Cognitive Droughts ‡

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ABSTRACT

Does poverty divert attention and impede cognition in ways that could distort decision-making and generate poverty traps? If yes, which dimension of poverty contributes more to such adverse impacts: is it low *levels* of income or income *uncertainty* that could drag consumption below subsistence levels? We examine these issues using randomly occurring real-life shocks faced by farmers in a drought-prone region of Brazil: payday variation affecting income levels, and rainfall shocks that affects income uncertainty. We find that it is income uncertainty that systematically impedes cognitive function; low income levels have adverse cognitive effects only among the poorest households. The net adverse impacts on cognitive function prevail even though both dimensions of poverty trigger an increase in attention allocated to scarce-resource tasks. These results broaden our understanding of the impacts of uncertainty by exploring a psychological channel distinct from risk aversion, and help reconcile apparently contradictory evidence on the cognitive impact of poverty in previous studies.

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1 Introduction

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, which 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 captures two fundamental dimensions of poverty: it involves enduring (1) low income *levels* on average, but also (2) high income *uncertainty*, at the risk of not having enough to meet even basic needs on any particular day.[1] This paper studies the extent to which each of these two dimensions drive the psychological effects of poverty on attention allocation and cognition.

Income uncertainty is a central feature of the lives of the poor in the developing world (e.g. Scott, 1977; Karlan et al., 2014), although not exclusively so; in fact, income uncertainty has now become a reality for poorer households in rich economies like the US too.[2,3] Rational responses to that uncertainty – such as through risk-aversion – have been extensively studied by economists. However, risk aversion in choices made under these circumstances does not fully capture how pervasive the effects of income uncertainty under poverty can be. When a person is one bad harvest away from financial ruin, one illness away from being unable to work and feed one's family or one missed rental payment away from eviction and all its consequences, the scepter of

[1] We use the term *uncertainty* as a stand-in for a mixture of *risk* and *known uncertainty* (as in Bloom, 2014) rather than unknown (or Knightian) uncertainty (Knight, 1921).

[2] Over half a billion people worldwide live in arid regions without access to irrigation. 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 alone, 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 share of agriculture in GDP (Benson and Clay, 1998). See also the World Bank's *World Development Report 2014* on "Risk and Opportunity," for a description of the huge variety of macro and micro risks faced by households and firms in developing countries.

[3] For instance, Dynan et al. (2012) find that American household incomes became 30 percent more volatile between the early 1970s and the late 2000s – a pattern corroborated by other studies drawing on numerous nationally representative data sets including the Survey of Income and Program Participation (SIPP) and the Current Population Survey (CPS). See Morduch and Schneider (2019), Chapter 1 for details. The findings from both their financial diaries project and the nationally representative Survey of Household Economics and Decision-making (SHED) show that a disproportionate burden of such income uncertainty falls on poor families, making it a hidden source of inequality in the US.

such bad shocks loom large – even if they never come to pass.[4] The economic consequences of such psychological toll from enduring income uncertainty and the risk of being driven into a downward income spiral under poverty have not been systematically examined. Such worries can keep a poor person up at night and distract him during the day, potentially affecting decision-making even in domains unrelated to the source of risk – be it engaging in conversation with one's children or monitoring their homework, or being able to concentrate on tasks at work.[5]

No doubt, living on low income *levels* itself involves continually making difficult financial tradeoffs between expenses that feel equally important – for instance, paying for children's school books versus a parent's doctor visit, or between shoes needed to walk to work and fixing a leak in the roof. Such tradeoffs can be cognitively taxing and emotionally exhausting, with adverse knock-on effects on other decisions. At the same time, the psychological tax from worries induced by income uncertainty may be larger, because of the sense of helplessness and loss of control over one's decisions that come with it. For instance, the findings of the 2014 Pew Charitable Trust survey are consistent with such a more damaging psychological impact of income uncertainty: when asked if they would prefer to be a little richer or have a steadier, more stable financial life, 92% of American respondents chose stability over mobility (Morduch and Schneider, 2019). In this paper, we provide first-hand evidence of the differential effects of these two channels on attention allocation and cognition among the poor.

We focus on these specific aspects of psychological impact because attention allocation is a unifying theme for much of behavioral economics (Gabaix, 2018), and because cognitive function is at the core of all decision-making (Burks et al., 2009; Dohmen et al., 2010; Benjamin et al., 2013). To guide our empirical investigation, we draw on Mullainathan and Shafir (2013), which proposes two possible types of cognitive effects of poverty. First, worries that preoccupy a poor

[4] See Poor Economics (2011, Chapter 3) on poverty traps created by a single illness – a cycle of lost income, greater health expenditures and mounting debts. See Desmond (2015) for a searing ethnographic account of the consequences of housing evictions among the poor in the US: worse prospects for future housing and jobs, children pulled out of school, and moving to neighborhoods with higher rates of crime. Studies have linked eviction to psychological trauma (Fullilove, 2005) and have identified it as a risk factor for suicide (Serby et al., 2006).

[5] Evidence shows that the poor are less productive workers (Kim et al., 2006) and less attentive parents (McLoyd ,1998). Fewer conversations with children has been linked to a vocabulary gap of 15 million or more words between children from poorer versus richer households by the age of four (Fernald, 2013).

person could deplete (or tax) her overall mental attention resources. In the cognitive psychology literature, this effect is referred to as *cognitive load*. Second, a poor person's available attention resources can be captured by (or reallocated to) the urgent or imminent challenge where scarcity looms. This effect is referred to as *tunneling*. Together, these two effects could affect the quality of decisions of the poor, by reducing attention resources available for important issues or by diverting them too narrowly to issues that are urgent.[6] We provide multiple field-based measures of both these conceptually-grounded aspects of attention allocation under poverty.

To study the impact of income uncertainty under poverty, we exploit (daily) natural rainfall variation during the rainy season.[7] We combine this shock with lab-in-the-field survey experiments that randomly expose some farmers to drought-related worries (what the cognitive psychology literature calls *priming*). This two-pronged exogenous variation approach to studying uncertainty allows us to combine the strengths of both approaches.[8]

To examine the impact of low-income *levels*, the natural shock we use is variation in the timing of payments under *Bolsa Família*, Brazil's flagship conditional cash transfer (CCT) program. Under *Bolsa Família*, a beneficiary's monthly payday is determined by the last digit of his or her social security identification number (*Número de Identificação Social*, or NIS), which is randomly assigned. Hence the distance to his next payday at the time of our surveys is as good as random. The program has been in place for over ten years, hence beneficiaries in our sample know exactly

[6] While these effects could be rational responses to changes in the relative price of allocating attention to different tasks, lab experimental evidence is also consistent with an element of *unconscious* (or irrational) 'mental capture'. For instance, tunneling responses triggered by particular stimuli occur at speeds below the consciousness threshold (Mullainathan and Shafir, 2013). Such unconscious mental responses are consistent with the idea of 'System 1' thinking described by Kahnemann (2011) or stage 1 of a two-stage optimization process as modeled under the Sparsity framework (Gabaix, 2014).

[7] We focus on a post-LASSO summary measure of recent rainfall shocks most predictive of farmers' worries. In addition, we use indicator variables of whether it rained 3 or 7 days prior to the survey (both selected as LASSO instruments) to provide a direct benchmark to the effects of distance to payday (respectively, 3 and 7 days before payday).

[8] Rainfall shocks provide external validity because they occur naturally in farmers' daily lives, with the variation being at the regional (municipality) level. In contrast, survey experiments shed light on the specific psychological mechanism of interest hence providing internal validity; they also improve precision because they are at the individual (phone call) level. Conceptually, priming manipulates *exposure* to uncertainty (as the underlying distribution of states is unaffected), while rainfall shocks manipulate both *exposure* (especially early in the rainy season, when uncertainty still has not unraveled) and *realization*.

what they will be paid every month and when. Thus, the only thing that varies by time distance to a person's next payday is his *level* of income; there is no uncertainty whatsoever.

To separate the cognitive effects of low income levels and uncertainty, we study the impact of the shocks described above within the *same* setting, in a sample with substantial spatial and temporal variation: 47 municipalities in the drought-prone Ceara region in northern Brazil, over four months of the rain season (March-June), where we tracked the behaviour of over 2,800 farmers. We introduce multiple innovations to overcome the logistical challenges of studying subtle psychological effects over such a wide field in space and time. We survey farmers over automated phone calls (IVR), adapting standard (visual) attention tests to audio in order to gather reliable data on attention and cognition in response to shocks across space and over time. Farmers do not pay to participate, and are incentivized by airtime credit. Cognitive function is captured by participants' overall performance in tasks that measure their working memory, attention and impulse control. Attention reallocation is captured by participants' relative performance in tasks involving scarce resources (money and water) and by their stated valuation of scarce resources in trade-offs against other, non-scarce resources.

We have two key findings that we describe below. First, income uncertainty and risk (due to negative rainfall shocks and priming) induces significant cognitive load as well as tunnelling effects. The drop in performance in tasks measuring cognitive function is equivalent to 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), suggesting this hitherto unexamined *psychological* impact of income uncertainty could have sizeable implications for behavior and decision-making of the poor. These adverse cognitive load effects peak at the low end of income distribution, and gradually taper of in richer municipalities.

Such income uncertainty also increases tunneling: respondents are better and faster at finding words related to scarce resources, likely to value such resources higher in trade-offs against non-scarce resources, and less sensitive to framing in tasks involving these resources when exposed to priming and bad rainfall shocks. Further, being primed with worries in the face of bad rainfall shocks compounds such tunneling effects.

Unlike with income uncertainty, low income *levels* induced by random payday variation *do not* systematically increase cognitive load, except *within the poorest municipalities*. Within these poorest areas, the magnitude of cognitive load is significantly higher than that due to income uncertainty. Such load is fully consistent with the fact that CCT payments are a much larger share of family income in these areas, creating significant stresses from managing difficult financial tradeoffs on a tight budget as the next payday approaches. Low income levels also induce a tunneling effect, both within these poorest municipalities and across the rest of theincome spectrum. In terms of the relative impact, low income levels (at three days before a *Bolsa Família* payday) induce a tunneling effect that is two thirds that of uncertainty (from a negative rainfall shock).

Given such attention reallocation towards scarce resources, one possible concern is that, in real-world tasks that the poor engage in under financial pressure, tunneling may overturn the adverse cognitive load effects of being poor. The effect sizes of each mechanism across the income distribution suggest otherwise. Using standardized measures for our cognitive load and tunneling outcomes, we find that it is the cognitive load effect that dominates. In other words, taken together, our results show that the cognitive burden imposed by income uncertainty makes farmers 'penny wise and pound foolish'.

Our final insight is about how these two sets of findings allow us to reconcile apparently contradictory results across previous studies on the cognitive impacts of poverty. Field evidence from sugarcane farmers around harvest (Mani et al., 2013) and workers around payday (Kaur et al., 2019) as well as lab-in-the-field evidence from the US (Shah, Mullainathan and Shafir, 2012, 2015) shows significant cognitive load from poverty. In sharp contrast, a recent study of low-income US households does not find evidence of adverse cognitive effects (Carvalho, Meier and Wang, 2016). Our results offer a simple explanation for this seemingly contradicting evidence: farmers in the sugarcane study faced uncertainty in the amount and/or the timing of their harvest payments; this is unlikely for respondents in the Carvalho, Meier and Wang (2016) study, since only respondents who provided complete information about the number of payments and payment dates were included (see footnote 13 in their paper). As we show, it is income uncertainty under poverty that systematically depletes mental attention resources of the poor;

low income levels create cognitive load effects only among the poorest of the poor. Thus, the absence of cognitive load effects in the US study by Carvalho, Meier and Wang (2016) could be explained by (i) the lack of uncertainty in payment timing and amounts, coupled with (ii) higher income levels of the US respondents, relative to Indian farmers in Mani et al. (2013) and to the poorest Brazilian farmers in this study.

