variables and at the individual level for priming and insurance;
5. *** p<0.01, ** p<0.05, * p<0.1;
6. $Corr$(cognitive load (May), harvest losses) $= 0.100$ s.d.;
7. $Corr$(cognitive load, schooling) $= -0.022$.

**Table 4 – Mean effect sizes of rainfall shocks and priming on summary measure of focus**

| | (1)<br>focus<br>enhancement | (2)<br>focus<br>enhancement | (3)<br>focus<br>enhancement | (4)<br>focus<br>enhancement | (5)<br>focus<br>enhancement |
|---|---|---|---|---|---|
| Below-normal rainfall | -0.015 | 0.005 | | | |
| | [0.0344] | [0.0308] | | | |
| Negative rainfall shocks | | | -0.001 | -0.003 | |
| | | | [0.0435] | [0.0336] | |
| Priming | | | | | 0.050* |
| | | | | | [0.0267] |
| Harvest loss (cubic polynomial) | No | Yes | No | Yes | No |
| Municipality fixed-effects | Yes | No | Yes | No | No |
| Survey fixed-effects | Yes | Yes | Yes | Yes | No |
| Municipality-survey fixed-effects | No | No | No | No | Yes |
| Observations | 9,370 | 9,370 | 9,370 | 9,370 | 9,370 |
| Number of clusters | 47 | 47 | 47 | 47 | 1,917 |

Notes on Table 4:

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for tunneling and sensitivity to framing; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that positive values means enhanced focus (higher relative valuation of scarce resources, and lower sensitivity to framing in decisions involving scarce resources);
3. Columns (1) and (2) SUR are based Generalized Least Squares (GLS) municipality-level regressions with the number of respondents in each city, wave, and call as weights. Column (3) SUR is based on an Ordinary Least Squares (OLS) regression.
4. Bootstrapped standard errors in brackets, with 1,000 simulations, clustered at the municipality level for rainfall variables and at the individual level for priming and insurance;
5. *** p<0.01, ** p<0.05, * p<0.1;
6. $Corr$(focus (May), harvest losses) $= 0.025$ s.d.;
7. $Corr$(focus, most drought-prone) $= 0.011$.

**Table 5 – Mean effect sizes of rainfall shocks and priming on summary measure of relative demand**

| | (1) relative demand | (2) relative demand | (3) relative demand |
|---|---|---|---|
| Below-normal rainfall | -0.203** | | |
| | [0.0934] | | |
| Negative rainfall shocks | | -0.0929 | |
| | | [0.0696] | |
| Priming | | | -0.0377 |
| | | | [0.0923] |
| Municipality fixed-effects | Yes | Yes | No |
| Survey fixed-effects | Yes | Yes | No |
| Municipality-survey fixed-effects | No | No | Yes |
| Observations | 867 | 884 | 884 |
| Number of clusters | 47 | 47 | 693 |

Notes on Table 5:

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\widehat{\beta}_j}{\widehat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for relative demand for production-related credit and insurance offers relative to its consumption equivalent; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that positive values means enhanced focus (higher relative valuation of scarce resources, and lower sensitivity to framing in decisions involving scarce resources);
3. Columns (1) and (2) SUR are based Generalized Least Squares (GLS) municipality-level regressions with the number of respondents in each city, wave, and call as weights. Column (3) SUR is based on an Ordinary Least Squares (OLS) regression.
4. Bootstrapped standard errors in brackets, with 1,000 simulations, clustered at the municipality level for rainfall variables and at the individual level for priming and insurance;
5. *** p<0.01, ** p<0.05, * p<0.1;
6. $Corr$(relative demand (June), harvest loss) $= -0.027$.

**Table 6 – Effects of insurance on worries about rainfall**

| | (1)<br>worries | (2)<br>worries | (3)<br>worries | (4)<br>worries |
|---|---|---|---|---|
| Insurance (ITT) | 0.0325 | -0.00149 | 0.0323 | 0.0157 |
| | [0.0394] | [0.0543] | [0.0545] | [0.0453] |
| Insurance (ITT) x Below-normal rainfall | | 0.0704 | | |
| | | [0.0717] | | |
| Insurance (ITT) x Priming | | | 0.00197 | |
| | | | [0.0760] | |
| Insurance (ITT) x Below-normal rainfall x Priming | | | | 0.0769 |
| | | | | [0.0812] |
| Priming | | | 0.0615 | -0.0306 |
| | | | [0.0452] | [0.0490] |
| Below-normal rainfall x Priming | | | | 0.155** |
| | | | | [0.0718] |
| Municipality-wave fixed-effects | Yes | Yes | Yes | Yes |
| Observations | 3,871 | 3,871 | 1,848 | 1,848 |
| Number of clusters | 1,902 | 1,902 | 1,063 | 1,063 |
| R-squared | 0.118 | 0.119 | 0.119 | 0.119 |

