Cognitive Droughts[‡]

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ABSTRACT: We study whether poverty impedes cognitive function, distinguishing across the effects of different types of income shocks. We draw on survey experiments and exposure to rainfall variation among poor farmers in Brazil to test the effects of the *salience* and *occurrence* of *unanticipated* shocks on cognitive function, contrasting those to the effects of random variation in their cash transfer payday, an *anticipated* shock with *potentially no effect* on permanent income. We find that the former generate *cognitive load* – worse performance in attention, memory and impulse control tests – and *tunneling* – better relative performance in tasks involving scarce resources –, *even before* unanticipated shocks actually materialize. In contrast, payday variation does *not* systematically generate cognitive load; it only induces tunneling. Together, those findings indicate that risk exposure and risk materialization drive poverty's psychological tax. That is not to say that payday variation has no psychological effects on the poor: distance to payday *does* cause cognitive load within the poorest municipalities; more broadly, we show that attentional shifts triggered by both types of shocks generate inefficient focus when the stakes involved are high, even outside of our experiments.

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1 Introduction

Risk is a central dimension of the lives of the poor in the developing world.[1] While rational responses to risk have been extensively studied, its psychological consequences have been overlooked.[2] This matters, since the effects of risk on decision-making, operating through risk-aversion, do not capture how it can have pervasive effects on decisions – even for those outside the specific domain of risk, and even before shocks actually materialize –, by creating worry and impairing attention, memory and impulse control, particularly among the poor. This paper studies whether the psychological effects of risk *exposure* and risk *materialization* – respectively, the *salience* and the *occurrence* of unanticipated shocks to expected income – impede cognitive function among the poor, distinguishing its effects from those of payday variation (the focus of a growing literature, e.g. Carvalho, Meier and Wang, 2016; Kaur et al., 2019).

It is widely acknowledged by economists that anticipated and unanticipated income shocks generate differential responses: if subjects can perfectly smooth consumption, anticipated shocks should generate smaller responses at the time of occurrence than otherwise identical unanticipated shocks (Friedman, 1957; Jaimovich, 2017). That should hold even among the poor – who face liquidity constraints and market imperfections –, as long as *some* consumption smoothing is feasible.

For this reason, we hypothesize that the psychological effects of unanticipated shocks should be larger than those of otherwise identical anticipated shocks. The threat of a one-time shock that could derail a family and drag it into a downward spiral is likely to take a huge psychological toll on the poor, above and beyond the decision-making challenges of making do on a meagre income. Furthermore, we hypothesize that such psychological effects might take place even *ex-ante*, before unanticipated shocks actually hit – and, in fact, even if it turns out that risk *does not* materialize moving forward.

In their influential book *Scarcity*, Mullainathan and Shafir (2013) provide a persuasive picture of this *psychological tax* of poverty on mental bandwidth and decision-making. While every individual is occasionally hit by shocks of different natures, when the *poor* are hit by negative income shocks, those make them engage in counterfactual thinking: what can they no longer buy now that resource constraints are even tighter? In psychological terms, negative income shocks that affect the poor are equivalent to a *relative price increase* of allocating mental bandwidth to decisions that do *not* involve scarce resources. Since mental budget is constrained – just like monetary budget is –, the effects of such shocks can be framed

[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 rise almost one-to-one with the GDP share of agriculture (Benson and Clay, 1998).

[2] Caplin and Leahy (2001) develop a theoretical model of how feelings of anticipation and anxiety might affect intertemporal decisions. Testing such mechanism empirically is challenging, and that is the evidence gap we refer to.

in a way familiar to economists. First, an *income effect*: scarcity lowers the mental bandwidth available for *all* decisions – an effect known in the cognitive psychology literature as *cognitive load*. Second, a *substitution effect*: scarcity makes it relatively cheaper to allocate mental resources to decisions involving scarce resources, an effect known as *tunneling*.[3] Together, those income and substitution effects presumably deteriorate the quality of decisions among the poor, by inducing subjects to overlook fundamental aspects of the problem or to focus too narrowly on short-term consequences.

Empirical evidence for these cognitive impacts of poverty, however, is mixed. Findings that poverty significantly impairs subjects' decisions in the lab (Shah, Mullainathan and Shafir, 2012, 2015) and farmers' cognitive function in the field (Mani et al., 2013) contrast with those from a recent experiment in the US in which it does not seem to affect cognitive function among low-income respondents (Carvalho, Meier and Wang, 2016). We believe that this apparent inconsistency in the previous empirical evidence is due to having overlooked conceptual nuance in the psychological effects of poverty on two counts, which we address in this paper.

First is the distinction between anticipated and unanticipated income shocks. The experimental design in Carvalho, Meier and Wang (2016) involves an anticipated income shock – only subjects who knew exactly when they were going to be paid, and how much, were included in the study. This is in contrast to the farmers studied in Mani et al. (2013), for whom the shock was unanticipated: all farmers were uncertain about when they would get paid (including whether they would be paid at all), and some were uncertain about how much they would get paid as well. When a shock is perfectly anticipated – as with payday variation, which on top might not affect permanent income unless in combination with other market imperfections[4] –, it is not obvious how it should affect cognitive load. On the one hand, it could generate anxiety from having to manage with a meagre income until payday – a negative emotion that could tax mental bandwidth. On the other hand, it could generate positive emotions such as hopefulness or anticipation of an imminent rise in income, acting in the opposite direction (as in Caplin and Leahy, 2001).

Second, while limited mental bandwidth predicts that poverty should induce both *cognitive load* and *tunneling* on scarce resources, Carvalho, Meier and Wang (2016) and Mani et al. (2013) do not measure

[3] Even though these effects are framed as *top-down* rational responses to changes in the relative price of allocating attention to different tasks, evidence from lab experiments shows that those are at least partly *bottom-up*, since *tunneling* is triggered even by stimuli at speeds below the consciousness threshold (Mullainathan and Shafir, 2013).

[4] For instance, if subjects can only borrow at a higher interest rate than the one on their savings, otherwise transitory shocks can have permanent effects on their income.

the latter. In doing so, they potentially overlook poverty's psychological tax operating through excessive focus on short-term scarcity.[5,6]

We contribute to this literature by examining poor farmers' psychological responses to both unanticipated *and* anticipated income shocks, and by capturing the psychological effects of poverty through both cognitive load *and* tunneling. What is more, we explore variation in both the salience *and* the occurrence of unanticipated shocks, allowing us to study both its *ex-ante* and *ex-post* psychological effects. Last, we do so across several locations with different income levels, allowing us to test whether the effects of each shock are concentrated on the poor.

To understand the effects of unanticipated shocks, we use two distinct sources of exogenous variation in farmers' expected income (or in their worries about it). On the one hand, variation in the *occurrence* of those shocks: natural rainfall variation over the course of the rainy season, across 47 municipalities of Brazil's drought-prone State of Ceará. On the other hand, variation in their *salience*: lab-in-the-field survey experiments with over 2,800 farmers, exposing some of them to questions that induce drought-related worries (what the cognitive psychology literature calls *priming*).

Combining these two sources of variation allows us to study both the ex-ante and ex-post effects of unanticipated shocks. Rainfall variation is the canonical example of risk in Development Economics, for both its unpredictability and its substantial effects on wages, income, and consumption. Recent rainfall shocks provide farmers with signals about the rainy season and future harvest, and so should affect their expectations about future income levels – making it a suitable unanticipated shock for our purposes. Rainfall shocks are likely to induce other responses, such as differential sleep or nutrition, which presumably magnify the effects of the *materialization of risk* on the psychological mechanism of interest. In turn, individual-level survey experiments may lack the external validity of actual income shocks, but can nail the *salience* mechanism precisely, as all cognitive measurements in our study take place within five minutes from the priming.

[5] Shah, Shafir and Mullainathan (2012) offers evidence of inefficiency arising from tunnelling in a laboratory setting. Lichand et al. (2019) shows that such effects are also present in the field: priming poor parents about financial worries induces them to focus on short-term returns at the expense of returns of an educational investments, leading to decrease willingness to invest in their children's education as predicted returns increase.

[6] Tunneling is a cleaner way of capturing the psychological consequences of poverty when participants' performance on cognitive tasks is incentivized. This is so because, in the presence of tunneling on short-term resources, short-term incentives are expected to *improve* cognitive performance (at the expense of performance in other tasks). That effect can make it harder to detect cognitive load through incentivized tests, as in the 'Wheel of Fortune' self-replication in Shah, Shafir and Mullainathan (2019), where primed subjects make more money in the incentivized experiments, Lichand et al. (2019), where primed subjects make more money in short-term incentivized attention and memory tasks, and Kaur et al. (2019), where workers primed about financial strain increase productivity in peace-meal payment tasks. In contrast, such effect of incentives should make it *even easier* to detect tunneling.

Next, for anticipated shocks, we use monthly payday variation in *Bolsa Família*, Brazil's flagship conditional cash transfer (CCT) program. Since Bolsa-Família has been in place for over 10 years at the time of data collection, and since beneficiaries are paid every month following a publicly available payday schedule, distance to payday should be *perfectly anticipated* at the time of the survey. We take advantage of the fact that Bolsa-Família's payday at each month is determined entirely by the last digit of each beneficiary's social security identification number (*Número de Identificação Social*, or NIS), which is *randomly assigned*. Hence, at the time of the survey, distance to payday is as good as random. Last, since Bolsa Família's monthly payments are about a third of the typical market value of family farmers' harvest in the region, and since average harvest losses over the previous 5 years were about a third, it turns out that, in our study, CCT payments have about the same expected value as farmer's harvest. As such, with very imperfect opportunities to smooth consumption[7], being close to payday should approximate an otherwise identical shock to the prospect of losing one's harvest – except that the former is anticipated, and might not affect permanent income unless in combination with other market failures.

Results are as follows. Starting with the psychological impacts of unanticipated shocks, we find that both priming and actual rainfall shocks (summarized by a linear combination of rainfall variables most predictive of worries about rainfall, picked using machine learning techniques) generate significant cognitive load and tunneling. Effect sizes are large: for cognitive load, they are equivalent to those of losing about 25% of one's harvest, at the end of the rainy season, or to downgrading a farmer from high school back to elementary school (in a cross-sectional comparison). For tunneling, the effect induced by priming is equivalent to that generated by the absence of rainfall in farmer's municipality 3 days prior to the survey. Rainfall risk not only improves relative performance, but also significantly reduces *reaction times* in tunneling tasks, consistent with the idea that it makes scarce resources *top-of-mind*. The two sources of variation differ in the timing of their effects. The effects of priming on worries and cognitive load peak early in the rainy season – before uncertainty unfolds, consistent with the *exposure* mechanism –, while those of negative rainfall shocks become larger over time – as hopes that they might still be reversed vanish, consistent with the *materialization* of risk.

In turn, payday variation does *not* systematically increase cognitive load. Having said that, there is clear evidence that it generates tunneling, with a sharp surge in effects close to payday. The effect of being within 3 days from Bolsa-Família's payment on tunneling is about 2/3 as that induced by negative rainfall shocks within that sample.

[7] Evidence from liquidity constraints comes from farmers' responses to opportunities to listen to information about credit and insurance offers during our phone surveys; opt-in rates are extremely high, of the order of 80%.

The fact that payday variation does not cause cognitive load *on average* does not mean it causes no psychological impacts on the poor, nonetheless. Taking advantage of variation in per capita income across the 47 municipalities where we conduct our study, we find that the effects of distance to payday *do* vary significantly with income. Payday variation turns out to significant generate cognitive load *within the poorest municipalities* – where CCT payments are much higher stake, and where anticipated shocks are much more likely to affect permanent income in combination with other market imperfections. The effects of unanticipated shocks are also concentrated on the poor. The fact that all shocks generate psychological effects concentrated on the poor rules out that their differential effects are merely driven by the loss of the power of certainty (Martínez-Marquina, Niederle and Vespa, 2019) – shown to impair contingent reasoning more generally. Rather, it corroborates that the psychological effects we document are triggered by scarcity thinking.

Together, those findings indicate that the effects of risk exposure and risk materialization drive poverty's psychological tax; among the poorest, however, all shocks drive cognitive load and tunneling. In face of those results, Mani et al. (2013) and Carvalho, Meier and Wang (2016) are not inconsistent after all. The former captures the effects of an unanticipated income shock, while the latter only documents the effect of an anticipated income shock within a sample in the US, not nearly as poor as the poorest municipalities in our sample or as the sample of the study in India.

What is more, we have shown that anticipated shocks generate tunneling – a dimension Carvalho, Meier and Wang (2016) does not measure. Consistent with the evidence from heterogeneous treatment effects that payday variation deteriorates decision-making when stakes are high, it turns out that tunneling effects are *magnified* in face of incentives. We provide a non-linear structure of incentives, granting the top-25% scores in our phone surveys additional airtime credit. Before payday, while both average *and* top performers do better on tasks involving scarce resources (earning significantly *more* money on those tasks), top performers actually do *worse* on other tasks (earning significantly *less* money), consistent with mental bandwidth reallocation in the range of performance for which incentives matter.

Do attentional shifts triggered by tunneling generate costs also outside of our experiments, in line with evidence from previous studies (Shah, Shafir and Mullainathan, 2015; Lichand et al., 2019)? We test this hypothesis by assessing whether rainfall shocks distort how subjects recall information, via face-to-face surveys in which we ask farmers to provide their best guesses of two real-world quantities: the number of rainy days in their municipality in the previous month, and the volume of water in their rain-fed water tanks. Both guesses could be verified by enumerators. The key insight is that knowledge about the number of rainy days in the previous month is *not consequential* to farmers in this region (given soil absorption properties), whereas knowledge about water left in their tanks is vital for both production and survival.[8] We find that recent negative rainfall shocks (when the previous month ranks among the 1/3 worst within the last 30 years in rainfall volume) *decrease accuracy* of farmers' recall for both guesses – consistent with cognitive load. Moreover, the larger the actual number of rainy days or the water volume in one's tank, the *lower* one's accuracy is (consistent with Weber's law; see Khaw, Li and Woodford, 2017). Negative rainfall shocks *attenuate* this effect for farmers' recall of rainy days in the previous month – consistent with tunneling –, but *not* for their recall of how much there is in their water tank.

These findings are important for two reasons. First, they illustrate that shocks that generate cognitive load and tunneling can have *real* consequences, by adversely affecting farmers' recall of information relevant for decision-making. Second, they illustrate that tunneling can be *inefficient*, by allocating mental resources to dimensions of scarcity that are salient, at the expense of information that is more consequential – consistent with the increase in workers' mistakes before payday in Kaur et al. (2019). Both anticipated and unanticipated shocks induce inefficient tunneling, making farmers 'penny wise and pound foolish'.

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 the State of Ceará in subsection 2.1. Next, subsection 2.2 describes the enrollment process and the matching procedure with Bolsa-Família's cadastre to recover CCT paydays. Subsection 2.3 follows with a detailed timeline of enrollment, data collection and the most important milestones in what comes to the rainy season, government insurance and production decisions in Ceará. Last, we provide details about our phone survey methodology 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 in the 4 years prior to the experiment. In an extreme year such as 2013, all municipalities except the State capital, Fortaleza, declared state of emergency, triggering 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. This setting generates a great deal of

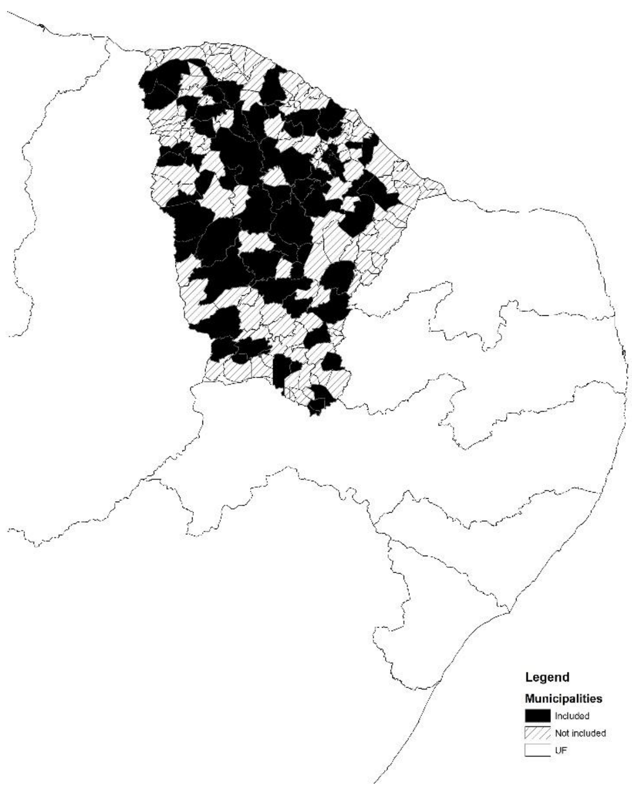
[8] Under soil conditions available in Ceará – with little to no water absorption properties – for most municipalities optimal seeding models depend almost exclusively on current and future expected rain, not on *past* rainfall (Andrade, 2005).

anxiety and mysticism linked to rainfall forecasts (see Taddei, 2013, for a detailed anthropological account), making it a promising environment in which to study the psychological effects of risk.