To summarize, our findings show that income uncertainty lies at the core of poverty's psychological tax. This result complements recent findings on the detrimental effects of uncertainty on decision-making: the fact that cognitive effects of uncertainty are worse at lower income levels distinguishes this mechanism from a loss of the 'power of certainty', which has been shown to impair contingent reasoning irrespective of income levels (Martínez-Marquina, Niederle and Vespa, 2019). Our findings also share links to a recently burgeoning literature on the economic effects of uncertainty (Bloom, 2014; Bianchi, Kung and Tsikhi, 2018; Bloom et al., 2018). For instance, Bloom et al. (2018) models recessions as (productivity) shocks with a negative first moment and a positive second moment – which, in terminology we use here, translate to adverse effects of lower income levels and greater income uncertainty, respectively. Our results suggest that uncertainty that characterizes recessions can impair decision-making at the micro level, especially among the poor. In turn, the cognitive responses to income uncertainty we examine here could then magnify the effects of such macro-level shocks.

Our findings also link to an emerging literature on the psychology of poverty and decision-making. Apart from the afore-mentioned work, this literature explores the impact of psychological channels such as stress (Haushofer and Shapiro, 2016) and conditions associated with poverty, such as alcohol consumption, pain, sleep deprivation, environmental noise and malnourishment, on decision-making (see, for instance, Schilbach, 2019; Bessone et al., 2019; Dean, 2019; Schofield, 2014). It also ties into a larger literature on behavioral development economics, which examines how various biases coming from non-standard preferences, beliefs and decision-making could explain various puzzles in developing economies (see Kremer, Rao and Schilbach, 2018, for a comprehensive survey).

Given uncertainty's potentially costly consequences for decision-making among the poor, our findings suggest new roles for existing policy instruments – such as providing insurance against income uncertainty (Lichand and Mani, 2019) – and reasons why those that boost income levels alone (such as cash transfers) may not be fully effective.[9] They also point to a need to combine these with new instruments to counteract detrimental effects of inefficient attention reallocation: for instance, increasing the salience of the relevant decision features and adapting choice architecture (Lichand et al., 2019) could support decision-making among the poor.

2. On the Ground: Study Setting, Sample and Survey Implementation Logistics

2.1 Setting: The State of Ceará, Brazil

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 experienced 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 a state of emergency, necessitating 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 anxiety and mysticism linked to rainfall forecasts (see Taddei, 2013, for a detailed anthropological account), making it an appropriate environment in which to study the psychological effects of risk and uncertainty.

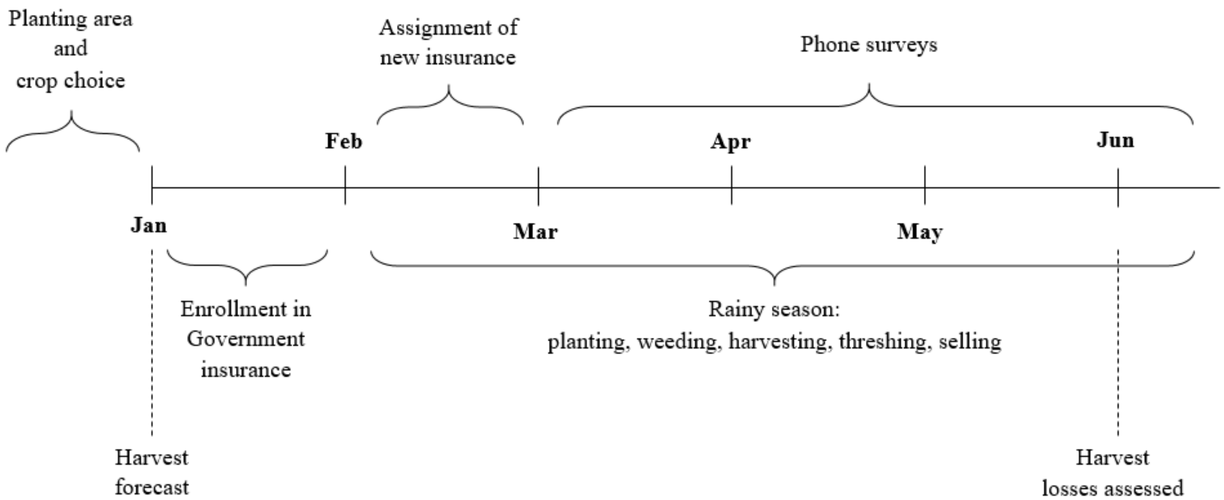
2.2 Timeline

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. Most production decisions – in particular, land preparation and crop choice – are undertaken before January, in time for the pre-season. If rainfall permits, farmers plant mostly corn and beans. Over the course of the rainy season, the margin that farmers can adjust on

[9] Bloom et al. (2018) also show that increased uncertainty makes 'first-moment' policies, like wage subsidies, temporarily less effective because firms become less responsive to price changes in the face of uncertainty.

involves labor. Enrollment in government crop insurance generally takes place by the end of January, before the onset of the rain.

Figure – Timeline

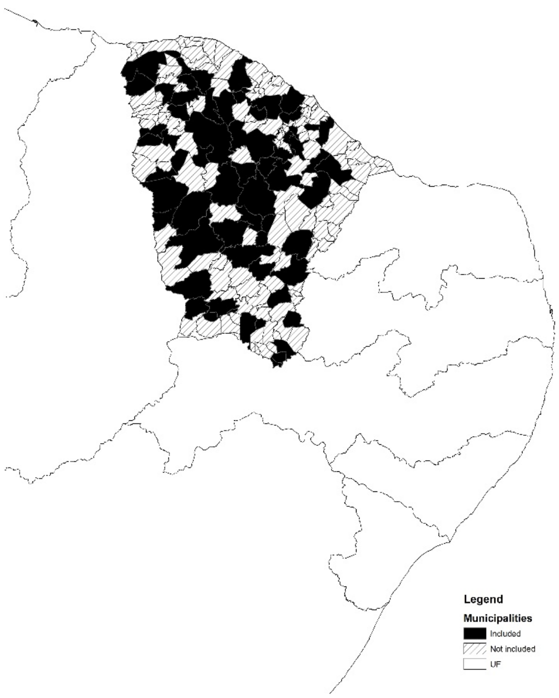


Our baseline data collection was carried out in the month of February, after farmers had made planting decisions. This timing is in keeping with our interest in measuring the psychological effects of income uncertainty on decision-making, through channels other than risk aversion (for instance, in crop choice). Subsequent waves of data collection occurred in the first two weeks of each of the following four months, from March through June

2.3 Data Collection Sample

In partnership with Ceará's Rural Development Secretariat, we enrolled 4,084 farmers across 47 municipalities of the hinterlands of the State in our study, in January 2015. In each of these municipalities, enrolment was carried out through agricultural extension workers, who were given 100 consent forms to be handed to the family farmers they oversee. Farmers who opted-into the study provided their mobile phone number to us through these forms. Within each municipality, we directed half of the forms to farmers living in the most drought-prone region in the municipality, and the other half to those living in the least drought-prone region. Due to the high heterogeneity in microclimates within-municipality, we use this information for stratifying treatment assignment in the survey experiment.

Figure – Geographic coverage of the surveys



Of the 4084 farmers enrolled, 2822 farmers responded to one or more of our survey phone calls over the 4 waves of data collection. Each wave consisted of 6 phone calls, resulting in a total of 24 phone calls. Table C1 shows the distribution of respondents for each number of calls, from 0 to 24. As seen there, about 50% of the sample took up to 8 calls while 1,262 farmers responded to none. About the latter group, we cannot tell if they did not respond because the phone number provided was wrong or inactive at the time of the surveys, if the telecommunications' tower coverage in some regions was poor enough that they never had a connection 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]

2.4 Cross Matching with Bolsa Familia payday data

In addition to our own data collection, we obtained data for a subset of our respondents on their family's monthly payday under Bolsa Familia (Brazil's flagship conditional cash transfer program, in place since 2003), as follows. First, we linked a family's farmer ID (*Declaração de Aptidão ao PRONAF*, DAP) to his or her unique individual social information number (*Número de Informação Social*, NIS) ~~obtained from the Ministry of Rural Development~~ (achieved for 96.4% of our respondents). Next, for every successful match, we obtained information from CadÚnico, the administrative cadaster for Bolsa-Família ~~housed in the Ministry of Social Development~~, to verify whether that household was actually receiving CCT payments at that 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) could be the one on which the payment schedule is based. Even though CadÚnico lists all NISs 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 among those is the NIS of the main beneficiary.

To make this assignment, we resort to the following procedure: Whenever there is only one NIS in the household, assignment is straightforward. However, if there are two, we apply an algorithm to identify female Brazilian first names[10], as women are primarily the main beneficiaries of Bolsa-Família (for 92% of the households, see Bartholo, 2016). Finally, if the algorithm identifies either none or both first names as female, our assignment algorithm picks the first NIS listed for that household in CadÚnico as the main beneficiary. Doing so yields 1,035 subjects in households receiving Bolsa-Família (36.7% of our sample) with information about their Bolsa-Família 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.

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

[Table C4]

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 to detect effects sizes similar to those we document for priming and rainfall shocks in the full sample, within reasonable time windows from payday.

2.5 Survey Implementation Logistics: The use of 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 in our setting. 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.

Equally, conducting 24 rounds of lab-in-the-field experiments across almost 50 different towns in the hinterlands of Brazil poses a non-trivial logistical challenge. We therefore 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 respond 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 in phone surveys can be high, particularly for longer calls. 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 manner suitable to conducting them 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).[11] Third, farmers may have no interest in taking these psychological tests seriously, a possibility that could greatly limit the statistical power of the tests we undertake. To deal with this issue, we introduce reverse billing whereby farmers received credit for the time they spent responding to our calls. We also 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.[12]

3 Measuring the Psychological Impact of Low and Uncertain Incomes: Concepts, Experimental Design and Estimation

Section 3.1 offers a conceptual framework that describes existing theories on how income uncertainty and risk can affect attention resources and decision-making, via channels other than risk aversion. Subsection 3.2 describes the study design including variations – some experimental and others naturally occurring -- that we exploit to examine these psychological effects. Next, subsection 3.3 describes the main outcome measures we use to capture these effects. Finally, subsection 3.4 gives details on the econometric procedures we use to estimate the effects of interest, address selective attrition challenges and account for multiple hypotheses testing.

3.1 Conceptual framework

Living on a low level of income without access to opportunities to smooth consumption implies that the poor find it harder to anticipate and cope with even known ("Knightian") uncertainties and risks associated with their income. As a result, the threat of a one-time shock that could derail a poor family and drag it into a downward spiral could take a huge psychological toll, over and above the toll from having to manage on a meagre income. We hypothesize that such psychological effects would arise *ex-ante* from *exposure* to the risk alone – whether or not the risk actually *materializes*.

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

Psychological theories consider a variety of mechanisms -- other than risk aversion – through which such income uncertainty and risk may affect decision-making. One 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 due to exposure to future risk, penalizing present consumption experiences. It could also affect expected utility, with a knock-on effect on decision making.

An alternative mechanism is the affect heuristic (Finucane et. al, 2000). According to this theory, feelings influence how individuals *perceive the probability distribution of future states* and hence the expected outcomes of such a lottery. For instance, a previous negative experience could mean that a person perceives the probability of the bad state to be 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).

Other theories posit that exposure to uncertainty and risk may result in psychological effects that cause people to move away from optimizing behavior. For instance, 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. Stress could also increase the influence of external stimuli (Eysenck et al., 2007). Even if through a different mechanism, most predictions from this model also operate through risk aversion. Relatedly, the "risk as feelings" hypothesis (Loewenstein et al., 2001) 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. In this latter case, risk could deteriorate the quality of *all decisions,* not only those related to consumption smoothing.

Finally, the Scarcity hypothesis (Mullainathan and Shafir, 2013; Schilbach, Schofield and Mullainathan, 2016; Dean, Schilbach and Schofield, 2019) posits that individuals worrying about scarce resources suffer two types of consequences. First, such worries deplete mental resources (or bandwidth), hence reducing what remains available to be gainfully harnessed, i.e. they induce a *cognitive load*. This effect predicts lower attention and memory, and increased susceptibility to biases. Second, worries make scarce resource challenges top-of-mind, hence they result in mental resources being more narrowly diverted to scarce-resource tasks, i.e. they induce *tunneling* on such tasks. This theory differs from the previous ones inasmuch as it predicts not just an overall deterioration in quality of decisions and task performance – but also relative *improvement* in specific decisions and tasks -- those involving scarce resources.

