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Par Michael John Alexander Murphy

**Comprendre le risque et l'adoption en réponse à l'incertitude  
environnementale**

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Soutenue le 19 décembre 2024

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## Résumé en français

### **Comprendre le risque et l'adoption en réponse à l'incertitude environnementale**

Les individus doivent souvent prendre des décisions dans des conditions d'incertitude, où les risques associés à leurs choix sont difficiles ou impossibles à quantifier. Cela est particulièrement vrai pour les interactions avec l'environnement où les agents économiques peuvent manquer de compréhension des systèmes naturels complexes. À l'aide de trois études sur la prise de décision dans le contexte des systèmes alimentaires dans les économies en développement, cette thèse examine les décisions individuelles de changer de comportement en réponse à l'incertitude environnementale. Les chapitres de la thèse explorent chacun ce sujet dans un contexte différent, en utilisant un ensemble distinct de techniques analytiques.

Le premier chapitre analyse une expérience de terrain encadrée menée auprès d'agriculteurs du nord du Ghana. Il se concentre sur l'incertitude entourant les avantages associés à un ensemble de pratiques agroécologiques connues collectivement sous le nom de techniques d'« agriculture de conservation ». Ces pratiques ont des caractéristiques de bien public, mais alors que le coût d'adoption est immédiat, les avantages privés pour les producteurs s'accumulent au fil du temps. L'expérience incitative simule la décision de l'agriculteur d'adopter (ou de ne pas adopter) des pratiques d'agriculture de conservation au fil des saisons. L'analyse teste l'efficacité de deux approches pour réduire l'incertitude autour des avantages privés associés à l'adoption, afin d'évaluer les approches politiques potentielles pour encourager l'adoption.

Le deuxième chapitre est une analyse de données de panel secondaire examinant l'effet des chocs de température sur la production de blé en Inde dans le contexte de la Révolution verte. L'incertitude autour des résultats météorologiques a toujours été une préoccupation des agriculteurs, mais comprendre comment les producteurs s'adaptent – ou ne s'adaptent pas – aux conditions météorologiques imprévues est une question de plus en plus urgente à l'ère du réchauffement climatique. J'applique les avancées récentes de l'économétrie d'estimation de l'adaptation agricole au climat à un nouvel ensemble de données de panel et j'explore comment l'adoption de la technologie de la Révolution verte sous la forme de semences de variétés à haut rendement et d'intrants associés a affecté la capacité des agriculteurs à s'adapter aux chocs de température.

Le troisième chapitre se concentre sur les consommateurs, en mettant en œuvre un essai contrôlé randomisé auprès de ménages urbains à faible revenu au Kenya. L'étude fournit aux ménages traités des informations importantes sur un problème de sécurité alimentaire – la contamination de la farine de maïs par l'aflatoxine – en utilisant les données d'une étude antérieure menée dans la région. L'analyse examine comment la fourniture d'informations aux participants sur ce danger affecte leurs perceptions subjectives du risque, et à son tour comment cette mise à jour des croyances affecte leur comportement d'achat. Dans ces trois contextes différents, la thèse montre comment différentes approches microéconomiques de l'inférence causale peuvent aider à comprendre comment les individus réagissent à un environnement changeant. Ces résultats visent à améliorer notre compréhension de la prise de décision dans des contextes d'incertitude où elle est rarement étudiée, et visent également à contribuer à une base de données concrètes permettant

aux décideurs politiques des pays en développement de concevoir des politiques améliorant le bien-être des citoyens et améliorant l'accès à l'information et aux nouvelles technologies.

**Mots clés: incertitude ; risque ; agriculture**

## Résumé en anglais

### Understanding risk & adoption in response to environmental uncertainty

Individuals must often make decisions under conditions on uncertainty, where the risks associated with their choices are difficult or impossible to quantify. This is particularly true of interactions with the environment where economic agents may lack understanding of complex natural systems. Using three studies of decision-making in the context of food systems in developing economies, this thesis examines individual decisions to change behaviour in response to environmental uncertainty. The chapters of the thesis each explore this topic in a different setting, using a distinct set of analytical techniques.

The first chapter analyzes a framed field experiment carried out with farmers in northern Ghana. It focuses on uncertainty surrounding the benefits associated with a set of agroecological practices known collectively as ‘conservation agriculture’ techniques. These practices have public good characteristics, but whereas the cost to adoption is immediate, the private benefits to producers accrue over time. The incentivized experiment simulates the farmer’s decision to adopt (or not adopt) conservation agriculture practices across seasons. The analysis tests the effectiveness of two approaches to reducing uncertainty around the private benefits associated with adoption, in order to evaluate potential policy approaches to encourage take-up.

The second chapter is a secondary panel data analysis looking at the effect of temperature shocks on wheat production in India in the context of the Green Revolution. Uncertainty around weather outcomes has always been a concern of farmers, but understanding how producers adapt- or fail to adapt- to unpredicted weather is an increasingly urgent question in the age of global warming. I apply recent advances in the econometrics of estimating agricultural adaptation to climate to a novel panel dataset and explore how the adoption of Green Revolution technology in the form of high-yielding variety seeds and associated inputs affected farmers’ ability to adapt to temperature shocks.

The third chapter focuses on consumers, implementing a randomized control trial with low-income urban households in Kenya. The study provides treated households with salient information on a food safety issue- aflatoxin contamination of maize flour- using data from a prior study carried out in the area. The analysis examines how providing participants with information about this hazard affects their subjective risk perceptions, and in turn how this belief updating affects their purchasing behaviour.

Across these three different settings, the thesis show how different microeconomic approaches to causal inference can help to understand how individuals respond to a changing environment. These findings aim to improve our understanding of decision-making under uncertainty in contexts where it is rarely studied, and also aim to contribute to a practical evidence base for policymakers in developing countries to design welfare-enhancing policies that enhance citizen access to information and new technologies.

**Keywords:** Uncertainty ; risk ; agriculture

*Il n'est pas certain que tout soit incertain.*

- *Pascal, Pensées*

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## Introduction générale

*Though the success of a particular day's fishing maybe a very uncertain matter, yet the local situation of the country being supposed, the general efficacy of industry in bringing a certain quantity of fish to market, taking the course of a year, or of several years together, it may, perhaps, be thought is certain enough; and it, no doubt, it so.*

- Adam Smith, The Wealth of Nations

Uncertainty is a fundamental condition of agricultural production. Even in the most technologically advanced settings, when a farmer plants their seed they cannot know with certainty how much their field will yield, whether a storm will damage their crop, or what the market price will be at harvest time. Globally, there are over 570 million farms, five of every six of which are smallholdings less than two hectares in area, and three of every four of which are operated by families (Lowder, Scoet, & Raney, 2016). Hundreds of millions of smallholders in the Global South must rely on these farms for their livelihoods without access to many biophysical or informational technologies which could reduce the uncertainty they face.

The goal of this thesis is to understand how changes in available technologies can ameliorate uncertainty and thus affect outcomes for food systems actors in developing countries. It comprises three studies, each of which is situated in a different developing economy and each of which utilizes a different microeconomic methodology, but which have the common aim of analyzing how individuals respond when uncertainty around the risks they face is reduced.

The first chapter analyzes this question by studying uncertainty in the payoffs associated with taking up a novel set of agroecological practices. I present the results of a framed field experiment carried out with farmers in northern Ghana. In this experiment, we invited local farmers to participate in an incentivized simulation in which we introduce a time-limited subsidy to mitigate the uncertainty associated with the uptake of a set of farming practices collectively known as ‘conservation agriculture’.

The second chapter is a secondary analysis of panel data for wheat producers at the district-level in India. I construct a long-run dataset linking weather and agricultural outcomes, and use this to examine the extent to which the introduction of high-yielding variety seeds as part of the ‘Green Revolution’ enabled farmers to mitigate the effects of uncertainty in terms of inter-annual deviations in temperature from the long-run norm.

In the third chapter, the focus is on the uncertainty faced by consumers. The study takes the form of a randomized control trial which was implemented among low-income households in urban and peri-urban Kenya. The intervention was to provide participants assigned to treatment with information regarding a food safety risk and measure how this information treatment affects their purchasing decisions. The analysis looks at how improving the information available to consumers affects their perceptions of food safety hazards, and in turn how this affects purchasing behaviour.

In this introduction, I will provide a brief overview each of these chapters, and the results of their respective analyses. Before doing so however it will be useful to provide some discussion of the concept of ‘uncertainty’ in the context of environmental economics and clarify some important distinctions.

One reasonable definition of microeconomics (or at some subset of it) is that it is the study of individual decision-making and its outcomes. This necessarily implies a temporal aspect: an individual or firm makes a choice among some alternatives, each with their own potential outcome, an outcome is realized, and their utility goes up or down or stays the same accordingly. Each choice maps to some set of outcomes, over which they have some preference relation. I will argue that this relationship between choice sets and outcomes can be subject to three potential definitions of uncertainty, all of which are used within economics, often without careful delineation.

The first definition of uncertainty which I will consider is often typically referred to as ‘risk’ in economics and is likely to be the most familiar. This is a situation in which the relationship between choice sets and outcomes is subject to a defined set of probabilities, and those probabilities are known to the decisionmaker. This is the basis of the famous choice lottery from which von Neumann and Morgenstern (1947) demonstrate that given a set of assumptions regarding the preference relations of the decisionmaker- namely completeness, transitivity, continuity, and independence- a rational individual will always choose the lottery which maximizes their expected utility. In this case what is ‘uncertain’ is simply that the draw from the probability distribution that determines the outcome conditional on the individual’s choice is not known *ex ante*. This conception of uncertainty is useful in making a number of empirical problems appear tractable, but it is not the only definition of uncertainty which has historically been used within economics.

A second scenario which we may consider is a situation in which the probabilities associated between a choice and its potential outcomes are known but at least one is known imperfectly. This is the case in the eponymous paradox identified by Ellsberg (1961), in which he demonstrates that faced with a decision between a bet with a fixed probability, and one with an bounded unknown probability with a higher expected value, individuals prefer the choice for which the probability is known, even though on average the payoff is lower. This definition of uncertainty is typically referred to as ‘ambiguity’ within the literature and has been applied in a wide variety of contexts (Ilut & Schneider, 2022).

A third potential definition of uncertainty is what is typically referred to within economics as ‘Knightian’ uncertainty: situations in which the probabilities associated with outcomes (and perhaps even the outcomes themselves) are entirely unknown. The basis for this definition is taken from Frank Knight’s *Risk, Uncertainty and Profit* (1921)<sup>1</sup>:

*Uncertainty must be taken in a sense radically distinct from the familiar notion of Risk, from which it has never been properly separated [...] [A] measurable uncertainty, or “risk” proper, as we shall use the term, is so far different from an unmeasurable one that it is not in effect an uncertainty at all.”*

The key point for Knight is essentially epistemological, he draws a distinction between that which is simply subject to risk (ie. can be mapped probabilistically) and that which is truly unknown. Under Knightian uncertainty, the probabilities associated with outcomes are entirely unknown, and indeed the potential outcomes themselves may be partially or completely

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<sup>1</sup> It should be noted that a similar conception of this idea is also found contemporaneously in Keynes’ *Treatise on Probability* (1921). See Packard, Bylund and Clark (2021) for discussion.

unknown to the decisionmaker. This presents a problem for analysis, since fully unknown outcomes are inherently unquantifiable.

The extent to which this third definition of uncertainty is relevant to economics as a whole is a topic of some dispute. Kay & King (2020) refer to this as ‘radical uncertainty’ and argue that it is prevalent across a range of issues in economics. Sunstein (2023) focusing primarily on the US regulatory context, argues that true Knightian uncertainty is relatively rare and that most apparent cases can be resolved by treating them as ambiguity problems or repeated decision problems (ie compound lotteries). While it is difficult to assess these competing claims in totality, in the case of economic decision-making involving human interactions with the natural environment, there are good reasons to believe many problems are not questions of risk or ambiguity but involve true Knightian uncertainty.

A characteristic of many such problems involving natural systems is simply informational- the underlying biochemical or biophysical mechanisms underlying many human interactions with the natural environment may be understood poorly or not at all (Pindyck 2007, Aldy & Viscusi 2014), preventing assigning even a bounded probability. A related issue is that many environmental risks are ‘fat-tailed’ (Weitzman, 2011), where the extremes of the probability distribution are characterized by catastrophic loss. Additionally, environmental interactions are often irreversible (Arrow & Fisher 1974, Aldy & Viscusi, 2014), limiting individual’s ability to learn by doing. Lastly, many environmental decisions are subject to complexity (O’Connor et al., 1996) such that they involve multi-step processes where the choice set in subsequent stages is defined and constrained by prior choices and may not be observable *ex ante*.

While these issues are often discussed in terms of large-scale environmental decision-making processes, they apply equally to individual choices made under uncertainty in agricultural and environmental economics. The risk of catastrophic loss is as much a concern to a farmer trying a new technique as it is to a policymaker considering responses to global climate change. In the same way individual producers and consumers interacting with food systems often face decisions characterized by irreversibility or complexity.

In the next sections of this general introduction, I will introduce the respective chapters of the thesis and present the decisions made under environmental uncertainty by different groups of individuals from a range of developing country contexts and attempt to flip the focus of Smith’s analogy- rather than looking to the aggregate success of the fishing industry, I will aim to understand the decisions of the individual worried about coming home with empty nets.

In the first chapter, co-authored with Kate Ambler and Alan de Brauw, we implement a lab-in-the-field experiment with staple crop farmers in Northern Ghana. The objective of this experiment is to better understand farmers’ adoption of Conservation Agriculture (hereafter CA) techniques. CA is a set of four inter-related agroecological practices which aim to provide farmers with private benefits through improved yields and reduced vulnerability to rainfall shocks, while also generating public goods in the form of increased carbon sequestration, reduced biodiversity loss and reduced soil runoff into water catchments (Hobbs 2007; Bell et al. 2018).

Despite the claimed benefits, adoption of CA practices remains low (Giller et al. 2009; Michler et al., 2019), including in our setting, despite farmers in the area having been sensitized to the technology and demonstrating familiarity with it- over 95% of respondents in established farmer-based organizations report having heard of CA. We hypothesize that a key barrier to

adoption is the difference in timing of costs and benefits resulting from uptake. One of the main components of CA is the use of minimal soil disturbance (no-till) planting, where rather than ploughing the soil farmers instead use residual vegetation from the previous season as mulch. Adoption results in immediate increased costs for farmers in the form of increased labor and/or herbicide application for weeding but yield gains are only achieved in the long-run, taking up to ten years to be realized (Giller et al., 2009).

To explore this, we designed a framed field experiment which aimed to capture these key features of the technology. We invited farmers to participate in a stylized decision-making exercise which simulated their choice at the start of an agricultural season between using ploughing for land preparation and using minimal soil disturbance. The experiment consisted of ten rounds, each representing an agricultural season, at the beginning of which they would choose between practices. For each round, they paid out an amount of money (from an initial allocation we provided) to represent the costs associated with their choice of practice. They then received a payout based on their chosen practice, their choice in previous rounds, and a randomly determined realization of a variable representing ‘normal’ or ‘poor’ rainfall.

Our experimental design sought thus sought to capture the key features of CA in the form of greater resilience to rainfall shocks, and the realization of yield benefits under sustained continuous adoption. To replicate this yield gain, we increased the payout from choosing CA over multiple rounds, however we introduced random variation in the timing of the benefit (which would be realized after 5-7 rounds of continuous adoption). In this way, we sought to capture a key feature of CA technology- uncertainty in the amount of time needed to realize private benefits.

Using this structure, we randomly assigned farmers to two overlapping treatments, both aiming to address this uncertainty. The first of these was an incentive treatment, akin to a subsidy, which they were guaranteed to receive for the first four rounds of the experiment, conditional on choosing to adopt CA. The second treatment was a purely informational treatment in which farmers received randomly assigned vignettes about peers’ experiences relating to adoption.

Importantly, both treatments were designed with the intention of mitigating, but not completely alleviating uncertainty. The incentives payment was structured such that it was guaranteed to phase out at least one round before the yield gain could be realized under continuous CA adoption. The information treatment was designed to increase the salience of the information regarding adoption benefits by framing it in terms of peer effects, but did not contain any information not already conveyed to participants, and did not affect their incentives in the experiment.

We find a significant and positive effect associated with the incentives treatment, with treated farmers choosing CA practices for an additional 0.5 seasons on average relative to the control group. We do not find an effect for the information treatment overall, though there is some evidence to suggest it increased the probability of long-run adoption.

Our results contribute to a nascent literature within agricultural economics on conditional payments for the uptake of practices with environmental benefits. Several recent studies have looked at the relationship between payments for not burning (another core CA principle) and environmental public goods (Jack et al. 2023; Edwards et al., 2024). Oliva et al. (2020) study uptake of an agroforestry intervention and find evidence that uncertainty around profitability

due to the timing of information discovery reduces the cost-effectiveness of subsidies and results in suboptimal outcomes for increasing tree cover.

The second chapter of the thesis also focuses on agricultural producers, looking at an alternative source of uncertainty: variation in temperature outcomes and its effects on production in the short- and long-run. In contrast to the first chapter, this analysis uses secondary data to explore how the dissemination of improved high-yielding variety (HYV) seeds in India affected producers' ability to adapt to temperature shocks. In this paper, I build a unique dataset of which merges panel data on temperature and precipitation outcomes across India from 1951-2017 with data from the World Bank and ICRISAT on district-level wheat production to create a long run panel which mapping weather data to production outcomes. Using this resource, I integrate estimation of the effects of HYV dissemination to a specification which tests for adaptation to short- and long-run variations in weather to test whether technological change impacted farmers' ability to adapt to temperature fluctuations.

This paper contributes to a rapidly growing literature on the effect of climate on agricultural outcomes. At the global level it is estimated that agricultural total factor productivity has been lowered by 21% as a result of climate change, with the most severe impacts on developing countries in the tropics (Ortiz-Bobea et al., 2021). A number of authors have used aggregate yield estimates to try to measure agricultural adaptation to climate in high-income countries, primarily the United States (Deschênes & Greenstone, 2007; D'Agostino & Schlenker, 2016; Burke and Emerick, 2016; Hsiang et al. 2017; Cui 2020), in part because low- and middle-income countries typically do not have sufficiently long historical time series data on both weather and agricultural output to estimate climate effects.

To my knowledge, the first paper to attempt to estimate effects for India was is Guiteras (2009) who estimates crop losses associated with short-run weather shocks. Recent work by Kumar and Khanna (2023) and Constanza (2023) builds on recent econometric advances in estimating short- and long-run adaptation using penalized regression models applied to India (McIntosh & Schlenker, 2006; Mérel & Gammans 2022). I build on this work by demonstrating that the typical assumption of a steady technological trend is unlikely to be appropriate for the case of Indian agriculture over the late twentieth century, in which many producers experienced rapid technological change which varied by location as part of the 'Green Revolution'.

In the analysis, I estimate a difference-in-differences specification which exploits plausibly exogenous spatial variation in the introduction of HYV seeds for Indian wheat production, using modern econometric methods to account for the staggered nature of HYV deployment (Goodman-Bacon, 2019; Callaway & Sant'anna, 2023). I demonstrate that HYV introduction was associated with a large and sustained increase in yields, which continued as HYV technology diffused within districts. I then interact measure of HYV introduction with the penalized adaptation framework described in Mérel & Gammans (2023) to test for how farmers' production responded to environmental uncertainty in the form of temperature shocks under two different technological regimes.

My results show that failing to account for this change results in an estimated null effect suggesting that Indian wheat farmers' production is not well explained by temperature in the long- or short-run. Using the interacted specification, I show that the ability of wheat producers to respond to inter-annual temperature variation varied by whether HYV seeds had been introduced to a district. Post-adoption, districts experience a significant increase in yields, but also report relatively higher yields in years where the temperature was higher than the district's



long-run average. Conversely, pre-adoption districts report relative losses in years with higher than expected temperatures. This suggests that the introduction of HYV technology, in addition to raising productivity also in expanding the set of potential inputs to production, gave farmers greater ability to adjust their allocations within season in response to conditions, making them better able to adapt to climatic uncertainty.

The third chapter, co-authored with Vivian Hoffmann and Sarah Kariuki, considers uncertainty from the consumer perspective. We implement a randomized control trial among low-income households in informal settlement areas in urban and peri-urban Nairobi, Kenya in which we provide information about a health hazard related to food safety and observe the effect of this information on treated individuals' subjective risk perceptions and purchasing behaviour. Here the hazard is subject to true Knightian uncertainty- a large majority of study participants at baseline are unaware of any hazards related to the food product we study, with less than 10% able to name or describe the specific hazard.

This lack of information is reflective of a larger policy failure. Food safety hazards are pervasive in low- and middle-income settings but have received relatively little attention in the development economics literature. The burden of disease as a result of foodborne illness is large, comparable in magnitude to HIV/AIDS, malaria or tuberculosis and approximately one-third of diarrheal disease is transmitted through food (WHO 2015; Hald et al. 2016).

The focus of our intervention is aflatoxin contamination of maize flour, a commonly consumed staple in our study area. Aflatoxins are a group of pathogens caused by a type of soil fungus prevalent in many developing countries. Consumption of foods contaminated with aflatoxins can cause cancer and liver damage, and has particularly deleterious effects for young children, impairing their development (Strosnider et al. 2006; Turner, 2013).

Our study therefore targets households with the greatest potential health burden from aflatoxin contamination. Using data from local community-health volunteers, we created a sampling frame of 1500 low-income households (who are likely to lack access to adequate healthcare services) with children under the age of five, from four settlement areas in and around Nairobi.

The basis of the information intervention is data which we collected in a previous project with co-authors from the University of Nairobi (Hoffmann et al, 2023). In this study we carried out a bi-monthly monitoring of aflatoxin levels in purchased maize flour from a nationally representative range of sites for one year. Across ten sampling locations including Nairobi, we purchased multiple samples of maize flour from both the formal and informal sector, allowing us to provide contextually relevant risk information to our participants. We find a much higher rate of contamination above the regulatory limit in informally processed flour (25%) relative to maize flour from the formal sector (10%).

We use this data to form the basis of an informational intervention. We randomly assign our study households with equal probability to one of three treatment statuses: control; a relative risk information treatment and an absolute risk information treatment. We carry out a baseline household survey which participants in each of these treatment arms. At the conclusion of the baseline interview, households assigned to the relative risk information treatment receive a script giving some information about aflatoxin risk and a recommendation that purchasing formally processed flour will reduce their potential exposure to this hazard. Households assigned to the absolute risk information treatment receive this information and in addition are informed of the relative probabilities of a formally or informally processed bag of maize flour

being contaminated with aflatoxin. We then follow up with households approximately two months later to repeat the interview and observe what type of maize flour is present in the household.

Our results demonstrate that the intervention was effective in changing the risk perceptions of treated households relative to the control, and that this in turn led to a change in their purchasing behaviour towards safer formally milled flour. Subjective probabilities of contamination risk for informally milled flour increase on both the extensive and intensive margins. At baseline, 17% of households in either treatment report knowing of any potential risk regarding informally milled flour, which increases to 77% at endline. Treated households estimated a 7% probability of a given bag of informal flour being contaminated at baseline, which increases to 48% at endline. At baseline there were no statistically significant differences in the share of households with formally milled flour present at the time of the interview. At endline, treated households are fourteen percentage points more likely to have formal flour in the home than control households, whose share is not significantly changed from baseline.

The study demonstrates the potential for simple informational interventions to mitigate health risks, even in very low resource situations. By making consumers aware of a potential hazard, we were able to induce change in their actual purchasing decisions without relying on any form of monetary or other form of incentive. They were able to do so both because the recommendation they received was closely tailored to their context, and highlighted an available substitute good through which they could lower their health risk.

Our analysis provides several contributions. We add to an existing literature on mitigation of aflatoxin contamination, which to date has largely focused on post-harvest practices (Pretari, Hoffmann and Tian, 2019; Bauchet et al. 2021; Magnan et al., 2021). Our results form part of a growing evidence base on the potential to use information provision to drive consumer demand for safer food, incentivizing producers and vendors to take up safer practices (Daniele et al. 2021; Hoffmann & Kariuki, 2023). Beyond food safety, our analysis contributes to the literature on role of salience in affecting economic behaviour (Bordalo, Gennaioli, & Shleifer, 2022). Importantly, we demonstrate that reducing health risks is an important concern, even for very poor individuals. In combination, informing them of a risk and highlighting a safer alternative allowed them to reduce the risk exposure they and their families faced.

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## Chapitre 1

### **Increasing the adoption of conservation agriculture: A framed field experiment in Northern Ghana**

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#### **Abstract**

Conservation agriculture techniques have the potential to increase agricultural production while decreasing CO<sub>2</sub> emissions, yet adoption in the developing world remains low—in part because many years of continuous adoption may be required to realize gains in production. We conduct a framed field experiment in northern Ghana to study how incentives and peer information may affect adoption. Incentives increase adoption, both while they are available and after withdrawal. There is no overall effect of peer information, but we do find evidence that information about long-term adoption increased adoption, particularly when that information shows that yield gains have been achieved.

## 1.1 Introduction

Climate change is a serious threat to the livelihoods of millions of smallholder farmers in developing countries, particularly in Africa, where farmers are largely dependent on rainfed agriculture and vulnerable to droughts, flooding, and seasonal rainfall pattern disruptions (UNDP, 2017). Smallholder productivity is further threatened by increasing soil degradation, which reduces land productivity over time (UNCCD, 2017). A package of practices called Conservation Agriculture (CA) has been proposed as one solution to the consequences of climate change and soil degradation. Proponents argue that CA combines private benefits to adopters—by increasing yields and reducing vulnerability to rainfall shocks— and public good characteristics, via carbon sequestration in soil and reduced soil runoff into water catchment systems (Hobbs 2007; Bell et al. 2018a).

Despite these claims about CA, adoption of its practices in developing countries remains low (Giller et al. 2009; Michler et al. 2018). One explanation is that while benefits take time to be realized, adoption costs are borne up front: yield gains from improved soil health can take up to ten years to be realized, but adopting CA requires immediate additional investments in the form of labor and/or herbicide application for weeding (Giller et al. 2009). Poor farmers may be unwilling to take on these up-front costs for uncertain future gains. In this paper, we conduct a framed field experiment with farmers in northern Ghana to test two strategies to encourage adoption by reducing this uncertainty: offering time-limited subsidies and providing information on others' adoption decisions.

A key challenge to understanding CA adoption is the long time-horizon required for private benefits to be realized, which makes it difficult to conduct research leading to causal inference. A direct consequence is that there have been few rigorous evaluations of CA adoption. The framed field experiment we implement simulates the decision-making process to understand how participants respond under uncertainty in benefits. While the experiment does

not capture all the real-world aspects of an adoption decision, framed field experiments have been used in a variety of related contexts to test how farmers respond in conditions of risk or uncertainty and to predict actual decision-making (Alpizar, Carlsson & Naranjo 2011; Tjernström et al. 2021).

The experiment is designed to answer two primary research questions. First, we examine whether providing incentives to implement CA practices increases adoption both in the “short-run” (while conditional incentives are available) and the “long-run” (after incentives have been withdrawn). Second, we study whether farmers who receive information on the returns achieved by others in their community who have (or have not) adopted CA practices are as likely to adopt as those who do not receive information. We randomize farmers into an incentive treatment, a cross-randomized peer information treatment, and a control group, and study how the proportion adopting CA practices varies across groups.

In the incentive treatment, we provide a temporary subsidy conditioned on adoption of CA practices. Since private benefits (via increased yields) should persist over time, in theory farmers should continue to use CA practices even after the subsidy is withdrawn. The positive environmental externalities can justify such incentives from a policy perspective, since the incentives can be designed to be welfare-enhancing overall. A related concept has been tested in different settings, by paying people to preserve land endowments that provide ecological benefits (e.g. Jayachandran et al. 2017; Alix-Garcia et al. 2018).<sup>2</sup> Therefore, the primary contribution of this paper is to provide causal evidence on the potential for incentives to increase CA adoption, using the framed field experiment to study the dynamics of incentivized adoption over many “seasons.”

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<sup>2</sup> Jayachandran et al. (2017) implement a randomized evaluation of a program in Uganda that gave households payments contingent on maintaining tree cover on their land. They found that payments were successful in reducing deforestation and that the environmental benefits compensated for the program cost. Alix-Garcia et al. (2018) use a regression discontinuity design to study the impacts of payments for participation in a land management program in Mexico. They find that payments improve land management activities and community social capital.



Our second treatment explores an alternative strategy to address uncertainty, by providing information about the experience of peers to farmers. By randomizing information in the context of our framed field experiment we can study how learning about different types of peer behavior can affect farmer choices, and how social learning might affect adoption in the context of a technology requiring a long time-horizon to be profitable. The role of individual learning is particularly important in the adoption of agricultural technologies, as observing peers can reveal information both about the profitability of a technology and information on management practices (Foster & Rosenzweig, 2010). Conley and Udry (2010) find evidence of both processes in studying pineapple adoption among farmers in Ghana: farmers adjust their own input use after observing unexpected profits (or losses) from a neighbor’s previous input allocation. Evidence suggests that peers can be just as, if not more, influential than community leaders and extension workers (Krishnan and Patnam, 2013; BenYishay and Mobarak 2019; Ambler, Godlonton and Recalde 2021). Of particular relevance to the technology we consider, Crane-Droesch (2018) conducts an experiment on the diffusion of information on a soil amendment technology in Kenya and finds observed variability in peer outcomes has a strong negative effect on adoption. Also related, Bell et al. (2018a) find an association between peer effects and adoption in Malawi, though they do not disentangle the way different types of information are related to adoption.<sup>3</sup>

Our results provide support for the potential of time-limited incentive payments to cause farmers to adopt CA practices in the long run. Across specifications we find a positive and statistically significant effect of assignment to the incentive treatment on the extent of CA

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<sup>3</sup> Other relevant studies that examine information dissemination through peer networks and agricultural technology adoption include Beaman et al. (2021) who find that targeting “central” farmers increases adoption of pit planting in Malawi, Kondylis et al. (2017) who find minimal impacts of peer farmers on adoption of pit planting in Mozambique, Bandiera and Rasul (2006) who find an increase in adoption of a new crop as more individuals in the person’s network adopt, and Carter, Laajaj, and Yang (2021) who find large impacts on adoption of improved seeds and fertilizer with the addition of contacts who have received past subsidies for these items.

adoption across a range of specifications. Treated participants are more likely to adopt the CA practice, maintain adoption until they achieve the private returns to choosing CA, and are less likely to return to conventional practices after choosing CA. For the information treatment, we find being told that a peer has successfully adopted CA over the long term increases adoption, but do not find effects for other types of information, or for receiving information in general.

## **1.2 Background**

Before describing the experiment design, we provide a brief description of CA and its current status in northern Ghana. CA is defined by three principles: minimal soil disturbance, permanent soil cover, and crop rotation (FAO, 2007). Minimal soil disturbance is the replacement of traditional ploughing with direct seeding to reduce the effects of planting on the soil structure. Permanent soil cover involves leaving residues from the previous crop on plots, combined with the planting of cover crops during fallow periods. Crop rotation is the practice of planting different crops in sequential seasons to diversify nutrients available to micro-organisms and create variation in the soil depth in which roots are established.