2.2 Enrollment and matching with CadÚnico

In partnership with Ceará's Rural Development Secretariat, we enrolled 4,084 farmers across 47 municipalities of the hinterlands of the State, in January 2015. Extension workers in each municipality received 100 consent forms to be handed to the family farmers they oversee, and through which farmers who opted-in to participate in the study informed 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 assignment in the survey experiment.

Figure – Geographic coverage of the surveys



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 C presents detailed balance and selective non-response tests. Table C1 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 took at most 8 calls.

[Table C1]

For 96.4% of farmers in our sample, we are able to link their family famer's ID (*Declaração de Aptidão ao PRONAF*, DAP) to their unique social information number (*Número de Informação Social*, NIS) with the help of the Ministry of Rural Development. For every successful match, we were granted information from CadÚnico, Bolsa-Família (Brazil's flagship conditional cash transfer program, in place since 2003)'s administrative cadastre from the Ministry of Social Development, such that we could verify whether that household was actually receiving CCT payments at the time.

Payday for each matched household depends on the last digit of NIS for Bolsa-Família's *main beneficiary*. For this reason, we cannot assign payday simply based on the NIS of our matched subjects, as someone else in the household (e.g. the spouse or an elderly household member) might be the one on which the payment schedule is based. Even though CadÚnico lists all NIS's for each matched household in our sample (that of the main beneficiary, and that of the alternate, if available), we do not have information on which one is that of the main beneficiary. To assign it, we resort to the following procedure. Whenever there is only one NIS in the household, assignment is straightforward. Conversely, if there are two, we apply an algorithm to identify female Brazilian first names[9], as women are primarily the main beneficiaries of Bolsa-Família (for 92% of the households, see Bartholo, 2016). Last, if the algorithm identifies either no or both female first names, assignment picks the first NIS listed for that household in CadÚnico. Doing so yields 1,035 subjects in households receiving Bolsa-Família (36.7% of our sample) with information about Bolsa-Família's payday.

Distance to payday varies by call and month (exact dates are shown in Table C3). Payments always take place in the last 2 weeks of the month (other than weekends and holidays). The exact dates of our phone surveys varied a bit month-by-month for logistical reasons. Table C4 presents the distribution of distance to payday in our sample, by survey wave.

[Table C4]

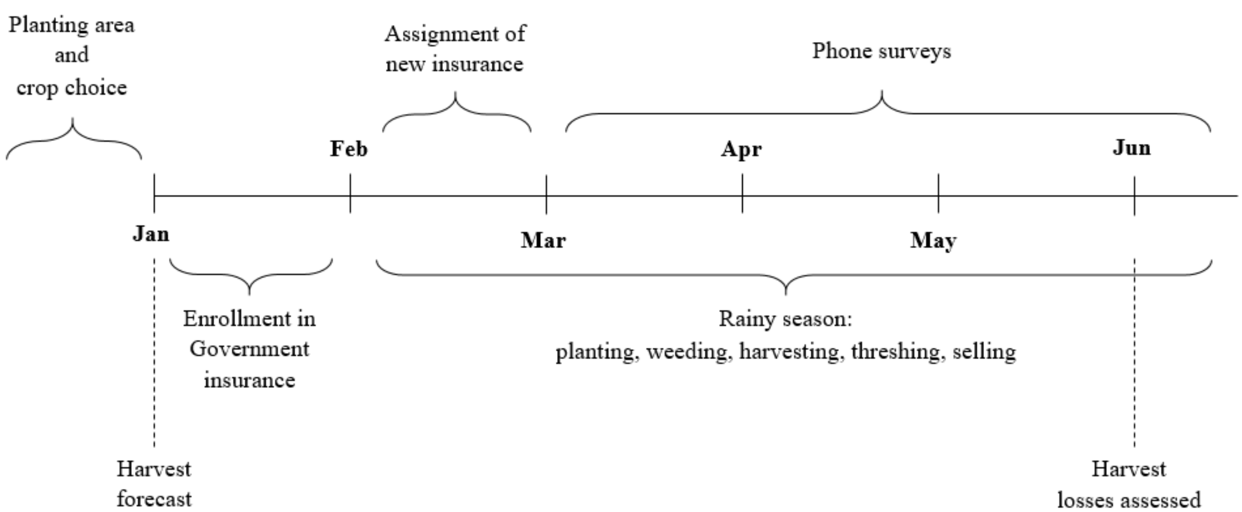
[9] https://github.com/meirelesff/genderBR

Taking all waves together, 33.2% of observations within the Bolsa-Família sample are within a week of payday (either before or after). Density is enough to allow us detecting similar effects sizes to those we document for priming and rainfall shocks in the full sample within reasonable windows from payday.

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.

Figure – Timeline



In an accompanying paper (Lichand and Mani, 2019), we offer a new index insurance product to part of our sample, assigned over the first two weeks of February. We collected baseline information at the same time 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.

Conducting 24 rounds of lab-in-the-field experiments across almost 50 different towns in the hinterlands of Brazil is a non-trivial logistical challenge. We take advantage of the fact that almost all poor Brazilian households have access to cell phones to run such experiments via phone surveys (interactive voice response units, or IVR). 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 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).[10] 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]

We run each call over the course of 2 days, to obtain response rates in line the sample size requirements informed by power calculations. Whether subjects pick the call on day 1 or 2 of each call is endogenous, and can have consequences for the effects we estimate for rainfall shocks or payday variation. Since we find that rainfall shocks do affect survey response rates, to avoid endogeneity we assign all subjects to the first day of the call, in the spirit of an intention to treat design in what comes to rainfall shocks and payday variation. In the Supplementary Appendix we show that results are robust to alternatively assigning subjects to the second day of the call.

[10] Those new tests were validated in the field through face-to-face surveys; results are shown in the Supplementary Appendix.
[11] The expected hourly wage from taking all surveys is USD 3.25, about four-fold the average hourly wage reported by our sample.

3 Experimental Design and Estimation

This section starts with a conceptual framework for the psychological effects of risk on cognitive function, in subsection 3.1. Subsection 3.2 discusses the experimental design. Next, subsection 3.3 describes the main outcomes we use to measure cognitive load and tunneling. Last, subsection 3.4 gives details on the econometric procedures we use to estimate the effects of interest, deal with standard errors and account for multiple hypotheses testing.

3.1 Conceptual framework

Psychological theories consider a variety of mechanisms other than risk aversion for how risk may affect decision-making. One such mechanism is anticipation and dread (Elster and Loewenstein, 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. This theory predicts time inconsistency: as information about risk realization unravels, anxiety disappears, and present selves would like to revise "over-cautious" past decisions.

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).

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.

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 mental bandwidth hypothesis (Mullainathan and Shafir, 2013; Schilbach, Schofield and Mullainathan, 2016; Dean, Schilbach and Schofield, 2019), which posits that individuals worrying about scarcity suffer consequences of two sorts. First, an income effect: worries act as a distraction or as cognitive load. This effect predicts lower attention and memory, and increased susceptibility to biases. Second, a substitution effect: by making scarce resources top-of-mind, worries generates tunneling. This effect predicts relatively better performance in tasks involving scarce resources, and lower susceptibility to biases in trade-offs involving those resources – potentially at the expense of performance in all other tasks.[12]

While this mechanism speaks to the effects of facing low income *levels*, we conjecture that not only such mechanism should extend to risk, but also that this should be the main source of the psychological effects of poverty on decision-making. This is the first paper to test the mental bandwidth hypothesis for the effects of income risk among the poor, separately for the effects of higher risk exposure (through higher salience of its consequences) and for those of risk materialization (when shocks actually hit).

We hypothesize that both effects generate cognitive load tunneling (in particular, lower sensitivity to framing in trade-offs between scarce resources and time; Shah, Shafir, and Mullainathan, 2015). We also hypothesize that the effects of risk on cognitive function are higher than those of payday variation, an anticipated income shocks with potentially no effect on permanent income.

3.2 Experimental design

The ideal experiment would independently randomize (1) risk *exposure*, (2) risk *materialization* – the extent to which shocks actually hit –, and (3) payday variation across participants. Moreover, to nail whether the effects of interest are confined to the poor, it would stratify randomization by income levels.

In what comes to risk exposure, even though it is impossible to randomly assign farmers to different rainfall risk, it is possible to randomize *worries* about droughts, in the spirit of mechanism experiments (Ludwig, Kling and Mullainathan, 2012). We approximate the ideal experiment through survey experiments that make some farmers, but not others, worry about the possibility of droughts within each survey (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.

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

[12] While this mechanism if essentially cognitive (while Loewenstein et al., 2001, emphasize the non-cognitive effects of risk), the effects of risks operating through mental bandwidth could be modelled as a specialization of the “risk as feelings” hypothesis.

across surveys in order to avoid systematically inducing a particular emotional state in the control group (Lerner et al., 2014).

In what comes to risk materialization, we exploit natural experiments linked to recent rainfall shocks; such shocks are ordinarily viewed in Economics as being as good as random. Rainfall variation affects seeding opportunities and land productivity in Ceará, mapping directly into expected income. Naturally, such shocks are also expected to prime subjects about risk exposure, providing farmers with signals about the rainy season and future harvest, and as such should also affect worries about future rainfall. Combining rainfall variation with survey experiments allows us to estimate the psychological effects of risk exposure and those of risk materialization separately, by comparing their effects in isolation and in interaction.

In face of a multiplicity of rainfall shocks to choose from, we resort to a data-driven selection procedure. We regress worries about rainfall (see Appendix A) on 51 features of rainfall over the course of the last 30 days in each municipality, from the occurrence and levels of rainfall at different days prior to each survey, to cumulative rainfall and deviations from historical averages. We include municipality fixed-effects to net out variation linked to other characteristics in local climate that is not randomly assigned. All rainfall variables derive from Ceará's official monthly rainfall data, collected by local meteorological stations for each municipality over the past 30 years.[13]

Using LASSO to trade-off goodness-of-fit against over-fitting, the algorithm picks the nine variables most predictive of rainfall. We then build a post-LASSO summary measure of negative rainfall shocks (which we call *No rainfall summary measure*), weighting each predictor by their coefficient in the LASSO regression (see Table C2 for the list of variables picked by the algorithm).

In most regression tables, we also highlight the effects of two of those predictors separately, namely, no occurrence of rainfall *3 days before* and *7 days before* the survey. The reason is two-fold. First, as we show, rainfall shocks generate selective non-response. However, Lee (2009)'s bounds – a correction procedure for selective attrition – can only be applied to binary variables. Second, out of the nine predictors that LASSO picks, those are the two variables with direct counterparts in payday windows (within 3 and 7 days, respectively), which we also look at separately as a useful benchmark.

For robustness checks and in the context of our face-to-face surveys, we also define a typical indicator of droughts used in the literature, 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.

[13] 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, we cannot explore information at finer aggregation levels.

Finally, in what comes to payday variation, we resort to random payday variation in Bolsa-Família's payday. The schedule of CCT payments is randomly assigned, based on the last digit of the NIS, and publicly available at the Ministry of Social Development website. That procedure is in place to avoid over-crowding banks and other cash collection points at payday, were the transfer paid to all beneficiaries at once (Kaufmann, La Ferrara and Brollo, 2012).

Combining random payday variation with our phone surveys allows us to accurately control the time of at which participants take the survey, making distance to payday as good as random in our study, an advantage with respect to the study design in Carvalho, Meier and Wang (2016).[14]

What is more, since the program had been in place for over 10 years at the time of the experiment, and since individual paydays are public knowledge and randomly assigned, this is a great setting to study the effects of anticipated income shocks together with those of unanticipated shocks. In contrast, Kaur et al. (2019) cannot completely rule out confounding drivers of differences in performance across groups before and after payday, such as reciprocity or experimenter demand effects.

Since Bolsa Família's monthly payments at the time were about a third of the typical market value of family farmers' harvest in the region, and since average harvest losses over the previous 5 years were about a third, it turns out that, in our study, CCT payments have the same expected value as farmer's harvest. As such, with very imperfect opportunities to smooth consumption, being close to payday should approximate an otherwise identical shock to the prospect of losing one's harvest – except for the fact that the former is anticipated.

We analyze three different variables to capture the effects of payday variation: a linear measure of distance to payday (ranging from -15 to 15; see Table C4), and indicator variables of payday *within 3 days* and *within 7 days*, as mentioned. In all regressions that use those indicator variables, we compare subjects not paid within that window only to subjects already paid since that same window, i.e. those paid *since 3 days or less* and *since 7 days or less* at the time of the survey, respectively.

Last, it is worth highlighting we can use municipal-level harvest losses to benchmark the effects of income shocks. 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

[14] Respondents in Carvalho, Meier and Wang (2016) could choose when they started and completed their online follow-up survey within the 7-9 day window around payday. Given that these choices are potentially endogenous to financial pressure, the study design in that paper does not make it feasible to measure the cognitive impact of their payday shock by distance to respondents' payday.

predictions account for all information available before the rainy season (including planting area and crop choices), harvest losses can be considered randomly assigned.

With the exception of the survey experiment, while the variation we explore was not stratified by income levels, we can estimate heterogeneous treatment effects of each shock by municipality's per capita income (from the Brazilian Institute of Geography and Statistics' 2010 Census) to gauge whether their effects are specific to the poor or, rather, hold more generally. While farmers selected to be part of the study are all poor, the extent to which they can access resources should correlate with that variable.

3.3 Outcomes

Cognitive outcomes are organized into two categories, in line with the mental bandwidth theory.[15] 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 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. We have validated the versions of digit span and stroop we create to be ran over IVR using face-to-face surveys which draw upon the typical tests used in the literature, adapted from Mani et al. (2013); see the Supplementary Appendix.

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

[15] We have pre-registered the study at AEA Social Sciences RCT Registry, specifying how different outcomes would be grouped into cognitive load and focus.

(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.[16]

The second category is tunneling, 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). Tunneling 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 the effects are driven by the psychology of scarcity, through reallocation of mental bandwidth.[17]

We measure focus (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. Focus 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 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

[16] 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).
[17] 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.

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.

We also analyze the effects of each source of variation on subjects' reaction times – accurately captured through the IVR technology – within each outcome category, as scarcity has been shown to deteriorate both accuracy and reaction speed in previous studies (e.g. Mani et al., 2013). Last, we analyze the effects of each shock on money earned in the experiments to study the effects of incentives, taking advantage of a non-linear reward structure we set in place, providing additional airtime credit to the top-25% performers in each call (computed within the outcomes that could be scored), within each outcome category.

We complement phone surveys with face-to-face surveys conducted by State extension workers, described in detail in Section 7.

3.4 Estimation and summary measures

For each outcome, we estimate the empirical counterparts of β_j in equations (1), (2), and (3), where each outcome Y^j is indexed by municipality m, individual i and survey t:

$$\begin{cases} Y_{mit}^{j} = \alpha + \theta_m + \beta_j^1 Priming_{mit} + u_{mit} & (1) \\ Y_{mit}^{j} = \alpha + \theta_m + \beta_j^2 Rainfall_{mt} + u_{mit} & (2) \\ Y_{mit}^{j} = \alpha + \theta_m + \beta_j^3 Payday_{mit} + u_{mit} & (3) \end{cases}$$

In equations (1) to (3), α is a constant term; θ_m stand for municipality fixed-effects and survey fixed-effects; $Priming_{mit}$ equals 1 if individual i was primed at survey t, and 0 otherwise; $Rainfall_{mt}$ is a measure of negative rainfall shocks at municipality m before survey t; $Payday_{mit}$ is a measure of distance to payday for subject i at survey t; and u_{mit} is an error term. We cluster standard errors at the individual level, in order to account for potential serial correlation in residuals. Following Belloni et al. (2012), we use conventional standard errors for the effecs of the post-LASSO *no rainfall summary measure*.

While we could include wave fixed effects in all specifications to increase precision, it actually dampens a lot of the rainfall variation over the course of the rainy season. In fact, LASSO does not pick *any* predictor of worries about rainfall in the presence of municipality and wave fixed fixed-effects. Moreover, while the structure of our data would allow for including individual fixed-effects or even individual-survey fixed-effects; our panel is very unbalanced: many subjects do not respond to the same call at different waves (see Table C2). 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 (4):

$$\hat{\beta} = \frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}} \tag{4}$$

In equation (4), $\hat{\beta}_j$ 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.

