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# Persistent miscoordination: an economics of Amazon fires

## Vedvarende feilkoordinering: en økonomisk analyse av branner i Amazonas

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# Summary

This thesis analyzes micro-level causes of forest fires in the Brazilian Amazon, and the impact of policies and droughts on smallholders' decisions to use and control fires.

Fires in the Brazilian Amazon are an important driver of forest degradation, doubling biodiversity losses from deforestation. Fires have burned an area equivalent to half of the whole forest over the past 15 years[1], and produce yearly carbon emissions equivalent to 5% of the global amount. Fires affect local people, who suffer direct damages to their crops, houses and infrastructures, and are exposed to health hazards from air pollution. But fire is also "cheap labor" and an important livelihood tool for Amazonian smallholders. They rely on fire to clear land, fertilize soil and control pests. The same people suffering the most direct damages from forest and accidental fires are also the main fire users.

The first article elaborates on this apparent paradox, and seek to explain the persistence of low yield and land degrading practices through fire use. It is shown that fire risk externalities, arising outside the farmer's property, reduce investments in fire control and the uptake of fire-free techniques. Preventing own fire to escape to the rest of the property is costly, and a wasted effort if fires originating from outside are likely to destroy the farm anyway. If fire risk is too high, there is no incentive to invest in alternative fire free techniques, because more assets would be at stake in case of a fire. Fire risk causes more fire use and less fire control. The coordination nature of fire risk, fire use and related income makes these and inputs choices simultaneous to revenue. Generated instrumental variables are used to overcome this identification challenge, and to estimate the revenue elasticity to fire risk externalities for fire and non-fire users. The negative impact of externalities is large, and higher for the non-fire users, supporting the use of a coordination model to analyze fire use decisions.

The other three articles of the thesis report on a framed field experiment based on a coordination game. They assess the joint impact of droughts and policies on coordination for fire risk mitigation. Droughts cause fires to spread beyond the boundaries of the neighborhood or the community, affecting other groups. I test the impact of stable vs. increasing drought risk, miming a climate change scenario. In a within design, I also test three fire mitigation policies: command and control against uncontrolled fire, payments for environmental services conditional on uptake of fire-free techniques, and community-based fire management in the form of face-to-face communication.

The first article contrasts the impact of command and control vs. payments for environmental services, finding that both policies equally increase the adoption of fire-free techniques, but that the latter fails to mitigate fire risk because it crowds out fire control investments among the fire users. Farmers respond to drought risk with more uncontrolled fires, suggesting that the impact of droughts might partially be human mediated. All policies perform better in the increasing drought

[1]https://prodwww-queimadas.dgi.inpe.br/aq1km/

risk treatment, suggesting that participants are more reactive to policies in a climate change scenario.

The third article analyzes the impact of community based fire management through communication on coordination. A level-k model shows that communication fosters coordination. A social norm produces the same effect, by converging expectations, and by breeding a *taboo* on the proscribed choice. If communication and the norm operate together and the norm fails to provide sufficient assurance for coordination, the outcome is worse than for communication alone because the taboo hampers the self-signaling property of communication. Finally, communication of requests can improve coordination under a weak social norm if players are believed to be credulous enough. I find evidence of a weak fire control norm into the experiment, and support for the level-k model predictions. I conclude that community based fire management is more likely to mitigate fire risk when drought risk is high enough to weaken the norm, and when requests from community leaders are more influential.

The last article analyzes the impact of social and risk preferences and perceptions on coordination in stated and experimental data, and provides an external validity test of the experiment. Social and risk preferences and perception are theoretically relevant for coordination, affecting whether coordination tipping points are passed and even the number of equilibria in the game. Standard external validity tests based on correlation between experimental behavior and a counterfactual measure are prone to spurious correlation and may be theoretically inconsistent. It is described and implemented a novel test based on commonality of behavior predictors in and out of the experiment: predictor validity. The out-of-the-experiment counterfactual is built using stated behavior. Both stated and incentive compatible measures of preferences are also collected. It is found that risk perception but not risk aversion causes miscoordination. Pro-social preferences improve coordination outside but not within the experiment. Other predictors are also analyzed to exemplify the functioning and limits of predictor (external) validity tests. We conclude that the experiment is likely externally valid concerning fire use decisions. The tests seem to dismiss external validity of fire control choices, but not all assumptions of the test are met, leading to ambiguous interpretations. Policies to mitigate fire risk should target a reduction in perceived fire risk and consider pro-social preferences.

Fire risk traps farmers in low yield and environmentally degrading land use practices, namely uncontrolled fire use. Mitigating fire risk likely achieve three often competing goals: to reduce carbon emissions, to preserve biodiversity and to pave the way for economic development.

# Sammendrag

Denne avhandlingen analyserer årsakene til skogbrann i den brasilianske delen Amazonas, og virkningen av tørke og politiske virkemidler på småbønders beslutninger om å bruke og kontrollere skogbranner.

Branner i den brasilianske delen av Amazonas er en viktig drivkraft bak skogsforringelse, og dobler tapet av biologisk mangfold knyttet til avskoging. Et areal tilsvarende halvparten av hele regnskogen har brent ned de siste 15 årene[2], og karbonutslippene fra disse skogbrannene tilsvarer 5% av de globale utslippene. Branner påvirker lokalbefolkningen gjennom direkte skader på avlinger, hus og infrastrukturer og samt gjennom helseeffekter ved luftforurensning. Bruk av ild er imidlertid "billig arbeidskraft" ved oppdyrking, og utgjør dermed et viktig del av livsgrunnlaget for småbrukere i Amazonas. De bruker ild for å rydde land, gjødsle jorda og kontrollere ugras og skadedyr. Den samme befolkningsgruppen som påføres de mest direkte skader fra skogbranner, er dermed også de viktigste brukerne av ild.

Den første artikkelen tar sikte på å forklare dette tilsynelatende paradokset, og forsøker å forklare hvordan lav avkastning og landdegradering opprettholdes ved bruk av brann. Den viser at eksternaliteter ved brann, dvs. negative effekter av branner som oppstår utenfor småbondens eiendom, reduserer deres investeringer i brannkontroll og opptak av brannfrie dyrkingsteknikker. Å forhindre egenpåsatt brann i å spre seg til resten av eiendommen er dyrt, og bortkastet innsats hvis branner som kommer fra andre eiendommer trolig vil ødelegge gården uansett. Hvis brannrisikoen er for høy, har ikke småbøndene insentiv til å investere i alternative brannfrie dyrkingsteknikker fordi de likevel vil være utsatt for tap av eiendeler ved branner forårsaket av andre småbønder. Økt brannfare forårsaker mer brannbruk og mindre brannkontroll. Sammenhengen mellom graden av koordinering av brannrisiko, brannbruk og den tilhørende gårdsinntekten gjør at beslutningene må analyseres simultant. Utfordringene som dette skaper kan overvinnes ved bruk av instrumentelle variabler. Artikkelen estimerer inntektselastisiteten til eksternaliteter av brannrisiko, både for småbrukere som bruker brann og de som ikke bruker brann. Den negative eksternalitetene er store, og større for ikke-brannbrukere. Det støtter bruk av en koordinasjonsmodell for å analysere brannbeslutninger.

De tre andre artiklene i avhandlingen rapporterer om et økonomisk felteksperiment basert på et koordineringsspill. De vurderer den samlede effekten av tørke og politiske virkemidler på koordinering for å redusere brannrisikoen. Tørke fører til at branner lettere sprer seg utenfor nabolaget eller lokalsamfunnet og dermed påvirker andre befolkningsgrupper. Her testes virkningen av konstant vs. økende tørkerisiko, noe som gjenskaper et mulig klimaendringsscenario. Tre ulike politiske virkemidler for å redusere brannrisiko testes: i) direkte reguleringer av ukontrollert brann, ii) betalinger for miljøtjenester betinget av bruk av brannfrie teknikker, og iii) fellesskapsbaserte løsninger gjennom direkte kommunikasjon.

[2] https://prodwww-queimadas.dgi.inpe.br/aq1km/

Den første artikkelen kontrasterer effekten av direkte reguleringer (forbud og straff) med betalinger for miljøtjenester; og finner at begge virkemidlene øker bruken av brannfrie teknikker, men at sistnevnte ikke reduserer brannrisikoen fordi det reduserer investeringer i brannkontroll. Bønder reagerer på tørkerisiko med mer bruk av ukontrollerte branner. Det kan tyde på at virkningen av tørke delvis kan være menneskeskapt. Alle virkemidler har større effekt i scenariet med økende tørkerisiko; noe som tyder på at småbøndene påvirkes mer av virkemidlene i tørkeår.

Den tredje artikkelen analyserer virkningen av fellesskapsbasert brannhåndtering gjennom kommunikasjon om koordinering. En nivå-k modell viser at kommunikasjon fremmer koordinering. En sosial norm gir samme effekt, ved at forventningene konvergerer, og skaper et tabu for det forbudte valget (brann uten kontroll). Hvis kommunikasjon og normen virker sammen, og normen ikke gir tilstrekkelig garanti for koordinering, er utfallet verre enn for kommunikasjon alene fordi tabuet hemmer troverdigheten ved signalisering til kommunikasjon. Til slutt kan kommunikasjon av forespørsler forbedre samordningen under en svak sosial norm hvis spillerne antas å være troverdige. Jeg finner bevis på en svak brannkontrollnorm i eksperimentet, og støtter prediksjonene fra nivå-k modellen. Jeg konkluderer med at lokal og kommunikasjonsbasert brannhåndtering har større sannsynlighet til å redusere brannrisiko når tørkerisikoen er høy nok til å svekke normen, og når forespørsler kommer fra innflytelsesrike ledere i lokalsamfunnet.

Den siste artikkelen analyserer virkningen av sosiale- og risiko-preferanser og oppfatninger på koordinering i feltobservasjoner og eksperimenter, og gir en ekstern validitetstest av eksperimentet. Sosial- og risikopreferanser og oppfatninger er teoretisk relevant for koordinering, og påvirker om koordineringstippepunktene er passert, og til og med antall likevekter i spillet. Standard eksterne validitetstester basert på korrelasjon mellom eksperimentell oppførsel og et kontrafaktisk mål er sårbart for falsk korrelasjon og kan være teoretisk inkonsistent. Vi implementerer en ny test basert på sammenfall av atferds-prediktorer i og utenfor eksperimentet: prediktorvaliditet. Kontrafaktumet utenfor eksperimentet er basert på oppgitt atferd. Data for både oppgitte og insentivkompatible tiltak av preferanser er innsamlet. Vi finner at risikooppfattelsen, men ikke risikoaversjon, forårsaker feilkoordinering. Pro-sosiale preferanser forbedrer koordineringen utenfor, men ikke innenfor eksperimentet. Andre forklaringsvariabler analyseres også for å eksemplifisere funksjonen og grensene for prediktor (ekstern) validitetstesten. Vi konkluderer med at eksperimentet sannsynligvis er eksternt gyldig når det gjelder bruk av brannbruk. Testene synes å avvise ekstern validitet av brannkontrollvalg, men siden ikke alle forutsetninger for testen er oppfylte er ikke denne tolkningen entydig. Virkemidler som tar sikte på å redusere brannrisikoen bør være rettet mot en reduksjon i oppfattet brannrisiko og ta hensyn til pro-sosiale preferanser.

Brannrisiko fanger småbønder i en ond sirkel av lav avkastning og miljøforringende arealbrukspraksis i form av ukontrollert brannbruk. Virkemidler for å redusere brannrisiko kan derfor oppnå tre, ofte konkurrerende mål: i) å redusere karbonutslipp, ii) å bevare biologisk mangfold, og iii) å legge grunnlaget for økonomisk utvikling.

# Introduction

## 1 Introduction and motivation

The use and control of fire has shaped human evolution and civilization. Today, in the Amazon, fires is used to clear land, fertilize soil and control pests. Fire is a cheap worker and fertilizer, but whenever humans fail to govern it, enormous damage can occur to the surrounding people and the environment.

This thesis tells a story about the largest tropical forest left on the earth, and the struggle to find a path of development and agricultural practices that is different from the one trodden thus far. Fires attract major interest globally, because they interconnect with three of the main challenges of humanity: climate change mitigation, biodiversity conservation and poverty reduction.

### 1.1 Fires and climate

Although humans ability to control fire determined our survival and shaped our evolution (Wrangham and Carmody, 2010), this ability is still imperfect and harmful accidental fires occurs frequently (Bowman et al., 2012; Lohman, 2007). Recognition of fires as a phenomenon with global implications is recent, and connected with the emergence of climate change (Bowman, 2009, Pyne, 2007).

The IPCC reports that global warming increases the risk of extreme fire events worldwide (Solomon et al., 2007). Fires in turn affect climate in a number of ways: $CO_2$ and aerosols emissions, reduction of surface albedo and forests evapotranspiration. Yet, quantifying the feedback between climate and fires is extremely challenging, due to interactions among biogeochemical cycles, vegetation patterns and human action (Bowman et al., 2009). Although much research concentrated on understanding the natural factors underlying fires, such as droughts, it is still unclear whether anthropogenic or climatic factors are more important in determining fire patterns (Bowman et al., 2009).

Fire activity is connected to precipitations and thermal variations across geological eras, and annual and decadal scales, such as the North Atlantic and El-Nino Southern oscillations. The latter are in turn likely affected by global warming (Li et al., 2006). Precipitations in the tropics are also related to the regional climate, which is strongly affected by forest cover through evapotranspiration (Sheil and Murdiyarso, 2009). Deforestation, fires and other forms of degradation reduce forests' ability to retain humidity, therefore reducing evapotranspiration and local rainfalls (Mahli et al., 2009; Sheil and Murdiyarso, 2009). Precipitations explain the inter-annual variability and spatial distribution of fires, but not their trend over time, which is mostly associated with land-use change and human activity (Andela et al., 2017). Fires are generally decreasing worldwide, due to land conversion to agriculture and related fuel removal, especially in savannas and grasslands. The exception is the increase in the Amazon (Andela et al., 2017; Aragao and Shimabukuro, 2010), where agriculture is at the origin of fires.

Worldwide, fires produce gross $CO_2$ emissions in the order of half of those produced by fossil fuel combustion: 2 to 4 Pg C year$^{-1}$ versus 7.2 Pg C year$^{-1}$, but fires also increase carbon storage through vegetation regrowth and black carbon in the soil (Bowman et al., 2009). The net emissions varying across biomes. Fires increase carbon sink capacity in some areas, such as boreal forests, but contribute to making tropical forests a net carbon source, releasing about twice as much carbon as the gained amount (Baccini et al., 2017; Pearson et al., 2017). Fires in the Amazon are usually low intensity understory fires, rarely affecting the tree crowns directly. Emissions originate from combustion of the understory vegetation and from the slow death of trees, which are not adapted to fires of any intensity. Berenguer et al. (2014) estimated that logged and burnt forests lost up to 40% of the aboveground carbon stocks. Direct carbon emissions during drought years can be substantial. In 2010 they amounted to 510$\pm$120 MtC (Anderson et al., 2015), equivalent to ca. 5% of global emissions.[1]

Minor channels through which fires affect climate are aerosol emissions and albedo effects. Black carbon aerosols spread in the atmosphere after combustion, increasing the capture of solar radiation heat, and reducing local clouds formation, which in turn reduces precipitations and further increases fuel accumulation (Aragao et al., 2007; Bowman et al., 2009). Fires in boreal forests melt snow and ice locally, reducing the associated cooling albedo effect. On the other hand, after a fire, more bare land is covered with snow, possibly outweighing the initial negative effect on albedo and through aerosols emissions (Bowman et al., 2009). With no ice-related albedo effect to be expected in the tropics, fires have no cooling impact on climate in that region.

Global and regional climates and fires affect each other in the Amazon in a self-reinforcing loop, potentially leading to more fires in the future.

## 1.2 Fires and biodiversity

Worldwide, fire had and still have large ecological effects, acting as a weak selection filter in the evolution of many species (D'Antonio and Vitousek, 1992). The fire regime in a biome is characterized based on fuel type, temporal and spatial incidence and consequences (Bowman 2012). Some biomes are naturally free from fires, such as the Amazon, others are prone to fires, such as the Cerrado and African Savannas or the Mediterranean. The worldwide decline of fires in grasslands and savannas, for instance, mitigates aerosol pollution and $CO_2$ emissions, but also destroys the habitat of many fire-dependent species. In the words of Parr et al. (2014: p212): “The assumption that ‘more trees are better’ does not hold for tropical grassy biomes”.

At the opposite end, exposure to fire and logging in tropical rainforests doubles the biodiversity losses from deforestation (Barlow et al., 2016). In the Amazon, human induced fires have the potential to change the vegetation composition in favour of invasive fire-prone species, impeding the recruitment of endemic plants and reducing the assemblage diversity. Cochrane et al. (1999)

[1] According to the global carbon budget, there about 10GtC were emitted worldwide in the same year https://www.co2.earth/global-co2-emissions Last accessed 12/9/17.

and Nepstad et al. (2001) posit a “savanization” of the Amazon region, caused by dryer climate and fires, supported in the Southern and Eastern Amazon by Davidson et al. (2012) and experimentally supported in the South-Western Amazon by Silvério et al. (2013). Balch et al. (2015) find resistance to invasive species in the South-Eastern Amazon, but substantial grass invasion after repeated burns. The long term resilience of the Amazon biome to grass invasion is unknown.

Fires affect the species composition of the Amazon forest, and threaten the boundaries of the whole biome. Invasion from grassland in the Amazon might further increase fire susceptibility in the future (Balch et al., 2009; Nepstad et al., 2001; Silvério et al., 2013) switching the fire regime from tropical forest into a savanna one.

### 1.3 Fires, agriculture and poverty in the Brazilian Amazon

The North of Brazil is one of the poorest region of the country. In 2014, 5% and 17.5% of the population lived on less than USD 1 and 2 per day, respectively. With a Gini coefficient of 0.5 for income, inequality is among the highest in the world.[2] However, poverty diminishes significantly among smallholder farmers, once rural specific livelihoods are accounted for (Guedes et al., 2012).

Since the beginning of the Amazon colonization, fire has been the essential livelihood tool for the poorest, as well as the main way to claim land ownership for large cattle-ranchers (Nepstad et al., 2001; Nepstad et al., 1999). It acts as a cheap voluntary worker that maximizes the return on the farmer labour for the tasks of land clearance, fertilization and pest control (Pollini, 2014). Mostly studied in association with slash and burn and deforestation (e.g. Angelsen and Kaimowitz, 2001; Pollini, 2014), fire is also largely used on previously deforested land to manage invasive species in pastures or to clear fallow vegetation (Aragao and Shimabukuro, 2010). Fire is usually adopted in association with an array of other techniques and in a variety of complex agricultural systems for which it is hard to substitute, unless good market access is available for inputs and outputs (Pollini, 2014).

Fire is commonly stigmatized as an environmental degrading and backward technique. Such an assessment is often a counterfactual-free simplification that fails to understand the many benefits of using fire, and put fire users at the margins of agricultural development (Costa, 2004; Coudel et al., 2013). It is still not clear whether properly controlled fires and land sharing swidden agriculture produce worse ecological impact than land sparing intensive techniques (e.g. Padoch and Pinedo-Vasquez, 2010). Pollini (2009) found that the 20 years long effort to produce alternative techniques – the Alternatives to Slash and Burn program[3] - failed to engage rural populations in a transition out of fire use, possibly due to a narrow focus on technical solutions, without sufficient effort to nest them into local dynamics and narratives (Pollini, 2009). A similar flaw has been found for policy makers, analysing Brazilian legal requirements for controlled fires (Carmenta et al., 2013;

[2] www.ipeadata.gov.br Last accessed: 16/11/2107. Actual poverty lines are 70 and 140 BRL per month respectively.
[3] http://www.asb.cgiar.org/ Last accessed: 29/11/2017.

Costa, 2004) and their implementation (Cammelli, 2014). The academic literature also at times wrongly interpreted all fires as uncontrolled, destructive practices (as epitomized in Varma, 2003).

Fire control is expensive (Cochrane et al., 1999), and increasingly difficult in drought years and in progressively more flammable landscapes (Carmenta, 2013). Yet, fire control is – in some forms – frequently implemented by smallholder farmers to prevent their own fire to destroy the rest of their property (Bowman et al., 2008). Accidental fire risk originating from others' uncontrolled use of fire, has been posited to decrease the adoption of fire free techniques – because it reduces the expected return of (flammable) investments (Nepstad et al., 2001) – and to decrease fire control efforts, because the external fire risk to the property jeopardizes the benefits of controlling the own fire (Cammelli, 2014; Coudel et al., 2013). Increasing fire risk might induce farmers to adapt by reducing their risk exposure: adopting more fire intensive land use types and reducing fire control efforts. Combined, fire risk might lock farmers into environmentally degrading and low-yielding land-use practices (Article 1 of this thesis). A pattern that is frequently encountered in the Amazon (Garrett et al., 2017).

Studies indicate substantial fire risk externalities of smallholders' production. de Mendonça et al. (2004) estimate that 15,939 km of pasture fences were accidentally burnt annually between 1996 and 1999 in the Brazilian Amazon. Pokorny et al. (2012) report a 15-60% fire risk over 30 years affecting tree plantations, based on studies in Brazil, Bolivia, Peru and Ecuador. Simmons et al. (2002) find a similar fire risk along the trans-Amazonian highway. Schroth et al. (2003) report that 55% of their interviewed farmers remembered loosing trees in rubber agroforests because of fires. Cammelli and Angelsen (in this thesis) report that 43% of the 576 smallholder farmers in their (Eastern Amazonian) sample experienced at least one accidental fire in the previous five years. This suggests that direct damages are widespread. Indirect losses might also occur due to a risk-induced misallocation of resources, but are not documented.

Again, the interaction of fire with its cause can create feedback loops: if poverty increases the likelihood of uncontrolled fire use, more fire risk might reduce investments in higher yield techniques, generating a poverty trap.

### 1.4 Fires and deforestation in the Brazilian Amazon

For a long time, fire in the Amazon was confused with deforestation. Between 2004 and 2012 a complex set of policies and a favourable conjuncture of low commodity prices caused an 80% drop in deforestation (Assunção et al., 2015; Barreto and Araujo, 2012; Gibbs et al., 2015; Nepstad et al., 2014). Fires, however, showed no similar trend, and increased up to 59% in areas of reduced deforestation (Alencar et al., 2015; Aragao and Shimabukuro, 2010; Mahli et al., 2009; Morton et al., 2013). These different trajectories are explained by a combination of agricultural and accidental fires (Aragao and Shimabukuro, 2010; Cano-Crespo et al., 2015).

The decoupling of fires and deforestation surprised scientists as well as policy-makers: fire seemed invulnerable to the measures in place. A new set of tools needs to be designed for forest conservation policies, such as REDD+,[4] to achieve their goals of reducing degradation (Barlow et al., 2012). New research on the causes underlying fires reveal that mitigating deforestation and fires can be competing and not complementary goals for some policy tools. For instance, the uptake of efficient fire-free technologies mitigate fire risk while potentially increasing deforestation incentives (Morello et al., 2017). Other studies show that landowner absenteeism increases fires in the Peruvian Amazon (Schwartz et al., 2015), while lower population pressure should lower deforestation (e.g. Laurance et al., 2002).

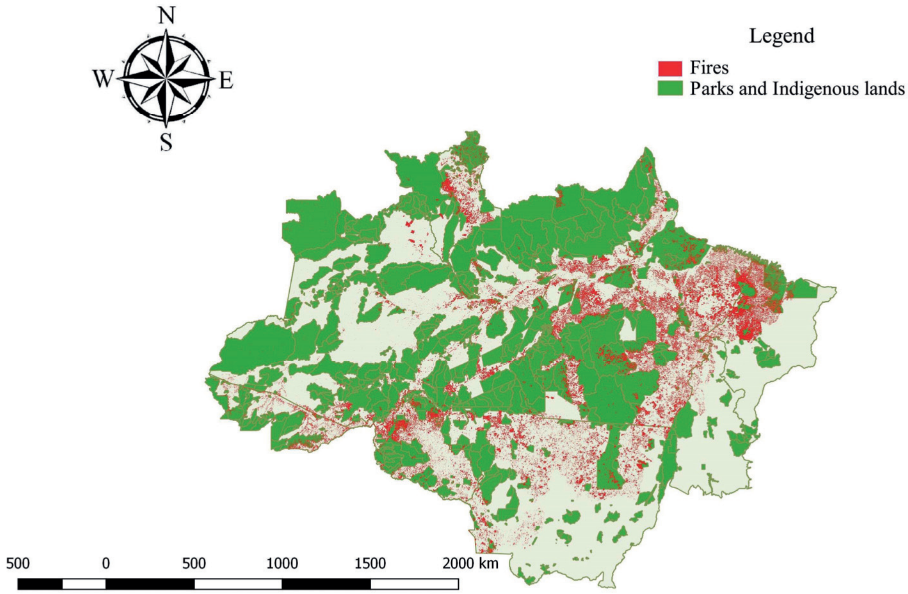
Fire is and has been used by all types of farmers, not only by smallholders but also by cattle rancher and soy producers (>500 ha), who accounted for half of deforestation between 2004 and 2011 (Godar et al., 2014). Yet, today, medium and large producers constrained by anti-deforestation policies are intensifying agricultural production (Godar et al., 2014; Macedo et al., 2012), and are therefore less likely to use fire. Such a transition has not taken place for smallholders (Godar et al., 2014), and it is unlikely to happen in the near term, unless new measures are taken.

Finally, research show that protected areas and indigenous land, but not sustainable use reserves, prevent fires (Adeney et al., 2009; Carmenta et al., 2016; Nelson and Chomitz, 2011; Nepstad et al., 2006; Soares-Filho et al., 2010), and that fires concentrate on private land (Figure 1). Yet, very little is known about fire patterns and their drivers in private lands, which are rich in forest of high conservation value (Barlow et al., 2007; Chazdon et al., 2009; Karthik et al., 2009; Moura et al., 2013) and are hard to regulate (Vieira et al., 2014).

This thesis focuses on smallholders' fire use and control behaviour, and on policies to prevent accidental fires on private land.

[4] Reducing emissions from deforestation and forest degradation plus conservation and enhancing forest carbon stocks in developing countries.

*Figure 1 The 2015 fires on parks and indigenous lands in the Brazilian Legal Amazon*




Source: own elaboration on fire data available at http://www.inpe.br/queimadas

## 2 A methodological interlude

Before moving to a description of the *problématique* of this thesis and the methods used to shed light on it, I review and critically discuss the methodologies used in this work and their limitations in a general fashion, while specific shortcomings are discussed in each single paper. The aim is to provide interpretative tools to read the rest of this work.

In the rest of this section, I present and discuss econometrics and economic experiments within the open system ontology of critical realism. This approach is informative because most of the controversial features of econometrics and economic experiments originate from the definition of the object of study. At the end of this section, I try to reconcile the critique with the research practice. A major claim is that, when the critique is valid, the merit of the analysis mostly depends on the specific hypotheses to be tested, their formulation, and the mindset of the researcher, rather than on the method itself.

The main limitations I address are the failures of identification in econometrics and the artificiality in economic experiments, and how these relate to theory. I will touch upon the related issues of external and internal validity, and the passive role imposed by assumption on the subjects of study (e.g. when the choice set is given).

In the last 30 years, economics underwent a transition, becoming more and more an empirical science (e.g. Thaler, 2016). Yet, what empirical means is far from clear. Critical realists oppose simple empiricism - real is what is perceived - by imposing attention on more complex ontologies: reality goes beyond perceptions (e.g., gravity is disguised by buoyancy in water). More generally, not all reality is available for observation and empirical refutation, for example, mechanisms and powers are disguised and not directly inferable from events and experience (Lawson, 1997). Econometrics and economic experiments share the purpose of identifying observable as well as underlying fundamental mechanisms, which are not directly observables. However, to what extent regression analysis and experiments can accomplish this mission is at the core of the critique of these two methods when applied outside natural sciences.

Society and the economy can be understood as open systems, in which many powers and mechanisms concur to the observed phenomena. In an open system as described by the Bhaskar (1978) relational model, individuals interact with other individuals, as well as with social structures. The directions and causes of behaviour are not constant in space and time because individuals are characterized by a high degree of spontaneity: they react to their own beliefs and reasons. Defined in opposition, closed systems are well ordered and characterized by two conditions (Lawson, 1997):

1. the intrinsic condition of closure (ICC): a cause always produces the same effects, i.e., the intrinsic structure of the object of study is constant; and
2. the extrinsic condition of closure (ECC): an effect always has the same cause, i.e., the mechanism operates in isolation from other factors.

The second condition is similar to identification in econometrics and to control in experiments. The first condition, that causal factors are constant, is strictly connected with the assumption of ergodicity (North, 2005) when stability relates to time; and more generally to external validity, when it relates to populations, space and time.

While the second condition is usually at the core of the discussion of empirical work in economics, condition number one is mostly approached by assuming atomism: if all individuals act independently from each other and independent from the context, the causes of their behaviour are stable across time, space and subjects.

From the definitions of open and closed systems it descends that empirical regularities are rare in open systems, and that a research program concentrated on seeking them, is misleading. Economists strive to enclose the behaviour object of analysis into closed systems which respect the two conditions above, because in a closed system causal relations can be individuated.

Below we discuss the implication of an open system ontology for econometrics and experimental economics. The aim is to present and to address some of the main critiques to the methods, and to discuss some caveats necessary to read the results of the articles.

## 2.1 Econometrics and causation in econometrics

While econometrics can be broadly defined as the application of regression methods to economic data, a more precise definition is made difficult by the wide array of methods with different assumptions. The standard approach can be defined as the "average economic regression" (Downward and Mearman, 2002). It consists of a linear specification of a dependent variable $Y$ (the consequence) as a function of its vector of causes $X$ in a *well-defined* model:

$$Y = X\beta + \varepsilon$$

where $\beta$ are the partial slope parameters, or the weights, associated with each element of X. $\varepsilon$ is a random error with zero average. The main assumption underlying the model is that it well defines the causal relations to be estimated. Under this assumption the model produces sample-specific statistics, which are often interpreted as empirical regularities generalizable to the population from which the sample is drawn.

Claims of causality, from $X$ to $Y$, and inductive claims to the future (forecasts) generate identification problems, which are mainly solved by establishing some connection with a theoretical model. In this sense, an econometric model is strictly bounded by the theory underlying it. A second order problem arises when the econometric model is meant to test the theoretical model underlying it: if several competing models fit the data, how to choose the right one? Problems of theory selection and causality have been addressed in the last two decades with an enlarged diagnostic toolbox: the attention has been shifting from the underlying theoretical model to the adherence of the model to the data. Yet, the focus of modern econometrics is still to test hypotheses and the underlying theories rather than describing data.

This basic characterization of econometrics raises at least three issues. First, there is a wide discretional space in the choice of covariates, functional forms and estimators associated with a model and in the choice of how to manipulate variables. Such a discretionary power hints that model results might be inventions rather than discoveries.

Second, the use of quantitative data requires qualitative invariance: measurements repeated in different time and space and across individuals should capture the same phenomenon (i.e., ICC). Qualitative invariance holds under atomism, but not under an open system ontology. In this case, social objects are only partially invariant, i.e., they are interdependent with each other and with their environment. This problem might be severe, especially when actual measurements are unavailable and proximate measures are used.

Third, inference requires specifying a probability distribution over the real events. This is done in an instrumental manner, conflating object and subject of analysis (the empirical model and the actual reality), and requires the closure assumption (ECC for identification), which is rarely achievable within the Bhaskar ontological model of society. The ultimate problem is of how to learn from estimates that are obtained from an empirical model and data manipulation that relies on a theoretical model of reality that might be false.

## 2.2 Experiments, causation, external validity and artificiality.

Experiments are tools to enclose parts of an open system into a well-defined closed system, where causal claims can be made. Experimental economics normally use cash-motivated subjects to mimic real world incentives and test microeconomic and behavioural theories. Experiments can be conducted in the laboratory or in the field, using a population, a task or a framing of interest (Harrison and List, 2004).

Economic experiments have at least three possible objectives: to test theory, to identify empirical regularities (not necessarily predicted by theory), and to advise policy makers (Schram, 2005; Siakantaris, 2000). To achieve these objectives, Wilde (1981) and Smith (1982) define four necessary conditions:

1. Non-satiation,
2. Saliency (of cash incentives),
3. Dominance: there are no *subjective* benefits or costs in participating in the experiment,
4. Privacy: non-scrutiny by other participants.

These conditions are claimed to be sufficient for closure (Siakantaris, 2000). A fifth condition is *parallelism* between the experimental situation and the real world. This implies that the findings in the lab hold, *ceteris paribus,* in the real world. Non-satiation is required for coherence with theory, saliency is required for ICC, dominance and privacy are required to establish atomism (except for the institutions and roles deliberately brought into the experiment by the researcher). Under these conditions, experimentalists claim that their findings are relevant outside the experiment and that they are more internally valid compared to econometric ones, which cannot benefit from controlled variation.

Parallelism, also referred to as external validity, is important mostly for experiments seeking empirical regularities and to advise policies, rather than theory testing (Schram, 2005). At one extreme, external validity is not an issue when testing general theories: finding merely one exception in a lab experiment is sufficient to reject the theory. Instead, whenever the goal of the experiment relates to empirical regularity, a policy, or a theory that restricts over a population, a task, a framing, time or space, the experiment design has more potential to influence the interpretability of results, and the relevance of the findings more critically relies on the parallelism assumption. This is also why external validity is more important for framed field experiments than for lab experiments.

External validity is ultimately an assumption about how the experiment approximates the out-of-experiment environment. If, in contrast to atomism, we assume a relational model of society, agents interact with the experimental environment as much as they interact with the out-of-the-experiment environment. The experiment is real, and so are the findings, but they might not originate from the same causes of the out-of-experiment behaviour, because the intrinsic closure condition (ergodicity) is not achieved. In other words: there is a gap between environmental factors in and out of the experiment that interacts with the behaviour observed in the experiment.

Then, if behaviour is not independent from the environment, a change in environment induces a change in behaviour.

External validity has often been conceived as a testable hypothesis. However, it is not clear under what assumptions this is testable: parallelism (or external validity) cannot be tested by means of another experiment. This would result in an infinite experimental regress (Siakantaris, 2000). It cannot be tested econometrically either, because proper control variables, and therefore a *well-specified* model do not exist (Siakantaris, 2000). Parallelism is more of an assumption than a testable hypothesis. The parallelism assumption is at odds with the open system ontology, which deems real the internal relationality of socio-economic mechanisms. This is indeed the reason why, under the Bhaskar relational model, economic experiments have no external validity.

If the parallelism assumption does not hold, economic experiments would have no ability to test theories and hypotheses about the out-of-the experiment world, with the exception of universal theories. All experiments which investigate empirical regularities and aim to give policy prescriptions would have no scope either.

To summarize, the ultimate critique deriving from open system ontology is that economic experiments fail to close the social phenomenon, because their natures are relational. The experimental relation should be considered a specific kind of interaction in itself. All findings are severely biased as they are induced by artificial powers, created by the experimenter. Paradoxically, the more specific and well-defined the laboratory interaction, that is, the more the experimenter is doing his job in controlling the experiment environment, the more the observed result becomes irrelevant because artificial.

### 2.3 Towards a reconciliation: open ontology, how open?

The open system ontology dismisses experiments, because atomism and parallelism do not hold. It also dismisses econometrics, because (i) lack of ICC impede the collection of qualitatively invariant (quantitative) data, (ii) the ECC for identification and inference is not verified, and (iii) the discretionary power in specifying empirical models is more likely to yield inventions, rather than discoveries. An open system implies a non-ergodic world in which no theory is possible to ground the empirical model, preventing all identification.

Notwithstanding the open system critique, critical realism fails to provide suitable alternatives for choosing among competing explanations of behaviour, and concentrates only on extreme cases. For example, there are no satisfactory positive definitions of open and closed systems (Mearman, 2006), nor of the degrees to which a social system can be closed or open and how openness is expected to change over time and space. Is an open system the one that is usually stable unless some shocks occasionally occurs, or is it instead a constant turmoil of active and messy mechanisms continually combining in novel ways? In the latter case, there is little chance for any science. Lack of a positive definition of a closed system led to a dualism where a system that is not closed is necessarily open and methodologically inscrutable with standard economic tools.

This is unlikely, and problematic on a realist perspective, i.e., most systems probably lie in between the two extremes (Mearman, 2006).

Whether closed system methods such as experiments and econometrics can contribute to knowledge depends on how much open systems are of the first type, and can under certain conditions, be subjects to some degrees of closure. This problem has been mostly ignored in the critical realist tradition: "The concern with ontological depth has been accompanied by relative ignorance of that depth. Perhaps this has occurred because of the central concern to criticize the orthodoxy" (Mearman, 2006: p71).

Yet, I believe that there is a possible reconciliation and practical implications of open system ontology for the practice of social sciences research. Sayer (1992: p124) introduces the concept of "approximate and spatio-temporally restricted" demi-regularities, which can locally be addressed with closed system methods. Relatedly, Setterfield (2003) discusses process-openness by which the social scientist always bear in mind the closure assumptions and bound their application within reasonable limits. For instance, a model that appears closed and typically involves mathematics can be built with no universal claims and an open system in mind: contingencies and time changes can be incorporated as arguments for choosing model features and explain model results. An open system can be decomposed into sub-systems which are demi-closed and can be analysed separately, bearing in mind that they are part of the main open system: a conditional closure.

Concerning experiments, a researcher in the early stage of a project rarely knows how and how much the subjects interact with the experiment. Experience and piloting are essential to achieve what Paluck and Shafir (2015) call a "shared construal" between subjects and the experimenter: the experimenter is aware of how subjects perceive the experiment, also beyond the theory underlying the design (see also Harrison and List (2004) argument for manipulating *framing* and running framed field experiments). In the end, external validity is possible and depends on details of the implementation, which are usually left to the experimenter wisdom. With some caveats and in some conditions, external validity can be tested (as discussed in the last article of this thesis).

On the other hand, while the use of observational data allows the agent to express the interaction with the environment and to be an agent of change, experiments are artificial in that the context of the agent choices is entirely given. Even though the experimenter can approximate the design to the outer environment, there is a limit to this approximation, and consequently, a limit to the applicability of experimental methods in social sciences, which relates to the level of closeness of the topic. In other words, messy complex open systems cannot be naively closed within experiments under the atomism and parallelism assumptions. Understanding where the marginal reasonable experiment design lies depends on the researcher wisdom and on the field specific norms.

A similar argument holds for econometrics: identification requires a thick description of the field of study, well beyond theory underlying the model and the hypotheses at test. When the researcher shows this knowledge, the underlying theoretical model is more credible and so are the estimates.

Yet, to the extent that the underlying model systematically deviates from reality, estimates lose interpretability. To what extent one should ground empirical work into normative or behavioural-descriptive theories (in the sense of Thaler (2016)) is still unclear in the literature. The rational choice model is acknowledged as a benchmark for theoretical speculation, but a false positive theory. Yet, it is often applied to ground empirical work, which is epistemologically inconsistent. This possibly happens because pure behavioural models of real world complex choices are not yet developed.[5] In sum, of the three critiques to econometrics listed in the beginning of this section, the third seems to me addressed at the core of most well-done empirical enquiries, while I deem the first and the second critiques to be a matter of process-openness.

Process openness can be understood as the result of researcher education and experience, and of triangulation between methods (Downward and Mearman, 2007), bringing insights on the same phenomenon from different traditions of social thought (rather than through mere hypothetic-deductive reasoning), for instance combining qualitative field data about agents' motivations together with econometric and experimental results. Reasons and motivations can then be part of the explanation of results as well as set the conditions for mechanism identification and description, by providing support for a sufficient demi-closure and the parallelism assumption to hold.

# 3 Understanding fires in the Amazon

Our understanding of fires on private land in the Brazilian Amazon is still marked by many uncertainties and data constraints, for example, concerning the interpretation of remotely sensed (satellite) data (Morello et al., 2017) or the reliability of stated fire control surveys (Carmenta, 2013). Yet, understanding the causes underlying fires is essential to design effective policies. This section briefly summarizes the data constraints, assumptions and conceptual framework of this thesis.

## 3.1 Data constraints

Fully understanding forest fires requires data from a variety of sources. Much of the literature use satellite data only. The advantage of this approach is the large scale of inquiry, necessary for policy making. However, little can be inferred from satellite images about the nature of the fire (Carmenta et al., 2011), what burned, whether it was intended or accidental, whether it originated in agriculture or from other causes such as trash burning or cooking. New remote sensing techniques distinguishing accidental from intended fires through the fractal dimension of the burnt area has been tested (Cano-Crespo et al., 2015), but are not yet popularized.

[5] New theories in economics are being developed with little fortune. Yet, celebrated economists in advanced career stage and even Nobel laureates have a tendency to trash theories in their field. See for instance «Rational fools» by Amartya Sen (1977) or «How did economists get it so wrong?» by Paul Krugman (2009), or more recently «The trouble with macroeconomics» by Paul Romer (2016). This phenomenon extend over the boundaries of economics with the physics Nobel laureate Frank Wilczek (2008) speaking in favour of aether. I am thankful to Dr. Fredrik Andersen for comforting me in the face of this empirical regularity.

Satellite data about fires are of two kinds. Hotspot pixels indicate the location and time of a fire. Burnt scars determine the location, time and area affected by the fire. Hotspot pixels are measured with substantial errors but higher frequency (cf. Article 1 of this thesis), and burnt scars are usually available on a coarse resolution (1 $km^2$). In both cases, it is hard to understand where the fire started, its cause and what burned. All three features are necessary to determine causal explanations of fire patterns that include human activity at a fine scale, e.g., at a sub-municipal level. Schwartz et al. (2015) produce and correlate fine scale burnt scars maps with survey data collected for 732 households in 37 villages in the Peruvian Amazon. This is a new and promising approach, yet not replicated by others.

Some studies have explored the finer scale of household decisions. Bowman et al. (2008) and Cammelli (2014) analyse stated fire control behaviour. However, Carmenta (2013) reports strong over-reporting of stated fire control data. Part of the over-reporting is due to a social norm (causing large accidental fires is not a morally accepted practice), and partly due to a mismatch in definitions and conception of fire control practices, as defined by the law and as implemented by the farmer. Such large over-reporting casts doubts about the use of fire control from stated data. One of the reason for adopting experimental methods in three of the papers in this thesis is to circumvent the problems of definition and over-reporting of fire control choices, as we return to in the last article of the thesis.

Turning to the evaluation of tropical forest conservation measures, they often suffer from poor data availability and except for protected areas, quasi-experimental situations are rare (Börner et al., 2016; Handberg and Angelsen, 2015). These problems are exacerbated in the case of fires in the Brazilian Amazon (Morello et al., 2017). With the exception of parks and indigenous land mentioned above, local policy implementations are scattered and poorly documented. Morello et al. (2017) generate data in an agent-based model to study the effectiveness of several fire policies on private land. With a similar aim, the second article of this thesis simulates policies within a framed field experiment.

Studying fires in the Amazon crucially depends on the evolution of remote sensing technologies and data collection methods on the ground, and on involving local policy makers. Recent research showed that human activity explains fire patterns more than expected (Andela et al., 2017), calling for more collaboration between social and natural scientists.

### 3.2 Main assumptions and generalizability

Studying fires in the Amazon is challenging, but the context allows to set a few assumptions that greatly simplify the analysis. First, there are no natural ignition sources, therefore there is univocal interpretation of fire events as consequence of human action. Second, there are no benefits associated with accidental fires, because forests that have not been slashed previous to suffer a typical low-intensity fire are not readily suitable for agriculture. Last, fire-fighting is irrelevant for smallholders because they don't have the means to carry out this activity effectively.

The absence of natural ignition sources in tropical rainforests drastically simplifies the interpretation of fire events as purely anthropogenic. A consequence is that, contrary to fire adapted biomes such as Boreal forests, Mediterranean, Australia, North America, African grasslands or the Brazilian Cerrado, there is no trade-off between reduced fires and biodiversity (cf. Moritz et al., 2014; Parr et al., 2014). All fires are environmentally harmful. There is a monotonic relation between human fire activity and environmental consequences.

Another important simplification is that there is no incentive to let a fire escape into a standing forest: all forest fires are accidental. This assumption is largely supported in the literature (Nepstad et al., 2001; Nepstad et al., 1999), yet never proven. The argument is a deductive one: there is no benefit associated with a standing forest burning, because forest fires are typically low-intensity understory events that leave all trees standing. In other words, a forest that has not been previously slashed and dried looks very similar before and after a fire (Figure 2) and the land is not suitable for agriculture.

*Figure 2 The same forest during and after a fire (Paragominas, 2013 and 2014)*

After a fire, it is not possible to plant any crop nor pasture in the standing forest. A labyrinth of secondary vegetation makes hunting more difficult, and fire kills fruit trees (Shanley, 2011). The only potential benefit that I am aware of is harvesting firewood from dead trees. Yet, during field interviews for this thesis, some farmers argued that it is too dangerous to log dry trees, because the vibration from the chainsaw or the axe might cause branches to fall with consequent risk of injuries. One might argue that with repeated burning, the forest slowly opens up and agricultural activities are made possible. Dr. Erika Berenguer observed this in Mato Grosso (personal communication). Yet, if the area accidentally burnt was intended to be allocated to pasture, why not slashing and burning the area on the first year, rather than waiting that accidental fires eventually produce the intended effect over five or more years? Eluding environmental regulation might provide a sufficient reason, because responsibilities for fires are less evident than for deforestation. A farmer could argue that the fire was started by the neighbour and that he has no guilt. However, current regulation enforces preservation of forest on 80% of a property, and a duty to reforest in the lack thereof. If farmers would systematically deforest illegally through arson fires, that pattern would be macroscopic, but at odds with the decoupling of deforestation and fires discussed in section 1.4. Moreover, one of the reforestation technologies proposed by EMBRAPA, the Brazilian Agency for agronomic research, involves ranching and building a fire-break around the area to reforest,[6] suggesting that landowners are expected to take measures to prevent fires in their forest reserves. Because tropical forest burning is of no economic use, accidental fires qualify as a pure risk factor for the local people.

Another assumption in this thesis is that firefighting is mostly irrelevant for smallholders. This is not the case for large landholders who own the means to defend their property from in-coming fires. Smallholders, on the other hand, have few tools and resources available to perform this activity. During six months of fieldwork, I attended a few cases of fire-fighting and many more were reported by the interviewed farmers. Yet, I can only remember two of them being successful, and not in extinguishing the fire, but in deviating it away from some valuable crops. Both cases were also exceptional examples of cooperation in which the farmer benefited from the help of the neighbours or of other villagers. One can consider firefighting as a collective action issue, whether to stay in one's own plot and work to prevent the fire to enter the plot, or to join a collective effort to stop the fire in neighbouring properties. Yet the collective action outcome rarely leads to fire extinction.

These assumptions are specific to the Amazon biome and to the population of interest. As such, analyses conducted in the Amazon cannot necessarily be generalized to other settings,[7] yet they have an intrinsic value: the Amazon is too big and unique to afford its loss.[8]

### 3.3 Conceptual framework and aim of the thesis

This thesis aims to explain the persistence of fire use and the lack of investments in fire control by Amazonian smallholder farmers. It also aims to explore behavioural drivers of coordination and compare and test *ex-ante* potential fire mitigation policies, and how these interact with climatic conditions in co-determining coordination for fire risk mitigation. Specifically, I address the short run and micro level determinants of individual and household fire use and control decisions, with a focus on strategic interaction.

Fire use and fire control decisions by Amazonian smallholder farmers have been analysed in a household model by Bowman et al. (2008). In the model, farmers use fire to clear land until the marginal benefit of increased agricultural production equals the marginal losses from forest product extraction. Farmers suffer accidental fires with a completely exogenous probability (all farmers are assumed to use fire on a regular basis, although in different cycles), and there is no strategic interaction. Investing in fire control reduces the potential loss of agricultural production

[6] https://www.embrapa.br/codigo-florestal/regeneracao-natural-sem-manejo Last accessed 16/11/2017.
[7] Section 4.1. further discussion the representativeness of the study area compared to the rest of the Amazon.
[8] See for instance http://csr.ufmg.br/amazones/ for an attempt to value a loss of part of the Amazon. Last accessed 16/11/2017.

if an accidental fire occurs. Labour allocated to fire control is traded off with labour allocated to production and leisure.

This approach has at least two challenges. First, it assumes that fire risk is entirely exogenous, and therefore, that fire control investments are protective and not preventive (e.g., firebreak around the whole property vs. firebreak around the area to be burnt). Second, it also assumes that all agricultural land use types are equally fire intensive in production and equally exposed to fire risk. The first assumption is problematic, because farmers operate fire control preventive investments on the area they burn, and not protective investments on the whole property, which would be overly burdensome, and limited to firebreaks. Rather, fire control investments encompass an array of preventive measures, and firebreaks alone are likely useless[9]. Related, fire risk is partly exogenous, associated with drought induced mega-fires (e.g. Alencar et al., 2015), partly endogenous to the household, stemming from the own fire use, and partly endogenous to the neighbourhood, stemming from the neighbours use of fire. Fire risk externalities arise at multiple levels. The second assumption is problematic because not all agricultural technologies equally require fire use (after land clearance), and not all agricultural land use types are equally exposed to fire risk. Rather, more extensive systems rely on fire more heavily than intensive systems, which instead require costly and flammable investments (e.g. additional pasture fences or agroforestry) that increase the farm fire risk exposure. This hypothesis is object of discussion and test in the first article of this thesis. The last important departure from Bowman et al. (2008) is that intensive systems give a higher yield, either in the form of higher return on the other factors of production, and/or because of a price premium. These last assumptions are intuitively verified, although they critically depend on market access conditions (Pollini, 2014).

I conceptualize the farmer's agricultural system as two different land use types with a production function each: one fire intensive with low-productivity and certain returns, and one capital intensive and highly productive, but exposed to fire risk. Fire control is applied to prevent own fire to burn own land allocated to capital intensive production. However, the latter is also exposed to neighbouring and exogenous fire risks. If fire risk external to the property is too high, more of the fire intensive technology is adopted, and less fire control is provided, causing a negative fire risk externality for the neighbours.

Because there is no benefit in starting an accidental fire (as established in section 3.2), there is no direct benefit for a farmer to cause a damage to the neighbour: the game is of common interest. To the contrary: the lower the fire risk from the neighbours, the higher the incentive to mitigate fire risk on the own property, either through fire control, or by allocating all agricultural land to the capital intensive fire-free production. There are strategic complementarities in fire risk mitigation.

Games with strategic complementarities are characterized by supermodular payoff functions and multiple Pareto ranked equilibria (Milgrom and Roberts, 1990; Topkis, 1979; Vives, 1990), and

[9] Or are perceived as such. Many farmers reported and I directly observed fire passing over unwatched paths and even a two lane asphalt roads in normal wind conditions. See also Carmenta (2013).

are usually referred to as stag hunt, assurance or coordination games.[10] In this class of games there are typically no free riding incentive or social dilemmas, rather, there is a coordination premium for choosing the same strategy as the opponent. Yet, these games are difficult to solve without assurance about the opponent choices. Both high and low equilibria are rationalizable based solely on the best response correspondence. A variety of selection criteria have been developed considering also risk and payoff dominance (Carlsson and Van Damme, 1993; Harsanyi and Selten, 1988) and most of the empirical literature is concentrated on analysing the salience of solution concepts and devices achieving or hampering coordination. Examples are the influence of risk and payoff dominance and the number of players (Van Huyck et al., 1990), coordination premium (Battalio et al., 2001), learning and bad precedents (Van Huyck et al., 1997) and communication (e.g. Cooper et al., 1992; see also the third Article of this thesis). Recently, some attention has been dedicated to the analysis of participants' features, such as patience, preferences and information about the others' preferences (Al-Ubaydli, 2011; Al-Ubaydli et al., 2013; Büyükboyacı, 2014; see also the last Article of this thesis). Yet, very little research exists in this direction compared to public good and common pool resource games, suggesting that coordination games have been perceived often as theoretically interesting constructs, but of little use in describing the real world, possibly because norms and context characterizing the real world are expected to generate focal points (Schelling, 1960) and to make these games of trivial solution. Rather, Nyborg et al. (2016) shows that many real-world problems are characterized by tipping points and externalities that induce strategic complementarities. Examples of these are climate action, diet and transport choices. Coordination is also shown to characterize adoption of institutions and technology (Alpizar et al., 2011; Aoki, 2001; North, 2005). Yet, there are very limited applications outside the lab or to natural resource management.

This thesis provides such application to a compelling coordination problem in the field. First, it aims to assess the virtue of the coordination framework, by testing strategic complementarities in fire risk mitigation in the Amazon and weighing the importance of fire risk externalities with other factors affecting farmers' fire use and control choices (Article 1). It also aims to experimentally assess the ex-ante impact of policies and droughts on farmers' coordination for fire risk mitigation and uptake of fire free techniques (Articles 2 and 3). The fourth paper assesses the role of preferences and perceptions on farmers' coordination and test the external validity of the experiment.

# 4 Areas of study and data

## 4.1 Areas of study

Three of the articles presented in this thesis report on a framed field experiment carried out in four municipalities of Pará: São Domingos do Capim, Irituia, Ipixuna do Pará and Paragominas. The region was first colonized by Portuguese missionaries who travelled along the Guama' and Capim

[10] A wider variety of order statistic games (e.g. minimum and median games) and many others reviewed in Milgrom and Roberts (1990) are also supermodular games.

rivers. São Domingos do Capim and Irituia are the older settlements in the sample. The first sources date back to 1758 and 1725, when the settlements were still part of the Belem municipality. In 1833, when São Domingos do Capim became an independent administrative unit, it was including the current municipalities of Paragominas and Ipixuna. The modern colonization of the two latter regions, however, started only in the second half of the 20th century, with the opening of the Belem-Brasilia highway.

Ipixuna was founded in 1958 as a farm and gas station, exemplarily called Km 108. It became a municipality independent from Paragominas in 1991.

All municipalities except Paragominas are inhabited by between 30,000 and 60,000 people, mainly in the rural areas. Paragominas, on the other hand inhabits almost 100,000 people, the large majority in the fast growing urban area. The surface of the municipality is larger and dominated by large scale producers, while small and medium producer dominate the landscape of the other, smaller municipalities.

The history of Paragominas is well documented and presented, among other places, at a local museum. While all previous information is collected on the IBGE website[11], most of the coming information were collected at this museum, or through first hand open-ended interviews with members of the institutions, small and large landholders, and through direct observation. Paragominas is an exemplar story of road colonization. Its name is due to the origin of the first colonizers, coming from Pará, but also Goias and Minas Gerais, in the South of Brazil. Following the construction of the Belem Brasilia highway, the region experienced large in-migration and land concentration with use of violence. Contrary to older colonization municipalities such as Irituia and São Domingos do Capim, where multiple generations live on the same plot of land, most of smallholders' settlements in the region came during the agrarian reform in the seventies. The land was expropriated from large illegal farms or previously abandoned, or through the instalments of small riverine communities in the thirties and fifties.

Paragominas followed a stage pattern of development, initially marked by cattle ranching expansion subsidized by the central government in an attempt to establish parity in the balance of payments and to fight poverty in the Northeast dry countryside. Extensive logging started in the eighties, and in the nineties Paragominas alone was producing 20% of the timber in the state of Pará. At the beginning of the new century, soy production started, and in 2010 the municipality was the largest producer of the State (Coudel et al., 2012; Gardner et al., 2013). In 2008, Paragominas was black-listed by the central government as one of the 36 municipalities with the highest deforestation rates in Brazil. After a violent conflict between large landholders and the Brazilian environmental agency (IBAMA), the municipality reacted in a surprisingly positive way, by aggregating consensus around the *Municipio Verde* (Sustainable Municipality) management model, which is deemed successful in reducing deforestation and is now extended to other municipalities in Pará (Barreto and Araujo, 2012; Viana et al., 2016). Nevertheless, in line with

[11] https://cidades.ibge.gov.br/ Last accessed 30/11/2017.

local history, smallholders where not invited at the negotiation tables of *Municipio Verde*. One of the rules agreed upon was the ban of all fires (municipal law 765/2011), but the law was not applicable without complementary enabling measures. Smallholder farmers complained about being left behind by agricultural development policies, forced out of the law by the fire ban, while the municipal officers claimed that fires are not a problem anymore in Paragominas, because they are banned (Coudel, personal communication). Despite all good deeds and the denial of the problem, a dense fire haze repeatedly invaded the town during the period of my fieldwork (September-December 2015).

A fire ban was not enacted in any of the other municipalities of study. During a field trip in 2013, I collected information about a bi-annual fire prevention program carried out by the municipal government of Ipixuna. The program involved extensive training and community based fire management operations coupled with some enforcement of the law banning uncontrolled fire. Specifically, two fines were applied during the whole program, one to a small and one to a medium producer. Interviews with smallholder farmers revealed a substantial change in perceived law enforcement, revealing a powerful deterrence effect of the fine (Cammelli, 2014). No municipal fire policies were encountered in the other municipalities.

The first article of this thesis uses data collected by the *Sustainable Amazon Network* (RAS) team[12] in the municipalities of Paragominas and of Santarem-Belterra in the Western part of Pará. Santarem is one of the oldest settlements in the Amazon dating back to the first Portuguese colonization. Smallholders' in-migration started more than a hundred years ago. Most of its development, however, is associated with the Trans-Amazonian highway and the development of mechanized agriculture by medium-scale producers (<1 000 ha). I am not aware of any specific fire policy in place in Santarem or Belterra, and the area is prone to fires. In 2015, flames devoured 7,200 $km^2$ only in Santarem, more than the whole deforested area in the Brazilian Amazon in the same year (Berenguer et al., 2016).

A relevant question of generalization is to what extent the study area is representative for the whole of Amazon. The Brazilian Amazon is a wide and diverse region encompassing a range of cultures and populations. Nonetheless, I believe that the conditions of smallholder farmers in the Eastern Amazon are fairly homogeneous and that our study area is representative for this part of the Amazon.[13] Little inference can be done to other populations, and to the South and Western parts of the region, which are marked by different historical paths, vegetation and population density, and infrastructure. Medina et al. (2015) produce the most recent picture comparing relevant family farmers' features across Brazilian municipalities (Figure 3). The maps display that Paragominas and Santarem stand out for better general family farmers' conditions, access to capital, market and socio-economic integration, while the other smaller municipalities seem closer to the regional average. Paragominas especially, seem to offer better access to land and capital. Interestingly these

[12] http://www.redeamazoniasustentavel.org
[13] Differences between riverine and mainland populations, origin of the colonizers, distance to urban centers etc. do matter, but I believe that they do not matter differently in our study site compared to the rest of the Eastern Amazon.

figures are at odds with smallholder political marginalization observed in Paragominas. Both Paragominas and Santarem smallholders benefit from potentially large trickle down effects from neighboring large farms (e.g., labor market and rental market for machineries). These effects do not take place where smallholders are the majority (Medina et al., 2015), like in Ipixuna. Figure 1 shows that the distribution of fires was more intense around the Paragominas region than in other parts of the Eastern Amazon. However, this figure is for 2015 only, and differences might vary significantly across time. Finally, all municipalities included in the study share some of the features of a *post-deforestation frontier*, or *forest-agriculture mosaics* (cf. Angelsen and Rudel, 2013). Results might generalize to smallholders belonging to municipalities falling in this same category.

As discussed below, relevant differences are found across communities within municipalities such as remoteness, land conflicts and level of deforestation. How these specificities affect generalizability outside the area of study is difficult to assess.