The purpose of CA practices is to increase soil organic matter to improve water and nutrient retention, which in turn allows farmers to sustainably intensify production while mitigating negative environmental impacts. While CA practices have been widely adopted in some developed economies (e.g. USDA, 2019), adoption is relatively low in low- and middle-income countries. The CA adoption literature does not yield much information about why farmers do not adopt; literature reviews find it to be highly context specific (Knowler & Bradshaw, 2007) and methodologically weak, often relying on observational data from projects aimed at promoting CA (Andersson & D'Souza, 2014). While several studies have used hypothetical choice experiments (in which decisions are not incentivized) to explore farmers' stated preferences for both financial and non-financial incentives to adopt (e.g. Marenja, Smith and Nkonya 2014; Ward et al. 2016; Schaafasma, Ferrini and Turner 2019), there is limited

evidence which exploits exogenous variation to test how farmers respond to actual incentives. One exception is work by Bell et al. (2018b, 2018c) which uses randomized assignment to test the effects of incentives on CA adoption in Malawi, and finds an initial increase in adoption after the first season of the project. This study shows that incentives can induce on-farm adoption of CA techniques.

In northern Ghana, about 80 percent of land is under customary tenure (Bugri & Yeboah, 2017). In practice, households have long-standing land use rights, which are patrilineally inherited. In the sample of farmers surveyed for the framed field experiment, households both had firm use rights and cultivated 81.2 percent of plots enumerated, with the second most common type of plot being communally owned. Other plots were either rented-in or sharecropped. Therefore, for most plots, households should reap longer term benefits from any investments they might make in soil fertility.<sup>4</sup> Information on respondent and household characteristics, and comparisons by treatment status, can be found in Appendix Table 1.7.

## **1.3 Experimental Design**

### **1.3.1 Conceptual framework**

In this section we describe a conceptual framework for the adoption of CA techniques that allows for interpretation and understanding of our experimental design.<sup>5</sup> We begin by assuming, consistent with the agronomic literature, that if a farmer consistently applies CA techniques their land will eventually become more productive; e.g. the expected yield will increase for the primary crop grown on that land. We further assume that input costs are higher for implementing conservation agriculture, because required labor inputs increase (Giller et al., 2009). Additionally, it is not clear how long it will take for the yield increase to occur. From a

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<sup>4</sup> However, farmers generally lack credit access; according to the Ghana Statistical Service (2019), at the regional level only 5.8 to 11.6 percent of respondents within the four regions in the study had applied for credit.

<sup>5</sup> While the experimental design focuses on minimum soil disturbance for simplicity, this framework speaks to CA techniques more broadly.

conceptual perspective, the uncertainty about the timing of the yield increase implies a risk averse farmer or one with a higher discount rate would be less likely to adopt. We further assume that using CA techniques leads to higher land productivity when weather is poor than traditional techniques, as soil begins to have better water absorption and retention properties. Finally, since farmers also have not necessarily used CA techniques (though they may have learned about them through extension workers), we assume there is some additional risk to adopting them.<sup>6</sup> As a result, a risk averse farmer would require a premium over simple expectations over profits to adopt CA.

We propose that there are two ways to overcome an individual farmer's aversion to adopting CA techniques. First, an external entity could provide incentive payments to farmers for a fixed period of time (e.g. number of years) after adoption to partially compensate the farmer for extra labor inputs and the risk premium required for the farmer to adopt.<sup>7</sup> This compensation need not last until the yield benefit occurred, but it would need to last long enough for the net present value of continued adoption to exceed that of abandoning CA. Second, farmers could learn more about CA techniques by observing other local farmers. If, for example, one observed a neighboring farmer using CA techniques with much higher yields in a bad year, non-adopters could be induced to adopt or try those techniques. These observations would reduce the risk of adopting CA practices by providing additional, credible information.

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<sup>6</sup> In northern Ghana, there have been past several CA projects, increasing the likelihood they knew of or had tried specific CA techniques. These projects included Sasakawa Global 2000 (Ito et al., 2007) which was active from 1986-2003 and promoted no-till farming and not burning crop residues. Other notable activities include the Savannah Resources Management Project implemented by the Ministry of Lands and Forestry (Boahen et al., 2007); work by the Center for No-Till Agriculture which is sponsored by the Howard Buffett Foundation and provides training to farmers on CA techniques; and the World Bank's Sustainable Land and Water Management Practice Project (SLWMP) which was active around the Kulpawn-Sissili and Red Volta watersheds.

<sup>7</sup> Implicit in this argument is the idea that CA adoption creates a public good. There are at least two arguments that it could do so. First, CA adoption should reduce greenhouse gas emissions by the agricultural sector, both through less burning crop residue and by lowering the need for fertilizer use. Second, conservation agriculture leads to improved soil water retention, which could reduce runoff in at least some neighbors' fields, particularly those that are downhill.

### 1.3.2 Experimental procedure

The framed field experiment was designed to represent these key features of CA practices over a medium to long time horizon.<sup>8</sup> Participants are asked to decide whether to adopt a single CA practice: minimal soil disturbance (MSD). While CA practices should be adopted jointly to realize maximum benefits, we focus on a single practice for experimental simplicity. MSD was selected because focus groups in the area suggested it was the CA practice with which participants had the least experience. Participants are asked to make an adoption decision in each of ten rounds, with each round modeled as an agricultural season.<sup>9</sup> Prior to the first round, the participant receives a monetary endowment for use in the activity. Each round then proceeds as follows:

- 1) The participant chooses one of two technologies to adopt for that round, either MSD or conventional practices (CP).
- 2) They pay a fixed price associated with that choice from their current endowment.<sup>10</sup> In the experiment, the price represents the cost of weeding associated with the chosen technology, and these costs are higher with MSD than with CP.
- 3) The enumerator reveals the rainfall for that season. Rainfall is determined randomly and is poor with 1/3 probability or normal with 2/3 probability.<sup>11</sup>

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<sup>8</sup> The scripts used in the experiment are included in Appendix A.

<sup>9</sup> The specific choice of ten seasons is used to present a reasonable representation of the timeframe required. Giller et al. (2009) suggest ten years as an upper bound for the realization of yield gains. We elected to simulate ten seasons with gains occurring within 5-7 seasons to observe behaviour after their (potential) realization, while limiting the time commitment required of participants. Participants knew in advance of any decisions that the total length of the experiment would be ten rounds.

<sup>10</sup> The experiment was structured such that the participant always had sufficient funds to choose either practice, independent of the outcome of prior rounds.

<sup>11</sup> To ensure consistency, all randomization was conducted in Stata prior to fieldwork and loaded into the software used for implementation. Enumerators could not change any randomized parameters as they were associated with a unique subject identifier.

- 4) The participant receives a payment based on their choice of technology and the rainfall realization. This payment represents the value of their harvest for that season. Payments are always higher with normal rainfall than with poor rainfall.

Before the beginning of each round, the participant was shown a choice sheet, which showed the two available choices, the price associated with each choice, and the two potential payoffs associated with each choice (four total). The choice options and associated prices were fixed throughout. The probability of each rainfall outcome was fixed and independent across rounds. The payments associated with each outcome vary by round based on if participants were assigned to the incentive treatment (described below) and on their adoption choices in the current and preceding rounds.<sup>12</sup> The choice sheets are shown in Appendix B.

For participants who chose MSD, if the choice was made continuously over multiple rounds, the available payments associated with that technology would increase once and remain at that higher level so long as they continued to adopt. This feature was intended to reflect the property that private benefits from CA adoption are realized over a medium to long time horizon, the length of which is not known by farmers *ex ante*. To model the uncertainty farmers face over when gains from CA adoption might occur, participants were randomly assigned with equal probability to receive the production increase with 5, 6, or 7 rounds of continuous adoption.<sup>13</sup> Participants were told in the script the value of the yield gain, and that abandoning CA would ‘reset’ the number of rounds of MSD adoption required. Hence the only two aspects of the experiment which were unknown to the farmer (and the enumerator) prior to realization were the weather outcome for each round, and the exact round in which the yield gain would occur.

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<sup>12</sup> Oliva et al. (2020) also study incentives for adoption of a technology with delayed payout under uncertainty, finding that offering incentives leads to increased adoption by people who are less likely to follow through.

<sup>13</sup> Note that adoption only needed to be continuous- a farmer could for example choose CP in the first round and still achieve the yield gain if they chose MSD for 5-7 consecutive rounds thereafter.

Both prices and costs were represented in pesewas, which are the sub-unit of the Ghanaian cedi. Images of coins and notes were used on visual aids showing payoffs, so participants could easily recognize the amounts involved. To prevent potential adverse issues during the experiment, play money with the same appearance as local currency was used and exchanged for real money following the conclusion of the final round.

As described in the script (Appendix A) enumerators provided a full explanation of the procedures for the experiment and conducted a practice round with the participant. During this explanation, participants were asked a series of questions to check that they understood the explanations being provided. Enumerators recorded their first response to each question and provided additional explanations if the participant misunderstood something. Overall, participants had a good understanding of the features of the experiment (Appendix Table 1.8).<sup>14</sup>

### **1.3.3 Incentive treatment**

The incentives treatment was designed to represent a subsidy payment to farmers adopting MSD. Participants were randomized into a group receiving incentives and a control group. Randomization was done at the individual level, stratified by farmer-based organization (FBO) and information treatment status.<sup>15</sup> The probability of being assigned to the incentives treatment was 2/3, with 1/3 assigned to the control. The reason for treating a larger portion was to ensure sufficient variation among treated individuals in the number of consecutive rounds required to achieve increased production.

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<sup>14</sup> Limiting our sample to participants who answered all the comprehension questions correctly first time does not affect our main results (Appendix Table 1.9).

<sup>15</sup> Our pre-analysis plan (<https://www.socialscisearch.org/trials/3973>) anticipated stratifying by gender. Unfortunately, accurate administrative data including participant gender was not available prior to the start of fieldwork, hence the stratification was not implemented. We test for heterogeneity by gender in Appendix Table 1.10. Since groups were not of uniform size, individuals did not always evenly divide into treatment groups within a stratum (i.e. a group of twenty people cannot be divided into thirds). For the additional 'misfit' observations we randomly allocate individuals independently across strata, using the procedure and associated *randtreat* command described in Carril (2017).

If assigned to receive incentives, the participant was eligible to receive an additional payment conditional on choosing MSD in any of the first four rounds of the experiment, which they received immediately after making their choice in each round. The amount of the incentive was fixed, and no incentives were available after the fourth round. The incentive was not conditioned on decisions in any previous round, so a treated individual choosing CP in Rounds 1-3 would still be able to receive a payment if they chose MSD in Round 4.

### **1.3.4 Peer information treatment**

Participants were also cross-randomized with equal probability into either a group assigned to receive information about a generic peer farmer or a control group.<sup>16</sup> Participants assigned to the information treatment were read a short vignette about a unnamed, hypothetical peer farmer before making their decision during the first four rounds of the experiment. Vignettes were used to exogenously vary information received by the participant. The four texts used were as follows:

- Last year they used conventional practices on their plots, they have always used conventional practices.
- Last year they used minimal soil disturbance on their plots. They had not used this technique before.
- Last year they used minimal soil disturbance on their plots. They have been using minimal soil disturbance for the last ten years.
- Last year they used conventional practices on their plots. They had used minimal soil disturbance before but decided to go back to conventional practices.

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<sup>16</sup> Assignment followed the same procedure as for the incentives treatment, but the treatment and control groups were of equal size.



Along with each vignette, they received information on the peer farmer earnings, which were calculated in the same way as for the participant, based on the realization of the rainfall variable in the previous round.<sup>17</sup> As a result there were eight possible variations of the information provided. The vignettes are representative of all possible payoffs and adoption histories (i.e. never adopted, early adoption, achieved yield gain, dis-adoption). For a given prior rainfall outcome, each variation was chosen via an independent random draw. An individual could receive the same vignette in different rounds, and the assignment for a given round did not affect the probability of assignment in other rounds.

### 1.3.5 Payoffs

The payoffs were calibrated to model the features of CA technologies, scaled to a reasonable budget for the project. Participants were paid a fixed fee of 5 cedis (0.93 USD) which was approximately the local wage for a day of agricultural labor at the time of the experiment, and could earn an additional 3-10 cedis over the course of the experiment.<sup>18</sup> The total payout therefore ranged from 8-15 cedis (1.49-2.80 USD). The mean payout for the experiment was 12.6 cedis (2.36 USD). Table 1.1 presents the available payouts in the experiment for CP and MSD.

*Table 1.1- Payoff amounts per round, by practice choice & scenario*

Technology		CP	MSD	MSD	MSD
Incentive treatment?		-	No	Yes	No
Production increase		-	No	No	Yes
Scenario (Choice Sheet)		A/B/C	A	B	C
Possible rounds		1-10	1-10	1-4	5-10
Normal rainfall	A. Price of choice	10	30	30	30
	B. Incentive payment	0	0	20	0
	C. Production payment	100	100	100	120
	Net payoff (C+B-A)	90	70	90	90
Poor rainfall	A. Price of choice	10	30	30	30

<sup>17</sup> For Round 1, participants in the information treatment were randomly assigned a rainfall outcome for the (hypothetical) preceding season.

<sup>18</sup> Participants started with an initial endowment of 1 cedi, and could earn 0.2-0.9 cedis per round.

B. Incentive payment	0	0	20	0
C. Production payment	30	50	50	60
Net payoff (C+B-A)	20	20	40	30
Expected value	66.7	53.3	70	73.3

Notes: Amounts shown are in pesewas, which are a division of the Ghanaian cedi. 100 pesewas = 1 cedi (approximately \$0.19 USD at current market rates).

Since the values for CP are fixed, there are three possible comparisons: CP vs. MSD without incentives; CP vs. MSD with incentives; and CP vs. MSD without incentives but with a yield gain realized.<sup>19</sup> Weeding costs are held constant throughout the experiment, and cost 10 pesewas for CP and 30 for MSD.<sup>20</sup> The incentive payment is 20 pesewas in the rounds in which it is offered, covering the difference between the cost of implementing CP and MSD. Production payments vary by rainfall and whether the yield gain has been achieved. Initially, in normal years, CP and MSD both pay 100 pesewa (not considering the weeding costs or incentives). After the yield gain has been achieved, the payment for MSD increases to 120 pesewas in normal years. In poor years, MSD always pays more, reflecting CA's resilience properties. Prior to the yield gain, in poor rainfall years CP pays 30 pesewas and MSD pays 50. After the gain, MSD pays 60 pesewas in poor years.

Comparing these three scenarios we can observe some straightforward features of the experiment: for a given round without incentives a risk-neutral participant will strictly prefer CP, since the payouts are higher than MSD under a normal rainfall outcome and equal to MSD under poor rainfall. With incentives, the reverse is true: MSD has equal returns under normal rainfall and better returns under poor rainfall.

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<sup>19</sup> Note that since the incentives were only available in Rounds 1-4, and the yield gain took at least 5 rounds to be realized, there is no scenario in which the participant could receive the incentives and the gain in the same round.

<sup>20</sup> While CA adoption could require a range of additional costs in terms of farm labor (including changes in land preparation and time required to do mulching) it was clear from discussions with participants in piloting that the largest and most salient cost for farmers was weeding. We therefore chose weeding to represent the increase in input costs associated with CA which in actual implementation would occur across a range of activities.

Combining payouts across rounds, the expected value of choosing CP across all rounds is  $66.7 \times 10 = 667$  pesewas. Without incentives, the earliest stage at which the yield gain could be achieved is Round 5. For this case, the highest possible expected value of always choosing MSD is therefore the expected value of MSD from scenario (A) for four rounds, plus the expected value of MSD from scenario (B) for six rounds. Hence:  $53.3 \times 4 + 70 \times 6 = 633.2$  pesewas. As a result, a risk neutral participant always chooses CP over MSD without incentives.

With incentives, the situation is reversed. For a participant always choosing MSD, the latest round in which the yield gain can be realized is Round 7. Therefore, the lowest expected payoff from choosing MSD with incentives will be the total of the expected value from scenario (B) (Rounds 1-4), plus the expected value from scenario (A) (Rounds 5-6), plus the expected value from scenario (C) (Rounds 7-10):  $70 \times 4 + 53.3 \times 2 + 73.3 \times 4 = 679.8$ . Hence the lowest possible expected payoff for continuous MSD adoption in the incentive scenario exceeds the expected payoff for continuously choosing CP.

This parameterization implies that for risk neutral individuals, it is preferable for individuals to choose CP when in the control group. However, if individuals are risk averse, preferences depend upon their degree of risk aversion. In other words, for some individuals it becomes preferable to select MSD over CP during all ten rounds. If we consider the constant relative risk aversion utility function, a risk averse individual who expected the MSD bonus to occur in round 6 would be neutral between choosing CP and MSD for a risk aversion coefficient of approximately 0.687.<sup>21</sup>

### 1.3.6 Limitations

The goal of the framed field experiment is to model real-life adoption decisions over the long time-horizon needed for the benefits of CA to be fully realized. There are four principal ways

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<sup>21</sup> Assuming a constant relative risk aversion function of the form  $U(C) = C^{1-\theta} / (1 - \theta)$  when  $\theta \neq 1$  and  $U(C) = \ln(C)$  if  $\theta = 1$ . MSD is preferable under this utility function for individuals with values of  $q$  between 0.687 and 1.

in which our experiment must necessarily deviate from the parameters of real-life CA adoption decisions. The first is the role of time discounting. When considering payoffs from land preparation decisions that may materialize over ten years, farmers will discount that income differently than payouts to be made over the course of a ninety-minute experiment. Specifically, we may expect farmers to be more present-biased in their actual decisions, and this will be a key additional element to explore in future research. Second, although not trivial for participants, the stakes in the experiment are much lower than those around actual planting decisions for a primary crop. This may affect their decisions, in particular their willingness to take risks.<sup>22</sup>

Third, we assume farmers do not face credit constraints in the experiment: they always have sufficient capital to choose either practice, and neither choice affects their ability to make other investments. Fourth, the income earned in the experiment is a windfall, whereas real-life planting decisions are made with regular income, and evidence has shown that windfall and regular income are often spent in different ways (Arkes et al. 1994; Milkman and Beshears 2009).

Fourth, participants' choices may have been influenced by the experimenter demand effects, particularly if they believed that providing the "right" answer might influence the likelihood that they would receive additional services in future. These effects may account for the relatively high level of CA adoption in the control, though the level effect should not bias our estimation of treatment effects. We are further able to test for heterogeneity in results among those who had or had not recently received training which may have encompassed CA principles and do not find differences in the effect of incentives between farmers who had or had not been sensitized (Appendix Table 1.13).

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<sup>22</sup> We test for heterogeneity based on self-reported measures of risk and time preferences (Appendix Tables 1.11 and 1.12) and do not find that our effects are driven by more risk-averse or impatient participants.

## 1.4 Data and estimation

### 1.4.1 Sample

This project was conducted in partnership with the Ghana Agricultural Sector Investment Programme (GASIP), a national initiative which aims to support the development of agricultural value chains within Ghana. As part of its activities, GASIP promotes CA principles as well as increased access to improved inputs such as certified seed and machinery. We obtained a list of 66 FBOs created by GASIP for their activities in four northern regions of Ghana.<sup>23</sup> Field staff visited each group in the second quarter of 2019 and obtained a listing of all current members. The FBOs joined GASIP in waves, with some groups joining in 2018 and others in 2019. The 2018 FBOs had exposure to one year of GASIP extension information (including CA and other techniques) at the time of the experiment, while implementation had not yet begun for the 2019 FBOs. The 66 FBOs enrolled were the universe of all FBOs exposed to GASIP CA activities in the north in 2018 or 2019.

The sample was composed of current FBO members: 1,328 individuals across 66 FBOs.<sup>24</sup> Each member was visited to confirm their sample status and conduct a household survey, with a separate team of enumerators returning a few days later to conduct the experiment. If the listed individual was not available within one week of the scheduled household interview a replacement was used. Replacements were required to be adults within the same household who were also involved in farming. Overall, 1,324 individuals were interviewed, of whom 38 were replacements.<sup>25</sup> Field work was conducted from April to June 2019.

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<sup>23</sup> These are Northern, Upper East, Upper West, and Brong Ahafo. Farmer groups are located in twelve districts within these regions. Note that the groups comprise speakers of six languages, spread over a large geographical area, suggesting that the potential for spillovers was limited.

<sup>24</sup> There are 30 2018 FBOs and 36 2019 FBOs. The average group size was 20 members. One FBO was substantially larger than the others, with 37 members. For this group we randomly sampled 20 members.

<sup>25</sup> There were cases where participants were members of the same household, so the total household survey sample is 1,117. For some cases, the field team was unable to match household data to individuals, as a result there are 25 experiment

Table 1.2 presents the share of respondents reporting knowledge and use of CA practices (MSD, cover-cropping, applying crop residues, not burning, and crop rotation). In general, most farmers are familiar with CA techniques, with the share somewhat higher in farmer groups which were targeted by GASIP in 2018, compared to 2019 FBOs.<sup>26</sup> For most practices, fewer than half of participants report applying them in the most recent agricultural season. The exceptions to this were the related practices of using residues for soil cover, and not burning residues.<sup>27</sup> Overall, individuals in the sample can be said to be somewhat sensitized to CA techniques, though very few people have adopted all of them.

*Table 1.2- Adoption and knowledge of CA techniques, by timing of FBO entry to GASIP*

	Old FBOs	New FBOs
<i>Heard of...</i>		
Conservation agriculture	0.95	0.79
Minimal soil disturbance	0.78	0.55
Cover cropping	0.79	0.54
Using residues	0.93	0.81
No burning	0.97	0.84
Crop rotation	0.84	0.73
<i>Adopted last season...</i>		
Conservation agriculture	0.89	0.83
Minimal soil disturbance	0.33	0.20
Cover cropping	0.36	0.27
Using residues	0.70	0.61
No burning	0.81	0.70
Crop rotation	0.48	0.41

Notes: Columns show mean proportion of baseline survey respondents responding “Yes” for each category. For “Heard of” the overall “Conservation Agriculture” category was asked separately from the sub-categories. For “Adopted” the overall CA proportion is an indicator for responding “Yes” to one or more sub-categories.

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participants for whom we do not have a full set of controls for regression specifications. We retain these individuals and include indicator variables for the relevant missing data. Excluding these individuals does not substantively affect results.

<sup>26</sup> The main results do not vary by 2018 and 2019 FBOs (Appendix Table 1.13).

<sup>27</sup> The use of fire to remove residues is actively discouraged by the government of Ghana. As a result, this measure may be over-reported.

### 1.4.2 Empirical strategy

To evaluate the impacts of the respective treatments on adoption of MSD in the experiment, we estimate three primary specifications using ordinary least squares at the participant level, following our pre-analysis plan. To address multiple hypothesis testing, we control for the false discovery rate (FDR) by calculating sharpened q-values (Benjamini, Krieger and Yekutieli 2006; Anderson 2008).<sup>28</sup> Our first specification is as follows:

$$Y_{ij} = \alpha + \beta_1 \text{Incentive}_{ij} + \beta_2 \text{Information}_{ij} + \beta_3 R6_{ij} + \beta_4 R7_{ij} + \gamma X_{ij} + \delta_j + \varepsilon_{ij} \quad (1)$$

where  $Y$  is one of three outcome variables (the number of rounds in which MSD was adopted; a binary indicator for whether the yield gain was realized; and a binary indicator for whether the respondent ever stopped choosing MSD after adopting). *Incentive* and *Information* are indicator variables for the respective treatments, and *R6* and *R7* are indicators for being in the groups that could realize the yield gain after choosing MSD for 6 and 7 consecutive seasons respectively (with 5 seasons as the omitted category).<sup>29</sup>  $X$  is a vector of control variables,  $\delta$  represents stratification cell fixed effects (farmer group dummies, with  $j$  denoting group membership), and  $\varepsilon$  is an error term robust to heteroskedasticity, computed using the HC3 method (Davidson & MacKinnon, 1993).<sup>30</sup> Since the experiment is individually randomized and we cover the universe of FBOs formed before the end of 2019 for the CA component of GASIP, there is no need to cluster standard errors (Abadie, Athey, Imbens, & Wooldridge, 2023). This specification differs from that listed in the pre-analysis plan only in that we initially

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<sup>28</sup> The FDR accounts for the percentage of false positives among rejected null hypotheses. The sharpened q-value is the expected proportion of false positive within a family of outcomes if the coefficient in question is assumed to be significant. All main results are robust to using sharpened q-values.

<sup>29</sup> Due to some enumerator errors (as a result of conducting an experiment using an incorrect ID on the tablet computer) there are a small number of cases (2 observations for the information treatment, 3 for the incentive & gain round assignments) where the implemented treatment did not match the assignment for the sample. We use the assigned status throughout, but the results of the analysis are not meaningfully altered by using actual assignment.

<sup>30</sup> Control variables include: household size, gender, age, risk and time preferences, value of assets owned, number of CA techniques used last season, value of crop production, number of GASIP crops grown, household has electric light, household has toilet access, household has cement walls, household has cement floors, household has metal roof, household grew tubers, the rainfall assigned in the practice round, and indicators for missing data.

indicated that we would show treatments in separate specifications. Because the treatments are orthogonal, including them in the same regression does not change the results. We therefore present results in a combined regression for simplicity of presentation.

We then estimate the same specification adding an interaction term:

$$Y_{ij} = \alpha + \beta_1 Incentive_{ij} + \beta_2 Information_{ij} + \beta_3 Incentive \times Information_{ij} + \beta_4 R6s_{ij} + \beta_5 R7s_{ij} + \gamma X_i + \delta_j + \epsilon_{ij} \quad (2)$$

To analyze the impact of the type of information received in the information treatment, among those who received the information treatment we estimate an alternative regression. This regression is estimated at the participant-round level for the first four rounds, and includes interactions with the rainfall realization in the previous round as it is referenced in the information:

$$Y_{ijr} = \alpha + \beta_1 InfoB_{ijr} + \beta_2 InfoC_{ijr} + \beta_3 InfoD_{ijr} + \beta_4 PoorRainfall_{ijr} + \beta_5 InfoB_{ijr} \times PoorRainfall_{ijr} + \beta_6 InfoC_{ijr} \times PoorRainfall_{ijr} + \beta_7 InfoD_{ijr} \times PoorRainfall_{ijr} + \gamma X_{ijr} + \delta_j + \rho_r + \mu_{ijr} \quad (3)$$

Here the outcome variable  $Y_{ir}$  is an indicator variable which takes the value 1 if participant  $i$  chose MSD in round  $r$ , and 0 if they chose CP. We include a round fixed effect  $\rho$ , and indicator variables representing the information received for a given round:

*InfoA*: Neighbor used CP (which is the omitted category);

*InfoB*: Neighbor used MSD for the first time;

*InfoC*: Neighbor used MSD for the last ten years; and

*InfoD*: Neighbor abandoned MSD (used CP after having used MSD).

*PoorRainfall* is equal to one if the rainfall in the previous season (i.e. the rainfall experienced by the neighbor/peer in the reported information) was poor.



This specification did not include the interaction terms in the pre-analysis plan, and we include both in the results table, but they are included here to help explain whether certain types of information are effective only when observing a positive or negative peer outcome.

### 1.5 Results

Table 1.3 shows the mean of each of the three main participant-level outcomes for each of the two randomized treatments as well as the randomized yield gain round.

*Table 1.3- Average Outcomes in the Experiment, by Treatment and Gain Round*

	Incentive treatment		Information treatment		Gain round		
	No incentives	Incentives	No	Yes	Round 5	Round 6	Round 7
No. rounds							
Chosen	7.83	8.41	8.25	8.18	8.42	8.09	8.14
Achieved gain	0.68	0.75	0.73	0.73	0.78	0.72	0.69
Abandoned							
MSD	0.34	0.27	0.29	0.29	0.26	0.30	0.31

Note: Columns represent the mean for each group.

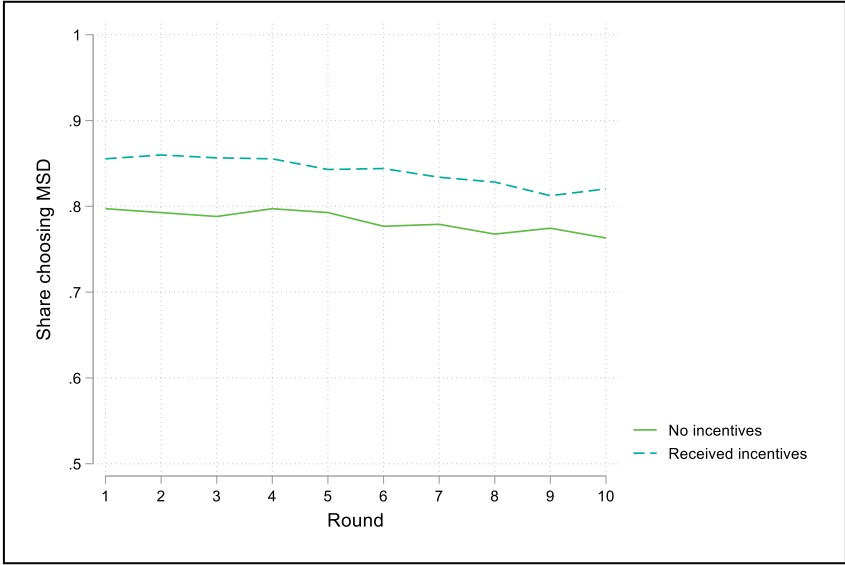
The share choosing MSD is high overall, with individuals in the no incentive group choosing it 7.83 rounds out of ten on average, but higher in the incentive group, chosen 8.4 times out of ten on average. 68 percent of non-incentivized individuals realized the yield gain, compared to 78 percent of incentivized individuals. Those in the incentives group were also less likely to abandon MSD after they had chosen it: 27 percent of them did so, relative to 34 percent of non-incentivized individuals. Conversely, there is no evidence of a difference in behavior by information treatment. The means by gain round group do suggest that the earlier the gain is achieved, the more likely it is the participant reaches that point. For example, 78

percent of those who received the gain after the fourth round achieved it, compared to 69 percent among those receiving it after round 6.<sup>31</sup>

We also examine how behavior may have changed over the course of the experiment.

Figure 1.1 shows adoption by round and incentive treatment status.

*Figure 1.1 – Share choosing MSD, by incentive treatment status*



Across rounds, adoption rates for the incentive groups are always higher than those in the no incentives group, and this difference is consistent over time. In both groups, adoption is steady across the first four rounds, and then declines slightly. Note that the level of adoption in the control group is higher than the actual level reported by farmers in FBOs which had been previously sensitized to MSD (33%). As described in the Limitations section, this finding may reflect an experimenter demand effect or local constraints (lack of availability of seed drills or potential gaps in implementation knowledge) which are not accounted for in our experiment.

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<sup>31</sup> We can also describe behaviour in the experiment in other ways. For example, 66.8 (57.4) percent of incentivized (non-incentivized) participants choose MSD in every round. 5.4 (6.2) percent choose CP in every round. It is uncommon for participants to switch once between technologies: 1.2 (2.7) percent of participants switch once from CP to MSD and 3.2 (1.6) percent of participants switch once from MSD to CP. Multiple switches are more common, 23.4 (32.1) percent of participants make multiple switches over the course of the experiment.

In either case, these should all be equal across treatment groups and thus do not threaten the internal validity of the experiment.

Figure 1.2 shows the same information separately by information treatment. The same time trend is visible, but there is little to no difference in average choices between the two treatment groups. Figure 1.3 shows adoption by round separately by gain round group assignment. We do not observe divergence among adoption rates by gain round assignment, though there is some variation in the initial share of participants choosing MSD.