4 Do risk exposure and materialization cause cognitive load and tunneling?

This section estimates the effects of priming and rainfall shocks on farmers' cognitive function. We start by discussing balance and non-response in subsection 4.1. Subsection 4.2 presents the effects of each shock on worries about rainfall, followed by their effects on cognitive load in subsection 4.3, and on tunneling in subsection 4.4. Robustness checks are summarized in subsection 4.5. Last, subsection 4.6 compares the effects of risk exposure – captured by priming – to those of risk materialization – captured by rainfall shocks –, concentrating on how those shocks interact, and on their patterns over the course of the rainy season and across different components of cognitive function summary measures.

4.1 Balance and non-response

We start with descriptive statistics of our sample, analyzing whether participants' characteristics collected at baseline are balanced across treatment conditions for priming and rainfall shocks. Table D1 showcases that about 2/3 of subjects enrolled in our study are female, averaging 35 years old. Most participants indeed rely exclusively on rainfall for agriculture – less than 14% of them have access to irrigation. Rain-fed irrigation, combined with the irregular rainfall regime in the region, sustains motivated beliefs about what determines a good rainy season: about 2/3 of farmers believe that the rainy season will be good if it rains on March 19th, the day of Ceará's patron saint, even though this rule of thumb is wrong about 70% of the time.[18] Slightly under 1/3 of participants own their plot, and only about 20% of them harvest cassava – a higher value cash crop, which proxies for market-oriented farming. Almost 80% of our subjects report to be enrolled in Bolsa-Família, and a similar share reports to have signed up for Government index insurance (which pays out if harvest losses in the municipality are 50% or higher, see Lichand and Mani, 2019).

[Table D1]

Even though priming is randomly assigned prior to each call, potential unbalances could arise from participants selectively hanging up after being primed about droughts at the beginning of a call. Table D1 shows that not to be the case for the baseline covariates we observe: most differences are not statistically significant. For the only two that are (about the expected rate given that we test differences across 12 covariates), differences in the number of rooms and in participant's schooling across treatment and control are tiny (about 1.5% of the average of the control group in both cases), even though precisely estimated.

Table D2 displays balance tests for no rainfall 3 and 7 days prior to each survey. Once again, whenever there are statistically significant differences in baseline covariates across treatment and control, they are very small in magnitude. In any case, we later show that the effects of priming and rainfall shocks on cognitive function are completely robust to controlling for all baseline controls.

[Table D2]

Next, we analyze explicitly if treatments lead to selective non-response. Table D3 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 separate regressions.

[Table C3]

While priming or distance to payday do not affect response rates, the absence of rainfall significantly affects non-response: a one standard-deviation increase in the no rainfall summary measure leads to a 1.3

[18] Anthropologists have pointed out, in the context of such beliefs in Ceará, that "[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.", Finan (2001, p. 6, emphasis added).

percentage-point higher probability of taking the call (from a baseline of 43.9 p.p.), significant at the 10% level, similar to the effect of no rainfall 3 days before the survey. It seems that, when it rains, farmers are more likely to be on the field, and less likely to take our phone calls.

Selective non-response raises potential concerns with differences across treatment and control being driven by non-observable characteristics, e.g. if the marginal farmers who take the survey after it has rained recently are not as concerned with the harvest, and hence perform better for reasons unrelated with recent shocks. To deal with that concern, with rely on Lee (2009)'s method to bound treatment effects in the presence of selective non-response.

Last, Table C4 analyzes the marginal effects of baseline characteristics on the probability of completing each survey.

[Table C4]

Some participant's characteristics significantly affect the average probability of completing the surveys. For instance, being poorer or more highly educated both increase response rates, while having access to irrigation significantly decreases participation in our surveys. This could matter in the presence of heterogeneous treatment effects, which we analyze in subsection 4.5. In the Supplementary Appendix, we show that our results are robust to re-weighting observations by their inverse probability of response predicted by this model.

4.2 Worries about rainfall

Worries are measured through survey questions about the extent to which someone in the household worried about rainfall in the previous week or the extent to which the household was able to cope with household bills (see Appendix A). We normalize these variable to z-scores in analyzing how each measure of worries respond to priming and rainfall shocks.

In Table 1, all columns use worries about rainfall as dependent variable, except for column (5), which uses worries about household bills. Columns (1) to (4) consider the full sample, estimating the effects of priming on worries about rainfall. Columns (5) and (6) restrict attention to March and April (the "early waves"), when uncertainty about the rainy season still is unfolding. Columns (7) and (8) estimate the effects of rainfall shocks on worries about rainfall, for the full sample and the Bolsa-Família sample, respectively. All columns are OLS regressions, with standard errors clustered at the individual level.

[Table 1]

Results are as follows. Priming increases worries about rainfall by 0.05 standard deviations (column 1). This effect is noisily estimated, but becomes larger and statistically significant at the 10% level when we include wave fixed-effects (in column 2). As a benchmark, this is about 1.5 the effect of losing access to irrigation on worries about rainfall in a cross-sectional comparison, equivalent to having about 1 day less of rainfall in the previous week or to losing about 20% of one's harvest by the end of the rainy season. The effects of priming on worries are concentrated early in the rainy season (column 3): it peaks in the first wave, and then decreases until it basically disappears between May and June, when uncertainty about the rainy season has been resolved. The fact that worries increase on average with every additional wave attests to the fact that the rainy season was again below-normal in the State in 2015, with harvest losses as prevalent and of the same order of magnitude as in the previous 4 years. Also interestingly, the average effect of priming is increasing in municipalities' harvest losses in the previous year – and basically zero where no losses took place –, consistent with an affect mechanism whereby priming activates memories of previous negative experiences.

What is more, columns (4) and (5) showcase not only that the experimental manipulation works, but that is sharply confined to the domain of interest: early on, priming does not make one worried about coping household bills at the time of the survey, but rather about future rainfall (as harvesting takes place only during the late waves, if rainfall allows).

The effect of the no rainfall summary measure is, unsurprisingly, much larger in magnitude (4-fold that of priming) and very precisely estimated (at the 1% level) – as LASSO was meant precisely to pick rainfall shocks most predictive of worries about rainfall. Interestingly, the coefficient of priming changes little in the presence of rainfall shocks, confirming both shocks to be independently distributed. Their interaction is not statistically significant. As we show later, this does not mean that the shocks cannot compound to magnify their effects on cognitive function. Rather, it is consistent with a threshold model of worries, in which stimuli above a certain cutoff make scarce resources *top-of-mind*; once this happens, additional stimuli will not drive additional worries, even though they might still affect cognitive function directly.

Within the Bolsa-Família sample, rainfall still has very similar effects (column 7); in contrast, neither priming nor distance to payday systematically affect worries – the coefficient of the latter is nearly zero.

4.3 Cognitive load

Next, we analyze the effects of priming and rainfall shocks on our summary measure of cognitive load. In Table 2, column (1) presents the effect of priming, column (2), that of the no rainfall summary measure, and column (3), that of the two shocks together and their interaction. Columns (4) and (5) present the effects of no rainfall at t-3 and t-7, respectively, which allows us not only to benchmark the effects of the other shocks to more intuitive measures, but also to verify whether effect sizes decay with the distance between the shock and the survey, in line with what would expect. All columns are SUR regressions, with standard errors clustered at the individual level, controlling for all baseline characteristics. Outcomes are normalized such that negative coefficients indicate worse performance in attention, memory and impulse control tests.

[Table 2]

Results are as follows. Priming generates cognitive load (column 1), decreasing performance by 0.046 standard deviation (statistically significant at the 5% level). The loss in cognitive performance coming from risk exposure is massive: tantamount to that which would arise from moving a farmer from high school back to elementary school (in a cross-sectional comparison). The effect of rainfall shocks are about two-fold that of priming (column 2), and very precisely estimated (at the 1% level), suggesting a direct link between worries and cognitive function (since LASSO picks the rainfall shocks most predictive of worries about rainfall). For cognitive load, the effects of the shocks in isolation is not magnified by their joint occurrence (column 3), suggesting each really speaks to a different mechanism, at least in what comes to cognitive load. Last, columns (4) and (5) document an intuitive pattern for the effect of rainfall shocks: the closer to the call, the more they impair cognitive performance. The effect of priming is about half that of lack of rainfall a week before the survey, and approximately that of losing 25% of one's harvest at the end of the rainy season.

Incidentally, the evidence that negative rainfall shocks increase cognitive load has implications for other Development Economics research. It suggests that rainfall shocks may not satisfy the exclusion restriction for a valid instrumental variable in uncovering the relationship between poverty and stress (Haushofer and Fehr, 2014) or that between poverty and conflict (Miguel, Satyanath and Sergentin, 2004), in face of the evidence that higher cognitive load leads to positive affect and higher fairness; Schulz et al. (2014).

Next, Figure 1 plots the density of the cognitive load summary measure separately for farmers primed and those not, to shed light on whether differences are particularly concentrated at specific parts of the distribution of cognitive load.

[Figure 1]

The figure showcases that the effects of priming are concentrated in the middle of the distribution (within one standard deviation above and below zero), where it is very clear that primed subjects display lower scores in our cognitive tests. Differences are enough to reject the null hypothesis of equality between the two distributions, at the 10% statistical level. Having said that, priming affects scores over the entire range of the distribution: even though the figure does not allow one to see clearly, priming affects money earned in the experiment (performance within the top-25% scores; see Section 7).

4.4 Tunneling

We turn to the effects of priming and rainfall shocks on our summary measure of tunneling. In Table 3, column (1) presents the effect of priming, column (2), that of the no rainfall summary measure, and column (3), that of the two shocks together and their interaction. Columns (4) and (5) present the effects of no rainfall at t-3 and t-7, respectively, which allows us not only to benchmark the effects of the other shocks to more intuitive measures, but also to verify whether effect sizes decay with the distance between the shock and the survey, in line with what would expect. All columns are SUR regressions, with standard errors clustered at the individual level, controlling for all baseline characteristics. Outcomes are normalized such that negative coefficients indicate lower relative attention in tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources.

[Table 3]

We find that priming generates tunneling (column 1), improving cognitive performance (or deteriorating it to a lesser extent) in tasks involving scarce resources by 0.04 standard deviation (statistically significant at the 5% level). The effect of rainfall shocks within this dimension are very similar to that of priming (column 2), with an effect size of 0.043 (significant at the 10% level), suggesting a direct link between worries and cognitive function (since LASSO picks the rainfall shocks most predictive of worries about rainfall). For tunneling, the effects of the shocks in isolation are magnified by their joint occurrence (column 3) to an enormous extent: the coefficient of priming increases 3-fold within subjects who experience recent negative rainfall shocks that drive worries. Even though priming and rainfall capture different mechanisms (as we saw for cognitive load), past risk materialization can reinforce the effects of risk exposure, consistent with the heterogeneous effects of priming on worries according to harvest losses in the previous year.[19] Once again, columns (4) and (5) document an intuitive pattern for the effect of rainfall shocks: the closer to the call, the more they reallocate bandwidth, improving relative performance in tasks involving scarce resources. In the case of tunneling, only shocks 3 days before the survey are statistically significant, and their effect size is about the same to that of priming.

[19] Once again, this is not inconsistent with the null effect of the interaction of priming and the no rainfall summary measure on worries about rainfall, but rather consistent with a threshold model of worries.

Figure 2 plots the density of the tunneling summary measure separately for farmers primed and those not, to shed light on whether differences are particularly concentrated at specific parts of the distribution of tunneling.

[Figure 2]

The figure showcases that the effects of priming are concentrated in the upper half of the distribution (above zero), where it is very clear that primed subjects display higher scores in our cognitive tests. Differences are enough to reject the null hypothesis of equality between the two distributions, at the 10% statistical level. For tunneling, priming also affects money earned in the experiment (performance within the top-25% scores; see Section 7).

Last, Table 4 analyzes the effects of each shock on reaction times within tests meant to capture cognitive load (Column 1), and those meant to capture tunneling (Column 2). Each cell is a different SUR regression, with standard errors clustered at the individual level. Reaction times are measured in seconds.

[Table 4]

We find no significant effects of shocks on average reaction times within cognitive load, even though most coefficients are positive (consistent with worse cognitive performance in those tests). Conversely, in what comes to tunneling, all coefficients are negative. The effects of rainfall shocks are statistically significant: a one standard deviation increase in the no rainfall summary measure decreases reactions times by 1.2 seconds (significant at the 1% level); no rainfall at either 3 or 7 days before the survey decreases reaction times by over 0.6 second (significant at the 5% level). Faster reaction times within tasks involving scarce resources are consistent with the idea that such shocks make scarcity top-of-mind.

4.5 Robustness checks

This subsection considers robustness checks to the effects of priming and rainfall shocks on cognitive function. In the main text, we concentrate on two dimensions: whether selective non-response drives the effects of rainfall shocks, and whether the effects of both shocks on cognitive function are driven by any particular components of our summary measures. The Supplementary Appendix presents additional robustness checks: alternative measures of rainfall shocks, alternative specifications for fixed-effects, and alternative measures of sensitivity to framing (namely, present-bias, one such manifestation in the domain of time preferences).[20]

Since respondents are more likely to take up the phone surveys if in face of negative recent rainfall shocks (Table D3), we follow Lee (2009)'s procedure to bound treatment effects in the presence of selective non-response. Table 5 presents lower and upper bounds for the effects of priming and negative rainfall shocks on cognitive load and tunneling, and confidence intervals for each bound at the 10% level. A limitation of the procedure is that it can allow be applied to binary treatment variables; for this reason, we cannot do it for the no rainfall summary measure, restricting attention to no rainfall at t-3 and t-7. In Table 5, Columns (1) and (2) present the lower and upper bounds, respectively, for the effects on cognitive load, while columns (3) and (4) do so for the effects on tunneling. Each cell is a different SUR regression, with standard errors clustered at the individual level. Outcomes are normalized such that negative coefficients indicate worse performance in attention, memory and impulse control tests; and lower relative attention in tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources.

[Table 5]

Naturally, the bounds for the effects of priming do not affect the conclusions from Tables 2 and 3, as we showed in Table D3 that it does not induce selective non-response. In what comes to rainfall shocks, while its effects on cognitive load are robust to the bound procedure (bounds are very tight, and effects are statistically significant at the 10% level), for tunneling, that of no rainfall at t-3 is no longer statistically significant (rainfall at t-7 already had no statistically significant effect on tunneling in Table 3). Having said that, its p-value is very close to 10%, as the lower bound for the confidence interval is only marginally below zero, and quite close to that of priming – even though the latter induces no selective non-response.

Next, Figures 3 and 4 display the effects of priming and rainfall shocks on cognitive load and tunneling, respectively, to analyze whether those are driven by specific components, rather than representing a general tendency of cognitive function in response to higher risk exposure or materialization. In Figure 3, the left-hand side panel showcases effect sizes of priming on cognitive load, and the right-hand side, those of the no rainfall summary measure. Bars reflect 95% confidence intervals, and thicker dots reflect higher levels of aggregation of summary measures (in line with pre-registration).

[20] Whether poverty induces time inconsistencies, and how to mitigate the consequences of present-bias among the poor, is an active research topic (e.g. Gruber and Köszegi, 2004; Schilbach, 2019; Ashraf, Karlan and Yin, 2006).

[Figure 3]

We can see that, other than digit span, priming and rainfall shocks negatively affect all components of the cognitive load summary measure What is more, their effects are quite symmetric across components.

In Figure 4, the left-hand side panel showcases effect sizes of priming on tunneling, and the right-hand side, those of the no rainfall summary measure. Bars reflect 95% confidence intervals, and thicker dots reflect higher levels of aggregation of summary measures (in line with pre-registration).

[Figure 4]

In this case, we do see a few systematic differences across different components. The effects of priming are primarily driven by higher scarcity focus, whereas those of rainfall shocks, primarily by lower sensitivity to framing. We discuss those differences in greater details in the next subsection. Having said that, it is not the case that the effects of either shocks is driven single-handedly by isolated components. Primed subjects are better able to locate water-related words in distorted audios (relative to neutral words, such as husband or brother), and present higher relative valuation of scarce resources. Those who experience recent negative rainfall shocks also present higher relative valuation for water (although not money), and are systematically less sensitive to framing biases when trading off money and water against time.

4.6 Risk exposure vs. Risk materialization

While both priming and rainfall shocks generate cognitive load and tunneling, evidence suggests that the two shocks capture different dimensions of risk. First, we have documented that they have independent effects on cognitive load. Second, we have highlighted their work on tunneling operates through different components of the summary measure. In this subsection, we document that their effects differ systematically over the course of the rainy season, confirming the interpretation that priming captures the effects of risk exposure, by altering the salience of income risk, while rainfall shocks capture the effects of risk materialization.

