Journal of Development Economics

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--Manuscript Draft--

Manuscript Number:	DEVEC-D-23-01726R3
Article Type:	Registered Report Stage 1: Proposal
Section/Category:	Insurance, Agriculture, Land, Credit, poverty, Africa - Micro, households, gender
Keywords:	Technology Adoption; Information provision; Tailored information; Learning; Agricultural Extension
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Response to Reviewers:	See attached cover letter/author statement

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General vs. Tailored Information for Technology Adoption: Evidence from a Cluster Randomized Controlled Trial in India

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Date of latest draft: 06/13/2024

Abstract

Recent studies highlight information constraints as an important barrier to technology adoption, but there is little evidence that allows to distinguish the roles of different information frictions for adoption decisions across different types of technology. We conduct a cluster randomized controlled trial among 1,200 farmers in Haryana, India, to promote adoption of early sown wheat varieties and zero tillage technology—two agricultural innovations that can help farmers adapt to climate change by increasing resilience to heat and water stress. Two distinct information treatments are studied, a one-time group training and an individual-specific advisory service. The experiment is designed to identify distinct impacts of general versus tailored agricultural advice across different types of technology and test for spillovers on other farmers' adoption decisions. This is guided by a theoretical framework that decomposes optimal usage choices of technology into a systematic component and an idiosyncratic component, generating testable predictions regarding the distinct roles of general and tailored information. The insights derived from this study have strong policy implications and help in understanding the role of information frictions for adoption decisions beyond the specific technologies considered here.

Keywords: Technology adoption, Information provision, Tailored information, Learning, Agricultural extension

JEL codes: D81, D83, D91, O13, O33, Q16

Study pre-registration: AEA RCTR-0012160

Proposed timeline

Preliminary field work was conducted in September and October 2022 to inform the design of the experiment and its interventions. Preparations for the cluster RCT started shortly after. The baseline survey was conducted in March 2024. Implementation of the first treatment package (Intervention 1) will take place between May and July 2024. Implementation of the second treatment package (Intervention 2) will start at the same time and run until April 2025. The endline survey will be conducted in April and May 2025. Upon completion of the endline survey, we will perform the analysis and write the research paper, which we hope to complete within seven months.

1. Introduction

Motivation

Much of the economic literature agrees that technology adoption constitutes an important pathway for improving countries' and individuals' economic prospects.[1] At the same time, there is a long list of technologies that are only imperfectly adopted and used, spanning various sectors and types of innovations (see the surveys in Rosenberg 1972, Keller 2004, Suri and Udry 2022). A large (and growing) literature discusses various channels to explain the many puzzling observations documented in this context.[2] By now, each of these channels has received empirical support from multiple studies. At the same time, there does not seem to be a single channel that can account for all the observed patterns of imperfect adoption and usage of technology. Rather, an emerging consensus in the literature is that each of these channels plays *some* role in explaining suboptimal adoption behaviors (see also Suri and Udry 2022).

What is much less understood, however, is how the role of each channel, as well as the roles of different constraints within each channel, vary across different types of technology. To a large extent, this is due to the fact that empirical studies in this context typically focus on testing the role of one particular constraint in determining adoption decisions of one specific technology (e.g., how imperfect learning affects fertilizer adoption), but are not designed to obtain evidence that would allow to assess the relative importance of different constraints across different types of technology. Relatedly, the theoretical literature usually treats technology as homogeneous, abstracting from features that make different types of technology more or less susceptible to certain constraints.

This study starts to address this gap by first proposing a theoretical framework for distinguishing different types of technology and different types of information, and then conducting a field experiment to test the

[1] In the macroeconomic literature, technology adoption is commonly viewed as a major driver behind differences in total factor productivity, which in turn can account for much of the variation in income levels across countries (Aghion and Howitt 1992, Grossman and Helpman 1993, Keller 2004, Hall 2005). In microeconomics, technology adoption is considered a key pathway for households and enterprises to escape poverty, particularly in the development context (Banerjee and Duflo 2005, Foster and Rosenzweig 2010).

[2] One group of explanations focuses on market frictions and studies a wide range of channels, including transaction costs, financial constraints, risk aversion paired with limited insurance, and imperfect property rights (Feder 1980, Moser and Barrett 2006, Caselli and Coleman 2001, Dercon and Christiaensen 2011, Suri 2011, Karlan et al. 2014, Bold et al. 2017). Another group of explanations focuses on information constraints, such as limited access to information, limited attention needed for processing available information, and imperfect learning (Parente 1994, Foster and Rosenzweig 1995, Jovanovic and Nyarko 1996, Bandiera and Rasul 2006, Conley and Udry 2010, Hanna et al. 2014, Naeher, 2022). Finally, explanations based on cultural norms and behavioral biases have been proposed (Ashraf et al. 2006, Duflo et al. 2011, Datta and Mullainathan 2014).

implications of the model. The model decomposes optimal usage choices of technology into a systematic component and an idiosyncratic component, generating testable predictions regarding the distinct roles of general information versus information that is tailored to individual-specific needs, in facilitating adoption of different types of technology. Most importantly, the model implies that the impacts of providing general and tailored information on technology adoption will differ across different types of technology. While, for some technologies, general information about optimal usage practice will be sufficient to facilitate profitable adoption, other technologies require information that is tailored to idiosyncratic conditions specific to each user. The model also generates testable predictions regarding information spillovers. Technologies for which tailored information is required tend to feature less scope for such spillovers, as optimal usage practices are dependent on each user's idiosyncratic conditions.

Our empirical application focuses on two agricultural technologies in the context of wheat farming in India, namely recently developed early sown wheat (ESW) varieties and zero tillage (ZT) technology, which will be introduced in more detail below. The field experiment will assess the impacts of different information treatments on adoption decisions of ESW and ZT in a sample of 1,200 wheat farmers in the Sonipat district (Haryana). Specifically, a cluster randomized controlled trial (RCT) with multiple treatment arms and two carefully-designed interventions will be studied. The first intervention comprises a one-time training workshop focused on increasing farmers' knowledge about the general features and benefits of using ESW and ZT, with the goal of facilitating uptake and usage of these two technologies. The second intervention consists in handholding support to individual farmers over the cropping season, providing tailored information and assisting them in staying attentive to changes in environmental and other factors related to the optimal use of ESW and ZT.

Data will be collected during a baseline and an endline survey in both treated and control villages, where the primary outcomes of interest are farmers' adoption decisions of ESW and ZT in the Rabi season 2024/25 (after the start of our interventions), and secondary outcomes include farmers' knowledge of and perceptions towards ESW and ZT, and plans for future adoption. Because the use of ZT is expected to reduce crop residue burning, a major environmental and health concern in the study region, we will also collect data on residue burning practices and farmers' perceptions towards air pollution.

The focus on information constraints is motivated by the fact that a growing number of studies highlight (lack of) information as an important barrier to technology adoption, including in the context of smallholder farming (Foster and Rosenzweig 1995, Jovanovic and Nyarko 1996, Munshi 2004, Bandiera and Rasul 2006, Conley and Udry 2010, Hanna et al. 2014, Beaman et al. 2021, Carter et al. 2021). These information constraints can take different forms, such as unawareness of the existence of a technology, uncertainty about optimal usage practices of a known technology, and imperfect information on the returns to the technology (under optimal usage). In some cases, measures that increase (monetary) incentives for adoption can help to mitigate informational market failures.[3] Addressing information frictions more directly, a rich empirical literature has documented significant effects of information provision on farmers' adoption decisions (Cole and Fernando 2012, Casaburi et al. 2014, Hanna et al. 2014, BenYishay and Mobarak 2019, Emerick and Dar 2021, Naeher and Schündeln 2022, Kondylis et al. 2023).

However, these studies are not designed to distinguish between the roles of different types of information in facilitating adoption of different types of technology. Specifically, the interventions considered in previous

[3] For instance, Carter et al. (2021) show that subsidies can be effective in addressing reluctance to learn-by-doing stemming from imperfect information on the returns to agricultural technology.

studies typically focus on a single technology and either involve only general information or a combination of general and tailored information, making it impossible to determine which of the two channels constitutes the bottleneck (and whether this varies across different types of technology). Perhaps closest to our intended contribution are the studies by Corral et al. (2022), examining the impact of tailored input recommendations on fertilizer use, and Fabregas et al. (2019), comparing tailored and general agricultural advice provided by SMS in the context of agricultural lime for reducing soil acidity. Our experiment therefore adds to the literature novel insights into the distinct impacts of in-person general and tailored agricultural advice on adoption decisions across different technologies.

Recent evidence also highlights the importance of information spillovers through social networks for technology diffusion (Conley and Udry 2010, BenYishay and Mobarak 2019, Beaman et al. 2021, Carter et al. 2021). To contribute further insights, we design our field experiment to allow for capturing information spillovers separately for general and tailored information provision. Given that these types of information provision can give rise to different (sometimes opposite) policy implications (see our discussion below), our contribution is not only academically significant but also directly relevant for policymaking.

The motivation for focusing on agriculture, and ESW and ZT in particular, is threefold. First, agriculture constitutes the main source of livelihood for most of the world's poor (World Bank 2016) and is a common focus in development economics and in the economic literature on technology adoption more generally. Moreover, farmers all over the world are increasingly confronted with changes in agricultural conditions due to climate change, which ESW and ZT can help to address. The insights from our experiment will therefore contribute to the broader effort to identify ways in which farmers can mitigate negative consequences of climate change. Finally, as elaborated below, ESW and ZT each have a distinct set of features that make them particularly suitable for empirically testing the hypotheses arising from our theoretical framework. This will allow us, for the first time, to characterize the distinct roles of general versus tailored information in affecting adoption decisions of different types of technology, and thereby make a more general contribution to the literature on barriers to technology adoption.

Overall, our study is designed to make two major contributions. First, we will generate the first field-experimental evidence on farmers' adoption of ESW and ZT, obtaining insights that are highly valuable for designing programs aimed at increasing uptake and optimal usage of these and potentially other agricultural innovations that allow farmers to mitigate the negative consequences of climate change, thereby helping to build resilience in food systems against environmental shocks.