*Figure 3 Smallholders' critical development factors across municipalities in Brazil (source: Medina et al., 2015)*

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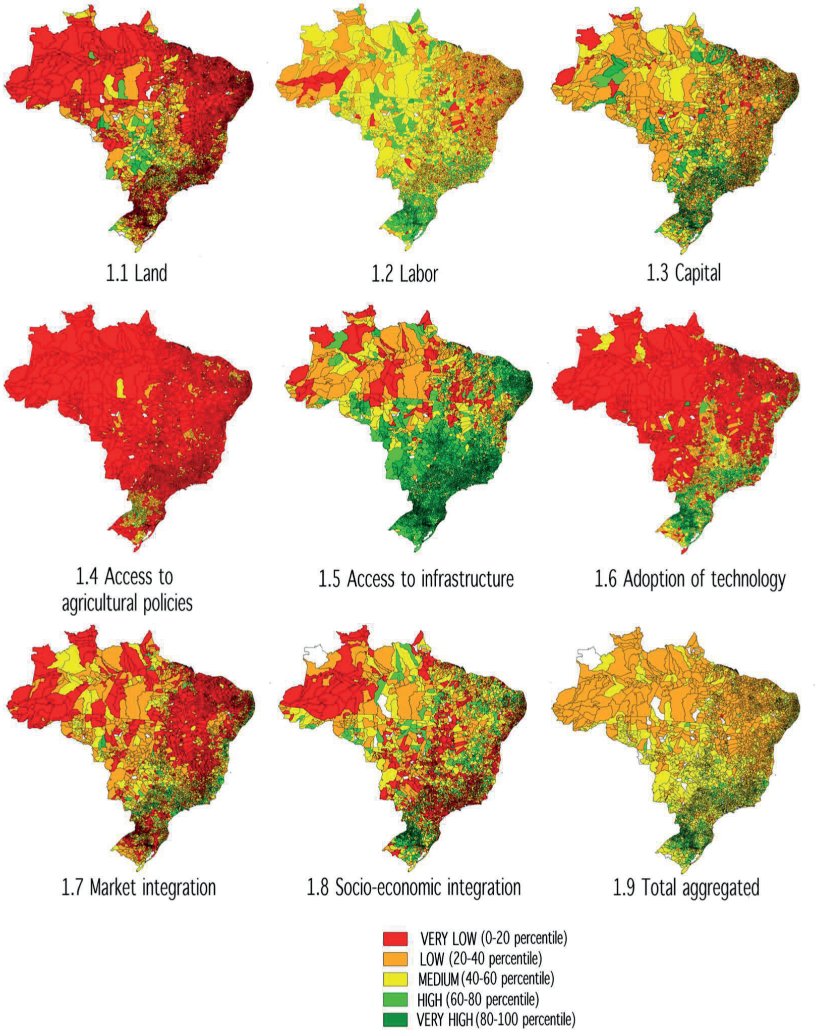
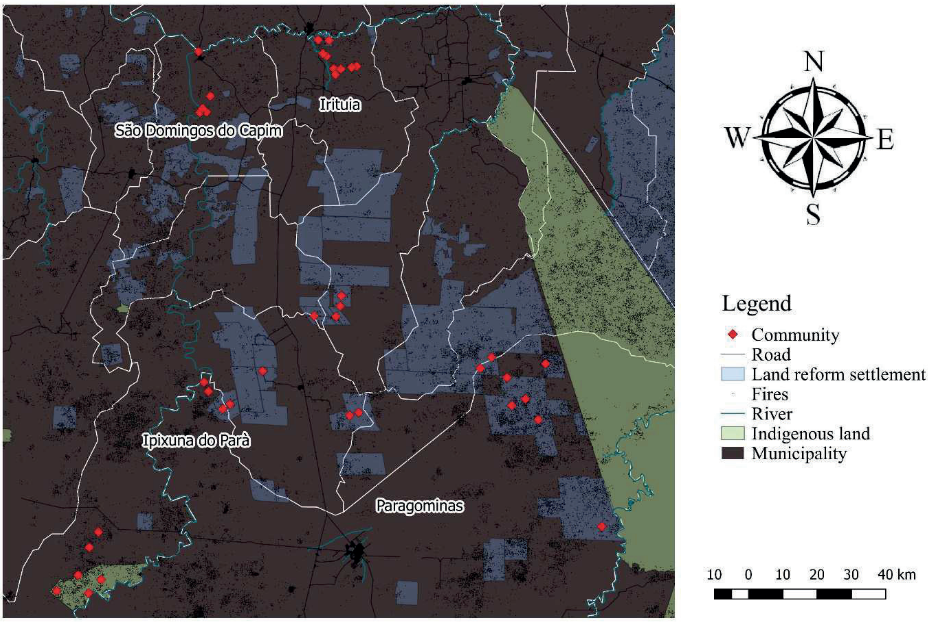



Figure 1. *Spatial description of the crucial development conditions per municipality in Brazil.*

### 4.2 Data and data collection

Three of the articles report on a framed field experiment. Data were collected in September-November 2015 in the four municipalities described above. 40 villages were chosen using snowball sampling, or previous connection with local leaders, trying to encompass a variety of old and newly formed villages, with different histories, forest cover, distance to the city and experience with fires. Figure 4 displays the location of the communities involved in the framed field experiments in relation to roads, rivers, urban centres and the distribution of fires during the months of fieldwork. The oldest communities are generally outside of land reform settlements and locate next to the rivers where the first colonization started. All communities from Ipixuna and São Domingos do Capim and two of the communities in Paragominas pertain to this category. Four communities in Ipixuna are located on indigenous land. Those four communities and the two located a few tens of km north were area of recent land conflict. The area pertained to a large farm that illegally extended over the indigenous reserve. About ten years ago the area was invaded by smallholders, and the invaders won the court trial. Local inhabitants reported that during the trial it was revealed that the indigenous family claiming ownership over the indigenous land was not actually indigenous, and also that the large landowner had no rights to occupy and deforest the area. The land colonized by smallholders is in course of regularization by the Brazilian Land Reform Agency (INCRA).

*Figure 4 Map of the communities involved in the Framed Field Experiment*



In each village, I conducted one or two experiments. The experiments were conducted on the same day to avoid spillover effects. I initially attempted to achieve a random sample, but this turned to be challenging. First, there was no complete list of inhabitants: the only lists identified were from local associations or cooperatives of their members, usually out of date.[14] I hoped to use farm boundaries available for land reform settlements and randomly draw from them. Alternatively, I tried to randomize an azimuth from a point in the village, and invite the farms on that azimuth. Unfortunately, both options turned unfeasible in practice. Property shapefiles are not available for newer or irregular settlements, especially outside land reform areas. Locating houses corresponding to the farms on the azimuth was also challenging because of the limited infrastructural network.

During the pilot phase, the research assistant and I tried to invite participants directly. Gaining the farmers' confidence in a short time and handing the invitation with reasonable confidence that the farmer would show up at the experiment turned to be infeasible. Relying on local leaders (of associations, cooperatives, religious groups or health officers) increased the show up rate.[15] To mitigate inviter bias we instructed local leaders to not select participants based on gender, age, affiliations to local institutions or their relational vicinity. About 20% of the participants systematically failed to show up. In this case, I personally recruited members from the households located close to the site of the experiment. This yielded a counterfactual sample to test a potential inviter bias. The second article of this thesis tests for non-random social ties and affiliations between invited and back-up participants and find no positive result. For each household we recruited one active member with influence over agricultural decisions. As expected, this yielded a gender biased sample, possibly reflecting the gender division of labour.

A last challenge during the fieldwork related to the perception of payments in the experiments by local researchers, who cannot pay participants and feared a negative externality on future participation in their research. The mediation of a respected researcher on both sides cleared the misunderstanding. While not considered an issue in my study population, such externality might be substantial in other populations with high research fatigue.

Survey data for the first article are collected by RAS in the municipalities of Santarem and Paragominas, randomly selecting households at the watershed level. The dataset also includes watershed level information of soil quality and georeferenced property boundaries (Gardner et al.,

[14] After completing the experiments, I realized that lists are available for each health unit, and associated to an health agent. These lists are not overlapping with villages, and might cover considerably large areas, without indicating the location of the domicile. Yet, random sampling could have been achieved drawing from the lists and asking the health agent to invite participants. However, some areas are not assigned a health agent (this was the case for some communities in the sample), and in many cases, lists are not updated (Morello, personal communication).

[15] Supports from local leaders did not grant universal trust from the participants. In one case, villagers that experienced land grabbing-related threats accused me of being there to steal their water. In another, I was associated with a usurer previously operating in the area, an unsolvable misunderstanding related to payments in the experiment. In yet another, a radical Pentecostal believer repeatedly and loudly accused me to be the devil. After repeated requests of explanations, I understood that in her eyes, because gambling is a sin, the administrator of a lottery on risk preferences could only be the devil.

2013). Data about slope and fires were elaborated in R and Qgis from a 30m STRM digital elevation model and hotspot-pixels data from INPE. Specific information on these data is provided in the first article.

# 5 Summary of the thesis, contributions, limitations and paths for future research

## 5.1 Summary of the thesis

In the context of the literature presented above, this section elaborates the key challenges, methods and findings of each paper. The first paper provides the foundation for the others, which assess the behavioural drivers of coordination and the *ex ante* impact of policies and droughts.

### 5.1.1 Article 1: Fire risk causes persistent poverty and fire use in the Brazilian Amazon *(co-authored with Rachel Garrett)*

This article aims to assess the role of fire risk externalities on Amazonian farmers' revenue, and whether fire use and control decisions are an issue of coordination. A household-game-theoretic model shows that there are strategic complementarities in fire use and control if the revenue elasticity to fire risk externalities is higher for non-fire users than for fire users. This is expected because non fire users have more assets at stake exposed for fire risk, and because they have a higher tolerance to fire losses: in an imperfect capital market, the opportunity cost of not investing in higher yield fire free techniques, is higher for capital abundant households.

We join georeferenced cross-sectional survey data collected by the RAS team in the municipalities of Paragominas and Santarem with spatial data about fires and farm slope. We build a measure of fire risk based on fire density in a buffer of each property. Estimating fire risk elasticities is challenging because fire use, fire risk and capital inputs are endogenous, and because fire risk is measured with error and there is potential spatial autocorrelation in revenue due to unobserved ecological and organizational factors. The last problem is ruled out with Moran's I tests. We use lagged variables for fire risk and fire use and generated instruments for fire use, fire risk and capital using the Lewbel (2012) method. Weak endogeneity persists even using lagged values of fire use and fire risk. Generated instruments are relevant, valid and strong.

We find that fire risk elasticities are higher for non-fire users than for fire users for total revenue and for farm revenue. However, for the latter, results are less robust to fire risk buffer definition and capital principal component specification. We also find that revenue elasticity to fire risk among non-fire users is higher than for any factor of production alone, such as capital, labour and land.

Because fire risk externalities are high, and higher for non-fire users than for fire users, the decisions to use and control fire configure as a coordination game with strategic complementarities. This suggests that policies mitigating fire risk should have a landscape approach, and that isolated farm level incentives might not be sufficient to incite a transition out of fire use.

### 5.1.2 Article 2: Amazonian farmers' response to fire policies and climate change *(co-authored with Arild Angelsen)*

This is the first of the three experimental articles included in this thesis. The experiment design is based on the model of fire use and control decisions outlined in the previous paper. In a repeated assurance game with risk, farmers are asked to prepare land with uncontrolled fire, controlled fire or with alternative (fire free) techniques. The first two options yield certain payoffs, with the first choice always strictly dominating the second. The third option returns up to double payoff of the first one, but declines down to zero depending on the number of participants choosing uncontrolled fire. Participants also face an exogenous, drought related, source of fire risk. If a drought occurs, participants who choose alternatives to fire use, face a less favourable payoff table. The game is repeated for ten rounds. At the end of each round, participants are informed about aggregate choice frequencies, drought occurrences and payoffs.

In a between design, half of the participants are assigned to a stable drought risk treatment, while the other half to an increasing risk treatment, miming increasing landscape flammability due to climate change. The hypotheses are that participants coordinate worse in high risk compared to low risk rounds, and that those facing an increasing risk (but on average, equal to the stable one) coordinate worse across rounds, because the expected benefit of coordination diminishes each round. Support for these hypotheses would hint that the negative impact of climate change in the Amazon does not only occur through fuel accumulation, but is also human mediated.

Policies are introduced in a within group design, after the fifth round. Participants allocated to payments for environmental services receive a higher payoff whenever choosing alternative fire free techniques. Participants allocated to the command and control treatment face a 30% risk of being caught and fined if they choose uncontrolled fire. This last treatment mimics the enforcement of current laws included in the Brazilian Forest Code. Payoffs are calibrated to reproduce actual yields from land use types and, on average, give equal material incentives across policies and drought risk treatments. Treatment impacts can thus only be attributable to framing effects (stable vs increasing risk, positive vs negative incentives, and interaction of the two) or risk aversion (to droughts and fines).

We find no impact of increasing drought risk *per se*, but significant impact of drought risk level on fire use and control, suggesting that the impact of climate change on Amazonians' fires is partly human mediated. We find that both policies perform better under increasing than under stable drought risk, hinting that farmers are more responsive to policies under intense drought years. Command and control scores better than payments for environmental services in mitigating fire risk and promoting the adoption of fire control measures. A higher drought risk reduces controlled fire and uptake of fire-free techniques. Policies offset droughts impact on controlled fires, but only command and control offset droughts impact on the adoption of fire-free techniques. These last two results might be due risk aversion to the fine, because command and control is directly affecting uncontrolled fire use choices, framing, and because command and control is aligned with social norms and an actual demand for justice. In a pre-experiment survey, we find that 43% of the farmers suffered at least one fire in the previous five years, but of these, only 2% were compensated for the damages. Also, half of the farmers stated that they would not be able to stop using fire, even if the law would forbid it. We conclude that law enforcement and payments for environmental services should be applied together.

#### 5.1.3 Article 3: How good norms lead to bad communication, miscoordination and fires.

In the same experiments as outlined for Article 2, I test for the impact of communication on coordination. In previous works, communication has been associated with community-based natural resource management, which in the Brazilian Amazon, has been widely applied to mitigate fire risk.

Testing the impact of communication on coordination for fire risk mitigation creates a theoretically interesting opportunity to explore the role of cheap talk on coordination in presence of a fire control norm. Fire control is prescribed by a social norm, which should be sufficient to achieve coordination; when it does not, the norm is weak (i.e., the norm is not strong enough to dictate choices). Communication of intentions is supposed to favour coordination, because messages are both self-signaling and self-committing.

In a level-k model, I show that communication of intents may be offset by a weak social norm if the latter breed a taboo about the proscribed choice. In other words, because agents have no incentives to declare a true preference that contradicts the norm, communication is no longer credible. We find that communication of requests (which is possible under our communication protocol), can improve coordination even in presence of a weak social norm. I find that this conjecture can explain the pattern emerging from the experimental data.

Community-based fire management in the Brazilian Amazon might have limited impact, because a weak social norm hampers trustful communication in favour of hypocritical communication. Yet, community based activities can improve coordination if they operate during drought years, when the norm does not provide sufficient assurance for coordination and when community leaders are able to motivate fulfilment of requests.

#### 5.1.4 Article 4: Behavioural predictors of accidental fires in the Brazilian Amazon: preferences and perceptions in coordination and predictors external validity

*(Co-authored with Øyvind Handberg)*

Most studies on coordination games address the role of game features and solution concepts. Few address the role of players' attributes in determining the efficient outcome. In this article, we test the impact of preferences, beliefs and perceptions on coordination, within and outside the experiment. Previously unexplored, we posit that social preferences positively affect coordination, because miscoordination is framed as a loss for participants choosing the efficient strategy. We analyse data from the five baseline rounds of the experiments as well as on stated choices about fire use and control. We show that our analysis also corresponds to a predictor validity test, a novel approach to external validity based on commonality of predictors between experiment behaviour and the out of the experiment counterfactual. We also establish the general conditions for a predictor validity test and use our analysis as an application.

We find that risk perceptions, but not risk aversion hampers coordination in and out of the experiment. Pro-social preferences improve coordination outside the experiment, but not within. Policies aiming to mitigate fire risk should target farmers' beliefs and perceived risk, and possibly account for pro-social preferences.

Overall, tests on these and other predictors suggest that, except for social preferences, the experiment is externally valid in regard to fire use choices. The test for fire control decisions is inconclusive because of potential bias in the stated behaviour counterfactual, and because no predictor fully satisfies all requirements for the test. We conclude that predictor validity is a stronger causal test for external validity, yet it is data intensive, which reduces its scope of application.

### 5.2 Limitations

The analysis presented in this thesis assumes some degree of market access, and feasibility of alternative land use types; assumptions that are mostly satisfied in the area of study. It also assumes some market orientation of the farmers, which I generally observed during the fieldwork. Any generalization to other areas and populations should be seen in light of these two crucial assumptions, e.g. findings are not likely to hold among traditional populations inhabiting very remote regions of the Amazon, or in regions where (lack of) market forces only allow a fire intensive use type.

The analysis is narrowly focused on short term fire use and control choices, and on strategic interaction. In the long run, more dimensions should be taken into account, such as the dynamic accumulation of wealth, which might constrain or lead to the capital investments needed for a transition out of fire use. Second, the underlying bio-economic drivers of clearing and management fires might differ across farms, and not all land is equally suitable for mechanized agriculture or perennial plantations. For instance, the transition out of fire use could be automatically induced by the exhaustion of fuel biomass, but this is impossible in a tightly forested landscape.

Because the thesis focuses on short term decisions and does not investigate specific land uses, I deemed it reasonable to ignore the long term factors outlined above. In other words, I assume that deforestation, and long run economic and bio-economic factors are in a "steady state". For this reason, findings would not generalize to a number of settings, such as active deforestation frontiers.

The analysis is also limited by caveats related to the experiment implementation, payoff calibration and treatments dose. With my current (*ex post*) knowledge of experiments design, I would convert the payoff table to simple rules that participants can learn by heart and introduce sessions with randomized drought risk levels. With slack time constraints, I would also have collected better data for preferences (also including betrayal aversion) and go back to the field to validate results in meetings with farmers. Farmers themselves bringing anecdotes and explanations for aggregate results from the survey and the experiment would be an important external validity test, and process-openness triangulation.

Natural continuations of the research done in this thesis include:

- extending the fire use and control choice framework to a dynamic analysis accounting for deforestation and capital accumulation, similar to (and extending) Mullan et al. (2017);
- explain burnt scars fire patterns including sensible variables about the underlying human micro-behaviour, similar to (and extending) Schwartz et al. (2015);
- assessing the role of institutions, social capital and other meso-level variables that extend beyond the group on coordination, similar to Agrawal (2001);
- exploring whether endowment heterogeneity, fairness and loss aversion are relevant factors for coordination. These could be induced with lotteries and production tasks prior to coordination decisions;
- running more coordination famed field experiments applied to actual coordination problems, for instance concerning the adoption of technologies or institutions, typically characterized by strategic complementarities.

More avenues for future research are also listed in the conclusion of the four articles.

# 6 Contributions and conclusions

The main contributions of this thesis are empirical and conceptual (Table 1) as well as some methodological (Table 2). The thesis synthesizes and brings about a new way of thinking about an understudied and compelling problem: the micro drivers underlying forest fires in the Brazilian Amazon. Persistence of fire use and insufficient investments in fire control are portrayed as the failure of coordination for fire-risk mitigation among smallholder farmers. The econometric analysis supports the conceptual model, which lends itself to experimental analyses reported in the last three articles of the thesis. The most noteworthy empirical contribution is the investigation of fire risk externalities, of the factors affecting farmers' fire use and control behaviour, and of the *ex ante* impact of policies and droughts.

I first show that fire risk externalities generate strategic complementarities in fire risk mitigation, and that their impact on revenue is worth more than capital, land or labour alone. Experimental results show that command and control outperforms payment for environmental services in mitigating fire risk, yet both are needed to ensure both efficiency and equity. There might be a human mediated impact of droughts on fire risk: as global warming increases drought risk and accidental fires, farmers might invest less in fire control, which is increasingly perceived as a potentially worthless investment, and also invest less in (flammable) alternative fire-free land use types.

*Table 1 Conceptual and empirical contributions*

| Topics | Findings and contributions | |
|---|---|---|
| Drivers of fire use and control, impact of fire risk externalities | Paper 1:<br>• There are strategic complementarities in fire use and control: fire-free technology adoption is an issue of coordination.<br>• Fire risk externalities affect revenue more than land, capital and labour alone.<br>• Fire risk mitigation policies have the potential to prevent $CO_2$ emissions, biodiversity losses, and bring development to the local people. | |
| Policies and drought impact on coordination for fire risk mitigation | Paper 2:<br>• There might be a human mediated impact of droughts on fire risk.<br>• Command and control (CAC) mitigate fire risk more than payments for environmental services (PES).<br>• CAC and PES should be implemented together to prevent welfare losses among the poorest. | Paper 3:<br>• Community based fire management – communication (COM) – is the least effective policy.<br>• A weak fire control norm explains persistent miscoordination in presence of communication of intents.<br>• Communication of requests might improve coordination under a weak norm when leaders are persuasive and drought risk is high. |
| Behavioural drivers of coordination | Paper 4:<br>• Risk perception but not risk aversion has a negative impact on coordination in the experiment and on stated behaviour.<br>• Pro-social preferences improve coordination on stated behaviour.<br>• The experiment is externally valid concerning fire use; test for external validity of fire control choices are inconclusive. | |

*Table 2 Methodological contributions*

<table>
<tr><th>Topics</th><th colspan="2">Findings and contributions</th></tr>
<tr><td>Household model, game theory, econometrics, generated instrumental variables, spatial analysis</td><td colspan="2">Paper 1:<br>• An analytical framework to understand fire risk externalities in the Brazilian Amazon<br>• Integrated analysis of fire hotspot pixels and household level data</td></tr>
<tr><td rowspan="2">Behavioural economics, bounded rationality, experimental economics</td><td>Paper 2:<br>• Ex ante experimental assessment of policies impact<br>• Development and application of a coordination experiment for natural resource management</td><td>Paper 3:<br>• A level-k model of communication and weak norms in assurance games<br>• An approximate experimental test of the level-k model</td></tr>
<tr><td colspan="2">Paper 4:<br>• Joint analysis of beliefs, risk and social preferences and of risk perception on coordination in and out of the experiment<br>• Predictor validity: a novel external validity test based on commonality of predictors between experiment and counterfactual measures of behaviour</td></tr>
</table>

Risk perception, but not risk aversion reduces coordination in the experiment and in survey measures of behaviour. Previously unexplored, pro-social preferences increase coordination in the survey but not in the experiment. Policies aiming to mitigate fire risk should target farmers' perceived risk. Climate alerts might reduce incentives for fire risk mitigation, yielding the opposite results compared with what were intended.

A major conceptual contribution in the thesis is the development of a level-k model to analyse communication of intents and requests in a multiplayer coordination game under weak social norms. I argue that weak social norms are pervasive in real-life coordination problems and I describe how these might lead to surprisingly persistent miscoordination. Improved communication among neighbouring farmers and community based fire management might not be enough to provide sufficient assurance to achieve coordination for fire risk mitigation. Local existing norms prescribe such behaviour, but imperfect compliance likely reduce credibility (self-signalling ability) of communication. Contrary to intuition, bringing more context (i.e., a weak norm) into coordination situations might hamper coordination.

Coordination games have often been perceived as artificial constructs of mere theoretical interest, because deemed of trivial solution in the richer context characterizing real life. I show that relevant real life coordination problems exist and might persist due to weak social norms. Coordination games synthesize a variety of actual issues, and yet are severely understudied in the field compared to, for instance, common pool and public good games. Analysing channels of beliefs formation and individual features in coordination games (for instance through weak norms, preferences and perceptions) is a first step to make coordination experiments relevant to describe actual problems. I hope that I have raised enough evidence to call for further research on the economic drivers of fires in the Brazilian Amazon, and to experiment more, possibly scaling up framed field experiments to field experiments analysing actual policies implementation.

The main methodological contribution of the thesis is on predictor (external) validity. To my knowledge, this approach is truly novel, and has the potential to become a complement to current behavioural validity tests. Assessing external validity is crucial to make economic experiments policy relevant.

The experimental findings of this thesis are bound by their laboratory-like setting, albeit being done with relevant subjects and a framing that resembles real-life decision-making. Drawing definitive conclusions related to policy is therefore premature. Despite this caveat, there are policy-relevant lessons to be learned. That fire risk externalities engender a coordination problem is a reason for concern, but that there is no social dilemma is good news.

Persistent miscoordination can be explained by weak social norms, lack of effective communication and access to justice. Previously ignored, we show that pro-social preferences might improve coordination.

Fires spreading over large areas threaten the benefits achieved through successful coordination in a neighbourhood or village. The distinction between neighbours' fires and *fires from afar* is a farmer heuristic that epitomizes how important risk perception is for successful coordination. If the scale of intervention is not large enough, policies mitigating fire risk are unlikely to achieve their goals, especially during drought years.

Access to alternative fire free techniques is limited, and the uptake, slow. Fire control is a crucial element in a transition to a fire free regime. Yet, providing the means for uptake of fire-free techniques is necessary, to mitigate fire risk and to ensure equity. When markets exist for inputs and outputs, a combination of payment for environmental services and law enforcement will contribute to achieving this goal.

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# Fire risk causes persistent poverty and fire use in the Brazilian Amazon

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**Abstract**

Sustainable development in the Brazilian Amazon is jeopardized by the persistence of low yield and environmentally degrading agricultural activities. We provide evidence that the uncontrolled use of fire might be a major factor explaining this pattern. Forest fires in the Brazilian Amazon cause large carbon emissions, biodiversity and welfare losses to the local populations. Most fires accidentally ignite from mismanaged swidden and pasture fires. Evidence of large damages suffered from local people hints that fire risk externalities are substantial and threaten investments in higher value and fire free techniques. In this paper, we show empirically that fire risk externalities are large compared to the return on other factors of production and trap farmers in a high fire risk and low revenue equilibrium. Fire risk externalities are lower for fire users than for non-fire users, discouraging investments in fire free agricultural technologies. Policies mitigating fire risk have the potential to achieve a triple win: to reduce greenhouse gas emissions, reduce environmental degradation and to bring economic development to local people.

**Keywords:** Brazilian Amazon, fires, strategic complementarities, household models, microeconometrics

**JEL code:** C21, C83, C70, Q15, Q54, Q56, Q58

# 1. Introduction

Tropical forests are critical to human well-being, yet disappearing faster than ever. Clearance and degradation of these forests areas for agriculture is often justified by their ability to generate foreign exchange and contribute to domestic food security and economic development. A majority of the people residing in forest regions remain impoverished because they are locked into environmental degrading and low-income land uses (Sunderlin et al., 2003). Nowhere is this challenge more apparent than the Brazilian Amazon, where millions of farmers engage in subsistence agriculture and extensive ranching for their livelihoods (Garrett et al., 2017; Valentim and Garrett, 2015).

Efforts aimed at ramping up environmental enforcement and harnessing market forces have succeeded in reducing deforestation among soybean and beef producers in the Amazon (Gibbs et al., 2016; Gibbs et al., 2015; Nepstad et al., 2014). Amidst this fall in absolute deforestation rates, however, forest degradation has continued to grow through the increased occurrence of fires (Alencar et al., 2015; Aragao and Shimabukuro, 2010; Malhi et al., 2008; Morton et al., 2013) causing large carbon emissions, biodiversity and welfare losses. Fires in the Brazilian Amazon release more $CO_2$ emissions than the whole Brazilian energy sector and reduce up to 40% of the potential carbon stock of standing forest (Anderson et al., 2015; Barlow et al., 2012; Berenguer et al., 2014).

Although fire spread is mostly associated with droughts (Alencar et al., 2015; Nepstad et al., 2004; Schwartz et al., 2015), there are no natural ignitions in the rainforest (Cochrane, 2003). All fires are human made and mainly related to agricultural activities (Cano-Crespo et al., 2015). Speculative arson fires are unlikely: when the forest is burned before being slashed, trees die while standing, and no agricultural activity is possible (Nepstad et al., 1999). Instead, fire use is primarily for agricultural purposes. Fires are ignited to clear vegetation, control pests, and fertilize soil. Fire use, reduces the amount of labour needed to achieve these ends, especially in the absence of mechanization, and the only major direct cost associated with fire use is clearing firebreaks around the area that is intended to be burnt and other fire control mechanisms (Bowman et al., 2008).

Nevertheless, the indirect costs of fire use can be substantial and widespread. Besides causing respiratory diseases (Diaz et al., 2002), fires can escape the intended area, burning crops, pastures, and farm structures (Bowman et al., 2008; de Mendonça et al., 2004). In a study of the Eastern Amazon, Cammelli and Angelsen (in this thesis) report that 43% of the 576 smallholder farmers experienced at least one accidental fire in the previous five years. Within a 30-year period, roughly 15%-60% of tree plantations in the Amazon region of Brazil, Bolivia, Peru and Ecuador are expected to experience losses from fire (Pokorny et al., 2012; Simmons et al., 2002).

Even farmers that spend time and resources to control their own fire on property can incur losses when they are exposed to fires started by their neighbours (Bowman et al., 2008; Cammelli, 2014; Nepstad et al., 2001). Theoretically, this situation could dilute incentives to spend time preventing fire risk on one's own property or investing in farm improvements necessary to adopt fire free techniques. Fire risk from neighbouring farms thus leads to a vicious cycle that traps farmers in poverty, and degrades the environment, perhaps explaining why efforts to tackling deforestation have not succeeded in reducing fire use and forest degradation (Barlow et al., 2012; Morello et al., 2017).

Understood in this light, the decisions to use and control fire configure as a coordination game with two equilibria: one with high fire risk and fire use, and little fire control investment, and another with little fire risk and fire use (Cammelli and Angelsen, in this thesis). Yet, these conjectures have not been tested empirically. Are fire risk and control decisions an issue of coordination? How large are fire risk externalities compared to the influence of other factors, such as capital endowments?

In this paper, we provide an analytical model linking household factor endowments with strategic interaction for fire use and control decisions. We hypothesize that there are strategic complementarities in fire use and control leading to a coordination game. This hypothesis is supported if losses from fire risk externalities are higher for non-fire users than for fire users. To examine losses from fire risk externalities among each group we estimate and then compare the elasticity of farm revenue to fire risk externalities for each group. Estimating the farm revenue elasticity to fire-risk and to production inputs will also lend insight into the overall cost of fires vis-à-vis other changes in management amongst rural households in Amazonia.

To accomplish this, we utilize a comprehensive social and environmental dataset from a cross sectional survey and transects assembled by the Sustainable Amazon Network between 2010-2011 in the Eastern Amazon (redeamazoniasustentavel.org; Gardner et al., 2013), as well as remotely sensed data on fire occurrence.

Pervasive simultaneity complicates the estimation of the revenue function. Because of imperfect capital and land markets, the choice of inputs, including fire, is simultaneous to revenue. Moreover, the farmer and the neighbours' fire use and control decisions are simultaneous: fire risk is also endogenous. We address the problem using lagged values of fire risk and fire use as well as instrumental variables generated from heteroskedasticity restrictions (Lewbel, 1997, 2012). We detect that endogeneity persists in the lagged variables and we correct it with valid and relevant generated instruments. Fire occurrences and their location are measured with measurement error related to cloud distortion and understory fires. We address this issue with instrumental variables and by defining fire risk over different buffer radius around the property.

We find that non-fire users earn more than fire users, but they also suffer higher losses from accidental fires. If fire risk would double, for instance because of a drought, non-fire users are expected to lose 59%-86% of their revenue against 0 to 25% of fire users. Difference in elasticities is barely significant for estimates on farm revenue only, but largely significant for estimates on the overall household revenue. This supports the hypothesis that fire use decisions exhibit coordination challenges between neighbours that create lock-in. We also find that the size of fire risk elasticity for non-fire users is larger than any other factors of production, such as labour capital or land, which suggests that spontaneous coordination out of fire use is unlikely. This finding is coherent with results from Garrett et al. (2017) that agricultural activities providing low income and high environmental damages are surprisingly persistent in the Brazilian Amazon.

Section 2 defines a conceptual model underlying the empirical model specification and section 3 discusses the identification strategy and estimation. Section 4 presents the data and variables definition, while section 5 presents the results and robustness tests. In section 6 and 7 we present the discussion and conclusion.

## 2. Analytical model

We build on a simplified household model of fire use and fire control choices inspired by Bowman et al. (2008), but also include endogenous fire risk formation, following Shafran (2008). We assume that farmers can produce from one or a combination of two alternative land use types: i) a low yield and fire intensive one (A), which gives a sure return even under exposure to fire, or ii) a high yield fire-free one (B) that is highly vulnerable to damages from fire exposure ($l$). While the fire-free land use type B can provide higher productivity and prices ($P^B > P^A$), it is substantially more exposed to accidental-fires losses. Fire risk depends on the neighbours' fire use and control choices. The profit function is supermodular, which leads to two Pareto ranked equilibria (Topkis, 1979; Vives, 1990). There is a fire risk threshold that farmers must overcome to achieve the higher profit fire-free solution. Otherwise, they will be trapped in a low profit, high fire risk and fire intensive technology.

Examples of land use types that yield higher revenues (via higher productivity and prices) include agroforestry, horticulture, tree plantations, intensive and rotated pastures, and annual crops with chemical inputs (Garrett et al., 2017; Hoch et al., 2009; Hoch et al., 2012; Nepstad et al., 2001; Pokorny et al., 2012). In contrast, lower value and fire intensive land use types, such as manioc production and extensive cattle ranching, likely face lower or no risk of losses from fires because there is no fuel left on swidden fields after the first burnt, and extensive pasture management requires less (flammable) fencing.

The $i^{th}$ farmer strives to control his fire by spending time $L_{ip}$ on fire control activities on a share $FC_i$ of the area burnt $S_{iA}$. However, expected losses from accidental fires $l(.)$, depend on both the own and the neighbours' uncontrolled fire, $S_{iA} - FC_i(L_{ip})$ and $\sum_{j \neq i}[S_{jA} - FC_j(L_{jp})]$. The resulting production functions are:

$$Q_i^A = Q_i^A\big(S_{iA}, K_{iA}, L_{iA}, FC_i(L_{ip})|\Omega_\mathrm{i}\big)$$

$$Q_i^B = Q_i^B(S_{iB}, K_{iB}, L_{iB}|\Omega_\mathrm{i})\left\{1 - l\left[S_{iA} - FC_i\big(L_{ip}\big) + \sum_{j \neq i}[S_{jA} - FC_j\big(L_{jp}\big)]\right]\right\}$$

Where $Q_p$ are the quantities produced, *S* is land, *K* is capital, *L* is labour and Ω are household and plot specific features.

In the short run farmers face a fixed supply of capital $\overline{K}_i$ and land $\bar{S}_i$, but can hire labour at a wage rate *W*.

$$\overline{K}_i = K_{iA} + K_{iB}$$

$$\bar{S}_i = S_{iA} + S_{iB}$$

We assume that labour and products are homogeneous: farmers equally value own and hired labour at a wage rate *W,* and the sale and purchasing prices of agricultural commodities are the same. When these conditions are met, the production and consumption decisions are separable (or sequential), and the household's production decisions can be portrayed as a profit maximizing problem (Singh et al., 1986). The profit function is:

$$\pi_i = P^A Q_i^A(S_{iA}, \overline{K} - K_{iB}, L_{iA} \,|\Omega_\mathrm{i}) + P^B Q_i^B(\bar{S} - S_{iA}, K_{iB}, L_{iB}|\Omega_\mathrm{i})[1 - l(.)] + I + W(\bar{L}_i - L_{iA} - L_{iB} - L_{iP})$$

Where *I* are government transfers (e.g. welfare payments), remittances and wages from off-farm labour.

$Q_i^A, Q_i^B$ and $FC_i$ exhibits diminishing returns to each factor. Further assuming that $Q_i^B$ is twice differentiable and convex in $l(Q_i^A)$, farmers face a convex production possibility frontier

$\frac{dQ_i^B}{dQ_i^A} < 0;\ \frac{d^2Q_i^B}{d^2Q_i^A} > 0$ with two corner solutions: full specialization in A or in B.[1] Notice that under full specialization in A, there is no incentive to invest in fire control because there are no B crops to protect from fire.

For this functional specification the game exhibits strategic complementarities in fire use and fire control: $\frac{d^2\pi_i}{dS_{iA}dS_{iB}} \geq 0$ and $\frac{d^2\pi_i}{dL_{iP}dL_{jP}} \geq 0$. The higher the neighbors fire use the higher the incentive for the own use of fire. The higher the neighbors fire control, the higher the benefit of own investments in fire control.

Strategic complementarities can also be stated as increasing differences in the profit function:

$$\pi_i(Q_{pi}^{B^*}; Q_{pj}^{B^*}) - \pi_i(Q_{pi}^{A^*}; Q_{pj}^{B^*}) \geq \pi_i(Q_{pi}^{B^*}; Q_{pj}^{A^*}) - \pi_i(Q_{pi}^{A^*}; Q_{pj}^{A^*})$$

where the quantities $Q_{pi}^*, Q_{pj}^*$ are produced under full specialization in A or B.

[1] In reality, farmers adopt largely diversified complex agricultural systems, encompassing a variety of products. Such systems allow hedging against risks, including fire, and may also reflect consumption preferences. This variety could be captured in a production function for a continuum of land use types varying in fire intensity, for which A and B are the corner, as we return to in the discussion. Within the current set-up, interior solutions could occur for risk averse and pro-social agents, or for agents valuing differently the own and the hired or purchased labor and produces. Yet, even including some preferences for diversification, there is still a monotonic negative impact of fire risk externalities tilting the production bundle away from capital-intensive (and fire-free) crops.

Rearranging, an increase in fire use by the neighbours reduces profits for B producers more than for A producers.

$$\pi_i(Q_{pi}^{B^*}; Q_{pj}^{B^*}) - \pi_i(Q_{pi}^{B^*}; Q_{pj}^{A^*}) \geq \pi_i(Q_{pi}^{A^*}; Q_{pj}^{B^*}) - \pi_i(Q_{pi}^{A^*}; Q_{pj}^{A^*})$$

For our specification of the profit function, the right hand side of the inequality is zero and the left hand side is positive, implying a zero and a negative revenue elasticity to fire use for A and B producers, respectively[2]. Strategic complementarities mean that the game is supermodular (Topkis, 1979; Vives, 1990) and exhibits multiple equilibria that can be Pareto ranked (Milgrom and Roberts, 1990): an assurance game.

Farmers in a given neighbourhood have an incentive to coordinate on A or B production together. Coordination to adopt fire-free land uses would lead to higher payouts for all, but without coordination, neither has incentive to adopt. Which of the two equilibria is chosen depends on relative prices, own and neighbours' fire use and control, and factor endowments.

Because fire free, capital-intensive land use type B generally leads to higher revenues, the farmer would likely choose to specialize in B if there was no fire risk. Yet, if the probability of fire losses $l$ is sufficiently high and given a convex production possibility frontier, the farmer fully specializes in the fire intensive good A.

Capital scarcity can also explain specialization in A. Here we assumed that capital markets are not available in the short term, given the fact that a lack of secure land tenure and high indebtedness are both common in the study region and the Amazon (cf. Barbier et al., 2016; Fearnside, 2001; Pereira et al., 2016). Thus capital is the main binding constraint in the household under production of B, since fire use is free, land is relatively abundant, due to fairly large farm sizes and imperfect enforcement of conservation requirements, and labour has a fixed price and is not constraining.[3] Labour can, however, only substitute capital to a limited extent. Thus, specializing in land use type B might never be optimal under a tight capital endowment $\overline{K}_i$ .

If the household is not able to borrow, there is a set of prices $P^B \geq P^A$ (and/or productivity differential) between A and B such that, for a given level of losses $l$, the value of production is still higher specializing in A, rather than in B.

$$P^A Q_i^{A*}(\overline{K}_i) > P^B Q_i^{B*}(\overline{K}_i)[1 - l(.)]$$

When fire losses $l$ rise, the benefit of producing B diminishes. There is a loss tipping point $\tilde{l}$ above which B is optimal, A otherwise. The existence of a tipping point explains why farmers may get trapped in an inefficient equilibrium characterized by high fire use. The tipping point depends on

[2] Intuitively, the sign of the elasticity is given by $\frac{d\pi_i}{dS_{jA}} = -P^B\, Q_i^B(.)\frac{dl}{dS_{jA}} < 0$ .

[3] In our study region farmers reported that land prices rose significantly, yet farms are large and land is not scarce compared to other factors. Similarly, there are substantial agricultural investments directed to the Amazon, but smallholder farmers often lack sufficient connection with the financial system to access them.

the endogenous risk of accidental fires, the fire risk from the neighbours as well as on factor endowments.

$$\tilde{l} = \frac{P^B Q_i^{B*}(\bar{K_\iota}) - P^A Q_i^{A*}(\bar{K_\iota})}{P^B Q_i^{B*}(\bar{K_\iota})}$$

Intuitively, the tipping point is higher for (relatively) capital abundant households, because the quantity produced under full specialization in land use type B is higher for them. This means also that capital abundant households have a higher tolerance to fire losses because they have higher opportunity cost of switching to A. Capital abundant households self-select in B land-use type. Conversely, for a marginal increase in the neighbours' component of fire risk, capital scarce households are more likely to pass the tipping point in the other direction, because their threshold $\tilde{l}$ is lower.

We expect higher losses among B than among A producers. This is the result of two effects: first, B producers have more to lose, and second, they are relatively capital abundant and thus tolerate higher losses before switching to A.

The main implication of the model can be summarized into three interrelated propositions:

> *P1: fire risk externalities are lower (at least not larger) for fire users than for non-fire users, therefore*
> *P2: fire use and control choices are issues of coordination, and*
> *P3: relative prices and productivity of land use types, input constraints (especially capital) and fire risk externalities determine the chosen equilibrium.*

By means of a household production function, we estimate the elasticity of fire risk for fire and non-fire users and test the first proposition.

## 3. Identification strategy, specification and estimation

We analyze revenue for profits, because the latter turns often to be negative, as we discuss in section 4.1. In order to estimate the revenue elasticity to fire risk externalities, we model household revenue as a function of the variables included in the profit function above.

Several challenges should be considered to achieve identification. First, the rigidity of land and capital markets make revenue and inputs use – including fire – simultaneous, violating the zero conditional mean assumption. Because fire risk is a function of the own fire use, fire risk is also endogenous. Second, as discussed in section 4.2 fire risk is measured with errors. Third, latent spatial patterns due to unobserved ecological factors, organizational or knowledge networks might bias estimates.

The endogeneity of fire use, fire risk and capital inputs arise because in the revenue function

$$y_i = b_0 + b_1 S_{ai} + b_2 \sum_{j \neq i} S_{aj}^q + b_3 S_{ai} \sum_{j \neq i} S_{aj}^q + b_4 S_{bi} \sum_{j \neq i} S_{aj}^q + b_5 K_i + \sum_k \gamma_k Z_k + \varepsilon_i$$

all terms except $Z$ are simultaneously determined. The first $(b_1 S_{ai})$ and second terms $(b_2 \sum_{j \neq i} S_{aj}^q)$ capture the impact of fire use and fire risk, difference in fire risk elasticities between groups $(b_4 S_{bi} \sum_{j \neq i} S_{aj}^q - b_3 S_{bj} \sum_{j \neq i} S_{aj}^q)$, if positive, indicate strategic complementarities in fire use and tests proposition 1. Turning to the controls, capital inputs is also endogenous because it is a function of wealth, which is in turn a cumulative function of revenue, and because it depends on the level of fire risk. Land availability is assumed to be fixed in the short run and therefore exogenous. Labour is mechanically exogenous: mostly measured through the availability of work in the household, and not the actual labour supply.

We used lagged values of fire use and fire risk, which are weakly exogenous to revenue. However, weak exogeneity might not be sufficient to achieve identification: if revenue is serially correlated, unobserved lagged revenue is part of the error term. Correlation between the error and the lagged values of fire and fire risk persists, causing endogeneity. This is the most pressing identification problem. We address and test it by means of instrumental variables. Instrumental variables are also expected to address the second problem, the attenuation bias originating from detection error of fire risk. We also address detection error of fire risk using a variety of definitions and robustness testing, as discussed in section 4.2.

Retrospective data about capital and labour inputs are not available. We use instruments for fire risk, fire use and capital inputs. Because we were not able to find exogenous and relevant instruments in the dataset, we generated instrumental variables from heteroskedasticity using the procedure from Lewbel (1997) and Lewbel (2012). Below goes the intuition.

Consider the simultaneous equation model composed by the equation of interest $Y_1 = \alpha_1 + \boldsymbol{\beta_1 X} + \gamma_1 Y_2 + e_1$, and the simultaneous $Y_2 = \alpha_2 + \boldsymbol{\beta_2 X} + \gamma_2 Y_1 + e_2$; assume heteroskedasticity in the second equation $cov(X, e_2^2) \neq 0$ , and exogeneity in the vector of variables **X**.

Lewbel suggests the use of $[\boldsymbol{X} - E(\boldsymbol{X})]\hat{e}_2$, or higher moments, as an instrument for $Y_2$. Such instrument is exogenous because the assumption $E(\boldsymbol{X}, e_1) = E(\boldsymbol{X}, e_2) = cov(\boldsymbol{X}, e_1 e_2) = 0$ guarantees that $[\boldsymbol{X} - E(\boldsymbol{X})]\hat{e}_2$ is uncorrelated with $e_1$. The instrument is also valid because the heteroskedasticity assumption attests that $[\boldsymbol{X} - E(\boldsymbol{X})]\hat{e}_2$ is correlated with $e_2$ and therefore with $Y_2$. These two properties of the instruments – and the related assumptions – can be checked with the standard Hansen J and rank tests, respectively. Under this approach we provide instruments for fire use, fire risk and capital and test their exogeneity by means of an heteroskedasticity robust C statistic (Baum et al., 2015).

We estimate the heteroskedasticity restrictions based on second moments of continuous exogenous plot and household specific features as well as on land features. These are exogenous because predetermined and fixed over time. The error $\hat{e}_2$ is estimated from the fitted values of simple OLS regressions on continuous variables, and from a linear probability model when the endogenous variable is binary. The linear probability model guarantees that residuals are heteroskedastic by definition[4], improving the likelihood of meeting the assumption of Lewbel (2012).

Elasticities for fire use and fire risk are estimated separately and for the interactions, therefore, instruments are generated separately for fire users and for fire risk among fire users and non-fire users. GMM estimation avoids the eventual forbidden regression problem arising from two stage least square inconsistency when carrying expectations and linear projections over non-linear functions (Wooldridge, 2010: p268). GMM also proved more efficient than two stage least square, yet, because it provides worst small-sample inference (Cameron and Trivedi, 2005: p187), two stage least square estimations are also provided.

Latent spatial patterns that are not captured by our measure of fire risk, or unobserved ecological factors or organizational and knowledge networks may create dependence among observations and confound estimates for the impact of fire risk. We test for conditional spatial independence in the residuals with a Moran's I test on the nearest neighbours, and as discussed in the next section, we conclude that unrealized spatial processes are not an issue.

The models are estimated in log-log form, robustness test for level-level and log-level are reported in appendix and tested against each other with a Ramsey reset test.

Following Battese (1997) and Klemick (2011), we deal with non-essential inputs by adding a dummy that takes value one when the input is not used, and substituting all zeros with ones in the input variable before log transformation. The dummy for no fire risk is also instrumented with generated instruments.

## 4. Data and study area

### 4.1. Survey data and study area

Survey data were collected by the Sustainable Amazon Network in 2010-2011 in the municipalities of Paragominas and Santarém, in the state of Pará, Brazil. Farms were randomly sampled within strata at the watershed level and asked retrospective questions about production in 2009. Watershed level information about soil quality were also collected through transects. The survey data on land use are coherent with census data within the same municipalities (Garrett et al., 2017). More details about the database are available in Gardner et al. (2013).

The database has a two level structure: plot and households. Some households owned more than one plot, while some others lived together on one plot. The first group, amounts to 32 observations and is discarded because identifying each plot's contribution to production was not possible. This in turn would have confounded the impact of fire risk externalities, which is a plot attribute. Households that displayed no farm activity (zero farm revenue) were also discarded from the

[4] Because residuals can only take values $1-\beta X$ or $-\beta X$ then: $Var(u|x) = \beta X(1-\beta X)^2 + (1-\beta X)(-\beta X)^2 = \beta X(1-\beta X) \neq 0$ for non-zero X and $\beta$.

analysis. When more than one household lived on the same plot, relevant variables were averaged or summed-up.

Because of large technological disparities between small and large landholders[5], the analysis is conducted only among family farmers, as defined by the Brazilian law (8.629/93): owning up to 4 fiscal lots. A fiscal lot in Paragominas corresponds to 55 hectares, while in Santarem it corresponds to 75 hectares[6].

### 4.2. Definition of fire risk and spatial variables

Fire as a source of risk operates in two ways: directly, through actual damages, and indirectly by reducing the expected benefits of investing in a fire-free technology. While the direct impact depends on actual fire occurrences, the indirect impact depends on risk perception, for which there is no direct data.

We measured fire risk as the density of fire occurrences in a buffer of the property between the beginning of June and the end of May (approximate beginning of the fire season) of each of the four years preceding 2009. We use the hotspot pixel maps elaborated by the Brazilian Spatial Agency (INPE) available at https://prodwww-queimadas.dgi.inpe.br/bdqueimadas.

This approach relies on the best data publicly available, but is subject to measurement error originating from cloud distortion, detection precision (+/- 1 to 6 km)[7], and from unobserved fire control investments. Lacking data about fire control, we are forced to assume that all fires equally contribute to fire risk, which is not true as fire risk depends on climatic conditions, landscape features as well as on the implementation of appropriate fire control measures. Moreover, risk perception is likely to be driven by risk exposure and previous experiences and might systematically deviate from objective risk (Slovic, 1987).

Measurement error from cloud distortion and detection precision are dealt with by considering a large time frame (4 years), an array of buffer definitions, from 1km to 5km, and instrumental variables. Unobserved fire control is unaddressed. Ideally, both measures of fire occurrences and of fire risk should be included in the model to capture the impact on revenue of both direct damages and of indirect effects through lower adoption of fire-free land use types. Fire-hotspot pixels are expected to proxy actual occurrences, and through these, also perceptions.

Farm slope is computed in Qgis from the 30m resolution STRM digital elevation model available at https://earthexplorer.usgs.gov .

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[5] In the study region, large landholders are abandoning fire use, and unlike smallholders, they own the means to perform effective fire-fighting. For them, the strategic interaction object of this paper is most likely irrelevant.

[6] http://www.incra.gov.br/sites/default/files/uploads/estrutura-fundiaria/regularizacao-fundiaria/indices-cadastrais/indices_basicos_2013_por_municipio.pdf Last accessed 5/12/17.

[7] http://www.inpe.br/queimadas/informacoes/perguntas-frequentes Last accessed 5/12/17.

### 4.3. Definition of revenue and other variables

The production functions are estimated on revenue rather than on profit, because the latter is often negative.[8] We analyse both farm revenue and total revenue. Although the latter is not directly affected by fires, farmers are likely making decisions taking into account all sources of income. Off-farm income includes wage income, remittances and government transfer (e.g. retirement pensions, Bolsa Escola and Bolsa Familia). When total revenue is the dependent variable, dummies for governmental transfers and remittances are introduced as controls.

Land area is measured as lot size. Quality of land is captured by water access, average travel time to the city, maximum farm slope and a soil quality principal component of acidity, silt and clay composition at the watershed level. Acidity, silt and clay together are indicator of soil quality in the tropical oxisol and ferrasols soils (Reed and Wood, 2016) characterizing our study area. I account for labour as the sum of the labour days provided by the household and by the hired workers on the property normalized for the productivity (as proxied by wage). Quality of labour is proxied with a dummy for technical assistance and the years of education received by the highest educated member of the household. Household head gender and age are also introduced to account for the household life cycle (Perz and Walker, 2002). Capital is measured by aggregating several items into a principal component. Capital measures include the count of all agricultural inputs and machinery, cattle stock and the kilometres of wire fences in the property. The capital principal component is not log-transformed, because it assumes negative values. The impact of fire risk on revenue is computed for non-fire users and fire users separately. Further heterogeneity or dynamics are not addressed. This is possibly the main limitation of the paper, but addressing it would require panel data, which is currently unavailable.

Instrumental variable regressions are benchmarked with simple OLS regression. Instruments are generated from farm size, travel time to city, farm slope, household size, soil quality, age of the household head and maximum education level attained in the household. Variables meet the Lewbel (2012) requirement of exogeneity[9] (while excludability from the second stage is not required). Whether sufficient heteroskedasticity is produced, is measured with standard tests for instruments relevance.

## 5. Results

### 5.1. Descriptive statistics

Descriptive statistics are displayed in Table 1and reveal the severity of fire risk in the region, with an average of 8 to 93 fires detected respectively in the 1 and 5 kilometre neighbourhood of each property in the previous 4 years. The large majority (77%) of farmers used fire for agriculture at

[8] This is coherent with the low-yield and environmentally degrading pattern observed by Garrett et al., (2017), and was also found in Rondonia (Caviglia-Harris, personal communication).

[9] Education can be a function of revenue. However, in our sample the correlation with farm and off-farm income are negligible or low (Pearson correlation coefficient = 0.0018, p=0.9744; Pearson correlation coefficient = 0.1214, p=0.0317). Farm size and distance to the market might be a consequence of farm revenue in the long term, but in the short term causality is most likely going from the formers to the latter.

least once in the previous 4 years. Compared to non-fire users, farmers using fire earn substantially less, use less capital (chemical inputs and mechanization, pasture fences, own less cattle and agricultural tools) and labour, and their land is farther from the market and in watersheds with poorer soil quality. Most importantly, they are substantially more exposed to fire risk.

Together these data support the assumptions of the model: farmers with better endowments self-select into land use type B, and they are less exposed to fire risk.

### 5.2. Estimation results

In this section, we present the estimated fire risk elasticities. We also compare their size to other factors of production and test whether they are equal or lower for fire users than for non-fire users. This corresponds to testing the inequality (but not strict inequality) defining strategic complementarities in fire use with a one side Wald test. Table 2 reports log-log estimate results for fire risk defined over a three km buffer around each property, and for both farm and total revenue.

The rank tests largely support relevance of instruments, and the Hansen J tests fail to detect correlation with the error term. The F-test is higher than ten for each of the instruments as well as for all instruments together. The C statistics testing endogeneity supports the IV model for farm revenue and the OLS model for total revenue. Although differences between TSLS and GMM estimates are not large, standard errors are smaller for the GMM estimator. The difference between OLS and IV estimates in the farm revenue model suggests that the use of lagged independent variables (fire use and fire risk) does not guarantee exogeneity, and that some endogeneity persists even in cross-sectional data using predetermined variables.

For both farm and total revenue, fire risk elasticities are large and significant for non-fire users, but not for fire users. Other factors preserve signs and significance across models, except for water access not explaining total revenue and fire use negatively affecting overall revenue. Fire risk externalities are even larger and more significant when turning to estimates for total revenue. As expected, for total revenue, land and land attributes display smaller coefficients, while life-cycle related household features change remarkably.

*Table 1 Summary statistics*

| | N | mean | sd | min | max | N | mean | sd | min | max | N | mean | sd | min | max | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Whole Sample | | | | | Fire users | | | | | Non fire users | | | | | Z-difference |
| Farm revenue 2009 (BRL) | 313 | 13,041 | 33,768 | 2.600 | 291,125 | 241 | 6,343 | 9,733 | 2.600 | 70,602 | 72 | 35,460 | 63,471 | 73 | 291,125 | -3.879 |
| Off-farm revenue 2009 (BRL) | 313 | 7,617 | 10,911 | 0 | 133,080 | 241 | 6,246 | 6,220 | 0 | 38,064 | 72 | 12,207 | 19,096 | 0 | 133,080 | -2.608 |
| Log- farm revenue | 313 | 8.168 | 1.626 | 0.956 | 12.58 | 241 | 8.008 | 1.367 | 0.956 | 11.16 | 72 | 8.704 | 2.218 | 4.290 | 12.58 | -2.523 |
| Fire user | 313 | 0.770 | 0.422 | 0 | 1 | | | | | | | | | | | |
| Fire density 05-08 (1km buffer) | 313 | 7.626 | 9.505 | 0 | 52 | 241 | 8.444 | 10.06 | 0 | 52 | 72 | 4.889 | 6.700 | 0 | 44 | 3.480 |
| Fire density 05-08 (2km buffer) | 313 | 21.85 | 26.76 | 0 | 129 | 241 | 24.00 | 28.21 | 0 | 127 | 72 | 14.63 | 19.66 | 1 | 129 | 3.182 |
| Fire density 05-08 (3km buffer) | 313 | 41.75 | 50.20 | 1 | 239 | 241 | 45.94 | 53.67 | 1 | 239 | 72 | 27.72 | 32.84 | 6 | 188 | 3.511 |
| Fire density 05-08 (4km buffer) | 313 | 66.47 | 80.01 | 1 | 361 | 241 | 72.71 | 85.82 | 1 | 361 | 72 | 45.58 | 51.56 | 13 | 295 | 3.303 |
| Fire density 05-08 (5km buffer) | 313 | 93.76 | 108.5 | 1 | 493 | 241 | 102.3 | 116.3 | 1 | 493 | 72 | 65.28 | 70.03 | 22 | 412 | 3.321 |
| Water access on farm | 313 | 0.604 | 0.490 | 0 | 1 | 241 | 0.602 | 0.491 | 0 | 1 | 72 | 0.611 | 0.491 | 0 | 1 | -0.136 |
| Technical assistance | 313 | 0.297 | 0.458 | 0 | 1 | 241 | 0.303 | 0.460 | 0 | 1 | 72 | 0.278 | 0.451 | 0 | 1 | 0.411 |
| Paragominas | 313 | 0.284 | 0.452 | 0 | 1 | 241 | 0.270 | 0.445 | 0 | 1 | 72 | 0.333 | 0.475 | 0 | 1 | -1.002 |
| Farm size | 313 | 46.625 | 49.159 | 1 | 300 | 241 | 44.545 | 44.969 | 1 | 300 | 72 | 53.585 | 60.99 | 1 | 283 | -1.167 |
| Travel distance to city | 313 | 1.96 | 1.178 | .083 | 5.5 | 241 | 2.175 | 1.137 | .0833 | 5.5 | 72 | 1.239 | 1.023 | .083 | 5 | 6.635 |
| Max education in household | 313 | 5.604 | 3.149 | 0 | 16 | 241 | 5.481 | 2.853 | 0 | 16 | 72 | 6.014 | 3.977 | 0 | 16 | -1.059 |
| Household size | 313 | 4.102 | 3.294 | 0 | 20 | 241 | 4.087 | 0.833 | 0 | 20 | 72 | 4.153 | 3.244 | 0 | 17 | -0.171 |
| Household head male | 313 | .837 | .352 | 0 | 1 | 241 | 0.833 | 0.36 | 0 | 1 | 72 | 0.851 | 0.324 | 0 | 1 | -0.403 |
| Household head age | 313 | 51.76 | 12.618 | 23 | 84 | 241 | 52.019 | 13.043 | 23 | 84 | 72 | 50.896 | 11.118 | 30.5 | 73.5 | 0.721 |
| Capital principal component | 313 | -0.004 | 2.766 | -2.706 | 18.76 | 241 | -0.663 | 1.710 | -2.706 | 9.397 | 72 | 2.202 | 4.163 | -2.142 | 18.76 | -5.698 |
| Cattle heads 2009 | 313 | 15.8 | 63.22 | 0 | 840 | 241 | 10.1 | 32.64 | 0 | 350 | 72 | 34.93 | 116.10 | 0 | 840 | -1.796 |
| Used tractor 2009 | 313 | .22 | .42 | 0 | 1 | 241 | .12 | .33 | 0 | 1 | 72 | .566 | .50 | 0 | 1 | -7.120 |
| Total labor days | 313 | 621.881 | 661.779 | 0 | 6031.55 | 241 | 550.359 | 536.665 | 0 | 3412.5 | 72 | 861.281 | 935.673 | 0 | 6031.55 | -2.691 |
| Maximum slope | 313 | 21.98 | 11.36 | 4.617 | 65.38 | 241 | 22.54 | 11.28 | 5.186 | 57.99 | 72 | 20.12 | 11.51 | 4.617 | 65.38 | 1.573 |
| Soil quality principal component | 313 | -0.212 | 0.897 | -1.993 | 7.236 | 241 | -0.359 | 0.602 | -1.993 | 0.758 | 72 | 0.283 | 1.410 | -1.586 | 7.236 | -3.762 |

Table 3 reports coefficient estimates for fire use and fire risk for different buffer definitions and for farm revenue and total revenue. All control variables are included, their coefficients conserve signs and significance across buffer definitions (full results for 3 to 5 km buffers are reported in appendix). The pattern of results is consistent for buffers larger than 3 km. For buffers of 1 and 2 km we introduced an intercept for farms experiencing no fires, needed after log transformation (Battese, 1997). For a buffer of 2 km, the dummy for no fire risk captures the whole impact of no fires detected around the property. The dummy captures only the effect for fire users, because all non-fire users experience at least one fire in the two km neighbourhood (Table 1). For a buffer of 1 and 2 km, there is no significant impact of fire risk on non-fire users in the farm revenue model, possibly because of the more severe measurement error of fire risk. We deem these estimates less reliable than the others and we do not discuss them further.