While this Scarcity mechanism has been tested specifically for those facing low income *levels*, we conjecture that such a mechanism should extend to the uncertainty and risk associated with poverty. In fact, we conjecture that the effects due to uncertainty and risk may well be the main source of the psychological effects of poverty on decision-making. Given the more precise predictions of the Scarcity hypothesis -- on domains of worse as well as relatively better decisions and outcomes -- we measure the psychological effects of risk and uncertainty using their concepts of *cognitive load* and *tunnelling.*

We also distinguish between the effects of risk and uncertainty coming from greater risk *exposure* (through higher salience of its consequences) versus actual risk *materialization* (when shocks are realized) – a nuance that has not been hitherto examined empirically. We hypothesize that both risk exposure and risk materialization could have the two types of psychological effects described above.

3.2 Study Design

Our study design uses a combination of lab-in-the-field experiments, naturally occurring rainfall shocks and administrative data that creates rich, exogenous variation in the income uncertainty and income levels of our study respondents. This exogeneity in variation allows us to take a causal interpretation of the cognitive impacts we observe in our data.

3.2.1 Variation in Income Uncertainty and Risk: Survey Experiments and Rainfall Shocks

The ideal experiment to study the psychological responses to uncertainty and risk would randomize the allocation of risk. Furthermore, to nail whether the effects of interest are concentrated among the poor, it would stratify random assignment of such risk by income levels. Practically speaking though, it is not possible to randomly assign farmers different degrees of rainfall risk.

However, given the psychological channel of interest to us, it is possible to randomize *worries* about income uncertainty, 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 at the individual level, and precisely linked to the mechanism of interest, which would bolster the internal validity of our findings.

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 (treatment group) or about soap operas (control group). 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. We note that this random assignment is at the level of each survey *call*, rather than across individual respondents. We also vary the specific wording of this opening question across calls in order to sustain greater participant interest and responsiveness across surveys (Lerner et al., 2014). Apart from this opening question, the others that follow are identical for the treatment and control groups within each call.

As a second source of variation in risk and uncertainty, we also exploit rainfall shocks using daily rainfall data. These shocks occur naturally in farmers' environments, which increases the external validity of our findings about their impact. Earlier in the rainy season, they affect seeding decisions and land productivity, and hence farmers' expectations and worries about future income; later, they affect harvest realizations. Thus, over the course of the rainy season, they create variation in both risk exposure and then risk materialization. Unlike with the survey priming experiments described above, this variation is captured at the municipality level rather than at the level of individual farmers.

Given the availability of multiple rainfall shock measures to choose from, we resort to a data-driven selection procedure based on their ability to predict *drought-related worries*, as follows. We regress worries about rainfall (see Appendix A) on 51 distinct measures 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 are 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, our algorithm picks nine variables most predictive of *worries* about rainfall. We then build a post-LASSO summary measure of negative rainfall shocks (which we call the *No rainfall summary measure*), weighting each predictor by its 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 specific predictors separately: no occurrence of rainfall *3 days before* and *7 days before* the survey. This is for two reasons. First, as we show, rainfall shocks generate selective non-response in our phone surveys. However, Lee (2009)'s bounds – a correction procedure for selective attrition – can only be applied to binary variables. We use these two variables for this purpose. Second, out of the nine predictors that LASSO picks, these are the two variables with direct counterparts in the payday time windows data that we use to study the impact of income level shocks (3 and 7 day-windows around payday, respectively – see the next section). This allows us to meaningfully compare the magnitude of impact due to uncertain incomes versus low levels of income.

3.2.2 Variation in Income Levels: Before versus After Bolsa-Família Paydays

Finally, to study the impact of changes in income level, we exploit random payday variation in farmers' Bolsa-Família's payday. The schedule for these CCT payments is randomly assigned based on the last digit of the NIS, and is publicly available at the Ministry of Social Development website. Staggered paydays are in place to avoid over-crowding at banks and other cash collection points at the time of payment (Kaufmann, La Ferrara and Brollo, 2012). Importantly, since the program had been in place for over 10 years at the time of the experiment, and individual paydays are public knowledge, there is *no uncertainty* about the timing of payment, or the likelihood of receiving it.

[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 do not explore information at finer aggregation levels.

The random assignment of payday coupled with the fact that we control the time at which respondents take the phone surveys in our study, makes the *distance to payday* at the time of their response as good as random -- an advantage over the study design in Carvalho, Meier and Wang (2016).[14] It also allows us to rule out concerns such as reciprocity or experimenter demand effects that arise in other studies, for instance in Kaur et al. (2019)- We capture the effects of income level shocks by comparing the outcomes of respondents *before* versus *after* payday. We do so using three different measures of *symmetric* time windows around individual paydays: an indicator variables of payday within 3 days, within 7 days (as mentioned earlier) as well as a linear measure of distance to payday (ranging from -15 to 15; see Table C4).

In terms of the relative magnitude of rainfall versus payday-related shocks, the size of the Bolsa Familia shock is roughly equivalent to that of the prospect of losing one's harvest. Since Bolsa Família's monthly payments at the time were about a third of the typical market value of a family farmers' harvest in the region, and since average harvest losses over the previous 5 years were about a third too, it turns out that, in our study, CCT payments have the same expected value as farmer's harvest.

Finally, one additional measure of income level shocks we use is municipality-level harvest losses. Harvest losses are measured by Government as the difference between estimated harvest – based on projections for planting area and yield in January (pre-season) – and actual harvest – verified in late May (post-season) through audits in randomly selected plots in each municipality. Since the January predictions account for all information available before the rainy season (including planting area and crop choices), harvest losses can be considered randomly assigned.

We note that, unlike with the survey experiment, the variations from rainfall shocks and Bolsa Familia paydays are not stratified by income levels. Despite this, we are able to estimate heterogeneous treatment effects of these shocks too by income at the municipality level, using per

[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. These timing choices are potentially endogenous to financial pressure, which could vary by distance to/from payday for those receiving paychecks in short time intervals. If so, a study design that allows participants with high frequency paycheck to choose time of response may not be suited to accurately measure the cognitive impact of poverty. See Mani et al (2019) for a discussion on this.

capita income data from the Brazilian Institute of Geography and Statistics' 2010 Census). This allows us to gauge whether the effects of interest are concentrated among the poor.

3.3 Measuring Outcomes

Following the conceptual framework outlined in Mullainathan-Shafir(2013) described in section 3.1 above, we measure cognitive outcomes in two categories: *Cognitive Load* and *Tunneling*.[15] In this sub-section we offer a description of the questions and tasks used for measurement, as well as the underlying rationale for how we interpret each outcome measure. We refer the reader to Appendix A for further details on the specific questions used these measures.

3.3.1 Cognitive Load Measures

Cognitive load includes tasks aimed at assessing executive functions (working memory, attention and impulse control; Diamond, 2013) as well as 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 the said range of executive functions is that they constitute the foundations of decision-making; attention, memory and impulse control should have pervasive effects across the different domains of farmers' decision-making. The motivation for looking at anchoring is to check whether respondents prioritize the right information for their decisions, or whether their attention is distracted by irrelevant details. For instance, our pilot data show that farmers' production decisions show anchoring effects as they try to anticipate future prices with past prices as reference.

We measure working memory through Digit span tests, in which subjects must recall the correct sequence of digits in a number string or progressively increasing length (the longer the correct string recalled, the higher their 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 just heard multiple times, the

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[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.

correct answer is not always the digit itself. We 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).

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 the price of another item (either the future price of beans in their municipality, or the price of a subway ticket in a different location). In this context, we define anchoring as the tendency to choose higher price bands when they hear a higher initial price for an unrelated item.[16]

3.3.2 Tunneling measures

The second category of measures, for Tunneling, uses tasks involving *scarce* resources (water and money in our setting), in comparison to tasks that do not involve these resources. In principle, worse performance in psychological tests could be entirely due to factors as stress or undernutrition (in the case of negative rainfall shocks). Given this possibility, 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 rather than overall depletion alone.[17] Also, tunneling measures are a potentially cleaner way to capture attention effects under poverty, relative to Cognitive Load measures.[18]

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[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.

[18] Cognitive load predicts worse performance on a task, but incentives would predict better performance, which makes the overall performance impact on an incentivized Cognitive Load task ambiguous. This is seen in incentivized tests offered in several recent papers: 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 tunnelling.

The tasks we use to measure Tunneling include (i) relative valuation of scarce resources in simple trade-offs, (ii) performance in word search games including words related to scarce resources and (iii) sensitivity to framing in trade-offs between scarce resources and non-scarce resources (time) (based on Shah, Shafir and Mullainathan, 2015). In these tasks, higher relative valuation of scarce resources, better performance on words linked to scarce resources and lower sensitivity to framing in decisions involving the scarce resource respectively results in a higher Tunneling score. The details are as described below.

For the first task, we examine the relative valuation of the scarce resources in simple trade-offs – between money and cashews, or between water and cashews – relative to the valuation of a non-scarce resource in the same trade-off – between oranges and cashews. Tunneling in this context is defined as the tendency to report higher rates of substitution (i.e. offering less money or water than oranges in exchange for the same quantity of cashews) under greater income uncertainty. Second, in the word search games, 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 words linked to the scarce resource(s) (*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 a widely-deployed question that examines subjects' willingness to spend time to obtain the same amount of discount on the baseline purchase price (or quantity) of two items, one high and the other, low. Sensitivity to framing occurs if, despite the discount amount being identical for both items, subjects' willingness to spend time to obtain it is *not consistent* across the high-versus-low baseline purchase price/quantity scenarios. We present respondents with question pairs involving such time versus money discount tradeoffs, for both non-scarce and scarce-resource items(water). Our measure of tunneling in this context is when respondents give *more consistent* answers across the high versus low price scenarios, when the item being purchased is a scarce one(i.e. water) – i.e. they are *less* susceptible to framing when it comes to scarce resources.[19]

[19] The analysis of this variable is restricted to subjects who (i) answered both questions that offered these trade-offs, which were spread across different calls within each (monthly) wave of the survey; and (ii)

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 effects of the post-LASSO *no rainfall summary measure*.

All regressions include municipality-level fixed effects, but not wave fixed effects or individual-fixed effects. While, in principle, we could include wave fixed effects in all specifications to increase precision, it dampens the rainfall variation over the course of the rainy season considerably, in practice. So much so, that LASSO does not pick *any* predictor of worries about rainfall in the presence of municipality *and* wave fixed fixed-effects. As for individual (or even individual-survey) fixed-effects, the structure of our data would allow for including these, but our panel is very unbalanced: many subjects do not respond to calls with identical content, across different waves (see Table C2). Hence we do not use these either. In the Supplementary Appendix we show that including individual fixed-effects does not affect our point estimates, but substantially decreases the precision of estimated coefficients.

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were equally primed (or not primed) in both calls. For this reason, we have fewer observations for this variable.

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. Therefore, we build summary measures for each set of outcomes and for cognitive load, following Kling, Liebman and Katz (2007). To construct these summary measures, we first normalize all outcomes to z-scores using the control group mean and standard deviation of reach variable. 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. In other words, each component within a summary measure is weighted by the inverse of its variance.

4 Data Description and Balance Checks

In this section, we first examine the basic features of our data and discuss issues related to balance across treatment and control groups, and non-response in section 4.1. Subsection 4.2 presents the effects of each shock on an intermediate outcome underlying the mechanism of interest, worries about rainfall. This helps verify whether the three sources of variation we exploit – priming, rainfall shocks and distance to payday – work in a manner consistent with predicted cognitive effects examined in the next section.

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.[20] 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]

[20] 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).

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 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 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 the inverse of their probability of response, as predicted by this model.

4.2 Intermediate Outcome: Worries about rainfall

In examining the psychological effects of income uncertainty and low levels of income, our implicit conjecture is that it is worries from coping with these challenges that affect attention allocation and decision-making. To thread this causal chain, we begin by ascertaining how worries themselves are affected by exposure to priming and rainfall shocks. As an outcome measure for this, we use 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 variables 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 the 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]

We find that 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 (column 2). In terms of relative magnitudes, this is about 1.5 times the impact of losing access to irrigation on (rainfall) worries, and equivalent to the impact of losing 20% of one's harvest or having about 1 day less of rainfall in the previous week (magnitudes based on a cross-sectional comparison within our sample).