*Figure 1.2 – Share choosing MSD, by information treatment status*

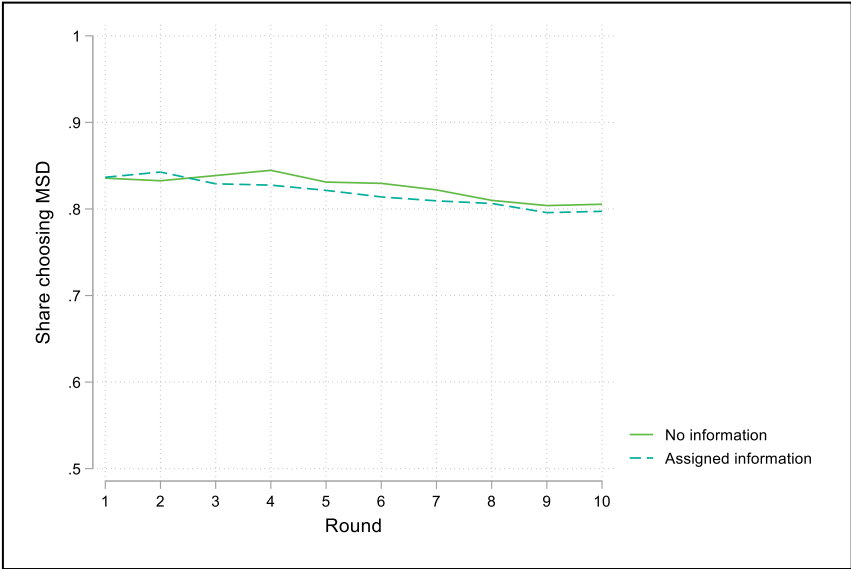
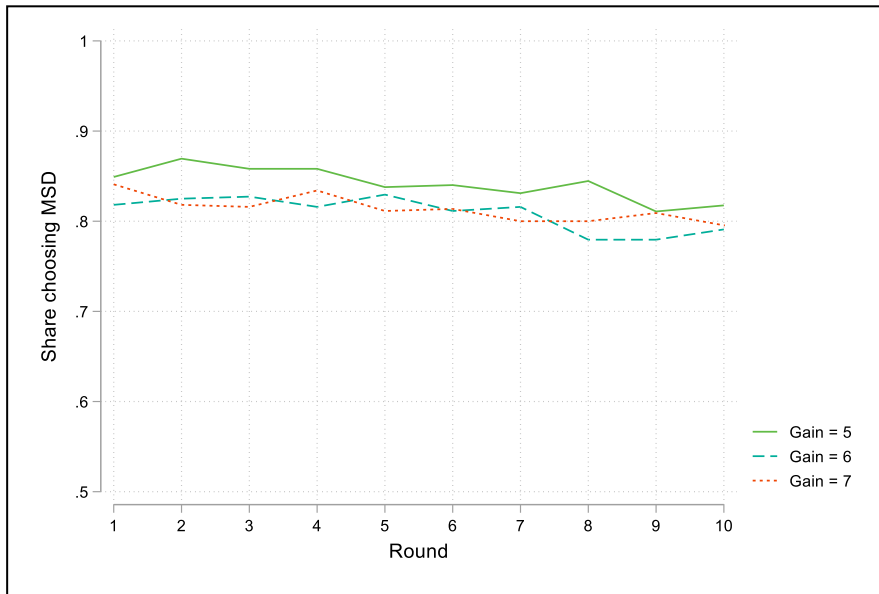


Figure 1.3 – Share of Participants choosing MSD, by gain round



We next turn to the main regression analysis, beginning with the participant-level analysis. Table 1.4 shows the results of estimating equation (1), including all three treatment randomizations.

Table 1.4- Average Impact of Incentive and Information Treatments on MSD adoption

	(1) Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Incentive treatment	0.598***	0.083***	-0.074***
<i>Standard error</i>	(0.182)	(0.026)	(0.027)
<i>p-value</i>	0.001	0.002	0.007
<i>Sharpened q-value</i>	0.003	0.003	0.003
Information treatment	-0.049	0.004	-0.009
<i>Standard error</i>	(0.162)	(0.024)	(0.025)
<i>p-value</i>	0.763	0.856	0.713
<i>Sharpened q-value</i>	1.000	1.000	1.000
Gain round: 6	-0.222	-0.038	0.022
<i>Standard error</i>	(0.199)	(0.029)	(0.030)
<i>p-value</i>	0.265	0.191	0.476
<i>Sharpened q-value</i>	0.661	0.661	0.661
Gain round: 7	-0.275	-0.090***	0.044
<i>Standard error</i>	(0.192)	(0.029)	(0.030)
<i>p-value</i>	0.153	0.002	0.147
<i>Sharpened q-value</i>	0.114	0.006	0.114
Mean: No incentives	7.829	0.677	0.337
Mean: No information	8.253	0.727	0.293
Mean: Gain Round = 5	8.417	0.777	0.261
Adjusted R-squared	0.119	0.114	0.083
Observations	1324	1324	1324

Notes: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Heteroskedasticity robust (HC3) standard errors are reported in parentheses. The following control variables are included, but not reported: household size; respondent is female; self-reported risk preference; self-reported time preference; value of all household assets; number of CA practices reported last season; estimated value of all crops last season; number of GASIP-promoted crops grown; has electric light; has toilet; has cement walls; has cement floors; has metal roof; grew tubers; has any missing crop value data; has any missing individual data; has any missing risk or time preference data; received poor rainfall in practice round. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.

The incentive treatment results in an economically and statistically significant impact on adoption. Participants in the incentive group choose MSD on average in 0.6 more rounds, an increase of 7.6 percent relative to the control group. They were 8.3 percentage points more

likely to achieve the yield gain (12 percent increase) and were 7.4 percentage points less likely to abandon MSD once they had chosen it (22 percent decrease).<sup>32</sup>

There is some evidence that those who received the gain after rounds 6 or 7 adopt MSD less overall, but the coefficient estimates are not typically statistically significant. The one significant outcome is that participants who receive the gain round in round 7 are 9 percentage points less likely to achieve the gain than those who receive it in round 5.<sup>33</sup> The estimates of the impact of the information treatment are close to zero in this specification.<sup>34</sup>

Table 1.5 reports the estimation of regression specification (2) and examines the impact of interacting the incentives and information treatments. The results remain suggestive that the information treatment did not have an impact and are not indicative of any complementary effects between the two. The coefficient estimates for the information treatment alone have the expected signs for each dependent variable, but are not statistically significantly different from zero. Similarly, estimated coefficients on the interaction term are not statistically across the three dependent variables.

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<sup>32</sup> Following our pre-analysis plan, Appendix Tables 1.10-1.12 test for heterogeneity by respondent gender, self-reported risk aversion, and self-reported time preferences. Appendix Table 1.13 tests for heterogeneity by wave in which the community was enrolled in the GASIP program. We do not find that the effects of treatments varied significantly for any of these groupings.

<sup>33</sup> In Appendix Table 1.14 we find a statistically significant negative differential effect for gain round 6, which is sufficiently large to cancel the incentive effect for this group of farmers. To further examine this result, we plot the impact of the incentive treatment for each incentive-gain round combination, separately by round (Appendix Figure 1.5). Across rounds, this effect is similar for gain rounds 5 and 7, and lower for gain round 6. Note the effect for gain round 6 is stable across rounds, including rounds 1-4 at which point none of the participants had discovered which gain round value they had been assigned. These results suggest the group of individuals assigned to gain round 6 within the incentives treatment were somewhat less likely to pick MSD *ex ante* than other individuals in the sample. These individuals are similar in terms of observable characteristics (Appendix Table 1.7), so this finding appears likely to be a statistical artefact.

<sup>34</sup> In Appendix Table 1.16 we present our main specifications in long form (with one observation per participant-round and round-level fixed effects), as indicated in the pre-analysis plan with the addition of a control for rainfall in the preceding round. We report our results separately by rounds 1-4 and rounds 5-10 in columns (3)-(6) to examine whether treatments impact the choice of MSD in each round. The results are similar, and there is no evidence that the impact of the incentives falls off after removing the incentive.

Table 1.5- Impact of treatments on MSD adoption, interacted treatments

	(1) Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Assigned incentives	0.871***	0.123***	-0.084**
<i>Standard error</i>	(0.260)	(0.038)	(0.040)
<i>p-value</i>	0.001	0.001	0.034
<i>Sharpened q-value</i>	0.003	0.003	0.012
Assigned information	0.310	0.057	-0.022
<i>Standard error</i>	(0.306)	(0.045)	(0.047)
<i>p-value</i>	0.310	0.205	0.636
<i>Sharpened q-value</i>	0.871	0.871	0.871
Assigned incentives X Assigned information	-0.536	-0.079	0.019
<i>Standard error</i>	(0.361)	(0.054)	(0.056)
<i>p-value</i>	0.138	0.146	0.730
<i>Sharpened q-value</i>	0.281	0.281	0.322
Gain round = 6	-0.209	-0.036	0.021
<i>Standard error</i>	(0.198)	(0.029)	(0.031)
<i>p-value</i>	0.291	0.213	0.487
<i>Sharpened q-value</i>	0.775	0.775	0.775
Gain round = 7	-0.276	-0.090***	0.044
<i>Standard error</i>	(0.193)	(0.029)	(0.030)
<i>p-value</i>	0.152	0.002	0.147
<i>Sharpened q-value</i>	0.113	0.006	0.113
Mean: No treatments	7.685	0.653	0.338
Mean: Gain Round = 5	8.417	0.777	0.261
Adjusted R-squared	0.120	0.115	0.082
Observations	1324	1324	1324

Notes: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Heteroskedasticity robust (HC3) standard errors are reported in parentheses. Control variables are included in the specification, but not reported (see Table 1.4 note). \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.

Finally, we estimate equation (3) to study the different types of information that were offered, using the subset of individuals who received an information treatment (Table 1.6).

Table 1.6- Impact of different information types on MSD adoption

	(1)	(2)
	Dependent variable: Chose MSD	
Info B: Used MSD (first time)	0.030	0.024
<i>Standard error</i>	(0.020)	(0.024)
<i>p-value</i>	0.142	0.334
<i>Sharpened q-value</i>	0.397	0.397
Info C: Used MSD (last 10 years)	0.037*	0.051**
<i>Standard error</i>	(0.020)	(0.024)
<i>p-value</i>	0.068	0.035
<i>Sharpened q-value</i>	0.074	0.074
Info D: Abandoned MSD	-0.001	0.004
<i>Standard error</i>	(0.021)	(0.025)
<i>p-value</i>	0.952	0.886
<i>Sharpened q-value</i>	1.000	1.000
Poor rainfall last round	-0.012	-0.002
<i>Standard error</i>	(0.015)	(0.031)
<i>p-value</i>	0.424	0.943
<i>Sharpened q-value</i>	1.000	1.000
Info B x Poor rainfall		0.018
<i>Standard error</i>		(0.041)
<i>p-value</i>		0.657
<i>Sharpened q-value</i>		0.49
Info C x Poor rainfall		-0.042
<i>Standard error</i>		(0.044)
<i>p-value</i>		0.336
<i>Sharpened q-value</i>		0.202
Info D x Poor rainfall		-0.015
<i>Standard error</i>		(0.045)
<i>p-value</i>		0.743
<i>Sharpened q-value</i>		0.591
<i>p-value: Info B = Info C</i>	0.688	0.233
<i>p-value: Info B = Info D</i>	0.119	0.401
<i>p-value: Info C = Info D</i>	0.054	0.043
<i>p-value: Info B + Info B x Prev Rainfall</i>		0.218
<i>p-value: Info C + Info C x Prev Rainfall</i>		0.087
<i>p-value: Info D + Info D x Prev Rainfall</i>		0.594
Mean: No information, previous normal	0.837	0.837
Adjusted R-squared	0.106	0.105
Observations	2644	2644

Notes: Sample restricted to information treatment group. Heteroskedasticity robust (HC3) standard errors are reported in parentheses. Observations are at the participant-round level, rounds 1-4. Ordinary least squares regression, with stratification-cell (FBO) and round fixed effects. Control variables are included in the specification, but not reported (see Table 1.4 note). \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.



Without interactions between rainfall and information (column 1), we find evidence of an effect of being told the neighbor had used MSD for at least 10 seasons. The coefficient is 3.7 percentage points, corresponding to a 4.6 percent increase relative to being told your neighbor had used CP. The coefficient estimates on the other forms of information are not statistically significant, and we can reject that the effect of being told a neighbor used MSD for the last 10 years is equal to being told that the neighbor abandoned MSD. We cannot however reject that this effect is equal to being told the neighbor used MSD for the first time.

In column 2, we interact the information with the rainfall from the previous season, because the rainfall outcome in the previous season affects what the participant was told about how much the neighbor earned. Recall that a payoff differential under poor rainfall is evident for all those choosing MSD (information groups B and C), but the payoff differential for choosing MSD in normal years is only evident for information group C (neighbor used MSD for 10 years). Because the information treatment occurred in the first four rounds of the experiment, this outcome is the only one participants could not have experienced for themselves, and as such, information group C in normal years may be the mostly likely to affect behavior.

When the rainfall is normal, the effect of being told your neighbor had used MSD for 10 seasons (and therefore received the production bonus) is now 5 percentage points, and statistically significant at the 5 percent level. The corresponding interaction term for poor rainfall is -4.2 percentage points, though not statistically different from zero. Regardless, it implies the total effect of group C information is near zero when rainfall is poor. This pattern is not repeated for those receiving the information that the neighbor used MSD for the first time (group B). These results are in line with the discussion above, that those who received information about the MSD yield gain in normal years were receiving new information and updating their behavior accordingly. Overall, the evidence suggests that when promoting a

technology like CA where there are deferred benefits, observing peers who have experienced those benefits can be useful for promoting adoption. It should be noted that the peer information provided was limited, and did not help to reduce farmer uncertainty about when the yield gain would occur. As such, a peer information intervention in the field could have larger impacts.

## **1.6 Conclusion**

Agronomists have argued that CA makes for more efficient use of natural resources than traditional farming methods in developing countries (e.g. Hobbs 2007). However, the long time frame associated with private gains to adoption, and uncertainty regarding these gains, contribute to adoption rates well below what would be socially optimal. Using a framed field experiment, this study finds that incentives for adoption might be an effective tool for increasing adoption of CA techniques prior to the point when they become privately profitable. Though there is no overall effect of information, we do find some evidence that being given positive information about neighbors experiencing the deferred benefits of CA increases adoption. While our results are limited to the experimental environment, they suggest that investing in pilot tests of these policy solutions would be worthwhile.

These findings point to both incentives and information campaigns that emphasize outcomes from early adopters as policy options for governments and other actors that want to increase the adoption of CA techniques, and also speak more generally to the promotion of technologies with deferred benefits. In considering how to design incentive and information programs, there are several points to consider. The form of incentives is important. In focus groups conducted in formative research farmers suggested that fertilizers or herbicides would be preferred to cash; such in-kind incentives could not be reflected in an experiment such as this one. It is also important to consider the way that farmers conceive of CA. Ward et al. (2018) find that in Malawi farmers think of choices about adopting CA as distinct decisions for each technique. However, the agronomic evidence that exists on yields concerns adoption of

the entire package, rather than just pieces of it. Therefore, effective policy would either need to consider ways to ensure that farmers were using the entire suite of CA techniques, or would need to also build evidence on the impacts of partial adoption of CA techniques.

## 1.7 References

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## 1.8 Appendix Tables

Table 1.7– Treatment balance

	Incentives treatment			Information treatment			N
	Control	Treatment	p-value	Control	Treatment	p-value	
Age	41	41	0.381	41	41	0.281	1324
Is female	0.49	0.55	0.055	0.51	0.55	0.110	1324
Received no schooling	0.66	0.65	0.842	0.63	0.67	0.149	1317
Received some primary education	0.12	0.10	0.347	0.12	0.10	0.164	1317
Completed primary school	0.03	0.03	0.753	0.03	0.02	0.262	1317
Received some secondary education	0.11	0.12	0.760	0.12	0.11	0.751	1317
Completed secondary school	0.08	0.10	0.214	0.09	0.09	0.827	1317
Primary activity: HH farmwork	0.91	0.88	0.091	0.89	0.89	0.632	1316
Reports secondary activity	0.48	0.50	0.695	0.47	0.52	0.069	1316
Reports any work off-farm	0.28	0.30	0.616	0.27	0.31	0.179	1316
Region: Northern	0.45	0.45	0.908	0.45	0.45	0.896	1317
Region: Upper East	0.29	0.28	0.711	0.28	0.28	0.706	1317
Region: Upper West	0.18	0.19	0.749	0.18	0.18	0.969	1317
Region: Brong Ahafo	0.08	0.08	0.958	0.08	0.09	0.747	1317
Household size	10.0	10.0	0.871	10.1	9.9	0.455	1324
Number of adults (14+)	5.6	5.8	0.442	5.8	5.6	0.465	1309
Number of children (<14)	4.2	4.1	0.672	4.1	4.2	0.573	1309
Household reports polygamy	0.30	0.31	0.890	0.29	0.32	0.372	1317
Religion: Catholic	0.16	0.19	0.111	0.19	0.17	0.544	1317
Religion: Other christian	0.33	0.31	0.533	0.32	0.31	0.856	1317
Religion: Muslim	0.34	0.33	0.620	0.32	0.34	0.483	1317
Religion: Traditional/animist	0.16	0.16	0.981	0.16	0.16	0.734	1317
Language: Buli	0.09	0.09	0.795	0.09	0.09	0.943	1317
Language: Dagbani	0.18	0.17	0.552	0.17	0.18	0.743	1317
Language: Frafra/Gruni	0.20	0.20	0.815	0.20	0.19	0.761	1317
Language: Likpakpa	0.18	0.18	0.904	0.18	0.18	0.969	1317
Language: Other	0.25	0.27	0.491	0.27	0.26	0.699	1317

*Table 1.8– Participant understanding after first explanation*

<u>Share of participants correctly answering...</u>	
<i>Which choice has better harvests under poor rainfall</i>	94.4%
<i>Which choice has lower weeding costs</i>	94.9%
<i>Which choice has better harvests after many seasons</i>	96.0%
<i>First round in which yield gain may become available</i>	87.6%
<i>Last round in which yield gain may become available</i>	90.7%
<i>If yield gain lost after dis-adoption</i>	92.9%
<u>All questions</u>	<u>72.6%</u>

Table 1.9– Impact of treatments on MSD adoption, subset answering all knowledge questions correctly

	(1)	(2)	(3)
	Number rounds		
	MSD	Achieved gain	Abandoned MSD
Incentive treatment	0.485**	0.066**	-0.062**
Standard error	(0.218)	(0.030)	(0.031)
p-value	0.026	0.030	0.046
Sharpened q-value	0.047	0.047	0.047
Information treatment	-0.076	0.003	0.010
Standard error	(0.191)	(0.028)	(0.029)
p-value	0.690	0.917	0.728
Sharpened q-value	1.000	1.000	1.000
Gain round: 6	-0.185	-0.045	0.033
Standard error	(0.234)	(0.034)	(0.036)
p-value	0.430	0.190	0.351
Sharpened q-value	0.755	0.755	0.755
Gain round: 7	-0.299	-0.082**	0.033
Standard error	(0.234)	(0.033)	(0.035)
p-value	0.202	0.013	0.346
Sharpened q-value	0.254	0.041	0.300
Mean: No incentives	7.997	0.710	0.309
Mean: No information	8.359	0.748	0.264
Mean: Gain Round = 5	8.479	0.797	0.238
Adjusted R-squared	0.146	0.147	0.095
Observations	961	961	961

Note: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Control variables are included in the specification (see Table 1.4 note), but not reported. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.



Table 1.10 - Heterogeneity by participant gender

	(1) Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Assigned: Incentives	0.486 (0.265)	0.075 (0.040)	-0.080 (0.042)
Assigned: Information	-0.026 (0.244)	0.032 (0.037)	-0.026 (0.038)
Gain round = 6	-0.313 (0.294)	-0.029 (0.045)	-0.021 (0.048)
Gain round = 7	-0.431 (0.272)	-0.114** (0.043)	0.052 (0.046)
Female	1.571 (2.250)	0.300 (0.293)	-0.402 (0.277)
Female x Incentives	-0.001 (0.390)	-0.020 (0.057)	0.021 (0.058)
Female x Information	0.021 (0.343)	-0.040 (0.051)	0.031 (0.052)
Female x Round = 6	0.265 (0.424)	0.000 (0.064)	0.049 (0.066)
Female x Round = 7	0.235 (0.401)	0.038 (0.061)	-0.013 (0.064)
Mean: No incentives	8.054	0.701	0.326
Mean: No information	8.393	0.738	0.287
Mean: Gain Round = 5	8.600	0.800	0.260
Adjusted R-squared	0.124	0.114	0.087
Observations	1324	1324	1324

Note: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Control variables are included in the specification, but not reported. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.

Table 1.11 - Heterogeneity by self-reported risk aversion

	(1) Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Assigned: Incentives	0.601* (0.239)	0.074* (0.036)	-0.081* (0.036)
Assigned: Information	-0.008 (0.208)	-0.001 (0.033)	-0.025 (0.034)
Gain round = 6	-0.421 (0.263)	-0.054 (0.040)	0.014 (0.042)
Gain round = 7	-0.328 (0.234)	-0.098* (0.038)	0.028 (0.040)
Risk averse	-1.448 (3.667)	-0.386 (0.396)	0.628** (0.233)
Risk averse x Incentives	-0.018 (0.420)	0.014 (0.060)	0.020 (0.061)
Risk averse x Information	-0.166 (0.379)	0.000 (0.054)	0.035 (0.055)
Risk averse x Round = 6	0.382 (0.454)	0.024 (0.065)	0.017 (0.067)
Risk averse x Round = 7	-0.070 (0.443)	-0.003 (0.064)	0.044 (0.067)
Mean: No incentives	7.895	0.684	0.349
Mean: No information	8.324	0.733	0.308
Mean: Gain Round = 5	8.631	0.797	0.273
Adjusted R-squared	0.113	0.101	0.094
Observations	1309	1309	1309

Note: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Control variables are included in the specification, but not reported. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.

Table 1.12 - Heterogeneity by self-reported patience

	(1)	(2)	(3)
	Number rounds		
	MSD	Achieved gain	Abandoned MSD
Assigned: Incentives	0.497*	0.072*	-0.071*
	(0.230)	(0.034)	(0.035)
Assigned: Information	0.127	0.032	-0.036
	(0.205)	(0.032)	(0.033)
Gain round = 6	-0.204	-0.027	-0.003
	(0.255)	(0.039)	(0.041)
Gain round = 7	-0.347	-0.100**	0.043
	(0.241)	(0.039)	(0.041)
Impatient	0.569	0.095	-0.178
	(2.356)	(0.307)	(0.315)
Impatient x Incentives	0.315	0.034	-0.040
	(0.416)	(0.059)	(0.060)
Impatient x Information	-0.691	-0.088	0.079
	(0.379)	(0.054)	(0.054)
Impatient x Round = 6	0.136	-0.006	0.067
	(0.438)	(0.063)	(0.066)
Impatient x Round = 7	0.100	0.017	0.017
	(0.453)	(0.064)	(0.067)
Mean: No incentives	7.990	0.686	0.345
Mean: No information	8.171	0.705	0.324
Mean: Gain Round = 5	8.500	0.776	0.283
Adjusted R-squared	0.137	0.122	0.099
Observations	1324	1324	1324

Note: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Control variables are included in the specification, but not reported. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.

Table 1.13 - Heterogeneity by degree of exposure to GASIP program

	(1) Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Assigned: Incentives	0.680** (0.248)	0.077* (0.038)	-0.048 (0.039)
Assigned: Information	-0.157 (0.213)	-0.013 (0.035)	-0.013 (0.036)
Gain round = 6	-0.123 (0.257)	-0.034 (0.042)	0.008 (0.044)
Gain round = 7	-0.126 (0.253)	-0.058 (0.042)	-0.002 (0.044)
New community	-1.925 (1.589)	-0.394 (0.220)	0.188 (0.230)
New community x Incentives	-0.215 (0.363)	0.001 (0.054)	-0.047 (0.055)
New community x Information	0.204 (0.324)	0.037 (0.048)	0.003 (0.050)
New community x Round = 6	-0.186 (0.396)	-0.011 (0.059)	0.031 (0.061)
New community x Round = 7	-0.286 (0.383)	-0.060 (0.058)	0.086 (0.061)
Mean: No incentives	8.321	0.746	0.269
Mean: No information	8.866	0.805	0.242
Mean: Gain Round = 5	8.874	0.828	0.237
Adjusted R-squared	0.115	0.112	0.084
Observations	1324	1324	1324

Note: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Control variables are included in the specification, but not reported. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.

Table 1.14 - Impact of incentives and gain round on MSD adoption

	(1) Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Assigned: Incentives	0.948***	0.139***	-0.100**
<i>Standard error</i>	(0.304)	(0.044)	(0.048)
<i>p-value</i>	0.001	0.001	0.034
<i>Sharpened q-value</i>	0.003	0.003	0.012
Assigned: Information	-0.034	0.006	-0.010
<i>Standard error</i>	(0.161)	(0.024)	(0.025)
<i>p-value</i>	0.310	0.205	0.636
<i>Sharpened q-value</i>	0.871	0.871	0.871
Gain round = 6	0.367	0.049	-0.016
<i>Standard error</i>	(0.366)	(0.053)	(0.056)
<i>p-value</i>	0.015	0.008	0.025
<i>Sharpened q-value</i>	0.024	0.024	0.024
Gain round = 7	-0.205	-0.070	0.031
<i>Standard error</i>	(0.387)	(0.056)	(0.057)
<i>p-value</i>	0.291	0.213	0.487
<i>Sharpened q-value</i>	0.775	0.775	0.775
Incentives x Gain Round 6	-0.903**	-0.134**	0.057
<i>Standard error</i>	(0.439)	(0.065)	(0.068)
<i>p-value</i>	0.152	0.002	0.147
<i>Sharpened q-value</i>	0.113	0.006	0.113
Incentives x Gain Round 7	-0.113	-0.030	0.019
<i>Standard error</i>	(0.448)	(0.067)	(0.069)
<i>p-value</i>	0.801	0.651	0.78
<i>Sharpened q-value</i>	1.000	1.000	1.000
Incentives + Incentives x Round 6	0.886	0.913	0.368
Incentives + Incentives x Round 7	0.011	0.028	0.098
Mean: No incentives	7.829	0.677	0.337
Mean: No information	8.253	0.727	0.293
Mean: Gain Round = 5	8.417	0.777	0.261
Adjusted R-squared	0.122	0.116	0.082
Observations	1324	1324	1324

Note: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Control variables are included in the specification (see Table 1.4 note), but not reported. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.

Table 1.15 - Balance comparison for participants assigned incentives and gain round = 6

	Incentives only			Gain != 6 or control	Full sample Gain = 6 and incentives	
	Gain != 6	Gain = 6	p-value			p-value
Household size	10.3	9.5	0.027	10.2	9.5	0.044
Is female	0.54	0.57	0.516	0.52	0.57	0.182
Age	41	40	0.147	41	40	0.094
Risk preference (1-10)	6.50	6.11	0.524	6.80	6.11	0.108
Time preference (1-10)	4.84	4.76	0.649	5.06	4.76	0.22
Household assets (USD)	8,270	7,960	0.778	8,490	7,960	0.649
CA practices last season	2.4	2.5	0.450	2.4	2.5	0.303
Crop value (USD)	2309	2021	0.53	2444	2021	0.252
Has electric light	0.58	0.60	0.149	0.58	0.60	0.157
Has toilet access	0.56	0.52	0.079	0.54	0.52	0.31
Dwelling: Cement walls	0.17	0.17	0.858	0.17	0.17	0.978
Dwelling: Cement floor	0.75	0.74	0.994	0.75	0.74	0.987
Dwelling: Metal roof	0.81	0.81	0.769	0.80	0.81	0.684
Poor rainfall (practice)	0.50	0.54	0.236	0.49	0.54	0.148

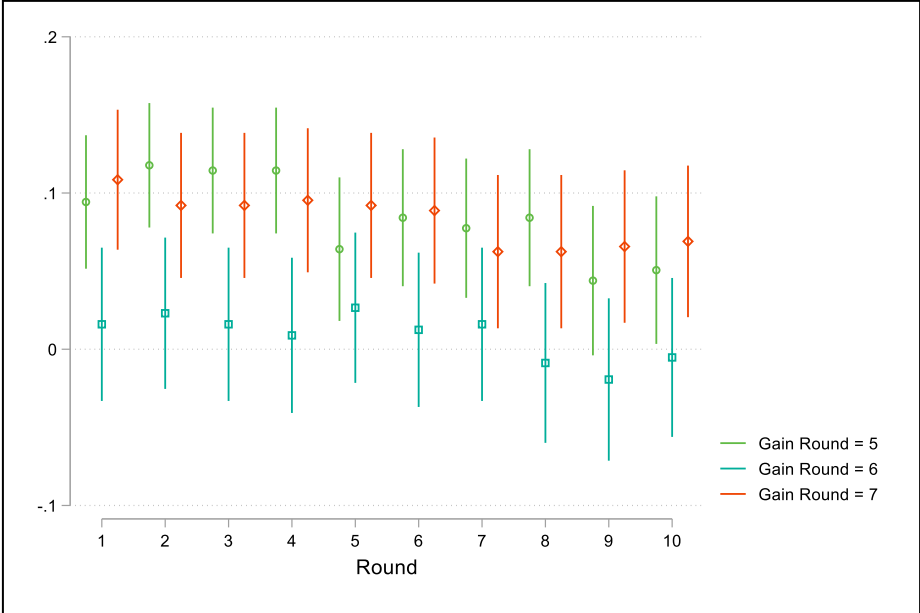
Table 1.16 - Impact of treatments on MSD adoption, by participant-round

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Chose MSD					
	All rounds		Rounds 1-4		Rounds 5-10	
Assigned: Incentives	0.060*** (0.017)	0.095*** (0.028)	0.064*** (0.017)	0.119*** (0.029)	0.057*** (0.018)	0.079*** (0.030)
Gain round: Round 6	-0.022 (0.019)	0.037 (0.034)	-0.028 (0.019)	0.055 (0.035)	-0.018 (0.020)	0.025 (0.037)
Gain round: Round 7	-0.027 (0.018)	-0.021 (0.036)	-0.032* (0.018)	-0.010 (0.038)	-0.025 (0.019)	-0.028 (0.038)
Assigned: Information	-0.005 (0.015)	-0.004 (0.015)	-0.001 (0.016)	0.001 (0.015)	-0.008 (0.016)	-0.006 (0.016)
Poor rainfall (Prior round)	-0.011* (0.006)	-0.011* (0.006)	-0.005 (0.010)	-0.004 (0.010)	-0.016* (0.009)	-0.016* (0.009)
Incentives x Gain Round 6		-0.090** (0.041)		0.128*** (0.042)		-0.066 (0.044)
Incentives x Gain Round 7		-0.011 (0.042)		-0.034 (0.043)		0.004 (0.044)
Incentives + Incentives x Round 5		0.878		0.777		0.678
Incentives + Incentives x Round 6		0.007		0.007		0.011
Mean: No incentives	0.783	0.783	0.783	0.783	0.783	0.783
Mean: No information	0.825	0.825	0.825	0.825	0.825	0.825
Mean: Gain Round = 4	0.842	0.842	0.842	0.842	0.842	0.842
Adjusted R-squared	0.104	0.106	0.100	0.104	0.103	0.104
Observations	13240	13240	5296	5296	7944	7944

Note: Ordinary least squares regression, with round and stratification-cell (FBO) fixed effects. Standard errors in parentheses are clustered at the individual level. Control variables are included in the specification, but not reported. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level respectively.