Between the 3 and 5 km buffers, the estimated farm revenue elasticity to fire risk ranges between -0.586 and -0.854. As expected, the estimated total revenue elasticities are lower because off-farm revenue is not directly affected by fires. If fire risk doubles, for instance because of a drought, the farm revenue of non-fire users would fall by 59% to 85%. The losses due to fire risk on farm revenue are higher than the single contribution of labour, capital and land. For instance, considering the 3 km farm revenue model and summary statistics for non-fire users, a one standard deviation increase in capital (a 189% increase at the mean) increases revenue by 42% $(.101 * 4.163)$, a one standard deviation increase of farm size ( a 114% increase at the mean) increases revenue by 24% $\left(0.215 * \frac{60.99}{53.58}\right)$, and an increase of one standard deviation of fire risk among non-fire users (about a 118% increase at the mean) reduces farm revenue by 69% $\left(-.586 * \frac{32.84}{27.72}\right)$, against 17% for fire users $\left(-.144 * \frac{50.2}{41.75}\right)$. Because of the large measurement error in fire detection, attenuation bias is likely even after IV correction, and these estimates can be considered as a lower bound.

We turn to testing the different impact of fire risk among fire and non-fire users. We always find significant differences for the total revenue models, but not always significant results for the farm revenue models. Considering only buffers larger or equal to 3 km, one side Wald test p-values range between 0.06 and 0.132 across buffer specifications. Two factors may explain this barely negative result. First, the dummy for fire use does not capture potentially relevant heterogeneity in fire control and risk exposure. Fire control and fire intensity of production – rather than discrete fire use – might be the key unobserved variables. Second, fire damages might relate more directly to risk exposure (invested capital inputs), rather than fire use (the result of a risk adapting behaviour). The capital principal component might capture part of risk exposure, and therefore part of the fire risk externality impact related to allocative inefficiency, as we robustness test below.

*Table 2 Estimate results for a 3km buffer on farm and total revenue*

| | OLS | IVgmm | IVtsls | OLS | IVgmm | IVtsls |
|---|---|---|---|---|---|---|
| | Farm revenue | | | Total revenue | | |
| Log-Fire density (non fire user) | -0.651 | -0.586* | -0.660 | -0.543*** | -0.722*** | -0.586*** |
| | (0.428) | (0.320) | (0.456) | (0.197) | (0.136) | (0.202) |
| Log-Fire density (fire user) | 0.0247 | -0.144 | -0.105 | -0.0131 | -0.0878 | -0.0960 |
| | (0.108) | (0.101) | (0.118) | (0.0779) | (0.0761) | (0.0884) |
| Fire user | -2.120 | -1.198 | -1.552 | -1.941*** | -2.192*** | -1.750** |
| | (1.383) | (0.995) | (1.470) | (0.674) | (0.428) | (0.719) |
| Capital (pc) | 0.0782 | 0.101*** | 0.0539 | 0.114*** | 0.100*** | 0.0983*** |
| | (0.0512) | (0.0344) | (0.0542) | (0.0287) | (0.0211) | (0.0293) |
| Log-Labour days | -0.00628 | -0.0524 | -0.00665 | -0.0355 | -0.0128 | -0.0399 |
| | (0.0663) | (0.0564) | (0.0631) | (0.0359) | (0.0312) | (0.0358) |
| No labour | -0.507 | -0.767** | -0.495 | -0.662*** | -0.579*** | -0.670*** |
| | (0.419) | (0.365) | (0.404) | (0.213) | (0.200) | (0.209) |
| Log-Farm size | 0.199** | 0.215*** | 0.246*** | 0.143** | 0.142*** | 0.171*** |
| | (0.0914) | (0.0815) | (0.0952) | (0.0570) | (0.0485) | (0.0562) |
| Education (years) | -0.0214 | 0.000425 | -0.0173 | 0.0135 | 0.0279* | 0.0153 |
| | (0.0325) | (0.0239) | (0.0315) | (0.0174) | (0.0149) | (0.0169) |
| Age household head | -0.0213*** | -0.0222*** | -0.0222*** | 0.0102** | 0.00984*** | 0.0101** |
| | (0.00652) | (0.00527) | (0.00636) | (0.00408) | (0.00381) | (0.00400) |
| Male household head | 0.579** | 0.482** | 0.593** | -0.0524 | -0.0724 | -0.0515 |
| | (0.273) | (0.245) | (0.264) | (0.140) | (0.124) | (0.137) |
| Household size | 0.0212 | 0.0174 | 0.0190 | 0.0368** | 0.0283** | 0.0363** |
| | (0.0239) | (0.0207) | (0.0232) | (0.0157) | (0.0129) | (0.0150) |
| Soil quality | 0.323*** | 0.384*** | 0.361*** | 0.115** | 0.0890*** | 0.134*** |
| | (0.0888) | (0.0690) | (0.0878) | (0.0519) | (0.0307) | (0.0519) |
| Slope | 0.0201* | 0.0220*** | 0.0199* | 0.00764 | 0.00618 | 0.00776 |
| | (0.0106) | (0.00841) | (0.0102) | (0.00610) | (0.00466) | (0.00593) |
| Log-Distance to the city | -0.281** | -0.275** | -0.332** | -0.256*** | -0.301*** | -0.271*** |
| | (0.136) | (0.110) | (0.136) | (0.0824) | (0.0574) | (0.0791) |
| Technical assistance | 0.0715 | 0.0735 | 0.0734 | 0.0721 | -0.0156 | 0.0734 |
| | (0.227) | (0.195) | (0.224) | (0.119) | (0.0987) | (0.117) |
| Water access on farm | 0.433** | 0.344** | 0.479*** | 0.0919 | 0.0729 | 0.119 |
| | (0.181) | (0.160) | (0.180) | (0.108) | (0.0946) | (0.106) |
| Paragominas | -0.235 | -0.0245 | -0.114 | 0.140 | 0.186 | 0.220 |
| | (0.254) | (0.227) | (0.252) | (0.162) | (0.144) | (0.164) |
| Receives government transfer | | | | 0.633*** | 0.541*** | 0.605*** |
| | | | | (0.155) | (0.127) | (0.149) |
| Receives remittances | | | | 0.322** | 0.225* | 0.295* |
| | | | | (0.156) | (0.133) | (0.153) |
| Constant | 9.858*** | 9.821*** | 9.603*** | 9.641*** | 10.19*** | 9.655*** |
| | (1.430) | (1.142) | (1.498) | (0.798) | (0.581) | (0.845) |
| Observations | 313 | 313 | 313 | 300 | 300 | 300 |
| R-squared | 0.312 | 0.298 | 0.307 | 0.476 | 0.460 | 0.472 |
| F-test | 0.0000 | 0.0000 | 0.0000 | 0 | 0 | 0 |
| Adjusted R2 | 0.273 | 0.258 | 0.267 | 0.440 | 0.424 | 0.437 |
| Hansen J | | 0.246 | 0.246 | | 0.173 | 0.173 |
| K-P rank test | | 2.19e-10 | 2.19e-10 | | 4.65e-09 | 4.65e-09 |
| F statistic | | 27.68 | 27.68 | | 23.14 | 23.14 |
| F statistic Fire risk fire non users | | 50.12 | 50.12 | | 44.60 | 44.60 |
| F statistic Fire risk fire users | | 47.02 | 47.02 | | 38.75 | 38.75 |
| F statistic Fire user | | 33.3 | 33.3 | | 28.46 | 28.46 |
| F statistic Capital pc | | 41.61 | 41.61 | | 46.94 | 46.94 |
| C statistic | | 0.00810 | 0.00810 | | 0.258 | 0.258 |
| Wald test between groups (p-value) | 0.1185 | 0.2301 | 0.2297 | 0.0082 | 0.0170 | 0.0170 |

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; first stage estimates are reported in appendix

*Table 3 Estimate results for farm revenue, total revenue and for different buffer definitions (all covariates are included in regressions, full results for 3 and 5 km buffers in Appendix).*

| | | OLS | IVgmm | IVtsls | OLS | IVgmm | IVtsls |
|---|---|---|---|---|---|---|---|
| | | Farm revenue | | | Total revenue | | |
| | Fire density (non fire user) | -0.0976 | 0.0287 | 0.00144 | -0.398*** | -0.566*** | -0.439** |
| | | (0.335) | (0.202) | (0.395) | (0.122) | (0.127) | (0.171) |
| | Fire density (fire user) | 0.126 | -0.0409 | 0.0662 | 0.0548 | -0.0729 | -0.0463 |
| | | (0.118) | (0.125) | (0.153) | (0.0824) | (0.0969) | (0.112) |
| | Fire user | -0.297 | 0.122 | 0.104 | -0.856*** | -0.856*** | -0.711** |
| | | (0.478) | (0.392) | (0.564) | (0.215) | (0.251) | (0.312) |
| | Fire density=0 | 0.304 | 0.530 | 0.465 | 0.122 | 0.00616 | 0.194 |
| 1 Km | | (0.400) | (0.362) | (0.431) | (0.262) | (0.284) | (0.310) |
| | C statistic (p-value) | | 0.263 | 0.263 | | 0.169 | 0.169 |
| | F statistic | | 25.55 | 25.55 | | 15.70 | 15.70 |
| | Hansen J | | 0.216 | 0.216 | | 0.113 | 0.113 |
| | Wald test difference (p-value) | 0.4989 | 0.7572 | 0.8746 | 0.0004 | 0.0002 | 0.0205 |
| | Reset test | 0.9438 | | | | | |
| | Adjusted R2 | 0.261 | 0.243 | 0.256 | 0.442 | 0.426 | 0.437 |
| | Fire density (non fire user) | -0.345 | -0.221 | -0.297 | -0.395*** | -0.519*** | -0.435*** |
| | | (0.313) | (0.203) | (0.334) | (0.141) | (0.123) | (0.162) |
| | Fire density (fire user) | 0.206* | 0.123 | 0.160 | 0.0390 | -0.0418 | -0.0635 |
| | | (0.112) | (0.112) | (0.146) | (0.0805) | (0.0872) | (0.110) |
| | Fire user | -1.357* | -0.771 | -0.969 | -1.330*** | -1.432*** | -1.124** |
| | | (0.815) | (0.596) | (0.872) | (0.401) | (0.362) | (0.541) |
| | Fire density=0 | 1.153* | 1.442*** | 1.502** | 0.486 | 0.190 | 0.440 |
| 2 Km | | (0.620) | (0.403) | (0.691) | (0.350) | (0.343) | (0.433) |
| | C statistic (p-value) | | 0.00822 | 0.00822 | | 28.60 | 28.60 |
| | F statistic | | 38.40 | 38.40 | | 0.445 | 0.445 |
| | Hansen J | | 0.374 | 0.374 | | 0.135 | 0.135 |
| | Wald test difference (p-value) | 0.0886 | 0.1279 | 0.2282 | 0.0031 | 0.0002 | 0.0344 |
| | Reset test | 0.8165 | | | | | |
| | Adjusted R2 | 0.274 | 0.263 | 0.270 | 0.438 | 0.426 | 0.434 |
| | Fire density (non fire user) | -0.651 | -0.586* | -0.660 | -0.543*** | -0.722*** | -0.586*** |
| | | (0.428) | (0.320) | (0.456) | (0.197) | (0.136) | (0.202) |
| | Fire density (fire user) | 0.0247 | -0.144 | -0.105 | -0.0131 | -0.0878 | -0.0960 |
| | | (0.108) | (0.101) | (0.118) | (0.0779) | (0.0761) | (0.0884) |
| | Fire user | -2.120 | -1.198 | -1.552 | -1.941*** | -2.192*** | -1.750** |
| | | (1.383) | (0.995) | (1.470) | (0.674) | (0.428) | (0.719) |
| | Fire density=0 | | | | | | |
| 3 Km | | | | | | | |
| | C statistic (p-value) | | 0.00810 | 0.00810 | | 0.258 | 0.258 |
| | F statistic | | 27.68 | 27.68 | | 23.14 | 23.14 |
| | Hansen J | | 0.246 | 0.246 | | 0.173 | 0.173 |
| | Wald test difference (p-value) | 0.1196 | 0.1641 | 0.2279 | 0.0082 | 0.0000 | 0.0170 |
| | Reset test | 0.7788 | | | | | |
| | Adjusted R2 | 0.273 | 0.258 | 0.267 | 0.440 | 0.424 | 0.437 |
| | Fire density (non fire user) | -0.727* | -0.643* | -0.722 | -0.596*** | -0.745*** | -0.642*** |
| | | (0.436) | (0.376) | (0.478) | (0.196) | (0.137) | (0.205) |
| | Fire density (fire user) | -0.0759 | -0.196** | -0.199* | -0.0559 | -0.127* | -0.136 |
| | | (0.103) | (0.0863) | (0.109) | (0.0744) | (0.0719) | (0.0837) |
| 4 Km | Fire user | -2.358 | -1.425 | -1.706 | -2.238*** | -2.428*** | -2.069** |
| | | (1.606) | (1.372) | (1.731) | (0.778) | (0.480) | (0.821) |
| | Fire density=0 | | | | | | |
| | C statistic (p-value) | | 0.0142 | 0.0142 | | 0.122 | 0.122 |
| | F statistic | | 25.98 | 25.98 | | 24.98 | 24.98 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | Hansen J | | 0.263 | 0.263 | | 0.303 | 0.303 |
| | Wald test difference (p-value) | 0.1355 | 0.2360 | 0.2722 | 0.0076 | 0.0000 | 0.0155 |
| | Reset test | 0.8354 | | | | | |
| | Adjusted R2 | 0.274 | 0.262 | 0.269 | 0.442 | 0.430 | 0.439 |
| | Fire density (non fire user) | -0.796* | -0.854** | -0.826 | -0.594*** | -0.771*** | -0.635*** |
| | | (0.460) | (0.396) | (0.511) | (0.213) | (0.157) | (0.223) |
| | Fire density (fire user) | -0.127 | -0.246*** | -0.242** | -0.0747 | -0.146** | -0.147* |
| | | (0.0988) | (0.0775) | (0.104) | (0.0718) | (0.0688) | (0.0811) |
| | Fire user | -2.652 | -2.180 | -2.132 | -2.346*** | -2.686*** | -2.192** |
| | | (1.811) | (1.566) | (1.968) | (0.897) | (0.593) | (0.941) |
| | Fire density=0 | | | | | | |
| 5 Km | | | | | | | |
| | C statistic (p-value) | | 0.0123 | 0.0123 | | 0.159 | 0.159 |
| | F statistic | | 27.96 | 27.96 | | 26.98 | 26.98 |
| | Hansen J | | 0.397 | 0.397 | | 0.299 | 0.299 |
| | Wald test difference (p-value) | 0.1391 | 0.1206 | 0.2411 | 0.0158 | 0.0000 | 0.0282 |
| | Reset test | 0.8101 | | | | | |
| | Adjusted R2 | 0.276 | 0.264 | 0.271 | 0.440 | 0.429 | 0.438 |

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all covariates from table 2 are included; first stage estimates for the 3-5km buffer are reported in appendix

### 5.3. Robustness tests

We found that fire risk externalities affect revenue more than any other input alone. We test robustness of this result by splitting the single capital principal component into two components: one that includes tractor, pesticide and fertilizer use and another that includes cattle-heads and km of wire fences. Measuring capital with higher precision is expected to reduce the impact of the fire user dummy and the impact of fire risk externalities for non-fire users whose effect is partially mediated by the (flammable) assets at stake. We find indeed that the fire user dummy coefficients for farm revenue decreases by half to one unit and that fire risk externalities are no longer significant for farm revenue on non-fire users in buffers smaller than 5 km. Results are attenuated, but still significant for total revenue. The mechanization principal component coefficient is large and always significant ranging from .164 to .253, but the one for cattle ranching is only significant for total revenue (results in Appendix). For a 5 km buffer definition, fire risk externalities on farm revenue are still significantly larger for non-fire users (one tailed Wald test p=0.0655). A one standard deviation increase in capital (mechanization) among non-fire users (about a 308% increase at the mean) increases revenue by 53% $(.198 * 2.674)$. A one standard deviation increase in fire, reduces their farm revenue by 98% $\left(-.825 * \frac{32.84}{27.72}\right)$. On average, fire risk externalities overweight mechanization investments for non-fire users.

Capital and labour are arguably endogenous controls because they could also be placed on the left side of the regression equations (Angrist and Pischke, 2008: p189). Their inclusion in the specifications complicate the interpretation of the estimate results, because part of the indirect impact of fire use and fire risk externalities on revenue might be captured by coefficients for capital and labor. A more parsimonious specification excluding these variables is estimated for a 3 to 5 km buffer and reported in appendix. Results do not change remarkably for non-fire users. The revenue elasticity for fire users increases in size and significance, possibly because the fire risk coefficient captures more of the allocative inefficiencies. One side Wald tests reveal significant differences in fire risk externality impacts among fire and non-fire users, except for the 3 and 4 km buffer for farm revenue. This result is similar to what found before and does not change the conclusions established above.

By means of a Ramsey reset test we compare level-level, log-log and log-level specifications across buffer definitions for the farm revenue model. Level-level is systematically rejected in support of log-level and log-log specifications (results in Appendix).

Table 4 displays Moran's I tests on the residuals of regressions on farm revenue for a 3 km buffer included in table 2, for a neighbourhood of 20, 25, 10 and 5 nearest neighbours. All tests reject spatial clustering. This suggests that there is no latent spatial pattern unaccounted for in the models. Given the design of the survey, all relevant fire risk, and ecological, organizational or knowledge network confounders are sufficiently taken into consideration.

*Table 4 Global Morans' I test for farm revenue (p-values)*

| Nearest neighbours | OLS | IV GMM |
|---|---|---|
| 5 | 0.6473 | 0.2871 |
| 10 | 0.8486 | 0.4733 |
| 15 | 0.4482 | 0.8984 |
| 20 | 0.6308 | 0.9114 |

Finally, we drop progressively all covariates, and find that estimate results for fire risk elasticities are reasonably robust to model mis-specification both for farm and total revenue models (results in appendix).

# 6. Discussion

## 6.1. *Fire risk externality creates a vicious cycle*

Fires in the Brazilian Amazon cause large $CO_2$ emissions and biodiversity losses (Alencar et al., 2006; Anderson et al., 2015; Barlow et al., 2012; Berenguer et al., 2014; Foley et al., 2007), as well as substantial damage to the local people (Cammelli and Angelsen, in this thesis; de Mendonça et al., 2004; Diaz et al., 2002; Hoch et al., 2009; Hoch et al., 2012; Morello, 2013; Nepstad et al., 1999). These damages might be higher for farmers that utilize more capital intensive and fire free systems, such as tree plantation and agroforestry (Hoch et al., 2009; Hoch et al., 2012; Pokorny et al., 2012; Tomich et al., 2002), slash and mulch fields covered with flammable woodchips (Denich et al., 2004; Kato et al., 1999), and intensive pastures (de Mendonça et al., 2004), even if those land uses would result in higher revenues (Bowman et al., 2008; Cammelli and Angelsen, in this thesis; Nepstad et al., 2001). If this hypothesis holds, then fire usage is a vicious cycle that helps to explain why few farmers in the Amazon choose to invest in capital intensive fire free systems: using fire reduces the profitability of investments in fire control and fire free techniques, which results in continued fire usage. This cycle cannot be broken unless coordination for fire risk mitigation is achieved. This vicious cycle, which is maintained by widespread capital constraints for smallholder farmers (Medina et al., 2015), may also help explain why poverty is still a widespread problem in the North of Brazil; in 2014 17.5% of households earned less than USD 2 per capita per day, and 5.3% earned less than USD 1.[1]

We tested this hypothesis by measuring the revenue elasticity to fire risk among fire and non-fire users. If the revenue elasticity to fire risk is higher for non-fire users than for fire users, it would suggest that mitigating fire risk is a coordination problem; farmers have a lower incentive to control fire and invest in alternative techniques when neighbours do not do the same. It would also explain why abundant fire usage with insufficient control persists in the Brazilian Amazon.

Our results confirmed that revenue elasticity to fire risk is indeed substantially higher for non-fire users than for fire users. We also found that the revenue elasticity to fire risk is higher than revenue elasticities to capital, labour and land stocks. This suggests that fire risk externalities might be one of the most important factors undermining investments in higher value, fire-free intensive production systems among smallholders in the region. We conclude that fire usage has created a poverty trap for many of the inhabitants of the Brazilian Amazon, one that is particularly difficult to overcome because it requires coordination between many individuals.

## 6.2. *Fire mitigation policies must consider the importance of fire risk externalities and enabling factors*

### 6.2.1. Mitigating fire risk requires a landscape approach

Given the insidious direct and indirect welfare losses caused by fire risk, reducing uncontrolled fire usage should be a critical policy goal for sustainable development in the Brazilian Amazon, which might help engender wide scale transitions to higher value land uses. Because transitions to higher value, fire-free land uses will require coordination, policies merely concentrated on technical feasibility, targeting fire usage and fire control only among individual farms and ignoring fire risk externalities, will likely fail to achieve their goals. Instead, policies should operate on a landscape level: mitigating fire risk on one farm must go hand in hand with alleviation of fire risk in the whole basin of fire contagion. This can be done by targeting contiguous neighbours, settlements or municipalities.

In a framed field experiment, Cammelli and Angelsen (in this thesis) and Cammelli (in this thesis) compare the ability to induce coordination for fire risk mitigation of individual payments for environmental services (PES), enforcement of a ban on uncontrolled fire (CAC) and community based forest management (COM) under varying drought risk conditions. Higher drought induced (exogenous) fire risk reduces coordination among neighbours, but both the introduction of CAC and PES improves it. Both policies equally increase the adoption of fire free techniques, but the latter of the two fails to mitigate fire risk, because it crowds out fire control investments among the fire users. Yet, the experiment fails to capture the heterogeneity in capital endowment, an important channel for PES impact. Targeting PES towards the poorest have the potential to provide sufficient assurance for coordination and creating the necessary condition for an uptake of fire free

[1] http://www.ipeadata.gov.br Last accessed 21/11/2017.

technologies. COM performed poorer than the other policy treatments, possibly because bad precedents in fire prone communities prevent trust formation. Yet, it was also found that training persuasive leaders have the potential to improve coordination. Non-pecuniary behavioural incentives based on peer observation (status) and social norms have shown large impacts for other coordination problems (Brekke et al., 2003; Nyborg et al., 2016). Because persistence of low yield and highly degrading practices are likely connected with preferences, cultural habits and social status (Garrett et al., 2017), campaigns promoting sustainable actions have the potential to substantially affect fire behaviour in the Brazilian Amazon.[2]

Alternatively, polices can devise explicit incentives for coordination such as PES at the landscape level, where a group of neighbouring farmers share responsibility for fire risk mitigation. This intervention increases the coordination premium, which is shown to ease coordination (Battalio et al., 2001) and is similar to the agglomeration bonus proposed by Parkhurst et al. (2002). Such a device has mainly been tested in lab experiments, displaying high coordination rate in groups of reasonable size (Banerjee et al., 2012), when coupled with communication (Alpizar et al., 2011; Banerjee et al., 2012; Parkhurst and Shogren, 2007; Parkhurst et al., 2002; Warziniack et al., 2007) or when substantial information is provided about the other participants (Banerjee, 2017). These findings suggest that landscape level PES coupled with enabling community-based interventions have the potential to favour coordination for fire risk mitigation. On the other hand, tying payments to collective outcomes when individual action is critical might be perceived as unfair. Fairness in turn is shown to reduce coordination (Drechsler, 2017). Future research should test the potential for agglomeration bonus payments in the field, among Brazilian smallholders.

In a simulation study, Morello et al. (2017) show that CAC effectiveness is limited by low enforcement capacity by local authorities especially due to measurement error in detection, and that mechanization subsidies or PES might reduce fires but also increase incentives for deforestation. Similarly, infrastructure development and regulations that favour market access and a price premium for fire-free products might also increase deforestation pressure, because profitability of converting forest to fields increases with market vicinity (Angelsen and Kaimowitz, 2001). A carrot and stick type of intervention is likely needed to ensure fairness, provide enabling conditions and to avoid deforestation leakages.

The timing and size of an intervention might also matter to achieve fire risk mitigation. The *Big Push* argument related to economic development (Rosenstein-Rodan, 1961; Sachs and Warner, 1999) states that a minimum level of incentives is needed to overcome coordination failures, which implies that one-time massive PES and CAC incentives are more likely to succeed than smaller step-by-step incentives. Moreover, because of the higher yields associated with fire free technologies, the outcome of a successful one-time intervention is likely stable over time.

[2] Importantly, such campaigns should meet fairness requirements and avoid stigmatization of fire use, see for instance Costa (2004).

Inevitably, interventions such as CAC and PES are likely to suffer from difficulties in defining responsibilities for fire events (Barlow et al., 2012) since during drought years fires can spread for kilometres across neighbourhoods and villages (Alencar et al., 2015). Farmers refer to these fires as "*fires from afar*" because of their unknown origin and because they have no control over them. Such exogenous fire risk hampers coordination locally, reduces incentives for fire control and reduces the scope for conditional payments at the neighbourhood level (Cammelli and Angelsen, in this thesis). Avoiding large-scale fire events by setting appropriate intervention zonings (which might not overlap with administrative units) is crucial for successful fire mitigation.

#### 6.2.2. There are synergies between fire mitigation and other conservation and social goals

Synergies are likely to exist between fire policies and other interventions targeting goals that are interlinked with fires, such as poverty reduction, agricultural development, forest restoration, deforestation and climate change. Addressing fire risk is a necessary condition to achieve these goals, and achieving these goals create incentives to mitigate fire risk. For instance, the 2012 reform of the Brazilian Forest Code requires investments in forest restoration (ranging from agroforestry to plantation of native species) that are flammable and less likely to break even in a short time-frame if fire risk is too high. Fire might jeopardize compliance with restoration norms. On the other hand, enforcing restoration at the landscape level might create sufficient incentives for single households to invest more in fire control and fire-free technologies, mitigating fire risk.

Another synergy arises by removing all implicit subsidies to extensive systems and fire intensive productions, such as extensive cattle ranching. Limiting credit policies that do not favour intensive systems likely reduces deforestation and contributes to fire risk mitigation. Cattle ranching is generally perceived as a less risky investment than crops - especially perennials - and receives dedicated credit lines. Since 2008, agricultural credit in the Brazilian Amazon is conditional on compliance with environmental regulation, however, such conditionality is much lower for smallholders (Assunção et al., 2013). Associating agricultural credit with conditionality related to controlled or no use of fire would create incentives for a transition to fire free land uses. Similarly, current implementation of the Rural Registry goes in the right direction. By strengthening property rights it increases the expected value of production, because it diminishes risks related to unclear tenure rights (Tomich et al., 2002).

### 6.3. *Limitations and future work*

Our analysis assumed that fire risk is objective, known *ex-ante* to the farmer, and that the related losses are also known. Subjective risk perceptions (Slovic, 1987) and subjective beliefs about the neighbours actions are likely affecting the farmer's choice. Cammelli and Handberg (in this thesis) analyse joint data from a survey and a coordination framed field experiment. They find that the likelihood of fire-free technology adoption is negatively influenced by risk perception but not by risk aversion, and that it is partly boosted by prosocial preferences. They also speculate that for a pro-social farmer perceiving high fire risk, producing with a fire intensive technology and investing in fire control might be optimal. In this case, the utility maximization problem yields an interior solution that is an equilibrium in the game: coordination for fire risk mitigation results mainly from fire control, rather than change in land use type. This pattern is indeed largely observed in the data. Fire control is a pragmatic fire risk mitigation strategy in the short term, and a necessary step for a transition out of fire use. Still, too little is known about fire control behaviour. In this paper we did not analyze fire control decisions because, as prescribed by social norms and by the law, stated choices are likely over-reported (Cammelli and Handberg, in this thesis; Carmenta, 2013). Burnt scars classification of accidental and intended fires (e.g. Cano-Crespo et al., 2015) are direct observations of behavior rather than stated measures. Yet, these data are rarely produced and analyzed.

Some land uses might reside in between the two extreme land use types considered so far. Horticulture, for instance, requires some sunk costs and capital for irrigation, pesticides and fertilizers, but is likely unaffected by accidental fires. Moreover, compared to other systems, horticulture provides more substitutability between capital and labour: cash constrained households can irrigate and weed manually and produce fertilizer on farm. Land use types that do not increase fire risk exposure and that offer substitutability between capital and labour potentially pave the way for a transition to fire-free systems. The Bragantine system, which involves intercropping and rotating corn, beans, and manioc to improve soil health (Cravo et al., 2005) also seems to offer good substitutability between capital and labour and limited flammability. More research is needed to assess the adoption potential of these systems as a transition to fire-free production. For instance, whether fire risk harms specific land uses more than others.

Our analyses only concern the short run. Panel data would allow unravelling long-term dynamics related to slack land constraints and capital accumulation (similar to Mullan et al., 2017). How is coordination for fire risk mitigation affected by wealth accumulation and vegetation dynamics? Is there a natural way out of fire as farmers get richer, or as forest shrinks? Do fire risk externalities affect land acquisition, abandon or migration?

Finally, our results might be affected by a high incidence of fires in our two study regions. New studies could assess the replicability of our findings to other parts of the Amazon or in wet tropics at other continents with different landscape and climatic features affecting fire occurrences and propagation. Many contextual factors should be taken into account while replicating the analysis. Fire in Borneo and Sumatra is still used to claim landownership or can be associated with tenure conflicts (Dennis et al., 2005; Tacconi, 2003; Tomich et al., 2002), therefore fires in standing forests might not be only accidental. In the Brazilian Amazon this practice seems to belong to the past (Barreto and Araujo, 2012; Barreto et al., 2008). Fires in other biomes (e.g. savannas, Mediterranean vegetation or boreal forests) are naturally occurring after lightning strikes flammable vegetation. In these settings, farmers face a truly exogenous fire ignition risk, and strategic complementarities in fire free technology adoption are less important. Except for the Amazon, Andela et al. (2017) find a worldwide decreasing trend of fires, mostly associated with higher GDP and previous land conversion to agriculture. Fire management outside the Amazon might still have features of strategic interaction but mostly related to fire defensible space (Shafran, 2008) and fire-fighting (Orszag and Stiglitz, 2002). The impact of fire risk externalities on farm and homeowner decisions can be substantial. However, the policy implications would be entirely different than in the Amazon.

## 7. Conclusion

Deforestation, forest degradation and persistent poverty remain pressing challenges in the Brazilian Amazon (Alencar et al., 2015; Aragão et al., 2016; Aragao and Shimabukuro, 2010; De Faria et al., 2017; Medina et al., 2015). These challenges derive from the fact that many farmers in the region are locked into a pattern of low yield and highly-degrading agricultural practices (Garrett et al., 2017). Numerous factors contribute to the persistence of these types of land use practices, including historical legacies, political instability, market failures and cultural lock-in among others. Our results indicate that fire risk is also an important factor, because it traps farmers into choosing low-capital and fire intensive strategies – such as extensive cattle ranching and swidden crops– to avoid major revenue losses when fires occur. Overcoming the fire poverty trap to move toward higher value land uses – such as agroforestry, mechanized agriculture or intensive cattle ranching – is extremely challenging because it requires neighboring farmers to reduce their fire usage simultaneously.

Potential solutions to the problem of persistent uncontrolled fire usage include enforcing fire control and subsidizing the uptake of fire free agricultural systems that require little sunk costs and have good substitutability between capital and labor (e.g., horticulture and mixed cropping). Programs to increase access to loans for machinery or improve community programs for machinery lending may also be a successful mechanism to reduce fire risk. On the other hand, both of these policy interventions could result in greater incentives to clear land so they would need to be coupled with stringent conservation and zoning policies to avoid land clearing. To overcome fire risk coordination thresholds these policy interventions must target whole communities, not individual farms, and eventually concentrate incentives in a larger upfront payment (*Big Push*). Group contracts for neighbors accessing PES, transfers, credit and technical assistance would also raise the premium of coordinating for fire risk mitigation (similar to agglomeration bonus).

Though large and coordinated investments toward fire risk mitigation may seem costly and daunting due to their scope and scale, their benefits are likely to greatly outweigh their costs due to synergies between fire control and other development and conservation objectives. Fire mitigation, by freeing people of the vicious cycle of fire risk and low investment, has the potential to achieve a triple win by simultaneously reducing carbon emissions, forest degradation and improving the incomes of local people.

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# 9. Appendix

Table 5 reports the first stage results for the full farm revenue specifications and for 3 to 5 km buffers. Tables 6 and 7 report estimates results for level-level and log-level specifications. Tables 8 and 9 reports results for specification with and without the endogenous controls (capital and labour). Table 10 to 13 report estimates results for a variety of specifications progressively excluding covariates. The other tables report on the robustness test for principal component capital assets.

*Table 5 First stage estimates for the full farm revenue specification and for 3 to 5 km buffers (v are generated instruments)*

| | Capital (pc) | FU | Log fire density NFU 3km | Log fire density FU 3km | Log fire density NFU 4km | Log fire density FU 4km | Log fire density NFU 5km | Log fire density FU 5km |
|---|---|---|---|---|---|---|---|---|
| v1b | 0.0826 | 0.00301 | 0.0238 | 0.0493 | 0.0243 | -0.0294 | 0.0842 | -0.0845 |
| | (0.183) | (0.0362) | (0.0960) | (0.0985) | (0.131) | (0.136) | (0.149) | (0.149) |
| v2b | 0.146 | 0.0355 | 0.117 | -0.0145 | -0.00905 | 0.0712 | -0.0868 | 0.120 |
| | (0.206) | (0.0370) | (0.0885) | (0.109) | (0.125) | (0.164) | (0.178) | (0.230) |
| v3b | -0.000503 | -7.46e-05 | 0.00218*** | -0.000631 | 0.00225** | -0.000303 | 0.00212** | -0.000202 |
| | (0.00142) | (0.000271) | (0.000754) | (0.000793) | (0.000939) | (0.00101) | (0.00108) | (0.00111) |
| v4b | 0.000116 | 7.42e-05 | 0.000178 | 8.37e-05 | 0.000233 | 0.000148 | 0.000455 | 2.14e-05 |
| | (0.000499) | (0.000156) | (0.000451) | (0.000578) | (0.000541) | (0.000670) | (0.000614) | (0.000750) |
| v5b | -0.000230 | -0.0124** | 0.0674*** | -0.0447*** | 0.0692*** | -0.0477** | 0.0629*** | -0.0457** |
| | (0.0192) | (0.00490) | (0.0108) | (0.0130) | (0.0171) | (0.0193) | (0.0198) | (0.0222) |
| v6b | -0.400 | -0.139*** | 0.0110 | 0.00423 | 0.570*** | -0.371* | 0.768*** | -0.594** |
| | (0.268) | (0.0435) | (0.0272) | (0.0312) | (0.154) | (0.205) | (0.213) | (0.272) |
| v7b | 0.00859 | -0.00294 | 0.0185 | -0.0120 | 0.0172 | -0.00898 | 0.0147 | -0.00452 |
| | (0.0157) | (0.00312) | (0.0134) | (0.0134) | (0.0130) | (0.0121) | (0.0115) | (0.0109) |
| v1e | 0.0112 | 0.0101* | -0.0333* | 0.156*** | -0.0382* | 0.158*** | -0.0271 | 0.151*** |
| | (0.0340) | (0.00568) | (0.0189) | (0.0295) | (0.0197) | (0.0301) | (0.0195) | (0.0294) |
| v2e | 0.412*** | 0.000294 | 0.0685 | 0.163 | -0.0194 | 0.267* | -0.0390 | 0.314** |
| | (0.130) | (0.0226) | (0.0783) | (0.104) | (0.0792) | (0.137) | (0.0829) | (0.146) |
| v3e | 0.000197 | 4.75e-06 | -5.70e-05 | 0.00116*** | -1.45e-05 | 0.00105*** | -1.77e-05 | 0.00106*** |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (0.000310) | (5.25e-05) | (0.000173) | (0.000264) | (0.000196) | (0.000281) | (0.000224) | (0.000302) |
| v4e | 0.000201 | -3.01e-05 | 0.000197* | 0.000405** | 0.000118 | 0.000420** | 0.000129 | 0.000424** |
| | (0.000216) | (3.66e-05) | (0.000119) | (0.000158) | (0.000128) | (0.000170) | (0.000136) | (0.000176) |
| v5e | -0.00987 | 0.000816 | -0.00458 | 0.0155*** | -0.00289 | 0.0136*** | -0.000838 | 0.00908** |
| | (0.00650) | (0.000864) | (0.00303) | (0.00543) | (0.00303) | (0.00496) | (0.00297) | (0.00444) |
| v6e | -0.0525 | -0.0214*** | 0.0720** | -0.0606 | 0.0860*** | -0.0717** | 0.0509* | -0.0472 |
| | (0.0516) | (0.00812) | (0.0314) | (0.0389) | (0.0284) | (0.0350) | (0.0267) | (0.0301) |
| v7e | -0.00210 | -0.000174 | 0.000815 | 0.00571 | 0.000696 | 0.00528 | 0.00113 | 0.00518 |
| | (0.00279) | (0.000846) | (0.00281) | (0.00411) | (0.00306) | (0.00396) | (0.00289) | (0.00401) |
| v1c | 0.187 | 0.0354 | 0.0134 | -0.0905 | 0.0132 | -0.375 | 0.189 | -0.594 |
| | (0.662) | (0.129) | (0.285) | (0.304) | (0.455) | (0.487) | (0.556) | (0.571) |
| v2c | -1.240 | 0.248 | -0.192 | -0.184 | -0.389 | -0.252 | -0.728 | -0.226 |
| | (0.916) | (0.173) | (0.352) | (0.409) | (0.582) | (0.705) | (0.776) | (0.951) |
| v3c | -0.00196 | 0.00133 | 0.00179 | -0.000470 | 0.00226 | 0.000930 | 0.00210 | 0.00127 |
| | (0.00529) | (0.00108) | (0.00258) | (0.00289) | (0.00371) | (0.00410) | (0.00462) | (0.00489) |
| v4c | -0.00113 | 0.000900 | -0.00226 | 0.00107 | -0.00157 | 0.00110 | -0.000960 | 0.000726 |
| | (0.00213) | (0.000659) | (0.00162) | (0.00210) | (0.00226) | (0.00285) | (0.00280) | (0.00344) |
| v5c | 0.0506 | -0.0300* | 0.157*** | -0.126*** | 0.187*** | -0.154** | 0.184** | -0.153* |
| | (0.0660) | (0.0169) | (0.0290) | (0.0361) | (0.0586) | (0.0654) | (0.0768) | (0.0846) |
| v6c | -1.159 | -0.260* | -0.605*** | 0.573** | 1.113** | -0.542 | 2.143** | -1.514 |
| | (0.993) | (0.155) | (0.202) | (0.237) | (0.537) | (0.700) | (0.871) | (1.106) |
| v7c | 0.0466 | -0.00337 | 0.0347 | -0.0406 | 0.0360 | -0.0341 | 0.0298 | -0.0201 |
| | (0.0576) | (0.0112) | (0.0392) | (0.0403) | (0.0448) | (0.0427) | (0.0445) | (0.0444) |
| v1d | 0.100*** | -0.00525** | 0.0113 | -0.00619 | 0.0166** | -0.00820 | 0.0207** | -0.0107 |
| | (0.0194) | (0.00238) | (0.00751) | (0.00877) | (0.00816) | (0.00946) | (0.00929) | (0.0108) |
| v2d | 0.106*** | -0.00591 | 0.00680 | -0.00253 | 0.0186 | -0.00973 | 0.0334 | -0.0197 |
| | (0.0336) | (0.00631) | (0.0176) | (0.0215) | (0.0212) | (0.0253) | (0.0239) | (0.0291) |
| v3d | 0.00133*** | 5.10e-05** | -0.000156** | 0.000147 | -0.000179** | 0.000167* | -0.000175* | 0.000157 |
| | (0.000177) | (2.45e-05) | (7.69e-05) | (9.11e-05) | (8.65e-05) | (9.82e-05) | (9.34e-05) | (0.000104) |
| v4d | 0.000755*** | -3.02e-05 | 7.59e-05 | -9.24e-05 | 0.000110 | -0.000123 | 0.000145 | -0.000144 |
| | (0.000183) | (2.15e-05) | (7.35e-05) | (7.29e-05) | (7.68e-05) | (8.06e-05) | (9.08e-05) | (9.57e-05) |
| v5d | 0.0132*** | 0.00106** | -0.00314*** | 0.00317** | -0.00381** | 0.00353* | -0.00433** | 0.00411* |
| | (0.00341) | (0.000504) | (0.00107) | (0.00137) | (0.00176) | (0.00207) | (0.00193) | (0.00216) |
| v6d | 0.0223 | -0.000936 | 0.0323* | -0.0249 | 0.00460 | -0.00897 | -0.0269 | 0.0156 |
| | (0.0365) | (0.00512) | (0.0167) | (0.0200) | (0.0175) | (0.0205) | (0.0255) | (0.0319) |
| v7d | 0.00188 | 0.000544** | -0.00160* | 0.00161* | -0.00194* | 0.00190* | -0.00231** | 0.00209* |
| | (0.00248) | (0.000273) | (0.000882) | (0.000947) | (0.00102) | (0.00106) | (0.00110) | (0.00113) |
| Log-Labour days | 0.0678 | -0.0146 | 0.0369 | -0.0783** | 0.0496 | -0.0875** | 0.0562 | -0.0948** |
| | (0.0430) | (0.00997) | (0.0301) | (0.0375) | (0.0344) | (0.0423) | (0.0392) | (0.0465) |
| No labour | 0.0921 | -0.0465 | 0.0669 | -0.149 | 0.138 | -0.200 | 0.158 | -0.224 |
| | (0.265) | (0.0615) | (0.189) | (0.235) | (0.214) | (0.263) | (0.239) | (0.284) |
| Log-Farm size | 1.244*** | -0.0454*** | 0.139*** | 0.0472 | 0.160*** | 0.0481 | 0.205*** | 0.0259 |
| | (0.0674) | (0.0105) | (0.0338) | (0.0418) | (0.0369) | (0.0441) | (0.0379) | (0.0446) |
| Age household head | -0.00457 | 0.00123 | -0.00460* | -0.00261 | -0.00458 | -0.00303 | -0.00561* | -0.00199 |
| | (0.00408) | (0.000804) | (0.00259) | (0.00314) | (0.00289) | (0.00335) | (0.00313) | (0.00356) |
| Male household head | -0.0227 | 0.0177 | -0.0244 | -0.00149 | -0.0524 | 0.0188 | -0.0682 | 0.0191 |
| | (0.157) | (0.0394) | (0.114) | (0.142) | (0.141) | (0.157) | (0.157) | (0.173) |
| Household size | -0.00177 | 0.00888*** | -0.0164 | -0.00327 | -0.0323*** | 0.00560 | -0.0453*** | 0.0134 |
| | (0.0171) | (0.00341) | (0.0105) | (0.0151) | (0.0115) | (0.0150) | (0.0130) | (0.0157) |
| Education (years) | 0.128*** | -0.00571* | 0.0116 | -0.0464*** | 0.0187 | -0.0498*** | 0.0236* | -0.0502*** |
| | (0.0186) | (0.00339) | (0.0111) | (0.0139) | (0.0122) | (0.0149) | (0.0130) | (0.0157) |
| Soil quality | 0.229** | -0.0399* | 0.0855 | -0.100 | 0.107 | -0.0994 | 0.139* | -0.0735 |
| | (0.0952) | (0.0209) | (0.0668) | (0.0823) | (0.0730) | (0.0905) | (0.0802) | (0.0963) |
| Slope | -0.0290*** | 0.00221** | -0.00772** | 0.0115*** | -0.00690* | 0.0104** | -0.00864* | 0.0115** |
| | (0.00627) | (0.00111) | (0.00362) | (0.00445) | (0.00405) | (0.00479) | (0.00448) | (0.00520) |
| Log-Distance to the city | -1.372*** | 0.205*** | -0.521*** | 0.795*** | -0.673*** | 0.898*** | -0.791*** | 0.994*** |
| | (0.0941) | (0.0195) | (0.0597) | (0.0699) | (0.0685) | (0.0784) | (0.0726) | (0.0830) |
| Technical assistance | 0.336** | -0.00727 | 0.0538 | 0.00620 | 0.0214 | 0.00149 | 0.0385 | -0.00114 |
| | (0.144) | (0.0315) | (0.0987) | (0.115) | (0.115) | (0.131) | (0.126) | (0.141) |
| Water access on farm | 0.324** | -0.00643 | 0.0351 | 0.0224 | 0.0318 | 0.0392 | 0.0357 | 0.0316 |
| | (0.127) | (0.0239) | (0.0731) | (0.0888) | (0.0839) | (0.0967) | (0.0935) | (0.105) |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Paragominas | -0.446** | 0.00598 | -0.0368 | 0.402*** | 0.000760 | 0.426*** | -0.0480 | 0.472*** |
| | (0.183) | (0.0388) | (0.118) | (0.141) | (0.141) | (0.161) | (0.155) | (0.172) |
| Constant | -3.776*** | 0.782*** | 0.674*** | 2.527*** | 0.757*** | 2.876*** | 0.851*** | 3.122*** |
| | (0.358) | (0.0718) | (0.226) | (0.275) | (0.259) | (0.301) | (0.288) | (0.323) |
| Observations | 313 | 313 | 313 | 313 | 313 | 313 | 313 | 313 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Each generated instrument $v$ is computed according to the Lewbel (2012; 1997) formula as $\left(X - E(X)\right)^2 \varepsilon_Y$, where $\varepsilon_Y$ is obtained by regressing all the exogenous variables $X$ listed below on each of the endogenous variables Y. Instruments are referred to with the alpha-numeric codes reported below

| Endogenous variable (Y) | | Exogenous variable (X) | |
|---|---|---|---|
| b | Log fire density non fire user | 1 | Farm size |
| c | Fire user | 2 | Distance to the city |
| d | Capital (pc) | 3 | Age household head |
| e | Log fire density fire user | 4 | Slope |
| | | 5 | Household size |
| | | 6 | Soil quality |

*Table 6 Short estimation results for a level-level specification for the farm revenue model*

| | | OLS | IVgmm | IVtsls |
|---|---|---|---|---|
| 1 Km | Fire density (non fire user) | -1,175** | -410.8 | -1,403** |
| | | (543.4) | (375.9) | (601.9) |
| | Fire density (fire user) | -158.8 | 146.6 | -241.4 |
| | | (245.0) | (159.2) | (296.4) |
| | Fire user | -11,392** | -4,429 | -14,061** |
| | | (4,616) | (3,977) | (6,107) |
| | C statistic (p-value) | | 0.307 | 0.307 |
| | F statistic | | 21.28 | 21.28 |
| | Hansen J | | 0.206 | 0.206 |
| | Wald test difference (p-value) | 0.0394 | 0.1271 | 0.0393 |
| | Reset test | 0.0000 | | |
| | Adjusted R2 | 0.372 | 0.257 | 0.369 |
| 2 Km | Fire density (non fire user) | -435.0** | -96.16 | -484.4** |
| | | (200.2) | (142.1) | (213.3) |
| | Fire density (fire user) | -76.96 | 16.44 | -106.5 |
| | | (92.24) | (52.21) | (102.5) |
| | Fire user | -11,548** | -3,922 | -14,029** |
| | | (4,784) | (4,190) | (6,222) |
| | C statistic (p-value) | | 0.412 | 0.412 |
| | F statistic | | 24.05 | 24.05 |
| | Hansen J | | 0.218 | 0.218 |
| | Wald test difference (p-value) | 0.0348 | 0.3958 | 0.0365 |
| | Reset test | 0.0000 | | |
| | Adjusted R2 | 0.374 | 0.265 | 0.372 |
| 3 Km | Fire density (non fire user) | -281.0** | -83.06 | -295.0** |
| | | (119.4) | (74.73) | (122.8) |

| | | | | |
|---|---|---|---|---|
| | Fire density (fire user) | -45.06 | 16.70 | -54.88 |
| | | (49.23) | (27.64) | (53.60) |
| | Fire user | -12,886** | -5,028 | -15,193** |
| | | (5,146) | (4,139) | (6,734) |
| | C statistic (p-value) | | 0.744 | 0.744 |
| | F statistic | | 22.69 | 22.69 |
| | Hansen J | | 0.295 | 0.295 |
| | Wald test difference (p-value) | 0.0181 | 0.1583 | 0.0193 |
| | Reset test | 0.0000 | | |
| | Adjusted R2 | 0.376 | 0.254 | 0.375 |
| 4 Km | Fire density (non fire user) | -188.9** | -72.64 | -189.5** |
| | | (80.96) | (46.39) | (81.21) |
| | Fire density (fire user) | -33.68 | 2.713 | -32.95 |
| | | (32.67) | (16.58) | (36.31) |
| | Fire user | -13,300** | -5,631 | -15,163** |
| | | (5,159) | (4,222) | (6,587) |
| | C statistic (p-value) | | 0.766 | 0.766 |
| | F statistic | | 0.766 | 0.766 |
| | Hansen J | | 0.210 | 0.210 |
| | Wald test difference (p-value) | 0.0178 | 0.0915 | 0.0179 |
| | Reset test | 0.0000 | | |
| | Adjusted R2 | 0.378 | 0.258 | 0.377 |
| 5 Km | Fire density (non fire user) | -137.1** | -48.52 | -136.1** |
| | | (60.70) | (35.78) | (60.03) |
| | Fire density (fire user) | -26.93 | 0.700 | -26.81 |
| | | (25.37) | (12.74) | (28.26) |
| | Fire user | -13,193*** | -4,857 | -13,996** |
| | | (4,977) | (4,274) | (6,130) |
| | C statistic (p-value) | | 0.602 | 0.602 |
| | F statistic | | 25.91 | 25.91 |
| | Hansen J | | 0.159 | 0.159 |
| | Wald test difference (p-value) | 0.0063 | 0.1456 | 0.0204 |
| | Reset test | 0.0000 | | |
| | Adjusted R2 | 0.378 | 0.258 | 0.377 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Table 7 Short estimation results for log-level specification for the farm revenue model*

| | | OLS | IVgmm | IVtsls |
|---|---|---|---|---|
| 1 Km | Fire density (non fire user) | -0.0601 | -0.0427 | -0.0625 |
| | | (0.0376) | (0.0265) | (0.0390) |
| | Fire density (fire user) | 0.0101 | 0.00858 | 0.0102 |
| | | (0.0106) | (0.00897) | (0.0113) |
| | Fire user | -0.363 | -0.271 | -0.240 |
| | | (0.329) | (0.286) | (0.361) |
| | C statistic (p-value) | | 0.217 | 0.217 |

| | | | | |
|---|---|---|---|---|
| | F statistic | | 21.28 | 21.28 |
| | Hansen J | | 0.0783 | 0.0783 |
| | Wald test difference (p-value) | 0.0624 | 0.0577 | 0.0668 |
| | Reset test | 0.2227 | | |
| | Adjusted R2 | 0.253 | 0.239 | 0.252 |
| | Fire density (non fire user) | -0.0188 | -0.0150** | -0.0176 |
| | | (0.0127) | (0.00746) | (0.0130) |
| | Fire density (fire user) | 0.00453 | 0.00359 | 0.00484 |
| | | (0.00377) | (0.00307) | (0.00436) |
| | Fire user | -0.365 | -0.137 | -0.220 |
| | | (0.331) | (0.278) | (0.366) |
| 2 Km | C statistic (p-value) | | 0.209 | 0.209 |
| | F statistic | | 24.05 | 24.05 |
| | Hansen J | | 0.314 | 0.314 |
| | Wald test difference (p-value) | 0.0700 | 0.0155 | 0.0936 |
| | Reset test | 0.3200 | | |
| | Adjusted R2 | 0.253 | 0.239 | 0.252 |
| | Fire density (non fire user) | -0.0121 | -0.00872* | -0.0114 |
| | | (0.00845) | (0.00471) | (0.00867) |
| | Fire density (fire user) | 0.00208 | 0.00195 | 0.00177 |
| | | (0.00195) | (0.00161) | (0.00225) |
| | Fire user | -0.420 | -0.181 | -0.253 |
| | | (0.358) | (0.281) | (0.386) |
| 3 Km | C statistic (p-value) | | 0.0989 | 0.0989 |
| | F statistic | | 22.69 | 22.69 |
| | Hansen J | | 0.339 | 0.339 |
| | Wald test difference (p-value) | 0.0919 | 0.0203 | 0.1273 |
| | Reset test | 0.2701 | | |
| | Adjusted R2 | 0.254 | 0.244 | 0.253 |
| | Fire density (non fire user) | -0.00788 | -0.00596* | -0.00725 |
| | | (0.00543) | (0.00308) | (0.00562) |
| | Fire density (fire user) | 0.00107 | 0.00122 | 0.00102 |
| | | (0.00121) | (0.000961) | (0.00142) |
| | Fire user | -0.424 | -0.220 | -0.261 |
| | | (0.366) | (0.285) | (0.389) |
| 4 Km | C statistic (p-value) | | 0.165 | 0.165 |
| | F statistic | | 25.84 | 25.84 |
| | Hansen J | | 0.421 | 0.421 |
| | Wald test difference (p-value) | 0.0955 | 0.0181 | 0.1319 |
| | Reset test | 0.3324 | | |
| | Adjusted R2 | 0.253 | 0.245 | 0.252 |
| | Fire density (non fire user) | -0.00577 | -0.00458** | -0.00518 |
| | | (0.00397) | (0.00216) | (0.00413) |
| | Fire density (fire user) | 0.000650 | 0.000767 | 0.000676 |
| | | (0.000944) | (0.000752) | (0.00112) |
| 5 Km | Fire user | -0.425 | -0.229 | -0.237 |
| | | (0.368) | (0.288) | (0.389) |
| | C statistic (p-value) | | 0.160 | 0.160 |
| | F statistic | | 25.91 | 25.91 |
| | Hansen J | | 0.443 | 0.443 |

| | | | |
|---|---|---|---|
| Wald test difference (p-value) | 0.0995 | 0.0114 | 0.1417 |
| Reset test | 0.3530 | | |
| Adjusted R2 | 0.252 | 0.245 | 0.251 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Table 8 Estimates with and without endogenous controls (capital and labour) for farm revenue and for a 3 to 5 km buffer*

| | 3 km | | | | 4 km | | | | 5 km | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | OLS3_re | OLS3_full | IVgmm3_re | IVgmm3_full | OLS4_re | OLS4_full | IVgmm4_re | IVgmm4_full | OLS5_re | OLS5_full | IVgmm5_re | IVgmm5_full |
| Log-Fire density (non fire user) | -0.714* | -0.651 | -0.571* | -0.586* | -0.789* | -0.727* | -0.680* | -0.643* | -0.858* | -0.796* | -0.946** | -0.854** |
| | (0.411) | (0.428) | (0.346) | (0.320) | (0.422) | (0.436) | (0.401) | (0.376) | (0.443) | (0.460) | (0.428) | (0.396) |
| Log-Fire density (fire user) | -0.0578 | 0.0247 | -0.208** | -0.144 | -0.156* | -0.0759 | -0.262*** | -0.196** | -0.201** | -0.127 | -0.307*** | -0.246*** |
| | (0.100) | (0.108) | (0.101) | (0.101) | (0.0918) | (0.103) | (0.0825) | (0.0863) | (0.0853) | (0.0988) | (0.0723) | (0.0775) |
| Fire user | -2.207 | -2.120 | -1.208 | -1.198 | -2.426 | -2.358 | -1.563 | -1.425 | -2.733 | -2.652 | -2.466 | -2.180 |
| | (1.344) | (1.383) | (1.099) | (0.995) | (1.584) | (1.606) | (1.487) | (1.372) | (1.779) | (1.811) | (1.709) | (1.566) |
| Capital (pc) | | 0.0782 | | 0.101*** | | 0.0741 | | 0.101*** | | 0.0730 | | 0.0897** |
| | | (0.0512) | | (0.0344) | | (0.0508) | | (0.0347) | | (0.0502) | | (0.0351) |
| Log-Labour days | | -0.00628 | | -0.0524 | | -0.0144 | | -0.0641 | | -0.0191 | | -0.0816 |
| | | (0.0663) | | (0.0564) | | (0.0668) | | (0.0556) | | (0.0671) | | (0.0555) |
| No labour | | -0.507 | | -0.767** | | -0.509 | | -0.741** | | -0.504 | | -0.798** |
| | | (0.419) | | (0.365) | | (0.421) | | (0.363) | | (0.422) | | (0.365) |
| Log-Farm size | 0.319*** | 0.199** | 0.360*** | 0.215*** | 0.333*** | 0.219** | 0.375*** | 0.220*** | 0.349*** | 0.237** | 0.378*** | 0.236*** |
| | (0.0888) | (0.0914) | (0.0803) | (0.0815) | (0.0889) | (0.0928) | (0.0799) | (0.0833) | (0.0891) | (0.0942) | (0.0798) | (0.0852) |
| Education (years) | -0.0134 | -0.0214 | 0.00514 | 0.000425 | -0.0127 | -0.0205 | -0.00124 | -0.00866 | -0.0113 | -0.0191 | -0.000882 | -0.00772 |
| | (0.0328) | (0.0325) | (0.0251) | (0.0239) | (0.0330) | (0.0328) | (0.0241) | (0.0232) | (0.0331) | (0.0329) | (0.0238) | (0.0229) |
| Age household head | -0.0226*** | -0.0213*** | -0.0229*** | -0.0222*** | -0.0228*** | -0.0215*** | -0.0221*** | -0.0222*** | -0.0231*** | -0.0218*** | -0.0210*** | -0.0221*** |
| | (0.00694) | (0.00652) | (0.00565) | (0.00527) | (0.00698) | (0.00659) | (0.00561) | (0.00515) | (0.00695) | (0.00659) | (0.00565) | (0.00517) |
| Male household head | 0.605** | 0.579** | 0.501** | 0.482** | 0.586** | 0.571** | 0.445* | 0.420* | 0.574** | 0.566** | 0.468* | 0.453* |
| | (0.277) | (0.273) | (0.255) | (0.245) | (0.277) | (0.274) | (0.253) | (0.243) | (0.277) | (0.274) | (0.254) | (0.246) |
| Household size | 0.00919 | 0.0212 | 0.00642 | 0.0174 | 0.00739 | 0.0191 | 0.00520 | 0.0217 | 0.00620 | 0.0176 | 0.00186 | 0.0200 |
| | (0.0235) | (0.0239) | (0.0212) | (0.0207) | (0.0235) | (0.0239) | (0.0212) | (0.0211) | (0.0233) | (0.0237) | (0.0208) | (0.0209) |
| Soil quality | 0.365*** | 0.323*** | 0.440*** | 0.384*** | 0.377*** | 0.337*** | 0.431*** | 0.387*** | 0.390*** | 0.350*** | 0.466*** | 0.417*** |
| | (0.0880) | (0.0888) | (0.0717) | (0.0690) | (0.0891) | (0.0900) | (0.0747) | (0.0701) | (0.0887) | (0.0904) | (0.0718) | (0.0696) |
| Slope | 0.0201* | 0.0201* | 0.0225** | 0.0220*** | 0.0207** | 0.0210** | 0.0209** | 0.0244*** | 0.0206** | 0.0212** | 0.0220** | 0.0260*** |
| | (0.0104) | (0.0106) | (0.00919) | (0.00841) | (0.0103) | (0.0106) | (0.00890) | (0.00783) | (0.0102) | (0.0105) | (0.00887) | (0.00776) |
| Log-Distance to the city | -0.346*** | -0.281** | -0.366*** | -0.275** | -0.349*** | -0.283** | -0.345*** | -0.238** | -0.359*** | -0.292** | -0.330*** | -0.239** |
| | (0.131) | (0.136) | (0.115) | (0.110) | (0.129) | (0.133) | (0.112) | (0.110) | (0.127) | (0.131) | (0.111) | (0.108) |
| Technical assistance | 0.0787 | 0.0715 | 0.00442 | 0.0735 | 0.0688 | 0.0653 | -0.0299 | 0.0348 | 0.0630 | 0.0611 | -0.0656 | -0.00873 |
| | (0.230) | (0.227) | (0.209) | (0.195) | (0.228) | (0.225) | (0.206) | (0.193) | (0.230) | (0.226) | (0.204) | (0.196) |
| Water access on farm | 0.513*** | 0.433** | 0.464*** | 0.344** | 0.536*** | 0.453** | 0.464*** | 0.358** | 0.540*** | 0.457** | 0.504*** | 0.390** |
| | (0.174) | (0.181) | (0.161) | (0.160) | (0.176) | (0.185) | (0.160) | (0.163) | (0.177) | (0.187) | (0.163) | (0.166) |
| Paragominas | -0.124 | -0.235 | 0.0443 | -0.0245 | -0.00306 | -0.105 | 0.189 | 0.127 | 0.0573 | -0.0324 | 0.268 | 0.224 |
| | (0.232) | (0.254) | (0.218) | (0.227) | (0.234) | (0.259) | (0.214) | (0.227) | (0.237) | (0.264) | (0.215) | (0.227) |
| Constant | 9.684*** | 9.858*** | 9.127*** | 9.821*** | 10.24*** | 10.40*** | 9.739*** | 10.31*** | 10.76*** | 10.91*** | 10.79*** | 11.29*** |
| | (1.337) | (1.430) | (1.136) | (1.142) | (1.572) | (1.648) | (1.499) | (1.484) | (1.790) | (1.879) | (1.722) | (1.661) |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Observations | 313 | 313 | 313 | 313 | 313 | 313 | 313 | 313 | 313 | 313 | 313 | 313 |
| R-squared | 0.293 | 0.312 | 0.282 | 0.298 | 0.297 | 0.314 | 0.290 | 0.302 | 0.300 | 0.315 | 0.293 | 0.305 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Adjusted R2 | 0.259 | 0.273 | 0.248 | 0.258 | 0.264 | 0.274 | 0.256 | 0.262 | 0.267 | 0.276 | 0.260 | 0.264 |
| Hansen J | | | 0.106 | 0.246 | | | 0.129 | 0.263 | | | 0.253 | 0.397 |
| K-P rank test | | | 1.89e-10 | 2.19e-10 | | | 1.04e-09 | 5.25e-10 | | | 7.22e-10 | 6.28e-10 |
| F statistic | | | 26.24 | 27.68 | | | 35.74 | 25.98 | | | 37.34 | 27.96 |
| C statistic | | | 0.0320 | 0.00810 | | | 0.0798 | 0.0142 | | | 0.0645 | 0.0123 |
| Wald test difference (p-value) | 0.1185 | 0.1196 | 0.2976 | 0.1641 | 0.1412 | 0.1355 | 0.3035 | 0.2360 | 0.1398 | 0.1391 | 0.1357 | 0.1206 |