Columns (4) and (5) highlight not only that the experimental manipulation works, but also that its effect is sharply confined to the domain of interest: early on, priming affects worries only about future rainfall, but *not* about coping with household bills (because it is rainfall that will determine harvest outcomes later). We also note that 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 amount of rainfall has been resolved. [21]

The effect of the no rainfall summary measure is much larger in magnitude (4-fold that of priming) and very precisely estimated (at the 1% level) (column (7)). This could be due to the fact that it is a real-world shock, ad also because the LASSO was set up precisely to select the rainfall shock variables most predictive of worries about rainfall. Interestingly, the coefficient of priming changes little in the presence of rainfall shocks, confirming that both shocks are 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, our findings here are consistent with a *threshold* model of worries, in which (real-world) *stimuli above a certain cutoff* make scarce resources top-of-mind; once this happens, additional (priming) stimuli may not create additional worries, although they could still affect cognitive function directly. This could also explain what we observe for the Bolsa-Família (CCT) sample (column 8): priming and distance to payday do not have a marginal impact on worries – possibly because the rainfall shock already creates a high threshold level of worries. Having established that the experimental and naturally occurring shocks do influence worries among respondents, we now proceed to examine the effects of these factors on their cognitive function.

5 Effects of Uncertainty on Cognitive Function

We describe the effects of income uncertainty for the two sets of outcome measures described in detail in section 3, first for cognitive load (section 5.1) and then for tunneling (section 5.2). Robustness checks are summarized in subsection 5.3. Last, subsection 4.6 compares the effects of

[21] The fact that worries increase on average with every additional wave could be explained by the fact that rainfall in Ceara in 2015 was below-normal for the fifth consecutive year, with harvest losses as widely prevalent and as large as those in the previous 4 years. 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.

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.

5.1 Cognitive Load under Income Uncertainty

In Table 2, cognitive load outcomes are normalized such that negative coefficients indicate worse performance in individual tests. Column (1) presents the cognitive load 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 two intuitive rainfall measures: no rainfall 3 and 7 days prior to the survey respectively, which allows us to verify whether effect sizes decay with greater distance from the time of the survey, as one would expect. Results displayed in all the columns are based on SUR regressions, with standard errors clustered at the individual level and controls included for all baseline characteristics.

[Table 2]

As the table shows, priming generates cognitive load, decreasing performance by 0.046 standard deviation ((column 1 -- statistically significant at the 5% level). The loss in cognitive performance coming from risk exposure is sizable: it is equivalent to the gap between those with a high school education versus an elementary school education (in a cross-sectional comparison). The cognitive load of rainfall shocks is about twice as large as those 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 these two shocks individually is not magnified by their joint occurrence (column 3). This could be because the nature of the worries, and hence cognitive load, created by priming and rainfall shocks operate through different mechanisms. Finally, columns (4) and (5) document that more recent occurrences of no rainfall (3 days, rather than 7 days prior to the call) have a larger adverse cognitive impact. [22]

5.2 Tunneling under Income Uncertainty

The results for tunneling outcomes in Table 3 are normalized such that negative coefficients indicate lower relative attention to tasks involving scarce resources, lower valuation of scarce resources, and higher sensitivity to framing biases in tasks involving scarce resources. Column (1) presents the effect of priming, column (2), that of the no rainfall summary measure, and column (3), both the independent and interaction effects of these two shocks. As in Table 2, columns (4) and (5) present the effects of two intuitive rainfall measures, no rainfall 3 and 7 days prior to the survey respectively. All columns are SUR regressions, with standard errors clustered at the individual level, controlling for all baseline characteristics.

[Table 3]

We find that priming generates tunneling, improving cognitive performance in tasks involving scarce resources by 0.04 standard deviation (statistically significant at the 5% level) (column 1). 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 a considerable extent: a recent experience of a negative rainfall shock magnifies the impact of priming three-fold. Even though priming and rainfall capture different

[22] 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).

mechanisms (as we saw for cognitive load), past risk materialization can reinforce the effects of current risk exposure, consistent with the heterogeneous effects of priming on worries, as a function of the previous year harvest losses.[23] Columns (4) and (5) confirm that a more recent adverse rainfall shock results in greater reallocation of mental bandwidth to tasks involving scarce resources: only adverse shocks 3 days prior to the survey result in more tunneling, with an effect size similar to that of priming.

[Table 4]

Table 4 examines whether reaction times within individual tests used to measure cognitive load and tunneling are sensitive to risk and uncertainty, as captured by priming and rainfall shocks. Although there are no significant effects of shocks on average reaction times within cognitive load, most coefficients are positive, consistent with worse cognitive performance (column 1). The negative coefficients for tunneling outcomes in column 2 show that respondents who face priming and rainfall shocks respond *faster* in scarce-resource tasks. The effects of rainfall shocks (row 2, column 2) are statistically significant: a one standard deviation increase in the no rainfall summary measure decreases reaction 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 (rows 3 and 4, column 2: 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.

5.3 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,

[23] 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.

alternative specifications for fixed-effects, and alternative measures of sensitivity to framing (namely, present-bias, one such manifestation in the domain of time preferences).[24]

5.3.1 Addressing Selective Non-response under Negative Rainfall shocks

Since respondents are more likely to take up the phone surveys in the 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, hence we focus on the measures of no rainfall 3 days and 7 days prior to our phone surveys. 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 as in Tables 2 and 3 above, such that negative coefficients indicate worse performance in individual 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]

The bounds for the effects of priming do not change the conclusions from Tables 2 and 3, as one would expect given that there was no selective non-response in priming (Table D3). As for rainfall shocks, the effects on cognitive load are robust to the bounding procedure (bounds are very tight, and effects are statistically significant at the 10% level). For tunneling, the magnitude of the impact of no rainfall 3 days prior to the survey is very similar to that of priming, where the latter had no selective non-response. The p-value is also just short of being statistically significant

[24] 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).

at the 10% level. The lower bound for the confidence interval is only marginally below zero and similar to that for priming too(Rainfall 7 days before the survey already had no statistically significant effect on tunneling in Table 3).

5.3.2 Effects of Individual Components of Summary Cognitive Function Measures

Next, we examine the effects of priming and rainfall shocks on individual components of the cognitive load and tunneling summary measures, to see whether the overall effects are driven by specific components, rather than representing a general tendency of cognitive function in response to risk and uncertainty. We present these disaggregated results for cognitive load and tunneling in Figures 3 and 4 respectively, for priming on the left-hand panel and for rainfall shocks, on the right-hand panel. Bars reflect 95% confidence intervals, and thicker dots reflect higher levels of aggregation of summary measures (in line with pre-registration).

[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.

[Figure 4]

In this case, we do see a few systematic differences in effects among different components, and across the two sets of shocks. 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 any specific component. Primed subjects do relatively better at water-related word search as well as present higher relative valuation of scarce resources. Those who experience recent negative rainfall shocks 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.

5.4 Risk exposure vs. Risk materialization

While both priming and rainfall shocks generate cognitive load and tunneling, the evidence examined so far suggests that the two shocks capture different dimensions of risk. First, we have seen that they have independent effects on cognitive load(as documented in Table 2). Second, as noted in Figure 4, tunneling operates through different components of the summary measure across these two types of shocks. In this subsection, we document that there are also *temporal* differences in their effects over the course of the rainy season, consistent with the interpretation that they capture different aspects of the effects of risk: while priming captures the effects of risk exposure by making such water-related uncertainty *top of mind*, rainfall shocks capture the effects of realization or materialization of risk.

Table 6 estimates heterogeneous treatment effects of each shock by contrasting their effects across early versus late survey waves (March-April, and May-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]

As seen in Table 6, the effects of priming on cognitive load are concentrated in *early* waves: its interaction coefficient with late waves is *positive* indicating better cognitive function later, even if 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 is resolved, consistent with the *exposure* mechanism that makes drought concerns top of mind. In contrast, negative rainfall shocks become larger over time – as hopes that they might still be reversed vanish, consistent with the *materialization* of risk.[25] In fact, both the better performance in word-search tasks under priming and the low sensitivity to framing after the realization of the rainfall shock (see in Figure 4) make perfect sense in light of *when* these shocks have cognitive impacts.

5.5 Cognitive Effects of Risk and Uncertainty in the Bolsa-Família sub-sample

In this sub-section we examine whether the results described earlier in this section also hold for the sub-sample of respondent households for whom we were able to find a match with their Bolsa-Família payday details. The rationale for looking at this sub-sample is that sets the stage to directly compare the effects of income uncertainty and risk (as captured by priming and rainfall shocks) with those from income level shocks (as captured by distance to Bolsa-Família paydays). The latter analysis is presented in section 6 below. As we did for the full sample, we first verify whether the payday shock affects worries among our matched Bolsa-Família respondents and then present results on the cognitive load and tunneling outcomes (subsection 6.1).

5.5.1 Worries due to Risk and Uncertainty in the Bolsa-Família sample

[Table 7]

In Panel A of Table 7, columns 1 and 2 describe the effects of priming and rainfall shocks respectively on intermediate outcomes of interest within the Bolsa-Família sample: worries about rainfall and worries about household bills (rows (1) and (2) respectively). Panels B and C within these first two columns then present the effects of these two shocks on cognitive load and

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[25] An alternative explanation could be that subjects get inured to priming experiments – such that their effect ceases to exist over time. However, Panel B shows that the tunneling effects of priming only emerge in late waves. This could be due to the fact that making droughts more salient *after* uncertainty has been resolved could be highlighting the materialization of rainfall risk, thus approximating the tunneling effects of rainfall shocks.

tunneling outcomes for this group. As before, 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. Each cell in table 7—(rows 1 and 2) is the coefficient from a different SUR regression, with standard errors clustered at the individual level.

A comparison of the four coefficients in Panel A -- columns 1 and 2, shows that the effects of uncertainty on worries about rainfall and bills within the Bolsa-Família sample are qualitatively similar to those for the full sample (Table 1, columns 1, 6 and 7): The main worry-inducing effect – both about rainfall and managing household bills -- comes from the rainfall shock. At the same time, we remind the reader that there could be compounding effects between the different types of shocks if there is a *threshold* level of worries such that further inducements to worry (i.e. as through priming) or distance to payday may have no marginal impact on worries, but could still affect cognitive function.

Coming to cognitive load and tunneling (Panels B and C respectively), the direction of impact is the same as that for the full sample. However, the magnitude of the cognitive load impact of the priming and rainfall shocks is roughly 50% larger in this sub-sample (respectively 0.064 versus 0.0458 – column (1), Table 2 and 0.15 versus 0.108 – column(2), Table 2); the tunneling effects are of similar magnitudes as in the full sample, but measured more noisily.

These results for the Bolsa-Família sample set the stage for a comparison of the psychological impact of income risk and uncertainty with those due to low levels of income. We examine the latter issue in the next section.

6 Effects of Income Level Shocks on Cognitive Function

We address this issue by using fluctuations in income levels of our respondents around their Bolsa-Família payday as the exogenous source of variation. As in section 5.5 above, our analyses are based on the subset of respondents whom we were able to match to their Bolsa-Família registry details (as described in section 3.2.2). We estimate the psychological impact of income level shocks by comparing those (randomly) surveyed *before* their payday relative to those surveyed *after*, where the latter group would higher incomes.

6.1 Worries, cognitive load and tunneling

Table 7 (columns (3) to (7)) presents the effects of different payday shock variables on the outcomes of interest: worries, cognitive load and tunneling (panels A, B and C respectively). The three income-level shock (payday) variables compare before versus after payday differences in cognitive function using: (i) linear distance to Bolsa-Família payday, and (ii & iii) indicator variables for being within 3 and 7 days of this payday[26]. As before, 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. Each cell in table 7 is a different SUR regression, with standard errors clustered at the individual level.

In panel A (columns (5)-(7)) we see that none of the three payday shock variables have a significant effect on worries about household bills (coefficients for payday within 3 and 7 days, column 2). In Panel B, we see that, on average, a lower income level (before payday) does not systematically generate cognitive load either (columns (5)-(7)). This is not merely an artifice of smaller sample sizes: in contrast to the payday shock, the effects of priming and rainfall shocks on cognitive load within this Bolsa-Família sub-sample are actually *more* adverse than in the full sample (statistically significant at the 10% and 1% levels, respectively). In fact, the coefficients of the indicator variables of distance to payday on cognitive load are not just insignificant, but actually *positive*.