Notes on Table 6:

1. All columns are regressions with standardized concern with rainfall (z-score) as dependent variable. See Appendix A for the definition of each variable;
2. Columns (1) to (4) are Ordinary Least Squares (OLS) regressions;
3. Robust standard errors in brackets, clustered at the individual level;
4. *** p<0.01, ** p<0.05, * p<0.1;
5. $Corr(\text{worries, irrigation}) = -0.035$.

**Table 7 – Mean effect sizes of insurance on summary measure of cognitive load and focus**

| | (1)<br>cognitive load | (2)<br>focus enhancement |
|---|---|---|
| Insurance (ITT) | 0.003 | -0.047 |
| | [0.0237] | [0.0292] |
| Municipality-survey fixed-effects | Yes | Yes |
| Observations | 8,356 | 9,370 |
| Number of clusters | 1,950 | 1,917 |

Notes on Table 7:

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that positive values means worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring) and enhanced focus (higher relative valuation of scarce resources, and lower sensitivity to framing in decisions involving scarce resources);
3. Columns (1) and (2) SUR are based Generalized Least Squares (GLS) municipality-level regressions with the number of respondents in each city, wave, and call as weights. Column (3) SUR is based on an Ordinary Least Squares (OLS) regression.
4. Bootstrapped standard errors in brackets, with 1,000 simulations, clustered at the municipality level for rainfall variables and at the individual level for priming and insurance;
5. *** p<0.01, ** p<0.05, * p<0.1;
6. $Corr$(cognitive load (May), harvest losses) $= 0.100$ s.d.;
7. $Corr$(cognitive load (June), payout eligibility) $= -0.023$ s.d.;
8. $Corr$(focus (May), harvest losses) $= 0.025$ s.d.;
9. $Corr$(focus (June), payout eligibility) $= -\ 0.013$ s.d.

**Table 8 – Heterogeneity in cognitive load: interaction of insurance with proxies for high worries**

| | Cognitive load | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Proxy: | Below-normal rainfall | Primed about drought | Without Irrigation | Without Government insurance | Most drought-prone |
| Insurance (ITT) x proxy | 0.05 | 0.05 | -0.04 | -0.05 | 0.068* |
| | [0.0564] | [0.0415] | [0.0626] | [0.0627] | [0.041] |
| Insurance (ITT) | -0.02 | -0.02 | 0.05 | 0.01 | -0.04 |
| | [0.0408] | [0.0293] | [0.0565] | [0.0273] | [0.0298] |
| Proxy | - | 0.03 | -0.02 | -0.02 | - |
| | | [0.0246] | [0.0419] | [0.0384] | |
| Municipality-survey fixed-effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 9,444 | 19,143 | 13,847 | 13,913 | 19,143 |
| Number of clusters | 1,238 | 2,214 | 1,197 | 1,486 | 2,214 |

Notes on Table 8:

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) based on Ordinary Least Squares (OLS) regressions with standardized dependent variables (z-scores) for executive functions and anchoring; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that positive values means worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring);
3. *Without irrigation* equals 1 if farmer's plot is not even partially irrigated, and 0 otherwise; *Without Government insurance* equals 1 if the farmer is not covered by Garantia-Safra, and 0 otherwise; *Most drought-prone* equals 1 if farmer's plot is at the region most susceptible to droughts within the municipality, according to the extension authority, and 0 otherwise; *Below-normal rainfall* equals 1 if in the previous month farmer's municipality faced a rainfall level ranked among the 30% worst months in the last 30 years, and 0 otherwise; *Primed about droughts* equals 1 if the individual was experimentally primed, and 0 otherwise;
4. Bootstrapped standard errors in brackets, with 1,000 draws, clustered at the municipality level for rainfall variables and at the individual level for priming and insurance;
5. *** p<0.01, ** p<0.05, * p<0.1;
6. $Corr$(cognitive load (May), harvest losses) $= 0.100$ s.d.;
7. $Corr$(cognitive load (June), payout eligibility) $= -0.023$ s.d..