# 1.9 Appendix Figures

Figure 1.4– Coefficient of assigned incentives  $x$  round, by gain round





## Appendix A – Experimental Script

### Framed Field Experiment Script: Incentives to Adopt Conservation Agriculture

#### Before You Start

The lab-in-the-field script should be read exactly as written. The document is split into numbered sections to reflect the screens which will be shown as individual pages on the tablet used by the enumerator. The text written **in bold** indicates an instruction to the enumerator and should not be read aloud. Questions requiring a response are followed by answer options in *italics*.

#### Outline

Section 1: Informed consent

Section 2: Introduction

Section 3: Instructions for the activity

Section 4: Practice

Section 5: Main activity

Section 6: Closing

The text also includes variables which are highlighted in yellow and indicated by a dollar sign followed by braces, ie.  $\{\text{example\_variable}\}$ . These indicate variables used in the survey program which will be populated with text or number values in the tablet program. The main part of the activity is repeated ten times, the text is repeated with different variable values depending on the stage of the activity, the treatment status of the respondent, and the respondent's previous decisions. There is some text that will only display if the respondent is on a certain stage and has made certain choices. This text is highlighted in pink. The beginning and end of the iterated section of the text is highlighted in red.

Each respondent may be assigned to one (or both) of two treatments: an incentive treatment and an information treatment. Some portions of the texts are only shown to respondents who are assigned to a treatment. Text shown only to respondents in the incentive treatment is highlighted in green while text shown only to respondents in the information treatment is highlighted in blue.

#### Script Start

##### Section 0 – Household Identification

*Screen 0.1*

Enter the unique household ID

Re-enter the unique household ID to confirm

*Screen 0.2*

**Community:**  $\{\text{pl\_community}\}$

**Farmer group:**  $\{\text{pl\_farmgroup}\}$

Compound Name:  $\{pl\_compound\}$

Respondent name:  $\{pl\_name\}$

Respondent gender:  $\{pl\_gender\}$

Confirm that the person listed is the person who will complete the activity.

If the person you are talking to is NOT the listed respondent, go back to check that the household ID has been entered correctly. If the household ID is correct, but the person is NOT the listed respondent, do not proceed.

Contact your Team Leader or Field Manager to resolve the issue.

## **Section 1 – Informed Consent**

### *Screen 1.1*

Good morning/afternoon. I am \_\_\_\_\_ from Innovations for Poverty Action (IPA) a research institute dedicated to discovering and promoting solutions to global poverty problems. We are working with the International Food Policy Research Institute (IFPRI) which is an international research organization focused on sustainable solutions to hunger and poverty. We are doing a study with farmers in Northern Ghana to understand how farmers make decisions about which agricultural practices to use. You recently participated in a household survey for this research, and we would now like to invite you to participate in an additional activity.

### *Screen 1.2*

In the activity, you will be asked to make decisions about which farming practice to use. Researchers will study these choices. They will do this to learn how farmers make decisions about what to do on their farm. The goal of the activity is to provide the government with better information, so that they can improve conditions for farmers.

Provide respondent with the informed consent form

### *Screen 1.3*

If you agree to participate, we will ask you to take part in an activity in which we ask you to make choices about which agricultural practices to use. In the activity we will ask you to make a decision on which practice to adopt. These are not real choices that you will make for your field, just decisions that you make in the activity.

You will receive 5 cedis today, and you will also have the opportunity to receive additional money based on the decisions you make in the activity. Whatever money you earn will be yours to keep and you will not have to pay this money back at any time.

### *Screen 1.4*

The decisions you will make are not difficult. All you need to think about is making the decisions that seem right to you. It is important to think seriously about your decisions because they will affect how much money you will keep at the end of the activity. The activity will take approximately one hour for you to complete.

### *Screen 1.5*

You will make all of your decisions in private, and receive the money in private, so no one will know how much you earn today, unless you choose to tell them. We will not share any information about the decisions you personally make or the amount of money that you may keep at the end of the activity. Your name and address will not be stored with other information we collect about you. The list connecting your name with your number will be kept safe and will only be accessible to the research team.

Any personal information we obtain during the research will be kept strictly confidential. There will be no risk as a result of your participating in the study. Your participation is completely voluntary. You are free to refuse to participate or end participation at any time during the activity.

#### *Screen 1.6*

If you agree to consent, you are agreeing to the following:

The researcher read to me orally the consent form and explained to me its meaning. I agree to take part in this study. I understand that I am free to discontinue participation at any time if I so choose, and that the enumerator will gladly answer any question that arise during the course of the study. I will receive a copy of the signed and dated consent form.

Do you have any questions?

Answer any questions and ensure the participant understands the consent form before proceeding

#### *Screen 1.7*

Are you willing to provide your consent to participate in the activity today?

- Yes

If the respondent is willing to provide their consent, ask them to sign the copy of the informed consent document. Give an unsigned copy of the consent form to the respondent. Retain the signed copy and give it to your supervisor at the end of the day.

- No

If the respondent is unwilling to provide consent, thank them for their time and issue them a voucher for 5 cedis, and direct them to see your team leader to redeem the voucher for cash. If they do not provide consent, the activity ends here.

## **Section 2 – Introduction**

### *Screen 2.1*

Before we start, I am going to tell you about two farming practices. Then we will explain the activity, so you understand what you will be doing. After that, we will do some examples so that you can practice. Then we will do the main activity. Once the activity is completed, we will find out how much you earned. Then you will receive the payment, and we will be finished.

### *Screen 2.2*

I want to tell you about the 2 different farming practices during the land preparation phase. During the activity, you will choose between these two practices.

The first choice is conventional tillage.

Using conventional tillage, farmers prepare the land by tilling the soil. This can be done with hand tools, or it can be done with animals or by using mechanized tools such as a tractor. This prepares the land to grow the seeds and reduces weeds, but this also makes the soil loose so it does not keep as much water and can make the soil less fertile.

Because it does not hold as much water, if the rains are poor the harvest is lower. And when the living material in the soil is reduced, over several seasons the harvest will not be as high.

### *Screen 2.3*

The second choice is called minimal soil disturbance.

You may have heard about this practice before. Minimal soil disturbance is a different way to prepare the land. Using this practice, the soil is not disturbed with hand tools or a tractor before planting.

Instead, 'residue' is chopped off from last season's crop and left on top of the soil. During planting season, holes are made in the residue to plant the seeds. This keeps the soil firmer, so it can hold more water than using conventional tillage.

So, if the rains are poor, you can harvest more from a plot compared to using conventional tillage. Also, because the soil is not disturbed, over several seasons the soil will produce more. However, because the soil is not tilled, there may be more weeding required when using minimal soil disturbance versus conventional tillage.

### *Screen 2.4*

So, now I have told you about the two practices. I would like to ask a few questions before we start the main activity.

Can you tell me, which choice will have a greater harvest if the rains are poor, conventional tillage, or minimal soil disturbance?

Record participant's first answer

- Conventional tillage
- Minimal soil disturbance

### *Screen 2.5 [2.4 = Conventional tillage]*

That is not correct, please review the information with the respondent before proceeding.

### *Screen 2.6*

Which choice requires less weeding?

Record participant's first answer

- Conventional tillage
- Minimal soil disturbance

### *Screen 2.7 [2.6 = MSD]*

That is not correct, please review the information with the respondent before proceeding.

### *Screen 2.8*

Which choice has better harvests after many seasons?

Record participant's first answer

- Conventional tillage
- Minimal soil disturbance

*Screen 2.9 [2.8 = CP]*

That is not correct, please review the information with the respondent before proceeding.

*Screen 2.10*

Review the information if necessary and answer any questions. Do not provide additional information on minimal soil disturbance other than what is written above.

### **Section 3: Instructions for the main activity**

*Screen 3.1*

Now, I will explain how to complete the activity. I will give you paper money to use during the activity. At the end of the activity, I will give you a receipt for the amount of paper money you have, which you can take to my team leader to exchange for real money.

*Screen 3.2*

During the activity, you will be asked to choose between 2 farming practices. We will ask you to imagine that you have a one-acre plot on which you grow crops. This is not a real plot, it is just for the activity.

The main activity will be completed in 10 rounds. You can think of each round of the activity as one farming season. Therefore, at the end of the activity, you will have completed 10 choices for 10 agricultural seasons. Each round will have three stages.

*Screen 3.3*

For each season, the first stage is land preparation. You will decide how to prepare the land. You will decide to use conventional tillage or minimal soil disturbance.

*Screen 3.4*

The second stage is weeding. Depending on the practice you choose for land preparation, you will pay a weeding cost during this stage using the paper money. The cost is lower for conventional tillage, and higher for minimal soil disturbance.

*Screen 3.5*

The last stage is harvest. We will find out if the rainfall was normal or poor for the season. This will be done randomly using my computer. You or I will not know what the rains will be for that round until the harvest stage. Depending on the rainfall and the practice you chose at the start of the round, I will give you some paper money for the harvest.

*Screen 3.6*

After I give you the paper money for the harvest, we will begin a new round. Before we begin the main activity, we will practice the activity with a few examples.

*Screen 3.7*

Because minimal soil disturbance preserves more living matter in the soil, over time the production will be better. This is the same in the activity. After some time the production will increase. But when it happens is not certain. The reason we do not know when the increase will occur for sure is because everyone's fields are different, so the process of improving the soil may take more or less time.

So, if you always choose MSD, the production will increase in the fifth, the sixth or the seventh season. But only if you always choose MSD. If you choose CP for any season, you will again have to wait until between five and seven seasons for the production to increase.

*Screen 3.8*

So, if you always choose MSD, what is the first season when you might receive the gain?

Record participant's first answer

*Screen 3.9 [3.8 != 5]*

That is not correct. Repeat the information as required to ensure they understand the answer is 5.

*Screen 3.10*

So, if you keep choosing MSD, you might receive the gain in production in Round 5. But it is not certain.

What is the latest season when you would receive the gain?

Record participant's first answer

*Screen 3.11 [3.10 != 7]*

That is not correct. Repeat the information as required to ensure they understand the answer is 7.

*Screen 3.12*

That's right. If you choose MSD for seven seasons, it is certain that in the seventh season your production will increase with MSD. But what if you then choose conventional tillage, after that will the production amount for minimal soil disturbance still be increased in the next round?

Record participant's first answer

- Yes
- No

*Screen 3.13 [3.12 = 1]*

That is not correct. Repeat the information as required to ensure they understand they understand.

*Screen 3.14*

Remember, if you keep choosing MSD, between the fifth and seventh season of use your production will increase. You will find out that if your production has increased when you receive money at the end of the round for the harvest. After that you will continue to receive the increased amount if you continue to choose MSD.

*Screen 3.15*

Ok, so as we have seen there are three important characteristics of the practices that affect the activity. The first is that minimal soil disturbance is better for production if the rainfall is poor. The second is that minimal soil disturbance requires more weeding, so the cost of weeding is higher for minimal soil disturbance.

The third is that minimal soil disturbance benefits production over time, if you choose minimal soil disturbance the production payout will increase between the fifth and seventh round.

#### *Screen 3.16*

Next, now that we know about the activity I am going to explain how we will complete it, and we will practice it together. Do you have any questions before I do that?

ANSWER ANY QUESTIONS AND REFER BACK TO PREVIOUS EXPLANATIONS IF REQUIRED BEFORE PROCEEDING TO PART 4

### **Section 4: Practice**

#### *Screen 4.1*

We will practice the activity before the main activity begins. I am going to give you 1 cedi's worth of pesewa coins – this is for practice only.

GIVE THE PARTICIPANT:

- 1 FIFTY PESEWA COIN
- 5 TEN PESEWA COINS

#### *Screen 4.2*

Ok, so now that you have the money to use for the practice, I want to show you the choice sheet that we will use.

SHOW PARTICIPANT CHOICE SHEET A

#### *Screen 4.3*

At the beginning of each round I will show you a choice sheet like this one. First, we have land preparation [**POINT**]. This is the part of the round where you make a choice about which practice you want to use.

#### *Screen 4.4*

Second, we have weeding [**POINT**]. Just like on a real plot, someone must work to remove the weeds after planting. So, in the activity, you must pay a cost to remove weeds in each round. This cost depends on which choice you make for land preparation. If you choose conventional tillage the cost is  $\${cp\_cost}$ . If you choose minimal soil disturbance, the cost will be  $\${msd\_cost}$ .

So, you can see that the cost for MSD is higher for weeding. This is because minimal soil disturbance requires more weeding. After you make your choice, you must pay me  $\${cp\_cost}$  if you chose conventional tillage or  $\${msd\_cost}$  if you chose minimal soil disturbance.

#### *Screen 4.5*

Third, we have harvest **[POINT]**. This is when we find out how much your harvest was for this round. Just like a real season, at the end is the harvest when we find out how much crop was produced on your plot. This depends on the rain. The rain can be normal or it can be poor. On average, the rain will be normal 2 out of every 3 rounds, and poor 1 out of every 3 rounds. But we do not know what the rain will be like for each round until this third stage of the round. When you choose which practice to use for the round, you will not know if the rain will be normal or poor. Here we can see what the production will be for each practice if the rain is normal **[POINT]** and if it is poor **[POINT]**. So, in the harvest stage, my computer will randomly determine what the rainfall was for that round. Then you will receive the amount for that type of rainfall, based on what practice you used.

*Screen 4.6*

At the bottom here **[POINT]** you can see there is an additional amount below for MSD. This is an additional amount that you could receive for your harvest if you have practiced MSD for at least five seasons in a row. So, for the first four seasons it will not be possible to receive this. But if you choose MSD for at least five seasons, you may receive this additional amount either in the fifth, sixth or seventh season if you continue to choose MSD.

*Screen 4.7*

After you receive it in one round, it will be available to you for certain in the next round if you choose MSD. Then I will show you this sheet.

SHOW PARTICIPANT CHOICE SHEET B

Here you can see the same amounts have been added to the total. This indicates that you will receive the additional amount for sure if you choose MSD. But only if you continue to choose MSD. If you decide to choose CP, then you will have to wait again for another five seasons before you could receive the increase again.

*Screen 4.8*

Ok, so now we have explained the choice sheet, we are going to do a round to practice. This is just to know how to complete the activity, you will not receive any money for this round.

SHOW CHOICE SHEET A. Use this sheet for the practice round

This is the sheet we will use for the practice round. We will use the same sheet at the start of the activity.

*Screen 4.9*

Are you ready to begin?

CONFIRM PARTICIPANT IS READY AND ANSWER ANY QUESTIONS BEFORE PROCEEDING

*Screen 4.10*

We are now going to begin the practice round. The first stage is land preparation. Your choice is either to use conventional tillage or minimal soil disturbance. Which choice would you like to make for this practice round?

RECORD ANSWER

- Conventional tillage



- Minimal soil disturbance

*Screen 4.11*

Thank you, you have chosen  $\{\text{practice\_choice}\}$  for this round. This means that you will pay  $\{\text{practice\_cost}\}$  for this round for weeding. And you will receive either  $\{\text{practice\_normal}\}$  if the rainfall is normal or  $\{\text{practice\_poor}\}$  if the rainfall is poor.

Please provide me with  $\{\text{practice\_cost}\}$  from the money I have given you to pay for the cost of weeding for this round.

HAVE PARTICIPANT PROVIDE  $\{\text{practice\_cost}\}$ . VERIFY THAT THE AMOUNT GIVEN IS CORRECT BEFORE PROCEEDING.

*Screen 4.12*

Thank you. Now we have made the choice and paid the cost for weeding, we will find out the rainfall amount for this round and calculate your payout for this practice round.

*Screen 4.13*

The computer has determined that the rainfall for this season was  $\{\text{pl\_rainfall\_example}\}$ . So, because you chose  $\{\text{practice\_choice}\}$ , you will receive  $\{\text{practice\_payout}\}$ .

GIVE TOKENS CORRESPONDING TO  $\{\text{practice\_payout}\}$  TO THE PARTICIPANT. VERIFY THE AMOUNT IS CORRECT BEFORE PROCEEDING

*Screen 4.14*

FOR THE FOLLOWING EXPLANATION DEMONSTRATE USING PAPER MONEY

This concludes our practice round. Because you chose  $\{\text{practice\_choice}\}$  you paid  $\{\text{practice\_cost}\}$  for weeding. Because the rainfall was  $\{\text{pl\_rainfall\_example}\}$  you received  $\{\text{practice\_payout}\}$ . So overall, you received  $\{\text{practice\_net}\}$  for this round.

If you had chosen  $\{\text{practice\_alt\_choice}\}$ , you would have paid  $\{\text{practice\_alt\_cost}\}$  for weeding, and received  $\{\text{practice\_alt\_payout}\}$  for your production. So overall, the payoff would have been  $\{\text{practice\_alt\_net}\}$  for this round.

*Screen 4.15*

Ok, that concludes our practice. Do you have any questions for me?

ANSWER ANY QUESTIONS AND MAKE SURE THE RESPONDENT HAS A GOOD UNDERSTANDING BEFORE PROCEEDING

*Screen 4.16*

Now we are going to begin the main activity. Before we start I want to remind you of three things:

- If you choose MSD the cost for weeding will be higher
- If the rainfall is normal, you will receive the same production from CP as for MSD. But if the rainfall is poor, you will receive more production from MSD.
- If you choose MSD continuously, your harvest will increase sometime between Round 5 and Round 7. If your harvest is increased, you will receive more from production if you choose MSD.

Do you have any questions before we begin the main activity?

ANSWER ANY QUESTIONS, THEN COLLECT ALL TOKENS FROM THE PARTICIPANT BEFORE BEGINNING.

### Section 5: Main activity

#### Screen 5.1

Now we will begin the main activity. I am going to provide you with 1 cedi worth of play money to use. At the end of the activity, you will redeem the play money for real money.

GIVE THE PARTICIPANT:

- 1 FIFTY PESEWA COIN
- 5 TEN PESEWA COINS

Any money you do not spend you can keep until the next round.

#### Screen 5.2

Before we begin, I would like to inform you that you have been selected to receive an additional bonus payment of  $\${pl\_bonus}$  each round. However, to receive the bonus payment, you must choose MSD in at least one of the first 4 rounds.

This bonus will be provided to you after the weeding stage of each round if you choose MSD. Once Round 5 begins, you will not be eligible to receive any bonus.

#### BEGIN LOOP

#### Screen 5.3.1-10

PROVIDE PARTICIPANT WITH CHOICE SHEET  $\${choice\_sheet}$

We are now going to begin Round  $\${round\_number}$ . Here is the choice sheet.

The first stage is land preparation. Your choice is either to use conventional tillage or minimal soil disturbance. Before you make your choice, I would like to remind you that you are eligible to receive a bonus payment of  $\${pl\_bonus}$  if you choose MSD for this round. Before you make this choice, I would like to give you some information about your neighbor.  $\${info\_treatment}$ . The weather was  $\${info\_weather}$ , so they received  $\${info\_net}$  overall for the season.

Which choice would you like to make for this round?

- Conventional tillage
- Minimal soil disturbance

#### RECORD ANSWER

#### Screen 5.4.1-10

Thank you, you have chosen  $\${choice}$  for this round. This means that you will pay  $\${choice\_cost}$  for this round for weeding. And you will receive either  $\${display\_normal}$  if the rainfall is normal or  $\${display\_poor}$  if the rainfall is poor. Please provide me with  $\${choice\_cost}$  to pay for the cost of weeding for this round.

VERIFY THAT THE CHOICE AND AMOUNT GIVEN IS CORRECT BEFORE PROCEEDING. IF THE CHOICE IS INCORRECT, GO BACK TO PREVIOUS SCREEN

Screen 5.5.1-4

[IF BEFORE ROUND 5 & MSD CHOSEN]

Because you chose MSD for this round you are eligible to receive a bonus of  $\${pl\_bonus}$ . I will now provide you with that amount.

PROVIDE PARTICIPANT WITH  $\${pl\_bonus}$

Screen 5.6.1-10

Now we have made the choice and paid the cost for weeding, and paid your bonus, we will find out the rainfall amount for this round and calculate your payout for this season.

Screen 5.7.5-10 [If production increase triggered this round]

Because you have chosen MSD for at least five seasons in a row, your production has now increased. You will receive an additional amount from harvest for this round and in future rounds if you continue to choose MSD. In the next round I will show you a different choice sheet with the additional amount included.

Screen 5.8.1-10

The computer has determined that the rainfall for this season was  $\${rainfall\_outcome}$ . So, because you chose  $\${choice}$ , you will receive  $\${payout}$ .

GIVE TOKENS CORRESPONDING TO  $\${payout}$  TO THE PARTICIPANT. VERIFY THE AMOUNT IS CORRECT BEFORE PROCEEDING

Screen 5.9.1-9

Thank you, we will now begin the next round.

END LOOP

## Section 6 – Closing

Screen 6.1

Ok, we have now completed Season 10 and this concludes the activity. Overall, you have earned  $\${total\_payout}$ . I will now write a voucher for the  $\${total\_payout}$ , for you to exchange for real money.

LAY THE TOKENS FROM THE ACTIVITY IN FRONT OF THE PARTICIPANT, AND RECORD THE TOTAL AMOUNT ON THE RECEIPT

Thank you for your participation today.

ANSWER ANY QUESTIONS, THEN INSTRUCT THE PARTICIPANT TO TAKE THE voucher TO YOUR SUPERVISOR TO RECEIVE THEIR PAYMENT.

Screen 6.2

ENUMERATOR: ON A SCALE OF 1-10 where 10= Very good understanding and 1=Very poor understanding, how would you rate the participant's understanding of the activity? (Note that this is not about how much money they received, but how well you think they understood the explanation of the activity).

Screen 6.3

Did you experience any problems implementing the activity?




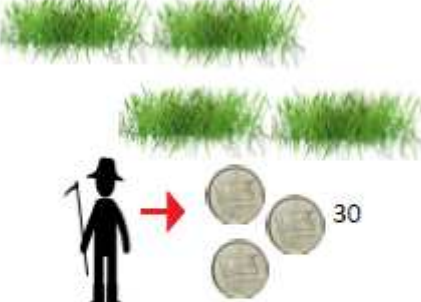
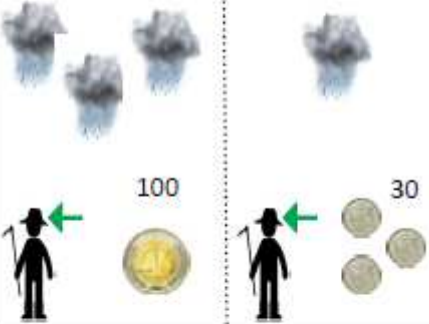
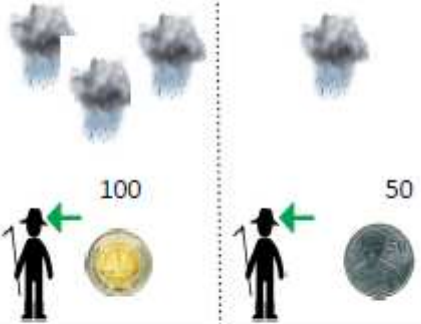
- Yes, the participant did not have a good understanding
- Yes, the participant did not agree with my explanation
- Yes, the activity was interrupted for some time
- Yes, other problem (specify)
- No, no problem

*Screen 6.4*

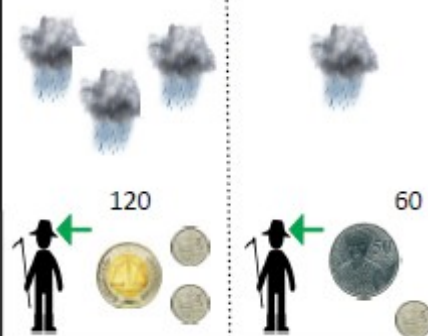
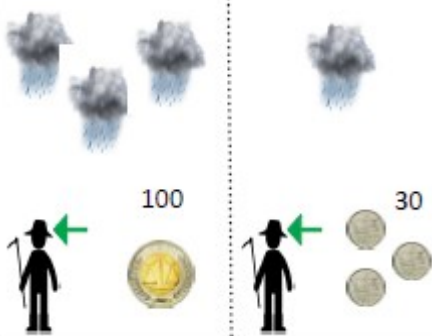
ENUMERATOR: PLEASE RECORD ANY COMMENTS ABOUT YOUR IMPRESSION OF THE SESSION, OR ANY ADDITIONAL DESCRIPTION OF PROBLEMS YOU ENCOUNTERED

**End of Script**

# Appendix B – Visual Aids

Choice Sheet A	
	
	
	

Choice Sheet B



## **Chapitre 2**

### **Seeding Resilience? Climate adaptation and the Green Revolution in Indian wheat**

#### **Abstract**

Climate change increases the uncertainty that agricultural producers face. Understanding the extent to which farmers are able to adapt to changes in the weather outcomes they face is an important concern for ensuring global food security, particularly in developing economies. This paper uses a long-run panel of wheat producing districts in India to estimate the extent to which farmers are able to adapt to changing weather conditions in the context of technological change. Specifically, I exploit variation in the timing of the diffusion of high-yielding seed varieties to test for differences in adaptive capacity under different technological regimes. I find that farmers in early-adopting districts are more resilient to temperature shocks than late-adopting farmers in the years following adoption, but these effects do not persist into recent decades. My findings have important implications for the design of agricultural adaptation policies to protect livelihoods and ensure food security in a warming world.

## 2.1 Introduction

Increased atmospheric concentrations of greenhouse gases are altering the distribution of weather outcomes globally, in a rapid shift from the conditions which have prevailed through recorded human history (Xu, Kohler, Lenton, & Scheffer, 2020). Climate change in turn affects agricultural production, as the growth of plants and livestock are in part determined by the range of temperatures, precipitation and other weather conditions to which they are exposed as they develop. These changes have already imposed large-scale welfare costs globally. Ortiz-Bobea et al. (2021) estimate that climate change experienced has reduced agricultural total factor productivity by 21% since 1961, with the most severe impacts in the tropics where most poor farmers are located.

In such a context, a crucial question facing global agriculture is to what extent farmers are able to alter other inputs to the production process in order to re-optimize output in response to this new distribution of weather outcomes, in other words to adapt to climate change. This can take the form of long-run adaptation to *climate*, for example by investing in physical capital or altering land allocations, or short-run adaptation to *weather* based on forecasts or conditions experienced in the planting season, for example by shifting the timing of activities or the allocation of labor. The extent to which farmers are able to adapt across either time horizon is a particular concern in low- and middle-income countries, where access to optimal production technologies is constrained and infrastructure such as irrigation and road networks which may improve resilience to weather shocks is often not present (Aufhammer & Kahn, 2018).

Despite the importance of this issue, the bulk of the evidence currently available on climate adaptation comes from high income countries, primarily the United States (Deschênes & Greenstone, 2007; Agostino & Schlenker, 2016; Burke and Emerick, 2016; Hsiang et al. 2017; Cui 2020), at least in part because the long-run datasets on weather outcomes needed to understand how farmers respond to deviations are not available for most developing economies. This presents a challenge, because the capacity of agricultural producers operating in contexts with well-functioning capital markets, high levels of mechanization and with access to detailed weather predictions to respond to climate change may be quite different than for a farmer operating in markets where only some or none of these are present. The agriculture sector in developing economies currently faces the double challenge of attempting to move toward the production frontier as improved technologies diffuse, while also adjusting to greater uncertainty in the distribution of weather outcomes which in part determine production outcomes.

In this paper, I help to address this evidence gap by testing for adaptation to seasonal deviations from temperature norms. To do so, I construct a novel dataset which combines two long-run datasets on district agricultural production outcomes and maps these to a gridded spatial dataset of weather outcomes from the India Met Office, to produce a district-level panel of weather and production outcomes spanning the years 1951-2017, which enables me to test for yield responses to weather variations across multiple decades. I focus on wheat because in addition to its importance as a major staple and commercial crop, it was also subject to a clearly identifiable shift in available production technologies.

The deployment of Green Revolution technologies- in the form of high-yielding variety (HYV) seeds and associated inputs- varied in its timing across districts, which allows me to compare the adaptive capacity of farmers with distinctly different available technology sets. This technological shift has been credited with major increases in yields, and hence with associated



welfare gains through improvements in income (Gollin, Hansen, & Wingender, 2021), though has also been associated with negative effects on environment and inequality, and increases in the incidence of chronic diseases (Mann, 2018; Baranski 2022; Sekhri & Shastry, 2024). For the purposes of this paper, I do not explore these secondary effects, but will focus on primary effects on yields, and how they affect resilience to weather shocks.

I employ this dataset to apply current methods from the climate econometrics literature to the case of wheat production in India. I use penalized regression approach of Mérel and Gammans (2021, 2024) to estimate a specification capturing the short- and long-run yield responses. I combine this with a simple difference-in-differences approach, by interacting the specification with a measure of HYV adoption. I use this to compare the evidence for short-run adaptation before and after adoption.

The role of technological change has not generally been considered in extant studies looking at adaptation in the Indian context, but has considerable importance to contemporary policy debates. Farmers in developing countries are experiencing both improvements in technological access and also increased uncertainty from weather shocks. By providing evidence on how farmers adapted in similar circumstances in the past, I hope to inform contemporary debates on how best to adapt agricultural production to a changing climate.

My results contribute to a developing literature on climate change adaptation in Indian agriculture. Recent microeconomic studies have demonstrated that farmers in this context do respond to information regarding climate. Kala (2019) uses a long-run village panel to demonstrate that farmers respond to signals from rainfall in a way that is consistent with rational updating of an implied distribution of rainfall outcomes. Burlig et al. (2024) experimentally vary access to forecast information on monsoon forecasts in India and similarly find that farmers update their beliefs and alter input allocations consistent with a model of rational adaptation. These studies provide an important foundation by providing micro-level evidence demonstrating that farmers recognize changes in the extent of uncertainty in weather outcomes, and that they alter their allocation of inputs and choice of practices in response to these changes.

This paper builds on these findings by contributing to a nascent literature on aggregate production. An important early paper by Guiteras (2009) uses inter-annual variation in outcomes to estimate losses associated with short-run weather shocks and finds significant (4.5-9%) yield penalties associated with them, implying large (25+% yield reductions) in the absence of adaptation. Taraz (2018) analyzes the ability of Indian farmers to adapt to increased heat, comparing the yield response of farmers in historically hotter and less hot districts to heat shocks of a given magnitude, and finds some evidence of adaptation to moderate shocks, but limited ability to respond to extreme heat.

Closest to this paper in its approach is work by Kumar & Khanna (2023) and Constanza (2023) who similarly applies the penalized regression method to district-level India data to estimate adaptation for wheat, rice and maize, and rice and chickpeas, respectively. In my initial analysis, I reproduce the former's specification for wheat, using an expanded panel and a location-specific cropping season definition. In line with their results, I find no evidence of an adaptive response to inter-annual deviations from expected temperature averages. I then demonstrate that this apparent lack of a response is masking important variation caused by differences in the technology set available to farmers in different districts across the panel. Exploiting variation in the timing of the dissemination of Green Revolution technology by district, I first show that adoption of HYVs (and associated technology) led to a statistically significant increase in wheat

yields. I then build on this to provide evidence that farmers in late-adopting districts suffer losses when temperatures deviate from long-run averages, while those in early-adopting districts experience relative increases in yield under the same deviation. I show that these gains are driven by anomalies within the optimal temperature range for wheat growth, and caution that absent further technological improvements, farmers' resilience in future will likely be constrained by the increased incidence of damaging temperature days.