At the same time, as we see in panel C, these same indicator variables do have very large and statistically significant effects on tunneling (column 4). Indeed, the tunneling effects of being 3 and 7 days away from payday are larger -- both in size and statistical significance -- than the effects of priming and rainfall shocks in the Bolsa-Família sample (the latter similar to those

[26] 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.

estimated for the full sample). As with the rainfall shocks, these 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 compared to 0.34 standard deviations within 3 days of the payment (both coefficients significant at the 5% level).

In the Supplementary appendix, figure 6 reports both individual components and summary measures of the cognitive load and tunneling outcomes for the Bolsa-Família sample in a 7-day-window before relative to after payday. Figure 7 in the same appendix reports non-parametric estimates for just the summary measures of cognitive load and tunneling before relative to after payday, but in symmetric time windows ranging from 1 to 15 days around payday.[27] Both figures 6 and confirm that the pattern of payday shock effects described in Table 7 above is not an artifact of the influence of particular components of the summary measure or specific distances from a person's Bolsa-Família payday respectively. The bottom line is that, *on average*, having a lower level of income before payday (relative to after) has no adverse impact on cognitive load, and induces a large and statistically significant increase in tunneling, more so closer to payday (as shown in Figure 7). This leads us to our next question below.

7 Do Income level shocks have no adverse psychological effects on the poor?

One possible reason why income level shocks from Bolsa-Família payouts have no cognitive load effects is that they do not constitute a significant fraction of individual income for most respondents. If so, this is likely to be truer for relatively richer respondents within our sample, with access to other sources of income and/or loans. Indeed, the effects of income uncertainty and risk could also plausibly vary by income levels. In section 7.1 below, we therefore examine heterogeneous treatment effects by municipality-level per capita income for all three shocks,

[27] For each 'dot' coefficient in Figure 7, we hold distance to payday fixed, restricting attention to participants *at most D days* before versus after payday, with D ranging from 1 to 15. Each dot represents the value of the coefficient, while bars reflect the 95% confidence intervals.

taking advantage of variation across the 47 locations of our study. Next, subsection 7.2 analyzes money earned in the experiments -– taking advantage of the non-linear incentive structure in place for the phone surveys.

7.1 Heterogeneous effects by municipalities' per capita income

Table 8 reports heterogeneous cognitive effects of each shock ('treatment') examined in Table 7 by municipality-level per capita income, by introducing an additional interaction term between the two (in natural logarithms). Panel A presents the results for cognitive load, and Panel B, for tunneling. As before, 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 income level shocks from Bolsa-Família payouts vary considerably with income, causing high cognitive load among the poorest municipalities within a 7-day window around payday (column 7, significance at the 5% level). In fact, among the poorest municipalities, the magnitude of impact from this income level (payday) shock is more than three times as large as those from income uncertainty induced by priming or adverse rainfall shocks (row 1, columns (1) and (4)). At the same time, the cognitive load due to a tight budget right before payday shock decreases sharply with income (row 2, column (7) -- significant at the 5% level), eventually becoming *positive* at income levels where most of our sample lies. Combining these negative effects on the poorest with the positive effects among the richer segments of our sample is what generates the overall average null effect on cognitive load (Table 7, panel B – column(7)). In contrast to cognitive load, panel B shows that the tunneling effects of payday shocks do *not* vary with income.

[Figure 8]

Figure 8 plots the predicted effects of the three shocks – priming, no rainfall and Bolsa-Família paydays – using coefficient estimates in Table 8, across the full income range in our sample (Panels A, B and C respectively). In each panel, black lines depict cognitive load effects and grey lines depict tunneling effects, with negative values signifying worse outcomes. A quick visual comparison across these panels shows several noteworthy points.

First, income uncertainty and risk from priming and rainfall shocks (panels A and B respectively) create cognitive load *and* tunneling effects across most of the per-capita income range (identified by the vertical dotted line at zero on the Y-axis). This holds true for the tunneling effects of a payday shock as well (panel C, grey line); however, the cognitive load from this income level shock is concentrated only among the poorest municipalities (to the left of the dotted vertical line). The coexistence of cognitive load *and* tunneling in most of the income range is uniquely consistent with the *Scarcity* mechanism. Also, the absence of cognitive load at moderate income levels soon before payday (in panel C) is consistent with hopefulness and anticipation it creates (as in Caplin and Leahy, 2001).

Second, we draw attention to the effects on either end of the income range. It is fair to say that the poorest municipalities suffer the most adverse psychological effects of low and uncertain incomes. Cognitive load is highest in this income range across all three panels. It is the poorest alone who endure a cognitive load from a low level of income; in fact, the size of this load 7 days before payday is *much greater* than what they endure from priming or no rainfall. The ability to tunnel on tasks involving scarce resources is also weakest in the lowest income range. So much so, that the poorest perform *even worse* in tasks involving scarce resources when faced with a negative rainfall shock (panel B, to the left of the vertical dotted line).[28] In contrast, the richest segment of our sample face no cognitive burden from priming (panel A, to the right of the dotted vertical line).[29]

[28] This is a phenomenon referred to as *choking* (mistakes driven by high stakes, as in Ariely et al., 2009) -- the *opposite* of tunneling.

[29] This pattern consistent with *rational inattention*– since it enhances subjects' performance in tasks involving the object of the priming without deteriorating cognitive performance in other tasks.

To summarize, the pattern that emerges from our findings indicates that it is income uncertainty and risk, more so than low income levels, that drive poverty's psychological tax. The exception here is among the poorest, where *both* aspects of poverty have a significant cognitive impact, with a larger adverse impact coming from low levels of income itself.

Relating these two sets of findings to previous studies – specifically Mani et al. (2013) and Carvalho, Meier and Wang (2016) – shows that their findings are not inconsistent after all. The first study surveyed Indian sugarcane farmers who faced low income levels and uncertainty in the timing and/or amount of harvest payments. The second study focused on US respondents who faced low income levels before payday(s), where the date(s) and amount(s) of payment were known at the time of the survey. In other words, Mani et al(2013) documents the cognitive load effects of low *and* uncertain incomes while Carvalho et al(2016) documents these same effects due to low income levels alone, on a sample of US respondents who are not as poor as the poorest municipalities in our sample or the farmers in India.[30]

7.2 Do Tunneling effects overcome Cognitive Load? The role of monetary incentives

So far, we have separately reported the cognitive load and tunneling effects of income uncertainty, both of which are found to be present in most of the sample. However, given that these effects go in opposite directions, a question that arises is about their *overall* impact on attention allocation. To put it differently, is tunneling towards scarce-resource tasks efficient or is this reallocation of attention sub-optimal? We are able to address because the top 25% of performers in our phone surveys were incentivized with a monetary reward paid in the form of additional air-time credit. We use money earned as our metric of *overall* impact of the three exogenous shocks used.

In section 7.1 above, we showed that payday variation generates tunneling. Here we examine whether such effect is *magnified* in the face of incentives. We provided a non-linear structure of incentives, granting the top-25% scores in our phone surveys additional airtime credit. Table 9

[30] Neither paper studies tunnelling effects, which we find significant evidence of, both due to income uncertainty and low levels of income.

estimates whether each shock affects money earned in tests for cognitive load (Columns 1 and 3), and those for 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]

We find that priming and rainfall shocks increase money earned significantly across all tests (the former, only significantly for tunneling in the full sample). The effect sizes under income uncertainty are large: Priming increases money earned in tunneling tasks by over 5%, and rainfall shocks, by 13-16%, depending on the sample. It is remarkable that, in the range of scores for which performance incentives matter, the effects of income uncertainty and risk on cognitive load are *reversed*. In other words, among the top performers, income uncertainty does not result in efficient tunneling, when we consider overall earnings from experimental tasks. This finding lines up with those of a number of studies that find scarcity actually *improves* performance under incentives: for instance, the 'Wheel of Fortune' self-replication in Shah, Shafir and Mullainathan (2019), where primed subjects made more money in the incentivized experiments, Lichand et al. (2019), where primed subjects made more money in short-term attention and memory tasks if these were incentivized, and Kaur et al. (2019), where workers primed about financial strain increase productivity in piece-meal payment tasks.

With regard to income level shocks, proximity to payday creates large and significant differences in money earned in the experiments, despite no cognitive load effects, on average (Table 7, panel B). Before payday, while both average subjects *and* top performers do *better* on tunneling tasks (earning significantly *more* money on these) -- top performers actually do *worse* on the other tasks (earning significantly *less* money). This pattern is consistent with *inefficient* reallocation of mental bandwidth in the range of performance for which incentives matter.[31]

[31] This effect is weaker farther away from 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%

These findings are important for two reasons. First, they illustrate that shocks that generate cognitive load and tunneling can have *real* consequences. 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 income uncertainty and low levels of income induce inefficient tunneling, making farmers 'penny wise and pound foolish'.

8. Discussion and Conclusions

In this paper, we study the impact of income risk and uncertainty under poverty on cognitive function, distinct from such effects due to low levels of income, use a combination of survey experiments and naturally occurring shocks. The mechanism we study here is distinct from conventional rational responses to uncertainty, working through risk aversion: while risk aversion predicts effects on current decisions pertaining to the domain of risk, our mechanism predicts effects across *all* decisions, through adverse effects on attention allocation. Thus, it could result in lower payoffs in *every* future state, regardless of the materialization of the risk itself.

We find significant adverse effects both from exposure to risk (through survey experiments) and the materialization of such risk (negative rainfall shocks). These are distinct from the adverse effects of low income levels, that are concentrated among the poorest segments of our study sample. The simultaneous increase in farmers' cognitive load together with better performance in tasks involving scarce resources (tunneling) supports the interpretation that these effects are driven by the Scarcity mechanism (Mullainathan and Shafir, 2013). This paper is the first to provide evidence that the predictions from this theory carry over actually having too little to the *risk* of having too little, as well. In the range of performance for which incentives are relevant, we

and 5% levels, respectively); 3 days before payment, those effects increase to 26% and 32% (significant at the 10% and 5% levels, respectively). These outcomes in the presence of monetary incentives also help explain why cognitive load and tunneling effects in Table 7 (panel C, columns (6) and (7)) are significant at 7 days from payday, but not at 3 days away .

show that low income levels (before payday) can result in *inefficient* attention allocation to scarce resource tasks.

Our findings (on cognitive load) 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). At the same time, 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 – for instance, 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, which is uniquely consistent with a specific mechanism, of limited mental bandwidth under poverty.

Our results also help reconcile contradictory evidence across previous studies – notably Mani et al(2013) and Carvalho, Meier and Wang (2016) – explained by differences in exposure to income uncertainty as well as in income levels, across the study samples.

Are these psychological effects of poverty first-order? There are two reasons why that may 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*. Given that cognitive function lies at the foundation of every decision, these large effect sizes could imply significant efficiency losses across several domains.

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 interventions that could help mitigate those psychological effects by adapting the environment in which the poor make those decisions, from providing insurance (Lichand and Mani, 2019) to making the relevant decision features top-of-mind (Lichand et al., 2019).

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Appendix A – Definition of dependent variables

WORRIES

<u>Worries about rainfall</u>:

"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"

<u>Worries about household bills</u>:

"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

<u>Digit span</u>:

"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"

<u>Stroop</u>:

"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:

<u>Price of beans</u>:

"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"

<u>Price of subway ticket</u>:

"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[32]:

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):

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

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

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

Patience (water, week):

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

Patience (water, month):

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

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

- 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

<u>Children can succeed</u>:

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

<u>Educational investment</u>:

"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

<u>Credit related to production (waves 1 and 3)</u>:

"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."