Table 6 estimates heterogeneous treatment effects of each shock by the timing of their occurrence, contrasting early waves vs. late waves (March and April, and May and June, respectively). Panel A documents the results for the cognitive load summary measure, and Panel B, for the tunneling summary measure. All columns are SUR regressions, with standard errors clustered at the individual level. Outcomes are normalized such that negative coefficients indicate worse performance in attention, memory and impulse control tests; and lower relative attention in tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources.

[Table 6]

Results are as follows. The effects of priming on cognitive load are concentrated in early waves: its interaction coefficient with late waves is positive, even though not statistically significant (column 1). In contrast, the effects of rainfall shocks on cognitive load increase over time: although negative (but not statistically significant) at early waves, its effects are much larger and precisely estimated at later waves (column 2). The effects of priming peak early in the rainy season – before uncertainty unfolds, consistent with the *exposure* mechanism –, while those of negative rainfall shocks become larger over time – as hopes that they might still be reversed vanish, consistent with the *materialization* of risk.

While an alternative explanation would be that subjects get used to survey experiments – such that their effect ceases to exist over time –, Panel B rules this out, documenting that the effects of priming only kick-in in late waves. This might be due to the possibility that making droughts more salient after uncertainty has been resolved might, instead, reinforce the mechanism of risk materialization – rather than of risk exposure at this point –, approximating the effects of rainfall shocks on tunneling (which do not change systematically over time).

5 Does payday variation cause cognitive load and tunneling?

This section estimates the effects of payday variation on farmers' cognitive function. Subsection 5.1 presents its effects on worries, cognitive load and tunneling. Subsection 5.2 summarizes robustness checks.

5.1 Worries, cognitive load and tunneling

Table 7 presents the effects of each shock on worries and cognitive function within the sample of matched participants receiving Bolsa-Família payments. Column (1) presents their effects on worries about rainfall, column (2), on worries about household bills, column (3), on the cognitive load summary measure, and column (4), on the tunneling summary measure. We display the results of priming and no rainfall summary measure within that sample for comparison, and those of a linear measure of distance to payday (from -15 to 15; positive before payday, and negative after) and of indicator variables for payday within 3 and 7 days. For the indicator variables, we restrict attention to observations with paydays at most 3 days before or after the survey, and at most 7 days before or after the survey, respectively. Each cell is a different SUR regression, with standard errors clustered at the individual level. Outcomes are normalized such that negative coefficients indicate worse performance in attention, memory and impulse control tests; and lower relative attention in tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources.

[Table 7]

Results are as follows. While proximity to payday indeed correlates positively with worries about household bills (coefficients for payday within 3 and 7 days, column 2), effect sizes are tiny (and not statistically significant). Interestingly, the correlation with worries about rainfall (column 1) have the opposite sign, suggesting different shocks displace what is top-of-mind (and, possibly, mental bandwidth), and confirming that each shock manipulates the dimension it is supposed to.

Despite the correlation with worries about household bills, neither measure of distance to payday systematically generates cognitive load (column 3). This is not merely an artifice of smaller sample sizes: the effects of priming and rainfall shocks on cognitive load within that sample are actually *even more negative* than in the full sample (statistically significant at the 10% and 1% levels, respectively). In contrast, the coefficients of the indicators variables of distance to payday on cognitive load are not only insignificant, but actually *positive*. What is more, the indicator variables have very large and statistically significant effects on tunneling (column 4), even if the effects of priming and rainfall shocks in the Bolsa-Família sample are not significant at the 10% level (although effect sizes are extremely similar to those estimated for the full sample). Consistently with the patterns we found for rainfall shocks, tunneling effects decay with distance to payday. Within 7 days of the CCT payment, relative performance in tasks involving scarce resources increases by 0.21 standard deviations (significant at the 5% level); such effect increases to 0.34 within 3 days of the payment (also significant at the 5% level).

Figure 5 plots the density of the cognitive load summary measure (left-hand side) and that of the tunneling summary measure (right-hand side), separately for farmers within 7 days of payment (Yes) and those payed since at most 7 days (No), to shed light on whether differences are particularly concentrated at specific parts of their distributions.

[Figure 5]

The figure showcases that payday variation has systematic effects on cognitive load in the *wrong direction* for the most part; the distribution for those not yet paid displays much lower mass on the range of low scores (a *cognitive bonus*), and we reject the test of equality of distributions at the 1% level. The exception is at the very top: there is a visible increase in cognitive load at the upper tail of the distribution for farmers before payday, affecting money earned in the experiment (performance within the top-25% scores; see Section 7). In turn, the effects of payday variation on tunneling are present across most of the distribution of scores, but particularly from 1 standard deviation below zero onwards, where it is very clear that subjects before payday display higher scores in our cognitive tests. Differences are enough to reject the null hypothesis of equality between the two distributions, at the 1% statistical level. For tunneling, payday variation also affects money earned in the experiment (performance within the top-25% scores; see Section 7).

5.2 Robustness checks

This subsection considers robustness checks to the effects of payday variation on cognitive function, focusing on whether its effects (or lack of thereof) are driven by any particular components of our summary measures, and on whether those vary non-parametrically with distance to payday.

Figure 6 displays the effects of the indicator variable of payment within 7 days of the survey on cognitive load and tunneling, restring attention to observations with paydays at most 7 days before or after the survey. The left-hand side panel showcases effect sizes of this indicator variable on cognitive load, and the right-hand side, on tunneling. Bars reflect 95% confidence intervals, and thicker dots reflect higher levels of aggregation of summary measures (in line with pre-registration).

[Figure 6]

We can see that distance to payday positively (and very noisily) affects scores across all components of cognitive load. In what comes to tunneling, distance to payday increases scores systematically for almost all components, both within scarcity focus and sensitivity to framing.

Next, Figure 7 displays non-parametric estimates for the effects of payday variation on cognitive load and tunneling, for every possible window from payday. For each estimate, we hold distance to payday fixed, restricting attention to participants *at most D days* before or after payday, with D ranging from 1 to 15. Bars reflect 95% confidence intervals.

[Figure 7]

We find that payday variation does not generate cognitive load for any window. Even for the few coefficients that are negative, their effect size is rather small; in turn, the only significant coefficient is actually *positive* (4 days before payday). In contrast, one can see that its effects on tunneling display a clear surge in the vicinity of payday. Effect sizes are huge, precisely estimated despite the small number of observations. Intuitively, effects decay with distance to payday, but coefficients remain above zero (most of them statistically significant at the 10% level) across the entire range of distance to payday windows.

6 Does payday variation have no psychological effects on the poor?

We have shown that payday variation does not cause cognitive load *on average*. Does this mean it causes no psychological impacts on the poor? This section tackles this question by looking at the effects of payday variation when it is high stakes. Subsection 6.1 starts by documenting heterogeneous treatment effects of each shock by municipalities' per capita income to analyze whether payday variation actually causes cognitive load among the poorest. Next, subsection 6.2 analyzes money earned in the experiments – taking advantage of the non-linear incentive structure in place for the phone surveys –, followed by subsection 6.3, which assesses how tunneling effects affect farmers' information recall outside of our experiments, with the help of face-to-face surveys.

6.1 Heterogeneous effects by municipalities' per capita income

Payday variation has no *average* effects on cognitive load. Does it have significant effects among the poorest? Table 8 estimates heterogeneous treatment effects of each shock by municipality's per capita income (in natural logarithms), taking advantage of variation across the 47 locations where we conduct our study. Panel A presents the results for cognitive load, and Panel B, for tunneling. All columns are SUR regressions, with standard errors clustered at the individual level. Outcomes are normalized such that negative coefficients indicate worse performance in attention, memory and impulse control tests; and lower relative attention in tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources.

[Table 8]

We find that the effects of distance to payday vary with income, significantly so for those of payment within 7 days (column 7). Within the poorest municipalities – where CCT payments are much higher stake, and where anticipated shocks are much more likely to affect permanent income in combination with other market imperfections –, it *does* cause cognitive load; a large coefficient, significant at the 5% level. As municipality's per capita income increases, such effect decreases at a fast rate (also significant at the 5% level), eventually becoming positive at income levels where most of our sample lies, combining for a positive (although not statistically significant) average effect on overall cognitive performance. In turn, the effects of distance to payday on tunneling do not vary with income.

Figure 8 plots the predicted effects of payment within 7 days (Panel C) for the range of municipal per capita incomes observed in our sample.

[Figure 8]

Panel C illustrates that, within the income range of the poorest municipalities in our sample, we predict (using Table 8's estimates) that not having received the CCT payment within a 7-day window generates cognitive load *even to* a *greater extent* than priming or rainfall shocks. The coexistence of cognitive load and tunneling at that income range is uniquely consistent with the *mental bandwidth* mechanism. Such effect on cognitive load, however, sharply fades out with income, turning into a *cognitive bonus* at intermediate income levels (possibly consistent with hopefulness and anticipation, as in Caplin and Leahy, 2001), whereby subjects' cognitive performance improves overall, and particularly so within tasks involving scarce resources (as the large and positive effects of payday variation on tunneling barely vary with income).

We also find that the effects of risk exposure and risk materialization vary significantly with income. The effects of priming and rainfall shocks on cognitive load dissipate as municipality's per capita income increases for all measures (significantly so for priming, in column 1, and for no rainfall 7 days before the survey, in column 4). For tunneling, even though no interaction terms are statistically significant, they are positive and large for both priming and the no rainfall summary measure.

In Figure 8, Panel A shows that, within most of the income range in our sample, priming generates both cognitive load and tunneling, a pattern uniquely consistent with the *mental bandwidth* mechanism. For the least poor municipalities, while priming still triggers tunneling, it no longer causes cognitive load, a pattern consistent with *rational inattention* – since it enhances subjects' performance in tasks involving the object of the priming without deteriorating cognitive performance.

In Panel B, one can see that, within the poorest municipalities, rainfall shocks makes farmers' cognitive function worse overall (cognitive load) and *even worse* in tasks involving scarce resources – the *opposite* of tunneling, a phenomenon we call *choking* (mistakes driven by high stakes, as in Ariely et al., 2009). As municipal per capita income increases, scarcity triggers tunneling while still causing cognitive load. The pattern, consistent with the *mental bandwidth* mechanism, holds for most of the income range in our sample.

The fact that all shocks generate psychological effects concentrated on the poor rules out that their differential effects are merely driven by the loss of the power of certainty (Martínez-Marquina, Niederle and Vespa, 2019) – shown to impair contingent reasoning more generally. Rather, it corroborates that the psychological effects we document are triggered by scarcity thinking.

Together, those findings indicate that the effects of risk exposure and risk materialization drive poverty's psychological tax; among the poorest, however, all shocks drive cognitive load and tunneling. In face of those results, Mani et al. (2013) and Carvalho, Meier and Wang (2016) are not inconsistent after all. The former captures the effects of an unanticipated income shock, while the latter only documents the effect of an anticipated income shock within a sample in the US, not nearly as poor as the poorest municipalities in our sample or as the sample of the study in India.

6.2 Responses to incentives by top performers

While the effects of payment within 7 days on cognitive load do vary significantly with municipality's per capita income, the same is not true for those of payment within 3 days, which is not in line with intuition that cognitive effects should become stronger with proximity to payday. What is more, since per capita income is not randomly assigned, the variation we explore in subsection 7.1 is only correlational. To get around that issue, in this subsection we turn to whether high stakes in the context of our experiments – arising from the non-linear incentive structure we put in place – change the effects of payday variation on cognitive function.

We have shown that payday variation generates tunneling – a dimension Carvalho, Meier and Wang (2016) does not measure. We study whether such effect is *magnified* in face of incentives. We provide a non-linear structure of incentives, granting the top-25% scores in our phone surveys additional airtime credit. Table 9 estimates whether each shock affects money earned within tests meant to capture cognitive load (Columns 1 and 3), and those meant to capture tunneling (Columns 2 and 4). Columns (1) and (2) consider the full sample, while Columns (4) and (5), that of matched participants receiving Bolsa-Família payments. Each cell is a different SUR regression, with standard errors clustered at the individual level. Money earned is measured in R$.

[Table 9]

Results are as follows. Priming and rainfall shocks increase money earned significantly across all tests (the former, only significantly for tunneling in the full sample). Effect sizes are large. Rainfall increases money earned within tunneling tasks by over 5%, and rainfall shocks, by 13-16%, depending on the sample. The fact that, in the range of scores for which performance incentives become relevant, the effects of risk exposure and risk materialization on cognitive load are *reversed* is remarkable. This finding lines up with those of a number of studies that find scarcity to actually *improve* performance in face of incentives. as in the 'Wheel of Fortune' self-replication in Shah, Shafir and Mullainathan (2019), where primed subjects make more money in the incentivized experiments, Lichand et al. (2019), where primed subjects make more money in short-term incentivized attention and memory tasks, and Kaur et al. (2019), where workers primed about financial strain increase productivity in peace-meal payment tasks.

In what comes to payday variation, proximity to payday drives large and significant differences in money earned in the experiments, even on the absence of average effects. Before payday, while both average subjects *and* top performers do *better* on tasks involving scarce resources (earning significantly *more* money on tunneling tasks), top performers actually do *worse* on other tasks (earning significantly *less* money), consistent with mental bandwidth reallocation in the range of performance for which incentives matter. Effects decay with distance to payday, as one would expect: a week before payment, participants make about 14% more on tunneling tasks, but 15% less in other tasks (significant at the 10% and 5% levels, respectively); 3 days before payment, those effects increase to 26% and 32% (significant at the 10% and 5% levels, respectively). In contrast, the tests in Carvalho, Meier and Wang (2016) are not incentivized.

6.3 Information recall

Finally, we study whether attentional shifts triggered by tunneling generate costs also outside of our experiments, in line with evidence from previous studies (Shah, Shafir and Mullainathan, 2017; Lichand et al., 2019).

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. For instance, to evaluate real-world effects of priming, one would have to restrict attention to farmers' decisions possibly influenced by survey experiments; their effects, however, are short-lived.

To get around that issue, we concentrate on whether rainfall shocks distort how subjects recall information, via face-to-face surveys in which we ask farmers to provide their best guesses of two real-world quantities: the number of rainy days in their municipality in the previous month, and the volume of water in their rain-fed water tanks. The former could be verified through administrative data from meteorological stations that track daily rainfall in the State, on which all our rainfall variables are based; the former, by enumerators at the end of the survey.

The key insight is that knowledge about the number of rainy days in the previous month is *not consequential* to farmers in this region (given soil absorption properties), whereas knowledge about water left in their tanks is vital for both production and survival.[21]

Because the dates at which enumerators visit each farmer are not randomly assigned, we restrict attention to a coarser measure of recent negative rainfall shocks: an indicator of below-normal rainfall, equal to 1 when the previous month ranks among the 1/3 worst within the last 30 years in rainfall volume, and 0

[21] Under soil conditions available in Ceará – with little to no water absorption properties – for most municipalities optimal seeding models depend almost exclusively on current and future expected rain, not on *past* rainfall (Andrade, 2005).

otherwise. This variable is commonly used in the literature to define the occurrence of droughts (e.g. Shah and Millet Steinberg, 2017).

We estimate the effects of below-normal rainfall shocks on farmer's accuracy for both guesses. It uses an inaccuracy indicator as dependent variable, equal to 1 if the guess is within a close range of the correct answer, and 0 otherwise. In the main text, we code accurate answers as those within 5 days of the actual number of rainy days in the previous month, and those within 350ml of the volume in the water tank. We use those cutoffs to approximate 1/3 mean inaccuracy in our sample across both measures, ensuring we have enough statistical power to detect relevant effect sizes. The Supplementary Appendix documents that results are robust to alternative cutoffs.

In Table 10, columns (1) and (2) show the results for the number of rainy days in the previous month, while columns (3) and (4), for the volume of water in the water tank (measured in L). Odd-numbered columns display the effect of rainfall shocks in isolation; even-numbered ones add the actual figure (number of rainy days or volume in the water tank) to test for Weber's law, i.e. a positive correlation between the content of the guess and the extent of misperception (Khaw, Li and Woodford, 2017), and its interaction with the below-normal rainfall indicator. Since in the case of face-to-face surveys many subjects answer to multiple surveys (yielding 3,589 observations over the course of all waves), all columns include individual-fixed effects, besides wave fixed-effects, with standard errors clustered at the individual level.