Understanding the relationship between agricultural modernization and climate resilience is an important objective for global food policy. Hundreds of millions of farmers in developing economies remain reliant on successfully managing their production for their welfare. In the context of a changing global climate, it is essential to understand how resilient their production will be to weather shocks as they adopt new technologies and production methods. This paper contributes to this evidence base, while acknowledging that much work needs to be done to understand the extent to which farmers in low-income countries are able to adapt to a changing climate.

## **2.2 Data**

### **2.2.1 Weather data**

Historic weather data is taken from the Indian Meteorological Department's office of Climate Research and Services.<sup>35</sup> Their database provides gridded daily data on minimum and maximum daily temperatures (1.0 x 1.0 resolution) and on total daily rainfall (0.25 x 0.25 resolution) for the whole of India for the period 1951-2017. This level of high-resolution weather data across a significant time span is generally not available for developing economies. By providing long-run panel data, this resource allows for the application of econometric methods for long-run adaptation which have generally only been applied in developed economy contexts where such data are broadly available.

### **2.2.2 Agricultural production data**

Data on agricultural outcomes is taken from two principal data sources. The first is the World Bank's India Agriculture and Climate dataset (hereafter IAC) which is a panel of districts in India running from 1956-1987.<sup>36</sup> The second is the International Crop Institute for the Semi-Arid Tropics (ICRISAT) District Level Database for India (DLD), which covers the period 1966-2017.<sup>37</sup> The DLD dataset comprises two products: an 'apportioned' dataset- which covers the full period based on district boundaries as defined in 1966, consisting of 313 districts across 16 Indian states<sup>38</sup>- and an 'unapportioned' dataset which provides the same data using modern administrative boundaries consisting of 592 districts from 20 states. Unfortunately, the latter product only covers the period 1990-2017, and there is no reasonable way to disaggregate agricultural outcomes to match them to modern administrative units, hence I rely on the

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<sup>35</sup> Available online at <https://imd pune.gov.in/>

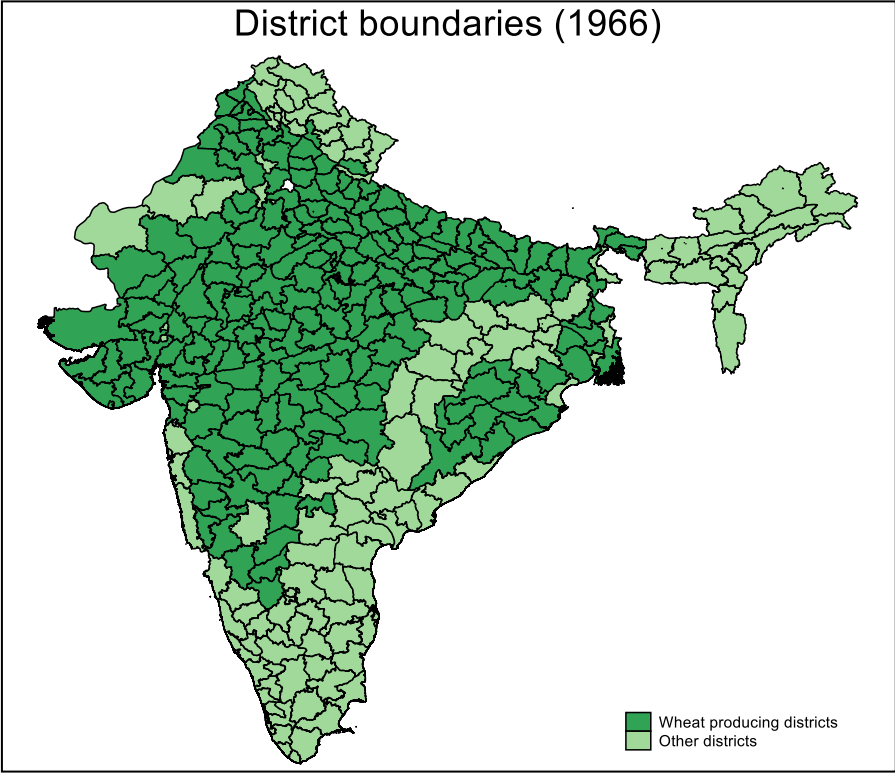
<sup>36</sup> To the best of my knowledge, this is not formally hosted by the World Bank. The dataset used here is available from BREAD at <https://ibread.org/data-sets/>

<sup>37</sup> Available online at <http://data.icrisat.org/dld/>

<sup>38</sup> With the exception of Assam, this dataset excludes the states of India's northeast as well as the union territory of Jammu and Kashmir.

apportioned DLD dataset for the primary analysis. A map of districts used in the analysis, using their boundaries as 1966 is shown below (Figure 2.1).

Figure 2.1 – Map of wheat producing districts included in panel dataset



The two datasets contain annual data at the district level on inputs, production and prices for major crops.<sup>39</sup> Though there is some variation in the specific variables included, both have in common the key outcome and explanatory variables used in the specifications described in the following section, primarily, the area of wheat sown, total output and yield, and the share of that area planted with HYV for each year. I harmonize the datasets by generating a common set of district level identifiers and merging on these. For the years in which the datasets overlap, I use the value from the DLD dataset unless the observation is missing, in which case the IAC is used. To test for robustness, I estimate my primary specifications under an alternative construction with the IAC value used in case of overlap, and find very similar results (Appendix Tables 2.7-2.9).

### 2.2.3 Combined dataset

To create the primary analysis dataset, I match the grid level data from the weather dataset to a shapefile containing the district boundaries from 1966. Constructed weather variables such as growing degree days (see *Methods* section) are generated at the grid-square level, and then aggregated to the district-level following the procedure described by Guitieras (2009). This

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<sup>39</sup> Specifically: rice, wheat, sorghum, pearl millet, maize, finger millet, barley, chickpeas, pigeon peas, minor pulses, groundnuts, sesame, rapeseed & mustard, safflower, castor, linseed, sunflower, soy beans, oil seeds, sugarcane, cotton, fruits, vegetables, potatoes, onions and fodder.

provides a panel of daily weather outcomes at the district-level for the period 1951-2017 which is then matched to the combined IAC-DLD dataset of agricultural outcomes. This provides coverage of the period prior to the introduction of HYV seeds (1951-1965), the initial adoption period (1966-1979) and their subsequent mass adoption (1980-2017).

From the set of all districts, I exclude urban and other non-agricultural districts, districts which do not produce wheat, and districts with a high share of missing data for primary outcomes, resulting in a final panel dataset of 206 districts from 1951-2017. This forms the main dataset used for subsequent analysis, unless otherwise noted.

#### **2.2.4 Cropping season**

Note from the description above that agricultural outcomes are recorded annually. In northern latitude countries such as the US, while there is some variation in the beginning and end period of wheat cultivation, there is only one cropping season. In India by contrast, in many areas two cropping seasons are the norm, for these cases I am unable to disaggregate inputs and outputs across the two seasons (or observe with certainty all cases in which wheat was double-cropped) and therefore treat them as a single season.

Another important consideration is that the main cropping season for wheat (*Rabi*) runs from approximately October to March or April of the following year. Since agricultural data are reported retrospectively from harvest, it is important to exclude weather outcomes which occur after the conclusion of the wheat season, since these by definition cannot influence production. Hence for the purposes of this paper I treat a ‘year’ as the period from 12 months prior up until harvest, so for example where harvest occurs in April, the ‘year’ 1984 in the dataset refers to May 1983-April 1984.

This leaves the question of how to define a cropping season. My preferred approach is to minimize the potential for post-season weather outcomes to enter into the analysis by allowing the cropping season to vary geographically. The Indian Ministry of Agriculture and Farmers Welfare provides state-level<sup>40</sup> definitions of cropping season for wheat (and other crops) which I use when aggregating weather outcomes. While I believe this to be the best approach to capture the relevant temperature and precipitation outcomes for the analysis, I additionally perform the main specifications in the paper using a common ‘All India’ cropping season for wheat, presented in Appendix Tables 2.10-2.11. An alternative data-driven approach is to use the method described in (Li & Ortiz-Bobea, 2022) to define a cropping season by best fit to the data. This is a promising alternative approach- though computationally intensive- which will be included in a subsequent version of this paper.

### **2.3 Methods**

A range of methods have been used in the literature to attempt to quantify the extent of impacts in variation in weather as a result of climate change on agricultural outcomes. Ortiz-Bobea (2021) provides a detailed summary and categorization of these approaches. I do not attempt to discuss the entirety of these approaches here, but instead provide a brief description of the

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<sup>40</sup> To the best of my knowledge this data is not available at the district level for the country as a whole from a consistent source. State-level data is hosted at: <https://desagri.gov.in/document-report/4-crop-calendar-of-major-crops/>

estimation strategies underlying my econometric approach. It is important to recognize however, that this constitutes one strand of a multi-disciplinary literature, and should be viewed as complementary to, for example, biophysical modelling approaches used in the physical sciences.

Following Merel & Gammans (2021), I begin from the assumption that farmers seek to achieve the greatest possible output ( $y$ ) conditional on a set of factors- such as input and labor allocation- over which they have control ( $\zeta$ ), weather outcomes which they do not control ( $x$ ), and an error term  $\varepsilon$ , which constitutes all non-weather factors influencing production outcomes. Thus, farmers in district  $i$  at time  $t$ , seek to maximize the function:

$$y_{it} = \theta_i(x_{it}, \xi_i, \varepsilon_{it}) \quad (1)$$

Importantly, the effect of weather variables on production is non-linear. For example, for temperature, there exists an optimum value to maximize the yield of a given crop. The marginal decline in yield is increasing with distance from this optimum as excess heat or cold impair the health of the plant. I capture this using a quadratic specification for a given weather variable. This gives the following equation determining output for a given input allocation:

$$y_{it} = \alpha_i + \beta_1 x_{it} + \beta_2 x_{it}^2 + \beta_3 \delta_i + \beta_4 \vartheta_t + \varepsilon_{it} \quad (2)$$

Where  $y_{it}$  is the (log) yield for farmers in district,  $i$  in year  $t$ . The term  $x_{it}$  represents the level of a given weather variable and its square,  $\delta_i$  and  $\vartheta_t$  are time and location (state) fixed effects respectively, and  $\varepsilon$  is a stochastic error term. This specification captures the long-run relationship between weather outcomes and output over the long-run. Underlying this approach is the assumption that over a sufficient time-period, farmers are able to vary all factors of production, including long-run capital investments. Hence for a static set of available technologies, the terms  $\beta_1$  and  $\beta_2$  should capture production optimization in the long term.

In estimating this equation however, a problem occurs due to the combination of a non-linear estimation and the presence of (non-zero) locational fixed effects. As McIntosh and Schlenker (2006) show, in the presence of non-linearity the fixed effects adjust for level *within group* whereas the quadratic terms are recovering a global relationship *across groups*. This implies that estimation will not recover the true parameters for  $\beta_1$  and  $\beta_2$  since the group means will enter into their identification. To resolve this, the argue for the inclusion of an additional ‘penalty’ term using the difference between a given weather realization  $x$  for location  $i$  at time  $t$ , and its long-run average and demonstrate that this allows valid estimation of the quadratic terms, ie. the long-run relationship between yield and a given weather outcome:

$$y_{it} = \alpha_i + \beta_1 x_{it} + \beta_2 x_{it}^2 + \beta_3 (x_{it} - \bar{x}_i)^2 + \beta_4 \delta_i + \beta_5 \vartheta_t + \varepsilon_{it} \quad (3)$$

Building on this insight, a number of subsequent authors<sup>41</sup> argue that in addition to allowing for the recovery of the parameters  $\beta_1$  and  $\beta_2$ , the penalty term itself can be usefully interpreted. Since the long-run average of a given weather outcome is the same as its expectation, the penalty term reflects the extent to which a given location’s realized weather in a given year

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<sup>41</sup> See Ortiz-Bobea (2021) for a summary

deviates from farmers' expectations. In a signal contribution to the literature, Mérel and Gammans (2021) demonstrate that equation (3) provides a valid weighted average of the responses to the long-run distribution (ie. the climate) and short-run annual realization (weather).

To illustrate this, begin from the assumption of a constrained farmer who can only set their allocations of labor and capital inputs based on their knowledge of the long-run distribution of weather outcomes. In such a case, their output will be defined purely by the long-run relationship, between that weather outcome and yield, since they have no ability to adjust for inter-annual variation (ie. their input allocation is always the same). As a result the estimated coefficient on the penalty term will be zero. Alternatively, if there exists some subset of actions which the farmer is able to take within a particular season based on observed weather, then they may be able to vary their input allocation in order to mitigate losses associated with individual weather realizations that deviate substantially from their long-run expectation.

The first stage of the analysis is therefore to estimate equations (2) and (3) using the full panel. I then add an additional term to the specification, representing an exogenous technological shock and interact it with the outcomes for weather:

$$y_{it} = \alpha_i + \beta_1 x_{it} + \beta_2 x_{it}^2 + \beta_3 (x_{it} - \bar{x}_i)^2 + \beta_4 z_{it} + \beta_5 x_{it} * z_{it} + \beta_6 x_{it}^2 * z_{it} + \beta_7 (x_{it} - \bar{x}_i)^2 * z_{it} + \beta_8 \delta_i + \beta_9 \vartheta_t + \varepsilon_{it} \quad (4)$$

Here the dummy variable  $z$  represents the introduction of HYV seeds to the district, taking the value 1 if a non-zero share of HYV seeds is reported, and 0 otherwise. Note that in this context, the introduction of HYV seeds was bundled with a range of advisory services and some provision of complementary inputs. The 'shock' should not therefore be thought of as purely driven by HYV seeds, but rather as a shift in the set of production technologies available to farmers. Disentangling the role of HYV versus other components is not empirically tractable, nor the objective of this paper. Rather the goal is to estimate whether the introduction of this set of technologies influenced the responsiveness of yields to deviations from their historical average. Under the null hypothesis that there was no difference in adaptation between adopting and non-adopting districts, the coefficient on the interaction terms should be zero and statistically insignificant.

Lastly, I carry out the same estimation using growing-degree days (GDD) in place of temperature while leaving the other variables in specification (4) unchanged. Since the relationship between plant growth and temperature exposure is non-linear, GDD are frequently used in the agronomic and phytological literature to capture the amount of time plants spend within a known optimal temperature band. I follow the approach of Schlenker and Roberts (2006) in using a sine interpolation to estimate fractional exposure times within daily minimum and maximum temperature reports, and then constructing total exposure using the following formula:

$$gdd(temp) \begin{cases} l & \text{if } temp \leq l \\ temp - l & \text{if } l < temp < h \\ h - l & \text{if } temp \geq h \end{cases}$$

Where  $gdd()$  is defined as a function of the daily observed temperature  $temp$ , with  $l$  and  $h$  representing a defined lower and upper bound representing the range of temperatures outside

of which plant growth is highly impaired. Wheat undergoes significant damage below freezing, and above temperatures of thirty-five degrees Celsius, hence I construct the GDD measure using the range 0-35°C (though in practice, only the upper bound is typically relevant for the Indian context) for each daily temperature value for each grid cell in the Met dataset, which I think average to aggregate to the district level<sup>42</sup>, and sum across the growing season to create a measure of annual cumulative beneficial temperature exposure, which is then used to estimate equation (4).

## 2.4 Results

### 2.4.1 Adaptation estimation without technology change

Table 2.1 presents the results of the initial estimation approach. I estimate the ‘naïve’ form (Equation 2) using only the quadratic for temperature, then include the penalty term (Equation 3) to estimate short-run adaptation. Both regressions are estimated with and without rainfall respectively, using the same functional form as for temperature in each case (Table 2.1).<sup>43</sup>

Table 2.1- Temperature-yield relationship with and without penalty term

	Log Yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temperature	0.02 (0.06) [0.787]	0.02 (0.06) [0.780]	0.00 (0.06) [0.936]	-0.04 (0.06) [0.463]
Temp ^ 2	-0.00 (0.00) [0.732]	-0.00 (0.00) [0.720]	-0.00 (0.00) [0.853]	0.00 (0.00) [0.609]
Penalty: Temp		0.01 (0.01) [0.396]		0.01 (0.01) [0.188]
Rainfall control	No	No	Yes	Yes
Fixed effects	State	State	State	State
Observations	12772	12772	12772	12772

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively. "Rainfall controls" indicates that the same quadratic specification as temperature (with or without penalty term) is included for precipitation.

As the results show, across specifications, the coefficient on each of the respective temperature variables is not statistically significant and close to zero. Reassuringly, despite the use of a longer panel and an alternative definition of growing season, these results are substantively the

<sup>42</sup> As Guiteras (2009) notes, it is important that the GDD measure is constructed prior to aggregation since if temperatures are not uniform within district, averaging will remove important variation in exposure to damaging temperatures.

<sup>43</sup> Full tables for all regressions with rainfall control estimates and intercept are provided in Appendix Tables 2.12-2.13.

same as those reported by Kumar and Khanna, who similarly report failing to identify a long-run relationship between temperature and productivity (the quadratic terms) or short-run adaptation (the penalty term).

This presents something of a puzzle. The lack of significance for the penalty term could indeed reflect evidence of absence, however the failure to observe a quadratic relationship to temperature in an almost seventy-year panel runs contrary to expectations, given that this is an agronomic relationship which is well understood. This suggests either substantial problems in measurement of either temperature or wheat yields (or both) which seems unlikely given the data sources, or that there is an alternative source of variation which affects productivity and is not being captured by the time or location fixed effects. In the following sections I present evidence to suggest that dissemination of HYV seeds presents a plausible candidate for an exogenous technological shock which was varied in its timing across locations, providing farmers in early-adoption districts with access to a different potential input set from those in late-adoption districts, thus biasing the temperature parameters in my initial specifications.

#### **2.4.2 Estimation of yield gains from HYV adoption**

The increase in yields in wheat as part of the ‘Green Revolution’ has been the subject of a longstanding literature (Dalyrymple, 1985; Gollin, Hansen & Wingender, 2021). The creation of dwarf wheat strains which resisted lodging (where the plant collapses beyond a certain height, resulting in crop losses) and the dissemination of these seeds and provision of farmer extension training on best practices for growing them led to a substantial increase in wheat yields. The mechanism for this has been extensively studied in plant genetics and agronomy. Attributing the relative contribution of HYV seeds themselves, the complementary inputs such as chemical fertilizer which were made available alongside HYVs, and the extension trainings farmers received on how to maximize yields using these technologies, is extremely difficult, and beyond the scope of this paper. Instead, I will demonstrate that the available district-level data is sufficient to demonstrate that the introduction of HYV seeds led to rapid gains in yields which are not plausibly explained by alternative explanations unless one can posit an alternative technology which was introduced in the same districts at the same times.

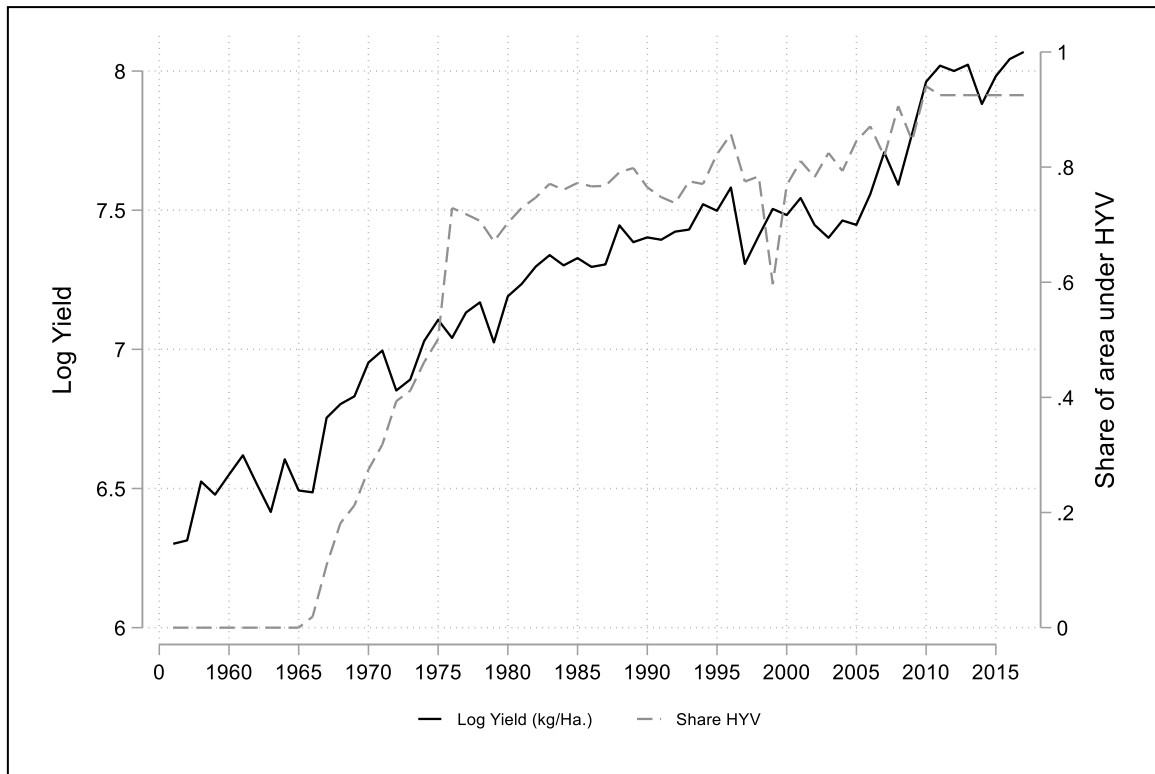
To illustrate this change descriptively, Figure 2.2 shows mean log wheat yields (left axis) and the mean share of cultivated wheat area planted with HYV seeds (right axis) for the included districts over the duration of the panel. HYV seeds for wheat were first introduced in 1966 in approximately half of the districts in our panel (105 of 206 districts) and had been disseminated to 99.5% of districts by 1976,<sup>44</sup> a period which coincided with a sharp increase in productivity in wheat.

*Figure 2.2 – HYV adoption and yield growth over time*

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<sup>44</sup> One remaining district reports initial adoption in 1979.





To test this relationship formally, I estimate a difference-in-differences regression, using the HYV introduction as a my ‘treatment’. To do so I construct a binary variable which takes the value one if a non-zero HYV share is reported by a district in any given year or in any prior years,<sup>45</sup> and zero if it the district has never reported HYV adoption. Initial HYV adoption took place in slightly more than 50% of the districts in my panel in 1966. A further 25% saw introduction between 1967 and 1975, with almost all of the remaining districts seeing initial adoption in 1976.<sup>46</sup> Since no district reports dis-adoption, each location unit remains ‘treated’ throughout the panel once it has been assigned as having adopted for a given year.

The staggered nature of HYV introduction, and the fact that it is universal across the panel (ie. there is no ‘control’ group to which one could compare individual treatment cohorts) present a problem for conventional difference-in-differences inference. As Goodman-Bacon (2021) shows, estimation of a simple difference-in-difference framework with two-way fixed effects with a time-varying treatment can lead to bias in estimation. Since the estimator of interest is a weighted average of all possible comparisons, variation in treatment timing can result in negative weights when there is variation in the treatment effect over time. To avoid this potential bias, I estimate effects for all potential pairwise comparisons across years, and aggregate them following the procedure described in Callaway and Sant’anna (2021).<sup>47</sup> Table 2.2 presents the results of these estimations, with aggregate estimates for the ten-year period before and after HYV introduction.

<sup>45</sup> From 2011 many districts which had fully adopted HYVs stopped reported land share under HYVs. I treat these as having adopted for the purposes of this analysis, though the results are not meaningfully affected by excluding these cases.

<sup>46</sup> One remaining district reports initial adoption in 1979.

<sup>47</sup> I implement this in Stata using the *csdid* package (Rios-Avila, Sant’anna, & Callaway, 2021).

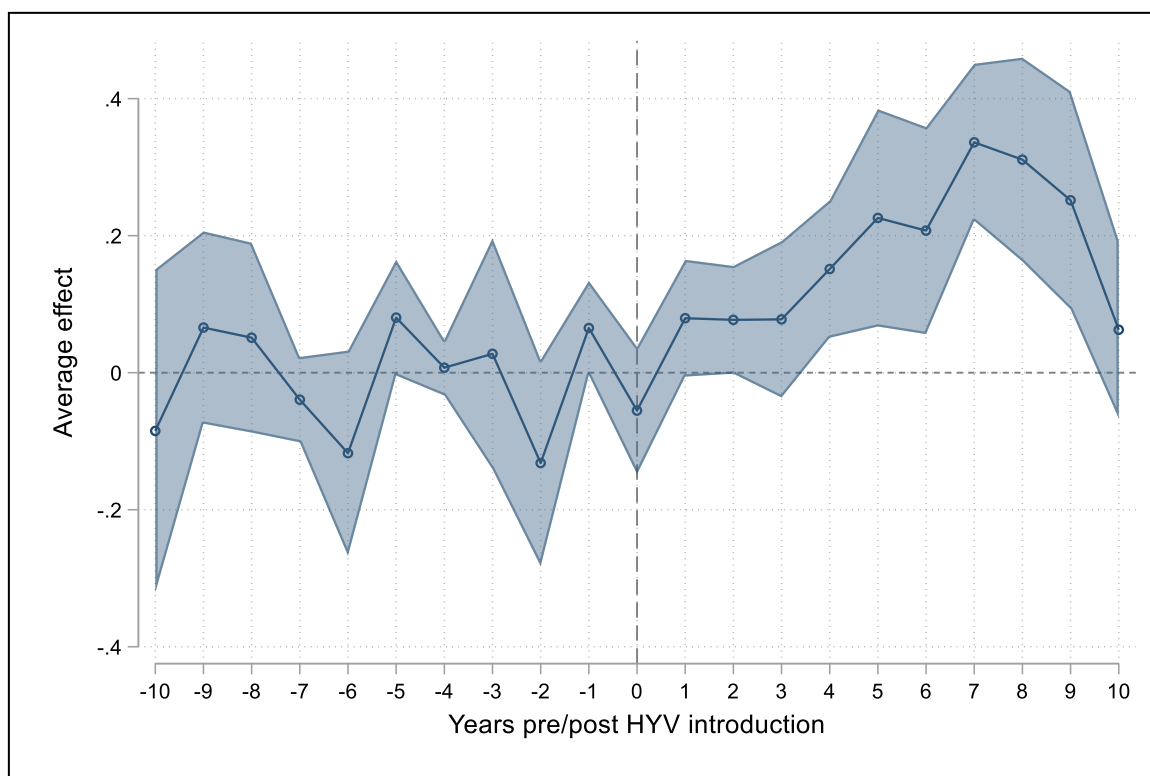
Table 2.2- Difference-in-difference estimate of effect of HYV adoption on yield

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Pre (10-year average)	-0.01 (0.01) [0.514]	0.01 (0.01) [0.474]	0.00 (0.02) [0.966]	0.00 (0.01) [0.968]
Post (10-year average)	0.16*** (0.05) [0.001]	0.09** (0.04) [0.047]	0.16*** (0.05) [0.001]	0.16*** (0.03) [0.000]
FE	State, year	State, year	State, year	State, year
Cohorts	All	All	1966/1976	1966/1976
Weather controls	No	Yes	No	Yes
Observations	4738	4738	3060	3060

Notes: Ordinary least squares regression with state and year fixed effects. Wild bootstrapped standard errors are reported in parenthesis, with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively. "Weather controls" indicates that quadratic and penalty terms for temperature and precipitation are included as dynamic controls. "All cohorts" indicates that the full panel is used, while "1966/1976" indicates that the sample is restricted to districts adopting in those two years (approximately 65% of the panel).

I estimate this with and without controls for temperature and precipitation, and also restricting the sample to those districts in which HYV seeds were introduced in 1966 and 1976 respectively. Across specifications, I find a positive and statistically significant effect of HYV introduction on yields, in the decade following adoption, equivalent to a 9-16 %-point increase. Across specifications, the coefficient for the average of the ten-year period prior to HYV introduction is insignificant and close to zero, suggesting that the introduction of the technology was responsible for these gains rather than a pre-existing trend.

Figure 2.3 – Plot of individual coefficient estimates



This is also the case for the individual year estimates. Figure 2.3 plots the coefficient estimates for each of the individual years aggregates into the pre- and post- periods in Specification 1 of Table 2.2. Prior to HYV introduction, ‘treated’ districts have no clear trend, and none of the point estimates is statistically significant. Following adoption, the coefficient is uniformly positive, and statistically significant for most one-year periods.

### 2.4.3 Comparing adaptation by HYV adoption status

The analysis so far has established two results. First, applying the penalized regression technique to the panel dataset finds no evidence of adaptation in Indian wheat production in the estimation period. Second, it has demonstrated that the introduction of HYV seeds and associated technologies significantly boosted wheat yields, and this increase is not explained by pre-existing trends. I aim now to test whether we can exploit the variation in the timing of the introduction of HYV technology to determine whether the spread of the Green Revolution affected farmers’ ability to respond to temperature shocks.

Returning to the simple theoretical model, the introduction of HYV seeds can be considered as an expansion in the set of available inputs to the production function over which the farmer has control. It is not necessarily clear *ex ante* how this would affect their ability to adapt to weather shocks. By increasing the set of available technologies they could potentially foster adaptation since farmers are able to try to optimize from a greater range of potential input allocations. Conversely, since the technology requires a range of complementary inputs to achieve yield gains, adopters may experience greater sensitivity to weather outcomes than when using traditional methods.

To explore whether HYV introduction affected farmers’ ability to adapt, I begin with a descriptive approach, simply splitting the sample into the period before HYV was fully

introduced (1951-1975) and the period after introduction (1976-2017)<sup>48</sup> and estimating Equation (3) for each. The results of this estimation are presented in Table 2.3.

*Table 2.3- Penalized regressions, pre- and post- HYV introduction in all districts*

	Log Yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temperature	0.05 (0.06) [0.386]	-0.01 (0.06) [0.863]	-0.00 (0.06) [0.994]	-0.06 (0.06) [0.366]
Temp ^ 2	-0.00 (0.00) [0.301]	-0.00 (0.00) [0.877]	-0.00 (0.00) [0.973]	0.00 (0.00) [0.457]
Penalty: Temp	-0.00 (0.01) [0.733]	-0.00 (0.01) [0.911]	0.02* (0.01) [0.086]	0.03** (0.01) [0.033]
Period	1951- 1975	1951- 1975	1976- 2017	1976- 2017
Rainfall control	No	Yes	No	Yes
Fixed effects	State, year	State, year	State, year	State, year
Observations	12772	12772	12772	12772

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively. "Rainfall controls" indicates that the same quadratic specification as temperature is included for precipitation.

The results for the period prior to introduction are same as for the overall sample- the coefficients on the quadratic terms are insignificant and close to zero, and the same for the penalty term- indicating that production did not respond differently to large deviations from expected temperature relative to realizations close to the historical average. For the post-introduction period, the long-run quadratic relationship is not identified, however there is a positive and statistically significant coefficient on the penalty term in specifications (3) and (4), indicating that following adoption there is some evidence to suggest farmers were able to adapt within season to temperature shocks, increasing their output under higher temperatures relative to the long-run average.

To formally test for this, I estimate Equation (4), interacting the indicator variable for HYV adoption with each of the temperature terms, and present the results in Table 2.4.