<u>Credit unrelated to consumption (waves 1 and 3)</u>:

"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

Notes on Table C2:

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 effect

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

	Frequency (%)				
Days until payday	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]

Notes on Table D1:

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	

Notes on Table D2:

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)

<u>Notes on Table D3:</u>

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

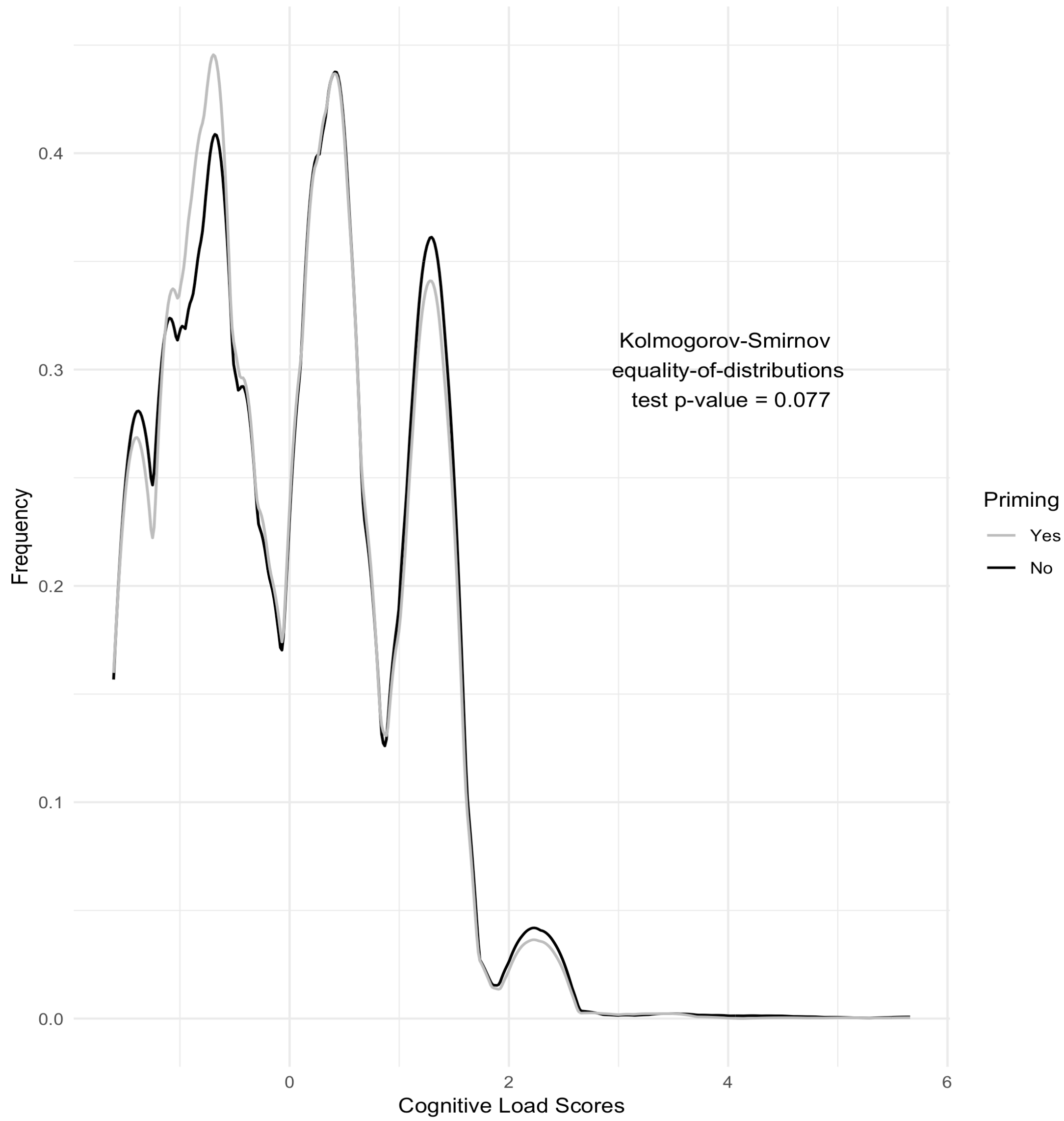
Variable	Marginal effect on probability of completing a call
Respondent lives in municipality's most drought-prone region	0.02**
Respondent is male	-0.01
Respondent's age	-0.00**
Respondent believes that rainy season will be good if it rains on March 19th	0.02
Respondent's plot is at least partly irrigated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

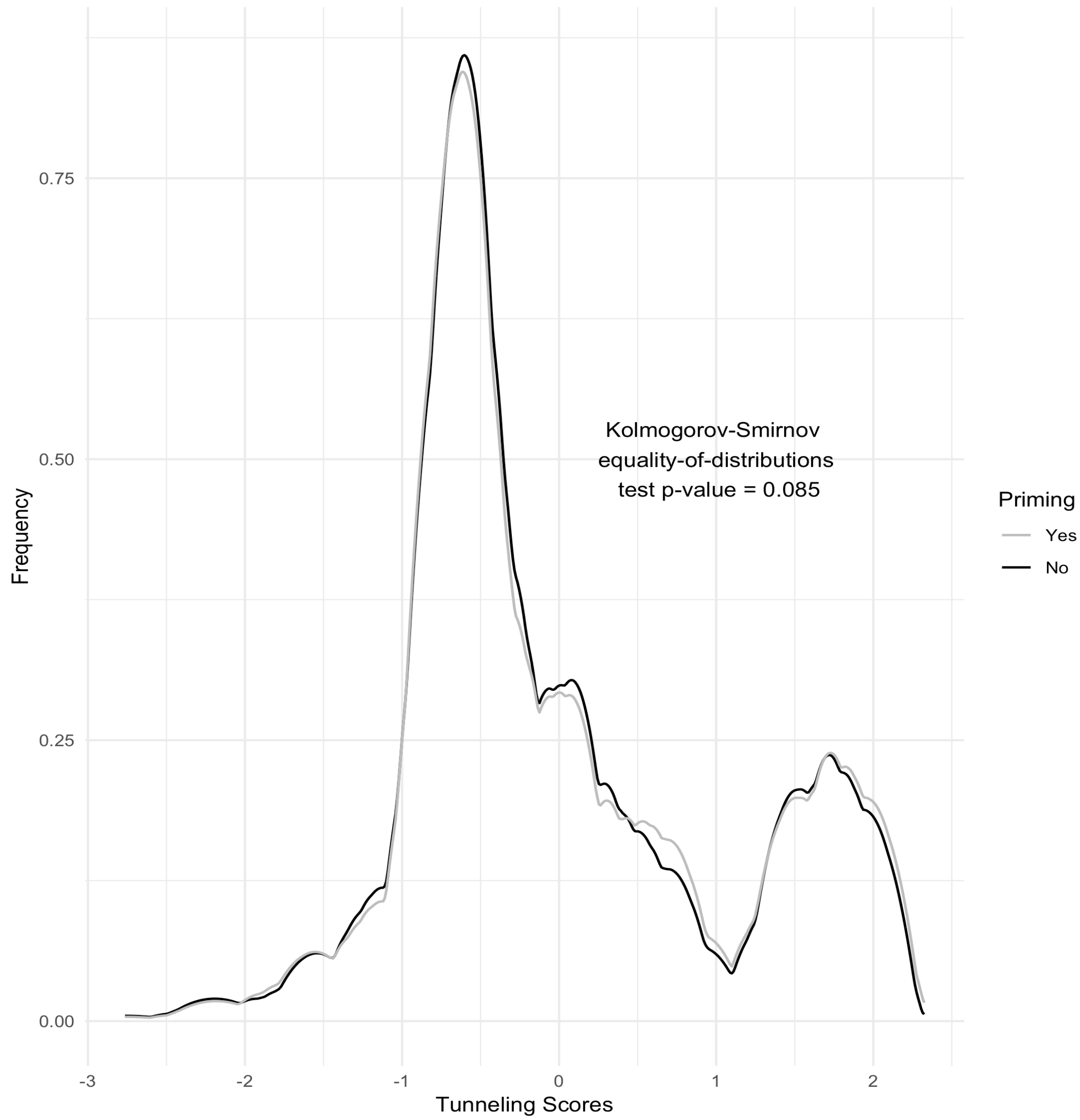
Figure 1 – Effect of priming on the distribution of cognitive load



<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.

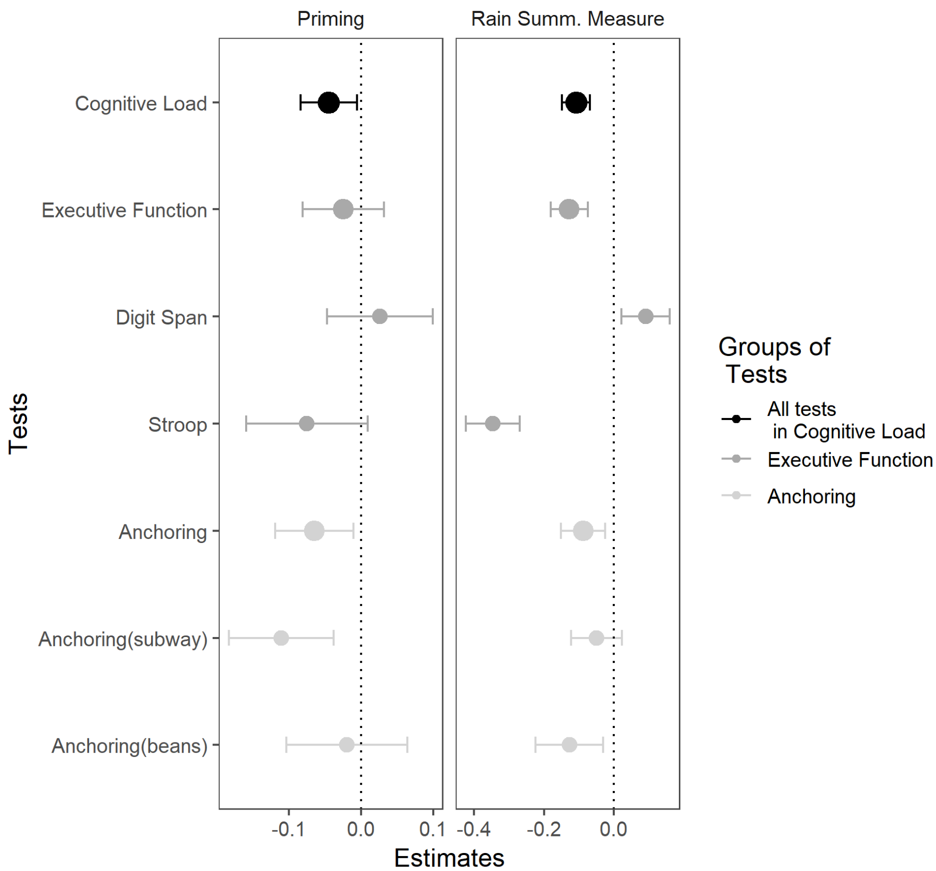
Figure 2 – Effect of priming on the distribution of tunneling



Notes on Figure 2:

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.

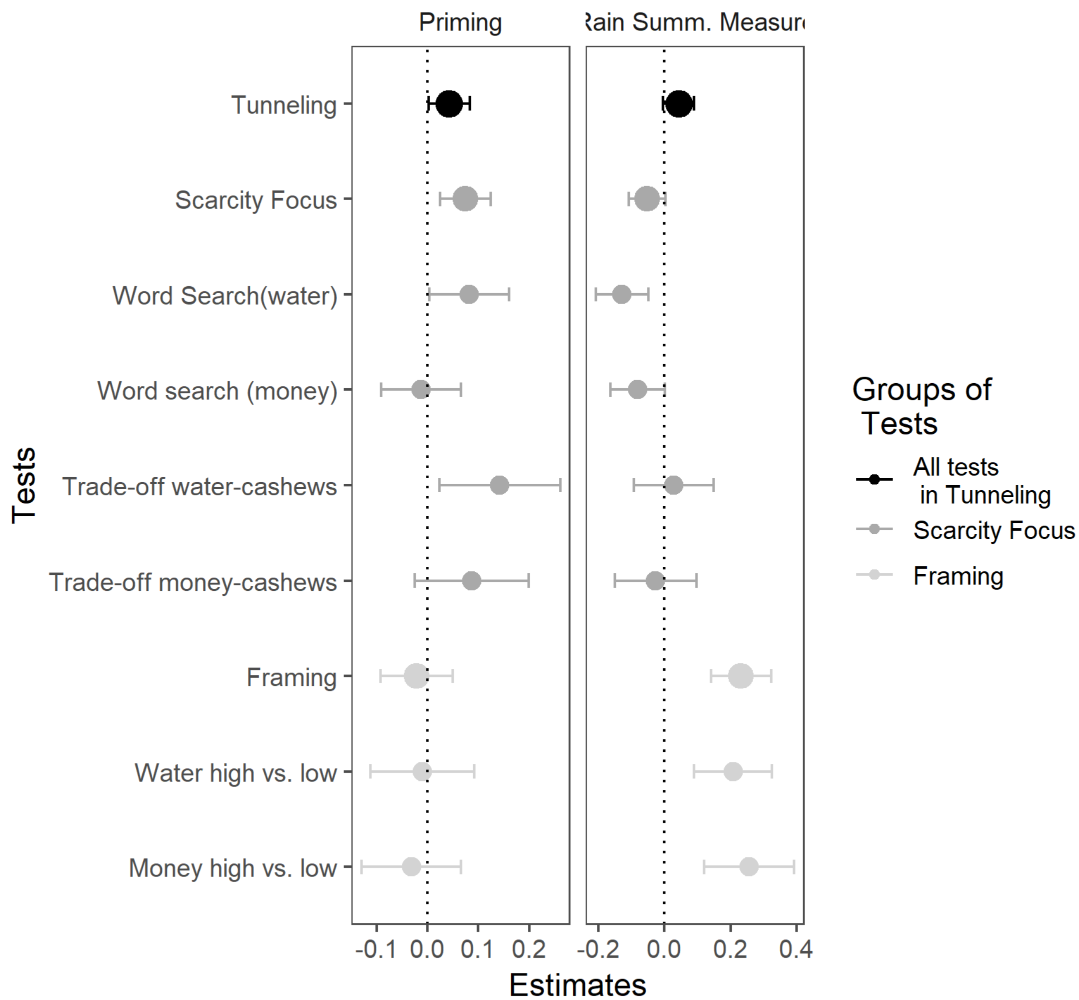
Figure 3 – Effects of priming and of the rainfall summary measure on the components of cognitive load



<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 $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\widehat{\beta}_j}{\widehat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring, 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).