[Table 10]

Results are as follows. Below-normal rainfall shocks *decrease accuracy* of farmers' recall for both guesses – consistent with cognitive load. Inaccuracy increases significantly precisely for farmers who experience scarcity the most (for whom rainy days or water volume is zero, the coefficient of the non-interacted term in columns 2 and 4), by 17.6% and 14.7%, respectively (both significant at the 1% level) – a huge effect size, almost 50% the mean inaccuracy rate. Moreover, the larger the actual number of rainy days or the water volume in one's tank, the *lower* one's accuracy is (consistent with Weber's law): an additional rainy day or an additional L in one's water tank increase inaccuracy by about 5% (both significant at the 1% level).

Most importantly, negative rainfall shocks *attenuate* this effect *only* for farmers' recall of the number of rainy days in the previous month – consistent with tunneling –; but *not* for their recall of how much water there is in their tanks. The interaction of below-normal rainfall shocks and the actual figure decreases inaccuracy by about 1% for each additional rainy day in the previous month (significant at the 5% level), but has no significant effect on the accuracy of subjects' guesses of the volume in their water tank (if anything, the coefficient even has the opposite sign).

These findings are important for two reasons. First, they illustrate that shocks that generate cognitive load and tunneling can have *real* consequences, by adversely affecting farmers' recall of information relevant for decision-making. Second, they illustrate that tunneling can be *inefficient*, by allocating mental resources to dimensions of scarcity that are salient, at the expense of information that is more consequential – consistent with the increase in workers' mistakes before payday in Kaur et al. (2019). Both anticipated and unanticipated shocks induce inefficient tunneling, making farmers 'penny wise and pound foolish'.

7 Discussion and concluding remarks

In the Netflix documentary *Living On One Dollar*, four Economics majors decide to live in a Guatemalan slum for about two months to experience poverty first-hand. In order to approximate the lives of the poor, each day they draw a paper slip from a bag that determines how much income they can use for living expenses on that day. The paper slips average 1 dollar – hence the title of the documentary –, but not all of them are worth the same; in particular, several paper slips are worth zero. This little experiment replicates two fundamental attributes of poverty: (1) it is about facing low income levels *on average*, and (2) it is about facing the *risk* of not having enough to meet even basic needs on any particular day. This paper documents that the second mechanism is the fundamental driver of the psychological effects of poverty on the quality of decision-making.

Using a combination of survey experiments and natural variation in recent rainfall shocks, this paper shows that rainfall risk increases farmers' cognitive load and their susceptibility to a variety of behavioral biases, even before shocks actually hit. The fact that survey experiments also improve farmers' performance in tasks involving scarce resources, and that those effects are concentrated on the poor, 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 decisions 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 decisions might be impacted by worries about rainfall – even those unrelated to consumption smoothing –, possibly leading to lower payoffs in *every* future state, regardless of the occurrence of a drought.

Our findings are in line with previous studies about the effects of scarcity on psychological outcomes (Mani et al., 2013; Shah, Shafir and Mullainathan, 2015; Haushofer and Fehr, 2014). 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 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 tunneling, providing crisp evidence for the mental bandwidth mechanism.

Our results also help rationalize the lack of effects of payday variation on cognitive load in Carvalho, Meier and Wang (2016). We find that the effects of payday variation are concentrated on tunneling – a cognitive dimension that their survey does not capture – and on the poorest – an income range that their sample does not cover. What is more, while their cognitive tests are not incentivized, we show that, in the range of performance for which incentives are relevant, payday variation induces inefficient focus. Farmers earn more in tasks involving scarce resources at the expense of money earned in other tasks, and tunneling induced by rainfall shocks leads farmers to focus on inconsequential information at the expense of the volume in their water tanks, critical for both production and survival – highlighting costly consequences of inefficient focus even outside our experiments.

Are the psychological effects of poverty first-order? There are two reasons for why that might be the case. 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 about 25% losses at the end of the rainy season. 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 farmers' recall of consequential information.

Could those psychological effects generate poverty traps? Ongoing work sheds light on those issues, by analyzing the psychological consequences of poverty for productivity (Schilbach et al, 2019) and on investments in children's human capital (Lichand et al., 2019). This is a promising avenue for future research, alongside with interventions that could help mitigate those psychological effects by adapting the environment in which the poor make those decisions.

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Appendix A – Definition of dependent variables

WORRIES

Worries about rainfall:

"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"

Worries about household bills:

"Was your household able to cope with ordinary bills and daily consumer items last week? If your household had no difficulty in coping, press 0, if it had some difficulty, press 1, if it had a lot of difficulties, 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"

TUNNELING

- Focus:

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.

OTHER OUTCOMES [not shown in this paper]

- Pro-sociality[22]:

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 inconsistencies:

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.

[22] Following Berg, Dickhaut and McCabe (1995).

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.

- 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.”

- Demand for financial services

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 consumption (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.”

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.”

Appendix B – Priming: treatment and control messages

- Call #1:

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

Control: "Please tell us after the tone 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 – Description of datasets

Table C1 – 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 C1:

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

Table C2 – List of rainfall variables picked by LASSO as predictors of worries about rainfall

- Rainfall level in t-2
- Rainfall occurrence in t-7
- Rainfall occurrence in t-3
- Accumulated rainfall in the past 21 days
- Number of days with occurrence of rainfall in the last 2 days
- Number of days with occurrence of rainfall in the last 5 days
- Number of days with occurrence of rainfall in the last 21 days
- Relative deviation from mean in t-7
- Accumulated absolute deviation in the past 21 days

<u>Notes on Table C2:</u>

1. Variables to which LASSO assigns non-zero weight, in a regression featuring worries about rainfall as dependent variable, and including 51 features of rainfall over the past 21 days, with municipality fixed effects.

Table C3 – Distribution of call dates and Bolsa-Family payments

		Wave			
		March	April	May	June
Call	1	9-10	6-7	4-5	15-16
	2	11-12	8-9	6-7	17-18
	3	13-14	10-11	8-9	19-20
	4	16-17	13-14	11-12	22-23
	5	18-19	15-16	13-14	24-25
	6	20-21	17-18	15-16	26-27
		Month			
		March	April	May	June
NIS's last digit	1	18	16	18	17
	2	19	17	19	18
	3	20	20	20	19
	4	23	22	21	22
	5	24	23	22	23
	6	25	24	25	24
	7	26	27	26	25
	8	27	28	27	26
	9	30	29	28	29
	0	31	30	29	30

Notes on Table C3:

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

Table C4 – Distribution of payday among Bolsa-Família beneficiaires

Days until payday	Frequency (%)				
	All waves	March	April	May	June
-15	1.64	1.19	0	0.45	0
-14	5.37	1.65	0	1.31	2.4
-13	3.52	1.19	0	1	1.34
-12	3.09	0.82	0	0.85	1.42
-11	2.58	0.78	0	0.45	1.35
-10	2.19	0.8	0	0.41	0.98
-9	1.62	0.35	0.31	0	0.96
-8	1.59	0.33	0.36	0	0.9
-7	1.91	0.35	0.6	0	0.96
-6	1.59	0.33	0.36	0	0.9
-5	1.04	0	0.6	0	0.44
-4	1.12	0	0.72	0	0.39
-3	0.56	0	0.56	0	0
-2	1.49	0	1	0.49	0
-1	1.88$	0.37	0.92	0.59	0
0	3.02	0.37	1.65	1	0
1	1.88	0.37	0.92	0.59	0
2	2.65	0.37	1.29	1	0
3	3.67	0.74	1.27	1.19	0.48
4	2.97	0.37	1.15	0.92	0.53
5	4.3	0.85	0.96	1.55	0.94
6	4.17	0.8	0.78	1.55	1.04
7	6.76	1.57	1.31	2.5	1.38
8	4.17	0.8	0.78	1.55	1.04
9	4.7	1.21	0.69	1.9	0.9
10	5.77	1.59	0.54	2.18	1.45
11	5.03	1.19	0.58	1.77	1.49
12	5.32	1.63	0.26	1.51	1.92
13	4.83	1.23	0.23	1.37	2
14	6.94	1.98	0.26	1.95	2.75
15	2.6	0.46	0.23	1.37	0.53

Notes on Table C4:

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

Appendix D – Balance and selective non-response tests

Table D1 – Balance tests: Priming

	Priming = 0	Priming = 1	Difference [1 - 0]	Difference [1-0] with Mun. FE
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]

<u>Notes on Table D1:</u>

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 D2 – Balance tests: Rainfall shocks

Panel A: No rainfall in t-3

	No Rainfall in t-3 = 1	No Rainfall in t-3 = 0	Difference [1-0] with Mun. FE	Observations
Male	0.337	0.345	0.003	15192
	(0.017)	(0.012)	0.004	
Age	34.224	34.616	0.174	7674
	(0.675)	(0.544)	0.139	
Believes in RoT	0.66	0.663	0.01***	14550
	(0.018)	(0.013)	0.004	
Irrigation	0.135	0.139	0	21084
	(0.013)	(0.011)	0.003	
Owns Property	2.031	2.036	0.008	20460
	(0.032)	(0.025)	0.007	
Plot Size	5.115	6.655	0.24	2154
	(1.182)	(0.858)	0.167	
Cassava	0.171	0.2	0.002	19968
	(0.015)	(0.012)	0.003	
Number of Rooms	5.563	5.475	0.022	15042
	(0.081)	(0.065)	0.018	
Household Income	1.649	1.662	-0.003	18426
	(0.031)	(0.025)	0.006	
Schooling	2.106	2.143	-0.012*	17376
	(0.034)	(0.027)	0.007	
Bolsa-Família	0.798	0.781	0.003	18234
	(0.017)	(0.014)	0.004	
Government Insurance	0.812	0.794	0.015**	21210
	(0.015)	(0.01)	0.007	
Municipality Fixed Effects	N	N	Y	
Wave Fixed Effects	N	N	N	

Panel B: No rainfall in t-7

	No Rainfall in t-3 = 1	No Rainfall in t-3 = 0	Difference [1-0] with Mun. FE	Observations
Male	0.319	0.338	-0.001	15192
	(0.019)	(0.013)	0.003	
Age	34.719	34.742	0.272*	7674
	(0.716)	(0.55)	0.144	
Believes in RoT	0.669	0.666	0.003	14550
	(0.02)	(0.013)	0.003	
Irrigation	0.121	0.135	-0.002	21084
	(0.014)	(0.01)	0.003	
Owns Property	2.042	2.039	0.014**	20460
	(0.033)	(0.026)	0.007	
Plot Size	4.821	6.571	0.289	2154
	(1.278)	(0.797)	0.217	
Cassava	0.141	0.191	-0.001	19968
	(0.016)	(0.012)	0.004	
Number of Rooms	5.494	5.459	0.003	15042
	(0.088)	(0.064)	0.028	
Household Income	1.642	1.66	-0.007	18426
	(0.032)	(0.025)	0.007	
Schooling	2.103	2.142	-0.009	17376
	(0.036)	(0.027)	0.008	
Bolsa-Família	0.808	0.784	0.004	18234
	(0.017)	(0.014)	0.004	
Government Insurance	0.818	0.796	0.011	21210
	(0.016)	(0.011)	0.007	
Municipality Fixed Effects	N	N	Y	
Wave Fixed Effects	N	N	N	

<u>Notes on Table D2:</u>

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 D3 – Selective non-response tests

	Complete call
Panel A: Full Sample	
Priming	0.000049
	(0.006)
No rainfall summary measure	0.013*
	(0.006)
No Rainfall in t-3	0.012*
	(0.007)
No Rainfall in t-7	-0.0011
	(0.006)
Panel B: Bolsa-Família Sample	
Priming	-0.013
	(0.011)
No rainfall summary measure	0.021*
	(0.012)
No rainfall in t-3	0.017
	(0.012)
No rainfall in t-7	0.0049
	(0.011)
Distance to payday	-0.00057
	(0.001)
Payment within 3 days	0.027
	(0.03)
Payment within 7 days	0.022
	(0.022)

Notes on Table D3:

1. Each cell is a different Ordinary Least Squares (OLS) regression, with dependent variable equal to 1 if the survey (call) was completed by the subject, and 0 otherwise, including municipality fixed-effects;
2. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D4 – Marginal effects of baseline characteristics on the probability of completing a call

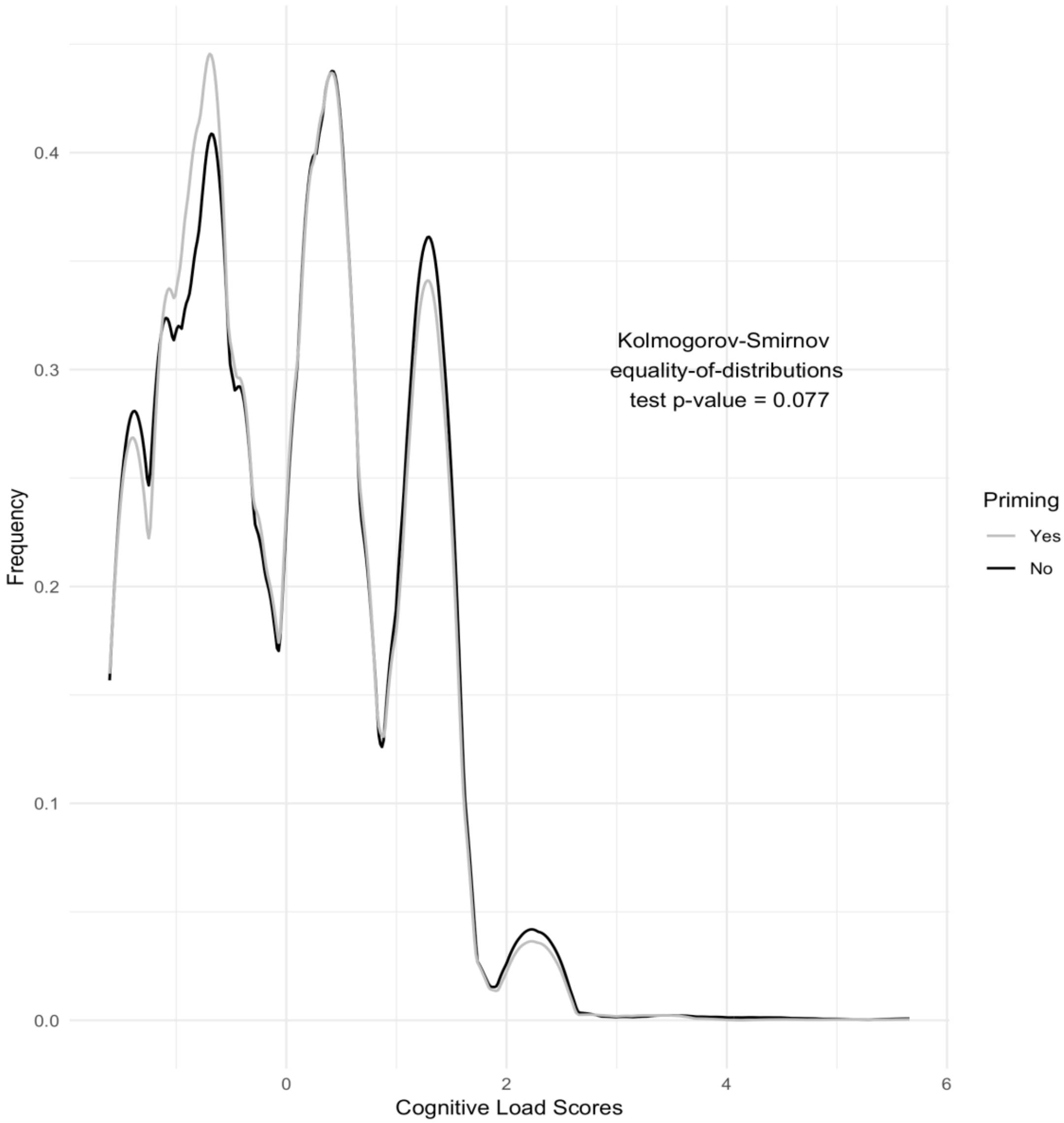
Variable	Marginal effect on probability of completing a call
Respondent lives in municipality's most drought-prone region	0.02**
Respondent is male	-0.01
Respondent's age	-0.00**
Respondent believes that rainy season will be good if it rains on March 19th	0.02
Respondent's plot is at least partly irrigated	-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 D4:

1. Cells are coefficients from an Ordinary Least Squares (OLS) regression, with an indicator variable as dependent variable, equal to 1 if the survey (call) was completed by the subject, and 0 otherwise, including municipality fixed effects;
2. *** p<0.01, ** p<0.05, * p<0.1.