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

*Table 9 Estimates with and without endogenous controls (capital and labour) for total revenue and for a 3 to 5 km buffer*

| | 3 km | | | | 4 km | | | | 5 km | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | OLS3_ re | OLS3_ full | IVgmm3 _re | IVgmm3_ full | OLS4_ re | OLS4_ full | IVgmm4_ re | IVgmm4_ full | OLS5_ re | OLS55_ full | IVgmm5_ re | IVgmm5_ full |
| Log-Fire density (non fire user) | -0.616*** | -0.543*** | -0.681*** | -0.722*** | -0.663*** | -0.596*** | -0.745*** | -0.745*** | -0.664*** | -0.594*** | -0.770*** | -0.771*** |
| | (0.203) | (0.197) | (0.146) | (0.136) | (0.211) | (0.196) | (0.169) | (0.137) | (0.233) | (0.213) | (0.193) | (0.157) |
| Log-Fire density (fire user) | -0.108 | -0.0131 | -0.137* | -0.0878 | -0.152** | -0.0559 | -0.181*** | -0.127* | -0.167*** | -0.0747 | -0.189*** | -0.146** |
| | (0.0717) | (0.0779) | (0.0727) | (0.0761) | (0.0675) | (0.0744) | (0.0690) | (0.0719) | (0.0645) | (0.0718) | (0.0669) | (0.0688) |
| Fire user | -2.056*** | -1.941*** | -2.070*** | -2.192*** | -2.313*** | -2.238*** | -2.402*** | -2.428*** | -2.432** | -2.346*** | -2.679*** | -2.686*** |
| | (0.702) | (0.674) | (0.491) | (0.428) | (0.838) | (0.778) | (0.622) | (0.480) | (0.985) | (0.897) | (0.766) | (0.593) |
| Capital (pc) | | 0.114*** | | 0.100*** | | 0.113*** | | 0.0981*** | | 0.114*** | | 0.0962*** |
| | | (0.0287) | | (0.0211) | | (0.0282) | | (0.0210) | | (0.0283) | | (0.0222) |
| Log-Labour days | | -0.0355 | | -0.0128 | | -0.0390 | | -0.0237 | | -0.0399 | | -0.0282 |
| | | (0.0359) | | (0.0312) | | (0.0355) | | (0.0311) | | (0.0356) | | (0.0310) |
| No labour | | -0.662*** | | -0.579*** | | -0.659*** | | -0.615*** | | -0.647*** | | -0.610*** |
| | | (0.213) | | (0.200) | | (0.214) | | (0.201) | | (0.215) | | (0.201) |
| Log-Farm size | 0.307*** | 0.143** | 0.265*** | 0.142*** | 0.316*** | 0.153*** | 0.277*** | 0.153*** | 0.325*** | 0.161*** | 0.289*** | 0.168*** |
| | (0.0577) | (0.0570) | (0.0475) | (0.0485) | (0.0575) | (0.0575) | (0.0473) | (0.0485) | (0.0574) | (0.0580) | (0.0466) | (0.0481) |
| Education (years) | 0.0270 | 0.0135 | 0.0356** | 0.0279* | 0.0276 | 0.0141 | 0.0348** | 0.0288** | 0.0288* | 0.0152 | 0.0342** | 0.0255* |
| | (0.0174) | (0.0174) | (0.0152) | (0.0149) | (0.0173) | (0.0172) | (0.0149) | (0.0143) | (0.0173) | (0.0172) | (0.0145) | (0.0141) |
| Age household head | 0.00953** | 0.0102** | 0.00960** | 0.00984*** | 0.00947** | 0.0102** | 0.0102** | 0.0108*** | 0.00940** | 0.0102** | 0.0101** | 0.0105*** |
| | (0.00463) | (0.00408) | (0.00426) | (0.00381) | (0.00462) | (0.00409) | (0.00428) | (0.00379) | (0.00463) | (0.00412) | (0.00427) | (0.00383) |
| Male household head | -0.0539 | -0.0524 | -0.139 | -0.0724 | -0.0698 | -0.0613 | -0.149 | -0.0890 | -0.0792 | -0.0665 | -0.153 | -0.109 |
| | (0.148) | (0.140) | (0.135) | (0.124) | (0.149) | (0.141) | (0.135) | (0.123) | (0.148) | (0.141) | (0.134) | (0.124) |
| Household size | 0.0236 | 0.0368** | 0.0193 | 0.0283** | 0.0223 | 0.0356** | 0.0202 | 0.0295** | 0.0217 | 0.0348** | 0.0212 | 0.0321** |
| | (0.0152) | (0.0157) | (0.0135) | (0.0129) | (0.0151) | (0.0156) | (0.0134) | (0.0127) | (0.0152) | (0.0156) | (0.0137) | (0.0130) |
| Soil quality | 0.171*** | 0.115** | 0.162*** | 0.0890*** | 0.179*** | 0.121** | 0.160*** | 0.100*** | 0.189*** | 0.130** | 0.161*** | 0.0998*** |
| | (0.0521) | (0.0519) | (0.0272) | (0.0307) | (0.0538) | (0.0536) | (0.0309) | (0.0348) | (0.0546) | (0.0544) | (0.0331) | (0.0371) |
| Slope | 0.00624 | 0.00764 | 0.00642 | 0.00618 | 0.00664 | 0.00824 | 0.00692 | 0.00774* | 0.00649 | 0.00821 | 0.00706 | 0.00822* |
| | (0.00601) | (0.00610) | (0.00507) | (0.00466) | (0.00598) | (0.00608) | (0.00527) | (0.00470) | (0.00595) | (0.00607) | (0.00532) | (0.00473) |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Log-Distance to the city | -0.352*** | -0.256*** | -0.408*** | -0.301*** | -0.360*** | -0.260*** | -0.420*** | -0.296*** | -0.373*** | -0.269*** | -0.437*** | -0.304*** |
| | (0.0761) | (0.0824) | (0.0542) | (0.0574) | (0.0750) | (0.0812) | (0.0545) | (0.0575) | (0.0746) | (0.0808) | (0.0551) | (0.0590) |
| Technical assistance | 0.0612 | 0.0721 | -0.0118 | -0.0156 | 0.0559 | 0.0681 | -0.00912 | -0.00222 | 0.0574 | 0.0694 | -0.0145 | -0.00505 |
| | (0.127) | (0.119) | (0.109) | (0.0987) | (0.126) | (0.118) | (0.115) | (0.100) | (0.128) | (0.120) | (0.116) | (0.0998) |
| Water access on farm | 0.206* | 0.0919 | 0.171* | 0.0729 | 0.218* | 0.0962 | 0.189* | 0.0948 | 0.215* | 0.0906 | 0.187* | 0.1000 |
| | (0.113) | (0.108) | (0.0989) | (0.0946) | (0.115) | (0.110) | (0.101) | (0.0959) | (0.117) | (0.112) | (0.104) | (0.0973) |
| Paragominas | 0.232 | 0.140 | 0.290** | 0.186 | 0.298* | 0.206 | 0.370*** | 0.257* | 0.319** | 0.235 | 0.398*** | 0.301** |
| | (0.154) | (0.162) | (0.141) | (0.144) | (0.154) | (0.161) | (0.144) | (0.145) | (0.155) | (0.162) | (0.144) | (0.144) |
| Receives government transfer | 0.601*** | 0.633*** | 0.521*** | 0.541*** | 0.594*** | 0.625*** | 0.519*** | 0.529*** | 0.586*** | 0.616*** | 0.507*** | 0.515*** |
| | (0.163) | (0.155) | (0.139) | (0.127) | (0.161) | (0.154) | (0.137) | (0.125) | (0.161) | (0.154) | (0.137) | (0.125) |
| Receives remittances | 0.236 | 0.322** | 0.107 | 0.225* | 0.252 | 0.327** | 0.142 | 0.254** | 0.264 | 0.331** | 0.166 | 0.250* |
| | (0.162) | (0.156) | (0.141) | (0.133) | (0.163) | (0.157) | (0.139) | (0.129) | (0.164) | (0.157) | (0.140) | (0.128) |
| Constant | 9.244*** | 9.641*** | 9.687*** | 10.19*** | 9.686*** | 10.08*** | 10.14*** | 10.51*** | 9.916*** | 10.27*** | 10.49*** | 10.90*** |
| | (0.825) | (0.798) | (0.562) | (0.581) | (0.948) | (0.887) | (0.708) | (0.634) | (1.095) | (1.008) | (0.851) | (0.743) |
| Observations | 300 | 300 | 300 | 300 | 300 | 300 | 300 | 300 | 300 | 300 | 300 | 300 |
| R-squared | 0.413 | 0.476 | 0.401 | 0.460 | 0.418 | 0.477 | 0.408 | 0.466 | 0.417 | 0.476 | 0.407 | 0.465 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Adjusted R2 | 0.380 | 0.440 | 0.367 | 0.424 | 0.385 | 0.442 | 0.374 | 0.430 | 0.384 | 0.440 | 0.374 | 0.429 |
| Hansen J | | | 0.455 | 0.173 | | | 0.632 | 0.303 | | | 0.611 | 0.299 |
| K-P rank test | | | 1.52e-09 | 4.65e-09 | | | 1.23e-08 | 1.88e-08 | | | 7.64e-09 | 1.86e-08 |
| F statistic | | | 24.99 | 23.14 | | | 34.70 | 24.98 | | | 34.66 | 26.98 |
| C statistic | | | 0.195 | 0.258 | | | 0.198 | 0.122 | | | 0.340 | 0.159 |
| Wald test difference (p-value) | 0.0113 | 0.0082 | 0.0003 | 0.0000 | 0.0156 | 0.0076 | 0.0007 | 0.0000 | 0.0359 | 0.0158 | 0.0021 | 0.0000 |

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

*Table 10 Robustness test to covariates introduction for the farm revenue models in the 3km buffer (OLS regressions)*

| | OLS1 | OLS2 | OLS3 | OLS4 | OLS5 | OLS6 | OLS7 | OLS8 | OLS9 | OLS10 | OLS11 | OLS12 | OLS13 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Log-Fire density (non fire user) | -0.702 | -0.629 | -0.634 | -0.762* | -0.751* | -0.866* | -0.870** | -0.827* | -0.818* | -0.833** | -0.678 | -0.666 | -0.800** |
| | (0.426) | (0.418) | (0.419) | (0.430) | (0.445) | (0.445) | (0.442) | (0.443) | (0.421) | (0.413) | (0.419) | (0.424) | (0.388) |
| Log-Fire density (fire user) | -0.0287 | 0.0194 | 0.0315 | -0.0107 | -0.0140 | 0.0470 | 0.0337 | 0.0451 | 0.0815 | 0.0823 | 0.189*** | 0.199*** | 0.213*** |
| | (0.0862) | (0.0829) | (0.0791) | (0.0728) | (0.0759) | (0.0684) | (0.0676) | (0.0664) | (0.0718) | (0.0704) | (0.0606) | (0.0639) | (0.0604) |
| Fire user | -2.094 | -1.975 | -2.025 | -2.450* | -2.384* | -3.061** | -3.026** | -2.929** | -3.061** | -3.130** | -2.868** | -2.872** | -3.945*** |
| | (1.374) | (1.357) | (1.352) | (1.371) | (1.412) | (1.391) | (1.376) | (1.385) | (1.321) | (1.287) | (1.320) | (1.341) | (1.249) |
| Capital (pc) | 0.0793 | 0.107** | 0.108** | 0.130*** | 0.122*** | 0.152*** | 0.155*** | 0.158*** | 0.154*** | 0.141*** | 0.192*** | 0.208*** | |
| | (0.0512) | (0.0482) | (0.0485) | (0.0465) | (0.0468) | (0.0456) | (0.0459) | (0.0458) | (0.0455) | (0.0465) | (0.0444) | (0.0450) | |
| Log-Labour days | -0.0212 | -0.0261 | -0.0217 | -0.0368 | -0.0139 | -0.00735 | -0.00330 | 0.0110 | 0.0174 | 0.0117 | 0.0465 | | |
| | (0.0636) | (0.0636) | (0.0627) | (0.0634) | (0.0594) | (0.0598) | (0.0592) | (0.0599) | (0.0591) | (0.0554) | (0.0581) | | |
| No labour | -0.541 | -0.538 | -0.522 | -0.552 | -0.446 | -0.423 | -0.329 | -0.262 | -0.245 | -0.240 | -0.160 | | |
| | (0.420) | (0.422) | (0.419) | (0.428) | (0.414) | (0.419) | (0.413) | (0.422) | (0.421) | (0.397) | (0.413) | | |
| Log-Farm size | 0.199** | 0.202** | 0.205** | 0.164* | 0.248*** | 0.196*** | 0.204*** | 0.215*** | 0.230*** | 0.230*** | | | |
| | (0.0902) | (0.0932) | (0.0931) | (0.0878) | (0.0748) | (0.0727) | (0.0722) | (0.0729) | (0.0727) | (0.0720) | | | |
| Education (years) | -0.0223 | -0.0269 | -0.0277 | -0.0293 | -0.0300 | -0.0449 | -0.0402 | -0.0425 | -0.0331 | | | | |
| | (0.0328) | (0.0324) | (0.0321) | (0.0290) | (0.0289) | (0.0277) | (0.0282) | (0.0278) | (0.0283) | | | | |
| Age household head | -0.0201*** | -0.0185*** | -0.0185*** | -0.0186*** | -0.0198*** | -0.0183*** | -0.0196*** | -0.0177*** | | | | | |
| | (0.00645) | (0.00647) | (0.00648) | (0.00616) | (0.00608) | (0.00621) | (0.00612) | (0.00614) | | | | | |
| Male household head | 0.582** | 0.613** | 0.609** | 0.605** | 0.591** | 0.528** | 0.486* | | | | | | |
| | (0.275) | (0.287) | (0.287) | (0.258) | (0.257) | (0.261) | (0.257) | | | | | | |
| Household size | 0.0265 | 0.0332 | 0.0320 | 0.0246 | 0.0204 | 0.0336 | | | | | | | |
| | (0.0240) | (0.0254) | (0.0259) | (0.0259) | (0.0256) | (0.0257) | | | | | | | |
| Soil quality | 0.316*** | 0.286*** | 0.287*** | 0.331*** | 0.305*** | | | | | | | | |
| | (0.0916) | (0.0870) | (0.0867) | (0.0793) | (0.0795) | | | | | | | | |
| Slope | 0.0212** | 0.0187* | 0.0189* | 0.0164* | | | | | | | | | |
| | (0.0103) | (0.0102) | (0.0101) | (0.00982) | | | | | | | | | |
| Log-Distance to the city | -0.286** | -0.289** | -0.288** | | | | | | | | | | |
| | (0.134) | (0.136) | (0.136) | | | | | | | | | | |
| Technical assistance | 0.0267 | 0.0966 | | | | | | | | | | | |
| | (0.220) | (0.213) | | | | | | | | | | | |
| Water access on farm | 0.422** | | | | | | | | | | | | |
| | (0.182) | | | | | | | | | | | | |
| Constant | 9.944*** | 9.864*** | 9.876*** | 10.61*** | 10.61*** | 11.12*** | 11.28*** | 11.35*** | 10.30*** | 10.21*** | 10.19*** | 10.36*** | 11.25*** |
| | (1.428) | (1.403) | (1.406) | (1.414) | (1.448) | (1.435) | (1.430) | (1.444) | (1.332) | (1.302) | (1.346) | (1.315) | (1.228) |
| Observations | 313 | 313 | 313 | 333 | 333 | 333 | 333 | 333 | 333 | 349 | 349 | 349 | 350 |
| R-squared | 0.310 | 0.298 | 0.297 | 0.288 | 0.280 | 0.257 | 0.254 | 0.242 | 0.223 | 0.201 | 0.179 | 0.168 | 0.077 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2.17e-10 | 4.75e-09 | 4.53e-08 | 3.82e-08 | 2.33e-05 |
| Adjusted R2 | 0.273 | 0.262 | 0.264 | 0.259 | 0.253 | 0.232 | 0.230 | 0.221 | 0.204 | 0.184 | 0.165 | 0.159 | 0.0691 |

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

*Table 11 Robustness test to covariates introduction for the farm revenue models in the 3km buffer (IV regressions)*

| | IVgmm1 | IVgmm2 | IVgmm3 | IVgmm4 | IVgmm5 | IVgmm6 | IVgmm7 | IVgmm8 |
|---|---|---|---|---|---|---|---|---|
| Log-Fire density (non fire user) | -0.586* | -0.557* | -0.551* | -0.542* | -0.628* | -0.542* | -0.556* | -0.553* |
| | (0.320) | (0.317) | (0.318) | (0.316) | (0.321) | (0.299) | (0.304) | (0.312) |
| Log-Fire density (fire user) | -0.144 | -0.139 | -0.0936 | -0.0669 | -0.0662 | -0.0344 | -0.0447 | -0.114 |
| | (0.101) | (0.0859) | (0.0845) | (0.0818) | (0.0817) | (0.0817) | (0.0840) | (0.0883) |
| Fire user | -1.198 | -1.102 | -1.165 | -1.241 | -1.550 | -1.342 | -1.362 | -1.425 |
| | (0.995) | (1.001) | (1.010) | (1.004) | (1.040) | (0.962) | (0.978) | (1.028) |
| Capital (pc) | 0.101*** | 0.101*** | 0.124*** | 0.121*** | 0.121*** | 0.130*** | 0.135*** | |
| | (0.0344) | (0.0345) | (0.0325) | (0.0326) | (0.0335) | (0.0327) | (0.0334) | |
| Log-Labour days | -0.0524 | -0.0491 | -0.0547 | -0.0504 | -0.0468 | -0.0391 | | |
| | (0.0564) | (0.0542) | (0.0535) | (0.0535) | (0.0532) | (0.0536) | | |
| No labour | -0.767** | -0.748** | -0.788** | -0.777** | -0.694* | -0.686* | | |
| | (0.365) | (0.365) | (0.364) | (0.364) | (0.358) | (0.368) | | |
| Log-Farm size | 0.215*** | 0.217*** | 0.224*** | 0.222*** | 0.225*** | 0.226*** | 0.254*** | 0.437*** |
| | (0.0815) | (0.0813) | (0.0847) | (0.0858) | (0.0848) | (0.0840) | (0.0861) | (0.0816) |
| Education (years) | 0.000425 | -0.000101 | -0.00181 | -0.00165 | 0.00235 | -0.00219 | -0.00757 | 0.00362 |
| | (0.0239) | (0.0241) | (0.0235) | (0.0233) | (0.0241) | (0.0223) | (0.0225) | (0.0236) |
| Age household head | -0.0222*** | -0.0214*** | -0.0208*** | -0.0206*** | -0.0211*** | -0.0197*** | -0.0205*** | -0.0199*** |
| | (0.00527) | (0.00534) | (0.00533) | (0.00539) | (0.00540) | (0.00538) | (0.00540) | (0.00566) |
| Male household head | 0.482** | 0.504** | 0.522** | 0.532** | 0.508** | | | |
| | (0.245) | (0.246) | (0.258) | (0.258) | (0.253) | | | |
| Household size | 0.0174 | 0.0199 | 0.0261 | 0.0257 | | | | |
| | (0.0207) | (0.0206) | (0.0221) | (0.0225) | | | | |
| Soil quality | 0.384*** | 0.374*** | 0.341*** | 0.356*** | 0.363*** | 0.314*** | 0.318*** | 0.372*** |
| | (0.0690) | (0.0720) | (0.0605) | (0.0599) | (0.0630) | (0.0405) | (0.0405) | (0.0412) |
| Slope | 0.0220*** | 0.0211** | 0.0166** | 0.0180** | 0.0178** | 0.0179** | 0.0191** | 0.0182** |
| | (0.00841) | (0.00825) | (0.00811) | (0.00821) | (0.00822) | (0.00815) | (0.00762) | (0.00867) |
| Log-Distance to the city | -0.275** | -0.305*** | -0.318*** | -0.296*** | -0.284** | -0.299*** | -0.292** | -0.455*** |
| | (0.110) | (0.112) | (0.114) | (0.113) | (0.112) | (0.111) | (0.114) | (0.114) |
| Technical assistance | 0.0735 | 0.0935 | 0.168 | | | | | |
| | (0.195) | (0.186) | (0.176) | | | | | |
| Water access on farm | 0.344** | 0.349** | | | | | | |
| | (0.160) | (0.161) | | | | | | |
| Paragominas | -0.0245 | | | | | | | |
| | (0.227) | | | | | | | |
| Constant | 9.821*** | 9.651*** | 9.813*** | 9.760*** | 10.14*** | 10.17*** | 9.847*** | 9.499*** |
| | (1.142) | (1.151) | (1.139) | (1.131) | (1.147) | (1.092) | (1.011) | (1.041) |
| Observations | 313 | 313 | 313 | 313 | 313 | 313 | 313 | 313 |
| R-squared | 0.298 | 0.298 | 0.284 | 0.286 | 0.285 | 0.271 | 0.266 | 0.245 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Adjusted R2 | 0.258 | 0.260 | 0.248 | 0.252 | 0.254 | 0.241 | 0.242 | 0.223 |
| Hansen J | 0.246 | 0.284 | 0.306 | 0.274 | 0.330 | 0.234 | 0.208 | 0.114 |
| K-P rank test | 2.19e-10 | 4.05e-10 | 3.99e-10 | 1.19e-09 | 2.29e-09 | 3.00e-09 | 9.36e-10 | 1.04e-10 |
| F statistic | 27.68 | 28.02 | 28.40 | 28.28 | 27.16 | 27.30 | 29.36 | 26.59 |
| C statistic | 0.00810 | 0.0110 | 0.0175 | 0.0321 | 0.204 | 0.124 | 0.101 | 0.169 |

Robust standard errors in parentheses

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

Some variables are never dropped from the GMM regressions because they are used to generate instruments

*Table 12 Robustness test to covariates introduction for the total revenue models in the 3km buffer (OLS regressions)*

| | OLS1 | OLS2 | OLS3 | OLS4 | OLS5 | OLS6 | OLS7 | OLS8 |
|---|---|---|---|---|---|---|---|---|
| Log-Fire density (non fire user) | -0.534*** | -0.546*** | -0.524*** | -0.508*** | -0.517** | -0.571*** | -0.568*** | -0.646*** |
| | (0.197) | (0.200) | (0.197) | (0.196) | (0.199) | (0.209) | (0.213) | (0.210) |
| Log-Fire density (fire user) | -0.0220 | -0.0554 | -0.0329 | -0.0222 | -0.00184 | -0.00896 | -0.00981 | 0.0319 |
| | (0.0768) | (0.0741) | (0.0597) | (0.0569) | (0.0551) | (0.0506) | (0.0514) | (0.0502) |
| Fire user | -1.876*** | -1.714** | -1.725** | -1.698** | -1.783** | -1.999*** | -1.982*** | -2.444*** |
| | (0.674) | (0.696) | (0.696) | (0.693) | (0.705) | (0.748) | (0.759) | (0.736) |
| Capital (pc) | 0.114*** | 0.115*** | 0.115*** | 0.121*** | 0.122*** | 0.138*** | 0.136*** | 0.157*** |
| | (0.0285) | (0.0304) | (0.0301) | (0.0291) | (0.0297) | (0.0273) | (0.0269) | (0.0267) |
| Log-Labour days | -0.0365 | -0.0290 | -0.0227 | -0.0237 | -0.0162 | -0.0265 | -0.0206 | -0.0161 |
| | (0.0357) | (0.0362) | (0.0340) | (0.0340) | (0.0327) | (0.0316) | (0.0309) | (0.0307) |
| No labour | -0.653*** | -0.569*** | -0.555** | -0.554** | -0.527** | -0.541** | -0.514** | -0.498** |
| | (0.213) | (0.218) | (0.218) | (0.219) | (0.215) | (0.222) | (0.222) | (0.223) |
| Log-Farm size | 0.145** | 0.114** | 0.115** | 0.115** | 0.122** | 0.0866 | 0.109** | 0.0728 |
| | (0.0567) | (0.0570) | (0.0570) | (0.0572) | (0.0569) | (0.0569) | (0.0519) | (0.0520) |
| Education (years) | 0.0159 | 0.00414 | 0.00451 | 0.00349 | 0.00212 | 0.0153 | 0.0151 | 0.00492 |
| | (0.0183) | (0.0188) | (0.0186) | (0.0186) | (0.0186) | (0.0179) | (0.0179) | (0.0180) |
| Age household head | 0.0107*** | 0.0191*** | 0.0186*** | 0.0190*** | 0.0190*** | 0.0195*** | 0.0191*** | 0.0202*** |
| | (0.00410) | (0.00422) | (0.00407) | (0.00406) | (0.00409) | (0.00399) | (0.00397) | (0.00405) |
| Male household head | -0.0815 | -0.103 | -0.105 | -0.0977 | -0.105 | -0.0666 | -0.0702 | -0.113 |
| | (0.137) | (0.139) | (0.138) | (0.138) | (0.137) | (0.127) | (0.127) | (0.125) |
| Household size | 0.0388** | 0.0480*** | 0.0457*** | 0.0472*** | 0.0452*** | 0.0394** | 0.0383** | 0.0473*** |
| | (0.0156) | (0.0152) | (0.0152) | (0.0150) | (0.0151) | (0.0155) | (0.0155) | (0.0153) |
| Soil quality | 0.115** | 0.148*** | 0.151** | 0.145** | 0.147** | 0.215*** | 0.208*** | |
| | (0.0523) | (0.0571) | (0.0594) | (0.0586) | (0.0584) | (0.0647) | (0.0636) | |
| Slope | 0.00774 | 0.00693 | 0.00647 | 0.00591 | 0.00627 | 0.00424 | | |
| | (0.00611) | (0.00643) | (0.00653) | (0.00636) | (0.00638) | (0.00630) | | |
| Log-Distance to the city | -0.252*** | -0.192** | -0.190** | -0.191** | -0.190** | | | |
| | (0.0825) | (0.0868) | (0.0880) | (0.0883) | (0.0891) | | | |
| Technical assistance | 0.0765 | 0.129 | 0.148 | 0.163 | | | | |
| | (0.117) | (0.117) | (0.113) | (0.111) | | | | |
| Water access on farm | 0.0900 | 0.0886 | 0.0935 | | | | | |
| | (0.108) | (0.111) | (0.111) | | | | | |
| Paragominas | 0.136 | 0.0992 | | | | | | |
| | (0.161) | (0.164) | | | | | | |
| Receives government transfer | 0.620*** | | | | | | | |
| | (0.154) | | | | | | | |
| Constant | 9.619*** | 9.715*** | 9.678*** | 9.661*** | 9.680*** | 9.942*** | 9.941*** | 10.29*** |
| | (0.799) | (0.820) | (0.820) | (0.813) | (0.823) | (0.862) | (0.869) | (0.852) |
| Observations | 301 | 313 | 313 | 313 | 313 | 333 | 333 | 333 |
| R-squared | 0.472 | 0.413 | 0.413 | 0.411 | 0.408 | 0.392 | 0.391 | 0.369 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Adjusted R2 | 0.438 | 0.380 | 0.381 | 0.381 | 0.380 | 0.367 | 0.368 | 0.347 |

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

*Table 12 continued: Robustness test to covariates introduction for the total revenue models in the 3km buffer (OLS regressions)*

| | OLS9 | OLS10 | OLS11 | OLS12 | OLS13 | OLS14 | OLS15 |
|---|---|---|---|---|---|---|---|
| Log-Fire density (non fire user) | -0.653*** | -0.668*** | -0.677*** | -0.738*** | -0.699*** | -0.676*** | -0.741*** |
| | (0.201) | (0.200) | (0.216) | (0.218) | (0.223) | (0.222) | (0.231) |
| Log-Fire density (fire user) | 0.0132 | 0.00917 | -0.0271 | -0.0419 | -0.0149 | -0.0106 | 0.000530 |
| | (0.0500) | (0.0500) | (0.0480) | (0.0478) | (0.0421) | (0.0428) | (0.0435) |
| Fire user | -2.395*** | -2.430*** | -2.298*** | -2.452*** | -2.386*** | -2.321*** | -3.046*** |
| | (0.712) | (0.710) | (0.759) | (0.742) | (0.760) | (0.755) | (0.777) |
| Capital (pc) | 0.160*** | 0.159*** | 0.164*** | 0.154*** | 0.167*** | 0.173*** | |
| | (0.0274) | (0.0274) | (0.0284) | (0.0300) | (0.0307) | (0.0299) | |
| Log-Labour days | -0.0104 | -0.0155 | -0.0219 | -0.0347 | -0.0259 | | |
| | (0.0307) | (0.0308) | (0.0307) | (0.0305) | (0.0301) | | |
| No labour | -0.367 | -0.391* | -0.407* | -0.446** | -0.426* | | |
| | (0.223) | (0.224) | (0.226) | (0.227) | (0.230) | | |
| Log-Farm size | 0.0840 | 0.0800 | 0.0657 | 0.0584 | | | |
| | (0.0521) | (0.0517) | (0.0534) | (0.0545) | | | |
| Education (years) | 0.0115 | 0.0123 | 0.00292 | | | | |
| | (0.0175) | (0.0174) | (0.0178) | | | | |
| Age household head | 0.0183*** | 0.0177*** | | | | | |
| | (0.00395) | (0.00389) | | | | | |
| Male household head | -0.172 | | | | | | |
| | (0.127) | | | | | | |
| Household size | | | | | | | |
| Soil quality | | | | | | | |
| Slope | | | | | | | |
| Log-Distance to the city | | | | | | | |
| Technical assistance | | | | | | | |
| Water access on farm | | | | | | | |
| Paragominas | | | | | | | |
| Receives government transfer | | | | | | | |
| Constant | 10.51*** | 10.49*** | 11.54*** | 11.86*** | 11.85*** | 11.57*** | 12.14*** |
| | (0.819) | (0.817) | (0.763) | (0.757) | (0.765) | (0.729) | (0.760) |
| Observations | 333 | 333 | 333 | 349 | 349 | 349 | 349 |
| R-squared | 0.352 | 0.349 | 0.310 | 0.261 | 0.259 | 0.250 | 0.116 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 | 1.70e-05 |
| Adjusted R2 | 0.332 | 0.331 | 0.293 | 0.246 | 0.246 | 0.242 | 0.109 |

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

*Table 13 Robustness test to covariates introduction for the total revenue models in the 3km buffer (IV regressions)*

| | IVgmm1 | IVgmm2 | IVgmm3 | IVgmm4 | IVgmm5 | IVgmm6 | IVgmm7 | IVgmm8 | IVgmm9 |
|---|---|---|---|---|---|---|---|---|---|
| Log-Fire density (non fire user) | -0.712*** | -0.751*** | -0.705*** | -0.692*** | -0.668*** | -0.684*** | -0.685*** | -0.719*** | -0.705*** |
| | (0.138) | (0.142) | (0.136) | (0.134) | (0.136) | (0.136) | (0.137) | (0.140) | (0.160) |
| Log-Fire density (fire user) | -0.0984 | -0.139** | -0.0793 | -0.0701 | -0.0592 | -0.0463 | -0.0545 | -0.0811 | -0.0978* |
| | (0.0751) | (0.0664) | (0.0566) | (0.0553) | (0.0543) | (0.0536) | (0.0534) | (0.0522) | (0.0574) |
| Fire user | -2.112*** | -1.997*** | -2.050*** | -2.036*** | -1.985*** | -2.138*** | -2.106*** | -2.148*** | -2.268*** |
| | (0.424) | (0.436) | (0.435) | (0.426) | (0.435) | (0.464) | (0.469) | (0.482) | (0.568) |
| Capital (pc) | 0.0979*** | 0.0948*** | 0.0980*** | 0.101*** | 0.0994*** | 0.102*** | 0.102*** | 0.108*** | |
| | (0.0204) | (0.0236) | (0.0230) | (0.0222) | (0.0233) | (0.0248) | (0.0249) | (0.0256) | |
| Log-Labour days | -0.0136 | -0.0216 | -0.00657 | -0.00589 | 0.00392 | 0.0105 | 0.00441 | | |
| | (0.0312) | (0.0319) | (0.0304) | (0.0303) | (0.0295) | (0.0297) | (0.0289) | | |
| No labour | -0.574*** | -0.532*** | -0.514** | -0.504** | -0.467** | -0.343* | -0.360* | | |
| | (0.200) | (0.204) | (0.204) | (0.204) | (0.201) | (0.203) | (0.202) | | |
| Log-Farm size | 0.148*** | 0.134*** | 0.118** | 0.117** | 0.123** | 0.128*** | 0.125*** | 0.153*** | 0.272*** |
| | (0.0479) | (0.0477) | (0.0473) | (0.0477) | (0.0479) | (0.0482) | (0.0482) | (0.0496) | (0.0479) |
| Education (years) | 0.0285* | 0.0140 | 0.0143 | 0.0132 | 0.0123 | 0.0170 | 0.0187 | 0.0125 | 0.0163 |
| | (0.0150) | (0.0146) | (0.0146) | (0.0144) | (0.0146) | (0.0140) | (0.0139) | (0.0139) | (0.0145) |
| Age household head | 0.0105*** | 0.0160*** | 0.0153*** | 0.0155*** | 0.0159*** | 0.0152*** | 0.0143*** | 0.0126*** | 0.0126*** |
| | (0.00383) | (0.00367) | (0.00358) | (0.00358) | (0.00361) | (0.00358) | (0.00348) | (0.00349) | (0.00372) |
| Male household head | -0.0869 | -0.104 | -0.103 | -0.0957 | -0.114 | -0.196 | | | |
| | (0.122) | (0.124) | (0.124) | (0.123) | (0.123) | (0.122) | | | |
| Household size | 0.0296** | 0.0403*** | 0.0350*** | 0.0364*** | 0.0360*** | | | | |
| | (0.0128) | (0.0125) | (0.0122) | (0.0119) | (0.0120) | | | | |
| Soil quality | 0.0890*** | 0.116*** | 0.114** | 0.113** | 0.117** | 0.120*** | 0.137*** | 0.137*** | 0.181*** |
| | (0.0306) | (0.0418) | (0.0448) | (0.0451) | (0.0457) | (0.0446) | (0.0416) | (0.0410) | (0.0413) |
| Slope | 0.00616 | 0.00529 | 0.00460 | 0.00417 | 0.00399 | 0.00365 | 0.00368 | 0.00473 | 0.00345 |
| | (0.00470) | (0.00473) | (0.00474) | (0.00468) | (0.00468) | (0.00471) | (0.00467) | (0.00467) | (0.00518) |
| Log-Distance to the city | -0.303*** | -0.243*** | -0.234*** | -0.237*** | -0.241*** | -0.216*** | -0.201*** | -0.195*** | -0.347*** |
| | (0.0581) | (0.0598) | (0.0612) | (0.0608) | (0.0623) | (0.0637) | (0.0617) | (0.0620) | (0.0586) |
| Technical assistance | -0.00804 | 0.0747 | 0.105 | 0.115 | | | | | |
| | (0.0974) | (0.0981) | (0.0961) | (0.0933) | | | | | |
| Water access on farm | 0.0604 | 0.0634 | 0.0626 | | | | | | |
| | (0.0945) | (0.0961) | (0.0961) | | | | | | |
| Paragominas | 0.185 | 0.212 | | | | | | | |
| | (0.143) | (0.140) | | | | | | | |
| Receives government transfer | 0.506*** | | | | | | | | |
| | (0.126) | | | | | | | | |
| Constant | 10.15*** | 10.41*** | 10.36*** | 10.34*** | 10.23*** | 10.47*** | 10.38*** | 10.48*** | 10.30*** |
| | (0.583) | (0.596) | (0.586) | (0.577) | (0.578) | (0.588) | (0.599) | (0.551) | (0.616) |
| Observations | 301 | 313 | 313 | 313 | 313 | 313 | 313 | 313 | 313 |
| R-squared | 0.456 | 0.400 | 0.399 | 0.398 | 0.394 | 0.383 | 0.382 | 0.368 | 0.320 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Adjusted R2 | 0.421 | 0.365 | 0.367 | 0.368 | 0.366 | 0.356 | 0.357 | 0.348 | 0.300 |
| Hansen J | 0.163 | 0.0949 | 0.0735 | 0.0783 | 0.0748 | 0.143 | 0.174 | 0.168 | 0.344 |
| K-P rank test | 2.84e-09 | 2.19e-10 | 4.05e-10 | 3.99e-10 | 1.19e-09 | 2.29e-09 | 3.00e-09 | 9.36e-10 | 1.04e-10 |
| F statistic | 23.67 | 27.68 | 28.02 | 28.40 | 28.28 | 27.16 | 27.30 | 29.36 | 26.59 |
| C statistic | 0.257 | 0.262 | 0.242 | 0.247 | 0.287 | 0.283 | 0.326 | 0.235 | 0.270 |

Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

*Table 14 Robustness to alternative definition of capital (Farm revenue)*

| | OLS 3km | GMM 3km | OLS 4km | GMM 4km | OLS 5km | GMM 5km |
|---|---|---|---|---|---|---|
| Log-Fire density (non fire user) | -0.465 | -0.307 | -0.457 | -0.487 | -0.563 | -0.825** |
| | (0.398) | (0.317) | (0.425) | (0.365) | (0.441) | (0.383) |
| Log-Fire density (fire user) | 0.0128 | -0.178** | -0.0763 | -0.223*** | -0.127 | -0.264*** |
| | (0.105) | (0.0905) | (0.100) | (0.0763) | (0.0951) | (0.0675) |
| Fire user | -1.431 | -0.331 | -1.304 | -0.890 | -1.652 | -2.114 |
| | (1.296) | (1.021) | (1.570) | (1.328) | (1.745) | (1.494) |
| Capital pc (tract+fert+pest) | 0.259*** | 0.175*** | 0.254*** | 0.198*** | 0.253*** | 0.198*** |
| | (0.0630) | (0.0487) | (0.0660) | (0.0482) | (0.0662) | (0.0483) |
| Capital pc (cattle ranching) | -0.0837 | -0.0180 | -0.0837 | -0.0263 | -0.0859 | -0.0254 |
| | (0.113) | (0.0955) | (0.110) | (0.0964) | (0.106) | (0.0920) |
| Log-Labour days | -0.00478 | -0.0348 | -0.0107 | -0.0487 | -0.0168 | -0.0679 |
| | (0.0652) | (0.0552) | (0.0655) | (0.0550) | (0.0657) | (0.0550) |
| No labour | -0.517 | -0.705** | -0.513 | -0.703* | -0.516 | -0.764** |
| | (0.415) | (0.359) | (0.416) | (0.359) | (0.417) | (0.360) |
| Log-Farm size | 0.234** | 0.262*** | 0.249*** | 0.280*** | 0.264*** | 0.295*** |
| | (0.0906) | (0.0812) | (0.0918) | (0.0815) | (0.0924) | (0.0820) |
| Education (years) | -0.00561 | 0.0125 | -0.00537 | 0.00155 | -0.00462 | 0.00380 |
| | (0.0308) | (0.0211) | (0.0310) | (0.0206) | (0.0312) | (0.0206) |
| Age household head | -0.0218*** | -0.0217*** | -0.0221*** | -0.0220*** | -0.0224*** | -0.0221*** |
| | (0.00652) | (0.00510) | (0.00658) | (0.00506) | (0.00656) | (0.00507) |
| Male household head | 0.577** | 0.519** | 0.570** | 0.463** | 0.566** | 0.457* |
| | (0.262) | (0.238) | (0.263) | (0.236) | (0.263) | (0.237) |
| Household size | 0.0198 | 0.0185 | 0.0180 | 0.0219 | 0.0168 | 0.0182 |
| | (0.0233) | (0.0201) | (0.0233) | (0.0201) | (0.0232) | (0.0200) |
| Soil quality | 0.291*** | 0.337*** | 0.305*** | 0.320*** | 0.315*** | 0.335*** |
| | (0.0829) | (0.0543) | (0.0826) | (0.0522) | (0.0832) | (0.0520) |
| Slope | 0.0205* | 0.0213*** | 0.0211** | 0.0233*** | 0.0214** | 0.0236*** |
| | (0.0104) | (0.00809) | (0.0104) | (0.00754) | (0.0103) | (0.00736) |
| Log-Distance to the city | -0.330** | -0.330*** | -0.334** | -0.321*** | -0.335*** | -0.314*** |
| | (0.132) | (0.104) | (0.129) | (0.100) | (0.128) | (0.0986) |
| Technical assistance | -0.0310 | 0.0836 | -0.0263 | 0.0434 | -0.0331 | -0.00915 |
| | (0.223) | (0.176) | (0.224) | (0.153) | (0.225) | (0.147) |
| Water access on farm | 0.577*** | 0.486*** | 0.589*** | 0.489*** | 0.596*** | 0.480*** |
| | (0.169) | (0.151) | (0.172) | (0.151) | (0.172) | (0.149) |
| Paragominas | -0.201 | -0.0364 | -0.0991 | 0.0900 | -0.0256 | 0.211 |
| | (0.253) | (0.209) | (0.260) | (0.208) | (0.264) | (0.208) |
| Constant | 8.996*** | 8.586*** | 9.167*** | 9.425*** | 9.727*** | 10.95*** |
| | (1.361) | (1.153) | (1.624) | (1.433) | (1.831) | (1.603) |
| Observations | 313 | 313 | 313 | 313 | 313 | 313 |
| R-squared | 0.352 | 0.332 | 0.351 | 0.340 | 0.354 | 0.342 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 |
| F statistic Fire risk non fire users | | 58.24 | | 66.2 | | 57.62 |
| F statistic Fire risk fire users | | 50.83 | | 58.55 | | 62.14 |
| F statistic Fire user | | 40.42 | | 48.99 | | 52.49 |
| F statistic Capital factor (mechanization) | | 29.37 | | 30.92 | | 32.85 |
| F statistic Capital factor (Cattle) | | 296.34 | | 254 | | 246.13 |
| Adjusted R2 | 0.312 | 0.291 | 0.312 | 0.300 | 0.315 | 0.302 |
| Hansen J | | 0.124 | | 0.107 | | 0.125 |
| K-P rank test | | 4.87e-09 | | 2.77e-08 | | 2.42e-08 |
| F statistic | | 36.34 | | 40.95 | | 38.70 |
| C statistic | | 0.000475 | | 0.00324 | | 0.00255 |
| Wald test difference (p-value) | 0.2377 | 0.6826 | 0.3704 | 0.463 | 0.3148 | 0.1310 |

Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

*Table 15 Robustness to alternative definition of capital (Total revenue)*

| | OLS 3km | GMM 3km | OLS 4km | GMM 4km | OLS 5km | GMM 5km |
|---|---|---|---|---|---|---|
| Log-Fire density (non fire user) | -0.467** | -0.508*** | -0.491** | -0.497*** | -0.500** | -0.455*** |
| | (0.202) | (0.144) | (0.214) | (0.158) | (0.234) | (0.174) |
| Log-Fire density (fire user) | -0.0310 | -0.0467 | -0.0705 | -0.0903 | -0.0889 | -0.0933* |
| | (0.0793) | (0.0610) | (0.0767) | (0.0592) | (0.0738) | (0.0556) |
| Fire user | -1.686** | -1.693*** | -1.844** | -1.738*** | -1.952** | -1.703** |
| | (0.701) | (0.474) | (0.848) | (0.573) | (0.989) | (0.677) |
| Capital pc (tract+fert+pest) | 0.169*** | 0.192*** | 0.164*** | 0.205*** | 0.169*** | 0.210*** |
| | (0.0330) | (0.0273) | (0.0387) | (0.0323) | (0.0400) | (0.0340) |
| Capital pc (cattle ranching) | 0.0522 | 0.0849*** | 0.0560* | 0.0826*** | 0.0549* | 0.0777*** |
| | (0.0329) | (0.0189) | (0.0311) | (0.0169) | (0.0293) | (0.0165) |
| Log-Labour days | -0.0330 | -0.0155 | -0.0358 | -0.0198 | -0.0373 | -0.0221 |
| | (0.0354) | (0.0270) | (0.0351) | (0.0274) | (0.0351) | (0.0273) |
| No labour | -0.706*** | -0.665*** | -0.701*** | -0.661*** | -0.694*** | -0.666*** |
| | (0.210) | (0.188) | (0.211) | (0.190) | (0.211) | (0.189) |
| Log-Farm size | 0.211*** | 0.153*** | 0.219*** | 0.159*** | 0.227*** | 0.161*** |
| | (0.0576) | (0.0419) | (0.0580) | (0.0423) | (0.0581) | (0.0426) |
| Education (years) | 0.0278 | 0.0167 | 0.0282* | 0.0160 | 0.0291* | 0.0173 |
| | (0.0170) | (0.0119) | (0.0169) | (0.0120) | (0.0169) | (0.0121) |
| Age household head | 0.0106** | 0.00912** | 0.0106** | 0.00992*** | 0.0106** | 0.0101*** |
| | (0.00433) | (0.00370) | (0.00433) | (0.00365) | (0.00435) | (0.00363) |
| Male household head | -0.0533 | -0.0893 | -0.0620 | -0.0779 | -0.0670 | -0.0743 |
| | (0.139) | (0.111) | (0.140) | (0.114) | (0.140) | (0.116) |
| Household size | 0.0375** | 0.0363*** | 0.0364** | 0.0336*** | 0.0358** | 0.0349*** |
| | (0.0154) | (0.0124) | (0.0154) | (0.0123) | (0.0154) | (0.0126) |
| Soil quality | 0.114** | 0.0548* | 0.121** | 0.0481 | 0.128** | 0.0521 |
| | (0.0539) | (0.0330) | (0.0553) | (0.0356) | (0.0563) | (0.0383) |
| Slope | 0.00657 | 0.00697 | 0.00698 | 0.00799* | 0.00702 | 0.00799* |
| | (0.00625) | (0.00445) | (0.00620) | (0.00443) | (0.00617) | (0.00445) |
| Log-Distance to the city | -0.312*** | -0.307*** | -0.316*** | -0.325*** | -0.323*** | -0.339*** |
| | (0.0798) | (0.0502) | (0.0782) | (0.0507) | (0.0776) | (0.0511) |
| Technical assistance | 0.0577 | -0.0145 | 0.0596 | -0.0257 | 0.0588 | -0.0349 |
| | (0.118) | (0.0885) | (0.118) | (0.0886) | (0.118) | (0.0884) |
| Water access on farm | 0.225** | 0.195** | 0.227** | 0.208** | 0.226** | 0.197** |
| | (0.104) | (0.0833) | (0.106) | (0.0849) | (0.107) | (0.0859) |
| Paragominas | 0.112 | 0.124 | 0.168 | 0.216 | 0.196 | 0.246* |
| | (0.165) | (0.137) | (0.167) | (0.133) | (0.165) | (0.131) |
| Receives government transfer | 0.626*** | 0.568*** | 0.619*** | 0.543*** | 0.611*** | 0.521*** |
| | (0.158) | (0.115) | (0.156) | (0.109) | (0.157) | (0.111) |
| Receives remittances | 0.309** | 0.343*** | 0.315** | 0.411*** | 0.320** | 0.415*** |
| | (0.150) | (0.107) | (0.150) | (0.103) | (0.150) | (0.105) |
| Constant | 9.095*** | 9.514*** | 9.395*** | 9.668*** | 9.597*** | 9.681*** |
| | (0.824) | (0.576) | (0.959) | (0.675) | (1.106) | (0.779) |
| Observations | 300 | 300 | 300 | 300 | 300 | 300 |
| R-squared | 0.474 | 0.463 | 0.474 | 0.462 | 0.474 | 0.461 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 |
| F statistic Fire risk non fire users | | 60.3 | | 56.64 | | 61.20 |
| F statistic Fire risk fire users | | 44.24 | | 46.99 | | 55.38 |
| F statistic Fire user | | 65.61 | | 59.15 | | 64.44 |
| F statistic Capital factor (mechanization) | | 2425.33 | | 1045.17 | | 1122.65 |
| F statistic Capital factor (Cattle) | | 176.98 | | 155.73 | | 163.6 |
| Adjusted R2 | 0.436 | 0.424 | 0.436 | 0.423 | 0.436 | 0.423 |
| Hansen J | | 0.147 | | 0.113 | | 0.106 |
| K-P rank test | | 7.06e-09 | | 1.06e-10 | | 8.00e-11 |
| F statistic | | 28.07 | | 42.13 | | 68.02 |
| C statistic | | 0.147 | | 0.125 | | 0.0985 |
| Wald test difference (p-value) | 0.0186 | 0.0017 | 0.0225 | 0.0093 | 0.0497 | 0.0324 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

To robustness test capital principal factor definition, we shrink the amount of variables analysed and cluster them in two main component, one reflecting mechanization (tractor, fertilizer and pesticide use) and one reflecting cattle ranching (cattle-heads and wire fences.)

*Table 16 PCA on selected asset components*

| | Eigenvalue | Difference | Proportion | Cumulative |
|---|---|---|---|---|
| Comp1 | 2.03999 | 0.398919 | 0.408 | 0.408 |
| Comp2 | 1.64108 | 1.06213 | 0.3282 | 0.7362 |
| Comp3 | 0.578944 | 0.163806 | 0.1158 | 0.852 |
| Comp4 | 0.415138 | 0.090292 | 0.083 | 0.935 |
| Comp5 | 0.324847 | . | 0.065 | 1 |

*Table 17 Factor loadings*

| | Un-rotated components | | | | | |
|---|---|---|---|---|---|---|
| Variable | Comp1 | Comp2 | Comp3 | Comp4 | Comp5 | Unexplained |
| Cattle-heads 2009 | 0.1748 | 0.6824 | -0.0391 | 0.3223 | 0.6311 | 0 |
| Km wire fences | 0.211 | 0.6646 | 0.2162 | -0.249 | -0.6365 | 0 |
| Value of fertilizer input | 0.5617 | -0.234 | 0.2546 | 0.7091 | -0.249 | 0 |
| Value of pesticide input | 0.5639 | -0.191 | 0.4627 | -0.5496 | 0.3597 | 0 |
| Tractor use in 2009 | 0.5398 | -0.0378 | -0.8203 | -0.1709 | -0.0723 | 0 |

| | Rotated components (Varimax rotation on first two components) | |
|---|---|---|
| Variable | Comp1 | Comp2 |
| Cattle-head | -0.0245 | 0.704 |
| Km wire fences | 0.0153 | 0.6971 |
| Value of fertilizer input | 0.6049 | -0.0663 |
| Value of Pesticide input | 0.5949 | -0.0244 |
| Tractor use in 2009 | 0.5285 | 0.1158 |

# Amazonian farmers' response to fire policies and climate change

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Abstract

Despite the fall in deforestation, frequency and severity of fires in the Brazilian Amazon are rising, causing huge carbon emissions, biodiversity losses and local economic costs. The ignition sources are anthropogenic and mostly related to the accidental spread of agricultural fires. Fire mitigation is a coordination problem with strategic complementarities: a farmer's benefit of mitigation depends on complementary action of other farmers. We experimentally assess the impact of two different policies under varying exogenous drought risk scenarios. Command and control is more effective than payments for environmental services in promoting coordination, possibly because of participants' risk aversion (to the fine) and a local demand for justice and law enforcement. We also find evidence of a human-mediated self-reinforcing loop of droughts and fires: droughts increase the exogenous component of fire risk, giving farmers less incentives to mitigate fire risk coming from their own farms and to invest in fire-free techniques.

Keywords: Brazilian Amazon, forest fires, climate change, framed field experiment, coordination games

JEL codes: C93, Q23, Q54

# 1 Introduction

Tropical forests are burning (Cochrane, 2003; Coe et al., 2013). The South and Eastern Amazon, despite an 80% fall in deforestation between 2004 and 2012, has experienced a clear upward trend in the number and extension of forest fires for the last two decades (Alencar et al., 2015; Aragao and Shimabukuro, 2010; Malhi et al., 2008; Morton et al., 2013). Fire plays a key role in Amazonian smallholders' livelihoods and it is widely used for land clearing, weed control and fertilization (Börner et al., 2007; Carmenta et al., 2013). Each time fire is used for agriculture or pasture maintenance, however, it represents a potential ignition source for forest fires (Cano-Crespo et al., 2015; Diaz et al., 2002). Such accidental fires double biodiversity losses from deforestation (Barlow et al., 2016), reducing the forest environmental services and the natural resource base of local communities (Barlow et al., 2012). Fires reduce up to 40% of the potential carbon stock of standing forest and generate large $CO_2$ emissions (Barlow et al., 2012; Berenguer et al., 2014). The 2010 fires alone generated $510 \pm 120$ MtC emissions (Anderson et al., 2015), equivalent to ca. 5% of the global carbon emissions. Fires can destroy agricultural produce and infrastructures, and make local populations suffer from health problems (de Mendonça et al., 2004; Hoch et al., 2009; Hoch et al., 2012; Nepstad et al., 2001; Nepstad et al., 1999; Pokorny et al., 2012; Smith et al., 2014).

Understanding fire use and control in private properties is key for the Amazon conservation. About 45% of the Amazon forest is under a special protection regime, but most fires occur outside them (Nelson and Chomitz, 2011; Nepstad et al., 2006; Soares-Filho et al., 2010). Private properties normally have abundant flammable vegetation (Alencar et al., 2015; Cochrane, 2003) with potentially high conservation value (Chazdon et al., 2009; Karthik et al., 2009; Moura et al., 2013; Parry et al., 2007) but difficult to govern (Vieira et al., 2014). Tasker and Arima (2016) show that anti deforestation policies probably reduced the number of agricultural fires, but did not reduce the area burnt due to the high frequency of accidental forest fires. Supplementary policies to target fires are needed (Barlow et al., 2012).

This paper reports on a framed field experiment (FFE) about fire use on private lands in the Brazilian Amazon. Similarly to Morello et al. (2017) but with a different method (FFE vs. agent-based model), we provide an *ex-ante* assessment of two key fire-preventing policies: Command and Control (CAC) and Payment for Environmental Services (PES). These two experimental treatments emulate highly relevant fire mitigation policies whose impact is mostly unknown (Morello et al., 2017).

CAC has been a cornerstone in the Brazilian efforts to end deforestation (Börner et al., 2015). Yet, there is little law enforcement concerning fires. The Brazilian Forest Code (Chap. IX, law 12651/2012) is the main legislation on fire use and control. Each state is responsible for the enforcement of the Forest Code on private properties, and its implementation varies. Mato Grosso, Acre, Roraima and Amapá has special fire management committees, while Pará has limited infrastructure to enforce the law. Municipalities could play an important role in fire prevention and suppression, as institutions closer to the forest areas and concerned populations.

PES represents the remuneration schemes for forest conservation and another key ingredient of the government's strategy to end deforestation, PPCDAM[1]. PES is also promoted by the international REDD+ initiative under the UNFCCC framework[2], and financed in Brazil through the Amazon Fund (Coudel et al., 2015; May, 2009). PES was pioneered in Brazil in 2003, and was recently relaunched with the *Bolsa verde* program (2011) and the *Assentamentos Sustentaveis na Amazonia* project (2015). PES has also been recognized as a forest conservation tool in the revised Forest Code of 2012 (Coudel et al., 2015), and has become integrated into NGO operations in the Amazon (e.g. Simonet et al., 2015). To effectively reduce forest losses, REDD+ initiatives should aim to reduce fire risk in the Amazon, but so far few specific institutional measures are in place (Barlow et al., 2012). Several challenges remain for measuring, reporting and verification, mainly because of unclear responsibilities and liabilities associated with fire events (Barlow et al., 2012).[3]

We examine these policies under a stable climate and a climate change scenario. Climate change is expected to increase drought risk in the Amazon (Dai, 2013; Malhi et al., 2008), which in turn increases fire occurrence and extension (Alencar et al., 2006; Brando et al., 2014; Brando et al., 2016; Davidson et al., 2012; Nepstad et al., 2004; Schwartz et al., 2015).

We design the experiment as a coordination game with strategic complementarities; the benefit of investing in risk mitigation depends on neighbors' complementary action(Cammelli and Garrett, in this thesis), generating a dilemma between a sure return and an uncertain social coordination outcome. The aggregate of local choices gives the neighborhood fire risk. The design also includes drought-induced, external fire risk. Nepstad et al. (2001) and Nepstad et al. (2008) hypothesize that neighbors' use of uncontrolled agricultural fire and drought conditions generate an external fire risk that reduces the individual incentives for fire-free agricultural practices. This might engender a feedback loop between higher expected losses, less fire control and thus even more fires. We test experimentally whether such a drought-fire self-reinforcing loop might exists. If it does, climate change will have a greater impact on fire risk than the one predicted by physical models that do not account for such a human-mediated climate effect.

Specifically, we aim to answer the following questions:

1. Is there a human-mediated, self-reinforcing loop between droughts (climate change) and fire use, as posited by Nepstad et al. (2001)?
2. Which policy is most effective in reducing fire risk by promoting fire control and fire-free practices, and how policies interact with drought risk?