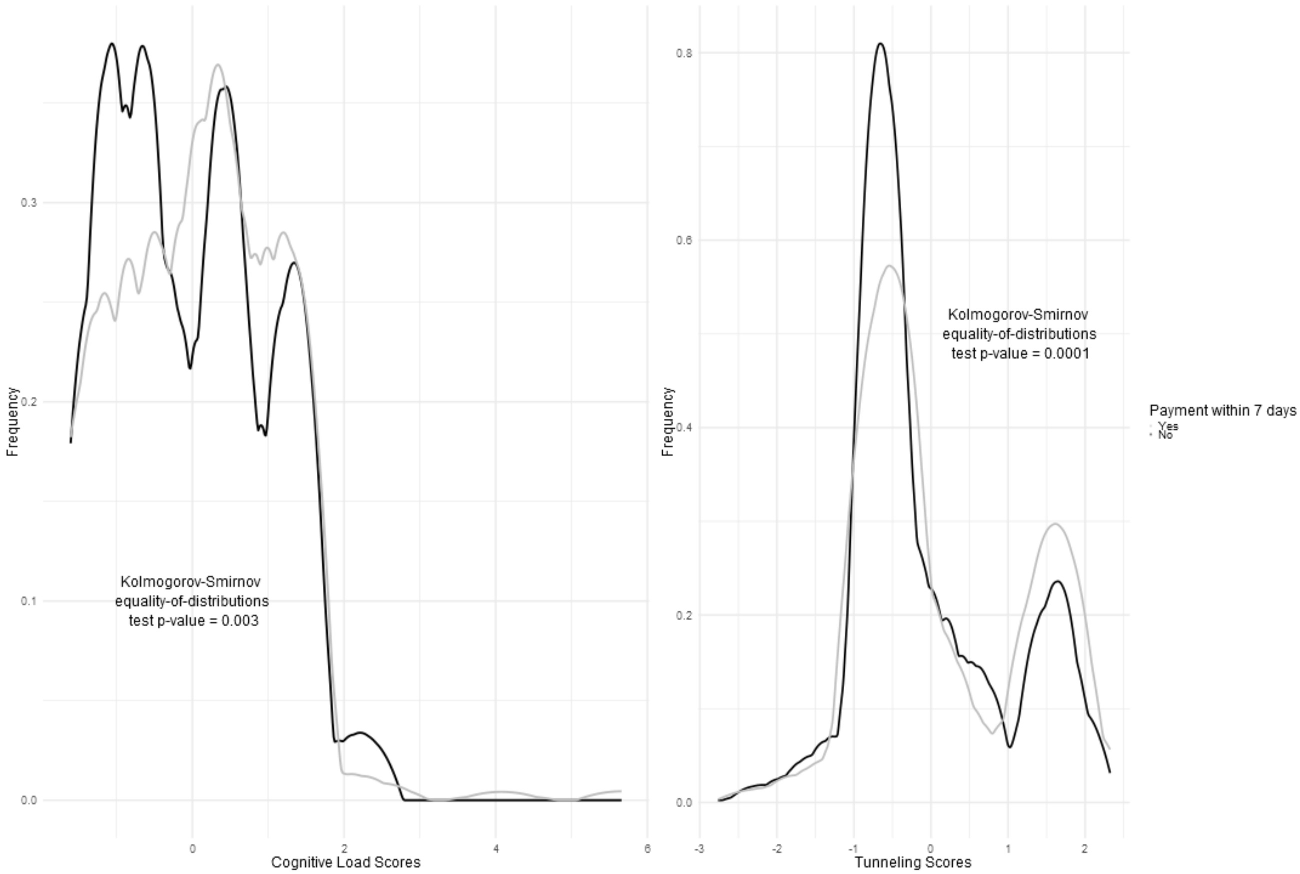
Figure 4 – Effects of priming and of the rainfall summary measure on the components of tunneling



<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);

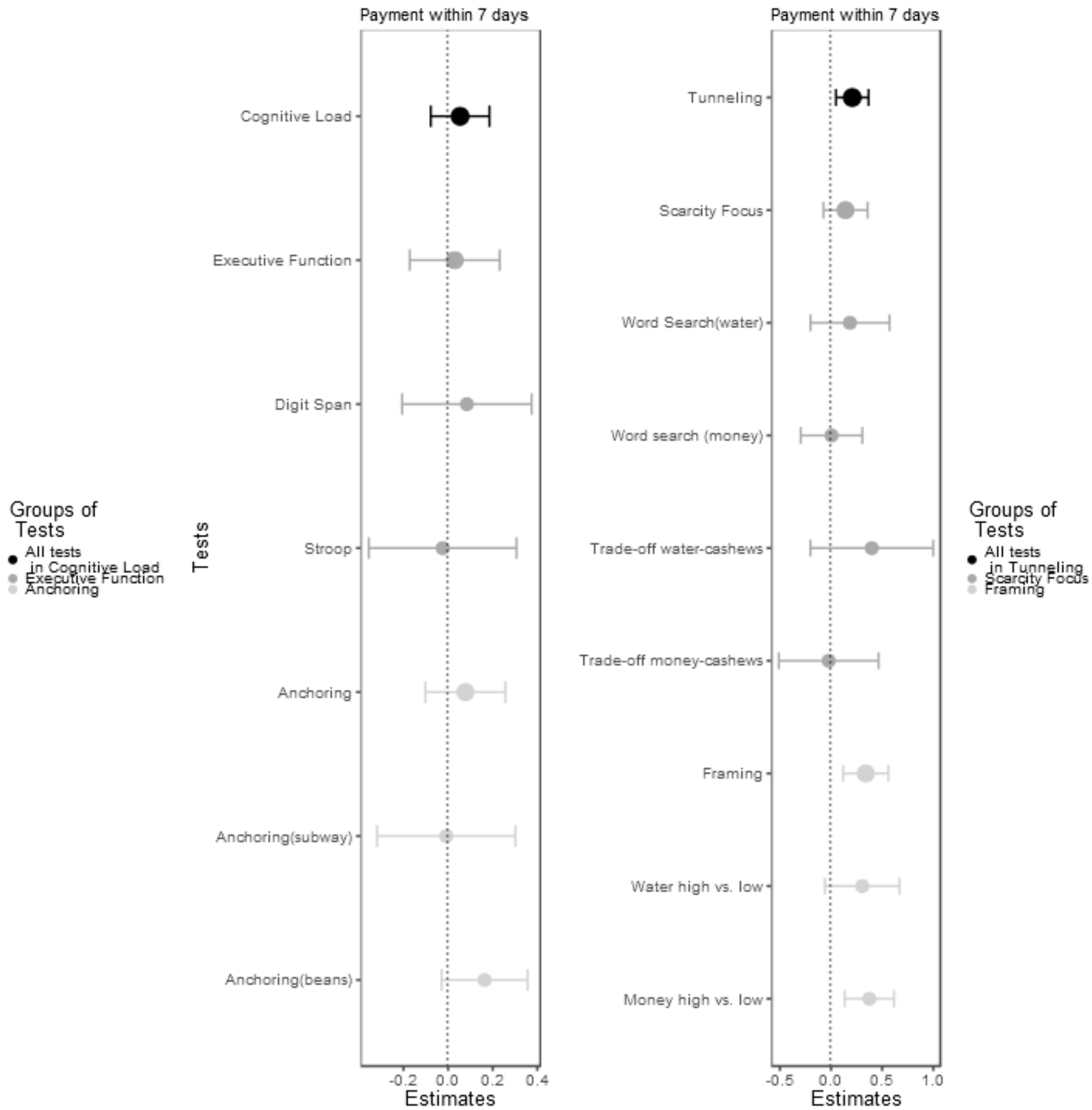
Figure 5 – Effects of distance to payday on the distribution of cognitive load and tunneling



<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 components of cognitive load and Tunneling within Bolsa-Família sample

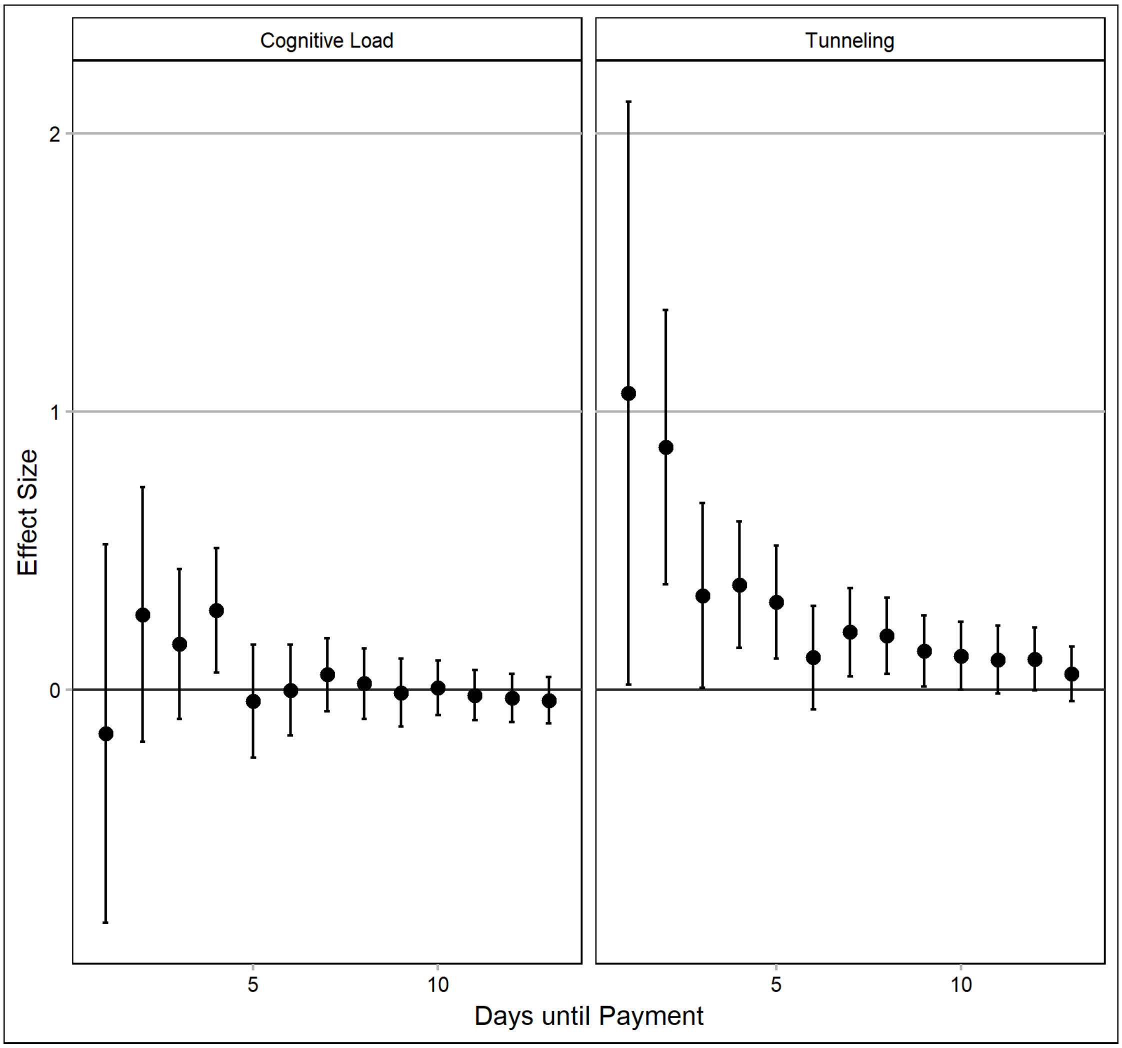


Notes on Figure 6:

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 $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\widehat{\beta}_j}{\hat{\sigma}_{j_c}}\right)$, 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.