Second, our study will contribute to the broader economic literature on barriers to technology adoption. Specifically, our empirical evidence will help in assessing the distinct roles of providing general versus tailored information in affecting adoption decisions across different types of technology. Because general and tailored information imply different degrees of information sharing among peers (social learning) compared to individual information processing, our experiment also relates to the literature on different information frictions, such as 'learning effects' and 'costly information processing' (or costly attention), as we discuss in more detail below. Importantly, information frictions have been shown to apply to a wide range of technologies, including not only agriculture, but also health, nutrition, production and management-related innovations, and many others (see the surveys in Handel and Schwartzstein 2018, Maćkowiak et al. 2023). The insights derived from our analysis will therefore be broadly applicable and have the potential to increase our understanding of the determinants of technology adoption beyond the specific technologies considered here.

Outline

The remainder of this document is organized as follows. The rest of this section provides additional information on ESW, ZT, and wheat farming in India. Section 2 presents the proposed research design, including the theoretical framework, hypotheses, interventions, sampling and randomization strategy, and power analysis. Section 3 describes the timeline, outcomes, and data collection methods, including insight from our preliminary field work. Section 4 discusses the empirical strategy as well as potential limitations and challenges of our study. Section 5 provides further insights on the intended interpretation of the results, contribution to the literature, and policy implications. Sections 7 and 8 contain information about the study protocols and administrative aspects of the project.

Background on ESW and ZT

Wheat contributes a crucial share of the calories and protein consumed in India, and is mostly grown in the North-western plains commonly referred to as India's wheat belt region (Tripathi and Mishra, 2017). One of the most pressing problems farmers in this region face are increases in temperature and water shortages at the end of the wheat growing season, which increasingly lead to widespread harvest failures and food insecurity (Mukherjee et al. 2019, Daloz et al. 2021). Recently developed ESW varieties offer a potential solution to this problem as they have been bred to feature higher early heat tolerance and thus can be sown earlier (in October) compared to conventional varieties which can only be sown later (in November or December).[4] Moving the entire cropping cycle (including harvest) forward helps to reduce the risk of crop damage and yield losses caused by heatwaves and water stress frequently arising at the end of the Rabi season (Lobell et al. 2013, Dubey et al. 2020). While ESW has been praised as a potential gamechanger for farmers in adapting to climate change, our preliminary field work indicates that adoption rates among smallholder farmers in the study region remain low, and systematic evidence on the roles of different barriers that these farmers face in adopting ESW is lacking. Specifically, when we interviewed 1206 wheat farmers in Haryana and Punjab in 2022, only 14.4% reported to use ESW.

Zero tillage (or no-till farming) is an agricultural technique where crops are sown without disturbing the soil through tillage, thereby decreasing the amount of soil erosion. Especially in areas with dry soils this offers several benefits, including an increased amount of water that infiltrates into the soil, soil retention of organic matter, and better nutrient cycling (Erenstein and Laxmi 2008, Keil et al. 2020). A modern approach to ZT is to use seeding machinery such as 'Happy Seeder' and 'Super Seeder' which, in one pass, open grooves in the soil, drop in wheat seed (and possibly fertilizer), and cover the seeded row. In contrast, the traditional method for planting wheat in India involves first removing the residues of the previous crop (often through burning), followed by multiple tractor passes to plow, harrow, plank, and sow. ZT is therefore commonly seen as a way to reduce crop residue burning, which is a major environmental and health concern in India.[5]

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[4] Conventional wheat varieties are not well-suited for early sowing in India since the warm temperatures in October lead them to grow rapidly and accumulate less biomass, resulting in lower yields (Dubey et al. 2020). ESW varieties mature more slowly and can, with appropriate management practices, exhibit a considerable yield advantage over conventional varieties (Paudel et al. 2023). Examples of ESW varieties are DBW 187, DBW 303, and WH 1270 which were developed by the International Maize and Wheat Improvement Center (CIMMYT) and released in India in 2020 following a series of government-coordinated trials which suggested potential yield increases of 0.5 to 1.0 tons per hectare (Kumar et al. 2021).

[5] ZT may be viewed as a complementary technology to ESW as it allows farmers to avoid time-intensive tasks associated with soil preparation prior to sowing, thereby facilitating early sowing (Lobell et al. 2013, Keil et al. 2021). At

Our field experiment will assess the impacts of different information treatments on farmers' adoption decisions of ESW and ZT. The interventions are aimed at supporting farmers in switching from conventional wheat cultivation to ESW, with the goal of reducing crop failure and thereby increasing wheat yields, farmers' incomes, and food security in India more generally. The interventions have been designed in close collaboration with two local partner NGOs to address some of the major constraints farmers face when adopting new agricultural technologies, and ESW and ZT in particular. In line with some of the above-mentioned studies, our preliminary field work identified information constraints as an important barrier to adoption. Specifically, when we asked wheat farmers about their main reasons for non-adoption or discontinuation in usage, "lack of information" was the most frequent response both for ESW (54.2%, multiple responses possible) and for ZT (58.9%).[6] Without ruling out the possibility of other relevant constraints in this context, the focus of our interventions lies on addressing different information frictions, such as limited knowledge about the profitability of ESW and ZT, imperfect information about the optimal timing of agricultural tasks when switching from conventional wheat varieties to ESW, and limited attention to environmental factors that affect optimal usage practices and ways of combining ESW with other agricultural inputs.

2. Research Design

Theoretical framework

This section presents a simple model to theoretically differentiate between general and tailored information provision in the context of technology adoption, which is key to understanding the broader contribution that our experiment seeks to make.

Individual decisions to adopt a specific technology can be studied using target input models (e.g., Foster and Rosenzweig 1995). An agent i has to make a choice whether and how to adopt a new technology. Non-adoption gives a fixed payoff which is normalized to zero. If the technology is adopted, the realized payoff U_i dependents on the proximity of the chosen action a_i to the optimal action a_i^*:

$$\max_{a_i} U_i = \bar{u} - |a_i - a_i^*| \tag{1}$$

where $\bar{u}$ is a fixed parameter value. The value of a_i^* is initially unknown so that the agent has to form expectations based on the set of information I available to the agent at that time:

$$a_i = E[a_i^*|I] \tag{2}$$

Based on this framework, general and tailored information can be incorporated and jointly studied by assuming that a_i^* is a function of two components:

$$a_i^* = f(\theta, \varepsilon_i) \tag{3}$$

the same time, the agronomic benefits of ZT apply equally to other wheat varieties, and ZT has been promoted in India long before ESW varieties were invented. Conversely, ESW was not specifically designed to be combined with ZT and is generally believed to be feasible and profitable even when cultivated using traditional tillage and soil preparation methods. We therefore expect the quantitative magnitude of the complementarity to be relatively small, and mostly limited to farmers with a very short fallow time between the Kharif and Rabi season (e.g., due to late harvesting of rice in the Kharif season).

[6] Other possible responses included limited access, high cost, lack of trust in the technology, unsuited soil conditions, fear of yield loss, limited service provision or machinery, and others.

The first term, θ, is a systematic component which captures all factors influencing a_i^* that are persistent across space (and time). For example, in the context of fertilizer adoption, this corresponds to the general knowledge about the features and 'best practices' of fertilizer use that essentially apply to all farmers and across different growing seasons. The second term, ε_i, is an idiosyncratic component that captures individual-specific factors influencing a_i^*. Unlike θ, the idiosyncratic component may vary across individuals (within the same period). For instance, one can think of ε_i as capturing fluctuations in individual-specific conditions that determine optimal fertilizer use such as current plot soil conditions, the household's financial situation, and availability of family labor.[7]

Both θ and ε_i are initially unobserved such that the agent faces uncertainty about the optimal action a_i^*. Consequently, we refer to information that reduces uncertainty about θ as general information, and to information that reduces uncertainty about ε_i as tailored information.

In practice, the relative importance of θ and ε_i in determining a_i^* will likely differ across technologies. For some technologies, knowing the systematic component may be enough for profitably using the technology. We will refer to these technologies as 'simple technologies'. For other technologies, being attentive to the idiosyncratic component will be crucial; that is, even perfect knowledge of θ is not sufficient for profitable usage (in expectation). We will call these 'complex technologies'. The idea is that technologies can be classified on a spectrum ranging from ‘simple’ to ‘complex’ technologies, based on the relative importance of θ and ε_i. To the extent that technologies belonging to each group can be identified, the theoretical framework gives rise to a rich set of predictions regarding the distinct roles of general versus tailored information in affecting optimal adoption decisions for different types of technology. The next section will use these predictions to derive testable hypotheses which in turn will inform the design of our field experiment.

Before we turn to these hypotheses, note that the model also generates testable predictions regarding information spillovers. To see this, suppose a program provides information about θ to a user of a new technology. Clearly, this user may share the information with other potential users as knowledge about θ will be equally useful to them. In contrast, a program which focuses on supporting users in reducing uncertainty about their individual-specific value of ε_i will not feature the same potential for information spillovers. Of course, these users may share the obtained information with others, but this information will be of little value to them given that their own realizations of ε_i will generally be different. The model thus implies that technologies for which tailored information is crucial will tend to feature less scope for information spillovers than technologies for which profitable usage practices can be derived from general information.

Hypotheses

The cluster RCT will test two sets of hypotheses arising from the theoretical framework above. The first set focuses on the distinct impacts that different types of information treatments (general versus tailored information provision) can have (or cannot have) on adoption decisions for different types of technology.

[7] Future research might find it useful to study an extension of the model that adds a time dimension and distinguishes between (a) factors that vary over time but not across individuals within the same area (e.g., general weather patterns and market prices) and (b) factors that vary across individuals but not across time (e.g., caste and religion in the context of India). For the application considered here, this is not necessary.