Our results show that farmers react to drought risk and droughts occurrence by increasing uncontrolled fire use and reducing uptake of fire-free techniques. This suggests that the climate change impact on fires is partially human-mediated, but policies have the potential to break this loop. The enforcement of the Forest Code appears to be the most effective policy in reducing uncontrolled fires, for three reasons: it addresses directly the source of the externality –

[1] http://www.mma.gov.br/images/arquivo/80120/PPCDAm/_FINAL_PPCDAM.PDF Last accessed 05/05/16.
[2] http://redd.unfccc.int Last accessed 05/05/16.
[3] Morello et al., (2017) report further details on CAC and PES policies against forest fires in the Brazilian Amazon.

uncontrolled fire use – changing farmers' beliefs about fire risk, it is aligned with the farmers demand for justice, and it has a leverage on risk aversion (to the fine). Further, we find that a strong fire control norm and perceived fire risk and technological constraints play an important role in farmers' choices, suggesting external validity.

Section 2 discusses the economics of forest fires and related policies. Section 3 outlines the experimental design, treatments and theoretical predictions. Section 4 reports on the study site and sampling, section 5 on the results, while section 6 discusses the findings and section 7 concludes.

## 2 The brief economics of fires and fire policies

### 2.1 Background

Lacking natural ignitions, fires in the rainforest appear due to a combination of droughts and land use practices by local populations (Cano-Crespo et al., 2015; Nepstad et al., 2001; Nepstad et al., 2008; Schwartz et al., 2015; Soares-Filho et al., 2012). The underlying socioeconomic drivers of fires have been explored only to a limited degree (Carmenta et al., 2011). Research has concentrated on smallholders, possibly because large-holders are already undergoing a transition out of fire use, while smallholders appear to have less incentives and/or capacity to abandon fire-intensive agricultural practices in the short and medium term (cf. Medina et al., 2015).

Controlled fire is a cheap "voluntary worker", substituting for capital and labor in land preparation, pest control and soil mineralization in pastures and croplands (Nepstad et al. 1999). Many obstacles to fire-free alternatives are well documented and relate to poor market access, high costs and unavailability of labor and capital, and network externalities associated with local knowledge (Harwood, 1996; Hoch et al., 2009; Hoch et al., 2012; Pokorny et al., 2012).

The population responsible for igniting accidental fires are also the one suffering their most direct damages (Nepstad et al., 1999; Schroth et al., 2003). They should therefore also be the agents most concerned about fire prevention. This apparent paradox can be portrayed as a coordination problem (Cammelli and Garrett, in this thesis). The farmer's benefits of investing in controlled fire consists of the avoided damage of his own plot burning from his own fire (Bowman et al., 2008). The benefits depend on how he values the fire sensitive assets, including other crops, infrastructure and the forest itself (Nepstad et al., 2001). Bowman et al. (2008) show that farmers' investment in preventive firebreaks indeed depends on the value of the flammable assets at stake on their land.

Fires may also spill into the property from neighboring fields and forests, irrespectively of the preventive measures undertaken by the farmer himself[4]. We term this risk the *neighborhood fire hazard* (NFH) because it is determined endogenously by neighbors' choices. In our sample, about 41% (N=238) of fire damages originate from fires ignited by neighbors. The private benefit of investing in preventive fire control measures therefore depends on the NFH faced by the farmer: the lower the NFH, the higher the benefit of controlling the own fire because the residual risk of asset losses is low. Since decisions are taken before the NFH is observed, it is the farmer's *belief* about the NFH that matters. Another consequence of the fire control coordination problem is that

[4] We do not consider fire protection investments; for smallholders, they would be even more technically demanding than fire control, and we never observed them in the field.

high-yield, fire-free (but fire-sensitive) technologies might be unappealing to smallholders if the external fire risk is too high[5]. Pokorny et al. (2012) report that the fire risk to an agroforest during its productive life ranges between 15 and 60%. Schroth et al. (2003) report that 55% of the interviewed farmers remember loosing trees because of fires, and that risk is the main limiting factor to the establishment of rubber tree plantations. de Mendonça et al. (2004) find that about 16,000 km of pasture fences and between 6,500 and 19,400 $km^2$ of pastures were lost between 1996 and 1999 because of fires.

The fire control problem represents a coordination game with strategic complementarities[6], exhibiting two Nash equilibria (Shafran, 2008). The uncoordinated ('bad') equilibrium involves high fire risk, high fire use and low yield, while the coordinated ('good') equilibrium entails low fire risk, low fire use and high yield. The equilibrium selection is determined, *inter alia,* by farmers' incentives and ability to coordinate with their neighbors, which again is influenced by a number of factors, as we return to.

In addition to neighborhood fires, there is a second source of fire risk. During drought years, fires increase in frequency and extension (Alencar et al., 2015), spreading for several kilometers due to high fuel availability. Brazilian farmers refer to these fires as *fogo de longe* (fires [coming] from afar). They are responsible for about half of the fire damages reported in our survey.

Climate change is expected to increase the frequency of droughts (Dai, 2013; Malhi et al., 2008). A higher drought risk reduces the incentives for fire-free agricultural practices and fire control because it increases the probability that costly fire prevention is a wasted effort. Moreover, increasing drought risk *per se* might also affects agents' behavior by initiating a spiral of self-fulfilling negative expectations. If drought risk is expected to increase over time, more agents may stop controlling fire, which in turn increase the neighborhood risk. Anticipating future drought and changes in others' behavior might, in itself, trigger higher NFH today.

### 2.2 Analytical framework

Each farmer $i$ decides on the fraction $t_i$ of his privately endowed land to operate with fire, yielding a per-unit amount $f$, and the fraction $1 - t_i$ to operate with a fire-free technique, yielding a per-unit amount $a$. The fire-free technique gives a higher return ($a > f$), but has an associated cost $r$ and the investments are exposed to fire risk. Further, farmers can choose to adopt fire control management practices on the fraction of land $\varphi_i$ of $t_i$ ($\varphi_i \leq t_i$), at a cost $c$. Fire risk depends on the choices of the participant, the average choices of the others, and the exogenous drought risk

[5] Alternatives to fire use are generally more productive but also more fire sensitive (Bowman et al., 2008; Hoch et al., 2012; Nepstad et al., 2001; Nepstad et al., 1999). Intuitively, any productive system needs more (fire-sensitive) inputs than slash and burn agriculture. Pasture rotation involves more pasture fences, which are typically flammable (see De Mendonca et al., 2005). Perennial crops, tree harvesting and agroforestry involve sunk costs (initial investment and opportunity cost of land) and can be entirely lost in a fire. Losses would be higher on plots where chemical inputs and machineries have been employed to substitute fire use. Slash and mulch techniques leave wood debris on the ground increasing accidental fire risk during the burning period.

[6] Strategic complementarities occur when agents have incentives to coordinate on the same choice. Strategic substitutes occur when agents are better off coordinating on opposite choices. Both cases are discussed in the context of fire prevention investments by Shafran (2008).

probability $\theta$. The severity of the exogenous risk is given by the loss rate $\lambda$, i.e. the share of yield $(1-t_i)a$ that is lost.

The endogenous fire hazard, the fraction of $(1-t_i)a$ lost due to a locally initiated (endogenous) fire, is determined by the choices of the farmers and given by:

$$\left[\frac{(\varphi_i-t_i)+\frac{\sum_{-i}(\varphi_{-i}-t_{-i})}{n-1}}{2}+1\right]^2.$$

The endogenous fire hazard depends on the aggregate land operated with fire techniques and no fire control. The chosen functional form gives increasing benefits from fire control and allocation of land to alternative techniques as other neighboring farmers do the same.

The resulting payoff function is:

$$V_i(\varphi_i,\varphi_{-i},t_i,t_{-i};\ a,f,\theta,\lambda,r)=t_if+(1-t_i)a\left\{\left[\frac{(\varphi_i-t_i)+\frac{\sum_{-i}(\varphi_{-i}-t_{-i})}{n-1}}{2}+1\right]^2-\theta\lambda\right\}-\varphi_i c-\ (1-t_i)r$$

Under the conditions:

$$\left\{a,f,\theta,\lambda,r,\textstyle\sum_{-i}(\varphi_{-i}-t_{-i})\ |\ f<a(1-\theta_t\lambda)-r\ ;\ f>a\left[\left(\frac{\sum_{-i}\varphi_{-i}-t_{-i}}{2(n-1)}+1\right)^2-\theta\lambda\right]-r\ \right\},$$

this game has two Nash equilibria and exhibits strategic complementarities. The first condition states that allocating all land to fire-free techniques is an equilibrium when fire risk is sufficiently low. The second condition implies that operating all land with fire is optimal when NFH is sufficiently high.

The CAC treatment simulates the impact of enforcing the Forest Code prohibition of uncontrolled fires. In each treatment round there is a probability $v$ of a police control. If a participant choses $\varphi_i<t_i$ and a control takes place, he receives a fine of $(\varphi_i-t_i)z$ points.

The resulting payoff function is: $U(\varphi_i,\varphi_{-i},t_i,t_{-i};\ a,f,\theta,\lambda,r,v,z)=V_i(\varphi_i,\varphi_{-i},t_i,t_{-i})-(\varphi_i-t_i)vz$

PES makes alternatives to fire use more attractive to farmers, as in the *Bolsa Floresta* program described in Bakkegaard and Wunder (2014). The PES treatment covers a share $s$ of the costs associated with allocating land to fire-free-techniques.

The payoff function after the payment is: $U(\varphi_i,\varphi_{-i},t_i,t_{-i};a,f,\theta,\lambda,r,s)=V_t(\varphi_i,\varphi_{-i},t_i,t_{-i})+(1-t_i)s$

# 3 The experiment

## 3.1 Design

The experiment consists of 10 rounds. A group of 8 participants is told that they are neighboring farmers with a plot of land of equal size. In each round, participants choose anonymously and simultaneously among three cultivation technologies:

F ($t_i = 1, \varphi_i = 0$): fire use without prevention measures
CF ($t_i = 1, \varphi_i = 1$): controlled fire, fire with prevention measures
A ($t_i = 0, \varphi_i = 0$): alternatives to slash and burn, e.g., mechanization, agroforestry or pasture rotation.[7]

We calibrate the payoff functions as follows: fire-free agriculture yields $a = 250$, with an associated cost $r = 50$, fire-using techniques yield $f = 100$, with an associated control cost $c = 30$, the loss parameter $\lambda = 0.5$ and the exogenous risk $\theta \in [0, 0.1, 0.3, 0.5, 0.6]$. The resulting payoff tables are displayed in Table 1

Fire-intensive crops are not flammable, thus choices F and CF yield a constant payoff of 100 and 70, irrespective of drought occurrence and NFH (number of other participants choosing F). The payoff of choice A varies. When a drought occurs, the payoff is reduced. The left panel of Table represents the normal situation, while the right panel gives the payoff when a drought occurs. Within each of these two scenarios, the payoff of A depends on the other participants' choices (NFH). Without fire risk, the payoff of A is twice the one of F. Although there is uncertainty about the overall return of fire-free techniques (Morello et al., 2017), to calibrate our model we relied on information collected from agronomists working in the specific region of study (Nepstad et al., 1999; Coudel, personal communication).

*Table 1 Payoff table in the absence (left) or the occurrence (right) of a drought*

| NFH | A | F | CF |
|---|---|---|---|
| 0 | 200 | 100 | 70 |
| 1 | 166 | 100 | 70 |
| 2 | 134 | 100 | 70 |
| 3 | 104 | 100 | 70 |
| 4 | 78 | 100 | 70 |
| 5 | 53 | 100 | 70 |
| 6 | 32 | 100 | 70 |
| 7 | 13 | 100 | 70 |

| NFH | A | F | CF |
|---|---|---|---|
| 0 | 75 | 100 | 70 |
| 1 | 41 | 100 | 70 |
| 2 | 9 | 100 | 70 |
| 3 | 0 | 100 | 70 |
| 4 | 0 | 100 | 70 |
| 5 | 0 | 100 | 70 |
| 6 | 0 | 100 | 70 |
| 7 | 0 | 100 | 70 |

[7] In reality, most farmers adopt a wider variety of $t_i$ and $\varphi_i$ combinations. This enhance food security and allows adapting to available resources. However, there would have been no gain in realism by introducing additional choices in the experiment: complexity would have increased significantly, without changing the salience of any solution concept. Any combination of F, CF or A is strictly dominated by A or F.

After reading the instructions aloud to the participants, and before the experiment started, one trial round was played and the experimenter answered all questions raised. In each round, participants were asked to box-tick their choice of cultivation technology on a tip of paper, and to state the anticipated NFH. The task was designed such that also illiterate participants were comfortable in making the choice on their own, thus minimizing the interaction with the experimenter and possible Hawthorne effects. To minimize unwanted interactions, participants were sitting in a circle with the chairs turned outwards. Policy treatments were introduced by reading aloud further instructions and answering all questions.

All participants played five baseline rounds and five policy treatment rounds. No communication was permitted during the experiment. At the end of each round, the experimenter collected the paper with participants' choices, announced the frequency of each choice, the random realization of a drought or not, and the resulting payoff for each choice. Whether a drought occurred or not was determined by rolling a ten-sided dice, with the probability known to the participants.

Participants in half of the sessions played with stable drought risk while the other half faced increasing risk over the five rounds in each of the two stages of the experiment (Table 2-3). To avoid spillover effects across sessions we never repeated an experiment more than twice and a treatment more than once in the same community. We balanced the sample size across treatment groups (Table 2).

*Table 2 Treatments set-up and number of sessions for each treatment*

| Fire policy→<br>Drought risk↓ | CAC | PES | N |
|---|---|---|---|
| Constant | 12 | 12 | 24 |
| Increasing | 12 | 12 | 24 |
| N | 24 | 24 | 48 |

The CAC treatment was obtained by setting the probability of a police control $c = 1/3$ and the fine $z = 73$; PES was given by the amount of the payment, $s = 30$. Under this specification and the parameters definition, PES and CAC are theoretically equivalent and directly comparable, as we return to below.

Under the constant risk treatment, there was a 30% drought probability, i.e. $\theta = 0.3$. Under the increasing risk treatment, the probability rose from 0 to 60%, i.e. $\theta \in [0, 0.1, 0.3, 0.5, 0.6]$ (Table 3), such that the baseline and policy treatment rounds are risk-equivalent with an average of 30% for both the stable and increasing risk treatments. This allowed testing for an increasing-risk effect on participants' choices.

*Table 3 Drought risk distribution across rounds and risk treatments*

| | Baseline | | | | | Policy Treatment | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Rounds | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
| Stable | 30% | 30% | 30% | 30% | 30% | 30% | 30% | 30% | 30% | 30% |
| Increasing | 0% | 10% | 30% | 50% | 60% | 0% | 10% | 30% | 50% | 60% |

A short questionnaire was administered before and after the experiment to collect data about agricultural production, fire use and control, and sociodemographic characteristics.

The points earned in the experiment were converted into cash at the end of the session at a rate of 80 points = 1 BRL. Participants were thus able to earn up to 35 BRL[8], approximately one local daily wage for unskilled labor in agriculture. Detailed instructions are reported in the Appendix.

### 3.2 Theoretical predictions and hypotheses

The game exhibits two Pareto ranked Nash equilibria, F and A. CF is always strictly dominated, thus we expect to observe few CF choices. A high number of CF choices might evidence a social norm prescribing controlled fire or pro-social preferences and risk-aversion.

A rational, selfish pay-off maximizing and risk neutral agent will choose A or F depending on the expected risk of drought, and his belief about NFH (between 0 and 7). There exists a NFH belief threshold above which the expected return of A is lower than the sure return of F. Below that threshold the agent is expected to choose A, F otherwise (Table 4).

*Table 4 Theoretical NFH belief threshold for a risk neutral and selfish agent to switch from A to F*

| Risk | Baseline | CAC | PES |
|---|---|---|---|
| 0 | 4 | 5 | 5 |
| 10% | 3 | 4 | 4 |
| 30% | 2 | 3 | 3 |
| 50% | 2 | 3 | 3 |
| 60% | 1 | 2 | 2 |

A risk averse participant has a higher belief threshold than a risk neutral one. A pro-social participant experiences a lower belief threshold as he accounts for the damage imposed on others if playing F (Cammelli and Handberg, in this thesis). Rounds with high fire risk (30% - 60%) are expected to give lower frequencies of choice A and higher of choice F, because the belief threshold are lower than in low risk rounds (0 - 10%).

Because the two drought risk scenarios are equivalent on average for the risk neutral player, any difference between the two treatments in baseline rounds might be due to the effect of increasing risk *per se*. In coordination games, expectations about the other participants' action is critical for own choice (Van Huyck et al., 1990). We posit that increasing risk *per se* creates more uncertainty about other players choices compared to a stable risk level, because the payoff of choosing A diminishes over rounds. Participants might thus anticipate their opponents F choice and play F themselves, engendering a spiral of negative and self-fulfilling beliefs leading to more F choices.

In addition, when risk increases, risk-averse participants become less prone to choose A. This will work partly through reduced expected own payoffs and partly through changes in expectations of what other participants will choose, i.e., a change in the believed NFH. Climate change (i.e.,

[8] At the end of the survey (December 2015), the exchange rate was ca. 4 BRL = €1.

increasing drought risk) might therefore trigger negative expectations that make participants even more likely to choose F instead of A, beyond the mere effect of risk level.

The chosen parameter values ensure that CAC and PES are theoretically equivalent in the sense that, for a risk neutral, selfish player, the belief thresholds are the same (Table 4). Any difference across treatments impact can be ascribed to the institutional difference (framing) between CAC and PES, or to preferences.

Our treatments may affect choices through crowding in/out of intrinsic motivations. Vollan (2008) defines CAC and PES in a Common Pool Resource game as the first being restrictive and the second being enabling (of participants choices), thus different crowding in/out effects can be expected. However, Gneezy and Rustichini (2000) and Cardenas et al. (2000) find that both payments and law enforcement might crowd out social norms when these are internalized. In our experiment, both CAC and PES are enabling of A, because F is not the only Nash equilibrium.

The equivalence of CAC and PES do not hold for non-risk neutral players: risk aversion (loving) reduces (increases) the evaluation of F payoff for any positive probability of being fined. CAC is thus expected to be more effective on risk averse players than PES.

Policy and drought risk treatments may interact. Increasing risk should raise participants' believed NFH, while policies should reduce it. The impact of policies is expected to be higher under increasing rather than stable risk. The opposite might occur if drought risk impact is stronger than the policies impact.

We test all hypotheses both on choices and on beliefs because treatments affect both payoffs and framing, and the latter is expected to affect choices through beliefs (Dreber et al., 2013).

## 4 Study site and sampling

We sampled 576 smallholder farmers in 40 communities in the municipalities of Paragominas, Ipixuna, São Domingos do Capim, and Irituia in the state of Pará, Brazil. Among the 576 farmers, 384 participated in the experiments reported in this article.[9] Fieldwork was carried out in October - December 2015. Following other field experiments (Cardenas and Carpenter, 2008; Perz, 2004), participants were selected in collaboration with local leaders and community organizations, attempting to get a representative sample (true random sampling turned out to be very challenging practically).

Since the experiment involved social preferences and participants were sampled non-randomly, a major threat to experiment validity is the sensitivity to non-random social ties due to village leaders being involved in the selection. We asked the inviter not to discriminate participants based on friendship, gender, participation to local association or unions. When some of the invited participants did not show up (19%), the first author recruited back-up participants close to the experiment location. We test for differences in social ties and social capital across invited and

[9] The other 192 farmers underwent a third randomly assigned treatment reported in Cammelli and Handberg (in this thesis).

back-up participants using a logit regression (see Appendix). No difference in affiliation to associations or unions is detected, nor in having more friends or relatives in the group.

The sampled participants were active smallholders, aged between 17 and 81 years. 74% of them were males. The average plot size was 50 ha. 87% of the farmers cultivated annual crops, and among them, 83% used fire. 50% of the farmers harvested at least 0.5 ha of perennial crops, mainly açai, cashew and pepper. 69% of the farmers owned pasture, but only 16% of these maintained it with fire.

43% (of the full sample of 576 farmers) had suffered damages from accidental fires over the last five years. The following types of damage has occurred at least once: pastures 52%; pasture fences 46%; perennial crops 44%; annual crops 36%; and houses or other outbuildings 5% (N=240). Fires originated in only 6% of the cases from the own plot, in 41% of the cases from neighbors, and in 53% of the cases from "fires from afar", involving several properties (N=238). When the fire did not originate from own plot, farmers were able to identify the offender in 47% of the cases. Only 4% asked for compensation, and only 2% obtained it (N=228). There is little or no (formal or informal) enforcement system, nor farmers feel that it is legitimate to ask for compensations, fearing retaliation in other spheres of community life.

## 5 Results

We first analyze the impact of possible motivations for participants' choices: norms, perceived constraints, and beliefs. Second, we present a visual inspection of choices over rounds. Third, we test the impact of drought risk treatments and policies. Finally, we test the impact of risk levels on choices during baseline and policies treatment rounds.

### 5.1 Participants' motivations: norms, constraints and beliefs

In addition to the pay-off structure, we have hypothesized that social norms, feasibility constraints and beliefs about others' behavior are important for the choices made. We examine each of these three factors in turn to see if they have an impact on participants' behavior.

Summarizing across all 48 sessions, participants chose A (alternatives to fire use) with a frequency of 51%, F (uncontrolled fire) of 16%, and CF (controlled fire) of 33%. Since fire control is costly (F always gives higher payoff than CF), the high number of CF choices indicates strong social preferences and/or a norm prescribing controlled fire. On average, each time a participant chose CF he gave up, 49 points (38%) of potential earning (conditional on the believed NFH).

A follow-up questionnaire revealed that farmers indeed strongly perceived controlling fire to be a duty: on an increasing five points Likert scale, participants answered the question: "Do you think that controlling fire is a farmer duty?", with an average score of 4.81.

The high number of CF choices might also reflect a "feasibility constraint" that participants brought into the experiment. About 56% of the farmers reported that, in real life, they would not be able to stop using fire, even if a law would forbid its use. These farmers were indeed more likely to choose CF and less likely to choose A in the experiment (Table 5).

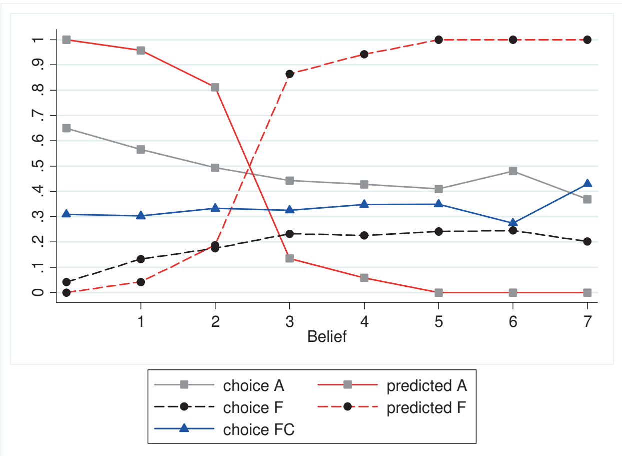
*Table 5 Summary statistics by choice (means or percentages)*

| | CF | F | A | All |
|---|---|---|---|---|
| Age (years) | 46 | 41 | 43 | 44 |
| Male (1 = male) | 74% | 73% | 76% | 75% |
| Plot size (ha) | 46 | 50 | 52 | 50 |
| No alternatives to fire | 63% | 59% | 50% | 56% |
| Fire user | 78% | 77% | 68% | 73% |
| Suffered fire damages | 41% | 38% | 46% | 43% |
| Fire control measures implemented (#) | 3.6 | 3.4 | 3.4 | 3.5 |
| Beliefs below the threshold | 53% | 33% | 61.% | 54% |

We also asked participants if they perceived accidental fires to be a threat for their property. Answers are heterogeneous and distributed at the extremes of the Likert scale. We found a strong correlation between perceived risk and perceived technological constraints (0.24, p<0.000) and between perceived risk and stated fire use (0.45, p<0.000). This supports one of our main hypothesis, namely that fire risk is a barrier to a transition out of fire use.

The third factor is participants' belief. Figure 1 shows that, as predicted, a higher anticipated neighborhood fire hazard (NFH) made a participant less likely to choose A and more likely to choose F, while the frequency of CF appears to be unaffected by beliefs (see also table 5).

*Figure 1 Theoretical predictions and actual choice probabilities on beliefs*
*(Predicted choice probabilities are not binary because switching points are averaged across policies and risk treatments)*




## 5.2 Choices over rounds

We introduced two alternative polices (PES and CAC) to promote a fire-free practice (A) or controlled fire (CF) under two drought risk scenario (increasing and stable risk). Figure 2 displays the frequencies of choices over time for each treatment group.

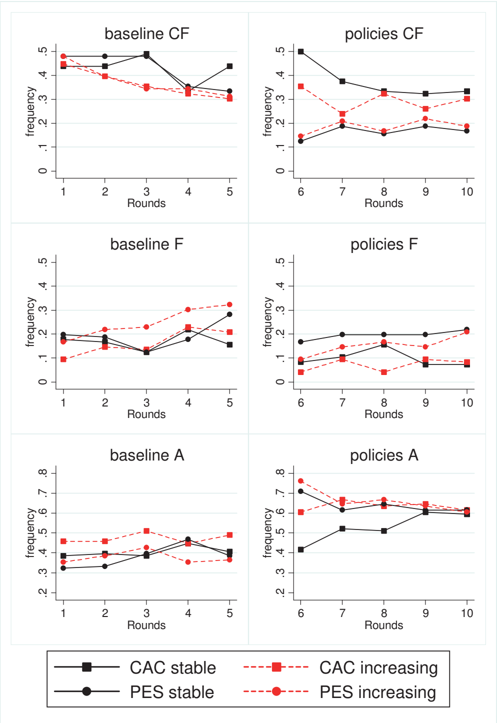
CF choices tend to decline during baseline rounds, especially under increasing risk. One explanation may be that players over time learned about actual payoffs and that the fire control norm weakened. Learning seemed especially important in the baseline rounds.

The introduction of PES reduced the number of CF choices, irrespective of the risk treatment, while CF choices did not decline under CAC. This might be the effect of an increase in salience of payoffs compared to the social norm, an institutional crowding out.

As expected, F choices increased in baseline rounds under the increasing risk treatment. Policies seemed to offset the increasing trend, and more so under CAC than PES.

Choices A remain constant during baseline rounds. The average policy impact is not visually distinguishable, because the effect varies over time and the choices converge in the last two rounds: PES has a high but decreasing impact, while the impact of CAC is increasing over rounds and stable across risk levels.

*Figure 2 Choices across rounds*



### 5.3 Policies and drought treatments impact

We turn to a more formal analysis of the relative merit of fire policies and drought risk treatments and of how they interact. Policies can impact behavior both by changing the payoffs directly and by changing the beliefs of participants, and we explicitly also test the latter effect.

We assess the impact of policies and increasing risk in a multinomial logit model. F is chosen as the base category because the natural policy question is how to reduce fire risk, i.e., the odds of choosing A or CF instead of F. To analyze beliefs about NFH, we used the same specification as for choices, but applied a Poisson regression as belief is a count variable.

Individual and session level correlations may lead to inconsistent and biased estimates and incorrect standard errors (Fréchette, 2012). We applied individual and session levels random effects as well as session clustered standard errors, which jointly capture the origin of both static and dynamic session level correlation (Fréchette, 2012). We always control for beliefs as these may be correlated with both the treatments and the outcome variables, and because they capture dynamic individual and session effects. Other control variables included individual age and gender as well as mean age and gender composition of the group. We also included a dummy if the participant stated to be a fire user, and a dummy if reported to have no access to real life alternatives to fire use. The inverse round trend (1/round) was added to control for learning, capturing both the round sequence as well as the difference between early and later rounds. To test for any adaptive expectations about external fire risk, and control for the uneven (although random) drought occurrences across sessions, we included the lag of the sum of drought frequency in previous rounds.

The impact of policies and increasing risk are estimated in the same model, as reported in the Appendix. Interactions are set to capture the full crossed-design of the experiment. The treatment impact of increasing compared to stable risk is estimated for baseline and policy treatment rounds and reported in Figure 3. Contrary to our predictions, increasing risk *per se* did not raise the NFH beliefs, nor did it have an impact on choices. An exception was the increase of choices A during the CAC treatment rounds, which also seems at odds with our predictions. The interaction between increasing risk and CAC is analyzed and discussed further below.

The non-significant impact of increasing risk may be best explained by participants inability to properly deal with probabilities, a concept proved to be difficult to understand (Slovic, 1987; Tversky and Kahneman, 1992). While the drought probabilities are fully explained to the participants in each round, they may rather adapt their expectations, to the recent drought exposure in the experiment. Indeed, we find that participants that were – by chance – more exposed to droughts in previous rounds played less A and CF (p=0.000 and p=0.013, respectively; Wald test of joint difference from zero). One explanation is that the drought experience increases their subjective probability of a drought occurring, and thus make them more likely to choose F.

*Figure 3 Increasing drought risk treatments impact*
*(Multinomial logit and Poisson regression, log odds ratio and log of expected count; 90% and 95% CI; full regression results are available in the Appendix)*

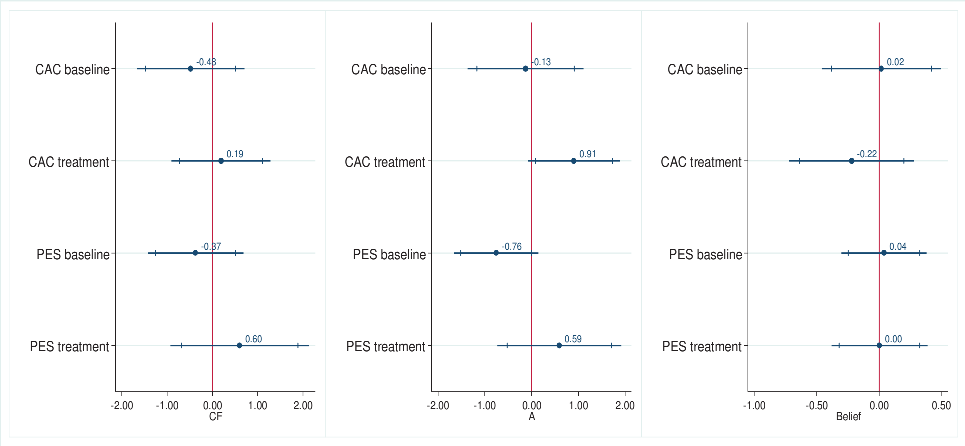



Figure 4 reports the impact of policies for each drought risk scenarios, while Table 6 presents additional Wald tests across treatments. Three results stand out. First, CAC and PES increased A choices by roughly the same magnitude, while PES reduced CF choices, possibly due to an institutional crowding out of the fire control norm. Second, CAC had a unique belief-mediated impact when combined with the increasing risk treatment. Third, in spite of increasing risk having no impact in itself, both policy treatments performed better under increasing risk pointing to some important interaction between increasing drought risk and policy treatments. This might be the consequence of higher risk reducing the odds of choosing CF or A instead of F during baseline rounds, and is tested in the next section. When risk increased, policies might have been perceived as more supportive than under stable risk, as we return to in the discussion.

Being a fire user or reporting technological constraints to end fire use had no direct impact on choices, but indirectly increased fire use through beliefs. This might have occurred because stated risk perception, fire use and perceiving a technological constraint are strongly correlated. This is also coherent with Dreber et al. (2013); framing mainly operates through changing beliefs, rather than directly through (perceived) payoffs.

*Figure 4 Policies treatments impact*
*(Multinomial logit and Poisson regression, log odds ratio and log of expected count; 90% and 95% CI; full results are available in the Appendix)*

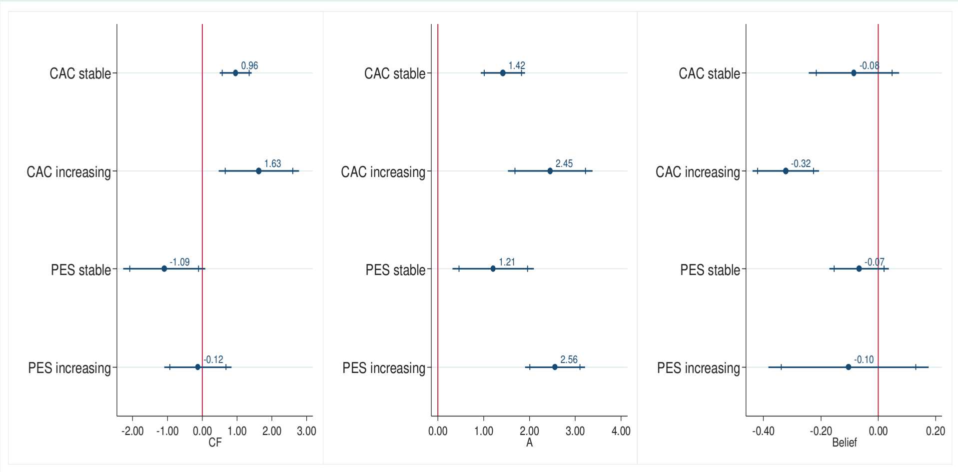



*Table 6 Wald tests of treatment impacts difference (p-values)*

| | A | CF | Beliefs |
|---|---|---|---|
| CAC increasing vs. stable | 0.025 | 0.147 | 0.008 |
| PES increasing vs. stable | 0.008 | 0.106 | 0.405 |
| CAC stable vs. PES stable | 0.342 | 0.001 | 0.428 |
| CAC increasing vs. PES increasing | 0.429 | 0.011 | 0.078 |

## 5.4 Impact of drought risk levels

Drought risk is expected to lower the expected payoff of choosing A in two ways: directly by reducing its expected payoff, and indirectly by raising the belief about other players not controlling their fires.

To test the overall impact of the risk magnitude on choices, we followed the same specification outlined above, and regressed each choice variable on an interaction term between risk levels (0 to 60%) and a categorical variable indicating policy treatment rounds (see Appendix). Admittedly, our results might suffer from an order effect because the levels of risk are not randomized across rounds, as we deemed increasing risk to be of special interest, simulating a climate change scenario. To mitigate this shortcoming, we pool data from the stable and the increasing risk sessions and control for differences across the two with a dummy variable. This also allows us to keep the number of sessions high enough to cluster standard errors at the session level. Clustering standard errors at the individual level and controlling for dynamic effects through beliefs only would be an efficient but less robust approach because it fails to control for dynamic session effects. Further, this would not be feasible when beliefs themselves are the dependent variable. With the chosen approach we prioritize the control of dynamic effects within each session.

*Figure 5 Risk level impact on choices and beliefs*
*(Multinomial logit and Poisson regression, log odds ratio and log of expected count; 90% and 95% CI; full results are available in Appendix)*

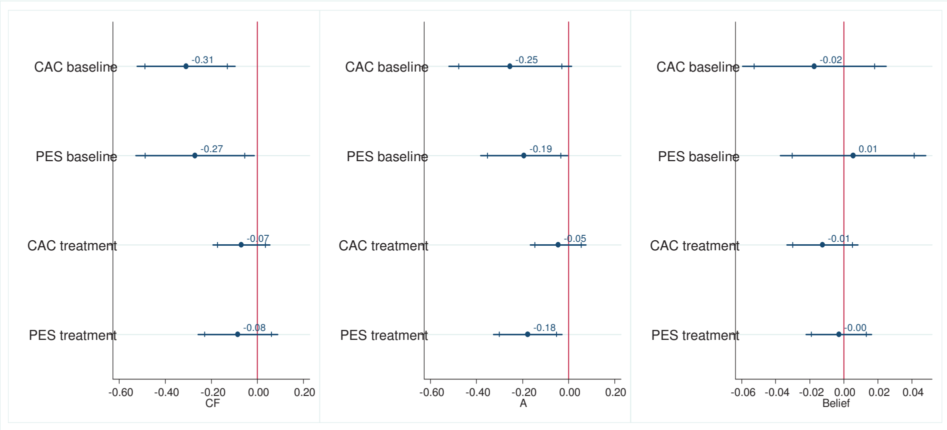



Figure 5 reports the impact of risk on choices and beliefs. During baseline, as risk increases, less CF and A, and more F choices are chosen. We may interpret these findings as if the exogenous risk broke down the fire control norm and created a feeling of anomie among farmers: despite controlling the own fire, fires were still occurring. Yet, this has no impact on beliefs about others' choices.

When policies were introduced, risk did not affect CF choices anymore, possibly because the policies were supportive of the fire control motivation. Introducing CAC, but not PES, offsets the impact of risk on A choices. Finally, that a higher level of risk does not affect beliefs suggest that drought risk in itself does not hamper trust in the other participants.

Although drought risk does not affect beliefs it affects choices, hinting that the observed increase in fires during drought years might be not only the consequence of increased fuel load, but also of the reduction in incentives to control fire, as posited by Nepstad et al. (2001). Therefore, our result hint that there might be a human mediated impact of droughts on fires.

# 6 Discussion

## 6.1 Experimental policy mechanism evaluation and external validity

The evaluation of tropical forest conservation measures often suffer from poor data availability and except for protected areas, quasi-experimental situations are rare (Börner et al., 2016). These problems are exacerbated in the case of fires in the Brazilian Amazon (Morello et al., 2017). Local policy implementations are scattered and poorly documented. The analysis of satellite data alone is unlikely to shed light on the reasons underlying policy effectiveness (Carmenta et al., 2011). Survey data are likely to be affected by over and under-reporting due to taboos and conflicts connected with fire accidents (Cammelli, 2014; Carmenta, 2013). Finally, the endogenous social effects, from the individual to the group and vice versa (Manski, 1993) generated by fire risk externalities cannot be easily accounted for using survey data (cf. Bowman et al., 2008).

Framed field experiments cope with these shortcomings, and help to identify the causal mechanisms behind the behavioural impact of policy instruments (Ludwig et al., 2011). Experiments can also inform policy makers about the potential impact and interaction with specific contextual factors *before* implementation (Handberg and Angelsen, 2015). Yet, caution is needed in generalizing from experimental tests on the relative merits of policies; the outcomes are likely to be influenced by the artificiality of the setting, the treatment dose (level of PES payment, and the probability and level of the fine), and threshold effects may occur.

The participants in our experiment have complex motivations that, taken together, support the external validity of the experiment. First, payoffs and expectations about others' choices (NFH) matter: when a participant believes others to choose more uncontrolled fire (F), he is more likely to use uncontrolled fire himself. Second, perceived fire risk is strongly correlated with stated fire use in real life, which in turn affects expectations in the experiment. This suggests a causal interpretation of fire risk perception on fire use both in real life and in the experiment. Third, a strong fire-control norm causes a high frequency of CF choices, even if the CF choice is always strictly dominated when only considering the payoffs. Questionnaire responses and field evidence suggest that the frequent choice of CF is best explained by the existence of a fire-control norm.

### 6.2 There is a human mediated self-reinforcing loop between droughts and fires

We hypothesized that higher fire risk might stimulate uncontrolled fire use among Amazonian farmers, because it has a direct impact on the benefits of controlling fire (Nepstad et al., 2001). In addition, increasing risk *per se* might foster uncontrolled fire use because it engenders self-fulfilling expectations about other farmers *not* controlling fire. We find no evidence of this second effect. We find, however, evidence of a higher risk leading to less controlled fire and lower uptake of fire-free techniques in baseline rounds. Policies offset the impact of a higher risk on CF; the effect on A level out under CAC, but persists under PES. This might be the effect of CAC strengthening the salience of the norm against uncontrolled fire use, while PES strengthens the salience of payoffs. Alternatively, the result can be the effect of how risk averse players evaluate contrasting risks (drought vs. fine) under CAC, relative to higher payoffs subject to the same drought risk under PES.

Although farmers respond to changes in risk levels, they fail to maximize the expected value of choices across the ten rounds. Failing to account fully for risk *a priori*, they change their behavior after a random drought occurrence. This finding suggests that after experiencing fires, investments in alternative techniques might be lower, and that a higher level of drought risk makes fire control slacker. Therefore, the negative impact of droughts on fires might be partly mediated by farmers controlling less fire, slowing down the uptake of alternative techniques, and producing more ignitions.

Exogenous drought risk shifts the responsibility of fires from participants to nature, hampering the positive impact of the fire control norm. Although on average there is no significant impact of increasing risk *per se*, we find that players are less likely to control fire as risk increases. Exogenous fires might ignite a feeling of anomie in participants, especially when risk is increasing, because fires occur irrespectively of compliance with the norm. Policies on the other hand might crowd in the fire control norm because they offer a focal point for coordination. This might explain why the policy impact is higher when risk is increasing.

### 6.3 Command and control outperforms payment for environmental services

We find that CAC, but not PES offset the impact of a higher risk on A, and that CAC increases and PES reduces the odds of choosing CF compared to F. The latter result might be due to either a norm crowding out effect or risk aversion.

Muradian et al. (2013) argue that crowding out of intrinsic motivations is likely to occur when the task is characterized by a high pro-social component and the context is marked by social norms, which both characterize fire control in our study site. Both CAC and PES have the potential to crowd-out social preferences because they might increase the relative salience of payoffs.

Any difference between CAC and PES might be due to risk aversion, with risk-averse participants being more responsive to the fine. Risk aversion increases (decreases) the expected payoff difference between F and A (CF) compared to PES, making the two treatments theoretically not equivalent any longer. Both PES and CAC might have reduced CF choices because of a crowding out effect of social preferences, however, CAC might more than compensate this negative effect because risk averse participants are more responsive to potential fines.

A recent simulation study by Morello et al. (2017) finds that subsidies to mechanization would perform better than CAC, in part due to the high monitoring and enforcement difficulties of sanctioning smallholders (Börner et al., 2015; Godar et al., 2014). They also point to an interesting dilemma and trade-off between effective fire and deforestation policies. Mechanization subsidies (and PES payments in general) might increase farming profitability, putting more pressure on forests. This suggests that different policy instruments are needed to achieve multiple objectives.

Our experimental results cannot account for general equilibrium effects, indirect land use change and insufficient monitoring and enforcement capacity. Yet, our design allows to analyze closely the strategic interaction involved in fire management and to account for exogenous drought shocks. Our results suggest that if monitoring of smallholder was implemented, for instance by increasing satellite monitoring resolution (cf. Assunção et al., 2014), less uncontrolled fires would occur than under PES.

## 7 Conclusions

We conducted a FFE to test the *ex-ante* impact of policies and drought risk on smallholder farmers' use and control of fire. We find that perceived accidental fire risk correlates with more stated fire use and that drought induced fire risk increases the uncontrolled use of fire in the experiment, in conformance with the Nepstad et al. (2001) hypothesis. This occurs directly through reduced expected benefit of investing in alternative techniques and indirectly, by undermining local fire control norms.

Our findings suggest that alerts about fire risk, proposed by, *inter alia,* Moran et al. (2006) and Brondizio and Moran (2008), might have an ambiguous impact on fires. If a high drought risk is announced, farmers have more information to make decisions. Socially conscious farmers may be reluctant to "play with fire". Nevertheless, the direct economic incentives suggest the opposite response. A higher drought risk may induce negative expectations and therefore reduce coordination, because each farmer's benefit of not using fire and to invest in fire control are reduced. The outcome depends on the impact of drought risk on farmers' beliefs and on complementary policies, such as CAC and PES. We have shown that these work best under a scenario of increasing drought risk (climate change).

We find that command and control scores better in reducing uncontrolled fires than payments for environmental services, partly through changes in beliefs. The enforcement of the Forest Code would reduce fires and promote the uptake of alternative techniques, and also meet the high demand for justice of local farmers. 43% of them suffered at least one fire accident in the last 5 years of which only 2% obtained a compensation for the consequent damages.

Of the smallholders in our sample, 56% reported to not be able to farm without fire, even if the law forbid it. Despite CAC seemingly being a superior policy instrument, complementing it with enabling measures such as PES and technical assistance is needed both for equity and political acceptance, and to generate win-win outcomes. The measures would match smallholders' norm and avoid the negative welfare effect of smallholders alone carrying the fire control costs.

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# 9 Appendix

## 9.1 Test of selection bias induced by participants being invited by community leaders.

*Table 7 Test of selection bias induced by participants being invited by community leaders (Logit regression)*

| | Invited participant |
|---|---|
| N of friends in the experiment | 0.096 |
| | (0.073) |
| N of relatives in the experiment | 0.096 |
| | (0.123) |
| Participate in association | 0.306 |
| | (0.288) |
| Participate in union | 0.307 |
| | (0.330) |
| Age | 0.009 |
| | (0.011) |
| Male | -0.112 |
| | (0.330) |
| Years in the community | 0.001 |
| | (0.012) |
| Constant | 0.368 |
| | (0.637) |
| | |
| N | 366 |
| Chi2 test (p-value) | 0.3632 |

## 9.2 Policies and drought risk treatments

*Table 8 Policies and drought risk treatments impact (multinomial logit and Poisson regression, log odds ratio and log of expected count).*

| | CF/F | A/F | Belief |
|---|---|---|---|
| PES group | 0.259 | 0.0944 | 0.144 |
| | (0.508) | (0.647) | (0.209) |
| Increasing risk | 0.116 | 0.100 | -0.210 |
| | (0.468) | (0.656) | (0.229) |
| PES group * Increasing risk | -0.598 | -0.635 | 0.254 |
| | (0.666) | (0.868) | (0.280) |
| Treatment rounds | 1.619*** | 1.101*** | -0.0425 |
| | (0.307) | (0.403) | (0.128) |
| PES group * treatment rounds | -2.749*** | -0.617 | -0.0130 |
| | (0.912) | (0.840) | (0.168) |
| Increasing risk * treatment rounds | 0.131 | 1.390* | -0.200 |
| | (0.699) | (0.711) | (0.158) |
| PES group * increasing risk * treatment rounds | 0.908 | -0.0145 | 0.225 |
| | (1.427) | (1.206) | (0.272) |
| Lag cumulative drought occurrences (CAC group * stable risk * baseline) | 0.237 | -0.560*** | -0.0373 |
| | (0.221) | (0.203) | (0.0630) |
| Lag cumulative drought occurrences (CAC group * stable risk * treatment) | -0.369** | -0.265* | -0.0758 |
| | (0.184) | (0.141) | (0.0567) |
| Lag cumulative drought occurrences (CAC group * increasing risk * baseline) | -0.312 | -0.771*** | 0.174*** |
| | (0.438) | (0.295) | (0.0498) |
| Lag cumulative drought occurrences (CAC group * increasing risk * treatment) | -0.424** | -0.805*** | 0.100** |
| | (0.177) | (0.171) | (0.0391) |
| Lag cumulative drought occurrences (PES group * stable risk * baseline) | -0.0880 | -0.848*** | 0.0655 |
| | (0.176) | (0.308) | (0.0428) |
| Lag cumulative drought occurrences (PES group * stable risk * treatment) | -0.0547 | -0.177 | 0.0549 |
| | (0.255) | (0.224) | (0.0445) |
| Lag cumulative drought occurrences (PES group * increasing risk * baseline ) | 0.0160 | -1.052*** | 0.0626 |
| | (0.262) | (0.264) | (0.0727) |
| Lag cumulative drought occurrences (PES group * increasing risk * treatment ) | -0.0144 | -0.407 | -0.00489 |
| | (0.283) | (0.264) | (0.0755) |
| Beliefs | -0.0894* | -0.211*** | |
| | (0.0456) | (0.0467) | |
| Male group | 0.0126 | 0.0113 | -0.00859 |
| | (0.0158) | (0.0128) | (0.00664) |
| Age group | 0.0243 | -0.00582 | -0.00963 |
| | (0.0376) | (0.0373) | (0.0163) |
| Age | 0.0140 | 0.0150 | -0.00840* |
| | (0.00989) | (0.00947) | (0.00470) |
| Male | -0.490* | -0.00463 | -0.0174 |
| | (0.288) | (0.291) | (0.109) |
| Perceived choice constraint | 0.238 | -0.360 | 0.258** |
| | (0.312) | (0.308) | (0.116) |
| Inverse round treand | 1.218*** | -0.432 | 0.191*** |
| | (0.337) | (0.371) | (0.0727) |
| Years of education | -0.157*** | 0.0113 | -0.0109 |
| | (0.0445) | (0.0381) | (0.0164) |
| Stated fire-user | -0.0333 | -0.165 | 0.351*** |
| | (0.262) | (0.335) | (0.129) |
| Constant | -0.882 | 1.321 | 1.471 |
| | (2.410) | (1.923) | (0.979) |
| Individual level RE | Yes | | Yes |
| Session level RE | Yes | | Yes |
| Observations | 3 840 | | 3 840 |
| Log pseudo-likelihood | -2824 | | -6588 |
| Number of clusters | 48 | | 48 |

Session clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

*Table 9 Increasing risk treatment impact.*

| | Effect | SE | z | p | 95% CI | |
|---|---|---|---|---|---|---|
| | CF | | | | | |
| CAC baseline | -0.479 | 0.606 | -0.790 | 0.430 | -1.666 | 0.709 |
| CAC treatment | 0.187 | 0.558 | 0.340 | 0.737 | -0.906 | 1.280 |
| PES baseline | -0.370 | 0.538 | -0.690 | 0.492 | -1.424 | 0.685 |
| PES treatment | 0.601 | 0.779 | 0.770 | 0.441 | -0.927 | 2.128 |
| | A | | | | | |
| CAC baseline | -0.128 | 0.630 | -0.200 | 0.839 | -1.363 | 1.108 |
| CAC treatment | 0.906 | 0.501 | 1.810 | 0.071 | -0.076 | 1.887 |
| PES baseline | -0.755 | 0.461 | -1.640 | 0.101 | -1.659 | 0.148 |
| PES treatment | 0.591 | 0.674 | 0.880 | 0.381 | -0.730 | 1.913 |
| | Belief | | | | | |
| CAC baseline | 0.019 | 0.243 | 0.080 | 0.938 | -0.458 | 0.496 |
| CAC treatment | -0.219 | 0.255 | -0.860 | 0.391 | -0.720 | 0.281 |
| PES baseline | 0.041 | 0.174 | 0.230 | 0.815 | -0.300 | 0.382 |
| PES treatment | 0.004 | 0.196 | 0.020 | 0.983 | -0.380 | 0.388 |

*Table 10 Policies treatment impact*

| | Effect | SE | z | p | 95% CI | |
|---|---|---|---|---|---|---|
| | CF | | | | | |
| CAC ST | 0.963 | 0.236 | 4.090 | 0.000 | 0.501 | 1.425 |
| CAC INCR | 1.629 | 0.590 | 2.760 | 0.006 | 0.472 | 2.786 |
| PES ST | -1.095 | 0.602 | -1.820 | 0.069 | -2.275 | 0.086 |
| PES INCR | -0.124 | 0.494 | -0.250 | 0.801 | -1.092 | 0.843 |
| | A | | | | | |
| CAC ST | 1.421 | 0.245 | 5.800 | 0.000 | 0.941 | 1.901 |
| CAC INCR | 2.454 | 0.469 | 5.230 | 0.000 | 1.535 | 3.374 |
| PES ST | 1.211 | 0.453 | 2.670 | 0.008 | 0.323 | 2.098 |
| PES INCR | 2.557 | 0.333 | 7.680 | 0.000 | 1.905 | 3.210 |
| | Belief | | | | | |
| CAC ST | -0.084 | 0.080 | -1.050 | 0.293 | -0.241 | 0.073 |
| CAC INCR | -0.322 | 0.059 | -5.450 | 0.000 | -0.438 | -0.207 |
| PES ST | -0.067 | 0.053 | -1.270 | 0.205 | -0.171 | 0.037 |
| PES INCR | -0.104 | 0.142 | -0.730 | 0.467 | -0.382 | 0.175 |

## 9.3 Risk level impact on choices

*Table 11 Risk level impact on choices and beliefs (Multinomial logit and Poisson regression, log odds ratio and log of expected count).*

| | CF/F | A/F | Beliefs |
|---|---|---|---|
| Risk level | -0.309*** | -0.254* | -0.0173 |
| | (0.109) | (0.136) | (0.0215) |
| Treatment round | 0.627 | 0.945* | -0.170** |
| | (0.463) | (0.535) | (0.0812) |

| | | | |
|---|---|---|---|
| Risk level * CAC group | 0.240* | 0.208 | 0.00478 |
| | (0.125) | (0.156) | (0.0227) |
| PES | -0.161 | -0.417 | 0.220 |
| | (0.549) | (0.591) | (0.174) |
| Risk level * PES group | 0.0386 | 0.0606 | 0.0228 |
| | (0.146) | (0.147) | (0.0282) |
| PES group * treatment rounds | -2.127** | -0.219 | 0.144 |
| | (0.853) | (0.738) | (0.163) |
| Risk level * PES group * treatment round | -0.0539 | -0.192 | -0.0132 |
| | (0.201) | (0.183) | (0.0281) |
| Lag cumulative drought occurrences (CAC group * stable risk * baseline) | -0.0216 | -0.723*** | -0.0433 |
| | (0.233) | (0.176) | (0.0625) |
| Lag cumulative drought occurrences (CAC group * stable risk * treatment) | -0.462** | -0.554*** | -0.0394 |
| | (0.188) | (0.152) | (0.0456) |
| Lag cumulative drought occurrences (CAC group * increasing risk * baseline) | 0.464 | -0.204 | 0.239*** |
| | (0.625) | (0.479) | (0.0851) |
| Lag cumulative drought occurrences (CAC group * increasing risk * treatment) | -0.228 | -0.417** | 0.0795* |
| | (0.186) | (0.178) | (0.0468) |
| Lag cumulative drought occurrences (PES group * stable risk * baseline) | -0.246 | -0.933*** | 0.0591 |
| | (0.180) | (0.292) | (0.0407) |
| Lag cumulative drought occurrences (PES group * stable risk * treatment) | -0.275 | -0.390* | 0.0482 |
| | (0.247) | (0.212) | (0.0523) |
| Lag cumulative drought occurrences (PES group * increasing risk * baseline) | 0.510 | -0.731** | 0.0454 |
| | (0.355) | (0.290) | (0.0874) |
| Lag cumulative drought occurrences (PES group * increasing risk * treatment) | 0.338 | 0.0735 | 0.00119 |
| | (0.216) | (0.273) | (0.0500) |
| Beliefs | -0.0879* | -0.213*** | |
| | (0.0466) | (0.0479) | |
| Male group | 0.0158 | 0.0146 | -0.00998 |
| | (0.0161) | (0.0132) | (0.00673) |
| Age group | 0.0173 | -0.0173 | -0.00646 |
| | (0.0379) | (0.0392) | (0.0170) |
| Inverse round trend | 0.355 | -1.116*** | 0.177** |
| | (0.373) | (0.426) | (0.0826) |
| Male | -0.503* | -0.0126 | -0.0166 |
| | (0.290) | (0.293) | (0.109) |
| Age | 0.0138 | 0.0147 | -0.00835* |
| | (0.00988) | (0.00949) | (0.00471) |
| Perceived choice constraints | 0.224 | -0.370 | 0.259** |
| | (0.317) | (0.311) | (0.117) |
| Years if education | -0.161*** | 0.00743 | -0.0105 |
| | (0.0452) | (0.0378) | (0.0164) |
| Stated fire-user | -0.0172 | -0.140 | 0.344*** |
| | (0.268) | (0.335) | (0.130) |
| Increasing risk | -0.485 | -0.252 | -0.110 |
| | (0.332) | (0.396) | (0.131) |
| Constant | 0.891 | 2.928 | 1.410 |
| | (2.434) | (2.006) | (0.984) |
| | | | |
| Individual level RE | Yes | | Yes |
| Session level RE | Yes | | Yes |
| Observations | 3,840 | | 3,840 |
| Log pseudo-likelihood | -2822 | | -6590 |
| Number of clusters | 48 | | 48 |

Session clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

*Table 12 Impact of risk levels on choices and beliefs.*

| | Effect | SE | z | p | 95% CI | |
|---|---|---|---|---|---|---|
| | CF | | | | | |
| CAC baseline | -0.309 | 0.109 | -2.84 | 0.005 | -0.522 | -0.096 |
| PES baseline | -0.271 | 0.131 | -2.06 | 0.039 | -0.528 | -0.013 |
| CAC treatment | -0.069 | 0.063 | -1.1 | 0.272 | -0.193 | 0.054 |
| PES treatment | -0.085 | 0.088 | -0.96 | 0.338 | -0.258 | 0.088 |
| | A | | | | | |
| CAC baseline | -0.254 | 0.136 | -1.87 | 0.062 | -0.521 | 0.013 |
| PES baseline | -0.193 | 0.097 | -2 | 0.045 | -0.383 | -0.004 |
| CAC treatment | -0.046 | 0.062 | -0.74 | 0.459 | -0.166 | 0.075 |
| PES treatment | -0.177 | 0.076 | -2.34 | 0.019 | -0.326 | -0.029 |
| | Beliefs | | | | | |
| CAC baseline | -0.017 | 0.022 | -0.81 | 0.42 | -0.060 | 0.025 |
| PES baseline | 0.005 | 0.022 | 0.25 | 0.801 | -0.037 | 0.048 |
| CAC treatment | -0.013 | 0.011 | -1.18 | 0.239 | -0.033 | 0.008 |
| PES treatment | -0.003 | 0.010 | -0.3 | 0.767 | -0.022 | 0.016 |

## 9.4 Instructions

Please refer to the appendix to all papers

# How good norms lead to bad communication, miscoordination and fires

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**Abstract**

Communication in coordination games of common interests is credible and provides sufficient assurance for coordination. I show that a social norm can provide the same assurance. However, it also induces a *taboo* about breaking the norm publicly. As a result, when communication and a social norm both exist, communication is no more credible and leads to *hypocritical* communication. If the norm is weak, less coordination is achieved with communication and a norm than with communication alone. However, if communication of requests is allowed and players believe in the others' credulity, communication can improve coordination even in presence of a weak social norm. I adopt a level-k model to analyze a real coordination problem about technology adoption and fire management by Amazonian smallholder farmers. Fires are one of the main challenges to forest conservation in the region. In a framed field experiment, farmers – who share a weak fire control norm – play a multiplayer assurance game with risk, with and without communication. Results support the conjectures arising from the level-k model, and implications are discussed for fire risk mitigation policies.

**Keywords:** level-k, coordination, social norms, Brazilian Amazon, fires.

**JEL codes:** C93, C70, Q56

## 1 Introduction

In coordination games of common interest, the Pareto dominant solution can be achieved through communication of intents (Cooper et al., 1992; Crawford, 1998). Communication of intents in assurance games is expected to improve coordination because it is both self-signaling and self-committing (Ellingsen and Östling, 2010; Farrell and Rabin, 1996). Communication of intents is self-committing if the sender has incentive to fulfill the signal when the receiver believes the latter. It is self-signaling if the sender has incentive to signal the truth.

Social norms are self-sustaining systems of shared beliefs about how the game has to be played (Aoki, 2001). They create focal points (Schelling, 1978), favoring coordination on the prescribed behavior and generating (what I call) a *taboo* on the proscribed behavior: a preference for not breaking the norm publicly.

Social norms and communication of intents are expected to be substitutes. Allowing communication in the presence of a norm does not convey additional information. However, because social norms induce taboos about the proscribed behavior, communication of intents is no longer self-signaling: there is no incentive to declare a true preference for breaking the norm.

When communication of intents is allowed in presence of a weak social norm, the outcome might be inferior compared with communication of intents alone. What I call *weak* social norms are such because they fail to provide full assurance for coordination. They are ubiquitous in society and might originate from precedents of low compliance or "bad" precedents (Van Huyck et al., 1997), contrasting sources of beliefs (Fehr, 2011), or misunderstandings (e.g., about which norm is salient). In coordination games, weak norms might generate *moral hypocrisy:* a separation between stated moral principles and actual behavior, or behavior motivated by moral appearance while avoiding the cost of acting morally (Batson et al., 1997; Rustichini and Villeval, 2014).

Less considered in literature, communication of requests can improve coordination when players are believed to satisfy at least part of the requests, or to be credulous (Ellingsen and Östling, 2010), I show that this result holds also in presence of a weak norm.