Figures

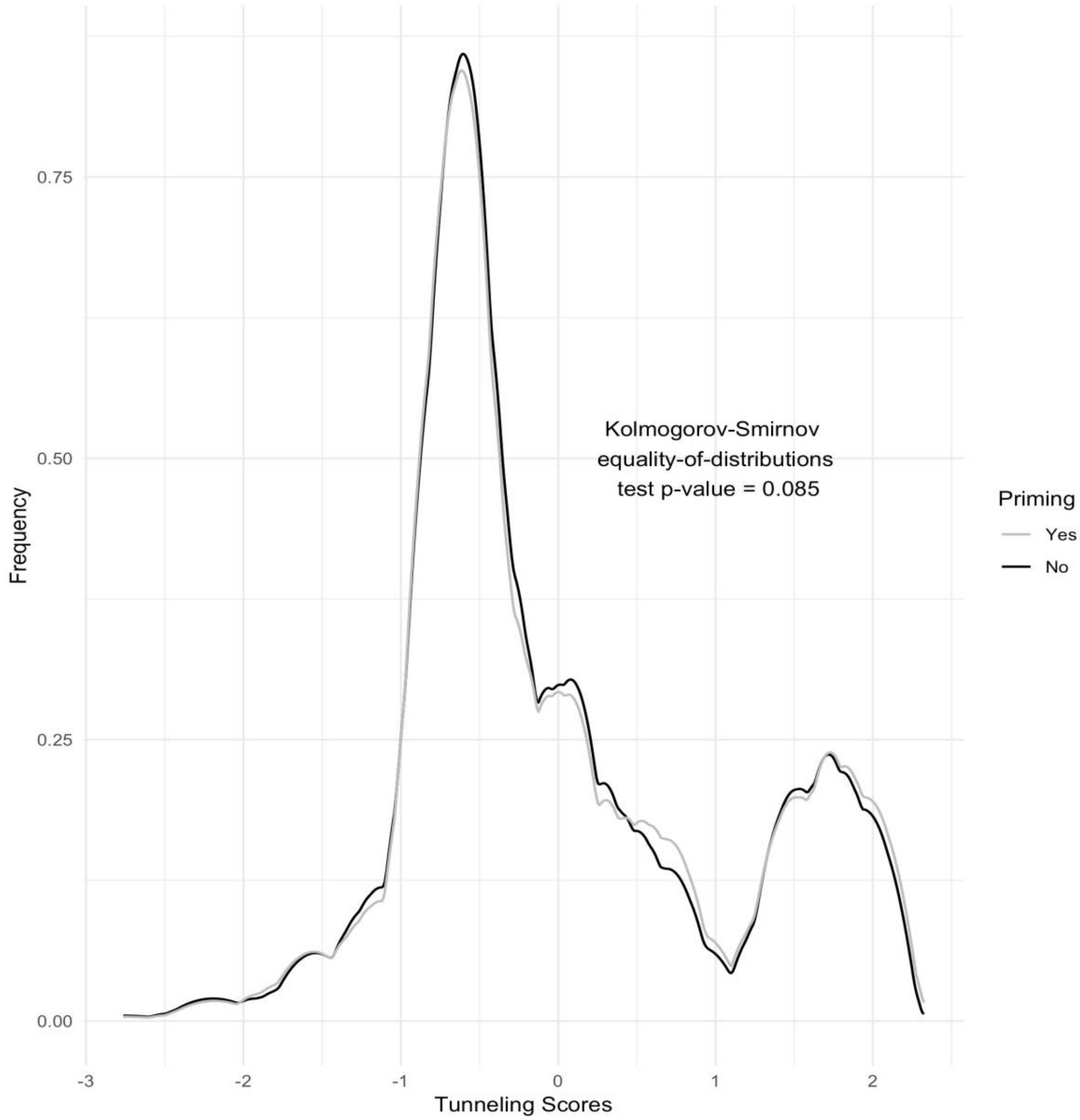
Figure 1 – Effect of priming on the distribution of cognitive load



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

1. Density of scores for the cognitive load summary measure, separately for subjects primed (in gray) and those not primed (in black) within each call;
2. The summary measure is computed as the average of its standardized components (z-scores) for executive functions and anchoring. See Appendix A for the definition of each variable. Outcomes are normalized such that negative values mean worse cognitive performance.

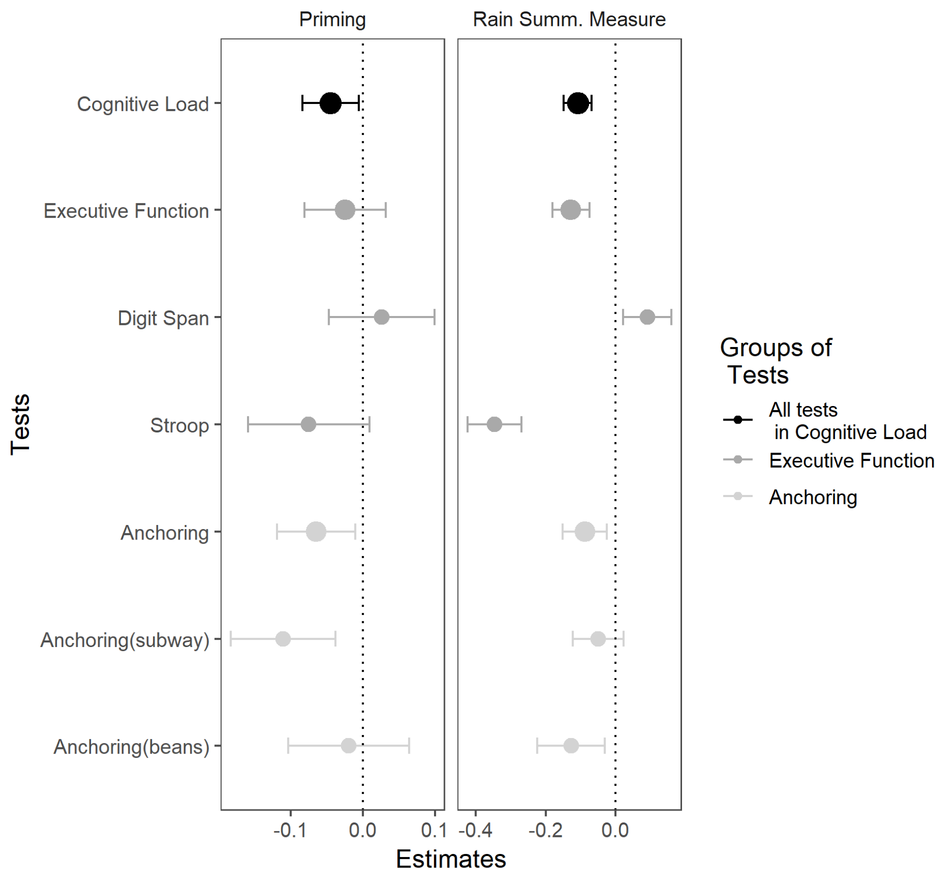
Figure 2 – Effect of priming on the distribution of tunneling



<u>Notes on Figure 2:</u>

1. Density of scores for the tunneling summary measure, separately for subjects primed (in gray) and those not primed (in black) within each call;
2. The summary measure is computed as the average of its standardized components (z-scores) for focus and framing. See Appendix A for the definition of each variable. Outcomes are normalized such that negative values mean worse cognitive performance.

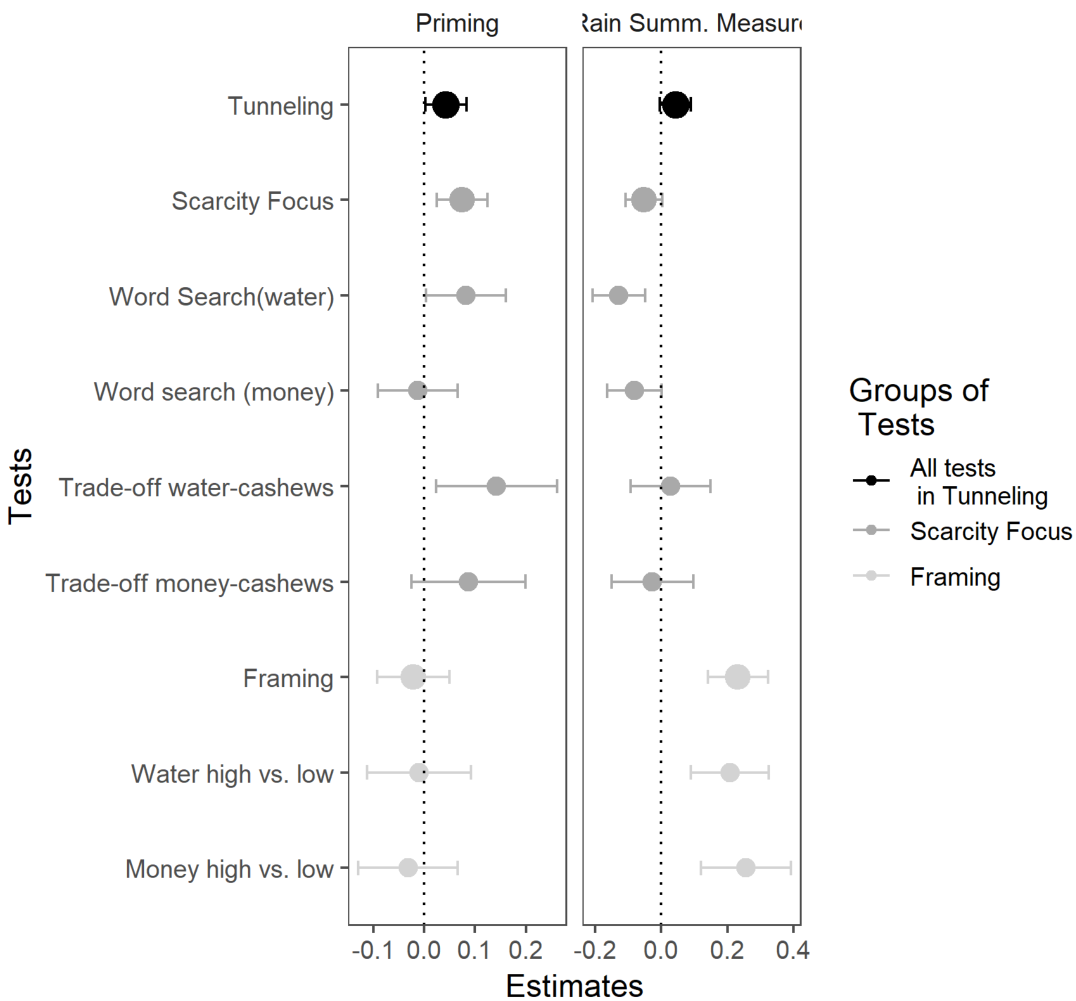
Figure 3 – Effects of priming and of the rainfall summary measure on the components of cognitive load



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

1. The figure displays coefficients and 95% confidence intervals for summary measures and for all components under each category;
2. All estimates 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, including municipality fixed-effects; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure component. See Appendix A for the definition of each variable;
3. Outcomes are normalized such that negative values mean worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring).

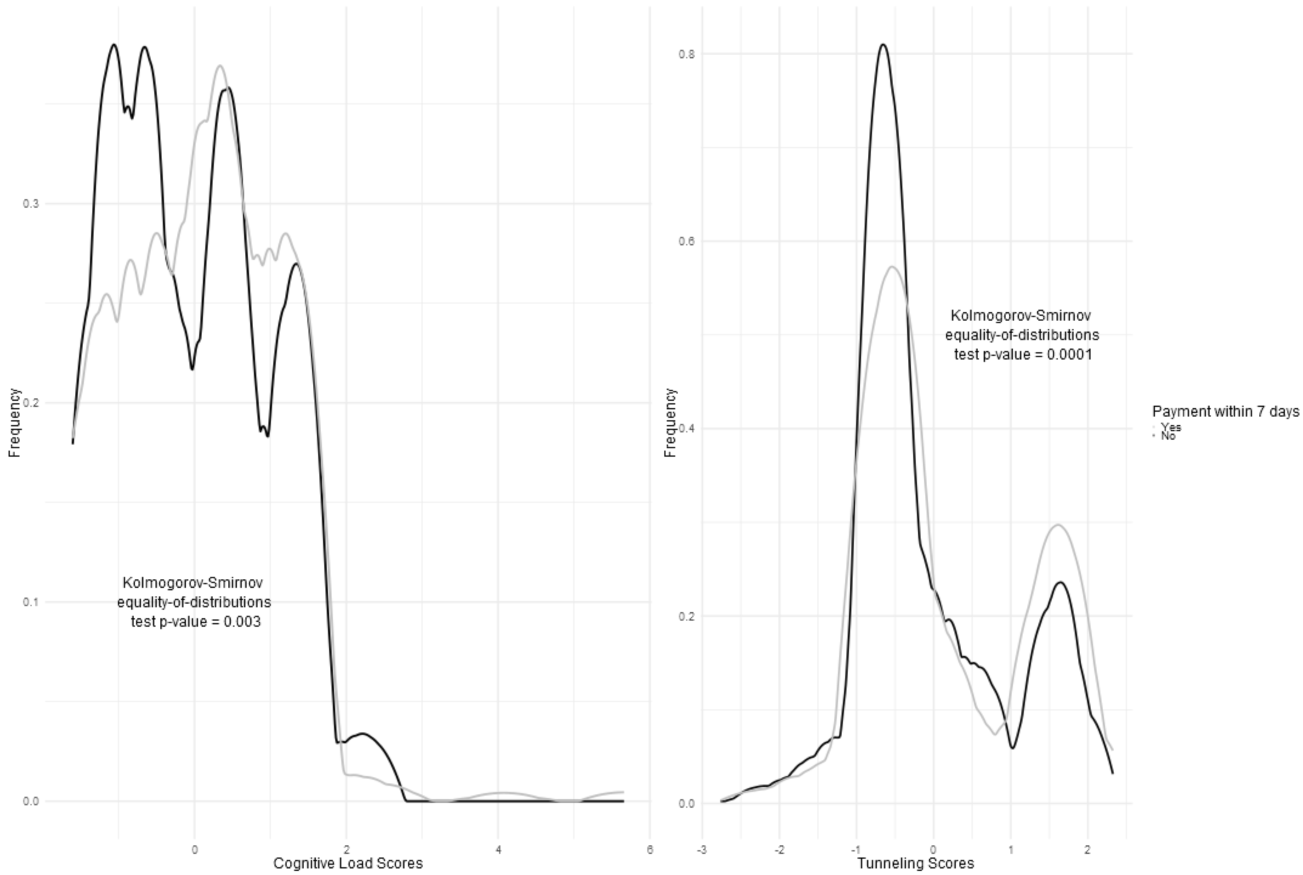
Figure 4 – Effects of priming and of the rainfall summary measure on the components of tunneling



<u>Notes on Figure 4:</u>

1. The figure displays coefficients and 95% confidence intervals for summary measures and for all components under each category;
2. All estimates 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 focus and framing, including municipality fixed-effects; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure component. See Appendix A for the definition of each variable;
3. Outcomes are normalized such that positive values mean better relative cognitive performance (higher relative attention towards scarce resources, higher valuation of scarce resources, and lower sensitivity to framing biases in tasks involving scarce resources);