3. The figure displays the effects of the indicator variable of the survey being within 7 days *before* payday (relative to after) on cognitive load and tunneling, restricting attention to observations in a 7-day window around payday. 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).
4.

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



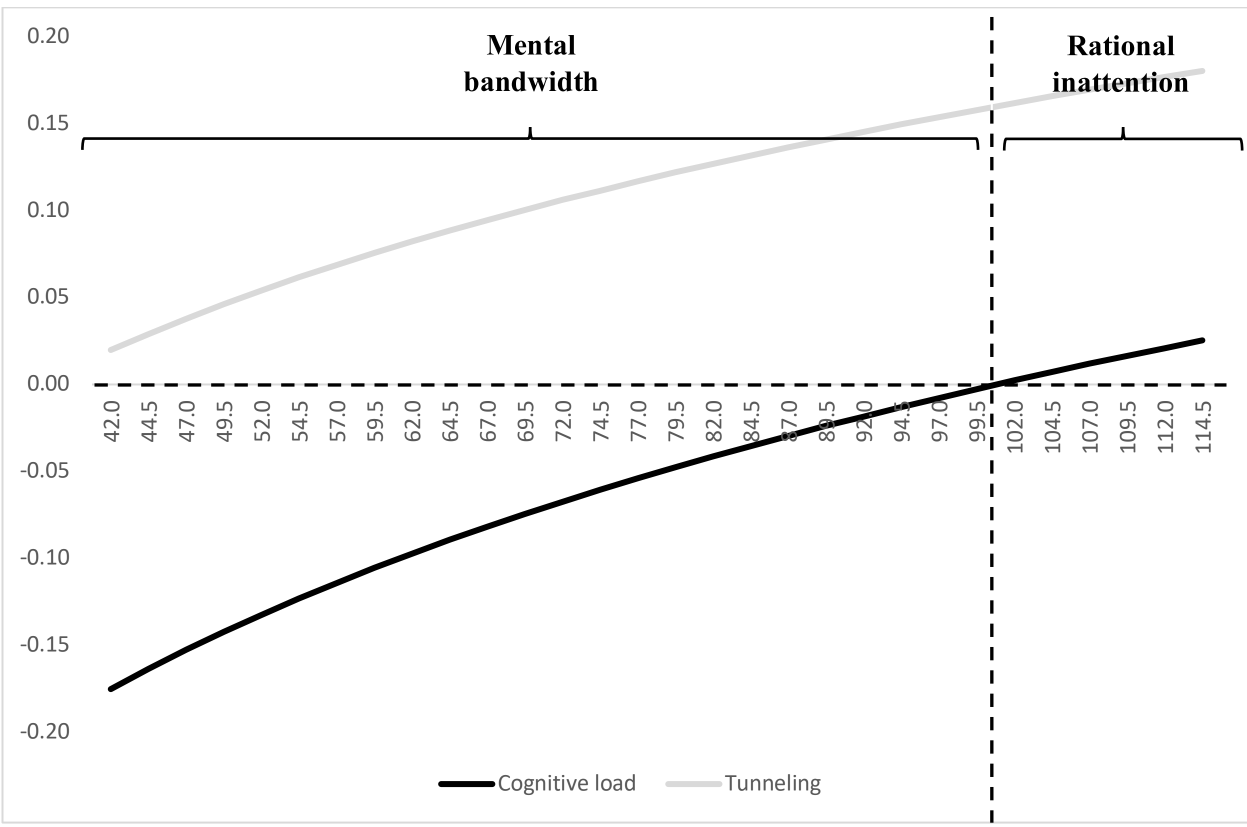
<u>Notes on Figure 7:</u>

1. The figure displays coefficients (dots) and 95% confidence intervals (bars) for the summary cognitive effects of being before (versus after) payday, in symmetric time windows around payday. Each (dot) estimate holds the window size fixed, only comparing subjects surveyed before vs. after payday within the time window specified on the X-axis. Regressions include 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

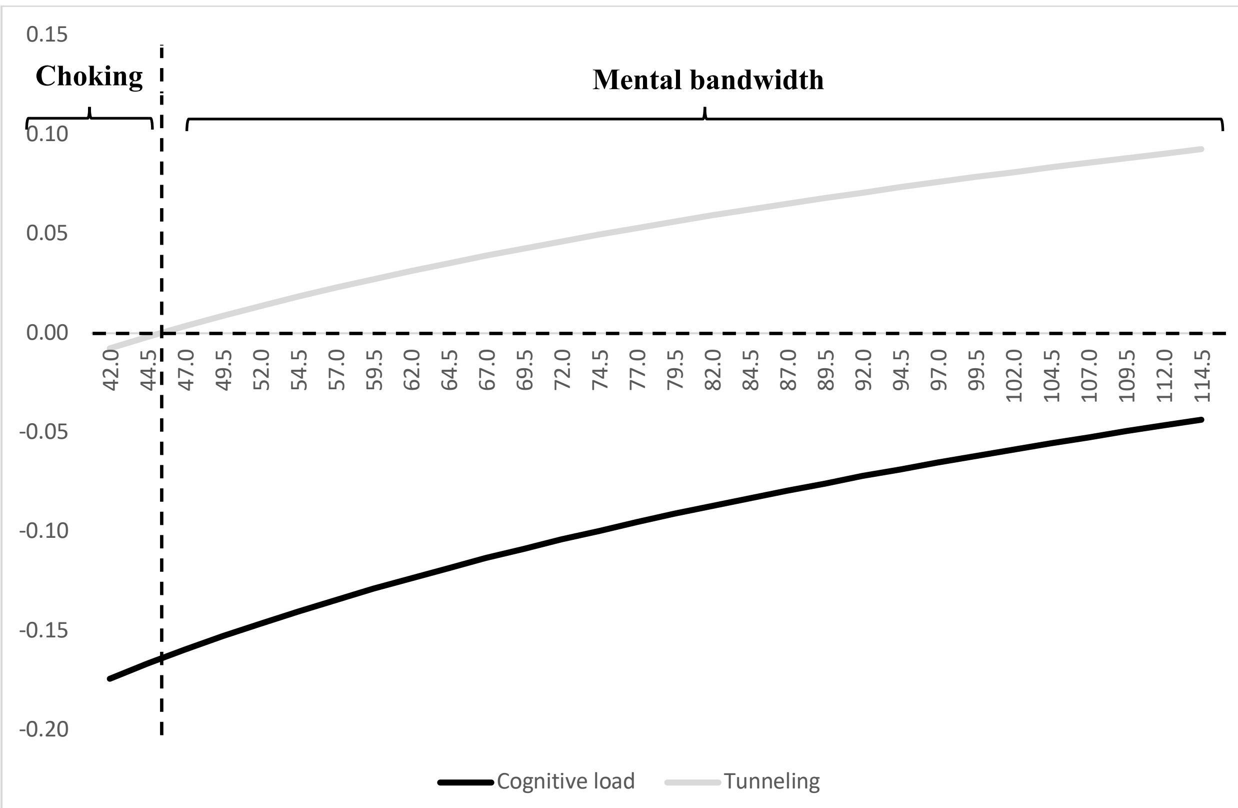


Notes on Figure 8 – Panel A:

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



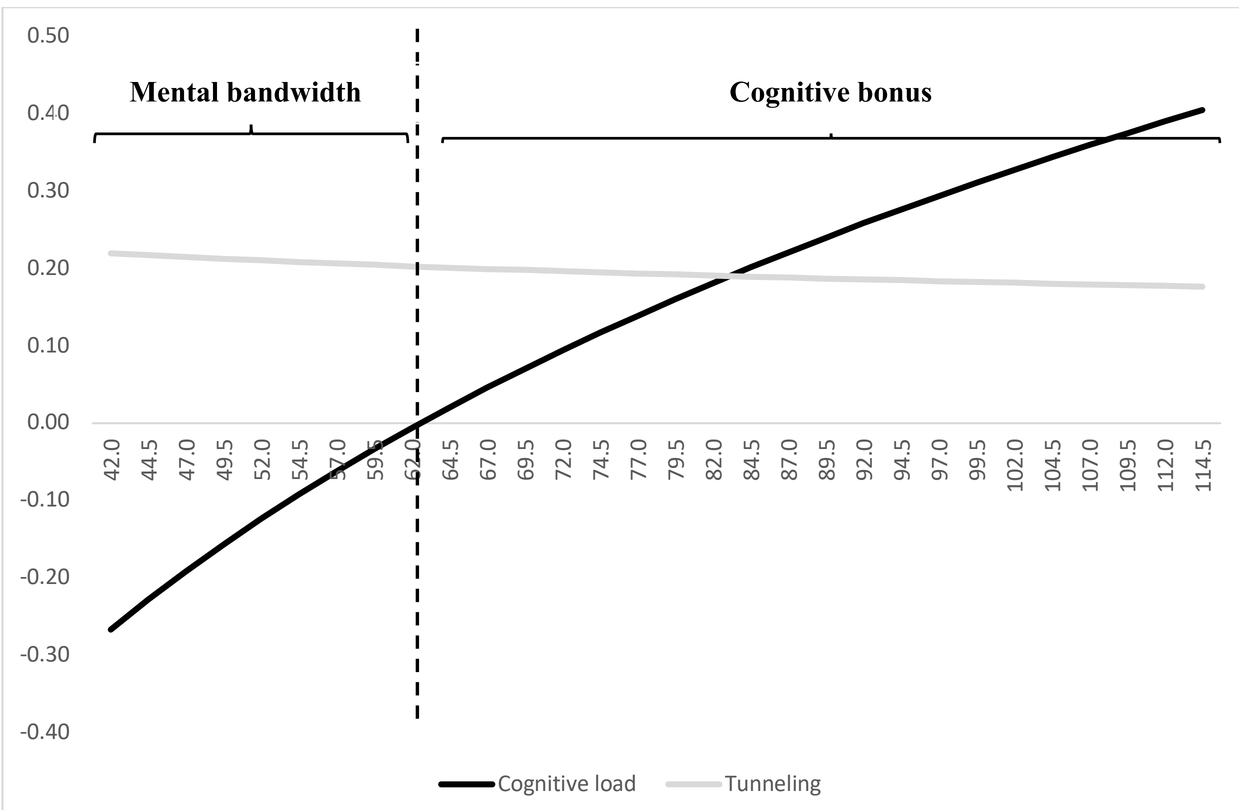
<u>Notes on Figure 8 – Panel B</u>:

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



Notes on Figure 8 – Panel C:

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
	Worries: Rainfall	Worries: Rainfall	Worries: Rainfall	Worries: Rainfall	Worries: Rainfall	Worries: Bills	Worries: Rainfall	Worries: 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 Previous year 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

Notes on Table 4:

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 $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\widehat{\beta}_j}{\widehat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for executive functions and anchoring, in 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

<u>Notes on Table 8:</u>

1. All columns are effect sizes $\left(\frac{1}{K}\sum_{j=1}^{K}\frac{\widehat{\beta}_j}{\widehat{\sigma}_{j_c}}\right)$, with $\hat{\beta}_j$'s computed from Seemingly Unrelated Regressions (SUR) with standardized dependent variables (z-scores) for 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.

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