Building on Ellingsen and Östling (2010), I set-up a level-k model of communication under a norm in a multiplayer assurance game. The game is similar to the case of interdependent security analyzed by Kunreuther and Heal (2003), of optimal fire department by Orszag and Stiglitz (2002), and of optimal fire defensible space by Shafran (2008).

I test the model prediction in a framed field experiment (Harrison and List, 2004) about fire management in the Brazilian Amazon, where farmers share a weak norm for fire control or non-fire use. The framed field experiment tests repeated communication of intents and requests (cheap talk) on an actual coordination problem, and allows for an actual weak social norm into the experiment.

I show that, in presence of communication of intentions and requests, a weak social norm might hamper coordination. I also show and find support for the level-k model predictions, that in presence of a weak norm, cheap talk has the potential to improve coordination only via communication of requests, when players are believed to fulfill them.

I am not able to test whether a weak social norm offsets the impact of communication. Such a test would require manipulating the norm in the experiment, which turns to be infeasible, and is discussed further below. Yet, in comparing results from similar experiments in the lab, I find higher pre-communication coordination and a lower impact of communication, in line with model predictions.

Finally, cheap talk in common pool resource games has been interpreted as indicative of real life community resource management (Handberg and Angelsen, 2015; Ostrom et al., 1994). Following the same assumption, the result of the experiment hints that community based fire management, one of the main fire mitigation policy addressing smallholder farmers in the Brazilian Amazon (Sorrensen, 2009), is unlikely to achieve its goal, but that initiatives targeting leaders and communities with weak fire control norms might have an impact.

The rest of the paper is organized as follow: section 2 presents the background related to fires in the Brazilian Amazon, section 3 presents the fire game, section 4 outlines the level-k model and the theoretical predictions, section 5 shows the experimental results, and section 6 discusses and concludes.

## 2 Fires in the Brazilian Amazon

Fires in the Amazon rainforest are on a rise, causing enormous environmental destruction (Alencar et al., 2015; Aragao and Shimabukuro, 2010; Malhi et al., 2008; Morton et al., 2013). Fires double biodiversity losses from deforestation (Barlow et al., 2016), generate large $CO_2$ emissions, and reduce up to 40% of the carbon stock of standing forests (Barlow et al., 2012; Berenguer et al., 2014). Fires also cause damages to the local people's health and assets, pastures, crops and perennial plantations (Bowman et al., 2008; de Mendonça et al., 2004; Hoch et al., 2009; Hoch et al., 2012; Nepstad et al., 1999; Pokorny et al., 2012).

There are no natural ignitions in the Amazon. Most fires are accidentally spreading from smallholder farmers' plot, who use them for slash and burn, pasture maintenance, fertilization and other agriculture related tasks (Börner et al., 2007; Cammelli and Angelsen, in this thesis; Carmenta et al., 2013; Cochrane, 2002).

The farmer's benefits of engaging in costly fire control activities, such as firebreaks clearing, is to prevent the rest of his land and amenities to burn unintendedly (Bowman et al., 2008). However the benefits to engage in such costly activities are jeopardized by neighboring farmers not using the same caution: their fires can spread to the first property causing large losses (Cammelli and Angelsen, in this thesis; Cammelli and Handberg, in this thesis). Farmers also face an exogenous source of fire risk, related to large scale fire events, increasingly frequent during drought years (Alencar et al., 2015; Malhi et al., 2008).

Farmers can adopt alternative fire-free agricultural techniques, such as mechanization, pasture maintenance through rotation, and perennial crops. These techniques give higher yields than those relying on fire use. Yet, they involve investments that increase the property exposure in case of fire, because crops, fences and trees are flammable (Bowman et al., 2008; de Mendonça et al., 2004; Hoch et al., 2009; Hoch et al., 2012; Pokorny et al., 2012). There is a trade-off between fire risk and the payoff of investing in fire free alternative techniques (Nepstad et al., 1999), which is at the origin of strategic complementarities in fire control and uptake of fire free techniques (Cammelli and Handberg, in this thesis).

Farmers are legally responsible for managing fires[1] and share a norm prescribing fire control. Carmenta (2013) speculated about how this norm might explain the large over-reporting of fire control measures uptake. In my survey, farmers answered the question "Do you think that controlling fire is a farmer duty?". On a 5 steps Likert scales, the average score was 4.81 (Cammelli and Angelsen, in this thesis). Further evidence of the social norm can also be inferred from the experimental results. However, in spite of the norm, fires are still prevalent and farmers report a high number of large scale fires and fire contagion events from neighboring properties (Cammelli and Angelsen, in this thesis), pointing to an insufficient compliance with the fire control norms.

I frame the choice to use and control fire as a repeated assurance game with two equilibria: uncontrolled use of fire, and adoption of alternative techniques. Controlled fire is also an option, but, because of the cost of implementing fire control measures, it is always strictly dominated by uncontrolled fire use. Coordination is challenged by strategic uncertainty and by the exogenous risk of fire, which is a random shock with a known probability.

# 3 The fire game

## 3.1 The baseline game

The game is an eight players assurance game with an exogenous (drought-related) fire risk. If a drought occurs, the least favorable payoff table is applied (table 1). The framing is obtained by labelling F as uncontrolled fire use, CF controlled fire, and A alternatives to fire use. Participants are told explicitly that by playing F their action damages those choosing A, alternatives to fire use. CF is always strictly dominated by F, as it involves fire control costs.

The game is symmetric in payoffs and exhibits strategic complementarities. Players have incentive to coordinate on the Pareto optimal equilibrium, A. However, in order to do so they need some form of assurance that their opponents will not choose F.

[1] As prescribed in the Brazilian Forest Code Chap. IX, law 12651/2012.

*Table 1 Payoff tables in case of no and drought occurrence.*

| #F | A | F | CF |
|---|---|---|---|
| 0 | 200 | 100 | 70 |
| 1 | 166 | 100 | 70 |
| 2 | 134 | 100 | 70 |
| 3 | 104 | 100 | 70 |
| 4 | 78 | 100 | 70 |
| 5 | 53 | 100 | 70 |
| 6 | 32 | 100 | 70 |
| 7 | 13 | 100 | 70 |

| #F | A | F | CF |
|---|---|---|---|
| 0 | 75 | 100 | 70 |
| 1 | 41 | 100 | 70 |
| 2 | 9 | 100 | 70 |
| 3 | 0 | 100 | 70 |
| 4 | 0 | 100 | 70 |
| 5 | 0 | 100 | 70 |
| 6 | 0 | 100 | 70 |
| 7 | 0 | 100 | 70 |

### 3.2 Treatments

In a crossed design, the game is repeated for ten rounds. After the fifth round, in each remaining round and before making their choices, participants are allowed one minute of face-to-face communication. To minimize noise and unintended communication, participants are asked to sit in a circle looking outward. During the one minute communication treatment, they are allowed to turn the chairs inward and to communicate freely.

In half of the sessions, the exogenous risk is constant at 30%, in the other half it is increasing over five rounds, ranging from 0 to 60% as displayed in table 2. The increasing risk treatment is intended to mimic a climate change scenario, with exogenous fires becoming more and more likely. The risk sequence re-starts after the introduction of communication. Risk levels are chosen such that the average risk is the same across both stable and increasing risk treatments, making treatment groups comparable.

### 3.3 Experiment implementation and data

During October-December 2015 I sampled 576 smallholder farmers from 40 villages in 4 municipalities of the Eastern Amazonian state of Pará. Descriptive statistics and an elaborate discussion of the context can be found in Cammelli and Angelsen (in this thesis). The analysis reported in this article only concerns the 196 participants that were allocated to the communication treatment. Full instructions are reported in appendix.

The same treatment is never administered twice in the same village, and all experiments are carried out in the same day to avoid spillover effects across sessions. A short survey is administered before and after the experiment.

Points were converted into cash at a rate of 80 to 1 BRL. Participants earned up to 35 BRL[2], approximately one local daily wage for unskilled labor in agriculture.

# 4 Theory

## 4.1 Introduction

A risk neutral and selfish player is expected to choose A or F, depending on his beliefs about the number of other players choosing F. There is a belief threshold below which the expected payoff of A is higher than the sure return of F, which is optimal otherwise (table 2).

*Table 2 Predicted belief thresholds, tipping points from A to F.*

| Risk | Belief threshold |
|---|---|
| 0 | 4 |
| 10% | 3 |
| 30% | 2 |
| 50% | 2 |
| 60% | 1 |

CF is strictly dominated and yet prescribed by the fire control norm. There is no reputational cost for breaking the norm because choices are anonymous in the experiment. I assume that players are more likely to play CF or A than F when they are indifferent among the three. In equilibrium no CF choice is expected to be played. This soft assumption seems to undervalue the norm, because rational selfish and risk neutral players are never indifferent among A, F and CF, but it has critical implications for less sophisticated players in the level-k model.

In absence of communication, the equilibrium model predicts coordination on the risk-dominant strategy (Frankel et al., 2003; Harsanyi and Selten, 1988). Generalized (g) risk dominance for n-players and n-strategy games has been developed by Peski (2010), and – in its ordinal form – is obtained as the best response strategy given that the other players randomize from a uniform distribution. If there is no other preference for compliance with the norm other than the one outlined above, and under common knowledge of rationality, players deem CF to be irrelevant, and estimates that F will be played 7/2 times (i.e., 50% chance for each of the seven other players). Whenever $q > 4$, there is a contrast between payoff dominance, which selects A, and g-risk dominance, which selects F. When payoff and risk dominance select the same equilibrium, higher coordination is expected even in absence of communication, to the opposite, communication is expected to improve coordination when the two criteria select different equilibria (Dugar and Shahriar, 2012).[3]

[2] At the end of the survey (December 2015), the exchange rate was about 4 BRL = €1.

[3] If players have a preference for CF and this is common knowledge, then the expected number of F choices declines to 7/3. Risk and payoff dominance select different equilibria for q equal to 3 or 4. Yet, in the same experiment,

The norm prescribing fire control might generate a focal point away from F, on A. In this case less than 7/2 F choices would be expected. There is higher pre-communication coordination on the Pareto dominant outcome but little additional coordination attributable to communication because the social norm and communication provide the same information.

I assume that the fire control norm imposes a taboo on stating intention to play and on requesting F: no participant is willing to reveal a true preference that involves breaking the norm, either in the form of intent or of request. This assumption is justified by observation during the experiment: no player ever communicated a request or intention to play F. It is also justified by over-reporting of stated fire control decisions (Cammelli and Handberg, in this thesis; Carmenta, 2013). Because of the taboo, if a fire control norm is introduced, communication of intentions is no more self-signaling and it is not expected to have any impact, regardless of the contrast between risk and payoff dominance.

In the following, I conduct a level-k analysis of the norm and communication in the fire game. I show that the assumption of bounded rationality gives the model two desirable features: to allow for CF choices even without assuming an intrinsic preference for compliance, and to explain selected but systematic impact of communication – even in presence of the taboo – occurring through requests rather than communication of intentions.

### 4.2 Level-k analysis

Level-k thinking was introduced by Nagel (1995) and Stahl and Wilson (1995). In level-k models, k denotes the level of sophistication of the player, the number of steps ahead he or she takes in thinking strategically. A k-type ($T_k$) player assumes that the other players are $T_{k-1}$, and chooses the best response to them. The starting point of any level-k model is the definition of the $T_0$ player, with the other types defined recursively. I adapt Ellingsen and Östling (2010) analysis of communication in coordination games to the fire game and extend it towards communication of requests, repeated communication, and communication in the fire game under a norm.

#### 4.2.1 The baseline fire game with no norm

Ellingsen and Östling (2010) assumes that $T_0$ players understand the set of strategies, but not how these map into payoffs. $T_0$ is indifferent among all of them and chooses at random from a uniform distribution. In absence of communication, $T_1$ assumes the other players to be $T_0$ and chooses the best response to them.

Whether $T_1$ players choose F or A depend on the threshold belief $q$ which varies with risk (Table 2). Because $T_1$ assumes that all other players randomize between A, F and CF, he expects a number of F choices of 7/3. If $q$>7/3 $T_1$ best responds to $T_0$ playing A, F otherwise. $T_2$ and higher k players

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Cammelli and Handberg (in this thesis) didn't find any role for social preferences shaping fire control decisions. A warm-glow preference for compliance can also be excluded because choices are anonymous. An intrinsic preference for CF cannot be excluded.

($T_{2+}$) best respond to $T_{1+}$ and choose A or F as well. Without norm nor communication, there is convergence on A only if $q$ equals 3 or 4.

### 4.2.2 Communication under no norm

Because the fire game is an eight players game, I only analyse multilateral communication. I assume that all players both send and receive messages. If this would not be the case, the outcome after communication would still be uncertain and communication would have no effect (Feltovich and Grossman, 2014). Players are also assumed to be lie-averse and to have a lexicographic preference for telling the truth: when they are payoff indifferent between sending a truthful and a false message they always choose the truthful message (Ellingsen and Östling, 2010).

#### *4.2.2.1 Multilateral and simultaneous communication of intentions*

This argument is generally demonstrated in Ellingsen and Östling (2010). Here, I provide the intuition and application to the fire game. I assume that all players send and receive the messages simultaneously. $T_1$, as a receiver, trusts the messages believed to come from a truthful $T_0$ and best responds by playing A if $q > m$ where $m$ is the number of opponents' messages signaling F, and choose F otherwise. As a sender, because $T_0$ chooses by randomly drawing from a uniform distribution, $T_1$ foresees to play A if q>7/3, and F otherwise, and sends a signal accordingly. $T_{2+}$ as a receiver plays A if $q > m - 1$. $m - 1$ is the number of F messages received by those players who sent an F message: $T_{2+}$ accounts for a $T_1$ player sending an F message to change his mind and play A. As a sender $T_{2+}$ believes that $T_1$ listens to his message, and although $m$ is unknown *ex ante*, it is weakly dominant for $T_{2+}$ to send an A message and to best respond to the actual realization of $m$. Provided that only $T_{2+}$ players exist, there is coordination on the Pareto dominant equilibrium (because $m = 0$). If only $T_0$ and $T_1$ exist, the outcome depends on the random choices of $T_0$ and $q$. If all types of players exist, the result depends also on the types' distribution.

#### *4.2.2.2 Multilateral and simultaneous communication of requests*

When requests rather than declarations of intention are communicated, credulity has a more important role than truthfulness (Ellingsen and Östling, 2010). A $T_0$ player is credulous if he tilts his choice distribution to comply with requests, although he sends random requests drawing from a uniform distribution.

$T_1$ believes $T_0$ to be credulous if the latter can be convinced to play F with probability $\frac{1}{3} - c$ ; $c \in \left(0; \frac{1}{3}\right)$ is a parameter associated with the (believed) receiver credulity, or the (believed) influence of the sender. If this is the case, $T_1$ always requests $T_0$ to play A or CF. $T_1$ plays A if $q > 7\left(\frac{1}{3} - c\right)$ and F otherwise.

$T_1$ ignores all requests coming from $T_0$, because they would not signal anything about the player intentions. $T_2$ as a receiver fulfills A requests coming from $T_1$ if $q > 7\left(\frac{1}{3} - c\right)$, because $T_1$ believes to direct them to a credulous $T_0$. Because $T_1$ always requests A, if $T_2$ receives a surprising request to play F, he believes it to be the mistake of $T_1$ and ignores the request.[4] $T_2$ as a sender does not expect to be believed by $T_1$, who believes messages to come from a babbling $T_0$.

$T_3$ as a receiver has no incentives to fulfill a request coming from $T_2$, but for $T_3$ as a sender it is always optimal to send a request for A or CF to $T_2$ , and to play A if $q > 7\left(\frac{1}{3} - c\right)$. $T_4$ requests, on the other hand, would not be believed by $T_3$. As a receiver, if $q > 7\left(\frac{1}{3} - c\right)$, $T_4$ has incentives to fulfil a request to play A or CF coming from $T_3$. And so on…

Odd-k types ($T^{O}_{1+}$) send A or CF requests and believe to be fulfilled, zero and higher even-k types ($T^{O}_{2+}$) have incentive to fulfil them if $q > 7\left(\frac{1}{3} - c\right)$. Because CF and A messages are weakly dominant for all types for $k > 0$, only CF and A messages are considered by non-zero type players. There is coordination on A if $q > 7\left(\frac{1}{3} - c\right)$, F otherwise.

That even-k types do not send any messages would not change the prospects for coordination because all F-requests are not fulfilled by odd-k types.

A curious case arises when only $T_0$ players exist. They exchange requests to play A, CF or F and fulfil them with equal probability. On average, F is chosen with probability $\frac{1}{3}$.

#### *4.2.2.3 Repeated communication of both intentions and requests*

In line with Crawford (2017), I assume that players do not use information conveyed in repeated rounds of communication to make inference on other players' types. Therefore, types are stable across communication rounds.

In repeated rounds of communication both requests and declarations of intent are possible. $T_1$ believes that $T_0$ is credulous with an associated parameter *c,* truthful, and that he randomizes across choices. In the first and the consecutive communication rounds, $T_1$ always request $T_0$ to play CF or A but ignores all requests, which are believed to come from $T_0$.[5] In the last rounds of communication, if $q < \min\left\{7\left(\frac{1}{3} - c\right); m\right\}$, $T_1$ plays F and communicates the intention to play F. He plays A if $q > \min\left\{7\left(\frac{1}{3} - c\right); m - 1\right\}$.

In the first and the consecutive communication rounds, $T_2$ always declares his intention to play A, fulfils A requests coming from $T_1$, but does not believe that $T_1$ would fulfil his requests. If in the last round of communication, $q < \min\left\{7\left(\frac{1}{3} - c\right); m - 1\right\}$, $T_2$ plays F.

$T^{O}_{1+}$ behave like $T_1$, and $T^{E}_{2+}$ behave like $T_2$. The outcome is the best of the possible outcomes under multilateral requests and statements of intents. If only $T_{2+}$ types exist, repeated rounds of

[4] Alternatively $T_2$ infers the existence of $T_0$ players, and change type himself. In this work I assume that players do not use messages to make inference about the existence of other players' types.
[5] In the first round, there is no reason for $T_1$ to forecast and communicate to play F.

communication always give *m=0,* and there is coordination on the Pareto dominant outcome. If $T_0$ and $T_1$ types also exist, for any *q,* communication of requests, compared to communication of intents increases coordination on the Pareto dominant equilibrium depending on *c*. Communication of requests is superior to statements of intents if $7\left(\frac{1}{3}-c\right) > m-1$, where $m$ is a function of the types distributions and of $T_0$ random choices. Table 3 summarizes the conditions for action and message F under communication.

*Table 3 Conditions for action and message F in the fire game with communication*

| | **Multilateral communication of intents** | |
|---|---|---|
| | **Sender (message)** | **Receiver (choice)** |
| **0** | random draw $Pr = \frac{1}{3}$ | random draw $Pr = \frac{1}{3}$ |
| **1** | $q < 7/3$ | $q < m-1$ |
| **2+** | never | $q < m-1$ |

| | **Multilateral communication of requests** | |
|---|---|---|
| | **Sender (message)** | **Receiver (choice)** |
| **0** | random draw $Pr = \frac{1}{3}$ | random draw $Pr = \left(\frac{1}{3}-c\right)$ |
| **$1^O$+** | never | $q < 7\left(\frac{1}{3}-c\right)$ |
| **$2^E$+** | never | $q < 7\left(\frac{1}{3}-c\right)$ |

| | **Repeated communication of intents and requests** | |
|---|---|---|
| | **Sender (message)** | **Receiver (choice)** |
| **0** | random draw $Pr = \frac{1}{3}$ | random draw $Pr = \left(\frac{1}{3}-c\right)$ |
| **$1^O$+** | never | $q < \min\left\{7\left(\frac{1}{3}-c\right); m-1\right\}$ |
| **$2^E$+** | never | $q < \min\left\{7\left(\frac{1}{3}-c\right); m-1\right\}$ |

#### 4.2.3 The fire game under a norm

I now turn to analyze the role of a norm for fire control. I follow the Crawford and Iriberri (2007) approach to model a $T_0$ type as payoff insensitive but label sensitive.

All types are assumed to be incompliance averse: if payoff indifferent, they comply with the norm. When the norm is weak, however, compliance probability is lower than 1: the fire control norm proscribes F, and $T_1$ believes that $T_0$ randomizes over a distribution tilted away from F towards CF and A.

The social norm has an additional implication for the quality of communication because it implies some internal cost of breaking the rule, a taboo: there is no incentive to request or to communicate a true preference for F.

Communication of intents is still self-committing, but not self-signaling: if the message would be believed, there would still be incentive to fulfill it; however, there is no guarantee for the receiver that the message is true.

The norm is one lexicographic level above lie aversion: $T_0$ is not believed to tell the truth if he chooses F, but is believed to tell the truth if he chooses A or CF. Because hypocritical (or untruthful) A and CF messages are perfectly correlated with F choices, communication does not convey any information to $T_1$.

*4.2.3.1 No communication*

In absence of communication and of the norm, $T_1$ believes that $T_0$ randomizes uniformly across choices. With a common social norm and no communication, $T_1$ believes that $T_0$ chooses F with probability $p < 1/3$. The social norm tilts the distribution of $T_0$ choices away from F. With a social norm and no need of communication, $T_1$ best responds to $T_0$ by playing A if $q > 7p$ , F otherwise. $T_{2+}$ best responds to $T_1$ by playing A or F according to the same rule. Coordination depends on types distribution, the inverse of the believed strength of the norm *p*, and *q*.

For any types' distribution, because $p$ is strictly lower than 1/3, for any *q,* there is higher coordination than under no norm.

*4.2.3.2 Multilateral and simultaneous communication of intentions*

If simultaneous communication of intentions is allowed, $T_1$ believes that $T_0$ randomize choices according to the tilted distribution, however, because of the taboo on F, all types, including $T_0$, are believed to communicate always CF or A irrespectively of their choices, therefore communication is not self-signaling. $T_1$ as a receiver disregards all messages. As a sender $T_1$ always states A, but only plays A if $q > 7p$. $T_{2+}$ behaves like $T_1$. Only A or CF messages are sent. The outcome equates the one without communication.

If only $T_0$ players exist, higher efficiency is achieved with than without the norm. If only $T_{2+}$ players exist, the norm hampers coordination, because in its absence, players only exchange A messages and play A. If also $T_0$ and $T_1$ players exist, whether communication is more effective without than with a norm depends on whether $m - 1 < 7p$, i.e., on $T_0$ random choices, the population types distribution and on the strength of the norm.

*4.2.3.3 Multilateral and simultaneous communication of requests*

Because of the norm, only CF or A are requested, irrespectively of the level-k type. $T_1$ believes that $T_0$ is credulous with an associated parameter $c \in (0; p)$, requests A or CF and expects to be fulfilled with probability $1 - (p - c)$. If $q > 7(p - c)$, $T_1$ best responds by playing A, F otherwise. $T_1$ disregards all requests because he believes them to come from $T_0$ and not signaling his choices. $T_2$ as a receiver listens to requests because he believes them to come from $T_1$ and to be directed to $T_0$. $T_2$ best responds by playing A if $q > 7(p - c)$. $T_3$ has no incentive to fulfil a request coming from $T_2$, because $T_2$ believes that $T_1$ is not credulous. As a sender, $T_3$ sends a request for A or CF and plays A if $q > 7(p - c)$.

Because of the taboo on F, all types only request A or CF. As receivers, $T_1$ and other odd-k types $T^O_{1+}$ send A or CF requests and believe to be fulfilled with probability $1-(p-c)$. As receivers, $T_2$ and higher even-k types $T^E_{2+}$ best respond to $T_1$ and play A if $q > 7(p-c)$.

When only $T_0$ players exist, they exchange requests to play A or CF and fulfil the messages with probability $c$. F is chosen with probability $p-c$, and the outcome is more efficient than with no norm. For any type distribution, the outcome is always superior to the case without a norm, because $p < \frac{1}{3}$.

*4.2.3.4 Repeated communication of both intentions and requests*

If repeated rounds of communication are allowed, both requests and declarations of intent are possible. Because declarations of intent are not self-signaling they are ignored by all types. Multiple rounds of requests have the same impact as simultaneous requests, because only A and CF messages are exchanged.

Whether communication under the norm is superior to communication without the norm depends on the population distribution of the types. If only $T_0$ players exist the outcome is more efficient with than without the norm, because $p < 1/3$. At the other extreme, if only $T_{2+}$ players exist, a weak norm undermines coordination, because in absence of the norm $m = 0$, and credible A messages would suffice to achieve coordination. If $T_0$ and $T_1$ also exist it is not possible to determine uniquely what choice environment is most favourable to coordination without knowing the types distribution.[6]

The impact of communication under a norm is restricted to the case in which the norm alone does not provide sufficient assurance, and belief in credulity is high enough to engender coordination, i.e. when $\begin{cases} q < 7p \\ q > 7(p-c) \end{cases}$. Merging the two inequalities results in $0 < -\frac{q-7p}{7} < c \in (0; p)$.

This last result holds for any population including at least one non-zero type[7].

Results for communication under a norm are summarized in table 4.

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[6] With no norm, for $T_{2+}$ players it is always weakly dominant to signal A and best respond to the actual realization of $m$. $T^O_{1+}$ always request A or CF. In the last round of communication the number of F messages $m$ depends on the number of random F messages sent by $T_0$ players (1/3 without or *0* with a norm). Whether there is more communication under a norm or not depends on the composition of the population. If $7(p-c) > m-1$ the prospects of coordination are higher under no norm, for any $q$.

[7] If only $T_0$ players exist, requests increase coordination by reducing the chance of randomly choosing F by c: $T_0$ players might request each other's to play A or CF and fulfill the messages. If more sophisticated players exist the above inequality holds for all non-zero types. In the following, I assume that at least one non-zero player exist in each session.

*Table 4 Conditions for action and message F in the fire game with communication and a fire control norm*

| | **Multilateral communication of intents** | |
|---|---|---|
| | **Sender (message)** | **Receiver (choice)** |
| **0** | never | random draw $Pr = p$ |
| **1** | never | $q < 7p$ |
| **2+** | never | $q < 7p$ |

| | **Multilateral communication of requests** | |
|---|---|---|
| | **Sender (message)** | **Receiver (choice)** |
| **0** | never | random draw $Pr = p$ |
| **$1^{O}$+** | never | $q < 7(p - c)$ |
| **$2^{E}$+** | never | $q < 7(p - c)$ |

| | **Repeated communication of intents and requests** | |
|---|---|---|
| | **Sender (message)** | **Receiver (choice)** |
| **0** | never | random draw $Pr = p$ |
| **$1^{O}$+** | never | $q < 7(p - c)$ |
| **$2^{E}$+** | never | $q < 7(p - c)$ |

### 4.2.4 Predictions and hypotheses

The above level-k analysis has shown that communication improves coordination in presence of a norm only if $p$ is small relative to $q$ (the norm is weak) and $c$ is sufficiently large ($T_0$ is credulous):

*Proposition 1: In presence of a norm, communication has an impact only if* $0 < -\frac{q-7p}{7} < c \in (0; p)$

Under a norm, communication is expected to have an impact only when beliefs are above the belief threshold, $q < 7p$ (i.e., coordination should not be obvious before communication, or, in other words, the norm should be weak enough compared to the belief threshold $q$), and when the belief in credulity parameter $c$ is large enough to induce coordination $-\frac{q-7p}{7} < c$. Table 5 shows that $-\frac{q-7p}{7} > 0$ only holds when $q$ is equal to 1 or 2.

*Table 5 The values of $-\frac{q-7p}{7}$ map the region of effectiveness of communication in the fire game under a norm (in bold).*

| p/q | 4 | 3 | 2 | 1 |
|---|---|---|---|---|
| 0 | -0.57 | -0.43 | -0.29 | -0.14 |
| 1/7 | -0.43 | -0.29 | -0.14 | 0.00 |
| 2/7 | -0.29 | -0.14 | 0.00 | **0.14** |
| 1/3 | -0.24 | -0.10 | **0.04** | **0.19** |

A caveat: the condition $-\frac{q-7p}{7} > 0$ is a necessary but not sufficient condition for proposition 1 to hold. Only the parameter $q$ is known, while the distribution of $p \in (0, \frac{1}{3})$ and $c \in (0, p)$ are not observed in the experiment. If communication has an impact only for $q$ equal to 1 or 2, conjecture 1 is supported. If not, it might be that the conditions on $p$ or $c$ are not met. Hence, proposition 1 leads to:

*Hypothesis 1: communication improves coordination only when q equals 1 or 2.*

Otherwise, when $q$ equals 3 or 4 coordination is obvious because the expected number of F choices is already below the threshold belief $q$. When the tipping point $q$ is high, coordination is straightforward and there is no possible improvement through communication.[8]

*Proposition 2: Under a norm, regardless of communication, coordination is higher when* $q > 7p$

With the same caveat as for hypothesis 1, this translates into the following hypothesis to be tested:

*Hypothesis 2: Under a norm, regardless of communication, coordination is higher when q equals 3 or 4.*

The model also yields an interesting but hardly testable prediction: that a weak norm hampers coordination. If this is true, less pre-communication and more post-communication coordination is expected without a norm. Even though I cannot build an experimental counterfactual without the norm, our results can be roughly compared to results from similar experiments conducted under a neutral framing (under the assumption that population types are constant across samples), as I return to in the discussion.

# 5 Results

I first present descriptive results of choices over rounds and for the average impact of communication for stable and risk treatments. I then move to a formal test of hypotheses 1 and 2.

## 5.1 Descriptive results

Figure 1 shows average choice frequencies across rounds for increasing and stable risk groups during baseline and treatment rounds. Despite the fact that CF is always strictly dominated, it is chosen with an overall frequency of 37%, indicative of the salience of the fire control norm.

Choices A are consistently more frequent than F across all rounds. Communication, introduced from round six, seems to have little impact on the frequency of A and F choices, confirming that, under a norm, there is high – although not perfect – coordination, even in absence of communication, and that introducing communication does not convey much additional

[8] When q equals 3 or 4 risk and payoff dominance select the same equilibria, different equilibria for q equal to 1 and 2. Level-k produces the same predictions than the equilibrium model, however, interpretation of results differ across the two, as we return to in the discussion.

information to the players. As expected, there is a downward trend in CF choices, because over time players become more sophisticated.

*Figure 1 Communication impact under increasing and stable risk scenarios*

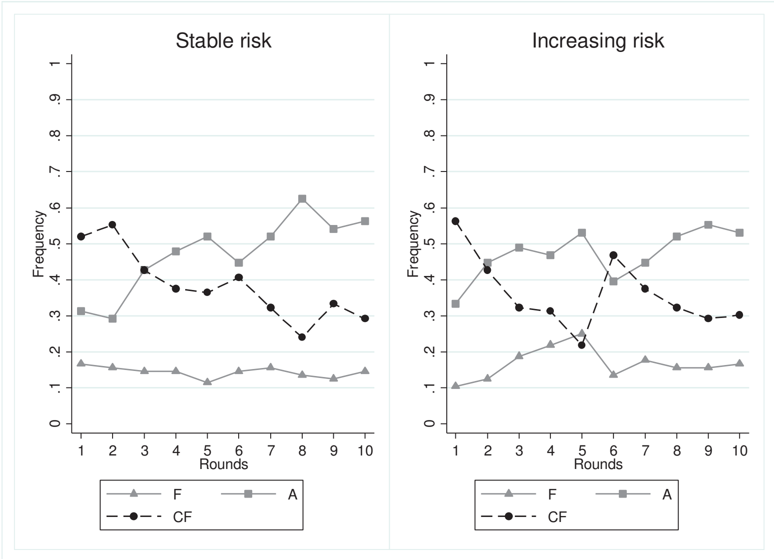



Table 6 shows the impact of communication on coordination, measured by the A-F ratio $\frac{A}{A+F}$ (at the round level for each session), for each risk treatment. Coordination is high in pre-communication rounds, and the increase after communication is small and insignificant for a t-test as well as for a non-parametric Wilcoxon rank-sum test.

*Table 6 Communication impact on A-F ratio*

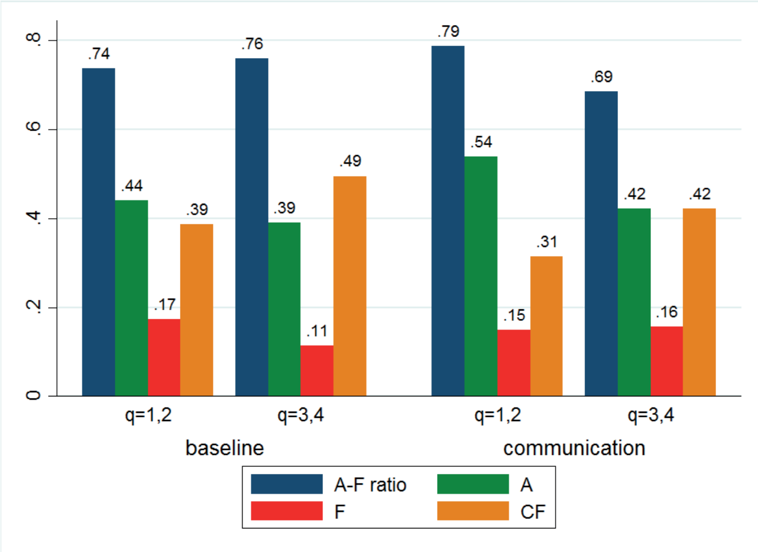
| $\frac{A}{A+F}$ | Baseline | Communication | Difference | T-test (p-value) | Wilcoxon (p-value) |
|---|---|---|---|---|---|
| Stable risk | 0.778<br>(0.224) | 0.813<br>(0.215) | 0.035<br>(0.0144) | 0.5 | 0.319 |
| Increasing risk | 0.731<br>(0.277) | 0.751<br>(0.259) | 0.020<br>(0.173) | 0.708 | 0.881 |
| Total | 0.754<br>(0.253) | 0.782<br>(0.240) | -0.027<br>(0.011) | 0.456 | 0.414 |

Numbers in parentheses are standard deviations.

Figure 2 displays average choice frequencies and A-F ratio for different belief thresholds $q$ during baseline and treatment rounds. There seems to be some pro-coordination impact of communication when $q$ is equal to 1 or 2, while this impact seems even negative for higher $q$, supporting hypothesis

1. Finally, there appears to be little evidence supporting hypothesis 2, that coordination is higher when $q$ is equal to *3* and 4. In the next sessions I formally test these hypotheses.

*Figure 2 Average choice frequency and A-F ratio during baseline and communication rounds and for different belief thresholds q*




## 5.2 Communication under a norm

I test the necessary condition for effective communication on individual choices in a multinomial logit model using F as the base category, and on round aggregate choices in an OLS regression on the A-F ratio. The latter is a more robust but inefficient model. The models test the interaction effect between communication and the belief threshold $q$. Admittedly, the test suffers from an order effect because $q$ is not randomized across rounds. To alleviate this shortcoming I merge data from sessions with increasing and stable risk and I control for learning with an inverse round trend, which captures both learning across rounds as well as the difference between early and later rounds[9].

The full specification for the multinomial logit includes individual and session level random effects, and the latter captures the origin of static intra-session correlations (Fréchette, 2012), while regression on the AF-ratio use round level observations and session level random effects. I introduce a variable which measures the cumulative occurrences in the experiment of drought related exogenous fires, capturing both the effect of an exogenous fire occurring in the previous

[9] Typically, models of coordination predict outcomes for one-shot games only. Experiments on repeated games are not strictly testing theory, because choices are affected by learning and signaling across rounds. I try to cope with this introducing the inverse round trend. And discuss further implications below.

round and the number of exogenous fires experienced in all the preceding rounds. Demographic variables are introduced to control for age and gender as well as group composition of age and gender. Finally, I introduce a variable capturing perceived real life restrictions to the adoption of fire-free techniques. This was found to be relevant in Cammelli and Angelsen (in this thesis), who analyzed other treatments in the same experiment. This variable controls for the lower chance of choosing the Pareto dominant alternative as a consequence of real life constraints. I test robustness dropping stable risk observations, the session level random effects and the inverse round trend, and introducing cluster robust standard errors at the individual (or round) level (full results in Appendix). Table 7 reports estimation results for four different model specifications (full results in Appendix). In the first model, only the treatment variables are included. The second adds experiment design controls, while the third also includes demographic and session level variables. The last specification drops the session level random effects.

*Table 7 Impact of communication on choosing A instead of F for varying q (Multinomial logit and OLS regression)*

| | A/F (1) | A/F (2) | A/F (3) | A/F (4) | AF ratio (1) | AF ratio (2) | AF ratio (3) | AF ratio (4) |
|---|---|---|---|---|---|---|---|---|
| Communication if q=3, 4 | -0.267 | -0.645 | -0.655 | -0.660 | -0.0742 | -0.0876 | -0.0873 | -0.0762 |
| | (0.394) | (0.505) | (0.506) | (0.505) | (0.0607) | (0.0782) | (0.0782) | (0.0978) |
| Communication if q=1, 2 | 0.576*** | 0.724*** | 0.699*** | 0.714*** | 0.0501* | 0.0783* | 0.0786* | 0.0931** |
| | (0.180) | (0.269) | (0.269) | (0.266) | (0.0304) | (0.0432) | (0.0433) | (0.0459) |
| Dummy if q=1, 2 | -0.388 | -0.903** | -0.893** | -0.914** | -0.0324 | -0.0696 | -0.0696 | -0.0685 |
| | (0.338) | (0.405) | (0.405) | (0.405) | (0.0511) | (0.0615) | (0.0615) | (0.0703) |
| Cumulative drought occurrence | | -0.423*** | -0.401*** | -0.416*** | | -0.0504** | -0.0507** | -0.0649*** |
| | | (0.144) | (0.145) | (0.140) | | (0.0229) | (0.0230) | (0.0215) |
| Inverse round trend | | -1.527*** | -1.503*** | -1.536*** | | -0.121 | -0.121 | -0.130 |
| | | (0.515) | (0.514) | (0.515) | | (0.0797) | (0.0797) | (0.0867) |
| Increasing risk | | -0.379 | -0.276 | -0.282 | | -0.0278 | -0.0414 | -0.0402 |
| | | (0.499) | (0.520) | (0.401) | | (0.0676) | (0.0732) | (0.0364) |
| Malegroup | | | 0.00600 | 0.00697 | | | 0.00257 | 0.00259** |
| | | | (0.0183) | (0.0146) | | | (0.00222) | (0.00121) |
| Agegroup | | | -0.0169 | -0.0152 | | | 0.00102 | 0.000708 |
| | | | (0.0373) | (0.0305) | | | (0.00457) | (0.00227) |
| Age | | | 0.0534*** | 0.0524*** | | | | |
| | | | (0.0153) | (0.0164) | | | | |
| Male | | | 0.0915 | 0.0725 | | | | |
| | | | (0.443) | (0.473) | | | | |
| No alternatives to fire use | | | -0.991** | -0.921** | | | 0.0499 | 0.0482 |
| | | | (0.389) | (0.395) | | | (0.153) | (0.0821) |
| Constant | 1.645*** | 3.017*** | 1.531 | 1.454 | 0.767*** | 0.879*** | 0.620*** | 0.622*** |
| | (0.409) | (0.658) | (1.977) | (1.539) | (0.0547) | (0.0957) | (0.239) | (0.0874) |
| Individual level RE | Yes | Yes | Yes | Yes | | | | |
| Session level RE | Yes | Yes | Yes | No | Yes | Yes | Yes | No |
| Log likelihood | -1512.8068 | -1491.565 | -1474.020 | -1483.621 | 11.850034 | 14.897636 | 15.574848 | -8.8190663 |
| Observations | 1,920 | 1,920 | 1,920 | 1,920 | 240 | 240 | 240 | 240 |

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; estimates for the odds of choosing CF instead of F are reported in Appendix.

In all models the impact of communication is significant only when $q \in [1,2]$, supporting hypothesis 1. This result is robust to all tests, except when all covariates and sessions level random effects are dropped (in Appendix). The dummy variable for $q \in [1,2]$ is negative, supporting hypothesis 2, but it is significant only for individual choices in the multinomial logit and after that experiment design covariates are introduced. Robustness tests in the appendix shows that the latter result also disappears when dropping observations from the stable risk sessions. Whether this is due to unbalance in $q$ frequencies in the whole sample or to an order effect arising from dropping the stable risk observations cannot be assessed.

Negative although insignificant coefficients reveal that communication might hamper coordination when $q \in [3,4]$. This is not predicted by the model, and might result from a form of betrayal aversion (Koehler and Gershoff, 2003), which is made salient by hypocritical communication (committed action profiles are not respected).

# 6 Discussion and conclusion

## 6.1 Communication of requests increases coordination when the norm is weak

I ran a framed field experiment to study the impact of communication under a norm on a real coordination problem, namely fire management and technology adoption by smallholder Brazilian farmers (Cammelli and Angelsen, in this thesis). Building on Ellingsen and Östling (2010), I conducted a level-k analysis and show that a norm generally improves coordination, but that a weak social norm undermines coordination in presence of communication of intentions when players are sophisticated enough. I also find that communication of requests can increase coordination if players are believed to be credulous and the norm is weak compared to the belief threshold (i.e. when coordination is not obvious).

The predictions from the level-k model are similar to the ones obtained from equilibrium theory for communication of intents, discussed in section 4.1. However, to achieve the same exact predictions about coordination and also allow for CF choices in equilibrium, assumptions are needed about an intrinsic preference (or internalized norm) for fire control directly affecting utility (so that CF is chosen by rational players), and that no taboo exists (so that coordination can be achieved through communication of intentions). The assumptions of bounded rationality, weak norm (only affecting beliefs) and the taboo are better suited to the context in which the experiment takes place, because in line with previous research, as discussed in section 2.

Similar to Dugar and Shahriar (2012), results show that under a norm when the risk and payoff dominant equilibria coincide ($q$ equals 3 or 4), coordination might be higher, even without communication (yet, this result is not robust to model specification), and that when the belief threshold is lower (and/or the social norm weaker) and payoff and risk dominance select different equilibria, communication conveys additional information.

The test, however, suffers from three main flaws. First, it is carried out in a within design across repeated rounds, while level-k and equilibrium theories make predictions for one shot games only.[10] Second and related, an order effect in the increasing risk treatment and communication, together with game repetition, might confound the impact of $q$ belief thresholds and communication with learning and signaling through previous rounds. Third, $q$ is varied through risk, rather than by changing sure payoffs, creating room for risk aversion to shape behavior. Yet, that risk aversion has an impact on coordination in this experiment is ruled out by Cammelli and Handberg (in this thesis).

I attempt to control for learning with an inverse round trend. In the multinomial logit model, results for hypotheses 2 depend on the introduction of this control variable. Accounting for learning and signaling would require clustering at the session level (Fréchette, 2012). Alternatively, explicit structural models of learning and signaling or dynamic panel data models can be used (e.g. Moffatt, 2015: p419). Similarly to many experimental works, I do not have enough data to cluster standard errors at the session level, and the use of lagged variables to estimate dynamic panel models would unbalance baseline and communication rounds. Analyzing other (more numerous) sessions in the same experiment, Cammelli and Angelsen (in this thesis) and Cammelli and Handberg (in this thesis) control for learning and signaling through beliefs across rounds and session level clustering. Including beliefs about other players choosing F would not make sense in the above model because beliefs themselves are the underlying drivers for the hypotheses at test. Finally, structural models of signaling and learning would be much of a contribution in itself and are out of the scope of this paper.

### 6.2 A weak social norm hampers coordination

The level-k model predicts that a weak social norm hampers communication of intentions when players are sophisticated enough. There is strong evidence of a fire control norm in our population. However, I did not provide an adequate counterfactual: what would have been the coordination outcome and the impact of communication in the absence of a social norm?[11]

Collecting data about coordination with and without a norm is challenging. It would have been difficult to design an *ad hoc* experiment because the norm is embedded in the population of study, and building a counterfactual under the same framing is not feasible. One way to offset the norm would be to recruit participants belonging to the same population to play the fire game without the fire labels and a neutral framing. Yet, the framing affects the game beyond the social norm and the two experiments outcome comparison would not restrict to the impact of the social norm alone. For instance, the fire game frames F as imposing a loss on players choosing A, an important leverage on social preferences. This effect would disappear under a neutral framing. Another way to restrict the impact of the norm could be to hide the identity of the sender offsetting the taboo on F messages. However, if part of this taboo occurs through internalized costs the design would fail

[10] In repeated game there is opportunity for learning about other players' type, $p$ and $c$ parameters, in addition to change in own type and cross-rounds signaling.

[11] For the purpose of this section, I assume that types distributions are stable across samples. An experiment designed to test communication with and without a norm should provide sufficient data to determine types composition across sessions.

to offset the norm. A third alternative is to build a norm into the experiment that is not embedded in the population, and to manipulate it by design, similar to the redistributive norm in Gächter et al. (2017). However, artificial norms are less convincing treatments than actual norms, especially concerning the establishment of a taboo.

In absence of an experimental counterfactual I compare results from the fire experiment with results from similar experiments conducted under a neutral framing. A large number of studies have tested the impact of communication on coordination in the lab (Blume and Ortmann, 2007; Charness, 2000; Charness and Grosskopf, 2004; Cooper et al., 1992; Duffy and Feltovich, 2002, 2006; Warziniack et al., 2007) and lab in the field (Brandts and Cooper, 2007). Crawford (1998) and Ellingsen and Östling (2010) review more experimental results. In general, these studies find limited coordination without communication.

Blume and Ortmann (2007) report on the experiment with the most similar design to the fire game. In a between design, they test the impact of communication in a variety of eight rounds repeated minimum and median games[12] with nine players. Using a communication protocol that allows for simultaneous statements of intent, they find little coordination in the baseline rounds, and high convergence on the Pareto dominant choice with communication. The results are robust across types of game: the level of strategic uncertainty associated with the risky choices does not hamper the impact of communication.

Their study is not directly comparable to the fire game, because communication is introduced in a between subjects design, requests are not allowed and there is no exogenous risk. Yet, I argue that the comparison is conservative, and suggests that the social norm hampers the impact of communication. I find about a 0.03 increase in average efficiency (A-F ratio) after communication, while Blume and Ortmann (2007) find at least a 0.2 increase in the median game. This happens despite I allow for face-to-face communication, a more generous communication protocol (Charness, 2000). Also, despite exogenous risk, I find a level of coordination in pre-communication rounds comparable to Blume and Ortmann (2007) in the median game. Yet, less coordination would have been expected in the fire game than in a design without exogenous risk, suggesting that the norm is at work.

### 6.3 Policy implications

Coordination problems are ubiquitous in society and the economy. Social norms and conventions are often sufficient to achieve coordination, but are at times too weak to provide sufficient assurance. When a norm is weak, belief in the others' hypocrisy leads to hypocritical communication and failure to coordinate, with related efficiency losses.

The level-k model shows that social norms and communication in coordination games are expected to have the same effect. However, when the norm and communication operate together, as a consequence of the taboo byproduct of the norm, communications of intent is no more credible.

[12] These games are assurance games with more than 2 strategies.

Then, when the norm is weak, and players are sophisticated enough, less coordination is achieved than with communication alone. Nevertheless, when communication of requests is allowed and players believe in the opponents' credulity, communication (of requests) can improve coordination even in presence of a weak norm.

Farmers in the Brazilian Amazon share a weak fire control norm: everyone states to implement appropriate fire control measures, but some underprovide them, protracting fire risk. Over time, deception degrades the norm and the self-signaling ability of communication. The pattern of farmers' behavior in the experiment is coherent with the level-k model predictions. Facing a weak social norm, community based fire mitigation policies have the potential to improve coordination when exogenous fire risk is high (as in drought years) by fostering communication persuasiveness, for instance by training local leaders.

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# 8 Appendix

## 8.1 Full estimate results

Table 8 reports the impact of communication on the odds of choosing CF instead of F for varying *q* for the multinomial logit model reported in text.

*Table 8 Impact of communication on the odds of choosing CF instead of F for varying q (multinomial logit)*

| | CF/F (1) | CF/F (2) | CF/F (3) | CF/F (4) |
|---|---|---|---|---|
| Communication if q=3, 4 | -0.639* | 0.113 | 0.0968 | 0.0679 |
| | (0.371) | (0.483) | (0.483) | (0.482) |
| Communication if q=1, 2 | -0.140 | 0.371 | 0.335 | 0.320 |
| | (0.187) | (0.279) | (0.279) | (0.276) |
| Dummy if q=1, 2 | -1.293*** | -0.925** | -0.912** | -0.941** |
| | (0.323) | (0.388) | (0.388) | (0.389) |
| Cumulative drought occurrence | | -0.224 | -0.192 | -0.180 |
| | | (0.148) | (0.149) | (0.142) |
| Increasing risk | | -0.675 | -0.867 | -0.855** |
| | | (0.552) | (0.534) | (0.372) |
| Inverse round trend | | 0.781 | 0.814* | 0.798* |
| | | (0.485) | (0.485) | (0.484) |
| Malegroup | | | -0.0229 | -0.0212 |
| | | | (0.0185) | (0.0133) |
| Agegroup | | | -0.00358 | -0.00132 |
| | | | (0.0376) | (0.0280) |
| Age | | | 0.0493*** | 0.0482*** |
| | | | (0.0137) | (0.0149) |
| Male | | | 0.140 | 0.125 |
| | | | (0.390) | (0.426) |
| No_alternative | | | 0.380 | 0.440 |
| | | | (0.352) | (0.360) |
| Constant | 2.423*** | 2.134*** | 1.283 | 1.123 |
| | (0.410) | (0.669) | (2.029) | (1.421) |
| Individual level RE | Yes | Yes | Yes | Yes |
| Session level RE | Yes | Yes | Yes | No |
| Log likelihood | -1512.8068 | -1491.5655 | -1474.0209 | -1483.6218 |
| Observations | 1,920 | 1,920 | 1,920 | 1,920 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9 and 10 show the impact of dropping the inverse round trend from the specification, and to drop the stable risk sessions from the sample. Multinomial logits 3 and 4 in table 10 are estimated without session level random effects. Conjecture 1 is always supported, while conjecture 2 only holds in the multinomial logit when the inverse round trend is introduced. When the trend is dropped the dummy for q equal to one or two is barely insignificant. The p-value further increase when dropping the stable risk sessions from the sample. Finally, Table 11 reports estimates with individual level random effects and clustered standard errors. Except when all covariates and random effects are dropped simultaneously on the AF ratio, results do not differ from what reported in the main text.

*Table 9 Robustness test for increasing risk sample and inverse round trend on the AF ratio (OLS regression)*

| | AF ratio (1) | AF ratio (2) | AF ratio (3) | AF ratio (4) |
|---|---|---|---|---|
| Communication if q=3, 4 | -0.0225 | -0.0873 | 0.0344 | -0.0589 |
| | (0.0660) | (0.0782) | (0.0707) | (0.112) |
| Communication if q=1, 2 | 0.103** | 0.0786* | 0.189*** | 0.168** |
| | (0.0405) | (0.0433) | (0.0630) | (0.0655) |
| Dummy if q=1,2 | -0.0192 | -0.0696 | -0.0161 | -0.0946 |
| | (0.0522) | (0.0615) | (0.0526) | (0.0898) |
| Cumulative drought occurrence | -0.0443* | -0.0507** | -0.0931** | -0.0963*** |
| | (0.0227) | (0.0230) | (0.0374) | (0.0373) |
| Inverse round trend | | -0.121 | | -0.163 |
| | | (0.0797) | | (0.151) |
| Increasing risk | -0.0294 | -0.0414 | | |
| | (0.0729) | (0.0732) | | |
| Male group | 0.00256 | 0.00257 | 0.00281 | 0.00285 |
| | (0.00222) | (0.00222) | (0.00311) | (0.00311) |
| Age group | 0.00115 | 0.00102 | 0.00688 | 0.00691 |
| | (0.00458) | (0.00457) | (0.00794) | (0.00794) |
| No alternatives to fire use | 0.0506 | 0.0499 | -0.254 | -0.256 |
| | (0.153) | (0.153) | (0.282) | (0.282) |
| Constant | 0.553** | 0.662** | 0.457 | 0.577 |
| | (0.259) | (0.268) | (0.421) | (0.436) |
| Only increasing risk session | No | No | Yes | Yes |
| Session level RE | Yes | Yes | Yes | Yes |
| Log pseudo-likelihood | 14.419065 | 15.574848 | 14.404842 | 14.98094 |
| Observations | 240 | 240 | 120 | 120 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Table 10 Robustness test for increasing risk sample and inverse round trend on the odds of choosing CF or A instead of F (multinomial logit)*

| | CF/F(1) | A/F(1) | CF/F(2) | A/F(2) | CF/F(3) | A/F(3) | CF/F(4) | A/F(4) |
|---|---|---|---|---|---|---|---|---|
| Communication if q=3, 4 | -0.336 | 0.118 | 0.0968 | -0.655 | -0.358 | 0.373 | 0.545 | 0.0308 |
| | (0.411) | (0.430) | (0.483) | (0.506) | (0.468) | (0.495) | (0.750) | (0.789) |
| Communication if q=1, 2 | 0.175 | 0.972*** | 0.335 | 0.699*** | 0.814** | 1.254*** | 1.013** | 1.179*** |
| | (0.264) | (0.253) | (0.279) | (0.269) | (0.408) | (0.416) | (0.429) | (0.438) |
| Dummy if q=1,2 | -1.259*** | -0.297 | -0.912** | -0.893** | -1.536*** | -0.192 | -0.777 | -0.475 |
| | (0.328) | (0.344) | (0.388) | (0.405) | (0.356) | (0.373) | (0.605) | (0.636) |
| Cumulative drought occurrence | -0.248* | -0.319** | -0.192 | -0.401*** | -0.222 | -0.542** | -0.190 | -0.558** |
| | (0.146) | (0.141) | (0.149) | (0.145) | (0.240) | (0.245) | (0.242) | (0.247) |
| Inverse round trend | | | 0.814* | -1.503*** | | | 1.569 | -0.623 |
| | | | (0.485) | (0.514) | | | (1.057) | (1.144) |
| Increasing risk | -0.0224 | 0.00533 | -0.0229 | 0.00600 | -0.00306 | -0.00440 | -0.00356 | -0.00417 |
| | (0.0184) | (0.0183) | (0.0185) | (0.0183) | (0.0154) | (0.0194) | (0.0155) | (0.0195) |
| Male group | -0.00464 | -0.0151 | -0.00358 | -0.0169 | -0.00554 | -0.0122 | -0.00586 | -0.0122 |
| | (0.0375) | (0.0372) | (0.0376) | (0.0373) | (0.0359) | (0.0455) | (0.0361) | (0.0457) |
| Age group | 0.0492*** | 0.0534*** | 0.0493*** | 0.0534*** | 0.0494** | 0.0961*** | 0.0492** | 0.0964*** |
| | (0.0137) | (0.0152) | (0.0137) | (0.0153) | (0.0193) | (0.0252) | (0.0194) | (0.0253) |
| Age | 0.142 | 0.0908 | 0.140 | 0.0915 | 0.765 | 1.341* | 0.764 | 1.345* |
| | (0.388) | (0.438) | (0.390) | (0.443) | (0.567) | (0.736) | (0.570) | (0.738) |
| Male | 0.368 | -0.970** | 0.380 | -0.991** | 0.164 | -1.894*** | 0.181 | -1.907*** |
| | (0.350) | (0.384) | (0.352) | (0.389) | (0.503) | (0.637) | (0.506) | (0.639) |
| No alternatives to fire use | -0.973* | -0.153 | -0.867 | -0.276 | | | | |
| | (0.528) | (0.516) | (0.534) | (0.520) | | | | |
| Constant | 2.052 | 0.264 | 1.283 | 1.531 | -0.263 | -1.617 | -1.413 | -1.185 |
| | (1.975) | (1.924) | (2.029) | (1.977) | (1.698) | (2.179) | (1.866) | (2.322) |
| Individual level RE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Session level RE | Yes | Yes | Yes | Yes | No | No | No | No |
| Log pseudo-likelihood | -1491.3133 | -1491.3133 | -1474.0209 | -1474.0209 | -751.74401 | -751.74401 | -747.55653 | -747.55653 |
| Observations | 1,920 | 1,920 | 1,920 | 1,920 | 960 | 960 | 960 | 960 |

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

*Table 11 Robustness test for clustered standard errors at the individual level on the odds of choosing CF or A instead of F and AF-ratio (multinomial logit and OLS regressions)*

| | CF/F (1) | A/F (1) | CF/F (2) | A/F (2) | CF/F (3) | A/F (3) | A-F ratio (1) | A-F ratio (4) | A-F ratio (7) |
|---|---|---|---|---|---|---|---|---|---|
| Communication if q=3, 4 | -0.647 | -0.273 | 0.102 | -0.645 | 0.0679 | -0.660 | -0.0742 | -0.0770 | -0.0762 |
| | (0.489) | (0.459) | (0.580) | (0.547) | (0.576) | (0.546) | (0.0890) | (0.0978) | (0.0978) |
| Communication if q=1, 2 | -0.139 | 0.579*** | 0.381 | 0.748*** | 0.320 | 0.714*** | 0.0501 | 0.0921** | 0.0931** |
| | (0.200) | (0.179) | (0.253) | (0.239) | (0.252) | (0.240) | (0.0355) | (0.0455) | (0.0459) |
| Dummy if q=1, 2 | -1.254*** | -0.377 | -0.957** | -0.924** | -0.941** | -0.914** | -0.0235 | -0.0686 | -0.0685 |
| | (0.382) | (0.375) | (0.465) | (0.460) | (0.463) | (0.459) | (0.0648) | (0.0709) | (0.0703) |
| Cumulative drought occurrence | | | -0.234 | -0.444*** | -0.180 | -0.416*** | | -0.130 | -0.130 |
| | | | (0.146) | (0.136) | (0.147) | (0.140) | | (0.0852) | (0.0867) |
| Inverse round trend | | | -0.648* | -0.369 | -0.855** | -0.282 | | -0.0279 | -0.0402 |
| | | | (0.379) | (0.392) | (0.377) | (0.390) | | (0.0361) | (0.0364) |
| Increasing risk | | | 0.756 | -1.563*** | 0.798 | -1.536*** | | -0.0639*** | -0.0649*** |
| | | | (0.533) | (0.545) | (0.535) | (0.547) | | (0.0198) | (0.0215) |
| Malegroup | | | | | -0.0212 | 0.00697 | | | 0.00259** |
| | | | | | (0.0137) | (0.0158) | | | (0.00121) |
| Agegroup | | | | | -0.00132 | -0.0152 | | | 0.000708 |
| | | | | | (0.0264) | (0.0292) | | | (0.00227) |
| Age | | | | | 0.0482*** | 0.0524*** | | | |
| | | | | | (0.0161) | (0.0171) | | | |
| Male | | | | | 0.125 | 0.0725 | | | |
| | | | | | (0.446) | (0.487) | | | |
| No alternatives to fire use | | | | | 0.440 | -0.921** | | | 0.0482 |
| | | | | | (0.349) | (0.395) | | | (0.0821) |
| Constant | 2.373*** | 1.630*** | 2.137*** | 3.042*** | 1.123 | 1.454 | 0.760*** | 0.885*** | 0.681*** |
| | (0.418) | (0.408) | (0.715) | (0.687) | (1.350) | (1.430) | (0.0592) | (0.0895) | (0.137) |
| Individual level RE | Yes | Yes | Yes | Yes | Yes | Yes | | | |
| Session level RE | No | No | No | No | No | No | No | No | No |
| Log pseudo-likelihood | -1525.5568 | -1525.5568 | -1502.3713 | -1502.371 | -1483.6218 | -1483.6218 | -16.2035 | -11.47812 | -8.8190663 |
| Observations | 1,920 | 1,920 | 1,920 | 1,920 | 1,920 | 1,920 | 240 | 240 | 240 |

Individual or round clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

### 8.2 Instructions

Please refer to the appendix to all papers

# Behavioural predictors of fires in the Amazon: preferences and perceptions in coordination and predictor validity

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## Abstract

Mitigating fire risk in the Brazilian Amazon is a coordination problem. We test the impact of preferences and perceptions on coordination. Through both experimental (incentive compatible) and survey (stated) data about smallholder farmers' behaviour, preferences and perceptions, we show that risk perceptions, but not risk aversion, strongly influences the propensity to coordinate. Pro-social preferences weakly increase the propensity to coordinate. These findings enlarge the toolbox that policymakers can use to tip behaviour towards the efficient equilibrium, and we suggest explicit policy implications for fire risk mitigation in the Amazon. We further test the external validity of the results through a novel approach based on commonality of predictors: predictor validity. This approach avoids spurious correlation and bias due to confounding factors, which are intrinsic to behavioural validity tests based on correlating experiment choices with stated or observed choices. We find that external validity holds for parts of the experiment, while the test is inconclusive for others. We describe and discuss the use of predictor validity, and identify new challenges for external validity tests.