Specifically, the framework outlined in equations (1) to (3) implies that interventions which solely provide general information (on θ) may be effective in increasing adoption rates of ‘simple’ technologies but not of ‘complex’ technologies. For ‘complex’ technologies, increasing adoption rates will also require providing advice which is tailored to each farmer’s individual-specific conditions or supporting them in being attentive to the factors captured by ε_i. Thus, the model gives rise to the following three predictions:

- **H1**: Providing general information on θ will be effective in increasing adoption rates of ‘simple’ technologies.
- **H2**: Providing general information on θ will not be effective in increasing adoption rates of ‘complex’ technologies.
- **H3**: Increasing adoption rates of ‘complex’ technologies requires the provision of information on θ and information tailored to each farmer’s individual-specific conditions ε_i.

The second set of predictions arising from the model focus on information spillovers between potential users of technology:

- **H4**: General information on θ can be shared between peers, such that providing information on θ to some users will generate positive spillovers on adoption rates of other potential users of ‘simple’ technologies.
- **H5**: Tailored information on ε_i cannot easily be shared between peers, such that providing advice to some users based on their individual-specific realization of ε_i will not generate spillovers on adoption rates of other potential users.

In testing Hypotheses H1 - H5 empirically, the primary outcome of interest will be observed adoption decisions of ‘simple’ and ‘complex’ technologies, respectively. Of secondary interest will be users’ perceptions towards the different technologies and plans for future adoption. Given the agricultural focus of our field experiment, it will also be of interest to collect and analyze data on agricultural outcomes and practices related to the adoption of the studied technologies, such as yields, occurrence of crop failure, agricultural income, and other farm input choices. As described in detail below, data on these variables will be collected during a pre-intervention baseline and a post-intervention endline survey using different survey instruments (see Section 3). Our main analysis will compare adoption rates between different treatment arms. Since the regression framework for this analysis will be based on data at the individual level, there is no need for specifying an aggregation method (see Section 4).

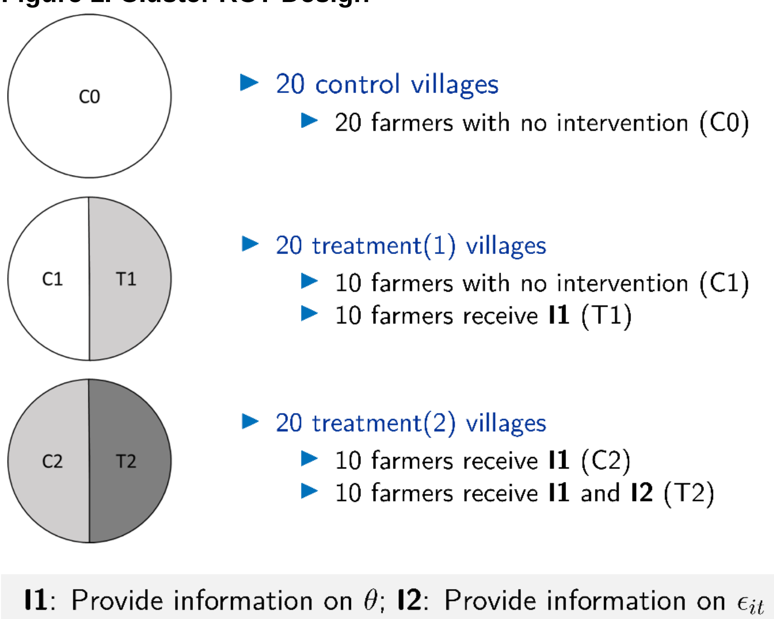
Identification strategy

We will conduct a cluster RCT with multiple treatment arms designed to test Hypotheses H1 - H5. First, this requires identifying two types of technology corresponding to a ‘simple’ and a ‘complex’ technology according to the above definition. In principle, these could be technologies from any sector (e.g., health or nutrition technologies) as the theoretical framework and predictions are very general. In the context of our study, the requirements will be met by focusing on ESW and ZT. In particular, recall from the Introduction that adopting ESW requires farmers to adjust the timing of all agricultural tasks arising over the cropping cycle (from land preparation for sowing until harvesting). Given the sensitivity of returns to the timing (as well as quantities and types) of farm inputs such as fertilizer and other agrochemicals, and tasks such as weeding and irrigation, this represents a complex set of decisions requiring farmers’ attention to individual-specific factors such as the current financial situation of the household, changes in soil moisture and early signs of upcoming pests or diseases on their plots, and temporary (un)availability of family labor, among many others. In sum, this suggests that adopting ESW in profitable ways requires farmers' continued attention to individual-specific factors; that is, knowledge of the general properties of ESW and its 'best practices' (i.e., those factors captured by θ) are not sufficient. Thus, ESW qualifies as a 'complex technology'. In contrast, ZT represents a 'simple technology', given that it only concerns the method of soil preparation and sowing, without affecting the optimal timing and other parameters of agricultural tasks over the rest of the cropping cycle.

Moreover, two different information treatments will be implemented, one designed to provide general information on θ and the other designed to provide tailored information based on ε_i (more details below).

To permit testing our hypotheses, randomization of the participating farmers into treatment arms will be conducted both between clusters (i.e., villages) and within clusters as illustrated in Figure 1. Specifically, there will be three types of villages: Control villages, Treatment(1) villages, and Treatment(2) villages. The Control villages constitute a pure control group in which the participating farmers will be asked to respond to our baseline and endline surveys, but will not benefit from any intervention. The participants within each Treatment(1) village will be randomly assigned to one of two groups. The first group (T1) will receive the information treatment on θ (Intervention 1), whereas the second group (C1) will receive no information treatment regarding ESW or ZT (to facilitate implementation and compliance, the farmers assigned to C1 will receive a placebo; see below). Similarly, the participants within each Treatment(2) village will be randomly assigned to either the T2 or the C2 group. Participants in T2 will receive the information treatment on θ (Intervention 1) <u>and</u> the information treatment on ε_i (Intervention 2), whereas the participants in C2 will only receive Intervention 1.

Figure 1. Cluster RCT Design



Notice that the farmers in C0 and C1 both receive no direct treatment. However, the farmers in C1 may potentially benefit *indirectly* via information spillovers from their neighbors and friends living in the same village but assigned to T1. In contrast, farmers in C0 are not exposed to such potential within-village information spillovers (we minimize the risk of information spillovers *across* villages by ensuring that all villages in our sample are located sufficiently far from each other). Similarly, both farmers in T1 and C2 receive the same direct treatment (Intervention 1). However, the farmers in C2 may potentially also be exposed to within-village information spillovers regarding ε_i from their peers in T2 (in the same village, respectively).

Based on this study design, we can test the hypotheses formulated above in a straightforward way by comparing the adoption rates of ESW and ZT across different treatment arms. Specifically, Hypotheses H1 and H2 assert that information treatments focused on θ may be effective in increasing adoption rates for 'simple' technologies but not for 'complex' technologies. This will be tested by comparing adoption rates (at endline) between the farmers in T1 and C0:

- **H1**: The adoption rate of ZT in T1 is larger than the adoption rate of ZT in C0.
- **H2**: The adoption rate of ESW in T1 is the same as the adoption rate of ESW in C0.

Moreover, Hypothesis H3 states that increasing adoption rates of 'complex' technologies also requires the provision of individual-specific advice based on ε_i. This will be tested by comparing adoption rates of ESW between the farmers in T2 and C0:[8]

- **H3**: The adoption rate of ESW in T2 is larger than the adoption rate of ESW in C0.

Hypotheses H4 and H5 pertain to information spillovers for which the splitting of participants to different groups within villages becomes crucial. Specifically, these two hypotheses assert that spillovers will arise for provision of general information on θ (Intervention 1) but not for information tailored to individual-specific conditions ε_i (Intervention 2). Recall from Figure 1 that the farmers in C0 and C1 both receive no direct treatment, but those in C1 may benefit indirectly through information spillovers regarding θ from the farmers in T1 (within the same village). Similarly, both farmers in T1 and C2 receive the same information regarding θ, but only those in C2 may potentially benefit from spillovers regarding ε_i from the farmers in T2. Thus, Hypotheses H4 and H5 will be tested by comparing adoption rates between the farmers in C1 and C0, and between C2 and T1, respectively:

- **H4**: The adoption rate of ZT in C1 is larger than the adoption rate of ZT in C0 (spillovers from T1 to C1 for ZT). But the adoption rate of ESW in C1 is the same as the adoption rate of ESW in C0 (no spillovers from T1 to C1 for ESW).
- **H5**: The adoption rate of ESW in C2 is the same as the adoption rate of ESW in T1 (no spillovers from T2 to C2 for ESW). And the adoption rate of ZT in C2 is the same as the adoption rate of ZT in T1 (no spillovers from T2 to C2 for ZT).

In principle, our random assignment is expected to lead to comparable groups so that these hypotheses can be tested simply by looking at differences in adoption rates between the respective groups at the end

[8] Our theoretical framework predicts that moving from information focused on θ to providing information on both θ and ε_i will have a systematically larger impact on adoption rates for 'complex' technologies (ESW) than for 'simple' technologies (ZT). Thus, while we do not rule out the possibility that Intervention 2 may also be helpful in (further) increasing uptake of ZT, the focus in empirically validating the theoretical framework here concerns ESW.

of the program (based on the endline survey). Since in practice some imbalances may arise even with successful randomization, and given our baseline data collection is in place, we will also test the hypotheses when controlling for baseline characteristics (see Section 4).

Interventions

Based on the theoretical considerations above, two different interventions will be implemented. Each intervention will consist of a package of information treatments designed to support farmers in adopting ESW and ZT by addressing one of the two information frictions described in Section 2.

Intervention 1 comprises a one-time training workshop focused on increasing farmers' knowledge about the general features and benefits of using ESW and ZT, with the goal of facilitating uptake and usage of these two technologies. The content of the training is developed by Charities Aid Foundation (CAF), our Indian partner organization, and Sir Syed Trust (SST), the NGO that will implement the program.[9] Our guiding principle for the design of the training materials was to focus on general information that is equally useful to all wheat farmers in the study region (i.e., addressing a need for information on the systematic component θ of using ESW and ZT). For instance, this intervention will inform the participating farmers about the nearest places where ESW seeds are available for purchase, provide a list of contact details of places where ZT equipment is available for lease, and communicate practical advice how ESW and ZT can be used in the study region (focusing on general 'best practices'). In contrast, no individual-specific information tailored to the needs of individual farmers will be provided in this intervention.