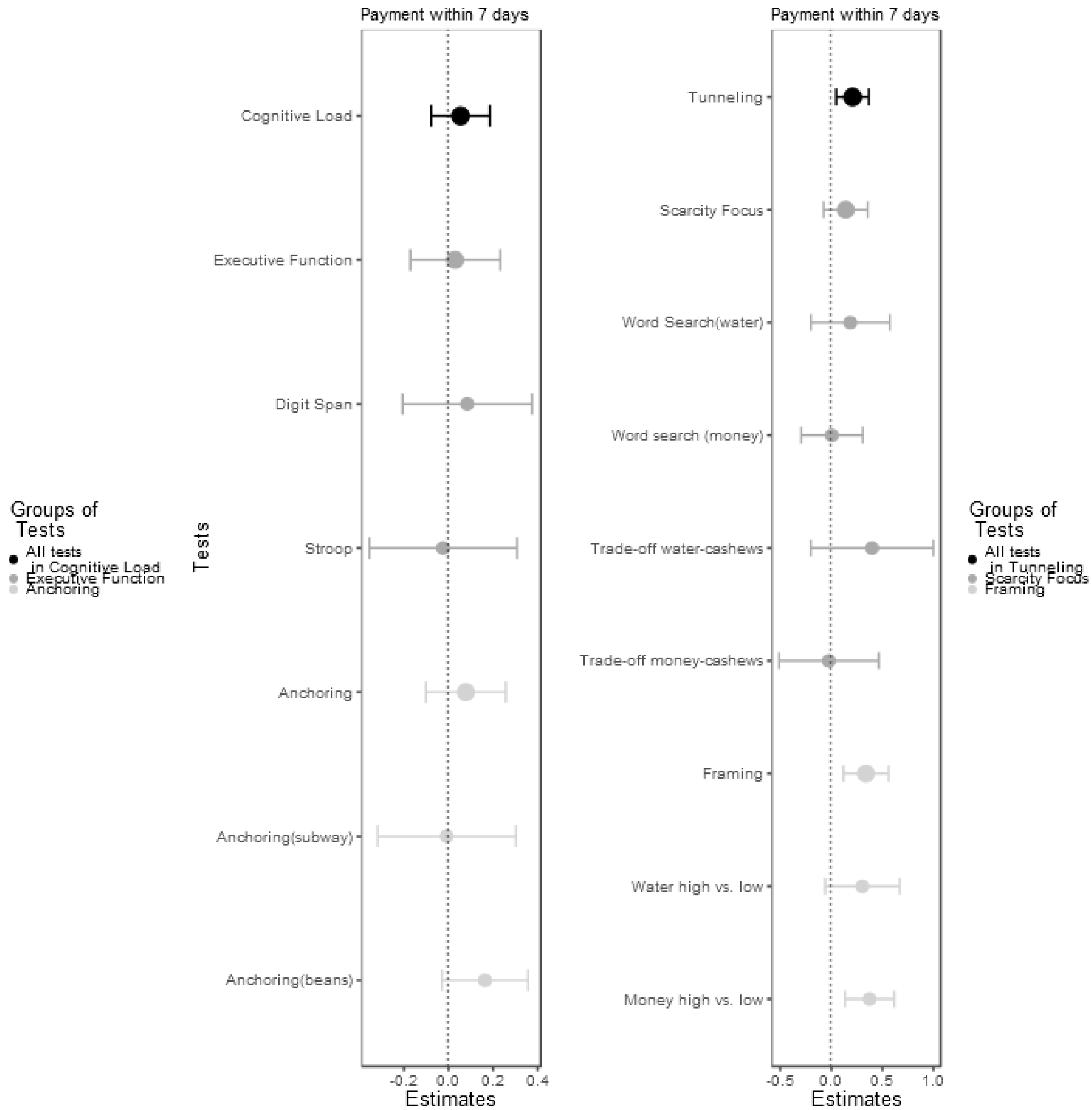
Figure 5 – Effects of distance to payday on the distribution of cognitive load and tunneling



<u>Notes on Figure 5:</u>

1. Density of scores for the cognitive load summary measure (left-hand side) and for the tunneling summary measure (right-hand side), separately for subjects within 7 days of their CCT payment (in gray) and those paid since at most 7 days (in black) within each call;
2. The summary measures are computed as the average of their standardized components (z-scores) for executive functions and anchoring, in the left-hand side, and for focus and framing, in the right-hand side. See Appendix A for the definition of each variable. Outcomes are normalized such that negative values mean worse cognitive performance.

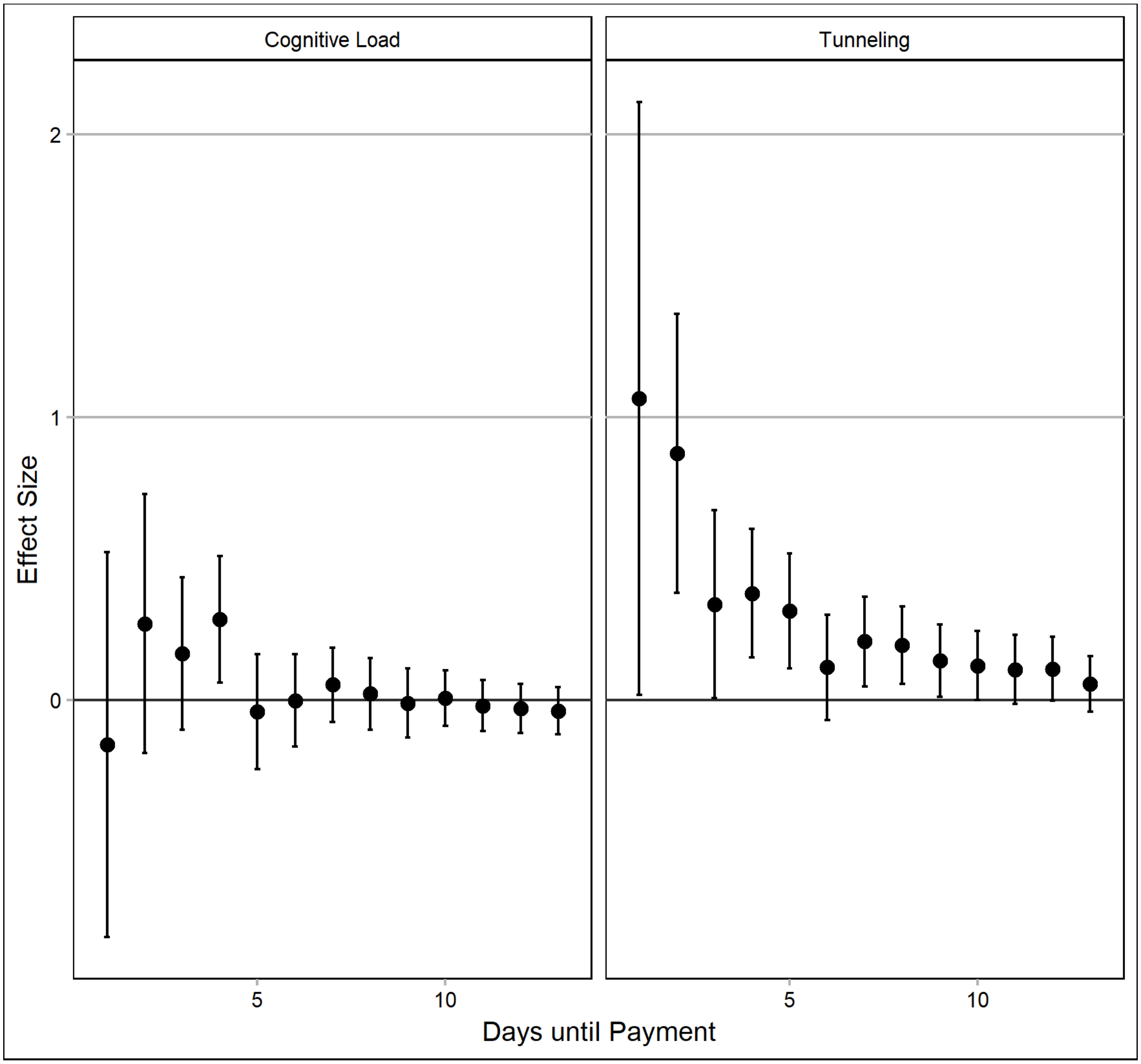
Figure 6 – Effects of distance to payday on the components of cognitive load and focus within Bolsa-Família sample



<u>Notes on Figure 6:</u>

1. The figure displays coefficients and 95% confidence intervals for summary measures and for all components under each category;
2. All estimates 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 executive functions and anchoring, in the left-hand side, and for focus and framing, in the right-hand side, including municipality fixed-effects; 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. Outcomes are normalized such that negative values mean worse cognitive performance.

Figure 7 – Non-parametric effects of distance to payday on cognitive load and tunneling

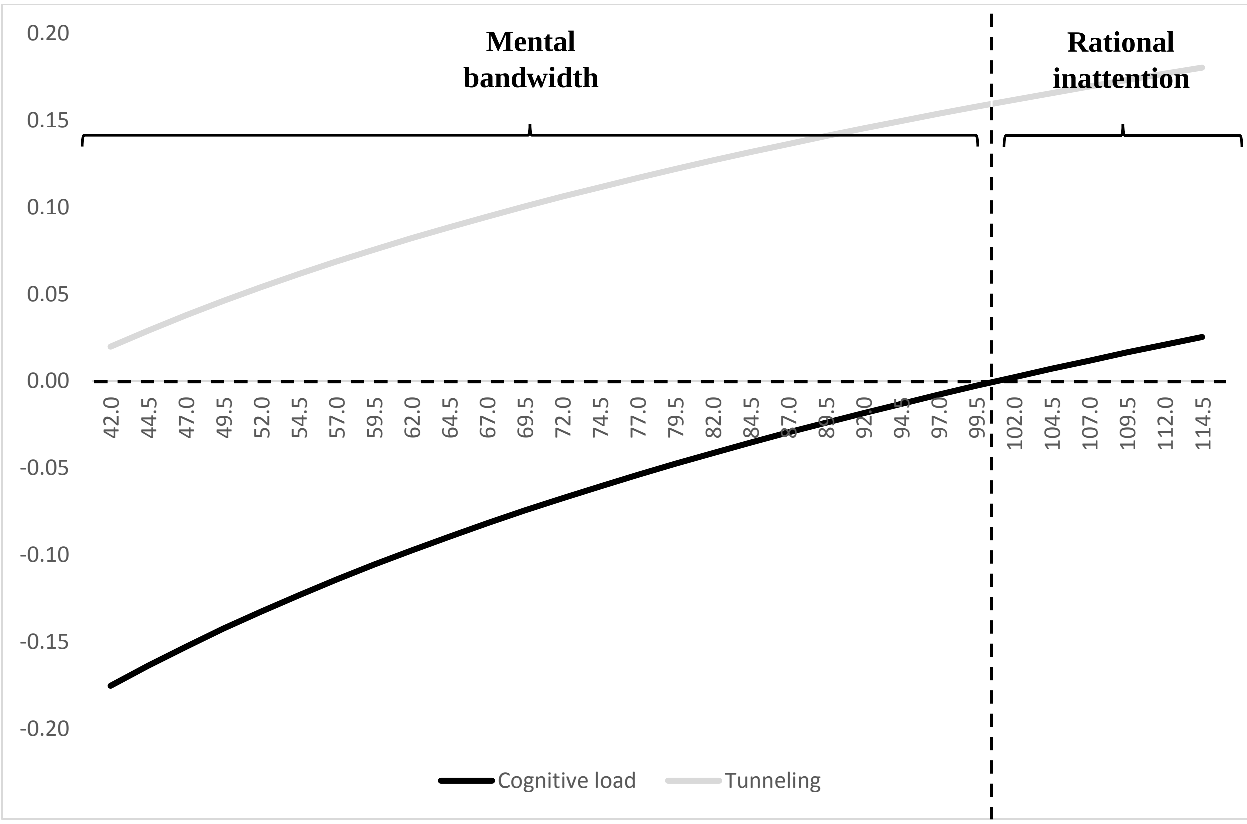


<u>Notes on Figure 7:</u>

1. The figure displays coefficients and 95% confidence intervals for the effects of distance to payday on each outcome category, for different cumulative intervals until payday. Each estimate holds the window size fixed, only comparing subjects within the same distance to payday (before vs. after), including municipality fixed-effects;
2. All estimates 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, in the left-hand side, and for focus and framing, in the right-hand side, including municipality fixed-effects; 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. Outcomes are normalized such that negative values mean worse cognitive performance.

Figure 8 – Predicted effects on cognitive function by municipality's per capita income (monthly USD)

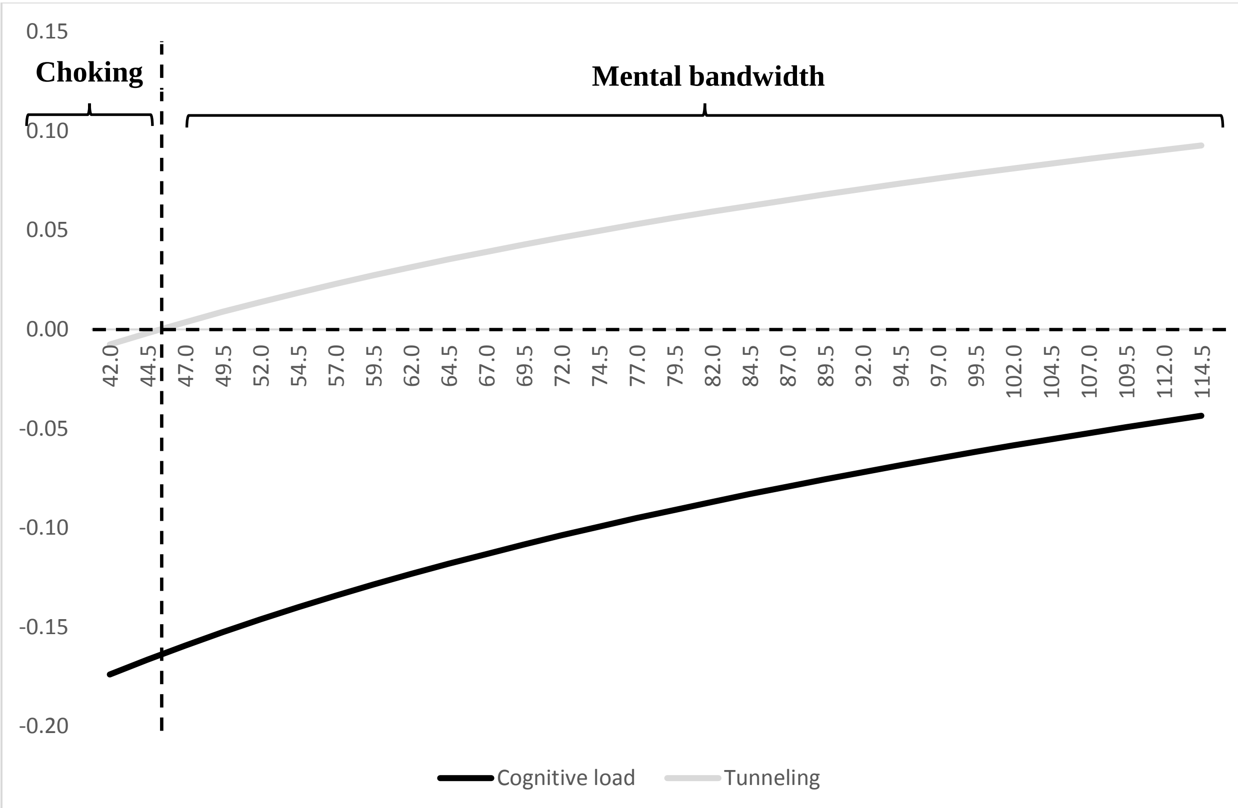
Panel A: Predicted effects of priming



<u>Notes on Figure 8 – Panel A</u>:

1. Predicted effects of priming on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample;
2. We call *mental bandwith* the simultaneous occurrence of cognitive and tunneling;
3. We call *rational inattention* the phenomenon that, among the subjects in the least poor municipalities, priming *only enhances* their performance in tasks involving scarce resources, without deteriorating their performance overall.

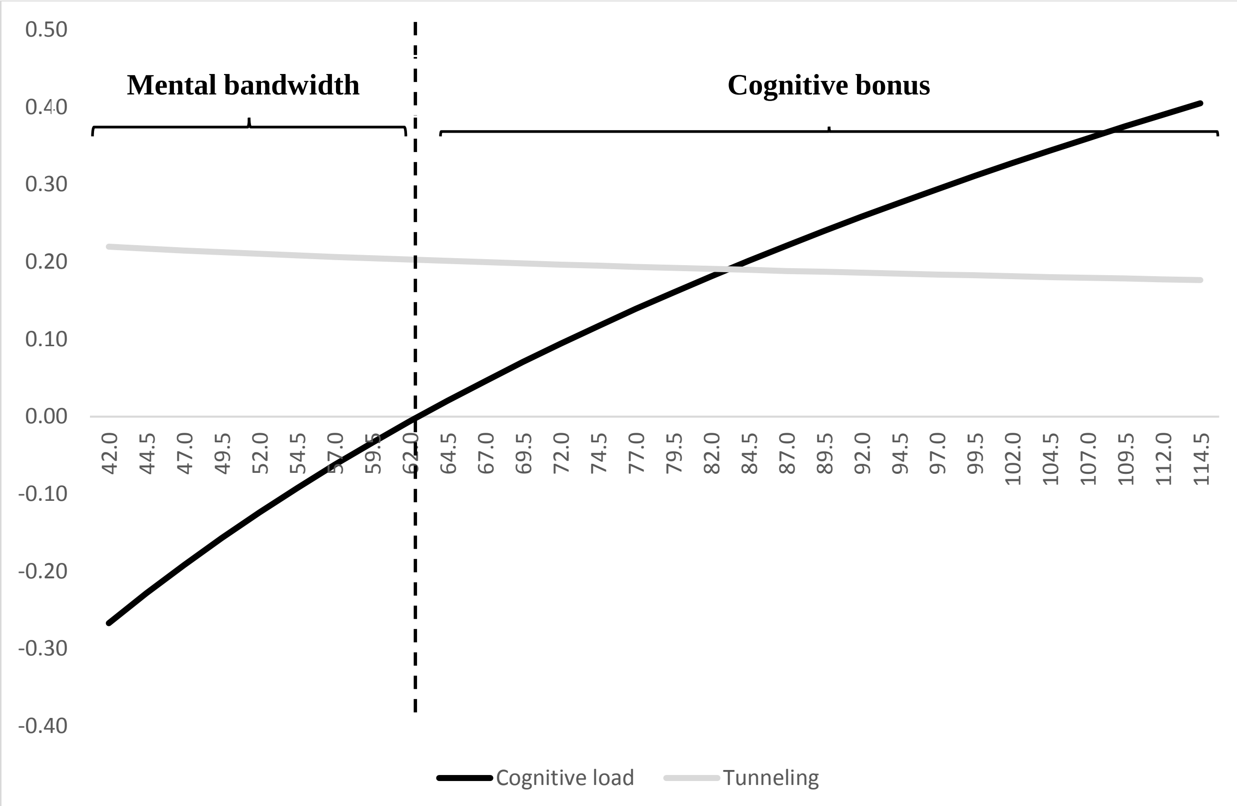
Panel B: No rainfall in t-3



<u>Notes on Figure 8 – Panel B</u>:

1. Predicted effects of no rainfall in t-3 on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample;
2. We call *choking* the phenomenon that the negative impacts of scarcity on cognitive function among the subjects in the poorest municipalities are *magnified* – rather than (partially) reversed –, within tasks involving scarce resources, presumably a reaction to high stakes in line with Ariely et al. (2009);
3. We call *mental bandwidth* the simultaneous occurrence of cognitive and tunneling.