**Keywords:** Coordination, external validity, Brazilian Amazon, preferences, framed field experiments

**JEL codes: B41, C93, Q56**

# 1. Introduction

Compelling global challenges as well as daily situations can be characterized as coordination problems. Choices related to a variety of situations, from diet or transport to pro-environmental behaviours are characterized by network externalities and social norms that induce strategic complementarities: one option is best for all, but no one has an incentive to choose it alone. There is a tipping point after which conformance with the others' choice is optimal for the individual. For example, the more electric vehicles on the road, the more charging stations will be made available. The higher the number of users of public transports, the lower the cost of improving the network. The stronger the norm for well-defined ethical or environmental standards, the lower the cost of adoption.

Focal points (Schelling, 1960), heuristics (Tversky and Kahneman, 1975) and social norms – defined as stable beliefs (Aoki, 2001: p10) or as widely spread patterns of behaviour (Ostrom, 2000) – define beliefs about others' choices and, therefore, whether a tipping point is passed or not. By changing beliefs, policies can create tipping points when they do not exist, or provide sufficient reasons to overcome them (Aoki, 2001; Bromley, 2008; Dixit, 2003; Nyborg et al., 2016). The ability of policies to affect beliefs is context dependent and especially affected by observability of choices (Andreoni, 1990; Brekke et al., 2003) and the coordination benefit (Battalio et al., 2001; Nyborg et al., 2016). The expected coordination benefit is defined by three elements: (i) beliefs, how many are believed to conform; (ii) perceptions, in the sense of McFadden et al. (1999)[1], how risky is the outcome; and (iii) preferences, how risky outcomes are evaluated and whether the outcomes of others are taken into consideration. Preferences and perceptions, conditional on beliefs, define whether and where tipping points exist. For instance, (social) preference for an inefficient but equitable outcome can establish otherwise unexpected tipping points, which favours a behaviour that is otherwise strictly dominated.

In this paper, we assess and compare the impact of beliefs, perceptions and preferences in the same model using experimental and survey data about a compelling coordination problem: technology adoption and fire risk mitigation among Amazonian smallholders in Brazil. Potential losses from accidental fires that originate from pasture and swidden fields prevent investments in fire free and higher yield techniques. Swidden fields provide more certain outcomes as they are already burned, and do not require any flammable investment. Everyone would be better off investing into more productive fire-free technologies, but high fire risk traps smallholder Amazonians into fire use (Cammelli and Angelsen, in this thesis; Cammelli and Garrett, in this thesis).

We collected incentive-compatible and stated measures of preferences through incentivized experiments and stated methods, evaluate their validity and test their ability to predict choices in the experiment and the survey. We contribute to the literature on coordination by exploring risk preferences, beliefs, social preferences and risk perceptions simultaneously. Previous research on coordination has focused on structural features of the game (e.g. Battalio et al., 2001; Van Huyck et al., 1990; Van Huyck et al., 1997) and the role of risk aversion (Al-Ubaydli, 2011; Al-Ubaydli

[1] "The cognition of sensations" (p.74)

et al., 2013; Büyükboyacı, 2014). Social preferences are relevant for the coordination characterizing fire management because miscoordination is a loss for those who choose the efficient strategy (i.e., high yield but risky technologies). Further, with the exceptions of Alpizar et al. (2011) and Chidambaram et al. (2014), previous research has taken place only in the lab. We take our test to the field, with a relevant sample facing an actual coordination problem.

We also contribute to the research on agricultural technology adoption in the context of risk and/or strategic uncertainty (e.g. Alpizar et al., 2011; Holden and Quiggin, 2017; Liu, 2013; Ward et al., 2013), by simultaneously considering beliefs, preferences and perceptions.

Finally, we assess the external validity of the experiment by introducing – alongside behavioural validity (Handberg and Angelsen, 2015) – a novel validity approach based on commonality of predictors between experimental and stated behaviour.

As discussed below, both survey and experiment measures of choices are possibly biased. Comparing choice frequencies across the two and choices correlation (i.e., behavioural validity) fail to identify external validity in two ways. First, by rejecting external validity when this is disguised by measurement error in the measure used as counterfactual, and second, by failing to reject external validity when the correlation is spurious, e.g. due to a common source of measurement error (cf. Guala, 2002; Siakantaris, 2000).

We propose a test based on commonality of predictors – predictor validity – that might mitigate the challenges mentioned above. The test assesses the extent relevant variables, identified through theory and previous empirical studies, predict both choices in and out of the experiment. The variables are also collected through stated and experimental (incentive compatible) methods. This paper assesses risk perception, belief and risk and social preferences measured through stated and experimental (incentive compatible) methods. We show that testing the impact of variables on choices in and out of the experiment is both a test of their role in coordination and a test of the external validity of the experiment.

If stated and revealed preferences and perceptions systematically predict (or do not predict) choices within and outside the experiment, both the impact of preferences and the predictor external validity of the experiment are individuated. However, when such correlations are not systematic (e.g. a variable predicts only choices in the experiment), distinguishing the role of the variable from lack of predictor validity is not straightforward. We show that predictors should be theoretically relevant, testable across behaviour measures (e.g. the predicting variable is relevant for both behaviours) and should not be correlated with the error in the behaviour measure. We discuss the consequences of violating these requirements with examples from our case.

Controlling for beliefs in the experiment, we find that (low) risk perception, but not risk aversion, is the strongest predictor of coordination, both in the experiment and in the survey. Pro-social preferences, elicited through both stated and incentive compatible method, predict coordination in the survey, but not in the experiment. We find support for both behavioural and predictor validity for some, but not all choices in the experiment. The predictor validity test suggests that the stated counterfactual might be biased for fire control choices. However, lack of predictors satisfying all criteria listed above prevents a clear conclusion for these choices. We suggest that policies intending to mitigate fire risk should consider and aim to influence beliefs and risk perceptions, and possibly social preferences.

The following section presents the background for the experiment and the coordination case. Section 3 presents a framework for predictor validity. Section 4 discusses methods, including the experiment, the preference elicitation procedures, theoretical prediction and empirical modelling. Section 5 presents the results, before section 6 discusses them and section 7 concludes.

## 2. Forest fires and farmers behaviour

Forest fires affect large parts of the Brazilian Amazon. Fires cause severe losses of biodiversity, release $CO_2$ emissions greater than those from the entire Brazilian energy sector, and threaten the livelihood of vulnerable smallholder farmers (Alencar et al., 2015; Anderson et al., 2015; Barlow et al., 2016; Berenguer et al., 2014; Nepstad et al., 1999). In the Amazon, fire ignition sources are anthropogenic, mainly related to swidden fields and pasture management fires that accidentally spread to neighbouring areas (Acevedo-Cabra et al., 2014; Bowman et al., 2008; Nepstad et al., 2001; Nepstad et al., 2009). Global climate trends – particularly related to *El Niño* events – dry forests, which creates conditions for fire ignition and propagation (Malhi et al., 2009; Malhi et al., 2008). This is reflected in the recent surge of Amazonian forest fires.

Amazonian farmers ignite fires for an array of agricultural purposes (Brondizio and Moran, 2008; Carmenta et al., 2013; Toniolo, 2004). While fire is a cheap agricultural method to clear, weed and fertilize the land, containing the fire within the intended plot (fire control) can cost as much as 80% of a farmer's profits (Nepstad et al., 1999). Farmers have benefits from investing in fire control, as fires may spread to the rest of their property, accidentally burning assets and other crops (Bowman et al., 2008). However, if neighbouring fire users do not control their fires, the property might nonetheless be burnt, meaning that the fire control investment was pointless. During drought years, fires are also likely to spread across large areas (Alencar et al., 2015) and affect farms located far from the original ignition source. Farmers refer to these fires as *fires* [coming] *from afar*, as they are exogenous to the farm and the neighbourhood.

Instead of preparing agricultural land with fire, farmers could invest in fire free technologies, which typically require flammable on-land investments (de Mendonça et al., 2004; Hoch et al., 2009; Hoch et al., 2012; Pokorny et al., 2012). These could be tree plantations, agroforestry, pasture rotation fences, and crops that require machineries and costly fertilizers. The techniques typically yield higher but less certain incomes than preparing land with fire. As with applying fire control, farmers have no incentive to apply fire-free techniques if the risk of being exposed to a fire is sufficiently high (Nepstad et al., 2001).

Since fire free techniques yield higher income than applying fire (with or without fire control), neighbouring farmers face a coordination problem, where everybody is better off coordinating on fire free techniques. Alternatively, the farmers are trapped in a high fire risk equilibrium, where no farmer is better off individually investing in fire free techniques (Cammelli and Garrett, in this thesis). Shafran (2008) shows that strategic complementarities arise in fire management and that policies should aim to tip behaviour towards the most efficient outcome. We analyse which factors affect coordination and can be influenced by policies. Each farmer makes decisions under the risk of drought and neighbour-induced fires, and may himself impose negative externalities on neighbours through uncontrolled fire use. Consequently, beliefs about the choices of others, fire risk perceptions, risk and social preferences may all influence agricultural technique decisions.

The few previous studies exploring individual determinants for agricultural fire use and control in the Amazon remain inconclusive about what factors predict farmers' fire use decisions. More own assets exposed to fire risk have been shown to increase the use of private fire control measures (Bowman et al., 2008). Stronger social capital (measured as involvement in local institutions and perceived quality of neighbourhood relations) is also correlated with fire control adoption (Cammelli, 2014), but communities with stronger political organizations do not appear to experience less forest fires (Simmons et al., 2004). Yet unexplored, pro-social attitudes could provide incentives for coordination, especially if the individual coordination benefit is low (e.g., during drought years) and the selfish incentive is not sufficient for coordination.

A large body of evidence suggests that risk aversion both reduces and delays the uptake of new agricultural technologies in developing countries. Liu (2013) finds that risk aversion reduces the adoption of genetically modified cotton in China. Holden and Quiggin (2017) find that risk averse Malawian farmers are more prone to adopt drought resistant maize and less likely to dis-adopt traditional maize in favour of other improved varieties. Ward et al. (2013) show that Indian risk averse farmers are more prone to engage in less risky crops. How risk aversion influences technology adoption seems to depend on whether the new technology is risk reducing or risk enhancing.

In exploring risk preferences in coordination problems, Alpizar et al. (2011) measure the ability to coordinate for technology adoption in a climate risky scenario among Costa Rican coffee farmers. They find that both risk and uncertainty aversions shape farmers' choices. Al-Ubaydli (2011) finds support for risk aversion hampering coordination in lab experiments. Other studies, however, suggest that rather patience (Al-Ubaydli et al., 2013) and information on others' risk aversion (Buyukboyaci, 2014) influence coordination.

Finally, in studies of consumers behaviour perceived risk has been found to be an important confounding factor of risk preferences (Petrolia, 2016; Petrolia et al., 2013).

By exploring both risk and social preferences, beliefs and risk perceptions, we aim to individuate the impact of each, while controlling for the others. Through applying both survey and experimental methods we discuss and assess the predictor external validity of the results.

## 3. Predictor validity

Experiments typically suffer from lower external validity than do direct measures of behaviour (e.g. survey or observation). For experiments seeking empirical regularities rather than theory testing – such as framed field experiments – assessing external validity is key (Schram, 2005). In this section we discuss a shortcoming of the standard behavioural validity test (cf. Handberg and Angelsen, 2015) and suggest a novel test based on commonality of predictors. Behavioural validity is the correlation between behaviour in the experiment and an out-of-the-experiment counterfactual measure of behaviour. The main shortcoming of this approach is the problem of identifying an appropriate behavioural counterfactual (Siakantaris, 2000). If this existed "in the wild", why run experiments at all? Building the counterfactual by running another experiment would in turn open the question about the external validity of the second experiment. Predictor validity assumes that both measures of behaviour can be biased. Predictor validity is achieved if the drivers of behaviour are the same in and out of the experiment.

Compared to behavioural validity, testing predictor validity provides a richer set of information about what determines the behaviour and the potential sources of measurement error. For instance, if behavioural validity is verified, but drivers of experimental and stated behaviour differ, behavioural validity might result from spurious correlation (type I error). If behavioural validity is weak or absent, but both measures of behaviour share the same predictors, measurement error in the counterfactual might disguise behavioural validity (type II error). Understanding the predictors may also ease generalization to settings where the predictor is more relevant (Ludwig et al., 2011). We show that behavioural validity critically relies on an unbiased out-of-the-experiment counterfactual measure of behaviour, often a stated measure or another experiment (e.g. Franzen and Pointner, 2013; Voors et al., 2012). Unbiasedness can hold exceptionally, but it does not in most cases (Siakantaris, 2000). Predictor validity, instead, only relies on the assumption that predictors are not correlated with the biases potentially affecting the two measures of behaviour.

Assume that behaviour $B_e$ is observed through an experiment, and behaviour $B_{-e}$ through an unbiased counterfactual method. Behaviour in the experiment is potentially affected by a systematic error $v_e$. The standard behavioural validity test is $corr(B_e + v_e, B_{-e}) \neq 0$ under the alternative hypothesis that $corr(B_e + v_e, B_{-e}) = 0$, where the first statement is assumed to imply that $v_e = 0$. This test relies on $B_{-e}$ being an unbiased measure of the behaviour of interest and independent from the error $v_e$ (i.e. the correlation is not spurious). However, such an assumption is rarely verified.

Instead, assume that $B_{-e}$ is measured with an error $v_{-e}$ and that both errors $v_{-e}$ and $v_e$ are not classical but systematic, due for instance to Hawthorn effects and artificiality. The errors do not disappear on average $E(v) \neq 0$ and are correlated with behaviour $\sigma_{vB} \neq 0$. A biased counterfactual behaviour may cause over-rejection of behavioural validity: $corr(B_e, B_{-e} + v_{-e}) = 0$ occurs even when behaviour in the experiment corresponds to the true behaviour, but the error term in the counterfactual causes an attenuation bias. For instance, if $B_e$ is an incentive compatible and anonymous choice, and $v_{-e}$ originates from scrutiny in a stated method, lack of correlation between the experimental behaviour and the stated counterfactual disguises external validity, causing a type II error.

Consider another example, when both behaviours are measured with non-zero mean errors, and $corr(B_e + v_e, B_{-e} + v_{-e}) > 0$. Behavioural validity tests might detect mere correlation among behaviours and errors, or even correlation between the two measurement errors, causing type I error. The latter event might occur, for instance, when both measures reflect compliance with a social norm.

A stronger test can be built based on commonality of predictors. Predictor validity does not assume unbiased measure of counterfactual behaviour, instead, it only assumes an error independence condition: that a relevant predictor and the related measurement error, $P + v_P$, are not related to the errors in the measures of behaviour $corr(P + v_P, v_e) = corr(P + v_p, v_{-e}) = 0$.

Given this assumption, predictor validity is verified when the conditional means for both measures of behaviour share the same sign and significance, or both are found to be zero $\begin{cases} E(B_e + v_e|P + v_P) \gtreqless 0 \\ E(B_{-e} + v_{-e}|P + v_P) \gtreqless 0 \end{cases}$, and the sign is predicted by theory.

Assuming linear expectations, the relations above can be estimated in a linear regression model, which also accounts for relevant covariates. Insofar as predictors are measured exactly and are independent from the measurement errors in behaviour, estimated slope coefficients are unbiased, although intercepts are, and standard errors are inflated (Wooldridge, 2010; p76). If predictors are measured with classical zero mean measurement error, estimated slopes are attenuated (Wooldridge, 2010; p78). If the predictors are measured with non-classical measurement errors, the OLS estimates of the slope coefficients are biased and can even display the wrong sign (Bound and Krueger, 1991).

A stricter (or quantitative) assessment, such as $\mathrm{E}(B_e + v_e|P + v_P) = E(B_{-e} + v_{-e}|P + v_P)$ would be misleading, because it requires a true model that relates behaviour within and outside the experiment (Falk and Heckman, 2009; Kessler and Vesterlund, 2015), and because coefficient estimates might suffer from attenuation bias due to $v_P$ and inflated standard errors from $v_e$ and $v_{-e}$. Under a qualitative interpretation of the test, predictors validity is rejected when opposite signs are found, or when a significant effect is found for behaviour only within or only outside the experiment.

Variables suitable for predictor validity tests should: (i) satisfy the error independence assumptions stated above, (ii) be theoretically relevant, and (iii) their impact should be testable across behaviour measures (e.g. the predicting variable is relevant for both behaviours). Consider the following examples, in which at least one of these three conditions is violated, and some caveats to interpret results. In the next sections we assess and discuss predictor validity for these variables.

In a first example, consider a potential violation of the first condition. Preferences and perceptions are relevant and comparable across behaviour measures, but the error independence assumption might not be entirely satisfied. Some of the error in preference measures might be correlated to the errors in the measures of behaviour. For instance, experimenter demand can affect stated experimental behaviour and social preferences measures in the same direction. If error in the predictor is correlated to measurement error in behaviour, the OLS zero conditional mean assumption is not satisfied and the estimated coefficients are biased (Bound and Krueger, 1991).[2]

Risk preferences and perceptions are more likely to satisfy the error independence assumption than social preferences, which might be more prone to experimenter demand effect, similar to stated behaviour. However, incentive compatible and stated measures of behaviour and preferences are unlikely to be affected by the same measurement errors. Measurement errors in the incentive compatible measures of preferences might relate to artificiality – the same measurement error affecting behaviour in the framed field experiment – but not to researcher demand effect in the stated measure of behaviour. Similarly, a researcher demand bias in stated preferences measures is expected to be unrelated to the artificiality bias in the incentive compatible measure of behaviour. When both stated and incentive compatible preference measures predict both incentive compatible and stated behaviour with the same sign we observe full predictor validity.

Demographic variables such as education, are comparable across behaviour measures and satisfy the error independence assumption, but are less relevant than preferences (violation of the second condition), because there is no clear *a priori* indication of what the direction of the effect should be. If the predictor is irrelevant, the predictor validity test is itself irrelevant.

Predictors such as past exposure to fire events within and outside the experiment are relevant and satisfy the error independence assumption, but are not directly comparable across behaviour measures (violation of the third condition): exposure to fire events in the experiment is not relevant for behaviour outside of it, and vice versa. Impacts of risk perception outside and inside the experiments are not directly comparable, because actual fire risk probabilities are different. Yet, it is comparable as a measure of attitude towards risk, in the sense of systematic probability over/under-weighting.

# 4. Methods

## 4.1. Sampling and study area

Data were collected in October-December 2015, during the fire season. We selected 40 villages in four municipalities in Parà, Brazil. The villages are all prone to forest fires, but vary otherwise in attributes, such as distance to urban areas, forest cover and main crop types. In collaboration with village leaders, we recruited 8 to 16 participants in each village. There were 576 participants in total. More information about the sample is provided in Cammelli and Angelsen (in this thesis). Experiment instructions and materials are provided in Appendix I.

## 4.2. Framed field experiment: design and implementation

### 4.2.1. Experiment design

We conducted a framed field experiment (Harrison and List, 2004), where the information and choice set are framed as agricultural technique decisions among neighbouring farmers. The payoff structure represents an eight-player repeated stag hunt game. In each round, participants choose

[2] In such extreme case one might attempt to devise a correction using an instrument that is independent from all errors $v_e, v_{-e}$ and $v_P$ (Hu and Schennach, 2008).

between three agricultural techniques: fire without control (*F*), fire with control measures (*CF*), or alternative techniques (*A*). Participants are better off coordinating on *A*, but the payoff depends on the number of others choosing *F*. *F* is the risk dominant strategy, but also imposes costs on the participants who chose *A*. For a risk neutral and selfish participant, *CF* is always strictly dominated.

Participants face an exogenous risk of fire occurrence (*fires from afar*), e.g., due to drought induced mega-fires originating from outside the community. This fire occurrence has a known probability for the participants: 0, 0.1, 0.3, 0.5 or 0.6.

Table 1 presents the payoff, depending on the choice made (columns) the number of other participants choosing *F* (rows), and if an exogenous fire occurred (right panel) or not (left panel). The payoffs are calibrated in collaboration with agronomists working in the region, with details provided in Cammelli and Angelsen (in this thesis). 80 points correspond to 1 BRL.[3]

*Table 1 Payoff tables without (left) and with (right) exogenous fire occurrence*

| #F | A | F | CF |
|---|---|---|---|
| 0 | 200 | 100 | 70 |
| 1 | 166 | 100 | 70 |
| 2 | 134 | 100 | 70 |
| 3 | 104 | 100 | 70 |
| 4 | 78 | 100 | 70 |
| 5 | 53 | 100 | 70 |
| 6 | 32 | 100 | 70 |
| 7 | 13 | 100 | 70 |

| #F | A | F | CF |
|---|---|---|---|
| 0 | 75 | 100 | 70 |
| 1 | 41 | 100 | 70 |
| 2 | 9 | 100 | 70 |
| 3 | 0 | 100 | 70 |
| 4 | 0 | 100 | 70 |
| 5 | 0 | 100 | 70 |
| 6 | 0 | 100 | 70 |
| 7 | 0 | 100 | 70 |

The expected payoff from choosing *A*, exceeds the sure payoff from *F*, if the number of other participants choosing *F* is sufficiently low. What tipping point has to be passed for *A* to be optimal is increasing with the level of exogenous fire risk. Table 2 reports tipping points for a selfish and risk neutral participant, under five different levels of exogenous risk.

*Table 2 Belief-tipping points below which A is optimal*

| **Exogenous fire risk** | *#F* |
|---|---|
| 0% | 4 |
| 10% | 3 |
| 30% | 2 |
| 50% | 2 |
| 60% | 1 |

[3] At the time of fieldwork, 4 BRL corresponded to approximately 1 EUR.

In addition to beliefs about the number of other participants choosing *F*, risk and social preferences influence individual evaluations of the outcomes. A stronger pro-social preference implies more weight on the outcomes of others and a higher belief-tipping point before switching from *A* to *F*, relative to Table 2. The belief-tipping point is similarly decreasing in risk aversion, i.e., the expected utility of choosing *A* needs to be larger to be preferred to a sure return from *F*. A risk averse and pro-social participant therefore faces contrasting effects on the belief-tipping point.

Table 3 summarizes the predicted impact of the preferences on the propensity to make each of the three choices, relative to a selfish and risk neutral participant. Stronger pro-social preferences increase the propensity to choose *A* and *CF* instead of *F*. Risk aversion increases the propensity to choose *CF* or *F* instead of *A*. We also predict that perceived risk influences choices as risk aversion does, because both reduce the expected utility of *A*.

*Table 3 Expected impact of preferences on choices*

| **Choice** | **Pro-social preferences** | **Risk aversion** |
|---|---|---|
| *A* | + | - |
| *CF* | + | + |
| *F* | - | + |

Preferences might change the number of tipping points in the experiment and lead to unanticipated equilibria. For example, for a strongly pro-social participant, *CF* may strictly dominate F. For, if as a consequence of pro-social preferences the utility of F is decreasing in the number of A players suffering damages, there are some beliefs for which CF dominates F and A. Then, two tipping points exist: between A and CF, and between CF and F. Alternatively, if risk aversion is large enough, A might never be optimal, and no tipping point exists.

4.2.2. Procedure

In addition to choosing agricultural technique, each participant reports his or her believed number of other participants choosing *F*. The game is repeated for five rounds.

In half of the experimental sessions, the exogenous fire risk probability is 0.3 in all five rounds, while it is increasing from 0 to 0.6 over the five rounds in the other sessions. At the end of each round, the experimenter rolls a dice to determine if an exogenous fire occurred or not. The occurrence of an exogenous fire, the frequency of each choice and the associated payoffs are notified to the group at the end of each round.

### 4.3. Preference measures

Before the framed field experiment, both stated and incentive compatible preferences are elicited. In incentive compatible games, participants face an incentivised trade-off. In the risk domain, the trade-off is between certain payoffs and higher expected (but risky) payoffs, while in the social domain the trade-off is often between own and others' payoffs. Common games to elicit preferences are gambles in the form of multiple price lists (Holt and Laury, 2002) for the risk domain, and the dictator game (Charness and Rabin, 2002; Forsythe et al., 1994) for the social domain.[4]

We also elicited stated preferences from the participants. In an extensive study, Dohmen et al. (2011) elicited responses to questions such as "How do you see yourself: are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?" They also asked similar questions in specific contexts, such as in health and financial matters. The responses, from 0 "not at all willing" to 10 "very willing", indicate the degree of risk aversion.

Two major criteria commonly used to evaluate methods are the consistency of the results (internal validity) and the extent the results are transferable to analogous settings and over time (external validity). Dohmen et al. (2011) and a related study by Falk et al. (2016a) find support for the former, as the stated preference measures strongly correlate with the incentive compatible measures aiming to elicit the same preferences within the same sample. Chuang and Schechter (2015) find that stated preference responses display higher stability over time than incentive compatible preference responses.

Dohmen et al. (2011) and Falk et al. (2016a) also find that the elicited stated preferences predict the respondents' stated decisions, with the context-neutral stated preferences providing the best overall explanatory power.

#### 4.3.1. Incentive compatible measures

The participants make choices in two incentive compatible (IC) games. First, in a variation of the dictator game (Kahneman et al., 1986), each participant was privately endowed with 100 tokens and asked how much (0-100) he or she would like to share with a randomly selected (and unknown) participant in the same session. The transfer was private, where neither the sender nor the receiver knew each other's identity. The measure intends to indicate the willingness to give up one's own payoffs for another's benefit. There is no apparent reason to give away anything, as it is an anonymous transfer with no material gain for the sender. Giving in the dictator game may be due to a mix of social preferences and norms (List, 2007), as we return to in section 5.1. Yet, the amount given is a better indication of evaluations of own vs. others payoffs compared with other common games, such as the ultimatum and trust games.

Second, participants made repeated choices between two gambles with fixed stakes, but varying probabilities (Holt and Laury, 2002). The gambles were presented as bags with varying shares of black and white balls, where picking a white ball is the favourable outcome (see Appendix II). The risky gamble returns 10 tokens for picking a black ball and 100 for a white ball, while the safer option returns 40 and 60 for black and white balls, respectively. To avoid order effects and anchoring, we randomized which gamble to be presented first, with subsequent presentation of gambles depending on the previous choice. The latter ensured a consistent outcome, i.e., only one

[4] A number of other games and preferences are also explored in the literature, such as trust, reciprocity, inequality aversion and fairness norms. We return to our choice for the dictator game in the next section.

switching point. Of ten gambles, one is randomly chosen, with subsequent earnings added to the total private earnings of the participant in the main experiment.

#### 4.3.2. Survey

Participants' stated risk and social preferences, their perceived fire risk and their stated agricultural fire use were also collected before they participated in the framed field experiment, together with other individual level characteristics.

For stated agricultural fire use, we construct a variable that is comparable to the choice set in the framed field experiment. Fire use (cf. *F*) corresponds to stated fire use in land preparation of annual crops or for pasture renewal, without control measures. Measuring adoption of fire control is more challenging, because it is prescribed by a social norm and by the law;[5] strong over-reporting has indeed been documented by Carmenta (2013). We thus applied a strict definition of controlled use of fire (cf. *CF*) as joint adoption of firebreaks, backfire and igniting only at the start of the rain season.[6] Lastly, fire-free agricultural techniques (*A*) correspond to stated adoption of alternative techniques. Appendix III presents and discusses the choice variable constructions in more detail.

In the same survey, participants responded to a set of statements and questions related to risk and social preferences. Based on Dohmen et al. (2011) and Falk et al. (2016b), we selected the items presented in Table 4. The responses to items I-1 to I-8 are to what degree they agree or disagree on an eleven-points Likert scale. We differentiate between domain specific and more general preferences. The social preference items are furthermore positively (1-2) or negatively (3-4) framed. The former reflects considerations for the relative payoffs of others, while the latter reflects considerations for relative private payoffs. We therefore expect responses to statements 1-2 and 3-4 to be negatively correlated. Lastly, to measure participants' stated fire risk perception, the participants responded to question 9 of Table 4 on a five-points Likert scale.

*Table 4 Statements and questions concerning social and risk preferences and risk perception*

| | |
|---|---|
| | **Social preferences** |
| I-1 | I am willing to make sacrifices for the good of those around me |
| I-2 | I participate in *mutirão*[7] in the community |
| I-3 | I am comfortable receiving benefits even if I don't contribute |
| I-4 | My personal happiness is more important than the well-being of the average farmer |
| | **Risk preferences** |
| I-5 | How do you see yourself: are you generally a person who is fully prepared to take risks or do you try to avoid taking risks? |
| I-6 | How do you see yourself: are you a person who is fully prepared to take risks in your agricultural practices or do you try to avoid taking risks in your agricultural practices? |
| I-7 | How do you see yourself: are you generally a person who is fully prepared to take risks related to health or do you try to avoid taking risks related to health? |
| I-8 | How do you see yourself: are you generally a person who is fully prepared to take financial risks or do you try to avoid taking financial risk? |
| | **Risk perception** |
| I-9 | Do you perceive accidental fires as a threat to your property? |

[5] Brazilian Forest Code (Chap. IX, law 12651/2012).
[6] Several other fire control measures can be adopted, but these three are the most fundamental ones.
[7] *Mutirão* is a Portuguese noun best translated as joint effort. It is widely used by Brazilian Amazon smallholders to describe villagers' collaboration for community work or to jointly help a member.

## 4.4. Statistical analysis

In a three-part analysis, we: (i) assess the internal validity of the preference measures through Bonferroni corrected pairwise correlations and aggregate them through principal component analysis; (ii) compare the experimental choices with the equivalent stated choices through Bonferroni corrected pairwise correlations; and (iii) estimate the impact of the perception and preference measures on both experimental and stated choices through a set of binary logit regressions.

To include stated preferences in the models of (iii), we first aggregate responses to the items of Table 4 through principal component analysis. Second, we estimate stated and experimental choices jointly, by appending the stated and the experiment datasets. This produces comparable coefficients. Three single logit models estimate the impact of the preferences on the three choices: *A*, *CF* and *F*.

To test robustness, the three models are estimated with four different specifications each. The basic specification includes risk perception ($p$), standardized risk ($r$) and social preferences ($s$), the dataset dummy ($d$), and $r$ and $s$ interacted with $d$. The second specification also includes control variables at the session level ($exp$). The third specification additionally includes control variables about exogenous fire occurrences in the session and at the farmer's property level ($prop$). In addition to these variables, the fourth specification includes control variables at the individual level. All coefficients are allowed to vary between the two datasets. All specifications include participant and session level random effects and standard errors are clustered at the session level to account for static and dynamic intra-session correlation (Fréchette, 2012). With $Y_{ite}$ being participant *i*'s choice in round *t* of experiment session *e*, our model is:

$$Y_{ite} = \beta_0 + \beta_1 p_i + \beta_2 r_i + \beta_3 s_i + \beta_4 d_i + \beta_5 p_i d_i + \beta_6 r_i s_i + \beta_7 r_i d_i + \beta_8 s_i d_i + \boldsymbol{\gamma exp_{ite} d_i} + \boldsymbol{\delta prop_{ite} d_i} + \boldsymbol{\theta ind_i d_i} + \tau_i + \mu_e + \varepsilon_{it}$$

The basic specification estimates all the $\beta$ parameters. The second specification also estimates $\boldsymbol{\gamma}$, the third specification also estimates $\boldsymbol{\delta}$, whereas the fourth specification estimates all parameters. As will be shown in the next section, the principal component analysis yield one stated risk preference component and two stated social preference components. Therefore, in addition to the three single logit models with four specifications each, we estimate three separate sets of equations for each of the preference measures: one for the incentive compatible measures and two for the stated preference measures. The total number of estimated equations is thus 36 (full results are reported in Appendix V).

The session level control variables are: a dummy for the increasing risk treatment, beliefs about other players choosing *F,* and an inverse round trend. Property level controls are the stated number of fires suffered in the last five years and the number of exogenous fires experienced by the participant in the experiment. Individual level features are age, gender and years of education, and age and gender composition of the participants group in the session (summary statistics and variables description are provided in Appendix V).

To assess the predictor validity, we test if the estimated preference and perception coefficients share the same sign and are significantly different from zero. Our assessment is thus qualitative, as we do not compare effect sizes directly. A quantitative assessment could produce misleading conclusions, as it requires a model that specifies how experimental and non-experimental behaviour relate (Falk and Heckman, 2009; Kessler and Vesterlund, 2015). A test of behavioural validity conducted on $\beta_4$ would also suffer from this shortcoming because the true model is unknown. Yet, we discuss it for sake of comparison with the other external validity measures.

# 5. Results

## 5.1. Internal validity of preference measures and items aggregation

Table 5-6 present correlation matrices for the social and risk preference measures, respectively. *IC-giving* is the amount sent in the dictator game (0-100). *IC-risk* is the switching point in the lottery (0-10), where a higher number indicates stronger risk aversion. The remaining are responses to the items presented in Table 4, with corresponding numbering.

In Table 5, we interpret responses to I-1 as the willingness to take private costs for the benefits of others (sacrifice), responses to I-2 as the willingness to contribute for the benefits of the community, responses to I-3 as the willingness to free-ride, and responses to I-4 as the degree of selfishness. In Table 6, we interpret responses to the four risk items as risk aversion in general (I-5) and in three specific domains: agriculture, health and finances (I-6, I-7 and I-8). We interpret responses to the ninth item as the extent of perceived fire risk exposure.

*Table 5 Pairwise correlations of the incentive compatible and stated social preference measures*

| | IC-giving | I-1 (sacrifice) | I-2 (contribute) | I-3 (free-ride) |
|---|---|---|---|---|
| I-1 (sacrifice) | 0.048<br>(1.000) | | | |
| I-2 (contribute) | 0.052<br>(1.000) | 0.334***<br>(0.000) | | |
| I-3 (free-ride) | -0.146***<br>(0.007) | -0.011<br>(1.000) | -0.014<br>(1.000) | |
| I-4 (selfishness) | -0.142***<br>(0.010) | -0.081<br>(0.599) | -0.104<br>(0.158) | 0.244***<br>(0.000) |

Bonferroni corrected *p*-values in parentheses. ***, **, *: significant at the 1, 5 or 10% level.

Table 5 indicates that the respondents' consideration for own well-being is unrelated to the considerations for the well-being of others. We expected *ex ante* the two to be negatively correlated; stronger private considerations imply weaker considerations for others. The results illustrate that giving in the dictator game is inversely related to private considerations but not to more considerations for others, indicating that the participants consider what they give up, rather than what they give to others. This supports the conclusions of Bolton et al. (1998: 295): that within the constraints of social norms, "dictators do behave in a self-interested manner." Our results thus suggest that one should be careful interpreting dictator game giving as an indicator of pure altruism.

*Table 6 Pairwise correlations of incentive compatible and stated risk preference measures*

| | IC-risk | I-5 (general) | I-6 (agriculture) | I-7 (health) | I-8 (finances) |
|---|---|---|---|---|---|
| I-5 (general) | 0.058<br>(1.000) | | | | |
| I-6 (agriculture) | 0.045<br>(1.000) | 0.495***<br>(0.000) | | | |
| I-7 (health) | -0.053<br>(1.000) | 0.341***<br>(0.000) | 0.523***<br>(0.000) | | |
| I-8 (finances) | -0.034<br>(1.000) | 0.337***<br>(0.000) | 0.340***<br>(0.000) | 0.410***<br>(0.000) | |
| I-9 (risk perception) | 0.009<br>(1.000) | 0.044<br>(1.000) | 0.002<br>(1.000) | -0.014<br>(1.000) | 0.0631<br>(1.000) |

Bonferroni corrected *p*-values in parentheses. ***, **, *: significant at the 1, 5 or 10% level.

For risk preferences (Table 6), correlations of the context-specific item responses (agriculture, health and finances) and the general item response is consistent with Dohmen et al. (2011). That these items are unrelated to the incentive compatible (IC) measure of risk preferences is, however, not consistent with their study. Reviewing the broader literature, Chuang and Schechter (2015) argue that the lack of negative results concerning the stability of preferences across methods might be due to a publication bias towards positive results. Risk perception is unrelated to risk aversion, indicating that respondents differentiate between their evaluation of risky options and the risk level.

To assess the impact of preferences on choices, we aggregate stated preference responses through principal component analysis (PCA). For risk preferences, the four items loaded positively on one component indicating risk aversion (RA) with eigenvalue 2.23, which accounts for 55.7% of the total variance. For social preferences, the four responses yield two components with eigenvalues 1.41 and 1.18, which account for 35% and 30% of total variance, respectively. We decided to keep them separate and operate with two social preference components. The former refers to responses of social preference items 1-2 (SP1), and the latter refers to the inverse of the responses of social preference items 3-4 (SP2). Appendix IV presents the PCA in detail.

### 5.2. Choices and behavioural validity

Table 7 indicates large discrepancies in the average choice frequencies between the experiment and the survey methods, suggesting weak behavioural validity.

*Table 7 Frequencies of stated and experimental choices*

| Choice | *A* | *CF* | *F* |
|---|---|---|---|
| Stated (S) | 23% | 24% | 53% |
| Experiment (FFE) | 40% | 41% | 18% |

Pairwise correlations between stated and experimental choices, on the other hand, indicate behavioural validity for some choices (Table 8). Choosing alternative techniques in the experiment (FFE-*A*) positively correlates with the equivalent stated choice (S-*A*) and negatively correlates with the stated uncontrolled use of fire (S-*F*). Similarly, experimental uncontrolled fire use (FFE-*F*) negatively correlates with stated choices of alternative techniques (S-*A*). FFE-*F* does not, however, correlate with its stated equivalent (S-*F*), neither does controlled fire use in the experiment (FFE-*CF*; S-*CF*). Both FFE-*F* and FFE-*CF* negatively correlate with S-*A*.

*Table 8 Pairwise correlations of experimental (FFE) and stated (S) choices*

| | FFE-*A* | FFE-*CF* | FFE-*F* |
|---|---|---|---|
| S-*A* | 0.1262*** | -0.1009*** | -0.0416** |
| | (0.000) | (0.000) | (0.0238) |
| S-*CF* | -0.0165 | -0.0052 | 0.0293 |
| | (1.000) | (1.000) | (0.3935) |
| S-*F* | -0.0949*** | 0.0913*** | 0.0113 |
| | (0.000) | (0.000) | (1.000) |

Bonferroni corrected *p*-values in parentheses. ***, **, *: significant at the 1, 5 or 10% level.

That stated controlled and uncontrolled fire uses (S-*CF;* S-*F*) do not correlate with the correspondent experimental choices could be due to the construction of the stated fire control choices. Whereas FFE-*CF* and FFE-*F* are measured directly, multiple variables construct the stated equivalents (Appendix III). Still, as discussed in the appendix, neither of the variables alone correlate with FFE-*CF*. Another potential explanation could be over-reporting of fire control in the stated responses. A strong social norm prescribes adoption of fire control measures and it is little cost for the respondent to over-report the use of such measures, particularly since definitions of control are at times ambiguous (Carmenta, 2013); for instance, how wide does a firebreak need to be? This over-reporting is costlier in the experiment than in the survey, which may explain the correlations between FFE-*CF* and S-*A* and S-*F*.

### 5.3. Preferences in coordination and predictor validity

To assess the impact of preferences on choices and to test predictor validity, we ran a set of binary logit regressions. Table 9 displays results for the full specification model (results for other specifications are reported in Appendix V). For social preferences, giving in the dictator game and SP2 (items 3-4) significantly predict stated choices *A* and *F*, both in line with predictions. None of the social preference measures robustly predict choices in the experiment.[8] Up to a violation of the error independence assumption, the test rejects predictor validity for social preferences.

For risk preferences, none of the measures significantly predict experimental choices, but IC-risk predicts fewer CF choices in the survey. Risk perception, on the other hand, predicts all choices except for FFE-*CF*, supporting predictor validity.

That risk aversion negatively predict *CF* is opposite to theory and might stem from the challenge of defining *CF* in the survey. Smallholder agricultural systems are complex and diversified (displaying a combination of *A* and *CF* land use types). Risk averse farmers might have diversified less, and applied less fire control because they have less assets at stake in case of a fire. Because the sign is opposite to theory and interpretation is unclear, we cannot conclude on *CF*s predictor validity. Also, *CF* is a choice that is not an equilibrium in the game; that risk aversion affects

[8] That SP1 predicts *CF* in the experiment seems incidental and due to multiple hypotheses testing, because it is not robust across model specifications and the p-value is low.[9] Speculating, more educated participants might be less affected by framing and take advantage of anonymity in the experiment to display less conformance with a fire control norm than they would outside the experiment. These might hint to some artificiality of the experimental results about fire control. Yet, any other conjecture is also possible.

coordination is unlikely, because more choices are performed on both *F* and *A*, but with no distinction between the two.

As expected, beliefs about other participants choosing more *F* predict less *A* choices and more *F* choices in the FFE. As discussed in Cammelli and Angelsen (in this thesis) and Cammelli (in this thesis), *CF* seems to be mostly motivated by a social norm, and to be unaffected by strategic motivations.

Turning to the other predictors, Table 9 shows that education increases *CF* and decreases *F* choices outside the experiment, but displays the opposite impact inside the experiment. We had no clear prior expectation for the sign of this variable.[9] A similar argument can be made for gender and age. These cases exemplify that theoretically irrelevant predictors produce irrelevant tests: lack of predictor validity for demographic variables does not justify less confidence in the experiment.

Experience with fire accidents reduces A and increases F choices in the FFE, but not in the survey. Because fire risk is measured with two different variables that are not comparable across methods (lag of cumulative drought occurrences in in the FFE vs neighbours and afar fire occurrences in the survey), we cannot reach a conclusion about the experiment validity.

This result, along with the result for IC-risk predicting *CF* in the survey, suggest some method-specific bias in stated fire control data, for instance over-reporting.

Lastly, the dataset specific dummy is systematically non-zero for *CF* choices, indicating no behavioural validity for this choice. The dummy is not significant for choices *A* and *F* in three and one out of the four specifications each (Appendix V). Yet, as briefly discussed in section 3, an external validity test based on a quantitative assessment is theoretically invalid. Moreover, the method specific intercept is possibly biased due to error in the counterfactual measure of behaviour (survey).

[9] Speculating, more educated participants might be less affected by framing and take advantage of anonymity in the experiment to display less conformance with a fire control norm than they would outside the experiment. These might hint to some artificiality of the experimental results about fire control. Yet, any other conjecture is also possible.

*Table 9 Estimates results for the full specifications (Logit models, log-odds ratios)*

| | SP1 & RA | | | SP2 & RA | | | IC | | |
|---|---|---|---|---|---|---|---|---|---|
| | A | F | CF | A | F | CF | A | F | CF |
| Survey dummy | 0.434 | 1.497 | -2.254* † | 0.478 | 1.546 | -2.265* † | 0.930 | 1.746 | -2.643** † |
| | (1.395) | (1.275) | (1.251) | (1.421) | (1.272) | (1.260) | (1.538) | (1.279) | (1.227) |
| IC-giving (FFE) | | | | | | | -0.0126 | -0.0611 | 0.0524 |
| | | | | | | | (0.0995) | (0.0799) | (0.0753) |
| IC-giving (Survey) | | | | | | | 0.584*** † | -0.187 | -0.112 |
| | | | | | | | (0.178) | (0.140) | (0.172) |
| IC-risk (FFE) | | | | | | | -0.0164 | -0.0213 | 0.0387 |
| | | | | | | | (0.0742) | (0.0883) | (0.0726) |
| IC-risk (Survey) | | | | | | | 0.0261 | 0.0918 | -0.197* † |
| | | | | | | | (0.169) | (0.127) | (0.115) |
| SP2 (FFE) | | | | -0.0311 | -0.0586 | 0.0739 | | | |
| | | | | (0.0952) | (0.0679) | (0.0942) | | | |
| SP2 (Survey) | | | | 0.420** † | -0.152 | -0.0831 | | | |
| | | | | (0.197) | (0.141) | (0.160) | | | |
| SP1 (FFE) | -0.163 | -0.0354 | 0.197* | | | | | | |
| | (0.117) | (0.0834) | (0.112) | | | | | | |
| SP1 (Survey) | 0.180 | -0.111 | -0.0315 | | | | | | |
| | (0.205) | (0.130) | (0.178) | | | | | | |
| RA (FFE) | -0.0502 | -0.110 | 0.117 | -0.0395 | -0.101 | 0.100 | | | |
| | (0.102) | (0.0898) | (0.0857) | (0.102) | (0.0904) | (0.0885) | | | |
| RA (Survey) | -0.194 | 0.109 | 0.0611 | -0.256 | 0.138 | 0.0760 | | | |
| | (0.206) | (0.139) | (0.158) | (0.196) | (0.145) | (0.165) | | | |
| Risk perception (FFE) | -0.153** † | 0.132** † | 0.0808 | -0.150** † | 0.132** † | 0.0772 | -0.166** † | 0.153** † | 0.0772 |
| | (0.0659) | (0.0635) | (0.0567) | (0.0656) | (0.0638) | (0.0573) | (0.0666) | (0.0644) | (0.0588) |
| Risk perception (Survey) | -0.769*** † | 0.195** † | 0.354*** † | -0.790*** † | 0.198** † | 0.357*** † | -0.817*** † | 0.167** † | 0.392*** † |
| | (0.110) | (0.0893) | (0.121) | (0.106) | (0.0887) | (0.120) | (0.107) | (0.0831) | (0.121) |
| Inverse round trend | -1.259*** † | -0.514* † | 1.476*** † | -1.258*** † | -0.513* † | 1.476*** † | -1.215*** † | -0.525** † | 1.424*** † |
| | (0.221) | (0.267) | (0.196) | (0.222) | (0.266) | (0.197) | (0.211) | (0.252) | (0.186) |
| Increasing risk | 0.174 | 0.0324 | -0.187 | 0.179 | 0.0332 | -0.191 | 0.162 | 0.00875 | -0.189 |
| | (0.267) | (0.291) | (0.240) | (0.266) | (0.290) | (0.239) | (0.286) | (0.276) | (0.242) |
| Beliefs | -0.106*** † | 0.170*** † | 0.0205 | -0.107*** † | 0.170*** † | 0.0211 | -0.114*** † | 0.176*** † | 0.0198 |
| | (0.0338) | (0.0376) | (0.0351) | (0.0340) | (0.0377) | (0.0351) | (0.0355) | (0.0373) | (0.0342) |
| Lag of cumulative drought occurrence | -0.518*** † | 0.227* † | 0.301*** † | -0.515*** † | 0.227* † | 0.301*** † | -0.544*** † | 0.214** † | 0.326*** † |
| | (0.0984) | (0.118) | (0.0984) | (0.100) | (0.118) | (0.1000) | (0.100) | (0.106) | (0.107) |
| Beliefs | -0.106*** † | 0.170*** † | 0.0205 | -0.107*** † | 0.170*** † | 0.0211 | -0.114*** † | 0.176*** † | 0.0198 |
| | (0.0338) | (0.0376) | (0.0351) | (0.0340) | (0.0377) | (0.0351) | (0.0355) | (0.0373) | (0.0342) |
| Neighbours fire occurrences (Survey) | 0.558 | -0.200 | -0.175 | 0.558 | -0.198 | -0.168 | 0.543 | -0.114 | -0.235 |
| | (0.376) | (0.245) | (0.228) | (0.398) | (0.253) | (0.224) | (0.434) | (0.234) | (0.236) |
| Afar fire occurrences (Survey) | 0.427 | -0.261 | 0.0546 | 0.416 | -0.241 | 0.0585 | 0.385 | -0.273 | 0.119 |
| | (0.271) | (0.196) | (0.220) | (0.269) | (0.196) | (0.221) | (0.267) | (0.183) | (0.228) |
| Age (FFE) | 0.0116 | -0.0205*** | 0.00471 | 0.0105 | -0.0207*** | 0.00622 | 0.00278 | -0.0162*** | 0.00927 |
| | (0.00804) | (0.00655) | (0.00745) | (0.00796) | (0.00669) | (0.00722) | (0.00800) | (0.00619) | (0.00698) |
| Age (Survey) | -0.0213 | 0.0141 | 0.00646 | -0.0203 | 0.0127 | 0.00553 | -0.0153 | 0.0110 | 0.000649 |
| | (0.0161) | (0.00982) | (0.0127) | (0.0168) | (0.0100) | (0.0126) | (0.0178) | (0.0108) | (0.0131) |
| Male (FFE) | 0.212 | 0.181 | -0.376* | 0.232 | 0.176 | -0.389* | 0.209 | 0.160 | -0.333 |
| | (0.242) | (0.191) | (0.213) | (0.244) | (0.194) | (0.215) | (0.240) | (0.185) | (0.213) |
| Male (Survey) | 0.264 | -0.762* | 0.751* | 0.273 | -0.773* | 0.750* | 0.102 | -0.582 | 0.650 |
| | (0.487) | (0.414) | (0.454) | (0.498) | (0.413) | (0.454) | (0.458) | (0.388) | (0.455) |
| Age group (FFE) | -0.00683 | -0.00590 | 0.0147 | -0.00700 | -0.00593 | 0.0130 | 0.00556 | -0.0127 | 0.0127 |
| | (0.0221) | (0.0232) | (0.0197) | (0.0225) | (0.0235) | (0.0199) | (0.0251) | (0.0222) | (0.0207) |
| Male group (FFE) | 0.00906 | -0.0107 | -0.00170 | 0.00871 | -0.0106 | -0.000970 | 0.00922 | -0.00612 | -0.00560 |
| | (0.0117) | (0.00967) | (0.0103) | (0.0117) | (0.00963) | (0.0102) | (0.0122) | (0.00951) | (0.0110) |
| Education (FFE) | 0.0759** | 0.0407 | -0.103*** | 0.0710** | 0.0420 | -0.0980*** | 0.0687** | 0.0543** | -0.100*** |
| | (0.0299) | (0.0270) | (0.0305) | (0.0303) | (0.0265) | (0.0303) | (0.0288) | (0.0253) | (0.0296) |
| Education (Survey) | 0.0376 | -0.113** | 0.0974** | 0.0265 | -0.113** | 0.0977** | 0.0336 | -0.134*** | 0.119*** |
| | (0.0582) | (0.0503) | (0.0486) | (0.0585) | (0.0498) | (0.0482) | (0.0606) | (0.0468) | (0.0456) |
| Constant | -0.209 | -1.279 | -1.670 | -0.146 | -1.280 | -1.700 | -0.337 | -1.576 | -1.533 |
| | (1.026) | (1.052) | (1.190) | (1.035) | (1.044) | (1.191) | (1.160) | (1.043) | (1.181) |
| Observations | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 3,288 | 3,288 | 3,288 |
| Number of groups | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 |
| F-test | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Log pseudo-likelihood | -1605 | -1387 | -1695 | -1604 | -1386 | -1698 | -1730 | -1504 | -1862 |

Session clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; † indicates that the coefficient sign is robust across specifications (when included in at least two specifications). The full result table is reported in Appendix V. Missing observations occur in preference measures.

## 6. Discussion

### 6.1. Preferences, perceptions and beliefs predicting coordination

Coordination situations characterized by strategic complementarities exhibit tipping points above which reaching the optimal solution is in everyone interest. Beliefs (eg. Shafran, 2008) and social norms (Nyborg et al., 2016) can be manipulated to overcome tipping points or to form them. We argue that, when possible, manipulating coordination benefits through preferences and perceptions is also an option. Preferences are also likely to affect the number of tipping points.

Previous studies on risk aversion as a choice determinant in coordination situations produce mixed conclusions. In our study, we find a pattern of negative results: risk aversion does not affect coordination, and only affects the strictly dominated choice outside the experiment. That risk aversion does not hamper coordination is consistent with Buyukboyaci (2014) and Al-Ubaydli et al. (2013), but not with Al-Ubaydli (2011). It is especially surprising that risk aversion does not affect technology adoption, in contrast with Liu et al. (2013) and Holden and Quiggin (2017). We find instead that, similarly to Petrolia (2016); Petrolia et al. (2013), risk perception is a strong predictor of choices, making the case for considering subjective probabilities (Kahneman and Tversky, 1979) and not only risk preferences. The more participants believe fire risk to be a threat, the more likely they are to apply (controlled or uncontrolled) agricultural fire themselves.

Previously unexplored, we find that strong social preferences improve coordination in stated choices. Giving in the dictator game and lower relative consideration for one's own payoffs intuitively predict stated choices *A*, but none of the experimental choices. That risk perception consistently and negatively predicts the *A* and *F* choices, and risk aversion consistently does not, is important to understand coordination among neighbouring farmers in the Amazon, and for policies to reach the efficient equilibrium. Policies that aim to mitigate fire risk by pushing farmers to pass the tipping point towards a no fire equilibrium should consider that more than one tipping point may exist (between A and CF and between CF and F) and that controlled fire use can be an equilibrium. The impact of risk perceptions and social preferences and the high frequency of *CF* choices support this statement.

Changing farmers' beliefs about neighbouring fire use might be challenging after a long sequence of bad precedents (Fehr, 2011; Van Huyck et al., 1997). Cammelli and Angelsen (in this thesis) find that of two policy mechanisms – payments for environmental services and command and control – only the latter successfully affects beliefs in the experiment, and only jointly with increasing exogenous fire risk. Climate alerts about drought induced fire risk might furthermore hamper coordination for fire control, as increased fire risk negatively affects coordination. Our findings further strengthen this result: climate alerts are likely to increase risk perception, which we find to hamper coordination. Community-based policies could support pro-social preferences, and reduce risk perceptions at the landscape level, mitigating the occurrence of accidental fires.

Subsidising fire control among the farmers who cannot invest in alternative techniques might provide sufficient assurance for those farmers who are able to invest in alternative techniques and are mostly deterred to do so by high perceived fire risk. Similarly, targeting a coalition of farmers who are close to the tipping point can provide sufficient incentives for the others to follow, possibly driven by pro-social preferences and lower risk perceptions.

### 6.2. Predictor external validity

External validity tests of economic experiments are often confined to behavioural validity, measured as the correlation between experimental choices and related non-experimental behaviour (Benz and Meier, 2008; Handberg and Angelsen, 2015; Lusk et al., 2006; Potters and Stoop, 2016; Stoop et al., 2012; Voors et al., 2012). Performing behavioural validity tests without an unbiased counterfactual means that we do not know if external validity (or the lack thereof) is due to limitations in the counterfactual method or if it is a true result. Identifying common predictors of behaviour within and outside the experiment decreases the likelihood that behavioural validity is conflated with spurious correlation or disguised by measurement errors in the non-experimental counterfactual. Understanding the predictors may also ease generalization to settings where the predictor is more relevant (Ludwig et al., 2011) as well as indicating the origin of bias in an experiment. We established three requirements for the predictor variables testing predictor external validity: (i) independence from the error in the methods to measure behaviour, (ii) theoretical relevance, and (iii) comparability across behaviour measures. Identifying predictors that satisfy all three requirements is challenging, and we are not able to identify variables that fully satisfy all the three assumptions together.

We tested the experiment's external validity by assessing behavioural validity as well as commonality of predictors for both experimental and stated choices: predictor validity. In addition to behavioural validity support for choices *A* through conditional and unconditional correlations, we find that: (i) risk perception predicts both experimental and stated choices, (ii) risk aversion predicts stated but not experimental choices and at odds with theory, (iii) social preferences predict stated, but not experimental choices, and (iv) education and gender have a significant but inverse impact on *CF* choices. Predictor validity appears to be stronger for result (i) than result (iii), while (ii) is challenging to interpret because at odds with theory; (iv) would reject predictors validity, but does not satisfy the second assumption underlying the test, namely theoretical relevance.

We provide examples of acceptance of the test (risk perception has the same predictive ability in and out of the experiment and in accordance with theory, there is no suspect of error dependence, yet the variable is not fully comparable across behaviour measures) and rejection (social preferences only predict stated behaviour, but the error independence assumption might be violated causing spurious correlation; education and gender predict choices with opposite signs, but they are not relevant predictors).

We conclude that, with some reserves concerning pro-social preferences, the experiment is externally valid for choice *A*, the uptake of alternative techniques. Regarding the decision to adopt fire control or not, the behavioural validity test might suffer from severe bias in the counterfactual (i.e. over-reported stated CF choices). In the absence of predictors fully satisfying all three requirements, the tests for predictor validity cannot be entirely conclusive.

We show that analysing several predictors that individually violate some of the three assumptions, but jointly respect them, is informative, although not easy to interpret. Joining information on behavioural and predictor validity is a more ambitious external validity assessment than merely exploring choices data. Providing richer, more conservative and stronger evidence of external validity is essential for making lessons from framed field experiments relevant to policy-makers.

## 7. Conclusions

Coordination problems are ubiquitous in society, with policies having the potential to tip behaviour towards the efficient equilibrium. To understand the role of preferences and perceptions in coordination we gather unique experimental and survey data about a compelling coordination problem: fire risk mitigation in the Brazilian Amazon.

Identifying and assessing determinants of farmers' fire use and control decisions is essential to mitigate fire risk in the region. These decisions can be analysed as a coordination problem among neighbouring farmers. Our approach compares relevant experimental and stated measures of behaviour, and combines multiple preference measures collected within the same sample. This ensures a stronger understanding and higher reliability of our assessed predictors.

We assess the role of risk and social preferences, risk perception and beliefs on coordination. We find that risk perceptions but not risk preferences affect coordination in stated and experimental behaviour. Pro-social preferences predict stated but not experimental behaviour.

We discuss the external validity of the experiment and, besides behavioural validity, we propose a novel approach based on commonality of predictors: predictor validity. This approach provides richer information and establishes a more conservative test of external validity. Our experiment produces examples of acceptance and rejection based on whether commonality of predictors is found or not. Yet all predictors used in the analysis fail to fully meet at least one of the assumptions of the test.

Ideally, a test of external validity requires an unbiased counterfactual and measures of behaviour predictors that are theoretically relevant, comparable across behaviour measures and independent from the measurement error of the elicited behaviour. We highlight three challenges in assessing external validity. First, it is challenging to collect the required data. Second, even with an unbiased counterfactual, support for behavioural validity may rather be spurious correlation. Third, without predictors satisfying the three above-mentioned conditions, there is no clear interpretation of the test discerning experimental external validity from method-specific limitations. Yet, when commonality of predictors is found, external validity is established with knowledge of its causes.