<sup>48</sup> As described above, one district did not see introduction until 1979, splitting by this year or dropping the district does not meaningfully alter estimation results.

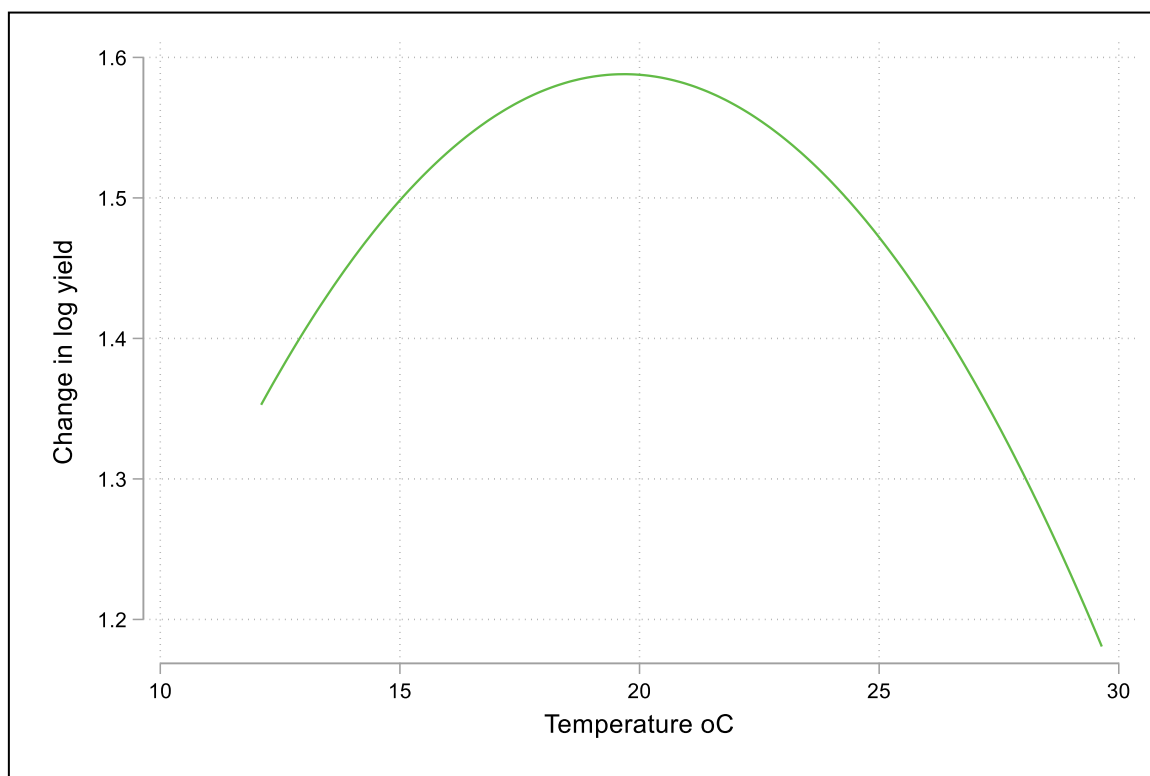
Table 2.4- Penalized regressions interacted with HYV adoption indicator

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Mean Temp	0.16** (0.07) [0.031]	0.16** (0.07) [0.024]	0.16** (0.07) [0.031]	0.11 (0.07) [0.120]
Mean Temp <sup>2</sup>	-0.00** (0.00) [0.024]	-0.00** (0.00) [0.019]	-0.00** (0.00) [0.024]	-0.00* (0.00) [0.079]
Penalty: Temp		-0.04** (0.02) [0.020]		-0.04** (0.02) [0.038]
Any HYV	1.78*** (0.53) [0.001]	1.85*** (0.52) [0.000]	1.78*** (0.53) [0.001]	1.91*** (0.52) [0.000]
Temp x Any HYV	-0.17*** (0.05) [0.001]	-0.17*** (0.05) [0.001]	-0.17*** (0.05) [0.001]	-0.18*** (0.05) [0.001]
Temp <sup>2</sup> x Any HYV	0.00*** (0.00) [0.001]	0.00*** (0.00) [0.001]	0.00*** (0.00) [0.001]	0.00*** (0.00) [0.001]
Penalty: Temp x Any HYV		0.06*** (0.02) [0.000]		0.06*** (0.02) [0.000]
Rainfall controls	No	No	Yes	Yes
Fixed effects	State, year	State, year	State, year	State, year
Observations	12772	12772	12772	12772

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*,\*\*,\*\*\* indicate significance at the 10%, 5% and 1% levels respectively. "Rainfall controls" indicates that the same quadratic specification as temperature (with or without penalty term) is included for precipitation.

These results demonstrate some important differences between temperature responses between districts where HYV was and was not introduced. For pre-introduction districts, the long-run relationship between yield and temperature is defined with the expected signs: a positive coefficient on temperature, and a negative coefficient on its square, and these estimates are significant across specifications with the exception of the coefficient on temperature in Specification 4 ( $p=0.12$ ). For these districts, output is primarily determined by this long-run relationship, which is in line with agronomic expectations based on the optimal temperature range for wheat. Output is increasing with temperature up to approximately 20°C, after which it begins to decline, with the rate of decline accelerating further from this optimum. Figure 2.4 plots this relationship.

Figure 2.4 –Plot of estimated relationship between yield effects of growing season temperature



In contrast, for districts where HYV had been introduced, the long-run relationship does not identify output- across each of the specifications the combined coefficients for the temperature and temperature square terms and their interaction approach zero and are not statistically significant. Hence, prior to adoption output in this estimation is determined primarily by the long-run temperature relationship, indicating limited ability of farmers to adapt in the short-run to unanticipated variation in temperature.

This can be observed from the coefficient on the penalty term in each case. For non-adopting districts the penalty term is statistically significant and negative: a one-degree deviation from the long-run temperature mean is associated with a 4% reduction in output. In contrast, farmers in districts where HYV has been introduced are better able to respond to these variations: a one degree deviation increase in temperature relative to the historical average is associated with a 2% increase in output ( $p=0.04$ ) in the specification without rainfall controls, and a 3% increase in output where rainfall controls are included ( $p=0.01$ ).

In addition to estimating on the extensive margin, I additionally estimate Equation 4 using the share of the total area cultivated with wheat under HYV. Since there are some issues with the reporting of the cultivated share in the ICRISAT dataset- some districts have years with implausibly low reported HYV shares between two years with high shares- I regress HYV share on yield with district-level fixed effects and use the predicted values to estimate HYV share. Table 2.5 presents these results.

Table 2.5- Penalized regressions interacted with share under HYV

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temperature	0.08 (0.06) [0.234]	0.08 (0.06) [0.226]	0.04 (0.06) [0.450]	0.04 (0.06) [0.440]
Temp ^2	-0.00 (0.00) [0.177]	-0.00 (0.00) [0.170]	-0.00 (0.00) [0.316]	-0.00 (0.00) [0.305]
Penalty: Temp		-0.01 (0.02) [0.358]		-0.01 (0.02) [0.434]
Share under HYV	2.07*** (0.65) [0.002]	2.09*** (0.65) [0.001]	2.43*** (0.63) [0.000]	2.46*** (0.63) [0.000]
Temp x HYV Share	-0.08 (0.06) [0.195]	-0.08 (0.06) [0.189]	-0.12* (0.06) [0.059]	-0.12* (0.06) [0.056]
Temp ^ 2 x HYV Share	0.00 (0.00) [0.165]	0.00 (0.00) [0.166]	0.00* (0.00) [0.051]	0.00* (0.00) [0.052]
Penalty: Temp x HYV Share		0.04** (0.02) [0.029]		0.05** (0.02) [0.018]
Rainfall controls	No	No	Yes	Yes
Fixed effects	State, year	State, year	State, year	State, year
Observations	12772	12772	12772	12772

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively. "Rainfall controls" indicates that the same quadratic specification as temperature (with or without penalty term) is included for precipitation.

Here, despite being an inherently noisier estimate, the results for short-run adaptation are similar to those observed for HYV introduction. In districts with no HYV adoption, the penalty term has the expected sign but the magnitude is close to zero and statistically insignificant. In contrast, in districts which fully adopted HYV seeds there is a five percentage point increase in yields associated with a one-degree increase in temperature relative to the historical average, in addition to the large gains in yields from full HYV adoption.

So far estimating these effects has been based on average growing season temperature. A concern with this approach is that it may mask important intra-seasonal variation, since a year with a constant temperature will be treated the same as one with substantial variability with the same mean value. As an alternative approach, I additionally re-estimate equations (3) and (4) using total growing degree days within the optimal range for wheat (0-35°C) in place of mean temperature (Table 2.6).

Table 2.6- Penalized regression specification with growing season degree days

	Log Yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
GDD (thousands)	-1.80 (1.16) [0.124]	-1.90 (1.15) [0.100]	-2.06* (1.15) [0.075]	-2.76** (1.08) [0.012]
GDD ^ 2	8.70 (6.64) [0.192]	10.20 (6.55) [0.121]	9.75 (6.52) [0.137]	14.08** (6.23) [0.025]
Penalty: GDD		-1.17* (0.67) [0.083]		-0.98 (0.67) [0.145]
Adopted HYV	0.10*** (0.03) [0.003]	0.10*** (0.03) [0.004]	0.10*** (0.04) [0.010]	0.02 (0.04) [0.605]
GDD x Adopted	0.62 (1.05) [0.557]	0.71 (1.05) [0.499]	0.73 (1.05) [0.491]	0.87 (0.99) [0.377]
GDD ^ 2 x Adopted	-2.89 (5.47) [0.599]	-4.55 (5.28) [0.390]	-3.30 (5.51) [0.551]	-6.07 (4.82) [0.210]
Penalty: GDD x Adopted		1.37** (0.68) [0.046]		1.40** (0.69) [0.046]
Rainfall control	No	No	Yes	Yes
Observations	12772	12772	12772	12772
Fixed effects	State, year	State, year	State, year	State, year

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively. "Rainfall controls" indicates that the same quadratic specification as temperature (with or without penalty term) is included for precipitation.

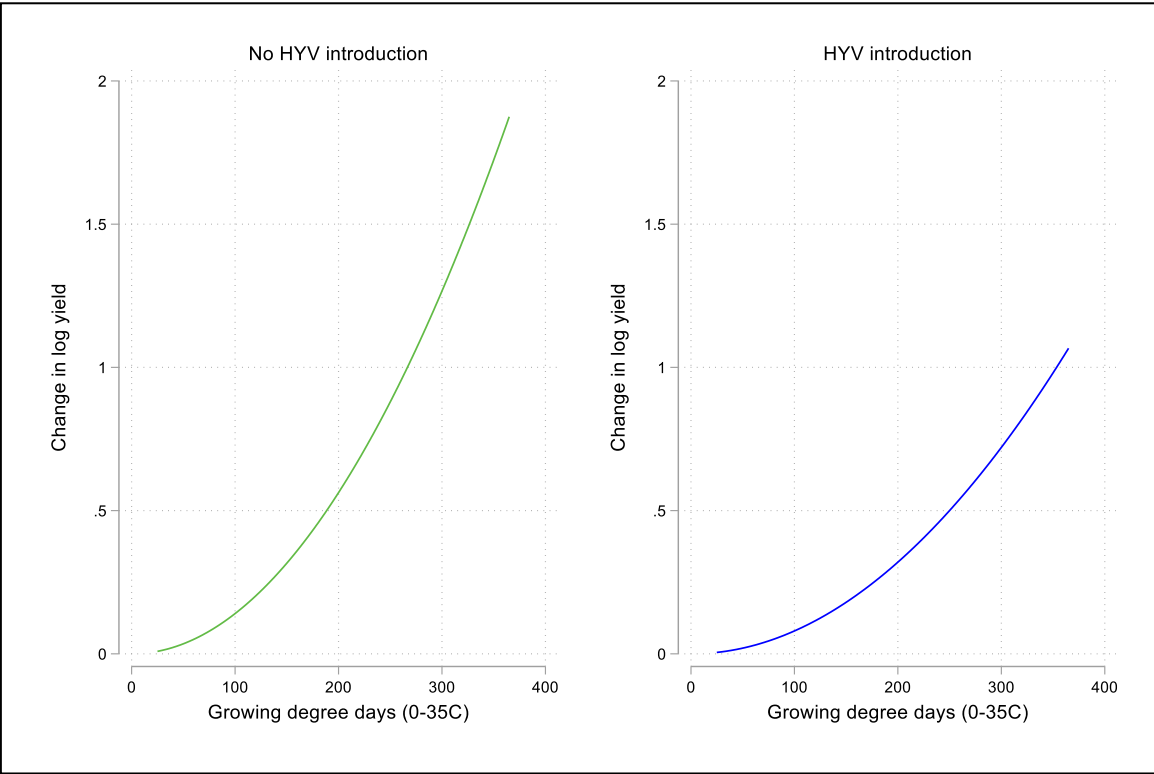
As Table 2.6 demonstrates, the relationship between yield and short-run adaptive behaviour is robust to this alternative specification. In terms of adaptation in the short-run to temperature shocks, a negative sign is again observed for pre-adoption districts on the penalty term, which is close to zero and only marginally significant when rainfall controls are not included. For HYV adopting districts, the effect is again positive and statistically significant, indicating that farmers in these areas were able to capitalize in relative terms on positive deviations in temperature from its long run average.

In terms of the long-run relationship between temperature and output, for districts in which HYV had not yet been introduced, the estimates for growing-degree days and their square are approximately consistent in magnitude across specifications, and statistically significant when the penalty term and rainfall controls are included. For districts in which HYV had been introduced, the combined coefficients for GDD and its square are also statistically significant ( $p=0.059$  and  $p=0.097$ ) respectively. In both cases the relationship follows the expected



behaviour, increasing non-linearly with the count of days in the growing season within the optimal range for wheat (Figure 2.5).

Figure 2.5 – Growing degree day relationship to yield, pre- and post-HYV introduction

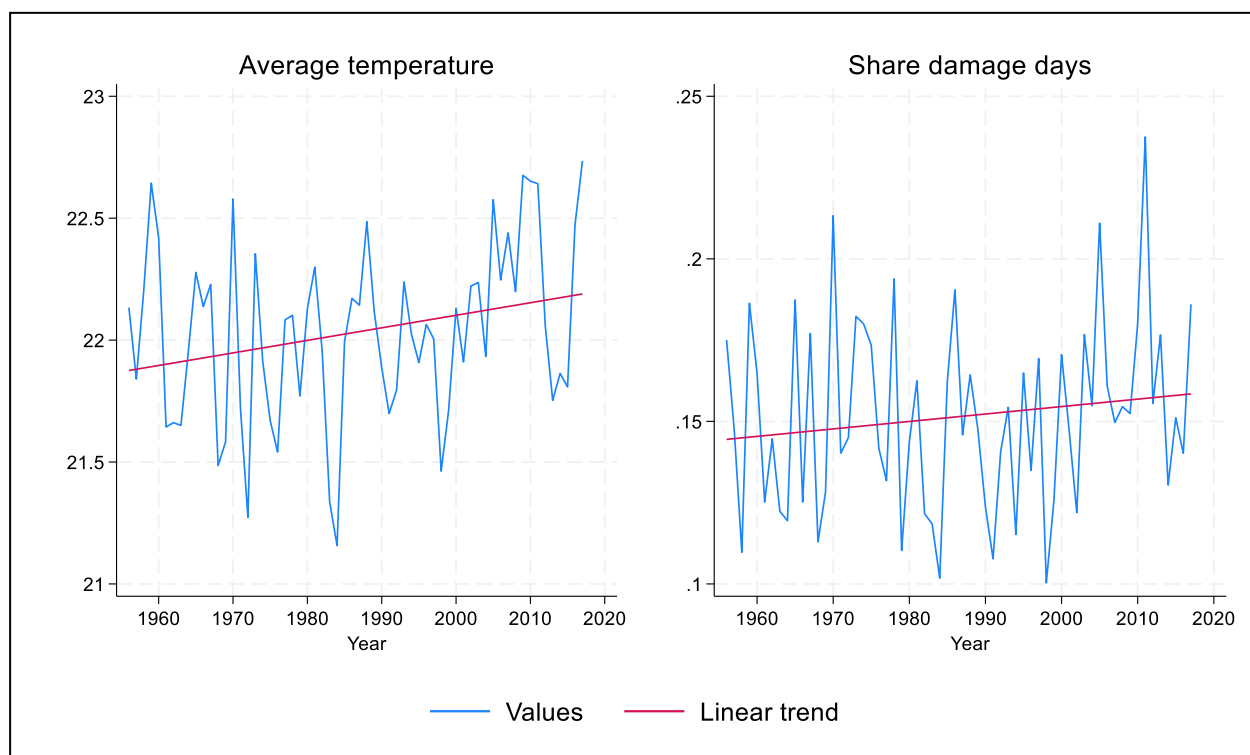


As Figure 2.5 shows, for pre-adoption districts, yield is far more sensitive to temperature over the course of the growing season. In contrast, the estimated marginal effect of an additional day within the optimal temperature boundary for wheat production is notably attenuated relative to districts where HYVs were not yet available.

Overall, the results of the analysis appear to favour an optimistic view of technology adoption. Not only does the uptake of HYV seeds lead to an overall large increase in yields, it also results in a greater relative resilience to temperature shocks. Pre-adoption, farmers’ output is lower and also determined to a greater degree by temperature and precipitation than following adoption. In contrast, uptake of HYV seeds appears to enable farmers to capitalize in to short-run positive temperature shocks by adjusting their practices within a season to increase output.

An important caveat to this analysis however is that each of these effects has been estimated based on retrospective data. The relationship between temperature and crop productivity is non-linear, and as described in the discussion of degree days dependent on temperatures falling within a defined range of values. Exposure to values outside of that range results in large scale losses. Since frost is not typically a concern in our context, this means in practice high levels of heat. As Figure 2.6 demonstrates, within the panel dataset the long-run temperature average, and the share of days where the temperature crosses above the upper boundary of 35°C for wheat is increasing across the panel.

Figure 2.6 - Panel trends in growing season temperature condition

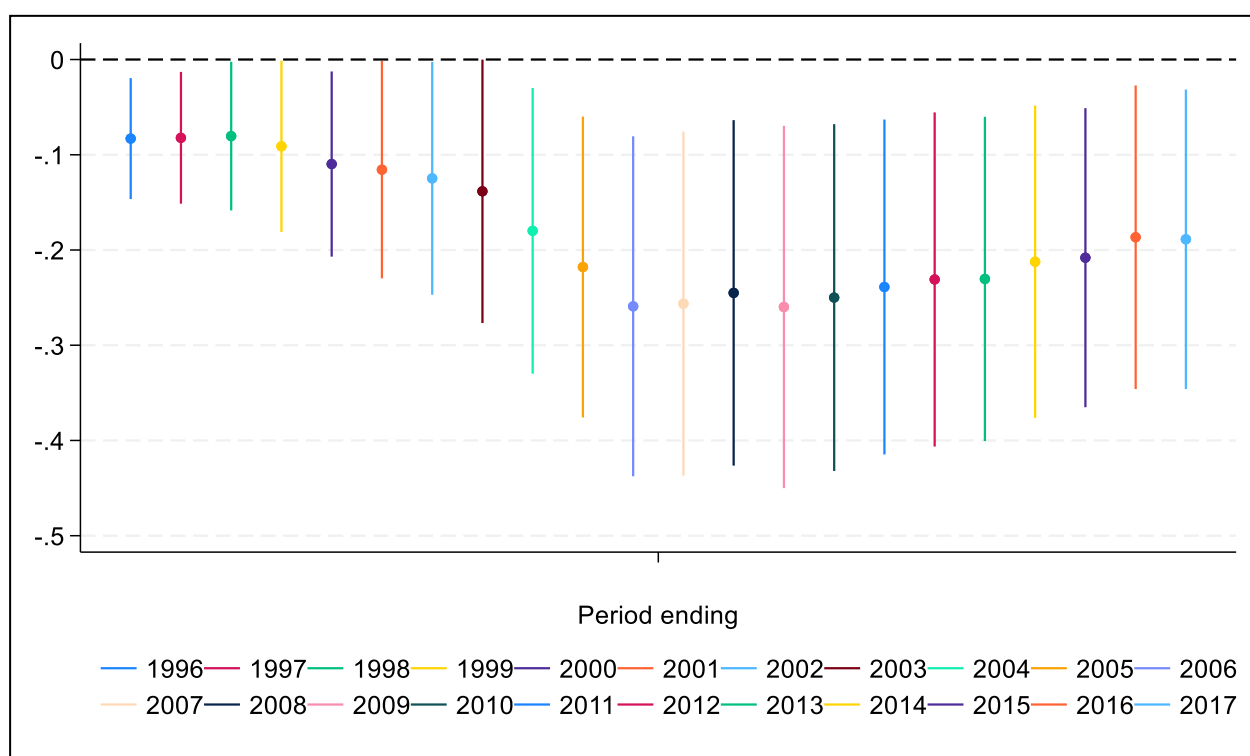


Absent very significant changes in current emissions trajectories, this trend is likely to continue in the coming decades. As a result, it is possible that many of the gains in resilience from the adoption of Green Revolution technologies may be undermined by an increase in the incidence of days with crop damaging temperatures. To explore this, I re-estimate Specification (4) from Table 2.5, and include an additional term to control for the share of growing season days in which the temperature was above 36°C at any time, and again interact this with HYV share. I split the sample into multiple rolling 30 year periods following HYV introduction,<sup>49</sup> and estimate each. I then plot the results for the effect of a 10%-point increase in the share of damage days and plot the coefficients in Figure 2.7.

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<sup>49</sup> Hence the specifications run from 1966-1996 to 1987-2017.

Figure 2.7 - Estimated response under HYV to 10%-pt increase in days > 35°C



As the figure shows, the proportional effect of an increase in damage days is increasing across the sample period, from an 8%-point decrease for the first period (1966-1996), to a 19%-point decrease for that last period (1987-2017). In relative terms, the sensitivity of overall output to extreme temperature days has increased across the span of the panel. Under the assumption that this effect remains constant for future years, the likely increase in the incidence of days exceeding 35°C is likely to reduce wheat production relative to a stable climate counterfactual where the probability of a day with crop-damaging temperatures is not increasing.

## 2.5 Conclusion

The analytical approach of this paper begins from the simple observation that farmers' production is determined by inputs within their control, in how they allocate labor and capital across a growing season, and by those inputs which they do not control, principally the weather outcomes they experience in that season, drawn from a climate distribution. Recent applied work drawing on climate econometrics has attempted to show how farmers adapt to both the climate over the long-run, and inter-annual variation in weather outcomes in the short-run. This approach has been primarily applied to high-income countries which are already close to the potential production frontier, with widespread functional credit markets.

I apply this approach to India, a large developing economy with whose agricultural sector plays an important role in its domestic and the international economy. I demonstrate that naïve estimation of climate adaptation will not properly identify adaptive responses to temperature in either the short- or long-run if it does not properly account for historical shocks to available technology. I exploit variation in the timing of the dissemination of HYV seeds and associated technologies as part of the Green Revolution to demonstrate how improved access to productive technologies reduced the extent to which production was determined by weather alone, and enabled farmers to adapt to positive short-run temperature shocks by expanding the set of

available input technologies. This improved resilience was a significant additional benefit of the Green Revolution, however it is important to recognize that retrospective analysis may be a limited guide to future outcomes as global temperatures increase. The evidence suggests that vulnerability to extreme temperatures is increasing across the study period. If we assume that this trend continues, future productivity will be increasingly impacted unless new resilience technologies can be developed and disseminated.

This analysis highlights both the promise and the peril of adapting agriculture to climate change in a developing country context. It demonstrates that Green Revolution technology not only increased the average output of wheat producers in India, it increased their capacity to respond to fluctuations in temperature outside of historical norms. The data also however demonstrate the non-linear nature of temperature effects on production, and the increasingly pernicious effects of extreme temperatures on output. While raising aggregate production in low-income contexts is important in ensuring global food security, the tails of the weather distribution are likely to play an increasing role in determining welfare under anthropogenic climate change. Greater attention must also be paid to understanding the range of adaptive strategies available to different groups of farmers, as those who are furthest from the possible production frontier are also often those most vulnerable to changing weather conditions as the world warms.

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## 2.7 Appendix Tables

Table 2.7- Table 2.1 estimated with alternative yield construction

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temp	0.02 (0.06) [0.714]	0.02 (0.06) [0.719]	0.01 (0.06) [0.831]	-0.04 (0.06) [0.514]
Temp ^ 2	-0.00 (0.00) [0.660]	-0.00 (0.00) [0.672]	-0.00 (0.00) [0.754]	0.00 (0.00) [0.654]
Penalty: Temp		-0.01 (0.01) [0.603]		-0.00 (0.01) [0.997]
Rain			-0.11*** (0.04) [0.003]	-0.37*** (0.10) [0.000]
Rain ^ 2			0.03*** (0.01) [0.004]	0.02*** (0.01) [0.005]
Penalty: Rain				0.33*** (0.09) [0.000]
Constant	7.06*** (0.59) [0.000]	7.06*** (0.59) [0.000]	7.21*** (0.59) [0.000]	7.93*** (0.62) [0.000]
Fixed effects	State, year	State, year	State, year	State, year
Observations	12769	12769	12769	12769

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*,\*\*,\*\*\* indicate significance at the 10%, 5% and 1% levels respectively.

Table 2.8- Table 2.2 estimated with alternative yield construction

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Pre (10-year average)	-0.01 (0.01) [0.518]	0.01 (0.01) [0.371]	0.00 (0.02) [0.966]	0.00 (0.01) [0.968]
Post (10-year average)	0.15*** (0.05) [0.001]	0.07* (0.04) [0.064]	0.15*** (0.05) [0.002]	0.14*** (0.03) [0.000]
FE	State, year	State, year	State, year	State, year
Cohorts	All	All	1966/1976	1966/1976
Weather controls	No	Yes	No	Yes
Observations	4735	4735	3060	3060

Notes: Ordinary least squares regression with state and year fixed effects. Wild bootstrapped standard errors are reported in parenthesis, with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively. "Weather controls" indicates that quadratic and penalty terms for temperature and precipitation are included as dynamic controls. "All cohorts" indicates that the full panel is used, while "1966/1976" indicates that the sample is restricted to districts adopting in those two years (approximately 65% of the panel).



Table 2.9- Table 2.3 estimated with alternative yield construction

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temperature	0.14*	0.14**	0.12	0.04
	(0.07)	(0.07)	(0.07)	(0.07)
	[0.059]	[0.050]	[0.104]	[0.581]
Temp ^ 2	-0.00*	-0.00**	-0.00*	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
	[0.054]	[0.046]	[0.091]	[0.478]
Penalty: Temp	1.57***	1.63***	1.41**	1.01
	(0.57)	(0.57)	(0.61)	(0.64)
	[0.006]	[0.004]	[0.021]	[0.117]
Adopted HYV	-0.14**	-0.15***	-0.13**	-0.09
	(0.06)	(0.06)	(0.06)	(0.06)
	[0.013]	[0.010]	[0.032]	[0.140]
Temp x Adopted	0.00**	0.00**	0.00**	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
	[0.016]	[0.012]	[0.036]	[0.151]
Temp ^ 2 x Adopted		-0.04**		-0.04**
		(0.02)		(0.02)
		[0.015]		[0.046]
Penalty: Temp x Adopted		0.05***		0.05***
		(0.02)		(0.02)
		[0.003]		[0.005]
Rain			-0.13***	-0.46***
			(0.05)	(0.10)
			[0.004]	[0.000]
Rain ^ 2			0.03*	0.02
			(0.02)	(0.02)
			[0.079]	[0.214]
Penalty: Rain			0.03	0.10
			(0.06)	(0.07)
			[0.617]	[0.155]
Rain x Adopted			0.00	0.01
			(0.02)	(0.02)
			[0.891]	[0.697]
Rain ^ 2 x Adopted				0.43***
				(0.10)
				[0.000]
Penalty Rain x Adopted				-0.12*
				(0.07)
				[0.091]
Constant	5.79***	5.73***	6.06***	7.11***
	(0.74)	(0.74)	(0.73)	(0.71)
	[0.000]	[0.000]	[0.000]	[0.000]

Fixed effects	State, year	State, year	State, year	State, year
Observations	12769	12769	12769	12769

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively.

Table 2.10- Table 2.1 estimated using national-level growing season

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temp	0.07 (0.05) [0.158]	0.07 (0.05) [0.157]	0.06 (0.05) [0.199]	0.05 (0.05) [0.294]
Temp ^ 2	-0.00 (0.00) [0.125]	-0.00 (0.00) [0.122]	-0.00 (0.00) [0.148]	-0.00 (0.00) [0.208]
Penalty: Temp		0.01 (0.01) [0.127]		0.02* (0.01) [0.068]
Rain			-0.08*** (0.03) [0.005]	-0.18*** (0.06) [0.001]
Rain ^ 2			0.00 (0.00) [0.172]	0.01** (0.01) [0.033]
Penalty: Rain				0.14*** (0.04) [0.000]
Constant	6.63*** (0.50) [0.000]	6.63*** (0.50) [0.000]	6.80*** (0.49) [0.000]	7.04*** (0.49) [0.000]
Fixed effects	State, year	State, year	State, year	State, year
Observations	12772	12772	12772	12772

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively.

Table 2.11- Table 2.3 estimated using national-level growing season

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temperature	0.16** (0.07) [0.016]	0.17** (0.07) [0.011]	0.14** (0.07) [0.033]	0.14** (0.06) [0.031]
Temp ^ 2	-0.00** (0.00) [0.012]	-0.00*** (0.00) [0.008]	-0.00** (0.00) [0.025]	-0.00** (0.00) [0.021]
Penalty: Temp		-0.06*** (0.02) [0.002]		-0.04** (0.02) [0.021]
Adopted HYV	1.29*** (0.49) [0.009]	1.38*** (0.48) [0.005]	1.05** (0.49) [0.033]	1.15** (0.49) [0.020]
Temp x Adopted	-0.12** (0.05) [0.014]	-0.12*** (0.05) [0.009]	-0.10** (0.05) [0.037]	-0.11** (0.05) [0.022]
Temp ^ 2 x Adopted	0.00** (0.00) [0.015]	0.00*** (0.00) [0.009]	0.00** (0.00) [0.039]	0.00** (0.00) [0.025]
Penalty: Temp x Adopted		0.09*** (0.02) [0.000]		0.08*** (0.02) [0.000]
Rain			-0.16*** (0.03) [0.000]	-0.26*** (0.07) [0.000]
Rain ^ 2			0.02*** (0.00) [0.001]	0.02*** (0.01) [0.003]
Penalty: Rain				0.19*** (0.06) [0.004]
Rain x Adopted			0.11*** (0.03) [0.000]	0.11** (0.04) [0.012]
Rain ^ 2 x Adopted			-0.02*** (0.01) [0.002]	-0.01* (0.01) [0.058]
Penalty Rain x Adopted				-0.07 (0.05) [0.168]
Constant	5.57*** (0.70) [0.000]	5.49*** (0.70) [0.000]	5.94*** (0.68) [0.000]	6.08*** (0.67) [0.000]

Fixed effects	State, year	State, year	State, year	State, year
Observations	12772	12772	12772	12772

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% levels respectively.