**Table 9 – Heterogeneity in focus: interaction of insurance with proxies for high worries**

| | Focus | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Proxy: | Below-normal rainfall | Primed about drought | Without Irrigation | Without Government insurance | Most drought-prone |
| Insurance (ITT) x proxy | -0.113 | -0.013 | 0.110 | -0.014 | -0.097* |
| | [0.0821] | [0.0567] | [0.0825] | [0.083] | [0.058] |
| Insurance (ITT) | 0.066 | -0.009 | -0.104 | 0.003 | 0.039 |
| | [0.0569] | [0.0423] | [0.0737] | [0.0375] | [0.0429] |
| Proxy | - | 0.056* | -0.101* | 0.006 | - |
| | | [0.0323] | [0.0547] | [0.0511] | |
| Municipality-survey fixed-effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 9,444 | 19,143 | 13,847 | 13,913 | 19,143 |
| Number of clusters | 1,238 | 2,214 | 1,197 | 1,486 | 2,214 |

Notes on Table 9:

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) based on Ordinary Least Squares (OLS) regressions with standardized dependent variables (z-scores) for tunneling and sensitivity to framing; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that positive values means enhanced focus (higher relative valuation of scarce resources, and lower sensitivity to framing in decisions involving scarce resources);
3. *Without irrigation* equals 1 if farmer's plot is not even partially irrigated, and 0 otherwise; *Without Government insurance* equals 1 if the farmer is not covered by Garantia-Safra, and 0 otherwise; *Most drought-prone* equals 1 if farmer's plot is at the region most susceptible to droughts within the municipality, according to the extension authority, and 0 otherwise; *Below-normal rainfall* equals 1 if in the previous month farmer's municipality faced a rainfall level ranked among the 30% worst months in the last 30 years, and 0 otherwise; *Primed about droughts* equals 1 if the individual was experimentally primed, and 0 otherwise;
4. Bootstrapped standard errors in brackets, with 1,000 draws, clustered at the municipality level for rainfall variables and at the individual level for priming and insurance;
5. *** p<0.01, ** p<0.05, * p<0.1;
6. $Corr(\text{focus (May), harvest losses}) = 0.025$ s.d.;
7. $Corr(\text{focus (June), payout eligibility}) = -0.013$ s.d.

**Table 10 – Interaction of insurance with average municipal trust**

| | (1)<br>worries | (2)<br>cognitive load | (3)<br>focus |
|---|---|---|---|
| Insurance (ITT) x Trust | -0.0961 | -0.149* | -0.139 |
| | [0.137] | [0.0879] | [0.104] |
| Insurance (ITT) | 0.0195 | -0.000544 | -0.0457 |
| | [0.0414] | [0.0261] | [0.0286] |
| Municipality-region fixed-effects | Yes | Yes | Yes |
| Observations | 16,758 | 8,356 | 9,370 |
| Number of clusters | 1,902 | 1,950 | 1,917 |
| R-squared | 0.131 | - | - |

Notes on Table 10:

1. Columns (1) is an Ordinary Least Squares (OLS) regression. Columns (2) and (3) are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) based on Ordinary Least Squares (OLS) regressions with standardized dependent variables (z-scores) for the components of each outcome category; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure. See Appendix B for the definition of each variable;
2. Outcomes are normalized such that positive values means worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring) and enhanced focus (higher relative valuation of scarce resources, and lower sensitivity to framing in decisions involving scarce resources);
3. Trust is defined as the municipality average of the z-score of subjects' transfer as player 1 or player 2 (respectively) of a conventional trust game (Berg et al., 1995), across all individuals in the first wave (March), considering only subjects that were not offered insurance and that were not primed at that survey;
4. Column (1) has robust standard errors in brackets, clustered at the individual level. Columns (2) and (3) have bootstrapped standard errors in brackets, with 1,000 draws, clustered at the individual level;
5. *** p<0.01, ** p<0.05, * p<0.1;
6. $Corr$(worries, irrigation) $= -0.035$;
7. $Corr$(cognitive load (May), harvest losses) $= 0.100$ s.d.;
8. $Corr$(cognitive load (June), payout eligibility) $= -0.023$ s.d.;
9. $Corr$(focus (May), harvest losses) $= 0.025$ s.d.;
10. $Corr$(focus (June), payout eligibility) $= -\ 0.013$ s.d.