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# 9. Appendixes

## Appendix I: Experiment material

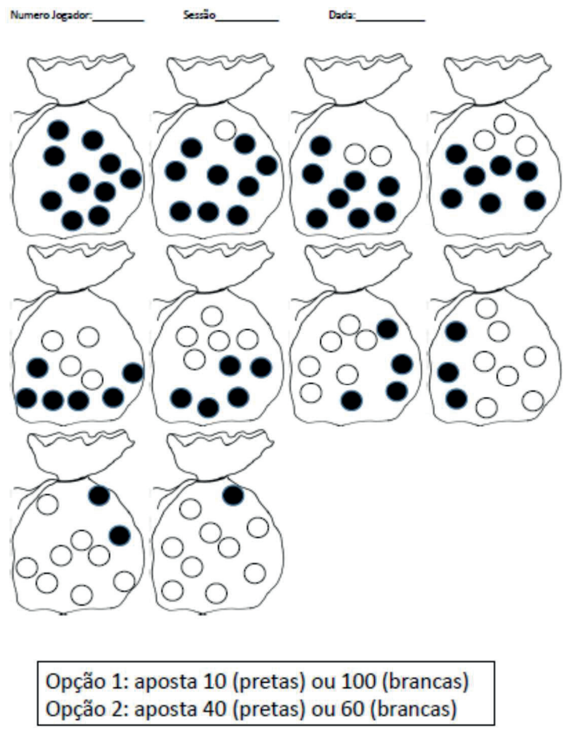
**Instructions**

Please refer to the appendix to all papers

## Appendix II: Preferences elicitation material and questionnaire

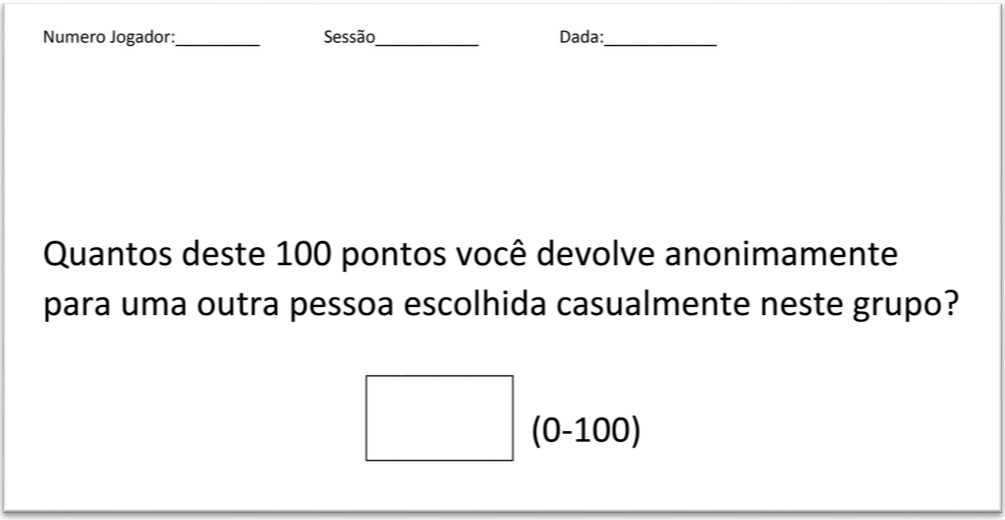
The lottery choice task (Figure ) consists of 10 choices over ten lotteries with constant prices and varying probabilities. For each bag displaying a probability, each participant had to choose between two options. Option 1 refers to a prospect with a gain of 10 points if a black ball is picked and of 100 if a white ball is picked. Option 2 refers to a prospect with a gain of 40 points if a black ball is picked and of 60 if a white ball is picked.

*Figure 1 Lottery choice task for risk preferences elicitation*



In the dictator game task (Figure 2), each participant was asked: “How many of these 100 points are you willing to give, anonymously, to another person randomly chosen from your group?” The participant was asked to write the number and turn the paper sheet before delivering it to the enumerator.

*Figure 2 Dictator game task for social preferences elicitation*



Numero Jogador:_________ Sessão___________ Dada:____________

Quantos deste 100 pontos você devolve anonimamente para uma outra pessoa escolhida casualmente neste grupo?

(0-100)

*Table 10 Questionnaire (Portuguese version)*

| N | Perguntas | Resposta |
|---|---|---|
| | **Personal Information** | |
| 1 | Idade | |
| 2 | Sexo (M/F) | |
| 3 | Você faz parte de alguma associação, cooperativa ou sindicato? (1/0) Se sim, qual? Você tem um papel de liderança? | QU:____________________<br>____ LIDE:___________________ |
| 4 | Anos de scolaridade | |
| 5 | De aonde você vem? (N, NE, S) | |
| 6 | Hà quantos anos que você mora nesta comunidade? | |
| | **Familia** | |
| 7 | Quantas pessoas moram com você na sua casa? Quantas >=18, masculina e feminina, <18 masculina e feminina? Hà algumas >=18 que estão sem possibilidade de trablahar? | MO,______; M>=18_____; F>=18_____; M<18_____; F<18_____; SEM______ |
| 8 | Você tem titulo de propriedade? Desde quando? Car? | TIT:_______;<br>QUA:_______; |
| 9 | Quantos na sua familia participam de uma associação, cooperativa ou sindicato? Ou da Igreja? | ASS:______; COOP:_______;<br>SIND:________; IGR_______ |
| 10 | Quanto è longe da sua casa para chegar no seu lote? | |
| 11 | Você acha que a associção esta trabalhando bem?<br>(1 muito mal, 2 mal, 3 Indifferente, 4 bem, 5 muito bem) | |
| 12 | Como è a relação com seus vizinhos?<br>(1 muito ruin, 2 ruin, 3 Indifferente, 4 boa, 5 muito boa) | |
| 13 | Como você considera a ultima safra?<br>(1 pior que a media, 2 na media, 3 melhor da media) | |
| | **Renda e ativos** | |

| | | |
|---|---|---|
| 14 | Você possui algum trator, moto, carro, casa de farinha ou curral? | TR:_______; MOT:_______;<br>CAR:______ CAS:________;<br>CU_______; |
| 15 | Você recebe alguma ajuda do governo: (1 Bps, 2 Bolsa Familia, 3 Aposentadoria ou outra pensao) | |
| | **Produção** | |
| 16 | Tamanho da propriedade (em ha)? O que hà e o que você cultiva na sua propriedade? (Mandioca, Milho, Feijão, acai, pimenta, dendé, outra perenne, hortaliças , pasto limpo, pasto degradado, floresta segundaria, floresta primaria, outro: detalhe) | TAM:_____; Man:_____; Mil:______;<br>Fe:______; A:_____; P:_____;<br>De:______;OP:_____; Hor:_____;<br>Pal:_____; Pad:_____; Flose:_____;<br>Flopri:______; outro:__________ |
| 17 | Que tipo de solos você tem na sua propriedade? | |
| 18 | Como você cultiva a sua roça? (1 corte e queima, 2 corte e tritura, 3 trator e fertilizante, 9: outro: detalhe) | |
| 19 | Como você maneja o seu pasto? (1 fogo, 2 rotação, 9 outro: detalhe) | |
| 20 | Você percebe os fogos accidentais como uma ameaça pela sua propriedade? (1 não, 2 mais não do que sim, 3 indifferente, 4 mais sim do que não, 5 sim) | |
| 21 | Você acha que os incendio acidentais ameaçam a sua propriedade mais do que os outros?<br>(1 não, 2 mais não do que sim, 3 indifferente, 4 mais sim do que não, 5 sim) | |
| | **Controle de fogo** (marca a confibilidade 1=lixo, 3 alta) | |
| 22 | Bastante pessoas usam fogo na agricultura. Quando foi o ultimo ano que voçê usous fogo no campo? (nas perguntas seguintes refere-se a aquele ano). | |
| 23 | Você usa alguma medida de controle de fogo especifica? (1/0) Se sim, o que? (1 aceiro, 2 aceiro com trator, 3 aceiro nas estacas, 4 conrafogo, 5 avisar os vizinhos, 6 esperar as primeira chuvas, 7 queima nas horas mais fresca com menos vento, 8 | _________<br>W:________ |

<table>
<tr><td></td><td>oriente as arvores em direção do centro da area quaimada, 9 Leva agua no lugar queimado, 10 queima junto com vizinhos)</td><td></td></tr>
<tr><td>24</td><td>A ultima vez que você queimou tinha licencia (IBAMA)? (1/0)</td><td></td></tr>
<tr><td>25</td><td>A area que você queimou estava cercada de floresta? (1/0)</td><td></td></tr>
<tr><td>26</td><td>A area que você queimou estava na beira de um igarapé ou de um rio? (1/0)</td><td></td></tr>
<tr><td>27</td><td>A area que você queimou estava no centro da sua propriedade (1), ou nas bordas (2)?</td><td></td></tr>
<tr><td>28</td><td>Você acha que as medidas de controle de fogo sugerida pelas autoridade são efficaz para prevenir os incendios accidentais na sua propriedade? (1 não, 2 mais não do que sim, 3 indifferente, 4 mais sim do que não, 5 sim)</td><td></td></tr>
<tr><td></td><td>Danos de fogo</td><td></td></tr>
<tr><td>29</td><td>Nos ultimos 5 anos você jà sofreu prejuizos devido a incendio acidentais? (1/0) Se sim, qual ano? O que foi prejudicado? (1 estacas ou arame 2 pasto, 3 roça, 4 perennes (inclusive açai ou dendé), 5 infraestruturas (e.g., casa de farinha ou moradia), 9 outro: detalhe)<br><br>Pode quantificar os danos em R$?<br><br>Este fogo escapo do seu lote (1), do lote do vizinho (2) era um fogo de longe (3)?<br><br>Conseguiu individuar o culpado (1)? Se sim, jà pediu uma compenseção (2)? Se sim, a consegiu (3)?</td><td><table>
<tr><td>Ano</td><td>11</td><td>12</td><td>13</td><td>14</td><td>15</td></tr>
<tr><td>Perda</td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td>Valor</td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td>Orig.</td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td>Culp.</td><td></td><td></td><td></td><td></td><td></td></tr>
</table></td></tr>
<tr><td>30</td><td>Você acha que os incendios na area estão diminuindo (1), estaveis (2) ou crescendo (3)?</td><td></td></tr>
<tr><td>31</td><td>Você acha que controlar o fogo de queimada seja um dever dos agricultores? (1 não, 2 mais não do que</td><td></td></tr>
</table>

| | | |
|---|---|---|
| | sim, 3 indifferente, 4 mais sim do que não, 5 sim) | |
| | **Experimento** | |
| 32 | Neste jogo você participou com algum amigo, familiar, vizinho? (1/0) Se sim, quantos? | |
| 33 | Você acha que entendeu bem o jogo? (1 não, 2 mais não do que sim, 3 indifferente, 4 mais sim do que não, 5 sim)<br>A partir de qual rodada? | ______________<br>RO:___________ |
| 34 | Durante o jogo você pensou em influenciar as escolhas dos outros com as suas? (1 não, 2 mais não do que sim, 3 indifferente, 4 mais sim do que não, 5 sim) | |
| | **Politicas municipais** | |
| 35 | Você conseguiria cumprir com uma regra que proibe o uso do fogo na agricultura? (1/0) | |
| 36 | Você aceitaria um pagamento para parar de utilisar fogo se o valor compensasse uma parte importante dos custos adicionais? (1/0) | |
| | **Preferências sociais** | |
| 1 | Estou disposto em fazer sacrificios pelo bens dos outros (0-10) | |
| 2 | Estou disposto em participar de mutirões na comunidade (0-10) | |
| 3 | Estou confortavel em receber beneficios das ações dos outros mesmo sem participar destas ações (0-10) | |
| 4 | A minha felicidade è mais importante do que o bem estar da pessoa tipica desta comunidade (0-10). | |
| | **Preferências ao risco** | |
| 5 | Você em geral é uma pessoa que está confortavel em assumir riscos ou você tenta evitar de correr riscos? (0-10) | |
| 6 | Você é uma pessoa que está confortavel em assumir riscos na suas praticas agricolas ou você tenta evitar | |

| | | |
|---|---|---|
| | de correr riscos na suas praticas agricolas? (0-10) | |
| 7 | Você é uma pessoa que está confortavel em assumir riscos com relação a saude ou você tenta evitar de correr riscos com relação a saude? (0-10) | |
| 8 | Você é uma pessoa que está confortavel em assumir riscos financeiros ou você tenta evitar de correr riscos financeiros? (0-10) | |

*Table 11 Questionnaire (English version)*

| N | Question |
|---|---|
| | **Personal Information** |
| 1 | Age |
| 2 | Gender (M/F) |
| 3 | Are you part of any association, cooperative or union? (1/0) If yes, which one? Do you cover any leadership role? |
| 4 | Years of education |
| 5 | From what region of Brasil do you come from? (N, NE, S) |
| 6 | For how long have you been living in this community? |
| | **Household** |
| 7 | How many people live in your house? How many >=18, males and females; <18 males and females? Is there anyone >=18 that is unable to work? |
| 8 | Do you own a tenure title? Since when? And the CAR? |
| 9 | How many in your family participate in an association, cooperative or union? |
| 10 | How far is your house from your field? |
| 11 | Do you think that the local association is working well? (1 very bad, 2 bad, 3 indifferent, 4 well, 5 very well) |

| | |
|---|---|
| 12 | How is the relationship with your neighbors?<br>(1 very bad, 2 bad, 3 indifferent, 4 well, 5 very well) |
| 13 | How do you rate the last harvest? (1 worst than average 2 average, 3 better than average) |
| | **Income and assets** |
| 14 | Do you own any tractor, mottorcycle, car, flower house or corral? |
| 15 | Do you receive any gouvernment help? (1 Bps, 2 Bolsa Familia, 3 retirement pension or other pension) |
| | **Production** |
| 16 | Property size (ha)? What do you cultivate in your property?(Mandioca, corn, beans, acai, pepper, oil palm, other perennial, vegetables, clear pasture, degraded pasture, primary forest, secondary forest/fallow, other) |
| 17 | What soils do you have in your property? |
| 18 | How do you prepare soil on your annual plots? (1 slash and burn, 2 slash and mulch, 3 tractor and fertilizer, 9 other: detail) |
| 19 | How do you manage your pasture (1 fire, 2 rotation, 9 other: detail) |
| 20 | Do you perceive accidental fires as a risk for your property? (1 No, 2 More no than yes 3 Indifferent, 4 More yes than no, 5 Yes) |
| 21 | Do you think that accidental fires threatens your property more than the others' one? (1 No, 2 More no than yes 3 Indifferent, 4 More yes than no, 5 Yes) |
| | **Fire control** (mark perceived reliability 1=trash, 3= high) |
| 22 | Many farmers use fire in agriculture. When was the last year that you use fire in your property? (In the next questions refer to that year) |
| 23 | Did you use any specific fire control measure? (1/0) If yes, what? (1 manual firebreak, 2 firebreak with a tractor, 3 firebreak around pasture fences, 4 backfire, 5 alert neighbors, 6 wait for the first rains, 7 burn in the coolest hours, with less wind, 8 orient fallen trees toward the center of teh burnt area, 9 carry water on the burnt area, 10 burn together with the neighbors) |
| 24 | Last time you used fire did you have a fire license? |
| 25 | The area that you burned was surrounded by forest? |
| 26 | The area that you burnt was on the shore of an *igarapè* or of a river? |
| 27 | The area that you burned was at the center or at the border of your property? |

| | |
|---|---|
| 28 | Do you think that the fire control measure suggested by the authorities are effective to prevent accidental fires in your property? (1 No, 2 More no than yes 3 Indifferent, 4 More yes than no, 5 Yes) |
| | **Damages from accidental fires** |
| 29 | Did you suffer any damage due to accidental fires in the last 5 years? If yes on what year? What suffered the damage? (1 pasture fences, 2 pastures, 3 annual crops, 4 perennial crops, 5 infrastructures, 9 others: detail )<br><br>Can you value the damage in R$?<br><br>The fire spead from your lot, the neighbor lot or *came from afar*?<br><br>Did you manage to individuate the culprit? If yes, did you asked for a compensation? If yes, did you obtain it? |
| 30 | Do you think that accidental fires in the area are diminishing, stable or increasing? |
| 31 | Do you think that controlling agricultural fires is a farmer duty?<br>Você acha que controlar o fogo de queimada seja um dever dos agricultores? (1 No, 2 More no than yes 3 Indifferent, 4 More yes than no, 5 Yes) |
| | **Experiment** |
| 32 | Did any of your friend, relative or neighbor participated in your experimental session? If yes, how many? |
| 33 | Do you think that you understood well the experiment rules? (1 No, 2 More no than yes 3 Indifferent, 4 More yes than no, 5 Yes)<br>From what round? |
| 34 | During the game did you think to affect the others' choice with yours? (1 No, 2 More no than yes 3 Indifferent, 4 More yes than no, 5 Yes) |
| | **Municipal policies** |
| 35 | Would you manage to comply with a norm prohibiting fire use in agriculture? |
| 36 | Would you accept a compensation to stop using fire it its value would compensate an important part of the additional costs of producing with another technique? |

## Appendix III: Constructing the choice variables

Through the survey we gather detailed data about the agricultural technologies used by each farmer. We define A as not using fire neither for clearance for annual crops nor for pasture maintenance.

Among those using fire we classify as CF those controlling fire, and F as all the others. The definition of CF is especially challenging because farmers adopt a variety of fire control measures depending on their perception of what is contingently more appropriate (Table 12). Yet, some fire control measures are more fundamental than others and also more burdensome in terms of labour requirement and because they are more likely to leave unburnt debris on the ground, which is a cost in terms of lower fertilization and land available for plantation. We consider as controlled fire only the joint adoption of firebreaks, backfire and waiting for the first rain before burning. Although these fire control measures might still be insufficient to effectively prevent a fire spread, they proxy the effort used in fire control.

We do not conduct explicit robustness test by comparing different definitions of the stated CF choice as this would largely escalate the number of regressions. We limit our analysis to the cross-correlation between CF choices in the experiment and the stated measures of adoption of fire control. We find that there is no significant correlation among the two (Table 13). We conclude that no other definition of stated CF would influence our conclusions about the external validity of the experiment or of preferences.

*Table 12 Fire control measures*

| | N | mean | SD |
|---|---|---|---|
| Firebreak | 435 | 0.897 | 0.305 |
| Backfire | 413 | 0.533 | 0.500 |
| Alert neighbors | 413 | 0.826 | 0.380 |
| Wait for the first rain | 412 | 0.534 | 0.499 |
| Abate all standing trees | 413 | 0.012 | 0.109 |
| Carry water on site | 413 | 0.552 | 0.498 |
| Have fire license | 430 | 0.077 | 0.266 |

*Table 13 Cross-correlation of CF choices in the experiment and stated fire control measures*

| | FFE-CF | Firebreak | Backfire | Alert neighbors | Wait for the first rain | Abate all standing trees | Carry water on site |
|---|---|---|---|---|---|---|---|
| Firebreak | 0.028 | | | | | | |
| | (1.000) | | | | | | |
| Backfire | -0.054 | 0.032 | | | | | |
| | (1.000) | (1.000) | | | | | |
| Alert neighbors | -0.080 | 0.179*** | 0.090 | | | | |
| | (1.000) | (0.001) | (1.000) | | | | |
| Wait for the first rain | -0.089 | -0.038 | 0.212*** | 0.076 | | | |
| | (1.000) | (1.000) | (0.000) | (1.000) | | | |
| Chop all standing trees | 0.027 | -0.044 | 0.073 | 0.009 | -0.027 | | |
| | (1.000) | (1.000) | (1.000) | (1.000) | (1.000) | | |
| Carry water on site | -0.076 | 0.119 | -0.042 | 0.219*** | -0.047 | -0.063 | |
| | (1.000) | (0.182) | (1.000) | (0.000) | (1.000) | (1.000) | |
| Have fire license | -0.051 | 0.022 | -0.060 | 0.060 | 0.102 | 0.018 | 0.042 |
| | (1.000) | (1.000) | (1.000) | (1.000) | (0.589) | (1.000) | (1.000) |

Bonferroni corrected p-values in parentheses. ***, **, *: significant at the 1, 5 or 10% level.

## Appendix IV: Constructing the stated preference variables

The stated measures of preferences collected outside of the FFE reveal a substantially pro-social population with more heterogeneity in the negatively framed statements. As shown in Figure 3, 33.5% of the participants gave in the dictator game, donating on average 25 of the 100 points they were endowed with. Stated risk preferences measures reveal a mainly risk averse population, with a distribution that varies slightly across domains. Responses to the lottery choice task shows some slight risk aversion, with 88,3% of the switching occurring in the 2 lotteries around risk neutrality, but contains very little variation. The stated measure of risk perception displays a large variance, with two modes at the opposite extremes (Figure 4).

*Figure 3 Social preferences measure*

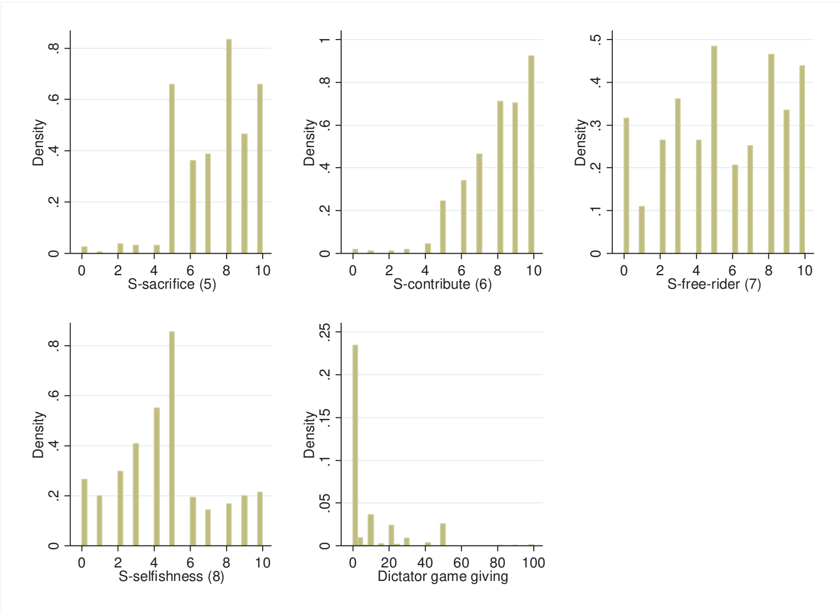
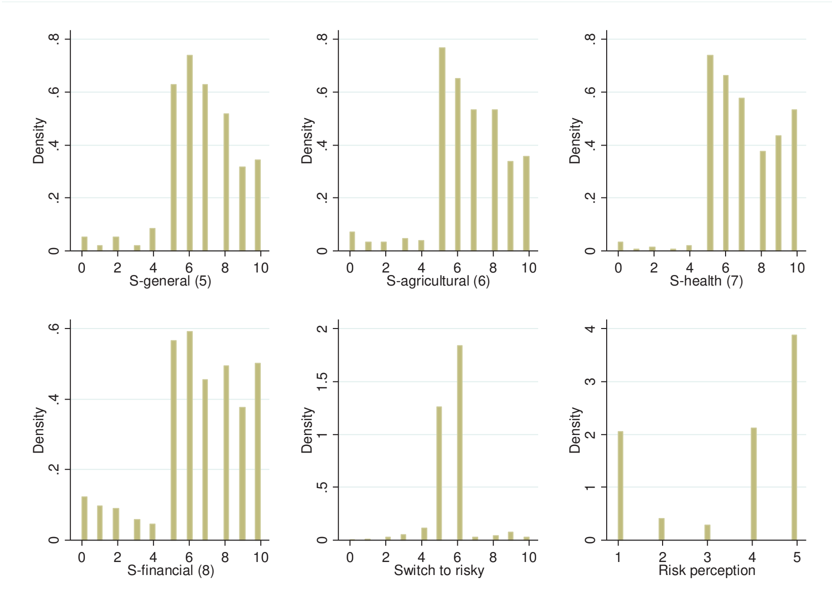


*Figure 4 Risk preferences measures*




The results of the PCA on social preferences shows that two factors emerge (Table 14-16). After rotation of the first two components it becomes apparent that the first two and the second two statements cluster together (Table 16).

*Table 14 Social preferences principal components*

| Component | Eigenvalue | Variance proportion | Cumulative variance |
|---|---|---|---|
| S-sacrifice (1) | 1.409 | 0.352 | 0.352 |
| S-contribute (2) | 1.181 | 0.295 | 0.647 |
| S-free-rider (3) | 0.746 | 0.187 | 0.834 |
| S-selfishness (4) | 0.665 | 0.166 | 1.000 |

*Table 15 Items loading on the first two components*

| Variable | Component 1 | Component 2 | Unexplained variance |
|---|---|---|---|
| S-sacrifice (1) | 0.588 | 0.381 | 0.341 |
| S-contribute (2) | 0.603 | 0.354 | 0.340 |
| S-free-rider (3) | -0.299 | 0.670 | 0.344 |
| S-selfishness (4) | -0.448 | 0.529 | 0.386 |

*Table 16 Item loading on the two components after varimax rotation*

| Variable | Component 1 | Component 2 | Unexplained variance |
|---|---|---|---|
| S-sacrifice (1) | 0.701 | 0.019 | 0.341 |
| S-contribute (2) | 0.699 | -0.012 | 0.340 |
| S-free-rider (3) | 0.094 | 0.728 | 0.344 |
| S-selfishness (4) | -0.107 | 0.686 | 0.386 |

PCA on risk preference items reveal one component only (Table 17). No further rotation is performed.

*Table 17 Risk preferences principal component*

| Component | Eigenvalue | Variance proportion | Cumulative variance |
|---|---|---|---|
| S-general (5) | 2.228 | 0.557 | 0.557 |
| S-agricultural (6) | 0.718 | 0.180 | 0.736 |
| S-health (7) | 0.639 | 0.160 | 0.896 |
| S-financial (8) | 0.415 | 0.104 | 1.000 |

Loading of each item on the extracted component is shown in Table 18.

*Table 18 Items loading on the first component*

| Variable | Component 1 | Unexplained variance |
|---|---|---|
| S-general (5) | 0.484 | 0.477 |
| S-agricultural (6) | 0.542 | 0.347 |
| S-health (7) | 0.516 | 0.407 |
| S-financial (8) | 0.454 | 0.542 |

## Appendix VI: Estimates results

Table 19 describes the variables included in the regressions. Tables 20, 21 and 22 report full estimates results.

*Table 19 Variables descriptions and summary statistics*

| Variable name | Description | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|---|
| Stated sample | Dummy variable distinguishing the stated dataset (1) from the experimental one (0) | | | | | |
| SP1 | Standardized stated relative evaluation of the others benefits | 3240 | 0 | 1 | -4.887 | 1.693 |
| SP2 | Standardized stated relative evaluation of the own benefits | 3240 | 0 | 1 | -2.198 | 2.231 |
| RA | Standardized stated risk aversion | 3126 | 0 | 1 | -4.297 | 2.025 |
| IC-giving | Standardized giving in the dictator game | 3456 | 0 | 1 | -0.515 | 5.721 |
| IC-risk | Incentive compatible measure of risk aversion (standardized switching point between the safe and the more risky lottery) | 3450 | 0 | 1 | -4.082 | 5.195 |
| Risk perception | risk perceptions | 3294 | 3.610 | 1.621 | 1 | 5 |
| Increasing risk | Increasing risk treatment in the experiment | 3456 | 0.417 | 0.493 | 0 | 1 |
| Belief | beliefs about other farmers playing F in the experiment | 2880 | 2.590 | 2.072 | 0 | 7 |
| Inverse round trend | one over the round number in the experiment | 3456 | 0.381 | 0.315 | 0 | 1 |
| Cumulative drought occurrence | lag of the empirical cumulative function of droughts occurrences in the experiment | 3456 | 0.333 | 0.585 | 0 | 3 |
| Neighbours-fire occurrence | number of stated fire occurrences caused by neighbours in the last five years | 3456 | 0.043 | 0.267 | 0 | 5 |
| Afar-fire occurrence | number of stated fire occurrences originating outside of the neighbourhood in the last five years | 3456 | 0.054 | 0.291 | 0 | 4 |
| Age | Age | 3456 | 43.694 | 13.680 | 17 | 81 |
| Male | Male | 3456 | 0.745 | 0.436 | 0 | 1 |
| Age group | Average age of the participants in the experiment session | 2880 | 43.694 | 5.945 | 27 | 53.875 |
| Male group | Share of male participants in the experiment session | 2880 | 59.583 | 13.066 | 30 | 80 |
| Years education | Individual years of education | 3456 | 4.729 | 3.576 | 0 | 15 |

*Table 20 Regression results, stated preferences - negative framing social preferences measure (log odds ratio)*

| | SP2_a_1 | SP2_a_2 | SP2_a_3 | SP2_a_4 | SP2_cf_1 | SP2_cf_2 | SP2_cf_3 | SP2_cf_4 | SP2_f_1 | SP2_f_2 | SP2_f_3 | SP2_f_4 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Survey dummy | 0.922** | 0.427 | -0.243 | 0.478 | -2.333*** | -1.835*** | -1.501*** | -2.265* | 1.365*** | 1.459*** | 1.755*** | 1.546 |
| | (0.406) | (0.434) | (0.430) | (1.421) | (0.507) | (0.536) | (0.549) | (1.260) | (0.400) | (0.426) | (0.472) | (1.272) |
| SP2 (FFE) | -0.0160 | -0.0239 | -0.0164 | -0.0311 | 0.0343 | 0.0351 | 0.0314 | 0.0739 | -0.0336 | -0.0193 | -0.0220 | -0.0586 |
| | (0.0939) | (0.0936) | (0.0942) | (0.0952) | (0.0965) | (0.0989) | (0.0986) | (0.0942) | (0.0713) | (0.0712) | (0.0718) | (0.0679) |
| SP2 (Survey) | 0.497*** | 0.501*** | 0.455** | 0.420** | -0.0918 | -0.0941 | -0.0829 | -0.0831 | -0.200 | -0.216 | -0.198 | -0.152 |
| | (0.191) | (0.193) | (0.195) | (0.197) | (0.157) | (0.160) | (0.157) | (0.160) | (0.146) | (0.143) | (0.145) | (0.141) |
| RA (FFE) | -0.0401 | -0.0432 | -0.0416 | -0.0395 | 0.0844 | 0.0866 | 0.0867 | 0.100 | -0.0722 | -0.0717 | -0.0726 | -0.101 |
| | (0.0986) | (0.0970) | (0.0978) | (0.102) | (0.0863) | (0.0877) | (0.0872) | (0.0885) | (0.0890) | (0.0882) | (0.0886) | (0.0904) |
| RA (Survey) | -0.207 | -0.198 | -0.231 | -0.256 | 0.0863 | 0.0842 | 0.0849 | 0.0760 | 0.0888 | 0.0855 | 0.0910 | 0.138 |
| | (0.181) | (0.179) | (0.190) | (0.196) | (0.160) | (0.161) | (0.159) | (0.165) | (0.143) | (0.141) | (0.143) | (0.145) |
| Risk perception (FFE) | -0.147** | -0.146** | -0.148** | -0.150** | 0.0715 | 0.0792 | 0.0804 | 0.0772 | 0.137** | 0.128* | 0.128* | 0.132** |
| | (0.0652) | (0.0656) | (0.0664) | (0.0656) | (0.0582) | (0.0582) | (0.0582) | (0.0573) | (0.0656) | (0.0657) | (0.0661) | (0.0638) |
| Risk perception (Survey) | -0.759*** | -0.752*** | -0.790*** | -0.790*** | 0.368*** | 0.361*** | 0.361*** | 0.357*** | 0.192** | 0.196** | 0.207** | 0.198** |
| | (0.106) | (0.106) | (0.108) | (0.106) | (0.118) | (0.119) | (0.119) | (0.120) | (0.0927) | (0.0908) | (0.0906) | (0.0887) |
| Inverse round trend | | -0.776*** | -1.255*** | -1.258*** | | 1.186*** | 1.465*** | 1.476*** | | -0.735*** | -0.513* | -0.513* |
| | | (0.217) | (0.222) | (0.222) | | (0.183) | (0.194) | (0.197) | | (0.220) | (0.263) | (0.266) |
| Increasing risk | | 0.289 | 0.186 | 0.179 | | -0.271 | -0.200 | -0.191 | | -0.0208 | 0.0288 | 0.0332 |
| | | (0.269) | (0.269) | (0.266) | | (0.246) | (0.244) | (0.239) | | (0.281) | (0.287) | (0.290) |
| Beliefs | | -0.106*** | -0.106*** | -0.107*** | | 0.0259 | 0.0240 | 0.0211 | | 0.166*** | 0.165*** | 0.170*** |
| | | (0.0348) | (0.0346) | (0.0340) | | (0.0349) | (0.0349) | (0.0351) | | (0.0372) | (0.0372) | (0.0377) |
| Lag of cumulative drought occurrence | | | -0.513*** | -0.515*** | | | 0.298*** | 0.301*** | | | 0.220* | 0.227* |
| | | | (0.102) | (0.100) | | | (0.0972) | (0.1000) | | | (0.118) | (0.118) |
| Neighbours fire occurrences | | | 0.630* | 0.558 | | | -0.177 | -0.168 | | | -0.242 | -0.198 |
| | | | (0.382) | (0.398) | | | (0.222) | (0.224) | | | (0.245) | (0.253) |
| Afar fire occurrences (Survey) | | | 0.361 | 0.416 | | | -0.0106 | 0.0585 | | | -0.165 | -0.241 |
| | | | (0.268) | (0.269) | | | (0.231) | (0.221) | | | (0.190) | (0.196) |
| Age (FFE) | | | | 0.0105 | | | | 0.00622 | | | | -0.0207*** |
| | | | | (0.00796) | | | | (0.00722) | | | | (0.00669) |
| Age (Survey) | | | | -0.0203 | | | | 0.00553 | | | | 0.0127 |
| | | | | (0.0168) | | | | (0.0126) | | | | (0.0100) |
| Male (FFE) | | | | 0.232 | | | | -0.389* | | | | 0.176 |
| | | | | (0.244) | | | | (0.215) | | | | (0.194) |
| Male (Survey) | | | | 0.273 | | | | 0.750* | | | | -0.773* |
| | | | | (0.498) | | | | (0.454) | | | | (0.413) |
| Age group (FFE) | | | | -0.00700 | | | | 0.0130 | | | | -0.00593 |
| | | | | (0.0225) | | | | (0.0199) | | | | (0.0235) |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Male group (FFE) | | | | 0.00871 | | | | -0.000970 | | | | -0.0106 |
| | | | | (0.0117) | | | | (0.0102) | | | | (0.00963) |
| Education (FFE) | | | | 0.0710** | | | | -0.0980*** | | | | 0.0420 |
| | | | | (0.0303) | | | | (0.0303) | | | | (0.0265) |
| Education (Survey) | | | | 0.0265 | | | | 0.0977** | | | | -0.113** |
| | | | | (0.0585) | | | | (0.0482) | | | | (0.0498) |
| Constant | 0.0744 | 0.552* | 1.033*** | -0.146 | -0.907*** | -1.426*** | -1.710*** | -1.700 | -2.368*** | -2.445*** | -2.660*** | -1.280 |
| | (0.293) | (0.314) | (0.307) | (1.035) | (0.248) | (0.327) | (0.327) | (1.191) | (0.285) | (0.331) | (0.368) | (1.044) |
| Observations | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 |
| Number of groups | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 |
| F-test | 0 | 0 | 0 | 0 | 3.17e-07 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Log pseudo-likelihood | -1640 | -1623 | -1610 | -1604 | -1743 | -1716 | -1712 | -1698 | -1424 | -1406 | -1403 | -1386 |

Session cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Table 21 Regression results, stated preferences - positive framing social preferences measure*

| | SP1_a_1 | SP1_a_2 | SP1_a_3 | SP1_a_4 | SP1_cf_1 | SP1_cf_2 | SP1_cf_3 | SP1_cf_4 | SP1_f_1 | SP1_f_2 | SP1_f_3 | SP1_f_4 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Survey dummy | 0.868** | 0.370 | -0.311 | 0.434 | -2.318*** | -1.817*** | -1.483*** | -2.254* | 1.390*** | 1.482*** | 1.782*** | 1.497 |
| | (0.412) | (0.435) | (0.434) | (1.395) | (0.513) | (0.542) | (0.556) | (1.251) | (0.403) | (0.428) | (0.471) | (1.275) |
| SP1 (FFE) | -0.136 | -0.140 | -0.137 | -0.163 | 0.161 | 0.163 | 0.161 | 0.197* | -0.0459 | -0.0369 | -0.0391 | -0.0354 |
| | (0.113) | (0.114) | (0.115) | (0.117) | (0.106) | (0.109) | (0.109) | (0.112) | (0.0846) | (0.0832) | (0.0834) | (0.0834) |
| SP1 (Survey) | 0.249 | 0.241 | 0.199 | 0.180 | -0.00313 | -0.00189 | 0.00816 | -0.0315 | -0.129 | -0.135 | -0.125 | -0.111 |
| | (0.203) | (0.205) | (0.206) | (0.205) | (0.169) | (0.175) | (0.175) | (0.178) | (0.129) | (0.127) | (0.128) | (0.130) |
| RA (FFE) | -0.0481 | -0.0528 | -0.0499 | -0.0502 | 0.0948 | 0.0974 | 0.0968 | 0.117 | -0.0786 | -0.0757 | -0.0771 | -0.110 |
| | (0.0987) | (0.0967) | (0.0977) | (0.102) | (0.0832) | (0.0843) | (0.0840) | (0.0857) | (0.0876) | (0.0866) | (0.0870) | (0.0898) |
| RA (Survey) | -0.127 | -0.112 | -0.158 | -0.194 | 0.0693 | 0.0674 | 0.0711 | 0.0611 | 0.0501 | 0.0440 | 0.0536 | 0.109 |
| | (0.192) | (0.189) | (0.200) | (0.206) | (0.153) | (0.153) | (0.152) | (0.158) | (0.135) | (0.134) | (0.135) | (0.139) |
| Risk perception (FFE) | -0.150** | -0.149** | -0.151** | -0.153** | 0.0741 | 0.0816 | 0.0828 | 0.0808 | 0.136** | 0.127* | 0.127* | 0.132** |
| | (0.0654) | (0.0659) | (0.0666) | (0.0659) | (0.0577) | (0.0579) | (0.0579) | (0.0567) | (0.0651) | (0.0653) | (0.0657) | (0.0635) |
| Risk perception (Survey) | -0.737*** | -0.730*** | -0.769*** | -0.769*** | 0.366*** | 0.358*** | 0.359*** | 0.354*** | 0.187** | 0.190** | 0.203** | 0.195** |
| | (0.109) | (0.109) | (0.111) | (0.110) | (0.119) | (0.120) | (0.120) | (0.121) | (0.0933) | (0.0915) | (0.0914) | (0.0893) |
| Inverse round trend | | -0.776*** | -1.256*** | -1.259*** | | 1.185*** | 1.463*** | 1.476*** | | -0.734*** | -0.511* | -0.514* |
| | | (0.217) | (0.221) | (0.221) | | (0.182) | (0.194) | (0.196) | | (0.220) | (0.263) | (0.267) |
| Increasing risk | | 0.285 | 0.185 | 0.174 | | -0.266 | -0.196 | -0.187 | | -0.0233 | 0.0254 | 0.0324 |
| | | (0.272) | (0.271) | (0.267) | | (0.248) | (0.246) | (0.240) | | (0.281) | (0.287) | (0.291) |
| Beliefs | | -0.105*** | -0.106*** | -0.106*** | | 0.0258 | 0.0240 | 0.0205 | | 0.165*** | 0.164*** | 0.170*** |
| | | (0.0345) | (0.0343) | (0.0338) | | (0.0350) | (0.0350) | (0.0351) | | (0.0370) | (0.0370) | (0.0376) |
| Lag of cumulative drought occurrence | | | -0.515*** | -0.518*** | | | 0.297*** | 0.301*** | | | 0.221* | 0.227* |
| | | | (0.100) | (0.0984) | | | (0.0964) | (0.0984) | | | (0.118) | (0.118) |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Neighbours fire occurrences | | | 0.656* | 0.558 | | | -0.190 | -0.175 | | | -0.248 | -0.200 |
| | | | (0.358) | (0.376) | | | (0.227) | (0.228) | | | (0.238) | (0.245) |
| Afar fire occurrences (Survey) | | | 0.368 | 0.427 | | | -0.0152 | 0.0546 | | | -0.184 | -0.261 |
| | | | (0.275) | (0.271) | | | (0.230) | (0.220) | | | (0.190) | (0.196) |
| Age (FFE) | | | | 0.0116 | | | | 0.00471 | | | | -0.0205*** |
| | | | | (0.00804) | | | | (0.00745) | | | | (0.00655) |
| Age (Survey) | | | | -0.0213 | | | | 0.00646 | | | | 0.0141 |
| | | | | (0.0161) | | | | (0.0127) | | | | (0.00982) |
| Male (FFE) | | | | 0.212 | | | | -0.376* | | | | 0.181 |
| | | | | (0.242) | | | | (0.213) | | | | (0.191) |
| Male (Survey) | | | | 0.264 | | | | 0.751* | | | | -0.762* |
| | | | | (0.487) | | | | (0.454) | | | | (0.414) |
| Age group (FFE) | | | | -0.00683 | | | | 0.0147 | | | | -0.00590 |
| | | | | (0.0221) | | | | (0.0197) | | | | (0.0232) |
| Male group (FFE) | | | | 0.00906 | | | | -0.00170 | | | | -0.0107 |
| | | | | (0.0117) | | | | (0.0103) | | | | (0.00967) |
| Education (FFE) | | | | 0.0759** | | | | -0.103*** | | | | 0.0407 |
| | | | | (0.0299) | | | | (0.0305) | | | | (0.0270) |
| Education (Survey) | | | | 0.0376 | | | | 0.0974** | | | | -0.113** |
| | | | | (0.0582) | | | | (0.0486) | | | | (0.0503) |
| Constant | 0.0874 | 0.565* | 1.046*** | -0.209 | -0.919*** | -1.439*** | -1.721*** | -1.670 | -2.364*** | -2.438*** | -2.654*** | -1.279 |
| | (0.294) | (0.314) | (0.307) | (1.026) | (0.247) | (0.325) | (0.325) | (1.190) | (0.284) | (0.330) | (0.368) | (1.052) |
| Observations | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 | 2,988 |
| Number of groups | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 |
| F-test | 0 | 0 | 0 | 0 | 7.29e-07 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Log pseudo-likelihood | -1643 | -1626 | -1612 | -1605 | -1741 | -1714 | -1710 | -1695 | -1425 | -1407 | -1404 | -1387 |

Session cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Table 22 Regression results, Incentive compatible measures*

| | ic_a_1 | ic_a_2 | ic_a_3 | ic_a_4 | ic_cf_1 | ic_cf_2 | ic_cf_3 | ic_cf_4 | ic_f_1 | ic_f_2 | ic_f_3 | ic_f_4 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Survey dummy | 0.884** | 0.405 | -0.288 | 0.930 | -2.640*** | -2.191*** | -1.858*** | -2.643** | 1.640*** | 1.734*** | 2.021*** | 1.746 |
| | (0.426) | (0.465) | (0.466) | (1.538) | (0.529) | (0.561) | (0.568) | (1.227) | (0.382) | (0.408) | (0.442) | (1.279) |
| IC-giving (FFE) | 0.0301 | 0.00698 | 0.00507 | -0.0126 | 0.0185 | 0.0232 | 0.0258 | 0.0524 | -0.0754 | -0.0426 | -0.0403 | -0.0611 |
| | (0.101) | (0.0989) | (0.0992) | (0.0995) | (0.0779) | (0.0775) | (0.0773) | (0.0753) | (0.0810) | (0.0801) | (0.0803) | (0.0799) |
| IC-giving (Survey) | 0.552*** | 0.566*** | 0.603*** | 0.584*** | -0.0889 | -0.0964 | -0.0985 | -0.112 | -0.181 | -0.213 | -0.220* | -0.187 |
| | (0.176) | (0.176) | (0.175) | (0.178) | (0.173) | (0.173) | (0.174) | (0.172) | (0.134) | (0.134) | (0.132) | (0.140) |
| IC-risk (FFE) | -0.0243 | -0.0222 | -0.0231 | -0.0164 | 0.0434 | 0.0403 | 0.0415 | 0.0387 | -0.0327 | -0.0275 | -0.0262 | -0.0213 |
| | (0.0746) | (0.0761) | (0.0770) | (0.0742) | (0.0697) | (0.0718) | (0.0723) | (0.0726) | (0.0833) | (0.0871) | (0.0864) | (0.0883) |
| IC-risk (Survey) | 0.0646 | 0.0593 | 0.0287 | 0.0261 | -0.210** | -0.206* | -0.204* | -0.197* | 0.115 | 0.105 | 0.100 | 0.0918 |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (0.160) | (0.161) | (0.167) | (0.169) | (0.105) | (0.108) | (0.110) | (0.115) | (0.119) | (0.120) | (0.121) | (0.127) |
| Risk perception (FFE) | -0.165** | -0.161** | -0.162** | -0.166** | 0.0710 | 0.0770 | 0.0770 | 0.0772 | 0.160** | 0.149** | 0.149** | 0.153** |
| | (0.0657) | (0.0663) | (0.0672) | (0.0666) | (0.0590) | (0.0593) | (0.0595) | (0.0588) | (0.0657) | (0.0656) | (0.0660) | (0.0644) |
| Risk perception (Survey) | -0.779*** | -0.775*** | -0.817*** | -0.817*** | 0.408*** | 0.403*** | 0.406*** | 0.392*** | 0.154* | 0.160* | 0.173** | 0.167** |
| | (0.106) | (0.103) | (0.108) | (0.107) | (0.120) | (0.122) | (0.122) | (0.121) | (0.0867) | (0.0853) | (0.0852) | (0.0831) |
| Inverse round trend | | -0.709*** | -1.216*** | -1.215*** | | 1.108*** | 1.410*** | 1.424*** | | -0.734*** | -0.516** | -0.525** |
| | | (0.207) | (0.211) | (0.211) | | (0.171) | (0.184) | (0.186) | | (0.210) | (0.249) | (0.252) |
| Increasing risk | | 0.280 | 0.164 | 0.162 | | -0.269 | -0.188 | -0.189 | | -0.0460 | 0.00494 | 0.00875 |
| | | (0.291) | (0.290) | (0.286) | | (0.255) | (0.252) | (0.242) | | (0.271) | (0.275) | (0.276) |
| Beliefs | | -0.114*** | -0.114*** | -0.114*** | | 0.0247 | 0.0221 | 0.0198 | | 0.175*** | 0.174*** | 0.176*** |
| | | (0.0361) | (0.0357) | (0.0355) | | (0.0347) | (0.0345) | (0.0342) | | (0.0363) | (0.0365) | (0.0373) |
| Lag of cumulative drought occurrence | | | -0.546*** | -0.544*** | | | 0.322*** | 0.326*** | | | 0.215** | 0.214** |
| | | | (0.102) | (0.100) | | | (0.107) | (0.107) | | | (0.107) | (0.106) |
| Neighbours fire occurrences | | | 0.641 | 0.543 | | | -0.208 | -0.235 | | | -0.196 | -0.114 |
| | | | (0.410) | (0.434) | | | (0.235) | (0.236) | | | (0.223) | (0.234) |
| Afar fire occurrences (Survey) | | | 0.342 | 0.385 | | | 0.0492 | 0.119 | | | -0.208 | -0.273 |
| | | | (0.269) | (0.267) | | | (0.239) | (0.228) | | | (0.179) | (0.183) |
| Age (FFE) | | | | 0.00278 | | | | 0.00927 | | | | -0.0162*** |
| | | | | (0.00800) | | | | (0.00698) | | | | (0.00619) |
| Age (Survey) | | | | -0.0153 | | | | 0.000649 | | | | 0.0110 |
| | | | | (0.0178) | | | | (0.0131) | | | | (0.0108) |
| Male (FFE) | | | | 0.209 | | | | -0.333 | | | | 0.160 |
| | | | | (0.240) | | | | (0.213) | | | | (0.185) |
| Male (Survey) | | | | 0.102 | | | | 0.650 | | | | -0.582 |
| | | | | (0.458) | | | | (0.455) | | | | (0.388) |
| Age group (FFE) | | | | 0.00556 | | | | 0.0127 | | | | -0.0127 |
| | | | | (0.0251) | | | | (0.0207) | | | | (0.0222) |
| Male group (FFE) | | | | 0.00922 | | | | -0.00560 | | | | -0.00612 |
| | | | | (0.0122) | | | | (0.0110) | | | | (0.00951) |
| Education (FFE) | | | | 0.0687** | | | | -0.100*** | | | | 0.0543** |
| | | | | (0.0288) | | | | (0.0296) | | | | (0.0253) |
| Education (Survey) | | | | 0.0336 | | | | 0.119*** | | | | -0.134*** |
| | | | | (0.0606) | | | | (0.0456) | | | | (0.0468) |
| Constant | 0.0722 | 0.534 | 1.042*** | -0.337 | -0.843*** | -1.315*** | -1.618*** | -1.533 | -2.486*** | -2.571*** | -2.779*** | -1.576 |
| | (0.306) | (0.337) | (0.333) | (1.160) | (0.255) | (0.333) | (0.332) | (1.181) | (0.289) | (0.335) | (0.362) | (1.043) |
| Observations | 3,288 | 3,288 | 3,288 | 3,288 | 3,288 | 3,288 | 3,288 | 3,288 | 3,288 | 3,288 | 3,288 | 3,288 |
| Number of groups | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 | 71 |
| F-test | 0 | 0 | 0 | 0 | 8.65e-09 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Log pseudo-likelihood | -1769 | -1752 | -1736 | -1730 | -1912 | -1886 | -1881 | -1862 | -1547 | -1526 | -1523 | -1504 |

Session cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

# Appendix to all papers

## Experiment instructions

Dear all, thanks for accepting our invitation.

First of all, we are going to explain the activities we are going to carry out, then we are going to play the game, and finally we will conduct a short survey. Let's start.

This game is a different and amusing way to actively engage in a research project about agricultural techniques, use and control of fires. After playing the game, we will ask you to answer a short questionnaire.

The reason why money is involved, is to replicate a situation close to your real life one. Using money, your choices will have a consequence for you. This is a new kind of research, rarely implemented. It is very different from other kind of research in which you might have been involved in the past, present, or you will be involved in the future. Therefore, do not expect payments from other researcher with whom you may be asked to collaborate.

All your choices are confidential, and your name will not be revealed to anyone. The number that you received will be the only identifier of your information.

This game is different from other games or surveys in which you or other people in this community might have been involved. Therefore, comments that you heard from other people does not necessary apply to this game.

In the end of the game, the points that you are going to earn will be converted in R$, the rate is 1R$ for each 80 points. All the funds are made available by a research institution.

[After distributing the baseline payoff table]

Let's now explain the rules of the game. Please, pay the highest attention to the instructions. If you understand instructions, you will be able to make better choices in the game. If you have any question, don't hesitate to raise your hand and ask us.

In this game each of you own a farm of the same size. The other players are your neighbors. You have to make decisions about how to cultivate your land: in the first option, called F, you can use fire, for example: to slash and burn or for pasture maintenance. This does not means that you don't have other plantations on your farm, but the main produces come from *roça* and pastures.

If you decide to use fire, you have to decide whether you will adopt any fire control measures, such as: firebreaks, backfire, to burn in the coolest hours of the day, and so forth… In this game these fire control measures allows to control the burnt, for sure it will not escape to other areas. Meanwhile fire control measures comes at some costs, due to additional work and a lower final quality of the burnt. Controlled fire, CF, is the second choice showed in the table.

If you will choose not to control fire, it will escape in other areas and burn the neighbors' farms.

With the third option, called A, you adopt alternative techniques such as direct planting, use of a tractor, or mechanical or manual pasture maintenance. Perennial crops, such as agroforestry systems, açai or oil palms are also plantations that do not require fire use.

In each round of the game you are going to chose which one of these three options you prefer to implement, and each one will lead to a different earning:

- If you choose to use fire you will always earn 100 points

- If you choose to use fire and to control it, you will always earn 70 points, 100 minus the 30 points' cost of fire control measures.

If you choose to use altenatives to fire use you can earn up to the double of what you would earn with fire, because of perennials and intensive agriculture. The earning is of 200 points. However, since these plantations can accidentally burn, you can loose a part of your earning. This depends on how many of your neighbors choose F, to use fire without controling it. On the other side of the paper sheet, the big green table shows the earning of each choice for each number of neighbors who chooses F, that are listed on the first column. Line 0 corresponds to 0 neighbors choosing F, line 1 correspond to 1 neighbor choosing F, line 2 to 2 neighbors choosing F and so on until line 7, which indicate the earning of each choice when 7 neighbors choose F. As you can see from this table, if you choose A, for each neighbor that burns without control you are going to loose some points, but the other cultivations, F and CF, ensure a constant earning. Choosing A you can earn from 200 to 13 depending on how many neighbors choose F, fire without control.

Let's now turn to some examples

If you choose A, the alternatives to fire use, and one of your neighbor chooses fire without control, you are going to earn 166 points. If two neighbors choose fire without control F, you are going to earn 134, if 5 neighbors choose fire without control F, you are going to earn 53, and so forth. But if you choose F yourself, you always earn 100 points, and if you choose controlled fire CF you always earn 70 points.

The others are going to make their choices at the same time as you, so you don't know how many of your neighbors are going to use fire and if they are going to control it or not. You cannot chat with your neighbors. So, you have to guess the others' choices. Accordingly, you can help yourself with the payoff table to decide what to plant.

One more example: if 5 neighbors are going to use fire without control you can earn 100 if you farm with fire, 70 if you farm with fire and control it and 53 if farming without fire. But if 2 neighbors are going to use fire without control you can earn 134 by choosing crops without fire, always 100 if using fire and 70 if using fire and controlling it.

Do you have any questions? Did you understand how the payoff table works?

[Answering questions]

Apart from fire coming from neighbors, there is also another source of risk: the risk of fires coming from afar. This happens, for instance, during years with less rain, or when pastures get dry, or if somebody from afar lose control over his fire, or throw a cigarette but without extinguishing it, etc etc.

If a fire from afar occurs you lose more of your fire-free crops. This damage cumulates to the damages caused by the fires from neighbors.

If a fire from afar occurs, you are going to earn the value specified in the smaller table, the red one on the bottom right side of the paper.

If 5 neighbors used fire without control and if a fire from afar occurs, you are always going to earn 100 points if you farm with fire, 70 if you farm with controlled fire, and 0 if you chose alternatives to fire use.

Another example: If you think that 2 neighbors are going to use fire without control, you can earn 100 points if farming with fire, 70 if you farm with controlled fire and only 9 if you chose alternatives to fire use and a fire from afar occurred; but if the fire from afar does not occurs you would earn 134.

[Showing payoffs on the table]

Any questions?

[Answering questions]

In order to decide if a fire from afar will take place, after making your choice, we are going to roll a 10-sided dice like this one.

[Showing the dice]

IF STABLE RISK TREATMENT APPLIES:

If the dice falls on 1, 2 or 3 a fire from afar occurs. This corresponds to a fire risk of 30%

IF INCREASING RISK TREATMENT APPLIES:

The risk will increase along the game, as if the climate gets drier and drier along the years.

In the first round, no dice is rolled and there is no risk of fire from afar.

In the second round, there will be a fire from afar if the dice falls on 1. This corresponds to a fire risk of 10%

In the third round, there will be a fire from afar if the dice falls on 1,2 or 3. This corresponds to a fire risk of 30%

In the fourth round, there will be a fire from afar if the dice falls on 1,2,3,4, or 5. This corresponds to a fire risk of 50%

In the fifth round, there will be a fire from afar if the dice falls on 1,2,3,4,5 or 6. This corresponds to a fire risk of 60%

In the sixth round, no dice is rolled and there is no risk of fire from afar.

In the seventh round, there will be a fire from afar if the dice falls on 1. This corresponds to a fire risk of 10%

In the eighth round, there will be a fire from afar if the dice falls on 1,2 or 3. This corresponds to a fire risk of 30%

In the ninth round, there will be a fire from afar if the dice falls on 1,2,3,4, or 5. This corresponds to a fire risk of 50%

In the tenth round, there will be a fire from afar if the dice falls on 1,2,3,4,5 or 6. This corresponds to a fire risk of 60%

[Showing a table that presents risk and rounds]

During the game, you can consult the two payoff tables. I suggest that you always compare the choices that you want to make in both scenarios: with and without a fire from afar occurring.

Do you have any questions?

[Answering the questions]

[Distributing paper tips for choices and beliefs collection]

Now it is very important for the success of the game that you keep silence in the room.

Overall, you are going to play two blocks of 5 rounds each. After the first 5 rounds we are going to interrupt the game and give you further instructions.

On one of the sides of the paper tip there is a question: How many of your neighbors are going to choose F? You should answer the number of neighbors that you think are going to choose fire without control, F, and that might represent a risk for you. This helps you to make choices based on the payoff tables. Each time that you guess the right number of neighbors that chose fire without control, F, you are going to earn 30 additional points.

After answering the question, on the other side of the paper tip, you are going to mark your farming choice:

- F farming with fire
- CF farming with controlled fire
- A farming with alternatives to fire use

Be careful that nobody see your choice. This is very important for the success of the game! If you are going to chat with each other the results are going to be invalidated and we will have to suspend the session, and nobody will get any reward.

After making your choice we are going to tell you how many of you actually choose F, CF or A, and if a fire from afar occurred or not.

This routine is going to be repeated each round

Do you have any questions?

[Answering questions]

Now we are going to play a trial session. This round is only to learn, and there is not going to be any compensation.

Remember that you cannot chat with each other during the game.

[Ask participants to sit backward to the circle]

[Up to two trial rounds are played and eventual questions are answered during the practice]

Now we are going to start the game. All your choices will now be remunerated!

[After 5 rounds]

The first part of the game is over. For the next five rounds we are going to introduce a new rule.

[Announce the rule]

IF POLICY IS COMMAND AND CONTROL

The government is intensifying controls, and if it finds out, punishes those who let the fire escape.

In each round, in order to know if a police control is going to happen, after you made your decisions, we are going to roll a 6 sided dice.

If it falls on 1 or 2 (around 33% of risk), there is going to be a police control, and if you used fire without control, you are going to receive a fine of 73 points, which is subtracted from your round earning. So, if a police check occurs, F only yields 27. Be aware that the fine does not affet yields from past or future rounds.

IF POLICY IS COMMUNICATION

An NGO and the Government promote the adoption of community rules to manage and control fires. Regular meetings are organized before the fire season and the neighbors can discuss on how to use fire. In each round before making your choices, you are allowed to discuss with your neighbors for a minute. After that period any other form of communication is forbidden again.

[During each minute of communication, participants are allowed to turn inward the circle, after that, participants turn outward again beofre making their choices]

IF POLICY IS PAYMENTS FOR ENVIRONMENTAL SERVICES

[Distributing the new payoff tables]

An NGO and the Government decide to start a project of payments for environmental services in your area to promote environmental conservation. They make the following offer: if you choose to invest in alternatives to fire use you are going to receive an additional payment of 30 points. Here goes a new payoff table.

# Federico Cammelli

Federico Cammelli was born in Florence, Italy, in 1990. He holds BSc. and MSc. degrees in Development Economics from the University of Florence (2011, 2014).

The thesis consists of an introduction and four independent papers investigating the economic causes underlying forest fires in the Brazilian Amazon and the policies to prevent them. Fire is largely used in many agricultural practices, and when it does not involve new deforestation, with limited damage for the environment. Yet, if (costly) fire control measures are not adopted, fire might escape out of the intended plot, causing large damages to the environment and the local people.

Paper I assesses the impact of fire risk externalities on Brazilian smallholder farmers. An econometric analysis of spatial and survey data reveals that the negative impact of fire risk externalities on farmers' revenue is large compared to the return on other factors of production and is higher for non-fire users than for fire users. This occurs because fires are detrimental to investments in alternative fire-free techniques. Fire risk externalities, originating from neighbors uncontrolled use of fire prevent investments in high yield but flammable alternative land use types, and create incentives favoring the persistence of uncontrolled fire use, generating a poverty trap and degrading the environment. Strategic complementarities arising from fire management engender a coordination problem, where both uncontrolled fire use and adoption of alternative techniques are equilibria.

The rest of the thesis reports on a coordination framed field experiment based on the findings of the first paper. The second paper assesses *ex ante* and compares the impact of payments for environmental services and command and control policies under stable and increasing drought risk scenarios. It is found that both policies increase adoption of alternative techniques, however the second, but not the first, reduces investments in fire control. It is also found that drought risk reduces adoption of both fire control and alternative techniques. The third paper reports on a level-k analysis of communication in coordination games under a weak social norm, discussing the potential of community based interventions, and a related experimental test. The fourth and last paper assesses the role of beliefs, social and risk preferences and perception on coordination both in the experiment and in stated actual behavior. Fire risk perception but not risk aversion negatively affects coordination in and outside the experiment, while pro-social preferences weakly improve coordination. A novel external validity test based on commonality of predictors in and outside the experiment is described. It is found that external validity holds for parts of the experiment, while the test is inconclusive for others. We describe and discuss the use of predictor validity, and identify new challenges for external validity tests.

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