Table 2.12- Results with all controls for Table 2.1

	Log Yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temperature	0.02 (0.06) [0.787]	0.02 (0.06) [0.780]	0.00 (0.06) [0.936]	-0.04 (0.06) [0.463]
Temp ^ 2	-0.00 (0.00) [0.732]	-0.00 (0.00) [0.720]	-0.00 (0.00) [0.853]	0.00 (0.00) [0.609]
Penalty: Temp		0.01 (0.01) [0.396]		0.01 (0.01) [0.188]
Rainfall			-0.13*** (0.03) [0.000]	-0.38*** (0.10) [0.000]
Rainfall ^ 2			0.03*** (0.01) [0.001]	0.03*** (0.01) [0.003]
Penalty: Rainfall				0.32*** (0.09) [0.001]
Constant	7.14*** (0.59) [0.000]	7.14*** (0.59) [0.000]	7.32*** (0.58) [0.000]	8.01*** (0.62) [0.000]
Fixed effects	State, year	State, year	State, year	State, year
Observations	12772	12772	12772	12772

Notes: Ordinary least squares regression with state and year fixed effects. Standard errors are clustered at the district level and reported in parenthesis with p-values in square brackets. \*,\*\*,\*\*\* indicate significance at the 10%, 5% and 1% levels respectively.

Table 2.13- Results with all controls for Table 2.3

	Log yield (kg/Ha.)			
	(1)	(2)	(3)	(4)
Temperature	0.05 (0.06) [0.386]	-0.01 (0.06) [0.863]	0.01 (0.07) [0.875]	-0.05 (0.07) [0.453]
Temp ^ 2	-0.00 (0.00) [0.301]	-0.00 (0.00) [0.877]	-0.00 (0.00) [0.823]	0.00 (0.00) [0.571]
Penalty: Temp	-0.00 (0.01) [0.733]	-0.00 (0.01) [0.911]	0.02 (0.01) [0.130]	0.02* (0.01) [0.093]
Rain		-0.38*** (0.10) [0.000]		-0.41*** (0.13) [0.002]
Rain ^ 2		0.02 (0.01) [0.156]		0.03** (0.01) [0.025]
Penalty: Rainfall		0.37*** (0.09) [0.000]		0.32*** (0.12) [0.007]
Constant	6.28*** (0.61) [0.000]	7.19*** (0.61) [0.000]	7.33*** (0.68) [0.000]	8.26*** (0.75) [0.000]
Fixed effects	State	State	State	State
Period	1951- 1975	1951- 1975	1976- 2017	1976- 2017
Observations	4120	4120	5150	5150

# Consumer Responses to Food Safety Risk Information

*Co-authored with Vivian Hoffmann & Sarah Kariuki*

## **Abstract**

Unsafe food imposes significant health and productivity burdens on developing countries. We test a simple experimental intervention by providing low-income urban consumers in Kenya with risk information on maize flour from the formal and informal sector. We find a 42 percent increase in the share of households consuming the lower risk variety at follow-up in the treatment group relative to controls, from a base of 33%. Our results demonstrate the potential for low-cost informational interventions to increase the salience of food safety as a product attribute in informal markets or where regulatory enforcement is weak.



### 3.1 Introduction

Foodborne illness is responsible for a large share of the global burden of disease, comparable in magnitude to HIV/AIDS, malaria and tuberculosis, with approximately one third of diarrheal disease transmitted through food (WHO 2015; Hald et al. 2016). These impacts are primarily felt by citizens of low- and middle-income countries, yet solutions for improving food safety in these contexts have received comparatively little attention. The food system in developing economies is characterized by a substantial informal sector, which is by its nature outside of the reach of state regulatory systems.

Like many low and middle-income countries, Kenya has strict *de jure* food safety regulations, based on risk analysis conducted by and for high-income countries, but weak enforcement. In settings where the advanced processing technology and near-universal access to cold chains and clean water infrastructure required to meet these standards are lacking, food safety violations can be the norm rather than the exception (Grace, 2015). A food sector made up of many small firms, low public budgets, and a complex and sometimes contradictory division of responsibilities across levels of government and line ministries compound the challenge of effective food safety surveillance and enforcement. In the case of the product we study, maize flour, four different government agencies are responsible for food safety compliance from farmer to consumer. Specific responsibilities are poorly defined, leading to poor overall enforcement (Hoffmann, Alonso, & Kang'ethe, 2023a). In this context, steering consumer demand toward safer products can be a complementary approach to the enforcement of food safety regulations at production and retail.

In this paper, we test whether providing information about the relative risk associated with a product category can influence consumers' choice of product. We implemented a randomized controlled trial with low-income consumers in urban settlements in Nairobi, Kenya. Treated households were provided with information on the prevalence of aflatoxin contamination, a serious food safety hazard, in formally and informally milled maize flour, which is a common staple in the study area. Providing this information led consumers to substitute lower risk packed flour in place of higher risk 'posho' flour from the informal sector.

In economic terms, food safety risk represents a classic negative externality. Contamination of food with pathogens, toxins or other health hazards can occur at any point in the production, processing, storage, transportation and distribution of food. In contrast to other negative product attributes such as bruising or spoilage, food safety hazards are not generally observable to consumers. In addition, illness or disability caused by contaminated food can be hard for buyers to attribute to a particular product, hence market prices fail to account for these hazards. Health costs are imposed on both affected individuals and public health systems, but where attribution is not possible or where legal systems are unable to transfer these costs to producers and retailers, their incentives to invest in food safety will be weak.

This study focuses on one such hazard: aflatoxin contamination in maize flour. Aflatoxins are a group of carcinogens (Strosnider et al. 2006), produced by *Aspergillus flavus*, a fungus endemic in soils which commonly affects stored crops including maize, and which are prevalent in the tropics. Consumption of aflatoxins can cause acute liver diseases and is suspected of stunting children's growth and suppressing immune function (Turner, 2013).

We implemented the study in low-income informal settlement areas in urban and peri-urban Nairobi. This is a context in which maize plays an important role in diets as an affordable staple,

and where aflatoxin contamination is prevalent. Enrollment was restricted to households who had recently consumed informally milled flour, and which included young children, since while aflatoxin poisoning has deleterious effects at an age, it is most pernicious for young children for whom it can cause additional impairment of their development.

Treated households were provided with information about the health risks associated with aflatoxin contamination and on the relative risk of contamination in formally milled packaged flour versus informally milled wholemeal ‘posho’ flour. While in general the track record of regulatory effectiveness in the formal sector in developing economies is mixed (Grace, 2015), previous research in Kenya has shown the processed and packaged maize flour is consistently less likely to be contaminated with aflatoxin than informally marketed posho flour, apparently due to the fact that the most contaminated elements of maize are removed during the formal milling process (Kariuki and Hoffmann, 2022; Hoffmann et al., 2023a).

Information provided through the experiment was based on data from a national-level aflatoxin monitoring project conducted in Kenya during the year prior to the study. Over a one-year period, samples of formally milled and posho flour were purchased by members of the research team every two months from ten distinct sampling areas designed to capture the major commercial sites for maize retail in Kenya. At each location, purchases of multiple brands were made from multiple retailers every two months, and five packaged flour samples and five posho flour samples were tested to determine their level of aflatoxin contamination. Full details of the study are described in Hoffmann et al. (2023b). We make use of data from this study from the Nairobi sampling site to provide participants with recent and accurate information representing the level of aflatoxin risk they face in local markets.

We find that households assigned to the information treatment were 14 percentage points more likely to have lower risk formally milled flour present in their home at endline than those assigned to the control group, an increase of 42% relative to the control group level of 33%. Treated individuals’ perceptions of the risks associated with both formal and informal flour were higher than those assigned to the control group on both the intensive and extensive margins. Effects on risk perceptions were stronger with regard to informally milled flour, in line with the relative risk information provided. We additionally observe a small but non-zero increase in perceived riskiness of maize flour among the control group, suggesting some spillover of information between treatment groups.

Within the treatment group, participants were divided into two sub-treatments with equal probability. Both groups received information about the risks associated with aflatoxin contamination, and a recommendation to purchase packaged flour if they wished to avoid aflatoxin risk. One group (T1, *Relative Risk Information Only*) was given only information on the relative risk of contamination in packaged versus posho flour, while the other (T2, *Full Risk Information*) was additionally informed of the probability that aflatoxin in posho flour exceeded the regulatory threshold in surveillance data.

Comparing the two versions of this treatment, we do not find differences in terms of impacts on purchasing behavior. We do however find that the *Relative Risk Information Only* treatment had a larger impact on the risk perceptions of participants than the *Full Risk Information*. This finding suggests that a negative recommendation without a specified risk level, may lead consumers to over-estimate the risk of a health hazard. In the context of this study, however, this overestimation has no detectable impact on behaviour.

We test for heterogeneity with respect to participants' baseline subjective probabilities of the risks associated with each product type. We find that the effects of the information treatment on the perceived risk associated with informal flour are driven by those respondents who reported being unaware of any such risk at baseline, in line with a standard model of learning. The treatment effect on purchase behaviour does not differ according to baseline beliefs. We conduct additional heterogeneity tests based on household income level at baseline, and find that treatment impacts are similar for those above versus below the sample median income level.

Our findings contribute to the literature on consumer demand for safer food. Much of the previous experimental work on this topic, as reviewed by Hoffmann, Moser and Saak (2019) is based on single-interaction experiment. The results of such experiments are likely to be affected by salience of food safety in the moment that participants make a choice. A recent exception is a study by Kariuki et al. (2023), in which participants were visited 2 months after they were given feedback on the contamination of maize flour they had in their homes. While that study showed a lasting impact on behaviour of informing consumers about contamination in the food they were consuming, the cost of scaling up such an approach would be prohibitive. The present paper shows that communicating relative risk information from routine surveillance can similarly motivate consumers to choose safer food.

We demonstrate that providing information on relative risk can make food safety a salient product attribute for low-income consumers and can lead to substantial changes in their purchasing behavior. This approach may be attractive to policymakers in settings where it is either infeasible or undesirable to remove food that does not comply with food safety regulations from the market. Beyond an immediate reduction in exposure among those consumers who follow recommendations, the negative demand shock to riskier foods could be expected to lead to a contraction in its supply.

## **3.2 Data**

### **3.2.1 Sampling**

The sample was assembled from lists provided by community health volunteers (CHVs) in four low-income residential areas in Nairobi County.<sup>50</sup> CHVs are responsible for maintaining contact with all households in their assigned area that include a child under five years of age or an expectant mother. Enumerators visited randomly selected households in person to confirm that there was a young child still living in the household and asked the respondent whether they had purchased posho flour in the past two weeks. If both conditions were met, the household was eligible for inclusion in the study and the enumerator proceeded to administer informed consent and conduct the baseline interview.<sup>51</sup>

### **3.2.2 Household interviews**

Baseline interviews were conducted with the member of the household identified as the primary caregiver of children in the home. Respondents provided data on the composition of their

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<sup>50</sup> There were: Kawangware, Kangemi, Kibera and Athi River

<sup>51</sup> We additionally screened for respondents reporting symptoms associated with Covid-19, and excluded households where members had recently reported symptoms and had not recently tested negative for the disease.

household and their food consumption as well as information about their social network (to capture spillovers), their knowledge of food safety issues, and their risk preferences. At the end of the baseline interview, households assigned to treatment received information as described below. Follow-up interviews eliciting the same information (aside from social networks module) were conducted approximately two months later.

### **3.2.3 Consumption data**

As part of the interview, the respondent was asked a short series of questions about types of maize flour which had been consumed by the household in the previous week. Households were asked about their consumption of packaged flour and posho flour separately, as well as a range of starches which could plausibly function as substitutes: whole maize grains; flour from other grains; breads; potatoes and sweet potatoes; cassava and rice. Enumerators asked to see any posho or packaged maize flour was present in the household, and for permission to photograph it.<sup>52</sup> We use this observational information, rather than the respondent's reported consumption, to prevent potential researcher demand effects for treated households, though in practice the two measures are very similar.

### **3.2.4 Eliciting subjective probabilities**

In both rounds of interviews, we collected data on participants' subjective perceptions of food safety risk. Each participant was first asked whether they were aware of any "food safety threats or concerns" regarding maize flour. If they responded affirmatively, we asked a series of follow-up questions to measure their knowledge of food safety risks, focusing on whether they named aflatoxin as a risk, or described potential negative health outcomes from consuming maize flour consistent with its effects. We then elicited subjective probabilities by asking them to estimate the risk associated with the problem they mentioned for packaged and posho flour.<sup>53</sup> To convey probabilities, participants were provided with beans to use as tokens which they could then use to express a likelihood by placing an amount corresponding to the likelihood of an outcome inside a circle drawn by the enumerator, and placing the remaining amount outside of the circle to indicate the converse. This elicitation method has been used in a variety of contexts where access to formal education is limited, and has been demonstrated to be robust to a number of variations in experimental design (Delavande, Gine, & McKenzie, 2011). We treat those who said they were not aware of any food safety risk as expressing zero probability of a risk in either flour type.

### **3.2.5 Treatment Assignment**

Assignment to treatment was implemented ahead of the baseline survey in two stages. In the first stage, each CHV was randomly assigned to one of two treatment statuses: either 50% (low intervention density) or 100% (high intervention density). In the latter case, every household for which that CHV was responsible was included in the sampling frame, while for the low density treatment half of the listed households were randomly selected for inclusion, and the

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<sup>52</sup> Some households stored formally milled flour in containers other than its original packaging. In such cases the enumerators were able to verify the source based on the consistency of the flour which is noticeably finer and has a more uniform consistency than flour from posho mills.

<sup>53</sup> To prevent anchoring bias in the survey responses, we randomly varied which type of flour was asked about first.

remainder excluded. Since CHVs are assigned to households geographically, this assignment generated exogenous geographical variation in the share of households included in the study, allowing us to explore potential spillovers.

In the second stage, households were stratified by CVH and randomly assigned to one of three groups with equal probability: *Control*, *Relative Risk Information Only* (T1) and *Full Risk Information* (T2). At the conclusion of the baseline interview, households assigned to either of the two treatment arms were provided with information on aflatoxin risk in maize flour. Enumerators read a standard treatment-specific script in either Swahili or English (based on the respondent's preference) and then provided them with a treatment-specific poster summarizing the key information, which they were encouraged to keep and display in their home.<sup>54</sup>

The script for both treatment groups informed participants that aflatoxin cannot be observed visually, and that it has negative health effects, particularly in young children. Participants in both groups were also told that a previous study had found that branded packaged maize flour (unga) contained less than half the level of aflatoxin as flour from posho mills. and given the advice that “if you want to reduce the risk that your family is exposed to aflatoxin, you can do so by buying packaged maize flour”. This sentence was also included on the posters that both groups received, along with a background showing locally available packaged flour brands.

In T2 households, the participant received additional information on the prevalence of aflatoxin contamination in posho flour. The data from the monitoring study found that 25% of posho sample from the Nairobi area had an aflatoxin contamination level in excess of the regulatory limit. This was conveyed to participants as “one in four tins of posho flour contain more than the legal limit for aflatoxin”, which was also included as text on the posters received by T2 households.

### 3.3 Statistical analysis

To estimate the effects of the treatment, we follow the registered pre-analysis plan.<sup>55</sup> Our primary outcome of interest is the share of participants who have lower-risk (formally milled) flour for consumption present in the household at the time of the endline survey. This is coded as a binary variable, taking a value of 1 if formally milled maize flour (or its packaging material) is observed and 0 otherwise. We use this observational measure (rather than household's reported consumption) to avoid bias from enumerator demand effects, though it should be noted that observed presence may understate the level of consumption since some respondents may consume a given flour type but not have any stored within the household at time of interview.<sup>56</sup>

We consider two pre-specified secondary outcomes of interest: respondents' subjective probability of aflatoxin contamination in lower- and higher- risk flour types, and total monetary expenditure per household member (using adult equivalent weights) on non-maize starches. Estimating the effect on individuals' subjective probabilities allows us to see whether the treatment led participants to update their risk beliefs, which in turn led them to update their purchasing decisions. We measure the effect on non-maize purchases to test for potential substitution of other starches for maize flour. Since the distribution of quantities is

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<sup>54</sup> Copies of the English language versions of the script are included in Appendix A.

<sup>55</sup> The AEA registered pre-analysis plan is available at <https://www.socialscicenter.org/trials/9075>

<sup>56</sup> Respondents could also decline to show the enumerator the flour, though in practice this was rare.

approximately log-normally distributed, we apply the log transformation to this outcome and additionally estimate a separate logistic regression to test whether the treatment affected the proportion of households reporting zero consumption of other starches.<sup>57</sup>

For each of our outcomes we estimate the following equation to capture the average treatment effect of receiving any information:

$$Y_{i,t=1} = \beta_0 + \beta_1 \text{AnyInformation}_{i,t=1} + \gamma_1 z'_i + \theta_i + \epsilon_i \quad (1)$$

Where  $Y_{i,t=1}$  is the outcome of interest for person  $i$  at time  $t$ , and *AnyInformation* is an indicator that takes the value 1 if the household was assigned to either information treatment, and 0 otherwise. We include a vector of control variables,  $z'_i$ , selected via post double-selection LASSO (Belloni, Chernozhukov, & Hansen, 2014) from the set of candidate controls listed in our pre-analysis plan.<sup>58</sup>

We also estimate the effect of each of the individual information treatments (with and without absolute risk information). To avoid bias by estimating a linear model with multiple treatment arms (Goldsmith-Pinkham et al., 2022) we estimate three separate forms of equation 1: two in which we include the control and the respective information treatment, and one in which we exclude the control group and compare the *Full Risk Information* treatment to the *Relative Risk Information Only* treatment, as specified below.,

$$Y_{i,t=1} = \beta_0 + \beta_1 \text{RelativeInformation}_{i,t=1} + \gamma_1 z'_i + \theta_i + \epsilon_i \quad (2a)$$

$$Y_{i,t=1} = \beta_0 + \beta_1 \text{FullInformation}_{i,t=1} + \gamma_1 z'_i + \theta_i + \epsilon_i \quad (2b)$$

$$Y_{i,t=1} = \beta_0 + \beta_1 \text{FullInformation}_{i,t=1} + \gamma_1 z'_i + \theta_i + \epsilon_i \quad (2c)$$

To explore potential information spillovers, we additionally estimate versions of these specifications with the inclusion of a spillover propensity score, and its interaction with the treatment indicator included. We calculate the spillover propensity score based on the following equation<sup>59</sup> which we estimate for the control group:

$$\text{Spillover}_{i,t=1} = \alpha_0 + \alpha_1 \text{HighDensity}_i + \alpha_1 \text{PropHH}_d + \alpha_3 \text{NoHH}_d + \alpha_4 \text{PropSocial}_d + \alpha_5 \text{NoSocial}_d + \gamma_1 z'_i + \epsilon_i \quad (3)$$

Where *Spillover* is a binary indicator that takes the value 1 if the (control group) respondent reports at endline having heard any new information about aflatoxin contamination in posho flour since the first interview, and 0 otherwise. The variable *HighDensity* is similarly a binary variable where 1 indicates assignment to the high-density treatment arm, and 0 to the low-density treatment arm. *PropHH* is the share of study households within a fixed radius  $d$  assigned to either treatment group, while *PropSocial* is the share of those households who are known to the respondent within the same radius, who are assigned to either treatment group.<sup>60</sup> The variables *NoHH* and *NoSocial* are indicators which take the value 1 if either of the respective

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<sup>57</sup> Here we deviate from our pre-analysis plan, which specified that we would use the inverse hyperbolic sine transformation. Chen & Roth (2024) provides a useful discussion of the issues involved in estimation where non-trivial shares of zero values are present.

<sup>58</sup> We implement the procedure in Stata using the *pdlasso* program (Ahrens, Hansen, & Schaffer, 2018).

<sup>59</sup> The version of this equation stated in the pre-analysis plan incorrectly included a fixed effects term for CHV list (the level at which the high-density treatment is assigned) which we omit here.

<sup>60</sup> Based on the respondent's self-report at endline.

proportion variables takes the value zero. While *HighDensity* and *PropSocial* are exogenous conditional on being defined, the missingness indicators *NoHH* and *NoSocial* are correlated with housing density and the number of respondents' social connections, respectively. For this reason, in cases where *PropHH* is selected for inclusion, we also include the *NoHH* variable, and we similarly include *NoSocial* if *PropSocial* is selected. In addition, each of the variables in equation (3), as well as the vector of baseline controls,  $z'_i$ , used in the preceding equations are included as candidates for selection via a logistic LASSO model.

We then use the estimation result from equation 3 to estimate the effect of knowledge spillovers on outcomes, and additionally the effect of assignment to either treatment net of information spillovers:

$$Y_{i,t=1} = \beta_0 + \beta_1 \text{AnyInformation}_{i,t=1} + \beta_2 \widehat{\text{Spillover}}_{i,t=1} + \beta_3 \text{AnyInformation} * \widehat{\text{Spillover}}_{i,t=1} + (\beta_4 \text{NoHH}_d) + (\beta_5 \text{NoSocial}_d) + \gamma_1 z'_i + \epsilon_i \quad (4)$$

Lastly, we conduct two heterogeneity analyses on the primary outcome of consuming the lower-risk maize product. The first of these, which was pre-registered, tests whether food safety risk information affects consumer choice according to a standard model of Bayesian updating of subjective probabilities, or by strengthening or making more salient the existing beliefs of those who already perceive food safety to be a problem. This test is implemented by testing whether the effect of the treatment varies according to participants' baseline beliefs about the contamination risk associated with the higher risk product at baseline.

The second heterogeneity analysis tests whether the treatment effect is stronger for households that reported higher income at baseline. We report this analysis even though this was not pre-registered due to its considerable policy significance. For each heterogeneity analysis, we add to equations (1) and (2) a binary indicator which takes the value 1 if the respondent's value of the variable for which the heterogenous effect is tested at baseline was below the sample median, and zero otherwise, and the interaction of this indicator with the treatment indicator:

$$Y_{i,t=1} = \beta_0 + \beta_1 \text{AnyInformation}_{i,t=1} + \beta_2 \text{BelowMedian}_i + \beta_3 \text{AnyInformation} * \text{BelowMedian}_{i,t=1} + \gamma_1 z'_i + \epsilon_i \quad (5)$$

$$Y_{i,t=1} = \beta_0 + \beta_1 \text{RelativeInformation}_{i,t=1} + \beta_2 \text{BelowMedian}_i + \beta_3 \text{RelativeInformation} * \text{BelowMedian}_{i,t=1} + \gamma_1 z'_i + \theta_i + \epsilon_i \quad (6a)$$

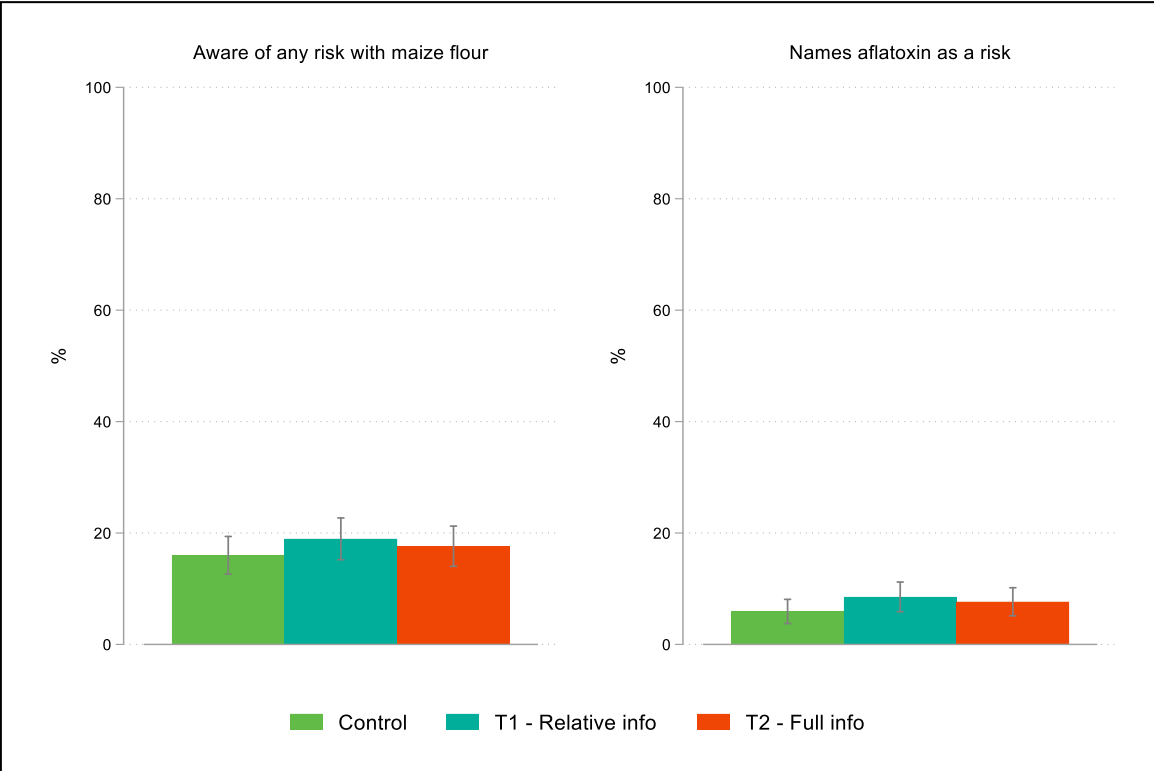
All specifications are estimated using logistic regression for binary outcomes, and ordinary least-squares for all other outcomes.

### 3.4 Results

Baseline characteristics are well balanced across the three treatment groups according to an omnibus test in which the treatment indicators are regressed on the full set of controls using a multinomial logistic regression. A handful of variables differ significantly across treatment groups. These include the presence of formally packaged flour in the household, which is similar across the control group and T2, but higher in T1 (Relative Information Only) (Appendix Table 3.1).

Reflecting the eligibility criteria,<sup>61</sup> consumption of maize flour is almost universal in the sample, with 99.8% of households reporting consumption by at least one family member in the seven days prior to the baseline interview. Almost all households (96.4%) report consuming informally milled maize flour, and many additionally report also consuming formally milled flour (37.5%). Consuming exclusively formally milled flour is rare in the sample, accounting for only 3.4% of households.

Figure 3.1 - Food safety risk awareness pre-intervention



Prior to the intervention, few individuals in the sample reported knowing of any risk associated with maize flour (Figure 3.1). At baseline, respondents were asked whether they knew of “any food safety threats or concerns” regarding maize flour, then asked to describe the risk if they answered affirmatively. A large majority of respondents (82.5%) responded “No” to this question. Only 7.3% of respondents mentioned aflatoxin specifically in their response, with the remaining 10.2% mentioning either another issue (6.1%) or were aware of an issue but unable to specify a name for it (4.1%). These responses were balanced across treatment groups (Appendix Table 3.5).

Following the intervention, treated households increased their consumption of formal maize flour and updated their perceptions of food safety risk (Table 3.1).

Table 3.1- Effects of receiving any information (Specification 1)

Observed packaged	Any risk (Packaged)	Risk (Packaged)	Any risk (Posho)	Risk (Posho)
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<sup>61</sup> Only households that reported purchasing posho flour within the past 2 weeks were included in the sample.

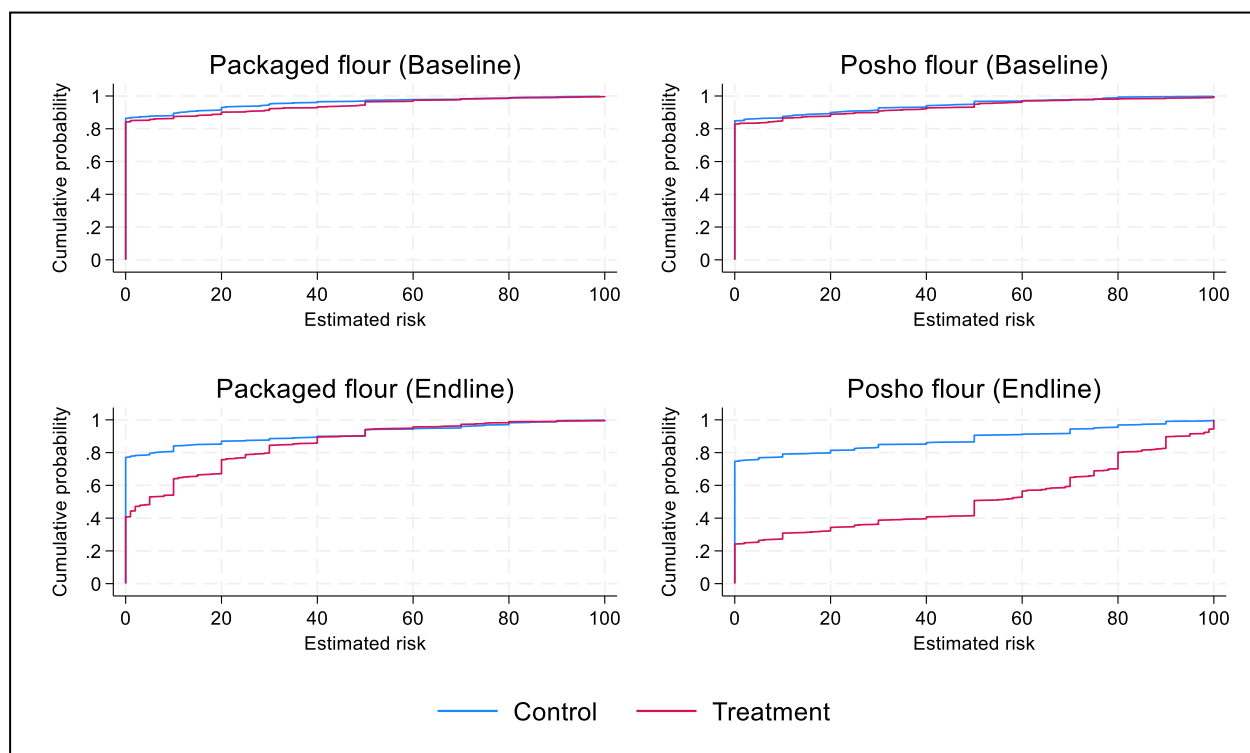


	(1)	(2)	(3)	(4)	(5)
Treatment: Any information	0.14*** (0.02) [0.000]	0.19*** (0.03) [0.000]	5.80*** (1.18) [0.000]	0.22*** (0.03) [0.000]	35.65*** (1.86) [0.000]
Packaged flour observed (BL)	0.15*** (0.04) [0.000]				
Any risk: Packaged flour (BL)		0.09*** (0.03) [0.002]		0.11*** (0.03) [0.000]	2.33 (5.88) [0.692]
Risk: Packaged flour (BL)			0.19*** (0.04) [0.000]		0.13 (0.09) [0.146]
Observations	1300	1299	1256	1299	1256
Control mean (BL)	0.29	0.14	4.39	0.15	5.66
Control mean (EL)	0.33	0.25	8.68	0.27	11.81

Notes: (1), (2) and (4) are marginal effects estimated via logit, (3) and (5) are ordinary least squares. All specifications include controls selected via LASSO and sampling list (community health volunteer) fixed effects. Where selected, baseline outcome variable results are included where selected. \*, \*\*, \*\*\* indicates significance at the 10%, 5% and 1% levels respectively.

As our primary specification shows, treated households were 14 percentage points more likely to have formally milled maize flour in the home than those in the control group at endline. When asked whether they were aware of any safety issue regarding maize flour, treated households were more likely than control households to report a risk. The share reporting any perceived risk was higher for both packaged flour (19 %-points) and for posho flour (22%-points). The average subjective probability of a food safety risk (the respondent's report that of the likelihood of a purchase of a given flour type from a local market) was likewise higher for both product categories, but the shift was much larger for posho flour. The mean risk for packaged flour was 5.8 %-points higher for households assigned to treatment relative to control households, while that for posho was 35.7 %-points higher. Figure 3.2 shows the shift in each measure graphically, plotting respondents' risk estimates at baseline and endline for each flour type, by treatment group.

Figure 3.2 - Cumulative distributions of subjective risk assessment, by survey round and flour type.



For both types of flour, large shares of individuals (83-86%) reported no risk at baseline across treatment groups. This share decreases somewhat in the control group in the follow-up survey (77% for packaged flour, 75% for posho flour), likely as a result of spillover effects (see discussion below) and dramatically in the treatment (23% for packaged flour, 39% for posho). For posho flour, the average subjective probability among control households of contamination in a given batch of flour is 12% at follow-up. For households receiving only relative risk information (T1) the level is much higher (51%) and higher than our measured risk level based on monitoring data (25%). Households in the full information treatment (T2) who were told this value have a lower average estimate (45%) though also over-estimate the level of risk.

To formally test for differences by type of information treatment, we estimate Specification 2c, restricting our sample to treated households, and regressing an indicator for assignment to the full information treatment (relative and absolute risk) to each of our primary outcomes. These results are presented in Table 3.2.<sup>62</sup>

<sup>62</sup> Results for specifications 2a and 2b are included in Appendix Tables 3.6 & 3.7.

Table 3.2- Comparative effects, by type of information treatment (Specification 2c)

	Observed packaged (1)	Any risk (Packaged) (2)	Risk (Packaged) (3)	Any risk (Posho) (4)	Risk (Posho) (5)
Treatment: Full information	0.00 (0.03) [0.907]	-0.09*** (0.03) [0.007]	-4.51*** (1.38) [0.001]	-0.10*** (0.04) [0.007]	-6.88*** (2.43) [0.005]
Packaged flour observed (BL)	0.12** (0.05) [0.012]				
Any risk: Packaged flour (BL)		0.19*** (0.05) [0.000]		0.28*** (0.07) [0.000]	
Observations	842	839	810	830	810
Control mean (BL)	0.36	0.16	6.41	0.18	7.55
Control mean (EL)	0.51	0.66	16.89	0.81	51.13

Notes: (1), (2) and (4) are marginal effects estimated via logit, (3) and (5) are ordinary least squares. All specifications include controls selected via LASSO and sampling list (community health volunteer) fixed effects. Where selected, baseline outcome variable results are included where selected. \*, \*\*, \*\*\* indicates significance at the 10%, 5% and 1% levels respectively.

As Table 3.2 shows, we find no differential effect across information treatments on the likelihood of a household having formally milled flour in their household at endline- those who received additional information on the absolute likelihood of posho flour contamination were as likely as those who received only relative information to have packaged flour in their home at endline.

In terms of perceived risk however, we do find statistically significant differences by treatment. As reflected in the distributions in Figure 3.2, households receiving the full information treatment revised their assessments of the probability of contamination upwards by less than those receiving only relative risk information, though both groups' subjective assessments on average were higher than the true risk level. Notably, the effect of the treatment on the extensive margin was lower in the full information treatment relative to the relative risk only treatment- households were less likely to report any risk associated with packaged flour (9%-points) or with posho flour (10%-points).

We additionally consider the possibility of heterogeneous treatment effects. We find evidence that the effect of treatment on changes in perceived risk associated with packaged flour is driven by respondents' baseline beliefs about the relative safety of posho flour but otherwise find no differential effects based on initial beliefs, and no differential effects by household income level (Appendix Tables 3.8-3.10).

We next explore the effect of the intervention on substitution into other starches (such as rice, potatoes and breads) to test whether households changed to an alternative food category (as opposed to substituting within categories of maize flour). To do this we estimate Specification 1, using expenditures on maize flour and quantity of other starches consumed (in per-person equivalents) as outcome variables (Table 3.3).

Table 3.3- Effect of treatment on expenditures on maize flour and substitutes (Specification 1)

	Log expend (Packaged)	Log expend (Posho)	Log expend (Maize flour)	Log quantity (Other starches)
	(1)	(2)	(3)	(4)
Treatment: Any information	0.13*** (0.05) [0.004]	-0.09* (0.05) [0.068]	-0.14*** (0.04) [0.000]	0.06 (0.04) [0.174]
Total expenditure on packaged flour (BL)	0.00*** (0.00) [0.005]			
Total expenditure on maize products (BL)	0.00*** (0.00) [0.000]		0.00 (0.00) [0.731]	
Total expenditure on posho flour (BL)		0.00*** (0.00) [0.000]		
Total expenditure on maize flour (BL)			0.00 (0.00) [0.330]	
Log quantity other starches (BL)				0.24*** (0.02) [0.000]
Observations	953	696	1300	1273
<i>Non-zero value of dependent variable reported</i>				
Treatment: Any information	0.22*** (0.01) [0.000]	-0.37*** (0.02) [0.000]	0.10 (0.16) [0.511]	-0.01 (0.04) [0.752]
Weekly expend (KSH), Control (BL)	98.99	382.11	481.09	-
Weekly expend (KSH), Control (EL)	163.68	328.26	491.94	-

Notes: Top panel: Ordinary least squares regression. Lower panel: Average marginal effect of treatment on probability of reporting zero for outcome, estimate via logit regression. All specifications include controls selected via LASSO and sampling list (community health volunteer) fixed effects. Baseline outcome variable results are included where selected. \*, \*\*, \*\*\* indicates significance at the 10%, 5% and 1% levels respectively.

The results for reported expenditure reflect our results for observed behaviour and risk perceptions. Treated respondents are twenty-two percentage points more likely to reported any expenditure on packaged maize flour in the past week ( $p < 0.01$ ), and those purchasing increase their reported expenditure by 13% ( $p < 0.01$ ). Conversely, they are thirty-seven percentage points less likely to report purchasing posho flour in the sample time period ( $p < 0.01$ ) with those purchasing reducing their expenditure by an average of 9% ( $p = 0.07$ ). Overall, there is no statistically significant change in the probability of reporting purchasing any maize flour, but total expenditure on maize flour decreased by 14%-points, reflecting the greater share of expenditures on posho vs packaged maize flour at baseline. Lastly, we find no evidence of substitution into other starches, based on quantities reported (though this may in part reflect measurement error in aggregate reported quantities of different food items).

Lastly, we consider the role of potential information spillovers on our results. To explore this, we asked households at endline whether they had heard any new information regarding food safety since their previous interview, and then used this report to estimate a propensity to receive this information in the control group Equation (3). Overall the rate of reported spillovers in the control group is low, at 10%, though contrary to our expectation it is lower in the high-intensity sampling area (8%) than in the low-intensity sampling area (15%).<sup>63</sup> We estimated this over a range of potential distances in increments of 10 metres over a range of 100-5,000m then selected the radius with the best fit based on each model's Bayesian Information Criterion (BIC) which was 1180m. We then use this predicted spillover propensity to estimate Equation (4) in Table 3.4 below.

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<sup>63</sup> It is unclear why this self-reported measure would be higher in areas with fewer sampled units. One potential explanation is that peers may be more likely to discuss the experience of being interviewed in cases where only one has been interviewed than in cases where both peers were interviewed.

Table 3.4- Treatment effects interacted with estimate spillover propensity

	Observed packaged (1)	Any risk (Packaged) (2)	Risk (Packaged) (3)	Any risk (Posho) (4)	Risk (Posho) (5)
Information treatment (Any)	0.16*** (0.04) [0.000]	0.43*** (0.03) [0.000]	6.52*** (1.54) [0.000]	0.57*** (0.03) [0.000]	37.65*** (2.44) [0.000]
Spillover propensity	-0.35 (0.28) [0.210]	0.49* (0.27) [0.069]	24.16** (12.22) [0.048]	0.54** (0.25) [0.028]	7.82 (19.36) [0.686]
Treatment x Spillover	0.05 (0.21) [0.823]	-0.68*** (0.20) [0.001]	-12.12 (9.21) [0.188]	-0.69*** (0.18) [0.000]	-18.78 (14.60) [0.198]
No study households in radius	0.39 (0.49) [0.427]	-0.37 (0.47) [0.423]	-12.14 (21.10) [0.565]	-0.45 (0.43) [0.293]	-27.33 (33.42) [0.413]
No known households in radius	-0.00 (0.04) [0.914]	-0.08** (0.04) [0.034]	-1.17 (1.73) [0.498]	-0.09** (0.03) [0.014]	-3.85 (2.74) [0.159]
Risk: Packaged flour (BL)	-0.00 (0.00) [0.999]	0.00 (0.00) [0.565]	0.16*** (0.06) [0.007]	0.00 (0.00) [0.498]	0.22** (0.10) [0.024]
Difference in risk (BL)	-0.00 (0.00) [0.449]	0.00 (0.00) [0.250]	0.01 (0.05) [0.775]	0.00 (0.00) [0.795]	0.07 (0.08) [0.387]
Packaged flour observed (BL)	0.18*** (0.04) [0.000]				
Any risk: Packaged flour (BL)		0.22*** (0.08) [0.010]			
Any risk: Posho flour (BL)				0.14 (0.09) [0.120]	
Observations	1309	1309	1256	1309	1256
Control mean (BL)	0.29	0.14	4.39	0.15	5.66
Control mean (EL)	0.33	0.25	8.68	0.27	11.81

Notes: (1), (2) and (4) are marginal effects estimated via logit, (3) and (5) are ordinary least squares. All specifications include controls selected via LASSO and sampling list (community health volunteer) fixed effects. Where selected, baseline outcome variable results are included where selected. \*, \*\*, \*\*\* indicates significance at the 10%, 5% and 1% levels respectively.

We do not find any effects associated with spillovers on the likelihood of households having packaged maize flour present in their home at endline, though we do find some evidence of spillover effects on subjective risk measures. The results in columns 2 and 4 imply that a household with an estimated spillover propensity of 1 has similar endline beliefs about the likelihood of contamination in packaged and posho flour specifically as those who were

provided information directly through the intervention. This effect is driven in each case by control households (in other words there is no evidence of an additional effect of spillovers on treated households). There is also a statistically significant increase in the average risk level reported for packaged flour, though not for posho flour which though positive, is not statistically significant.

### **3.5 Conclusion**

In this paper we test the effects of providing information on food safety risks for formally and informally milled maize flour to a vulnerable population. Systematic testing of formally versus informally traded maize flour has shown formally registered, packaged products to contain lower levels of foodborne hazards (Hoffmann et al. 2023b). The same is true for packaged versus informally traded milk (Baker et al, 2022). On the other hand, this result is not borne out in comparative studies on meat sold by formal sector versus informal butchers (Roesel & Grace, 2014). The results of the present study indicate that where reliable evidence on the relative safety of alternative products exists, information provision can be an effective and low-cost intervention for the reduction of foodborne illness.

We find that study households who received our information treatment updated their beliefs about the risks associated with consuming different categories of maize flour, and as a result changed their purchasing behaviour and substituted lower risk formally milled flour in place of higher risk informally milled posho flour. Comparing our two forms of information treatment, we find providing relative risk information alone, without information on the probability of contamination, led to higher subjective evaluations of contamination risk. This suggests that in the absence of specific risk information, recommendations may lead consumers to over-estimate the risk of a health hazard. However, given that this did not affect observed purchase behaviour, the difference may have little practical significance.

Treatment effects are statistically equivalent between those in the sample with relatively low versus higher incomes. This shows that even households with limited means put a high enough value on food safety. Further, we find no evidence of substitution away from the product class about which information about a food safety hazard was provided. While the nutritional value of maize versus alternative starches is trivial, substitution away from highly nutritious, but also high-risk foods such as meat and fresh vegetables is a potential risk of raising consumer awareness of food safety risks. The finding of no such effect in this context mitigates this concern to some extent. However, as substitution effects are likely to be product-specific, additional research on this point remains important.

To date, the majority of research on behavioural responses to information on aflatoxin risk has focused on maize-producing households. Interventions promoting production and post-harvest practices to mitigate this risk have proven effective (Pretari, Hoffmann and Tian, 2019; Bauchet et al. 2021; Magnan et al., 2021; Deutschmann et al., 2023). Our findings demonstrate that a low-cost information provision intervention was able to alter consumers' perceptions and induce them to switch to a lower-risk alternative. This demonstrates that product safety is a salient concern for consumers even in low-income settings.

Individuals in our study changed their purchasing behaviour without any form of monetary or other incentive. We demonstrate that informational interventions which require few resources have the capacity to reduce the environmental health risks consumers in low-income countries face, enabling them to mitigate the hazards they face in settings where regulatory enforcement

is partial or absent. Citizens in developing economies face significant environmental health risks, including that of unsafe food. Empowering them with information can complement supply-side regulation to improve health outcomes.

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### 3.7 Appendix Tables

Table 3.5- Baseline Balance

	Baseline Mean				Control vs. Treat	p-value		
	Control	Any treatment	T1	T2		Control vs. T1	Control vs. T2	T1 vs. T2
Packaged maize observed	0.29	0.32	0.36	0.28	0.139	0.009	0.867	0.007
Any risk from packaged flour	0.14	0.16	0.16	0.15	0.293	0.299	0.488	0.720
Risk from packaged flour	4.39	5.91	6.41	5.41	0.089	0.054	0.361	0.419
Any risk from posho flour	0.15	0.17	0.18	0.17	0.350	0.384	0.520	0.783
Risk from posho flour	5.66	6.75	7.55	5.98	0.259	0.183	0.769	0.325
Risk difference (Posho-Packaged)	1.27	0.85	1.14	0.56	0.600	0.905	0.414	0.634
Consumed: Posho	0.98	0.96	0.95	0.96	0.017	0.013	0.149	0.448
Consumed: Maize grain	0.33	0.32	0.32	0.31	0.579	0.713	0.572	0.796
Maize price (KSH/kg)	101	101	101	101	0.764	0.600	0.965	0.641
Total maize expenditure (KSH)	517	501	503	499	0.407	0.538	0.365	0.855
Household size	4.99	4.89	4.85	4.93	0.188	0.159	0.512	0.498
Any infant	0.10	0.10	0.10	0.10	0.811	0.824	0.851	0.987
Any toddler	0.26	0.29	0.28	0.30	0.200	0.421	0.267	0.640
Any adult > 50	0.13	0.12	0.10	0.14	0.644	0.239	0.627	0.101
Age of respondent	33	33	33	33	0.958	0.875	0.935	0.799
Female respondent	0.94	0.92	0.93	0.92	0.159	0.318	0.163	0.852
Married respondent	0.74	0.72	0.75	0.70	0.598	0.717	0.200	0.116
No formal education	0.16	0.14	0.13	0.15	0.194	0.078	0.599	0.237
Primary education	0.46	0.46	0.46	0.45	0.828	0.929	0.763	0.880
Secondary education	0.29	0.31	0.32	0.30	0.384	0.324	0.627	0.617
Technical education	0.07	0.09	0.09	0.09	0.344	0.439	0.440	0.980
University education	0.01	0.01	0.01	0.00	0.370	0.599	0.251	0.289
Monthly income (KSH)	11613	12020	11529	12501	0.574	0.920	0.297	0.267
Has smartphone	0.68	0.71	0.73	0.68	0.179	0.065	0.759	0.203
Has solar panel	0.02	0.03	0.03	0.03	0.062	0.259	0.071	0.476
Has electric connection	0.84	0.83	0.83	0.83	0.615	0.628	0.699	0.857
Has TV	0.73	0.75	0.76	0.73	0.470	0.157	0.988	0.203
Has computer	0.02	0.04	0.04	0.03	0.176	0.152	0.440	0.540
Has DVD player	0.13	0.15	0.16	0.13	0.201	0.095	0.644	0.297
Has electric cooker	0.08	0.08	0.09	0.06	0.823	0.308	0.330	0.042
Has charcoal stove	0.77	0.77	0.79	0.76	0.725	0.375	0.763	0.205
Has gas cooker	0.68	0.74	0.72	0.75	0.043	0.144	0.051	0.431
Housing index	-0.08	0.05	0.09	0.01	0.010	0.019	0.136	0.360
Daily fuel expend (KSH)	66	67	64	70	0.752	0.410	0.224	0.004
Daily lighting expend (KSH)	3	4	4	4	0.107	0.360	0.163	0.845
Monthly rent (KSH)	3518	3598	3590	3605	0.537	0.656	0.506	0.917
Risk preference (1-10)	4.98	5.11	5.00	5.21	0.433	0.915	0.262	0.421
Knowledge 1	0.05	0.06	0.05	0.06	0.457	0.660	0.419	0.706
Knowledge 2	0.02	0.02	0.01	0.03	0.915	0.404	0.583	0.084
Knowledge 3	0.01	0.01	0.02	0.00	0.763	0.343	0.469	0.129

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Knowledge 4	0.09	0.08	0.09	0.07	0.414	0.756	0.230	0.491
Knowledge 5	0.15	0.16	0.17	0.16	0.474	0.437	0.749	0.643
Knowledge 6	0.02	0.01	0.01	0.01	0.072	0.251	0.033	0.517
Knowledge 7	0.00	0.02	0.02	0.01	0.021	0.034	0.130	0.395
Knowledge 8	0.00	0.00	0.00	0.00	0.957	0.957	0.969	0.988
Knowledge 9	0.00	0.00	0.00	0.00	0.323	0.325	.	0.325
Knowledge 10	0.13	0.14	0.14	0.14	0.663	0.661	0.810	0.855

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Table 3.6- Control vs. relative risk information treatment (Specification 2a)

	Observed packaged (1)	Any risk (Packaged) (2)	Risk (Packaged) (3)	Any risk (Posho) (4)	Risk (Posho) (5)
Treatment: Relative information	0.13*** (0.03) [0.000]	0.22*** (0.04) [0.000]	7.53*** (1.42) [0.000]	0.26*** (0.05) [0.000]	38.23*** (2.02) [0.000]
Packaged flour observed (BL)	0.14*** (0.04) [0.001]				
Any risk: Packaged flour (BL)		0.08** (0.04) [0.048]		0.17*** (0.04) [0.000]	
Risk: Posho flour (BL)		0.00 (0.00) [0.238]			
Risk: Packaged flour (BL)			0.25*** (0.04) [0.000]		0.35*** (0.06) [0.000]
Observations	867	860	842	860	842
Control mean (BL)	0.29	0.29	0.29	0.29	0.29
Control mean (EL)	0.33	0.33	0.33	0.33	0.33

Notes: (1), (2) and (4) are marginal effects estimated via logit, (3) and (5) are ordinary least squares. All specifications include controls selected via LASSO and sampling list (community health volunteer) fixed effects. Baseline outcome variable results are included where selected. \*, \*\*, \*\*\* indicates significance at the 10%, 5% and 1% levels respectively.

Table 3.7- Control vs. full information treatment (Specification 2b)

	Observed packaged (1)	Any risk (Packaged) (2)	Risk (Packaged) (3)	Any risk (Posho) (4)	Risk (Posho) (5)
Treatment: Full information	0.13*** (0.04) [0.000]	0.25*** (0.02) [0.000]	4.00*** (1.28) [0.002]	0.34*** (0.02) [0.000]	32.95*** (2.04) [0.000]
Packaged flour observed (BL)	0.21*** (0.05) [0.000]				
Any risk: Packaged flour (BL)		0.20*** (0.04) [0.000]		0.17*** (0.05) [0.001]	
Risk: Packaged flour (BL)			0.15*** (0.06) [0.007]		
Any risk: Posho flour (BL)					5.70 (4.60) [0.215]
Risk: Posho flour (BL)					0.20** (0.10) [0.043]
Observations	848	878	860	878	860
Control mean (BL)	0.29	0.29	0.29	0.29	0.29
Control mean (EL)	0.33	0.33	0.33	0.33	0.33

Notes: (1), (2) and (4) are marginal effects estimated via logit, (3) and (5) are ordinary least squares. All specifications include controls selected via LASSO and sampling list (community health volunteer) fixed effects. Baseline outcome variable results are included where selected. \*, \*\*, \*\*\* indicates significance at the 10%, 5% and 1% levels respectively.

Table 3.8- Heterogeneity: No risk from posho reported at baseline

	Observed packaged (1)	Any risk (Packaged) (2)	Risk (Packaged) (3)	Any risk (Posho) (4)	Risk (Posho) (5)
Treatment: Any information	0.17*** (0.06) [0.006]	0.14*** (0.04) [0.001]	-3.90 (3.01) [0.196]	0.19*** (0.04) [0.000]	31.08*** (4.78) [0.000]
Reports no posho risk (BL)	0.01 (0.06) [0.888]	-0.05 (0.07) [0.483]	-8.74*** (3.02) [0.004]	-0.04 (0.06) [0.489]	3.82 (7.69) [0.620]
Treatment x No posho risk	-0.04 (0.07) [0.610]	0.10 (0.06) [0.106]	11.52*** (3.29) [0.000]	0.08 (0.05) [0.164]	5.47 (5.22) [0.295]
Packaged flour observed (BL)	0.15*** (0.04) [0.000]				
Any risk: Packaged flour (BL)		0.12*** (0.04) [0.004]		0.13*** (0.04) [0.000]	5.89 (6.63) [0.374]
Risk: Packaged flour (BL)			0.18*** (0.05) [0.000]		0.14 (0.09) [0.107]
Observations	1300	1299	1256	1299	1256
Control mean (BL)	0.29	0.29	0.29	0.29	0.29
Control mean (EL)	0.33	0.33	0.33	0.33	0.33

Table 3.9- Heterogeneity- Reports equal or lower risk from posho at baseline

	Observed packaged (1)	Any risk (Packaged) (2)	Risk (Packaged) (3)	Any risk (Posho) (4)	Risk (Posho) (5)
Treatment: Any information	0.23*** (0.07) [0.002]	0.21*** (0.05) [0.000]	-1.90 (3.96) [0.631]	0.25*** (0.05) [0.000]	34.21*** (6.27) [0.000]
Reports posho as safe or safer (BL)	0.03 (0.07) [0.661]	-0.02 (0.05) [0.675]	-12.98** (5.08) [0.011]	-0.03 (0.05) [0.515]	-3.36 (8.03) [0.675]
Treatment x Posho safer	-0.11 (0.09) [0.214]	0.00 (0.06) [0.944]	8.56** (4.16) [0.040]	0.00 (0.06) [0.950]	1.60 (6.58) [0.808]
Packaged flour observed (BL)	0.15*** (0.04) [0.000]				
Any risk: Packaged flour (BL)		0.09*** (0.03) [0.006]		0.12*** (0.03) [0.001]	7.42 (6.85) [0.279]
Risk: Packaged flour (BL)			0.26*** (0.07) [0.000]		0.14 (0.11) [0.217]
Any risk: Posho flour (BL)			-2.24 (3.58) [0.531]		-12.97 (8.32) [0.119]
Risk: Posho flour (BL)			-0.11 (0.07) [0.109]		0.09 (0.11) [0.420]
Observations	1300	1299	1256	1299	1256
Control mean (BL)	0.29	0.29	0.29	0.29	0.29
Control mean (EL)	0.33	0.33	0.33	0.33	0.33

Table 3.10- Heterogeneity- At or above median income at baseline

	Observed packaged (1)	Any risk (Packaged) (2)	Risk (Packaged) (3)	Any risk (Posho) (4)	Risk (Posho) (5)
Treatment: Any information	0.11*** (0.04) [0.003]	0.19*** (0.03) [0.000]	5.98*** (1.67) [0.000]	0.22*** (0.03) [0.000]	37.71*** (2.62) [0.000]
At or above median income (BL)	-0.06 (0.05) [0.202]	0.03 (0.02) [0.246]	2.42 (2.31) [0.296]	0.03 (0.02) [0.144]	4.31 (3.63) [0.236]
Treatment x Median income	0.07 (0.06) [0.238]	-0.01 (0.03) [0.676]	-0.35 (2.41) [0.885]	-0.02 (0.02) [0.303]	-3.96 (3.78) [0.295]
Packaged flour observed (BL)	0.16*** (0.04) [0.000]				
Any risk: Packaged flour (BL)		0.08*** (0.03) [0.004]		0.10*** (0.03) [0.001]	4.50 (5.91) [0.447]
Risk: Packaged flour (BL)			0.20*** (0.04) [0.000]		0.12 (0.09) [0.181]
Observations	1300	1299	1256	1299	1256
Control mean (BL)	0.29	0.29	0.29	0.29	0.29
Control mean (EL)	0.33	0.33	0.33	0.33	0.33



### **3.8 Appendix A – Treatment script**

#### ***T1- Relative information only treatment***

Now I'm going to give you information about aflatoxin, a food safety problem that affects maize.

Aflatoxin is invisible. You cannot tell by looking at maize or maize flour whether it is contaminated with aflatoxin.

Aflatoxin harms health. Consuming food with unsafe levels of aflatoxin can damage the liver and cause cancer. If aflatoxin is consumed by young children, their growth and development may be affected

Packaged unga is tested for aflatoxin by the government. The government of Kenya has set a rule for how much aflatoxin should be allowed in food that is sold, and regularly tests packaged unga to make sure that it is safe.

Our research team tested many samples of maize flour from all over Kenya.

We found that branded packaged maize flour (unga) contained less than half the level of aflatoxin as flour from posho mills.

Remember, aflatoxin is invisible, so even if the maize grains you buy for milling look good, they may contain a lot of aflatoxin.

If you want to reduce the risk that you or your family are exposed to aflatoxin, you can do so by buying packaged unga instead of posho flour.

#### ***T2- Full information treatment***

Now I'm going to give you information about aflatoxin, a food safety problem that affects maize.

Aflatoxin is invisible. You cannot tell by looking at maize or maize flour whether it is contaminated with aflatoxin.

Aflatoxin harms health. Consuming food with unsafe levels of aflatoxin can damage the liver and cause cancer. If aflatoxin is consumed by young children, their growth and development may be affected

Packaged unga is tested for aflatoxin by the government. The government of Kenya has set a rule for how much aflatoxin should be allowed in food that is sold, and regularly tests packaged unga to make sure that it is safe.

Our research team tested many samples of maize flour from all over Kenya.

One in every four tins of posho we tested contained more aflatoxin than is legally permitted in Kenya.

You see these four bags of posho? [show 4 laminated cut-out images of posho, one of which is marked with a “!” sign] We found that for every four batches of posho tested, one was contaminated with aflatoxin beyond the legal level set by the government of Kenya.

We found that branded packaged maize flour (unga) contained less than half the level of aflatoxin as flour from posho mills.

Remember, aflatoxin is invisible, so even if the maize grains you buy for milling look good, they may contain a lot of aflatoxin.

If you want to reduce the risk that you or your family are exposed to aflatoxin, you can do so by buying packaged unga instead of posho flour.

## Conclusion général

The goal of this thesis has been to explore in detail three cases studies of how people make economic decisions when faced with environmental uncertainty, focusing on individuals in developing countries interacting with the agrifood system. In this conclusion, I will try to take a step back and summarize the key feature of each study and explore how it can contribute towards a prospective agenda for future research.

The first chapter considered the case of adoption of conservation agriculture. This is a group of agroecological practices that proponents have argued combine public good characteristics (through carbon sequestration and support for biodiversity) and private benefits to adopters. In the experiment, we attempt to model two key features of these private benefits which we argue are likely to inhibit widespread adoption.

The first is simply that its payoff to farmers in terms of yield gain is dependent on the accrual of organic matter in soil which takes time, meaning that adopting farmers must both believe in the efficacy of the technology and have time preferences that align with the number of seasons required to achieve these benefits. The second related feature is that the amount of time required is itself subject to uncertainty *ex ante*. In our experimental context- under the assumption that participants trust the information we provide- we isolate this second feature to render the uncertainty as a problem of ambiguity by creating uncertainty in the timing when the private benefit is realized. We then demonstrate that an incentive treatment which mitigates this uncertainty induces greater adoption.

This situation, of a technology with identified agronomic benefits over an extended time period being under-utilized by farmers who would seem to gain from adopting it is far from unique to conservation agriculture. To take one example, in a recent working paper with my co-authors (Hoffmann, Murphy, & Harigaya, 2024) we analyze take-up of a range of improved agronomic practices following the training of coffee farmers in Uganda. One of these practices, stumping, shares similar characteristics to CA. Stumping involves removing all but the base of older coffee plants to allow the plant to rejuvenate. As with CA, this involves an upfront cost (labour to do the stumping, plus foregone harvest while the plant grows back) but yields a greater return over the medium term than continuing to harvest older plants annually. I am currently working with colleagues to draw on insights from our CA study for the development of a project to incentivize stumping on coffee farmers in Ethiopia. Since coffee is both an important cash crop in the country and a key source of export revenue, a successful evaluation of a policy to improve yields has the potential to confer important welfare benefits.

The second chapter explore the role of the Green Revolution in India in affecting wheat farmers' resilience to temperature shocks. This paper contributes to a recent literature which seeks to apply econometric techniques for estimating agricultural adaptation to climate and weather which have typically been used to analyze to developed economy agriculture to a developing country setting. I demonstrate the importance of accounting for changes in the set of available technologies which happened rapidly and non-uniformly in India. Here the expansion of the set of input technologies available to farmers led to an increase in their resilience to inter-annual temperature shocks. Broadening the choice set of potential actions enabled farmers to mitigate uncertainty. As anthropogenic climate change continues to increase the variability of weather outcomes and increasingly push agriculture in many tropical areas out of its historical

temperature range , it is crucial that the research community expands its understanding of agricultural adaptation strategies for low-income farmers.

In this area, I am interested in developing research which looks at adaptation strategies beyond simply production outcomes. Temperature shocks impact not only plant health, but also livestock and human health. Exposure to unsafe temperatures will be an increasing concern for agricultural producers in tropical economies, not least in South Asia where large populations of outdoor workers will be exposed to extreme high temperatures (Xu, Kohler, & Lenton, 2020). While there has been some work looking at cognitive and productivity outcomes for outdoor labourers (Masuda et al 2020, 2021), to my knowledge very little work has been done within economics to study potential interventions to mitigate both health and welfare outcomes associated with agricultural laborers' exposure to heat stress.

The third chapter looked at the effectiveness of an information intervention targeting a serious food safety hazard affecting low-income consumers in urban Kenya. Our analysis that this risk is unknown to most consumers and also would be difficult for them to attribute to a particular product since its effects accrue over time. We demonstrate that by informing consumers of the risk- and also crucially about a relatively safer available alternative- we were able to shift their purchasing behaviour, without requiring any form of monetary or in-kind incentive.

This study is an important proof of concept in that it demonstrates that even very poor consumers are concerned about the safety of their food, and are motivated to switch to less risky alternatives when they are informed and such substitutes are available. Food safety remains a topic that is significantly under-studied in development economies relative to its health burden (Hald et al., 2016). I am currently working with coauthors on two studies working with meat vendors- one in Ethiopia, one in Viet Nam- which attempt to leverage this insight by using targeted consumer information campaigns alongside trainings and voluntary certification schemes to attempt to drive incentives for vendors to offer safer foods. We are currently exploring other contexts in which to pursue this research agenda and hope to be able to develop policy-relevant interventions to mitigate health risk from unsafe foods.

The relationship between humans and the natural environment will only continue to grow in importance in policy debates in the coming decades, particularly in the context of economic development where many choices that benefit a decisionmaker come with public costs in the form of emissions or reductions in biodiversity. Achieving welfare-maximizing outcomes will require a nuanced understanding of how individuals make choices under environmental uncertainty, and I hope with this thesis and subsequent research to help to inform those debates.

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