Given the concern that excluding some participants in Treatment(1) villages from the training may lead to discontent and an increased risk of non-compliance, those participants assigned to the C1 group will receive a placebo training. Specifically, the implementing NGO will conduct two trainings in each Treatment(1) village which will take place at the same time but in different locations. While the real treatment (Intervention 1) focuses on ESW and ZT, the placebo training will provide information on mustard cultivation, a Rabi crop which many farmers in the study region grow in addition to wheat (on separate plots).[10]

Intervention 2 consists in providing handholding support tailored to each individual beneficiary, assisting farmers in staying attentive to changes in their respective environmental conditions and reacting in appropriate ways to any challenges arising on their wheat plots, thereby reducing uncertainty about optimal usage practices and timing of agricultural tasks related to ESW and ZT (i.e., addressing a need for being attentive to fluctuations in the idiosyncratic component ε_i). Specifically, prior to the start of the Rabi season 2024/25, the NGO will send out their staff to visit each farmer assigned to the T2 group to introduce themselves and offer support for ESW and ZT. Based on a soil test of the participant's plots, the staff will provide recommendations regarding the optimal usage of ESW, ZT, and related agricultural inputs. Farmers will also be given the contact details of agricultural experts whom they can reach out for support via phone or social messenger app at any time during the cropping cycle, including the possibility to request technical support at doorstep in the form of field visits to the farmer's plots. The NGO staff will follow up individually

[9] More information about our two partner NGOs can be found on their websites: https://cafindia.org and http://www.sirsyedtrust.com.

[10] Focusing on mustard as a placebo was recommended by our Indian partner NGO both for practical reasons (including sufficient interest among farmers) and because they expected this to have only neglectable effects on farmers' decisions regarding wheat cultivation. In contrast, focusing, for instance, on rice (a major Kharif crop which is often planted on the same plots where afterwards wheat is grown) was seen as problematic as it might influence the fallow times of these plots and thereby potentially affect the feasibility of early sowing of wheat.

with each farmer on a periodic basis to extend support for any upcoming issues. In contrast to Intervention 1, the type of information provided in Intervention 2 will be highly individual-specific and tailored to each farmer's individual conditions.

Sample and randomization

The program will engage with 1,200 farmers in 60 villages in the Sonipat district (Haryana), India. This area is part of India's wheat belt region and was selected for our cluster RCT because our partner NGO has been active in this area before, so that the design of our interventions can build on their experience in working with wheat farmers in this area. The selection of villages and participants proceeded in steps. First, the NGO followed their standard procedures to identify villages that they considered eligible for expanding their agricultural program. This resulted in a list of 100 villages. From this list, we selected 60 villages to be included in the sample for our baseline survey. To minimize the risk of information spillovers *across* villages (which would threaten the validity of our pure control group C0), we selected the 60 study villages under the restriction that they are located sufficiently far away from each other. Specifically, we considered the pairwise distance between villages, and then selected villages iteratively, at each step filtering out villages with less than 15 minutes travel time to any previously selected village.

The next step was to identify 20 participants in each of the 60 study villages. Based on our experience during the preliminary field work conducted in 2022, most villages do not maintain an up-to-date list of households living in the village or were unable to share it with us.[11] In the absence of access to any list of farmers that we could use for sampling, we will rely on the same ad-hoc approach that we had eventually adopted in our preliminary field work, and which turned out to work quite well in the study region. Specifically, we will instruct our enumerator teams to identify at least 22 potential participants in each village (20 to participate in the RCT and 2 as potential replacements) when arriving at a village for conducting the baseline survey (e.g., by approaching people in public spaces or by knocking at people's doors). There are four eligibility criteria for participants: (i) the respondent has to be at least 18 years of age, (ii) be a wheat farming household (cultivated wheat in the previous Rabi season and planning to cultivate wheat in the Rabi season 2024/25), (iv) have the ability to make their own farming decisions (e.g., which crops to plant and what technologies to adopt),[12] and (iv) be willing to participate in our study. This will result in a convenience sample of around 1,320 wheat farmers.

In a third step, following the completion of the baseline survey, we will randomly assign each of the 60 villages to either serve as a Control village, a Treatment(1) village, or a Treatment(2) village (see Figure 1). Finally, we will use the lists of potential participants in each village identified by our enumerators during the baseline survey to randomly assign farmers to treatment arms within each village. Specifically, in Treatment(1) villages, 10 farmers will be assigned to the C1 group and 10 farmers to the T1 group. In Treatment(2) villages, 10 farmers will be assigned to the C2 group and 10 farmers to the T2 group. In Control villages, all 20 farmers will be assigned to the C0 group (see also Figure 1). An overview of the number of farmers by intervention and treatment arm is provided in Table 1.

[11] We had initially been told that each village head in Haryana was supposed to keep such a list. However, when we visited villages during our preliminary field work, it was typically not possible for the village head to produce such a list for our enumerator team, and in the few cases where a list was presented, it mostly turned out to be incomplete.
[12] This requirement is made to exclude households who do not make their own farming decisions and thus are not able to choose which agricultural technologies to adopt (e.g., landless farm laborers).

Table 1. Number of Farmers by Intervention and Treatment Arm

Villages		Farmers			
		Baseline + Endline Survey	Intervention 1	Placebo Training	Intervention 2
Control	20	400	0 (C0)	0	0
Treatment(1)	20	400	200 (T1)	200 (C1)	0
Treatment(2)	20	400	400 (C2, T2)	0	200 (T2)
TOTAL	**60**	**1200**	**600**	**200**	**200**

Importantly, to guard against any potential conflict of interest, both the randomized selection of the 60 villages and the assignment of participants into treatment arms within each village will be conducted by the research team at University of Goettingen (independently of the implementing NGO). Also, as further explained below (Section 3), the NGO will only be engaged in the implementation of the interventions, whereas the baseline and endline data collection will be organized by a different team of enumerators who will be recruited, trained, and monitored directly by the research team from University of Goettingen.

Participants will be made aware of the overall focus of the study; that is, to learn about sustainable wheat cultivation practices in the study region and provide information on the use of ESW and ZT to mitigate climate-related challenges in wheat cultivation. At the same time, participants will not be informed about any of the trial hypotheses, underlying theoretical framework, and empirical strategy, in order to minimize the risk of performance and expectancy biases.

Power analysis

Our primary outcome variables, adoption of ESW and adoption of ZT, will be observed at the individual (farmer) level in the form of binary indicators. Our power calculations are therefore based on the formula for cluster randomized controlled trials with binary outcomes (Djimeu and Houndolo 2016):

$$\mu_1 = \frac{z - 2\mu_0(n - Jn)}{z(K^2 n - 1) + n - Jn}\left(\sqrt{z^2 - 4z\mu_0(z - 2n + 2Jn - zK^2 n)(\mu_0 - K^2 n\mu_0 - 1)}\right)^{-1} \quad (4)$$

with the following notation:

- $z = (z_1 + z_2)^2$, where z_1 is the z-value corresponding to the desired significance level (α) and z_2 is the z-value corresponding to the desired power (β),
- μ_0 is the true (population) proportion in the absence of the intervention,
- μ_1 is the true (population) proportion in the presence of the intervention,
- n is the number of individuals in each cluster (village),
- J is the number of clusters in each group,
- K is the coefficient of variation of true proportions between clusters within each group.

All power calculations are based on hypothesis tests with a 5% significance level (i.e., $\alpha = 0.05$). The assumed values of μ_0 are based on the data collected during our preliminary field work, where we found that 14.4% of farmers in areas close to the study region use ESW and 24.5% of farmers use ZT. As explained in more detail below (Section 3), we expect current adoption rates of ESW and ZT in our study region to be very similar to these rates. At the same time, our preliminary field work is not well suited to provide estimates of the coefficient of variation between clusters (see also below). We therefore follow a notion in the literature (see Hayes and Bennett 1999, p. 321) and calculate statistical power for different values of K ranging from 0.1 to 0.25.[13] Similarly, we allow β to vary between 0.7 and 0.9.

Our power analysis is designed to calculate the smallest value of μ_1 (i.e., the adoption rate of ESW or ZT in the presence of a particular intervention) that we can expect to be able to detect under the assumed parameter values. This approach mirrors the calculation of minimum detectable effects (MDE) typically considered in power calculations with continuous outcome variables, though the quantitative interpretation of the MDE estimates needs to be adjusted accordingly (for simplicity, we will refer to μ_1 as MDE).

Below, we first present power calculations for estimating the overall impact of the program (without accounting for information spillovers) and then for the refined specifications that will be used in testing the hypotheses.

Power calculations for estimating the overall impact of the program (without information spillovers)

Table 2 presents the estimates of μ_1 (MDE) when estimating the impact of Intervention 1 on adoption of ESW and ZT using the basic regression model specified in Section 4 (equation 5). These power calculations are based on the following parameter values: $\alpha = 0.05$, $\beta \in \{0.7, 0.8, 0.9\}$, $n = 10$, $J = 40$, $K \in \{0.1, 0.15, 0.2, 0.25\}$, $\mu_0^{ESW} = 0.144$, $\mu_0^{ZT} = 0.245$.

For instance, these estimates suggest that for $\beta = 0.7$ and $K = 0.1$, we will be able to detect a relative increase in the adoption rate of ESW of 6.6%-points (e.g., from a baseline of 14.4% to 21.3%) under Intervention 1, and an increase in the adoption rate of ZT of 8.2%-points (from 24.5% to 32.7%), both at a statistical significance level of 5%. For $\beta = 0.8$ and $K = 0.2$, we will still be able to detect an increase of 8.1%-points for ESW and an increase of 9.8%-points for ZT.

The power calculations for the treatment package that combines Interventions 1 and 2 (which is implemented in the T2 group) are based on the same parameter values as for Intervention 1 above, except that there are only 20 clusters in the treatment group (still 40 clusters in the control group). As a conservative approach, we use $J = 20$. The resulting estimates of μ_1 are reported in Table 3, and turn out to be only slightly higher than those in Table 2.

These MDE estimates are well within the range of effect sizes of similar agricultural extension interventions documented in the literature. For example, Benin et al. (2011) find that a nationwide extension program in Uganda (which provided specialized advisory services to farmers similar to our interventions) increased adoption rates among participating farmers relative to nonparticipants by 12%-points for improved seeds and 15%-points for another recommended agricultural practice called spacing. Cole and Fernando (2012) find that a mobile-phone based agricultural advisory service (similar to the phone-based advisory component in our Intervention 2) among farmers in India increased adoption rates of a new crop by 8%-points as well as adoption rates of more effective pesticides by 10%-points, relative to the control group.

Moreover, there are several factors which increase our confidence that we will be able to identify statistically significant effects in this experiment. First, our study focuses on two innovations that were only recently

[13] K is similar but not identical to the inter-cluster correlation (ICC) typically considered in power calculations with continuous outcome variables (for the relationship between ICC and K for binary outcomes, see Pagel et al. 2011).

introduced in the study region and have not yet been the focus of existing extension programs. Together with the fact that adoption rates of ESW and ZT are currently still quite low while farmers' interest in these technologies is generally high (according to our preliminary survey work in 2022 as well as anecdotal evidence from our Indian partner NGOs), and given recent exposure to heat waves and water shortages which ESW and ZT help to address, this suggests that there is large scope for increasing adoption rates of ESW and ZT in our study region.

Second, our preliminary survey work strongly suggests that the most important factor currently limiting the wider adoption of ESW and ZT is lack of knowledge and information (see Section 3, including farmers' self-reported reasons for non-adoption of ESW and ZT summarized in Table 4). In contrast, other potential constraints, such as financial constraints or unsuited (soil) conditions for these technologies, do not appear to be binding for most farmers. This provides a strong motivation for the focus of our interventions on addressing information constraints, and fosters our confidence that these interventions will lead to significant increases in the adoption rates of ESW and ZT among the treated farmers.

Finally, the MDE estimates presented in Tables 2 and 3 should be viewed as conservative because (a) they are based (for simplicity) on $n = 10$ while in fact the pure control group C0 will feature 20 farmers, and (b) our regressions will include covariates as controls, which should further improve the precision of our estimates of interest.

Table 2. Power Calculations for Intervention 1

Power (β)	Coefficient of Variation (K)			
	0.10	0.15	0.20	0.25
Early Sown Wheat ($\mu_0 = 0.144$):				
0.7	0.213	0.214	0.215	0.217
0.8	0.222	0.223	0.225	0.227
0.9	0.236	0.237	0.239	0.242
Zero Tillage ($\mu_0 = 0.245$):				
0.7	0.327	0.329	0.332	0.335
0.8	0.338	0.340	0.343	0.348
0.9	0.353	0.356	0.360	0.365

Table 3. Power Calculations for Combined Interventions 1 and 2

Power (β)	Coefficient of Variation (K)			
	0.10	0.15	0.20	0.25
Early Sown Wheat ($\mu_0 = 0.144$):				
0.7	0.245	0.247	0.250	0.253
0.8	0.260	0.262	0.265	0.269
0.9	0.280	0.283	0.287	0.292
Zero Tillage ($\mu_0 = 0.245$):				
0.7	0.364	0.367	0.372	0.378
0.8	0.380	0.384	0.390	0.397
0.9	0.402	0.407	0.415	0.424

Power calculations for testing Hypotheses H1 – H5 (accounting for information spillovers)

We now turn to the power analysis for testing our hypotheses using the regression model specified in equation 6 (Section 4). Notice that three of our hypotheses (H1, H3, H4) require identifying significant differences in adoption rates between different treatment groups, while the other two hypotheses (H2, H5) imply equal (or statistically not distinguishable differences in) adoption rates between groups. For the former, there are 20 clusters both in the relevant treatment group and in the relevant control group, respectively.[14] Thus, the power calculations for Hypotheses H1, H3, and H4 are equivalent to those reported in Table 3 (which should be viewed as conservative for the same reasons as stated above).

For Hypotheses H2 and H5, power calculations are less relevant at this stage because lower power will in fact work in our favor when verifying these two hypotheses (i.e., lower power will make it more likely that we cannot reject Hypotheses H2 and H5). Thus, for these two hypotheses, power calculations will play an important role when analyzing the results of the cluster RCT after its implementation. Specifically, for H2 and H5 we will conduct power calculations during our analysis based on the data collected in the baseline and endline surveys to determine whether the inability to reject these hypotheses is a valid finding or may be merely due to low statistical power.

3. Data

The data for our analysis will be collected during a pre-intervention baseline survey and a post-intervention endline survey which will be administered to all 1,200 farmers in our sample (across treatment arms). In addition, this section will describe the data already obtained during our preliminary field work.

Timeline

Preliminary field work was conducted in September and October 2022 to inform the design of our experiment and its interventions. Preparations for the cluster RCT started shortly after. The baseline survey will be conducted in March 2024, and be completed before the start of the Indian general election in April/May 2024. Implementation of the first treatment package (Intervention 1) will take place between May and July 2024. Implementation of the second treatment package (Intervention 2) will start at the same time and run until April 2025. The endline survey will be conducted in April and May 2025 (after wheat harvest at the end of the Rabi season 2024/25). Upon completion of the endline survey, we will perform the analysis and write the research paper, which we hope to complete within seven months. A more detailed timeline for the entire project is provided below:

- **September – October 2022**: Preliminary field work
- **November 2022 – May 2023**: Design of cluster RCT
    - Finalization of experimental design (informed by preliminary field work)
    - Identification of local partner NGOs for implementation

[14] Specifically, testing Hypothesis 1 involves comparing the adoption rates of ZT between the 20 villages in T1 and the 20 villages in C0. Testing Hypothesis 3 involves comparing the adoption rates of ESW between the 20 villages in T2 and the 20 villages in C0. Testing Hypothesis 4 involves comparing the adoption rates of ZT between the 20 villages in C1 and the 20 villages in C0.

    - Fine tuning of interventions, questionnaire, and sampling frame (in collaboration with partner NGOs)
- **June 2023 – February 2024**: Preparations for field work, hiring of enumerators
- **March 2024**: Baseline survey
- **May – July 2024**: Start Interventions
    - Implementation of Intervention 1
    - Start implementation of Intervention 2: NGO staff visit each farmer assigned to the T2 group to introduce themselves, share their contact details, and offer handholding support for ESW and ZT over the Rabi season 2024/25.
- **July – September 2024**: Continue Intervention 2 (prior to wheat sowing in October/November)
    - NGO staff continue to be responsive to farmers' requests for advice and actively reach out to farmer where adequate to offer support in preparing for the upcoming Rabi Season (e.g., where / how much / what varieties of seeds to purchase).
- **October 2024 – April 2025**: Continue Intervention 2 (during/after wheat sowing)
    - Farmers can reach out to NGO staff to receive on-phone support regarding agricultural practices (including by sharing photos via social messenger app).
    - Farmers can also reach out to NGO staff to request technical support at doorsteps in the form of field visits to farmers' plots at the farmer's request.
    - NGO staff visit farmers on a periodic basis to extend support for any upcoming issues by suggesting remedial measures.
- **April – May 2025** (after wheat harvest): Endline survey
- **June – December 2025**: Analysis and paper writing

Preliminary field work (Pilot data)

In September and October 2022, we interviewed 1,206 wheat farmers across 70 villages in Punjab and Haryana. The main purpose of this work was to collect information on the current diffusion of ESW and ZT and on the barriers to their wider adoption in this region, ultimately to inform the design of the interventions and experiment. In addition, we tested (and iteratively improved) our survey instruments and generated data to inform the power analysis for the cluster RCT.

The villages sampled for this preliminary work were located in five districts in Haryana (Gurugram, Kurukshetra, Mahendragarh, Nuh, Rewari) as well as two districts in Punjab (Amritsar, Hoshiarpur). These villages had been selected from a sampling frame of 1722 communities in Haryana and Punjab that had been identified as predominantly wheat growing areas based on remote-sensing data from satellite images (for more details, see Naeher et al. 2023). Thus, while we did not yet engage with farmers in the Sonipat district of Haryana (the study region of the cluster RCT), we have recent survey data from wheat farmers living in areas very close to our study region. Current adoption rates of ESW and ZT in our study region can be expected to be very similar to the rates observed in these areas. In the absence of any other data on ESW and ZT adoption from Sonipat itself, we thus use these adoption rates to inform the experimental design of our cluster RCT, including the power analysis. At the same time, our preliminary field work is not well suited to provide estimates of the between-cluster coefficient of variation needed in the power analysis, because, due to the different sampling strategy, the geographic locations and distances between clusters are not comparable to those in the RCT (in the power analysis in Section 2, we therefore rely on simulations for different values of the coefficient of variation informed by the literature).

More importantly, the data collected during our preliminary field work strongly suggests that (a) there is large untapped potential for increasing adoption rates of ESW and ZT in India's wheat belt region, and (b) in facilitating wider adoption of these technologies, addressing binding information constraints is crucial. Specifically, we found that farmers' perceptions towards ESW and ZT are generally very positive, including among those farmers who have already used these technologies on their own fields. At the same time, adoption rates still remain at relatively low levels, with 14.4% of the farmers in the sample reporting to use ESW and 24.5% using ZT. When asked about the main reasons for non-adoption or discontinuation in usage, "lack of information" was the most frequent response both for ESW (54.2%, multiple responses possible) and for ZT (58.9%). In contrast, other factors such as "High cost if the technology is adopted" or "Soil not suited for this technology" were stated much less frequently (see Table 4).

Moreover, it should be noted that, besides "lack of information", our interventions will also address several of the other constraints listed in Table 4, including "limited access" (by providing information about the nearest places where ESW seeds are available for purchase and where ZT equipment can be leased), "Difficulty of choosing best timing for sowing/other tasks when using ESW" (by providing advice on adequate cultivation practices under ESW and ZT, including individual-specific advice in Intervention 2), and possibly "Lack of trust in the technology". Together, this provides a strong motivation for the focus of our interventions on addressing information constraints, and fosters our confidence that these interventions will lead to significant increases in the adoption rates of ESW and ZT among the treated farmers.

Table 4. Self-reported Reasons for Non-adoption of ESW and ZT (Percent of Respondents, Multiple Responses Possible)

Reason	Early Sown Wheat	Zero Tillage
Lack of information about the technology	54.2%	58.9%
Limited access to ESW varieties	33.3%	-
Soil not suited for this technology	29.2%	3.6%
Limited service provision or machinery in the village	25.0%	40.6%
Difficulty of choosing best timing for sowing when using ESW	20.8%	-
Difficulty of choosing best timing for other agricultural tasks when using ESW	16.7%	-
Lack of trust that the technology will work on my plots	16.7%	11.2%
High cost if the technology is adopted	0%	4.0%
High rate of diseases, pests, or weed infestation with zero tillage	-	4.5%

Outcomes and Survey Instruments (Baseline and Endline)

The questionnaires for the baseline and endline surveys have been designed according to the best practices currently applied in agricultural household surveys in the development context (such as the Living Standards Measurement Study - Integrated Surveys on Agriculture; see World Bank 2023). To ensure the accuracy of the questionnaires and their clarity to the respondents, the survey instruments have been thoroughly tested during our preliminary field work (including in focus group discussions with local farmers and agricultural experts in India). Special attention was paid to adapting the questions, response items, and units of measurement to the local context, which has resulted in several improvements in the way questions were formulated and response items were stated (in Hindi).

Primary and secondary outcomes

The primary outcomes of interest are farmers' adoption decisions for ESW and ZT. These outcomes will be observed at the individual (farmer) level in the form of binary indicators based on the questions "*Did you cultivate early-sown wheat varieties on your farm in the last Rabi season?*" and "*Did you use zero tillage on any of your wheat plots in the last Rabi season?*"). Those farmers who report to have used ESW or ZT will also be asked to provide additional information on their usage practices, such as the name of the ESW variety, time of sowing, and type of zero-tillage equipment used (Happy seeder, Super seeder, etc.).

While our interventions are designed to have an impact on adoption rates of ESW and ZT in the Rabi season 2024/25, it is possible that some farmers require more time to switch to ESW and/or ZT even if our interventions are in general effective in facilitating such decisions. The interventions inform participants about ESW and ZT and their potential benefits. They should thus increase (a) farmers' general awareness of ESW and ZT ("*Have you heard of [ESW/ZT] ?*") and (b) farmers' perceived advantages of ESW and ZT over conventional wheat cultivation practices (e.g., "*Do you think there are benefits from [ESW/ZT] compared to conventional wheat cultivation practices along the following aspects ….?*").[15] The next step in the causal chain resulting from improved awareness and perceived advantages would be (c) farmers' stated intention to use ESW or ZT in the future (e.g., "*Are you planning to use [ESW/ZT] in the future?*"). These six variables (three each for ESW and ZT) constitute our secondary outcomes of interest in the causal chain towards adoption.

Other agricultural outcomes, practices, and perceptions

Besides the main outcomes described above, we will also collect information on other agricultural outcomes and practices related to wheat cultivation, including:

- Wheat yields and occurrence of crop failure
- Agricultural income from wheat cultivation (gross and net of input costs)
- Use of agricultural inputs (manure, chemical fertilizers, herbicide, pesticide) on wheat plots
- Irrigation practices and amount of water used on wheat plots
- Labor input and management decisions for different tasks in wheat cultivation (including land preparation, sowing, weeding, irrigating, applying fertilizer and other inputs, harvesting, threshing, grain cleaning)
- Reasons for (non-)adoption of ZT and ESW, respectively (including access, information, costs, trust, and other potential factors)
- Perceived types and magnitude of complementarities between ZT and ESW

The data on yields, occurrence of crop failure, and agricultural income will be used to assess whether adoption of ESW and/or ZT leads to benefits along these agricultural outcomes. The other variables capturing cultivation practices will be used to analyze potential channels through which these benefits are achieved (e.g., to see whether ESW requires less water for irrigation than conventional wheat cultivation, or ZT helps to reduce labor input required for land preparation).

[15] This question will be asked for several aspects, including wheat grain yield, achieved selling price, profit from wheat cultivation net of input costs, cost of non-labor inputs, amount of water used for irrigation, and labor time for different tasks such as sowing, weeding, fertilizer application, herbicide and pesticide application, and harvesting, respectively.

Moreover, because the use of ZT is expected to reduce crop residue burning, a major environmental and health concern in the study region, we will also collect data on:

- Agriculture waste disposal and instance of crop residue burning
- Farmers' perceptions towards the practice of residue burning and air pollution

In addition to survey questions about crop residue burning practices, which might suffer from misreporting bias, we will evaluate satellite images of the land surface from the MODIS database of NASA (https://modis.gsfc.nasa.gov) starting two weeks prior and ending eight weeks after the start of the harvest season. Data on thermal anomalies and fires are readily available in MODIS. Since cloud cover might limit the observation of fires, we will also evaluate changes in the images over time to measure land area where crop residue burning was practiced using learning algorithms. As an exploratory outcome, we will look at atmosphere images from the MODIS database to possibly identify signs of air pollution.

Household and farm characteristics

In addition, the following information will be collected for the purpose of assessing balance in baseline characteristics across treatment groups (in addition to the agricultural outcomes above), accounting for relevant covariates in the regression analysis, and analyzing heterogeneous treatment effects:

- Household demographics: number of adults and children by gender
- Household head (defined as the person in the household who makes farming decisions most of the time): age, marital status, religion, caste, primary occupation, literate, education
- Living standards: asset and wealth indicators, total income in past 12 months
- Perception, beliefs, preferences: perceived changes in weather patterns, past exposure to adverse shocks (including weather shocks such as heat waves and water stress), perceived likelihood of future exposure to shocks, importance of environmental protection, measures of risk aversion
- Farm characteristics: number of plots, total cultivated area, area cultivated with wheat, ownership status of agricultural land
- Social networks: each participant is asked to name up to five other farmers in the village with whom they regularly share information about farming. This information will be used to construct a measure of the connections each farmer has to other farmers who benefitted from the program, which will be used as a control variable to improve power to detect spillovers.

Village-level covariates

Information on additional covariates at the village level will be collected in key informant interviews, including village size (number of inhabitants) as well as the distance from the village center to the district capital and to the nearest seed market.

Contact and tracking information

Finally, we will collect information for the sole purpose of contacting participants as part of the interventions and tracking them between baseline and endline survey, including the name, gender, address, geo-coordinates of home, and mobile phone number of the participants.

Variations from the intended sample size

Attrition

Attrition might happen if some participants drop out during the intervention phase or cannot be interviewed in the endline survey. If this is the case, we will check for selective attrition by comparing the baseline characteristics of the participants who dropped out with those who did not drop out (both within and across treatment and control groups). Specifically, we will estimate a regression model where the dependent variable is an indicator of whether a household could be tracked till endline or not, and the explanatory variables are the same baseline characteristics as considered in our main analysis (see Section 4). The presence of selective attrition would imply that the coefficients of some baseline characteristics are statistically significantly different from zero. In addition, we will check for differences in the rates of attrition between the treatment and control groups (differential attrition). Given the precautions that will be taken during our interventions and survey work, we do not expect to observe differential attrition. However, in case the analysis will suggest otherwise, we will use inverse probability weighting and bounds of treatment effects as suggested in Lee (2009) to address attrition bias concerns in our analysis.

Non-compliance

Non-compliance might be a concern particularly for the farmers assigned to groups C1 and C2, as these farmers reside together in villages with other farmers who get to benefit from more interventions as they themselves (i.e., Intervention 1 in the case of C1, and Intervention 2 in the case of C2; recall Figure 1). To minimize the risk of non-compliance in Treatment(1) villages, the farmers assigned to the C1 group will receive a placebo training (see Section 2 - “Interventions”). The placebo training for the C1 group will be conducted simultaneously to the training on ESW and ZT (for the T1 group) at a different location within the village, and the trainers and NGO field staff will be instructed to ensure that only the farmers assigned to the T1 group will participate in the training on ESW and ZT. In Treatment(2) villages, non-compliance regarding Intervention 1 is less of a concern since farmers both in C2 and T2 receive Intervention 1. For Intervention 2, the implementing NGO will maintain confidentiality and provide individual support only to those farmers assigned to the T2 group. This will be independently monitored during sporadic field visits and based on the consultation records and protocols that the NGO staff are required to maintain.

Missing values

Sample size for the regression analysis may also be negatively affected by missing values in some variables. To guard against this issue, we will attempt to identify unusual missing values and outliers immediately during data collection using high frequency checks (on a daily basis) of incoming data. This will enable us to double check, consult with the respective enumerators, and potentially revise missing or implausible values caused by data entry errors during the data collection in the field. To the extent that implementing this approach will be feasible, we will consider the final dataset to be “complete” and thus do not plan to make additional corrections to the data during the analysis.

Data processing and management

All potential participants will be provided with relevant information and a written consent form prior to the start of the program (see Appendix A: Participant Information and Consent Form). In particular, the consent form specifies that participation is voluntary and that, after choosing to participate, participants have the right to opt out of the program and/or survey at any time without facing any consequences and without having to provide any reason for their decision.

All personal information about the participants will be treated confidentially and we will ensure that individual participants cannot be identified in any publication derived from this study. Specifically, survey responses will be recorded using computer assisted personal interviewing so that no hard copies of completed questionnaires will exist. The collected data will be securely stored in password-protected and encrypted databases. Access to the complete data will be restricted to the research team. A unique household identifier will be assigned to each household. The name, contact information, and geographic location of the respondents will be kept in a separate file to which only the principal investigators will have access. The analysis will be conducted by the research team using anonymized versions of the data that exclude these personal identifiers. The anonymized dataset will be made available for public access together with the publication of the research paper. After completion of the project, all sensitive data which are not required for future replication of the results (as required by some journals) or immediate follow-up projects will be deleted within a year of publication.

4. Analysis

Statistical model and methods

Basic regression model (without information spillovers)

We will start by estimating the impact of the program on outcomes of interest using different specifications of the linear probability model

$$Y_{iv}^{t=1} = \tilde{\beta}_0 + \tilde{\beta}_1 \tilde{T}1 + \tilde{\beta}_2 T2 + X_{iv}^{t=0} \tilde{\gamma} + \tilde{\varepsilon}_{iv}, \quad (5)$$

where $Y_{iv}^{t=1}$ is the outcome of participant i in village v at endline, $\tilde{T}1$ and $T2$ are time-invariant treatment indicator variables, $X_{iv}^{t=0}$ is a vector of controls observed at baseline, and $\tilde{\varepsilon}_{iv}$ is an idiosyncratic error term. The treatment dummy $\tilde{T}1$ is set equal to 1 if the participant is assigned to the T1 group or the C2 group (i.e., pooling all participants who receive Intervention 1 but not Intervention 2), and 0 otherwise. The treatment dummy $T2$ equals 1 for participants assigned to the T2 group (i.e., receiving Interventions 1 and 2), and 0 otherwise. The omitted treatment category in this specification comprises participants assigned to the C0 group or the C1 group (i.e., all participants who do not directly benefit from any intervention). The dependent variable is a binary indicator capturing either adoption of ESW or ZT (our primary outcomes of interest) or one of the secondary outcomes described in Section 3 (general awareness of ESW/ZT, perceived advantages of ESW/ZT, and intention to use ESW/ZT in the future). To increase precision, we will estimate equation (5) both with and without baseline controls $X_{iv}^{t=0}$. These will include the pre-treatment outcome $Y_{iv}^{t=0}$ (observed at baseline) as well as key household, farm, and village-level covariates described in Section 3.

We will estimate equation (5) using Ordinary Least Squares (OLS), clustering standard errors at the village level (alternative estimation methods, ways of clustering standard errors, and corrections for multiple hypothesis testing are described below). Because the autocorrelation of our outcome variables is likely low, this (ANCOVA) approach is expected to offer gains in statistical power relative to alternative methods such as difference-in-differences analysis (McKenzie 2012).

The coefficients of interest in equation (5) are $\tilde{\beta}_1$ and $\tilde{\beta}_2$, which capture the program's intention-to-treat (ITT) effects of the two interventions.[16] In this specification, we are mainly interested in testing whether $\tilde{\beta}_1, \tilde{\beta}_2 > 0$; that is, whether assignment to Intervention 2 and/or Intervention 1 led to an increase in the probability of adopting ESW and ZT, respectively. By including the participants across all treatment arms, the specification in equation (5) is intended to maximize statistical power in identifying ITT effects. It should be noted, however, that by pooling the participants from T1 and C2 (for the treatment dummy $\tilde{T}1$) as well as those in C0 and C1 (for the omitted category), this specification does not account for potential information spillovers within treatment villages (i.e., between groups C1 and T1 in Treatment(1) villages, and between groups C2 and T2 in Treatment(2) villages). These spillovers will be investigated in detail when testing Hypotheses H4 and H5 (see below). In interpreting the results from equation (5), we will therefore draw from those latter results and make sure that our interpretation is in line with any identified spillovers.[17]

Verifying that $\tilde{\beta}_1$, $\tilde{\beta}_2 > 0$ in the model in equation (5) offers a possibility to provide some first suggestive evidence that (a) information frictions are a binding constraint in farmers' adoption decisions of ESW and ZT, and (b) information provision programs can be an effective tool in addressing these constraints and in increasing adoption rates of these technologies. At the same time, our experiment is designed to generate much deeper insights into the distinct roles of different types of information treatments for adoption decisions of different types of technology, along the hypotheses specified above. The next subsection discusses how these hypotheses will be tested.

Testing our hypotheses

To test Hypotheses H1 - H3 in a clean way (i.e., ensuring that the results are not affected by information spillovers within villages), we will estimate the model

$$Y_{iv}^{t=1} = \alpha_0 + \beta_1 T1 + \beta_2 T2 + \beta_3 S1 + \beta_4 S2 + X_{iv}^{t=0}\gamma + \varepsilon_{iv}, \tag{6}$$

where the treatment dummy $T1$ is set equal to 1 if the participant is assigned to the T1 group and 0 otherwise. The treatment dummy $T2$ continues to equal 1 if the participant is assigned to the T2 group and 0 otherwise. $S1$ is a dummy that equals 1 for participants assigned to the C1 group and 0 otherwise. $S2$ is a dummy that equals 1 for participants assigned to the C2 group and 0 otherwise. The omitted treatment category in this specification thus comprises the participants assigned to the pure control group C0.

Based on the regression model specified in equation (6), Hypotheses H1 - H3 will be tested as follows:

- **H1**: $\beta_1 > 0$ for ZT
- **H2**: $\beta_1 = 0$ for ESW
- **H3**: $\beta_2 > 0$ for ESW

[16] Since our treatment indicators are defined based on treatment assignment rather than actual compliance (including take up), the estimated coefficients capture ITT effects.
[17] Notice that the presence of (positive) information spillovers in Treatment(1) villages will work against us in testing $\tilde{\beta}_1 > 0$ and $\tilde{\beta}_2 > 0$; that is, to the extent that we will find $\tilde{\beta}_1$ ($\tilde{\beta}_2$) to be statistically significantly positive, this will tend to provide a lower-bound estimate for the ITT effect of Intervention 1 (Intervention 2) on the considered outcome. In contrast, information spillovers in Treatment(2) villages will likely work in our favor when testing $\beta_1 > 0$ since the treatment dummy $\tilde{T}1$ pools the participants from the T1 and C2 groups. The role of information spillovers will be investigated in detail when testing Hypotheses 4 and 5, and we will use those results to inform our interpretation of the results obtained from estimating equation (5).

If we find either hypothesis H1 not to hold (e.g., we find $\beta_1 = 0$ for ZT) or hypothesis H2 not to hold (e.g., $\beta_1 > 0$ for ESW), then we will also test a weaker version of these hypotheses by verifying whether $\beta_1 < \beta_2$ for ESW and $\beta_1 = \beta_2$ for ZT (i.e., whether providing individual-specific advice in Intervention 2 further increases adoption relative to the provision of general information in Intervention 1 for ESW, but not for ZT).

The presence of within-village information spillovers between farmers according to Hypotheses H4 and H5 will be tested as follows:

- **H4**: $\beta_3 > 0$ for ZT and $\beta_3 = 0$ for ESW
- **H5**: $\beta_4 = \beta_1$ for ESW (and ZT)

Overall, this analysis will generate insights on whether there are systematic differences between general and tailored in-person information provision, including both their direct effects on ESW and ZT adoption and their respective scope for information spillovers among farmers.

Effects of ESW and ZT adoption on agricultural outcomes and practices

Provided that there is evidence that the interventions increase the adoption of ESW and/or ZT, we will make use of the exogenous variation in adoption rates generated by our experimental setup to estimate the causal effects of ESW and ZT adoption on agricultural outcomes and practices of interest. Specifically, we will estimate 2SLS regressions in which usage of ESW and ZT is instrumented with the different treatment assignments and the dependent variable is an outcome of interest from Section 3, including wheat yields, occurrence of crop failure, agricultural income, and practice of crop residue burning (for ZT).

In addition, we will consider the same 2SLS approach to shed light on potential channels through which adoption of ESW or ZT can affect these agricultural outcomes, by using as dependent variable the variables from Section 3 that capture agricultural practices (e.g., to see whether ESW requires less water for irrigation than conventional wheat cultivation and increases income net of input costs, or if ZT adoption lowers labor costs for land preparation).

Multiple outcome and multiple hypothesis testing

In addition to considering individual outcomes of interest separately, we will aggregate outcome groups with several individual variables into a composite index using principal component analysis. Specifically, we will create a composite index for agricultural outputs, agricultural inputs, and perceptions towards ZT and ESW.

When presenting the results of our main analysis, we will report both standard p-values and p-values corrected for multiple hypothesis testing using sharpened false discovery rate q-values as suggested in Anderson (2008). The correction will be applied within the set of primary outcomes and within the set of secondary outcomes.

Heterogeneous effects

We consider heterogenous treatment effects of our interventions across different technologies as a primary dimension of heterogeneity that informs our main hypotheses. For instance, by testing hypotheses H1 and H3, we will investigate whether the impact of providing general information (Intervention 1) differs across ESW and ZT.

In addition, we will estimate further specifications that allow for heterogeneous treatment effects by interacting potential moderators with the treatment indicators. This part of our analysis will focus on the following moderators:

- Gender of the household head
- Educational level of the household head
- Farm size (total cultivated area)
- Past exposure to adverse shocks (including weather shocks such as heat waves and water stress)
- Perceived likelihood of future exposure to shocks
- Risk aversion

Limitations and challenges

The risks associated with this study are expected to be very small. The only real risk associated with the implementation of the field experiment is the possibility of a delay in field work due to the Indian general election between April and May 2024 to elect the members of the 18th Lok Sabha. As described above, our plan is to conduct the baseline survey in March 2024 (finishing well before the start of the election) and to begin the implementation of the interventions in May or June 2024 (after the end of the election). In the rather unlikely event that there will be complications that significantly prolong or delay the election, we will either decide to begin with the interventions nevertheless (if our local partner organizations advice in favor of this) or delay the start of the interventions as necessary. In the latter case, there is still sufficient buffer time since, according to anecdotal evidence from our preliminary field work and various local experts that we spoke to, farmers in Haryana typically only start to prepare for the upcoming Rabi season (e.g., deciding which wheat varieties to plant and where to purchase the seeds) during the three months prior to sowing, i.e., July/August to early October.

Our project also poses minimal risk to the participants. The two agricultural technologies, ESW and ZT, that will be promoted through the program have been widely tested and are seen by relevant Indian authorities and NGOs as useful for farmers in reducing vulnerability of their crops to weather shocks such as drought and heat waves. The training and advisory interventions will be provided by local experts in a comparable manner as many NGOs and extension agencies are operating in this region. Thus, our study involves no more risk than is typical for agricultural extension programs and advisory services, and by collaborating with two experienced Indian partner NGOs it will be ensured that local norms and values will be respected.

Given the strong interest in ESW and ZT technologies among farmers in the study region identified in our preliminary survey work and in line with anecdotal evidence, we expect that (lack of) take up will not be an issue for our study. At the same time, there is no obligation for the participating farmers to follow any of the advice given during the group trainings and individual advisory sessions, and all participants are able to interrupt their participation in the study at any point in time at no cost. The informed consent will stress these points and will contain the contact details of a staff member to request withdrawal from the program (each participant will be provided with a hard copy of the consent form during the initial contact). Since we will not share any data with third parties that allow identification of individual participants, there is no risk of negative consequences from participation in the study for individuals.

One may be concerned about experimenter demand effects, since our primary outcomes (adoption of ESW and ZT) are measured via self-reports. To address this concern, our baseline survey includes a social desirability module which will be used to test whether self-reported adoption is similar in magnitude for respondents with a low versus high propensity for social desirability bias. Specifically, the baseline survey will contain a short form of the Marlowe-Crowne Social Desirability Scale, comprising 5 randomly selected items from the list of 13 items proposed by Reynolds (1982). The same approach has been applied in previous studies in the development context in South East Asia (Bahety et al. 2021, Czura et al. 2024).

Other potential challenges include issues of attrition, non-compliance, and missing data. As described in Section 3 ("Variations from the intended sample size"), various measures have been taken to guard against these issues, and in case they nevertheless arise, we have a clear roadmap in place how they will be dealt with in our analysis.

5. Interpreting Results

As fleshed out above, there is now a large literature that distinguishes between different channels and constraints to explain imperfect adoption of technology, but that so far has paid little attention to distinguishing between the roles that different types of information play in facilitating adoption decisions across different technologies, including with respect to information spillovers. Our study starts to address this gap by proposing a structural framework to differentiate between general and tailored information provision in the context of technology adoption, and generating empirical evidence to test the implications arising from the model. This will not only provide direct insights on the optimal design of information provision (e.g., in which situations extension programs should provide general or tailored agricultural information), but also help in evaluating the empirical plausibility of different models of learning discussed in the economic literature. For instance, if tailored extension was merely an increase in intensity of general extension, then both the direct and spillover effects of Intervention 2 should always (across both considered technologies) be larger than those of Intervention 1, which would violate several of our hypotheses. If, conversely, these hypotheses will be supported by the results of our experiment, then we will interpret this as being in line with the model's predictions regarding systematic differences between general and tailored information, thus demonstrating one (among possibly other) ways to rationalize empirical adoption decisions.

While our empirical application focuses on two specific innovations in the context of wheat farming in India, the insights derived from this analysis are more broadly applicable and have the potential to increase our understanding of the determinants of adoption decisions across different types of technology beyond the specific application considered here. In interpreting the results of our analysis, we will therefore focus not only on the immediate insights on the effectiveness of information treatments in the context of ESW and ZT in India, but also apply our findings to address more general questions discussed in the literature on agricultural innovations that help farmers mitigate the negative consequences of climate change, and in the literature on barriers to technology adoption more generally.

An ongoing discussion in this literature is concerned with the distinct roles of different information frictions and respective theories of technology adoption. One group comprises models in which learning is perceived as updating beliefs about fixed (or highly time persistent) target values based on information derived from observed outcomes (Besley and Case 1993, Foster and Rosenzweig 1995, Jovanovic and Nyarko 1996, Munshi 2004, Bandiera and Rasul 2006, Conley and Udry 2010, Hanna et al. 2014). The binding constraint in these models ultimately amounts to a lack of information about the systematic component θ, which can be addressed by generating more data (either via individual or social learning). In contrast, theories of technology adoption that are based on a costly attention channel focus on users' ability to reduce uncertainty about ε_i by being attentive to idiosyncratic factors. The binding constraint in these models is not lack of information, but users' limited capacity to process available information, as has been modelled, for instance, using the concept of rational inattention (Sims 2003, Naeher 2022, Maćkowiak et al. 2023). In the context of our experiment, costly attention models highlight the need for tailored information provision (Intervention 2), while significant effects of general information provision (Intervention 2) on adoption rates would pertain more to classic learning models. If we find empirical support for our different hypotheses regarding adoption of ESW and ZT, we will thus interpret this as being in line with the view that both channels, learning and costly attention, matter in explaining adoption decisions, and that the quantitative role of each channel varies across different types of technology.

Our study also has the potential to generate important implications for policymaking, including insights relevant to optimal program design, targeting, and cost-efficiency assessments. First, to the extent that we will find empirical support for the hypotheses arising from our theoretical framework, our study will indicate that different types of technology require different types of information treatments to facilitate adoption and profitable usage. Guided by the stylized concept of 'simple' and 'complex' technologies in our theoretical framework, including the respective scope for spillovers, this will include practical insights that can be used to classify innovations as belonging to a particular category, and thus inform the design of adequate information provision programs for promoting uptake of a given innovation.

For instance, it has been argued that one-off interventions providing advice along general 'best practices' are not sufficient to convince people to change their practices, and hence continuous forms of support tailored to each individual's needs are required to increase uptake and usage of new technologies. Conditional on the empirical verification, the insights from our study will suggest that while this may be true for some types of technology (i.e., 'complex' technologies like ESW for which profitable usage depends on knowledge about current idiosyncratic conditions), for other technologies one-time interventions can in fact be effective (i.e., when adoption decisions are mostly driven by a lack of general information). In this case, the cost-effectiveness of such interventions may be further increased by making use of information spillovers among beneficiaries.

Moreover, in the context of agricultural extension it has been argued that training peer farmers may be a more effective approach to increase technology adoption compared to traditional programs based on agricultural expert advice. Our study suggests that the effectiveness of peer farmers will systematically differ across different types of technology. Specifically, for innovations that fall into the 'simple' technology category, the advice that peer farmers can provide based on their own personal experience with a new technology may indeed benefit other farmers. For innovations that fall into the 'complex' technology category, however, relying on peer advice will not be sufficient and thus additional, individual-specific advice tailored to each farmer's own idiosyncratic conditions will be needed to facilitate adoption.

Overall, the insights generated by this project will help in understanding the distinct roles that different types of information play in affecting technology adoption decisions, including via their respective scope for information spillovers, and point towards ways in which the associated barriers can be addressed. These insights will benefit researchers, policymakers, and other stakeholders in making better decisions towards promoting uptake and efficient usage of technology.

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7. Appendices

Appendix A: Participant Information and Consent Form

Greetings! I have come as a member of a research project that studies wheat cultivation practices and adoption of agricultural technologies and their impacts in this region of India. The study is conducted by University of Göttingen (Germany) in collaboration with Charities Aid Foundation (CAF) India (Vasant Kunj, New Delhi). We would like to briefly introduce our work before asking you whether you are willing to participate. The information collected through this project is important for policymakers, researchers, and other stakeholders for making better decisions towards promoting agricultural technologies among farmers in this region.

If you participate in this study, we will ask you questions about your household and farm characteristics, agricultural practices, technology adoption decisions, and farming outcomes. Our team will interview the head of the household (who makes farming decisions most of the time) or another household member who is familiar with the household's agricultural decisions. It will take about 45 minutes of your time. We plan to revisit each household at the end of the Rabi season 2024/25 for a follow-up survey. You or other household members will neither be exposed to any risks by participating in this study, nor will be identified personally in any study report or publication. All personal information about your household will be kept strictly confidential.

If you agree to participate, there is a chance that you will be offered to receive a training intervention that provides information about sustainable agricultural practices with the potential to improve agricultural productivity. Specifically, some participants will be randomly assigned to receive an intervention, while others will be assigned to a control group that does not receive the intervention. Random assignment is a critical aspect of the study design to determine the impact of the intervention, and will be conducted in a fair and unbiased manner.

Participation in this study and any potential intervention is voluntary. You may choose not to participate, or you may withdraw your consent at any time without penalty and without providing any reason for your decision. You may ask any questions you have about the study. If you have questions later, they can be directed to Abhijeet Kumar (+91 7808664636) or Sujit Sahu (+91 9958599449).

I understand the study conditions and I agree to participate.

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Full name

____

Signature

____

Date

8. Administrative information

Funding: This work was supported by the M.S. Merian – R. Tagore International Centre of Advanced Studies ‘Metamorphoses of the Political: Comparative Perspectives on the Long Twentieth Century’ (ICAS:MP), an Indo-German research cooperation funded by the German Federal Ministry of Education and Research. All views expressed here are solely those of the authors.

Institutional Review Board (ethics approval): The study protocols for the cluster RCT have been reviewed and approved by the Ethics Committee of the University of Göttingen (date: 1 August 2023; approval number: none). The data processing protocol has been reviewed and approved by the Data Protection Officer at the University of Göttingen (date: 4 July 2023; approval number: none). Ethical approval for the preliminary field work has been received from the Ethics Committee of the University of Göttingen prior to the start of the field work (date: 8 September 2022; approval number: none).

Declaration of interest: None.

Acknowledgments: We thank the teams at Charities Aid Foundation India and Sir Syed Trust, specifically Nivedita Narain, Sujit Sahu, Chandan Sarma, and Asif Zaidi, as well as Abhijeet Kumar, for insightful discussions and precious help during different phases of the study preparation.