Panel C: CCT payment within next 3 days



<u>Notes on Figure 8 – Panel C:</u>

1. Predicted effects of CCT payment within next 3 days on cognitive load and tunneling based on the estimates from Table 8, within the range of monthly per capita income of the municipalities in our sample;
2. We call *mental bandwidth* the simultaneous occurrence of cognitive and tunneling;
3. We call *cognitive bonus* the improvement in cognitive performance across all dimensions, particularly in tasks involving scarce resources.

Tables

Table 1 – Effects of priming, rainfall and distance to payday on worries about rainfall and household bills

	Full Sample				Early Waves		Full Sample	CCT sample
	Rainfall	Rainfall	Rainfall	Rainfall	Rainfall	Bills	Rainfall	Rainfall
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Priming	0.050	0.057*	0.148***	0.003	0.114**	0.013	0.054	0.008
	(0.034)	(0.034)	(0.056)	(0.065)	(0.047)	(0.052)	(0.034)	(0.060)
Wave			0.106***					
			(0.021)					
Priming x Wave			-0.068**					
			(0.030)					
No Rainfall summary measure							0.221***	0.183***
							(0.048)	(0.062)
Priming x No Rainfall s.m.							-0.098	
							(0.062)	
Distance to Payday								0.002
								(0.005)
Priming x Harvest Loss (2014)				0.141				
				(0.156)				
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Wave Fixed Effects	N	Y	N	N	N	N	N	N
Observations	3,871	3,871	3,871	3,871	2,131	1,929	3,871	1,212
R-squared	0.031	0.043	0.038	0.031	0.047	0.040	0.038	0.065

Notes on Table 1:

1. All columns are OLS regressions with standardized worries (z-score) as dependent variable, about rainfall in columns (1)-(4) and (6)-(8), and about household bills in column (5). See Appendix A for the definition of each variable;
2. No rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
3. Robust standard errors in parenthesis, clustered at the individuals level; *** p<0.01, ** p<0.05, * p<0.1;
4. $Corr$(worries about rainfall, irrigation) $= -0.035$.

Table 2 – Effects of priming and rainfall on cognitive load

	Cognitive Load				
	(1)	(2)	(3)	(4)	(5)
Priming	-0.0458**		-0.0498***		
	(0.02)		(0.02)		
No rainfall summary measure		-0.108***	-0.113***		
		(0.02)	(0.027)		
Priming x No rainfall s.m.			0.00684		
			(0.036)		
No rainfall in t-3				-0.119***	
				(0.021)	
No rainfall in t-7					-0.0965***
					(0.021)
Municipality FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Observations	2,362	2,362	2,362	2,362	2,362

Notes on Table 2:

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 component. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that negative values mean worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring);
3. No rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
4. Controls include all baseline variables reported in Tables D1 to D3;
5. The number of observations reported is the minimum across all summary measure components;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 3 – Effects of priming and rainfall on tunneling

	Tunneling				
	(1)	(2)	(3)	(4)	(5)
Priming	0.0403**		0.0382*		
	(0.021)		(0.021)		
No rainfall summary measure		0.0429*	0.00317		
		(0.024)	(0.031)		
Priming x No rainfall s.m.			0.0814*		
			(0.042)		
No rainfall in t-3				0.0478*	
				(0.027)	
No rainfall in t-7					-0.00864
					(0.028)
Municipality FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Observations	1,138	1,138	1,138	1,138	1,138

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 focus and framing; where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure component. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that positive values mean better relative cognitive performance (higher relative attention towards scarce resources, higher valuation of scarce resources, and lower sensitivity to framing biases in tasks involving scarce resources);
3. No rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
4. Controls include all baseline variables reported in Tables D1 to D3;
5. The number of observations reported is the minimum across all summary measure components;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 – Effects of priming and rainfall on reaction times

	Cognitive Load	Tunneling
	(1)	(2)
Priming	-0.062	-0.13
	(0.043)	(0.222)
No rainfall summary measure	0.039	-1.2***
	(0.043)	(0.27)
No rainfall in t-3	0.056	-0.61**
	(0.046)	(0.281)
No rainfall in t-7	0.043	-0.63**
	(0.048)	(0.277)
Municipality FE	Y	Y
Observations	2,362	1,138

<u>Notes on Table 4:</u>

1. Each cell represents a different regression;
2. All cells are Seemingly Unrelated Regressions (SUR) with time taken to respond to each question/task as dependent variable, measured in seconds, within each outcome category;
3. No rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
4. The number of observations reported is the minimum across all summary measure components;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 5 – Lee Bounds for the effects of priming and rainfall on cognitive loads and tunneling

	Cognitive Load		Tunneling	
	Lower Bound	Upper Bound	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)
Priming	-0.045	-0.043	0.043	0.043
IC 90% =	-0.0833	-0.0042	0.0013	0.0837
No Rainfall in t-3	-0.12	-0.11	0.047	0.048
IC 90% =	-0.1604	-0.0716	-0.0065	0.1007
No Rainfall in t-7	-0.097	-0.1	-0.0086	-0.0086
IC 90% =	-0.1377	-0.0611	-0.0636	0.0463
Municipality Fixed-effects	Y	Y	Y	Y

Notes on Table 5:

1. Each row represents a different regression;
2. All cells 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, in columns (1) and (2), and for focus and framing, in columns (3) and (4); 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.
3. Outcomes are normalized such that negative values mean worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring; lower relative attention towards scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources);
4. Bounds computed following Lee (2009)'s procedure;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 6 – Effects of priming and rainfall on cognitive loads and tunneling: early vs. late waves

	Priming	No rainfall s.m.	No rainfall in t-3	No rainfall in t-7
	(1)	(2)	(3)	(4)
Panel A: Cognitive Load				
Treatment	-0.052*	-0.055	-0.051*	-0.078**
	(0.028)	(0.034)	(0.029)	(0.038)
Treatment x Late Waves	0.021	-0.14**	-0.11*	0.062
	(0.039)	(0.058)	(0.065)	(0.056)
Late Waves	-0.11***	-0.018	-0.085***	-0.044
	(0.027)	(0.032)	(0.026)	(0.038)
Municipality FE	Y	Y	Y	Y
Observations	2,632	2,632	2,632	2,632
Panel B: Tunneling				
Treatment	-0.015	0.083**	0.06*	0.021
	(0.032)	(0.041)	(0.035)	(0.036)
Treatment x Late Waves	0.094**	-0.0044	-0.015	-0.086
	(0.044)	(0.06)	(0.063)	(0.062)
Late Waves	-0.054*	-0.051	-0.024	-0.013
	(0.031)	(0.034)	(0.029)	(0.03)
Municipality FE	Y	Y	Y	Y
Observations	1,138	1,138	1,138	1,138

Notes on Table 6:

3. 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, in Panel A, and for focus and framing, in Panel B; 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. Outcomes are normalized such that negative values mean worse cognitive performance;
4. No rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
5. Late waves = 1 if the respond was surveyed in May or June, and 0 otherwise;
6. The number of observations reported is the minimum across all summary measure components;
7. *** p<0.01, ** p<0.05, * p<0.1.

Table 7 – Effects of priming, rainfall and distance to payday on cognitive loads and tunneling: Bolsa-Família sample

	Worries about rainfall	Worries about bills	Cognitive Load	Tunneling
	(1)	(2)	(3)	(4)
Priming	0.002	-0.063	-0.064*	0.045
	(0.06)	(0.068)	(0.036)	(0.039)
No rainfall summary measure	0.17***	0.094*	-0.15***	0.051
	(0.061)	(0.052)	(0.036)	(0.041)
Distance to payday	-0.0031	0.0014	0.003	-0.0017
	(0.003)	(0.003)	(0.002)	(0.002)
Payment within next 3 days	-0.054	0.0035	0.16	0.34**
	(0.114)	(0.163)	(0.137)	(0.169)
Payment within next 7 days	-0.054	0.0008	0.054	0.21**
	(0.086)	(0.088)	(0.067)	(0.081)
Municipality FE	Y	Y	Y	Y
Observations	759	357	1,212	1,055

Notes on Table 7:

1. Each cell represents a different regression. In cols (1) and (2), all cells are OLS regressions with standardized worries (z-score) as dependent variable. In cols (3) and (4), all cells 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, in column (3), and for focus and framing, in column (4); where $\hat{\sigma}_{j_c}$ is the standard deviation (at the individual level) in the control for each summary measure in each column. See Appendix A for the definition of each variable;
2. Outcomes are normalized such that negative values mean worse cognitive performance (lower attention, memory and impulse control, and higher sensitivity to anchoring; lower relative attention towards scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources);
3. No rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2.
4. The number of observations reported is the minimum across all summary measure components;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 8 – Effects of priming, rainfall and distance to payday on cognitive load and focus by municipality's per capita income

	Priming	No rainfall s.m.	No rainfall in t-3	No rainfall in t-7	Distance to payday	Payment within next 3 days	Payment within next 7 days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Cognitive Load							
Treatment	-1.2**	-0.35	-0.84	-1.1*	0.091	0.48	-3.7**
	(0.549)	(0.494)	(0.601)	(0.593)	(0.061)	(1.622)	(1.828)
Treatment x ln(per capita income)	0.2**	0.042	0.13	0.19*	-0.016	-0.07	0.67**
	(0.099)	(0.089)	(0.108)	(0.107)	(0.011)	(0.294)	(0.33)
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Wave Fixed Effects	N	N	N	N	N	N	N
Observations	2,632	2,632	2,632	2,632	759	759	759
Panel B: Tunneling							
Treatment	-0.85	-0.39	-0.52	0.021	-0.0051	2.6	0.44
	(0.566)	(0.596)	(0.672)	(0.733)	(0.063)	(2.577)	(2.704)
Treatment x ln(per capita income)	0.16	0.079	0.1	-0.0075	0.00063	-0.44	-0.043
	(0.102)	(0.107)	(0.121)	(0.132)	(0.011)	(0.471)	(0.486)
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Observations	1,138	1,138	1,138	1,138	357	357	357

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) with standardized dependent variables (z-scores) for executive functions and anchoring, in Panel A, and for focus and framing, in Panel B; 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. Outcomes are normalized such that negative values mean worse cognitive performance;
2. No rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
3. ln(per capita income) from the 2010 Census by the Brazilian Institute for Geography and Statistics (IBGE);
4. The number of observations reported is the minimum across all summary measure components;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 9 – Effects of priming, rainfall and distance to payday on money earned in the experiments

	Full Sample		Bolsa-Família sample	
	Cognitive Load	Tunneling	Cognitive Load	Tunneling
	(2)	(3)	(4)	(5)
Priming	0.023	0.057***	0.053	0.054
	(0.02)	(0.021)	(0.037)	(0.039)
No Rainfall summary measure	0.081***	0.13***	0.13***	0.16***
	(0.021)	(0.024)	(0.037)	(0.041)
No Rainfall in t-3	0.079***	0.11***	0.16***	0.15***
	(0.022)	(0.028)	(0.04)	(0.049)
No Rainfall in t-7	0.058***	0.0091	0.056	0.023
	(0.021)	(0.028)	(0.038)	(0.049)
Distance to Payday	-	-	-0.00054	-0.0012
			(0.002)	(0.003)
Payment within next 3 days	-	-	-0.32**	0.26*
			(0.15)	(0.151)
Payment within next 7 days	-	-	-0.15**	0.14*
			(0.071)	(0.081)
Municipality Fixed Effects	Y	Y	Y	Y
Observations	2,362	1,138	1,212	1,055

Notes on Table 9:

1. Each cell represents a different regression;
2. All cells are Seemingly Unrelated Regressions (SUR) with dependent variable equal to 1 if the subject earned R$ 1 in that call (by obtaining scores among the top-25% performers within the tasks that can be scored), and 0 otherwise, within each outcome category;
3. No rainfall summary measure is a Post-LASSO measure of rainfall shocks predictive of worries about rainfall; see Table C2;
4. The number of observations reported is the minimum across all summary measure components;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 10 – Effects of rainfall shocks on information recall outside of the experiments

	No. of rainy days in previous month		Volume in water tank	
	Inaccuracy = 0 if guess = actual +-5, = 1 otherwise		Inaccuracy = 0 if guess = actual +- 350ml, = 1 otherwise	
	(1)	(2)	(3)	(4)
Below-normal rainfall in previous month	0.00477	0.176***	0.0147***	0.0147***
	[0.0189]	[0.0363]	[0.00550]	[0.00566]
Actual		0.0546***		0.0500***
		[0.00435]		[0.0145]
Actual x Below-normal rainfall		-0.00977**		0.00129
		[0.00384]		[0.00120]
Wave FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Sample mean	0.36	0.36	0.38	0.38
Observations	3,589	3,589	3,508	3,508
R-squared	0.542	0.582	0.961	0.963

Notes on Table 10:

1. All columns are OLS regressions with an inaccuracy indicator as dependent variable, equal to 1 if the guess is within a close range of the correct answer, and 0 otherwise. In columns (1) and (2), we code accurate answers as those within 5 days of the actual number of rainy days in the previous month; in columns (3) and (4), as those within 350ml of the volume in the water tank;
2. Below-normal rainfall equals 1 when the previous month ranks among the 1/3 worst within the last 30 years in rainfall volume, and 0 otherwise;
3. In columns (1) and (2), Actual stands for the actual number of rainy days in farmer's municipality the previous month; in columns (3) and (4), for the actual volume in farmer's water tank (in L);
4. *** p<0.01, ** p<0.05, * p<0.1.

Supplementary Appendix – Additional Figures and Tables

Table S1 – List of variables used in LASSO's feature selection space

- Rainfall levels at t-1, t-2, t-3, t-4, t-5, t-6 and t-7

- Rainfall occurence at t-1, t-2, t-3, t-4, t-5, t-6 and t-7

- Cumulative rainfall levels in the past 2, 3, 4, 5, 6, 7, 14 and 21 days

- Number of rainy days in the past 2, 3, 4, 5, 6, 7, 14 and 21 days

- Absolute deviation from 30-year municipality-level average (adjusted for the window) at t-1, t-2, t-3, t-4, t-5, t-6 and t-7

- % deviation from 30-year municipality-level average (adjusted for the window) at t-1, t-2, t-3, t-4, t-5, t-6 and t-7

- Cumulative absolute deviation from 30-year municipality-level average (adjusted for the window) in the past 7, 14 and 21 days

- Product of cumulative absolute deviation and number of rainy days in the last 7, 14 and 21 days

- Distance to Mar-19th, Ceará's patron saint day, when farmers believe that whether it rains determines a good rainy season

Table S2 – Validation of audio versions of digit span and stroop adapted for phone surveys

Raw correlation		Phone: 1 month later	Phone: 2 months later	Phone: 3 months later
Phone (Mar)	Stroop	0.59	0.31	-0.03
	Digitspan	0.43	0.34	0.32

Raw correlation		Face-to-face (9 months prior): Stroop	Face-to-face (9 months prior): Digitspan	Face-to-face (9 months prior): Raven's matrices
Phone (Mar-Apr)	Stroop	0.18	-	0.24
	Digitspan	-	0.23	0.72

Notes on Table S2: