Digital Agricultural Technologies for Women Farmers in Malawi: How Do Delivery Channels, Platform Attributes, and Revealed Priorities Shape Learning and Decision-Making?

Pre-Analysis Plan

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Abstract

Gender gaps in agricultural productivity persist across Sub-Saharan Africa, driven in part by women farmers' constrained access to information, technologies, and markets, alongside time burdens and unequal decision-making power. This study evaluates whether short-run exposure to digital agricultural technologies (DATs) can improve women farmers' agricultural knowledge and decision-making in Malawi, and how preferences over DAT design features shape prospective uptake. Using village-based lab-in-the-field sessions implemented with women farmers recruited through Village Savings and Loan Associations in Mchinji (matrilineal) and Mzimba (patrilineal) districts, we combine three complementary experiments. First, we randomize participants to receive agricultural advisory content through alternative delivery channels (SMS/voice versus an app-based channel) or to a control condition. Second, in a subset of villages, we elicit stated preferences over DAT platform attributes using a discrete choice experiment administered at baseline and endline. Third, we measure economic priorities through an incentivized revealed choice task in which participants select between two goods of similar value (fertilizer versus sugar). Primary outcomes for the main randomized exposure experiment are standardized indices of soil and advisory knowledge and channel-specific usability/evaluation measures. Complementary outcomes include stated preferences and willingness to adopt DAT features elicited through the DCE and revealed investment-oriented choices measured through the fertilizer-versus-sugar task. The study contributes evidence on how delivery modalities and product design interact with gendered constraints and cultural context to shape women's engagement with digital agricultural services.

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1. Introduction

Agriculture remains central to livelihoods and national economies across Sub-Saharan Africa, and in Malawi it continues to employ most of the predominantly rural population. Within this sector, evidence consistently documents women's substantial involvement in both on-farm and off-farm activities (Singbo et al., 2020). Yet a large body of research also shows that women's agricultural productivity is systematically below that of men. For example, Singbo et al. (2020) estimate that female plot managers in Mali produce roughly 20% less than male plot managers, with differences in education, age, environmental conditions, and input use contributing to the gap. Similarly, in Nigeria, female-managed plots have been estimated to be almost 30% less productive than male-managed plots (Ojo and Baiyegunhi, 2023). Across contexts, a recurring explanation is that women farm under structurally different conditions: limited access to labor (including household male labor and hired labor), heavier unpaid care and domestic workloads, and constraints in accessing and using productivity-enhancing inputs.

Malawi reflects these broader regional patterns. Studies that decompose gender productivity gaps in Malawi repeatedly identify unequal access to resources and inputs as key drivers. Kilic et al. (2014) document gender differences in access to productive resources, while Nsanja et al. (2021) highlight gendered patterns of input use. Looking at policy interventions, Karamba and Winters (2015) show that Malawi's Farm Input Subsidy Program raises productivity for both men and women, but does not eliminate women's relative disadvantage - suggesting that constraints beyond subsidized inputs remain binding. Multi-country evidence also places Malawi among settings with larger gender gaps: Torkelsson and Onditi (2018) find women's plots are about 28% less productive than men's in Malawi (relative to smaller gaps in Tanzania and Uganda), and attribute much of the difference to land access and women's access to household male labour. Similar magnitudes are reported by Kilic et al. (2013), who estimate female-managed plots to be around 25% less productive than male-managed plots. More broadly, Rodgers and Akram-Lodhi (2019) argue that women's lower output is not due to lower efficiency, but rather to inequalities in access to inputs such as family labour, improved varieties, pesticides, and fertilizer.

A growing strand of this literature points to gender disparities in access to modern inputs and information services as especially important constraints. Makate and Mutenje (2021) find pronounced gender gaps in modern input use in Malawi and Tanzania, with disparities larger in Malawi, and emphasize the role of gender-sensitive advisory services; they further note marital status as an additional axis of vulnerability for women. Burke and Jayne (2021) similarly document that women are disproportionately likely to apply lower quality seed and less fertilizer on poorer soils than men, reinforcing productivity differences through both input quantity and input–soil matching. In related work, Tufa et al. (2022) show gender differences in technology adoption and estimate a crop productivity gap in the range of 14–23%, attributing much of this gap to structural factors that shape women's ability to adopt and benefit from technologies.

Technological advances can mitigate several of these constraints when they are accessible and usable for women. Achandi et al. (2018) argue that women's access to agricultural technologies can enable improved practices that raise productivity, while Thakur (2023) emphasizes that appropriate technologies can reduce drudgery, improve access to organized markets and cooperatives, and expand rural women's employment opportunities. However, the same literature highlights persistent barriers to women’s access and adoption. Thakur (2023), Rola-Rubzen (2020), and Nchanji et al. (2023) underline the roles of socio-cultural norms, constrained access to information, unequal control over resources, limited finance, and weak policy implementation. Decision-making power is repeatedly identified as a channel: Shahbaz et al. (2022) link women's technology adoption to household decision-making authority, while Mgalamadzi et al. (2024) document that women in many African contexts remain less involved in household decision-making, which can limit their ability to act on information or invest in technologies.

Within this broader technology landscape, digital agricultural technologies (DATs) - including SMS-, voice-, and app-based advisory services - are increasingly positioned as tools that can bridge information gaps and improve decision-making. Digitalization is often presented as a pathway to improve market access, facilitate access to financial services, and expand training opportunities for rural farmers (Yousif et al., 2025). At the same time, there is limited evidence on whether DATs reduce or reproduce gendered inequalities in practice. Hackfort (2021) argues that inequalities emerge in whom DATs are designed for, how benefits are distributed, and who has the digital literacy to use them. Others caution that where agricultural advisory systems are already gender-unresponsive, women may face additional challenges adapting to digital delivery, motivating a gender-responsive approach to digital innovation (Jarial and Sachan, 2021). Akello and Brunori (2025) similarly emphasize gaps related to gender-based technology design, socio-cultural norms restricting access and use, low digital confidence linked to low digital literacy, financial inclusion constraints, and “data disempowerment,” while also highlighting opportunities when tools are context-aware and co-created through participatory approaches.

These issues are closely connected to the rise of precision agriculture and climate-oriented advisory services. Precision farming is frequently described as improving yields and reducing vulnerability to climate and crop-failure risks (Chelliah and Raj, 2023), yet women often lag behind men in access, use, and benefits. Chelliah and Raj (2023) identify education, age, farm size, and farming experience as correlates of women’s precision agriculture adoption, and evidence from multiple contexts emphasizes illiteracy and limited literacy as major barriers (Afzal and Bell, 2023; Hamisu et al., 2024; Pandeya et al., 2025). This suggests that expanding digital and precision services among rural women requires careful tailoring of delivery modalities and platform attributes to women’s constraints and capabilities.

This study is designed to address these gaps by testing, in an experimental setting, whether short-run exposure to digital advisory channels improves women farmers’ learning and shapes their technology preferences and economic choices. Building on the view that women’s lower productivity reflects unequal operating conditions rather than lower efficiency (Quisumbing, 1994; Croppenstedt et al., 2013; Ragasa et al., 2013; Doss, 2018), the project empirically examines whether improving women’s access to timely, accurate agronomic information can strengthen knowledge and decision-making, consistent with arguments that productivity gaps narrow under more equal informational environments (Gallen, 2015; Palacios-López and López, 2015). It also engages capability-oriented perspectives on information as an input that can expand agency and choice (Mishra and Tripathi, 2011; Ortiz-Rodríguez and Pillai, 2017; Mbakaya, 2022; Garikipati, 2024), and it speaks to emerging debates on how digital and precision innovations can be made gender-responsive in contexts of climate risk (Rao et al., 2025; Raza et al., 2025; Ozor et al., 2025).

Empirically, this study combines three linked "lab-in-the-field" elements: (i) a randomized comparison of advisory delivery channels, (ii) a discrete choice experiment to elicit preferences over DAT platform attributes, and (iii) an incentivized revealed choice task capturing short-run economic priorities. The study is implemented in Malawi in settings that allow meaningful comparison across social and economic contexts, including differences in kinship systems and market access conditions. The evidence generated is intended to inform both scholarship and practice on how to design digital agricultural services that better match women farmers' constraints, strengthen their engagement with advisory information, and ultimately support more effective agricultural decision-making.

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2. Research Questions

There are four core research questions:

1) Does short-run exposure to digital agricultural advisory content improve women farmers' agricultural knowledge?

Specifically, participants are assigned to one of three arms:

i. An active control (wash video with no agricultural advisory content),
ii. SMS/Voice advisory, or
iii. App-based advisory

Compared to the control arm, we estimate the effects of each advisory delivery channel on soil and soil-management knowledge (immediate learning),

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2) Which delivery channel is more effective, and how do women experience each channel?

Comparing the two treatment arms (SMS/Voice vs App-based advisory), we examine differences in:

a) Comprehension of the advisory content received,
b) Usability of the delivery channel,
c) Self-efficacy: perceived ability to apply the information to improve farming outcomes,
d) Intention to act on the information and continue using the delivery channel.

3) What platform characteristics do women farmers value? Does their valuation change with experience?

Using the discrete choice experiment (DCE), we first ask how cost considerations, audio vs text format, contextualization, in-service support, and the provision of complementary market information drive women farmers' stated choices of DAT services. We then test whether the valuation for DATs change after exposure to specific advisory formats. E

4) Does DAT exposure shift how women farmers allocate priorities in an incentivized revealed choice?

In an incentivized revealed choice task, participants choose between two goods of equivalent monetary value: fertilizer (an agricultural investment good) and sugar (a consumption good).

Compared to the control arm, we estimate whether exposure to SMS/Voice or App-based advisory content increases the probability of selecting the investment-oriented option.

We also address two ancillary research questions:

5) Heterogeneity across social and economic context: Do treatment effects (RQ1 and 2), platform preferences (RQ3), and revealed choice behaviour (RQ4) differ systematically across
   a) matrilineal vs patrilineal settings (district)
   b) market proximity,
   c) household structure (female- vs male-headed households),
   d) baseline digital access and literacy (phone ownership and type),
   e) baseline empowerment (composite index of decision-making autonomy, income contribution, and land/crop control),
   f) baseline self-efficacy with technology (perceived ability to afford and use a phone), and
   g) baseline soil and soil-management knowledge (above vs below median)?

6) Baseline diagnostics for targeting and design: What are the baseline levels of (a) soil and soil-management knowledge, (b) current agricultural practices and DAT awareness/use, and (c) digital access and capability, including mobile phone ownership and access (personal vs shared), phone type (feature phone vs smartphone), network/charging constraints, literacy, and languages understood (for text and audio delivery)?

3. Research Strategy

3.1. Overview of experimental components

The research design combines three complementary lab-in-the-field components implemented during village sessions: (i) a randomized comparison of digital advisory delivery channels, (ii) a discrete choice experiment (DCE) to elicit stated preferences over digital agricultural technology (DAT) platform attributes, and (iii) an incentivized revealed choice task capturing economic priorities. These components are embedded within a baseline–exposure–endline session structure.

Baseline survey: All participants complete a baseline interview capturing household and individual characteristics, agricultural production and marketing activities, soil and soil-management knowledge, and prior exposure to and use of DATs. The baseline also records measures relevant to heterogeneous effects, including market proximity, household headship, phone access, and digital literacy. The baseline concludes with all participants receiving an identical introductory briefing about different modalities for receiving agricultural information.

Discrete Choice Experiment (DCE) sub-sample: In a subset of villages, all participants also complete a discrete choice experiment (DCE) eliciting stated preferences over digital agricultural technology (DAT) platform options. In each choice task, participants select their preferred option between two DAT services and an opt-out option, each described by a set of attributes. The DCE includes the following core attributes and levels: message format (text vs audio), in-service support (toll-free hotline vs designated person in the community), market links (rich market information vs none), contextualization (tailored to EPA vs generic), and cost (0, 500, 2,000, 5,000 Malawian Kwacha). The two DAT services are generically presented as "Phone service A" and "Phone service B", while the opt-out option is presented as listening to agricultural advisories on the radio, which is the common status quo in the study area. The levels for the opt-out (radio) option are fixed as audio messages, toll-free hotline, no rich market information, generic advisory and free of cost, reflecting the agricultural radio programmes currently available. The levels for the two DAT alternatives were assigned using a D-efficient design with 20 choice tasks in total, divided into five blocks of four choice tasks. The DCE is administered twice: each respondent completes one block of four choice tasks before the main RCT (advisory exposure), and one block of four choice tasks after, to assess whether the treatments shift the valuation for DATs and their characteristics. In each village session, the RCT treatment and control groups consist of five women farmers per group, so each of the five women in a treatment or control group is assigned to one of the five DCE blocks.

Randomized exposure to advisory delivery channels (RCT): Participants are then randomly assigned within the village session (five participants per arm) to one of three arms: (i) an active control with no DAT exposure, in which participants view a WASH video, (ii) exposure to advisory content delivered via SMS and voice, or (iii) exposure to the same advisory content delivered via the Zaulimi* smartphone application. Advisory content focuses on soils, soil types, and soil fertility management practices. The purpose of this experiment is to assess short-run learning and channel-specific usability and perceived relevance.

*Zaulimi is a digital agricultural advisory and market-linkage mobile application developed by the Agricultural Commodity Exchange for Africa (ACE) - a Malawi-based non-profit that facilitates structured agricultural markets (including price information and trading/market linkages) - in partnership with Welthungerhilfe (https://zaulimi.org/).

Endline survey (immediate post-exposure): Following the short-intervention, participants complete an endline module measuring knowledge of soils and soil management, perceptions of the advisory channel experienced (usefulness, ease of use, relevance), and related attitudes and intended practices. In DCE villages, participants also complete the DCE again at the endline.

Revealed choice experiment: At the conclusion of the session, participants complete an incentivized revealed choice task in which they choose between two items of similar size (fertilizer versus sugar). This task provides a behavioural measure of investment-oriented versus consumption-oriented priorities and is analysed in relation to treatment assignment and learning outcomes.

3.2. Sampling

Study participants

Participants in this study are 1,050 women farmers who are members of Village Savings and Loan Associations (VSLAs) working with NASFAM in two districts in Malawi: Mchinji and Mzimba. The sample is distributed equally across districts, with 525 women per district.

To be eligible for the study, participants must meet the following criteria:

- Live in a selected study village in Mchinji or Mzimba
- Be a woman farmer
- Be a member of a VSLA working with NASFAM

Two districts were selected to capture contrasting cultural contexts: Mchinji (matrilineal) and Mzimba (patrilineal). Within each district, five Extension Planning Areas (EPAs) were selected to reflect agro-ecological variation. From each EPA, seven villages were selected, yielding 35 villages per district and 70 villages in total. Village selection was stratified by proximity to agricultural markets, with approximately half of the villages close to a market (≤30 minutes travel time) and half far from a market (>60 minutes travel time).

In each selected village, 15 women were recruited from eligible VSLAs (70 villages × 15 women = 1,050) using stratified random sampling. Eligible women were first stratified by household headship, and then randomly selected within each stratum: 10 women from male-headed households (MHH) and 5 women from female-headed households (FHH), reflecting the lower prevalence of FHH in the population.

A subset of 28 villages was randomly selected for the DCE: 14 villages in Mchinji and 14 villages in Mzimba, with selection balanced by market proximity (half close and half far). DCE villages were selected using a computer-based randomization routine implemented in STATA, drawing within district × market-proximity strata. In DCE villages, all 15 participants complete the DCE, yielding a DCE sample of 420 women (210 per district).

3.3 Assignment to Treatment

This study uses random assignment to establish a causal relationship between exposure to different digital agricultural advisory delivery channels and women farmers' outcomes. Following the baseline survey modules and a common introductory briefing on different modalities for receiving agricultural information, participants are randomly assigned within each village session to one of three arms. The assignment procedure and session flow are summarized in Figure 1.

Unit and level of randomization

Randomization occurs at the individual level within villages. Each village session includes 15 women, who are randomly allocated in equal proportions to the three arms (five participants per arm). This within-village assignment ensures that treatment comparisons are not confounded by village-level characteristics, since all arms are represented in the same village session. Individual assignment is implemented in the field using a public lottery with five participants allocated to each arm.

Treatment arms

Participants are randomly assigned to one of the following groups:

1. Active Control (no DAT exposure): Participants do not receive digital advisory messages. Instead, they view a wash video during the exposure period.
2. Treatment Arm 1 (SMS/Voice): Participants receive agricultural advisory content delivered through SMS and voice.

3. Treatment Arm 2 (App-based advisory): Participants receive the same advisory content delivered through the Zaulimi smartphone application.

Across treatment arms, the advisory topic is held constant (soils, soil types, and soil fertility management practices). The primary contrast is therefore the delivery channel rather than message content.

Post-assignment measurement

Immediately after the exposure period, all participants complete an endline module measuring knowledge of soils and soil management and perceptions of the channel experienced (e.g., usefulness, ease of use, and relevance), as well as related attitudes and intended practices. In villages included in the DCE sub-sample, participants also complete the DCE again at the endline. The session concludes with an incentivized revealed choice task (fertilizer versus sugar). The various treatment arms and associated interventions are described in Figure 1.

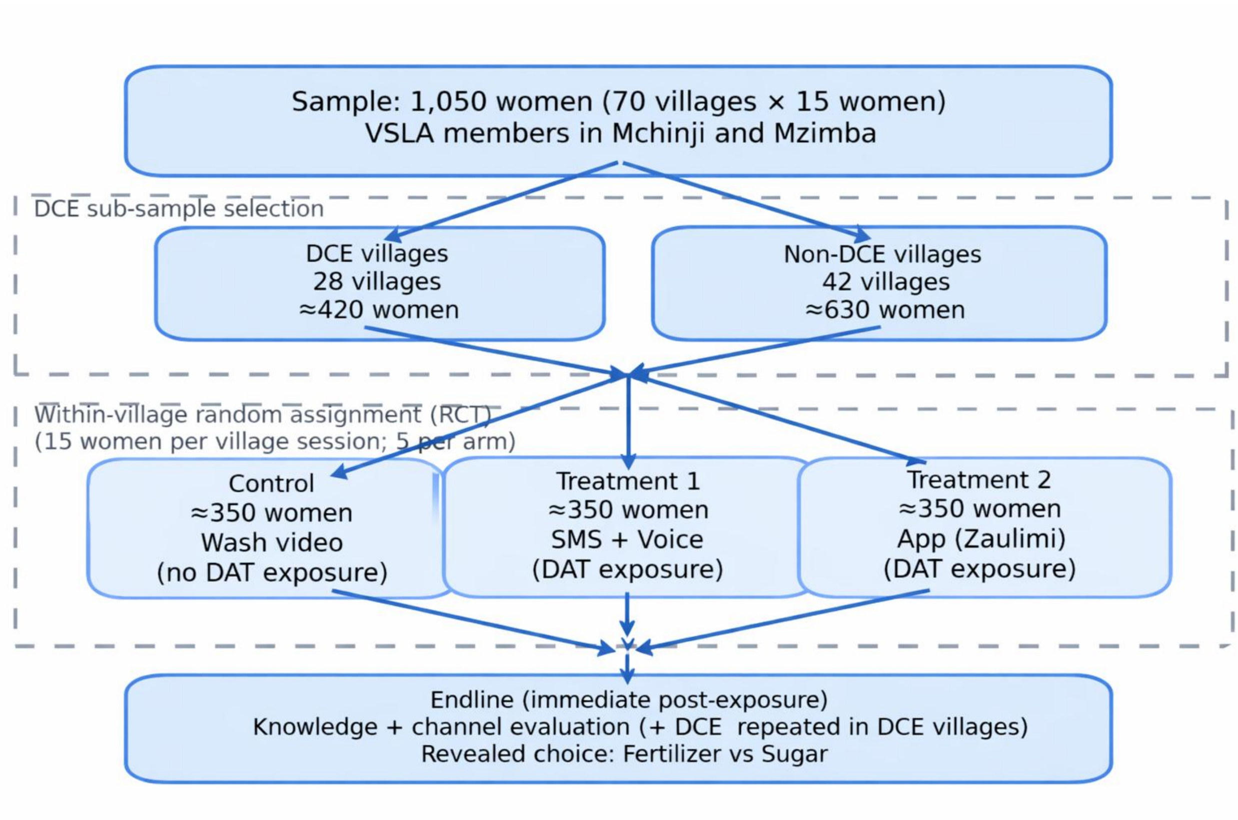

Figure 1. Assignment to treatment

3.4 Timelines of the experiment sessions

The lab-in-the-field session is implemented in each selected village with 15 women participants (10 from male-headed households and 5 from female-headed households). To minimize waiting time, 15 enumerators/research assistants conduct interviews in parallel, with each participant paired one-on-one with an enumerator for the survey modules.

The session proceeds as follows:

1. Arrival and group briefing (10–20 minutes). Participants are assembled and provided with an overview of the session procedures. All participants then receive an identical introductory briefing on different modalities for receiving agricultural information.
2. Baseline survey (≈20 minutes). Each participant completes an individual baseline questionnaire covering: (i) participant and household characteristics; (ii) agricultural production and marketing activities (land, crops, livestock); (iii) household dynamics, including decision-making and division of labour; and (iv) prior exposure to and use of digital agricultural technologies (DATs). In villages included in the DCE sub-sample, the baseline survey also includes the baseline DCE eliciting preferences over DAT platform attributes.
3. Randomized exposure period (≈30 minutes). Following baseline, participants are randomly assigned within the village session to one of three arms (five participants per arm):
   - Active Control: no DAT exposure; participants view a wash video;
   - Treatment 1: advisory content delivered via SMS and voice;
   - Treatment 2: the same advisory content delivered via the Zaulimi app. Advisory content focuses on soils, soil types, and soil fertility management practices.
4. Endline survey (≈20 minutes). Immediately after the exposure period, each participant completes an individual endline module measuring knowledge of soils and soil management and perceptions of the channel experienced (e.g., usefulness, ease of use, relevance), along with related attitudes and intended practices. In DCE villages, participants also complete the endline DCE.
5. Revealed choice task (≈5 minutes). The session concludes with an incentivized revealed choice task in which participants choose between two items of similar monetary value (fertilizer versus sugar).

Figure 2 presents the timeline of the session.

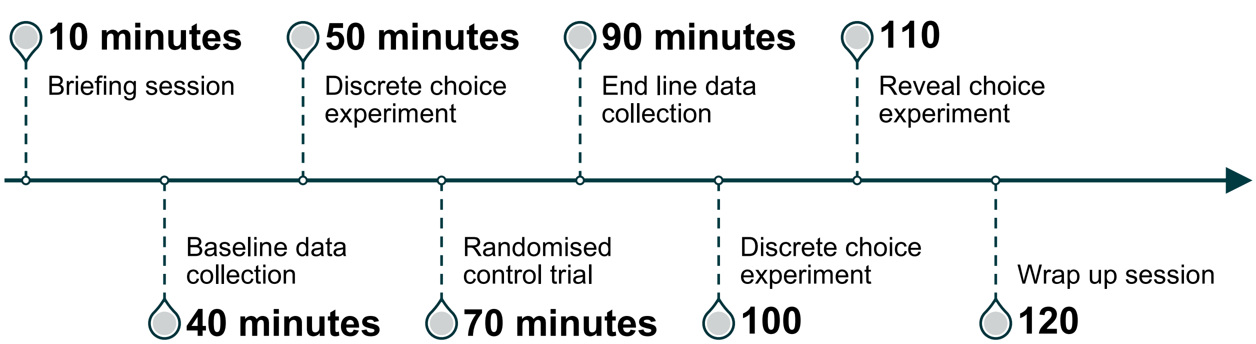

Figure 2. Timeline of the experiments

3.5. Study hypotheses

We expect to test the following hypotheses in the study:

1. **Knowledge gains (H1):** Exposure to DAT advisories increases women's knowledge of soils and soil management relative to the control condition. *(SMS/Voice vs Control; App vs Control; Endline knowledge index)*
2. **Channel effectiveness (H2):** App-based advisory exposure produces larger knowledge gains than SMS/Voice exposure. *(App vs SMS/Voice; Endline knowledge index)*
3. **Usability and perceived value (H3):** App-based delivery is rated as more useful and easier to use than SMS/Voice delivery. *(App vs SMS/Voice; Endline platform evaluation measures)*
4. **Attitudes and intended use (H4):** Exposure to DAT advisories increases positive attitudes toward DATs and intended future use relative to control, with larger improvements under the App arm. *(SMS/Voice vs Control; App vs Control; App vs SMS/Voice; Endline attitudes/intended use measures)*
5. **Revealed priorities (H5):** DAT exposure increases the probability of choosing fertilizer (vs sugar) relative to control, with the largest increase under the App arm. *(SMS/Voice vs Control; App vs Control; App vs SMS/Voice; Revealed choice outcome)*

For the DCE sub-sample:

6. **Cost sensitivity (H6):** higher cost reduces the probability that a DAT option is chosen. *(cost coefficient < 0)*
7. **Format (H7)**: audio format increases the probability that a DAT option is chosen compared to written format. (*audio format coefficient > 0*)
8. **In-service support (H8)**: community-based in-person support increases the probability that a DAT option is chosen compared to the hotline. (*in-person support coefficient > 0*)
9. **Contextualization (H9):** tailored/localized advice increases the probability that a DAT option is chosen compared to generic advice. *(contextualization coefficient > 0)*
10. **Market links (H10)**: a service that includes rich market information increases the probability that a DAT option is chosen compared to a service that does not include this information. *(market information coefficient > 0)*
11. **Phone-based services vs radio (H11)**: after controlling for all the characteristics included in the DCE, we hypothesize that phone-based services are preferred over the opt-out radio option, as they include additional benefits not explicitly captured by the characteristics listed in the DCE, such as the ability to access the advisory at any time, rather than the scheduled time of the radio programme. (*phone-based services coefficient > 0)*
12. **Effect of DAT exposure on DCE valuation (H12)**: we hypothesize that women exposed to DAT advisories are more likely to choose the phone-based services rather than the opt-out radio option, with a stronger effect for the App treatment. We also hypothesize that women exposed to DAT advisories may change their valuation for in-person vs hotline support and for the audio vs written format.

3.6 Attrition from the sample

The study is implemented as a single lab-in-the-field session per village and does not involve follow-up surveys outside the session. Therefore, we do not expect significant attrition after recruitment. However, some **within-session non-completion** may occur if participants choose to leave before finishing all session components. Participants who do not complete the full session will receive the show-up fee (and any compensation specified in the study procedures) but will not complete the remaining modules.

We will (i) record completion status for each participant, (ii) report attrition/non-completion rates by treatment arm, and (iii) test whether non-completion differs across arms at the 5% significance level. If differential attrition is detected, we will report robustness checks using standard bounding approaches (e.g., Lee bounds) where applicable.

3.7. Variables

We rely on (i) participants' responses in the baseline and endline surveys, (ii) randomized exposure to advisory delivery channels during the RCT, and (iii) stated and revealed choices in the DCE and revealed-choice task to measure the impact of DAT exposure on women's agricultural knowledge, channel experience, technology preferences, and investment-oriented decision-making outcomes. To make clear which outcomes correspond to which component, we group outcomes by experimental component (RCT, DCE, revealed choice). Primary outcomes are defined in Table 1 and secondary outcomes in Table 2. Baseline covariates used for balance checks and to increase precision are listed in Table 3.

Estimation: For outcomes measured at both baseline and endline (e.g., soil knowledge), we estimate treatment effects using ANCOVA (endline outcome controlling for the baseline value of the same outcome). For outcomes measured only at endline (e.g., channel experience indices), we estimate treatment effects using OLS including pre-specified baseline covariates and fixed effects for stratification variables (district, market proximity, household headship, and EPA where applicable).

Table 1. Primary outcomes

Variable	Definition
Treatment vs Control (RCT)	
Knowledge Index – Soils	Standardized index of baseline and endline responses on soil types, soil fertility indicators, and soil management practices: ● What soil type is recommended for growing maize? ● When is it recommended to apply manure in the maize field? ● How much manure is recommended? ● How many times is it recommended to apply fertiliser in a maize field?

	● What type of fertiliser is recommended? ● When do you apply basal fertiliser? ● When do you apply top dressing? ● When do you apply a combined fertiliser? ● How much fertiliser is recommended per acre? ● Is it recommended that basal dressing and top dressing can be combined?.
Revealed choice: fertilizer selected	Binary (0/1) – 1 if participant chose fertilizer over sugar in the revealed choice task, 0 otherwise.
Only within treatment arm comparisons (T1 vs T2) - (RCT)	
Comprehension Index:	Standardized index of participant's understanding and ability to recall the agricultural advisory information received during the session. ● The language used was clear ● I could easily understand the information provided ● I needed some help to understand the information provided ● I can recall the information I received without checking again ● In my opinion, the information I received is reliable and accurate
Usability Index:	Standardized index on whether the delivery channel was accessible, practical, and effective as a means of receiving information. ● Navigating the technology was easy for me ● It is faster to receive advice in this way than one-to-one in person ● Information received in this way is more understandable than traditional one-to-one advice ● The information I received is relevant to my individual situation ● The information received is affordable for me to use
Self-efficacy	Standardized index of three items on whether the participant feels able to apply what she learned ● The information I received will help me make better decisions ● I feel confident about using or practising the information received

	● I expect the information to help me achieve better outcomes
Intention to Act Index	Standardised Index on whether the participant intends to use the information received ● I intend to use the information I received either now or in future ● I plan to make at least one change to my practices based on the information received ● I intend to seek this type of information in the future even without external encouragement ● I intend to continue accessing this way of receiving information in the future even without external encouragement
Intention to Act	How many changes are you likely to make?
DCE - only in DCE Village	
DCE choice outcome (implied preferences for DAT's design features)	Chosen alternative in each DCE choice task, given the attribute levels of each alternative as shown in the choice task. DCE answers are analysed using a random parameter logit (mixed logit) model to analyse how characteristics of each alternative are traded-off with each other and derive the implied preferences and willingness to pay for each attribute.

Table 2. Secondary outcomes

Secondary outcome	Experimental component	Measurement	Construction	Main comparisons
Dissemination & spillover index	RCT (spillovers)	Endline only (post-exposure)	Standardized index of items capturing confidence to explain the information and intention to share within household and community.	T2 vs T1 (primary); descriptive vs Control
Knowledge of written-only (SMS-only) content	RCT (mechanism for modality)	Endline only (SMS/Voice arm only)	Binary indicator for answering correctly a knowledge question whose content appears only in the SMS message (not in the audio).	Within-arm descriptive; heterogeneity by literacy/phone type

DCE attribute attendance	DCE (process measure)	Baseline & Endline (DCE villages only)	0/1 indicators for whether each attribute is reported as driving choices in the post-DCE question (multiple selections allowed).	T2 vs T1 vs Control; heterogeneity by treatment/baseline traits
Preferred DAT design (post-DCE)	DCE (design preferences)	Endline only (DCE villages only)	Responses to 'build your preferred service' questions (device, format, precision, in-service support, language, additional information).	T2 vs T1 vs Control; heterogeneity by baseline phone access/literacy
Knowledge Index – DATs	Baseline diagnostics / complementary outcome	Baseline (and endline if repeated)	Standardized index of items on awareness/understanding of DATs and current sources/types of agricultural information received via DATs (where measured).	Descriptive at baseline; exploratory treatment effects if repeated
DAT utilization	Baseline diagnostics / complementary outcome	Baseline (and endline if repeated)	Binary indicator: reports receiving agricultural information through any DAT modality within the questionnaire reference period.	Descriptive at baseline; exploratory treatment effects if repeated

Table 3. Control variables (baseline)

Baseline control	Definition	Notes / rationale
District	1 = Mzimba (patrilineal), 0 = Mchinji (matrilineal)	Stratification / heterogeneity
EPA	Categorical indicator for Extension Planning Area	Context controls; can be fixed effects
Market proximity	1 = ≤30 minutes to market; 0 = >60 minutes	Stratification / heterogeneity
Household headship	1 = female-headed household; 0 = male-headed	Stratification / heterogeneity
Age	Participant age (years)	Precision

Education level	No formal / Primary / Secondary / Tertiary	Digital access and learning
Literacy	Binary/categorical measure of reading ability in relevant language(s)	Key for written vs audio mechanisms
Languages understood	Indicators for language(s) understood (e.g., Chichewa, Tumbuka, English)	Targeting and modality fit
Household size	Number of household members	Resources and constraints
Land size	Cultivated land (acres)	Farming scale
Mobile phone ownership	1 = owns a mobile phone; 0 = otherwise	Access constraint
Mobile phone type	No phone / Feature phone / Smartphone	Access constraint and DCE relevance
Phone access (shared)	Indicator for access to a phone even if not owned (if measured)	Access constraint
Prior DAT exposure	1 = ever used any DAT before baseline; 0 = otherwise	Learning heterogeneity
Baseline empowerment index	Standardized index of decision-making autonomy and control over resources (as defined in questionnaire)	Moderation / heterogeneity
Baseline technology self-efficacy	Perceived ability to afford/charge phone and buy airtime; perceived ease of use	Moderation / targeting

3.8 Balancing Checks

We will assess baseline balance across treatment groups using a joint test of orthogonality. First, we estimate the following regression:

$$Treat_i = \alpha + \sum_k \beta_k X_{ik} + u_i$$

where $Treat_i$is an indicator for treatment assignment (control, SMS/Voice, or App), $\alpha$is a constant term, and $X_{ik}$are baseline participant and household characteristics (e.g., age, education, household size, land size, phone ownership/type, prior DAT exposure, district, market proximity, and household headship).

We then test the joint hypothesis that the coefficients on baseline characteristics are jointly zero using an F-test. Because treatment assignment has three categories, we will also estimate a multinomial logit specification with treatment status as the dependent variable and test for joint orthogonality of coefficients across treatment arms.

In addition, we will report mean differences in baseline characteristics across arms and standardized differences. In the event of within-session non-completion (attrition), we will test whether attrition rates differ across treatment arms at $\alpha = 0.05$. If we detect differential attrition, we will implement bounding approaches (e.g., Lee bounds; Lee, 2009) as a robustness check.

3.9 Statistical Power

A total of 1,050 women, with 350 participants per arm (Control, Treatment 1: SMS/Voice, Treatment 2: App), is sufficient to detect a minimum detectable effect size (MDES) of 0.150 when comparing any single treatment arm to the control group at 80% power and α = 0.05.
For the DCE sub-sample, 28 villages with 15 women per village (approximately 420 women total) are sufficient to detect an MDES of 0.137 for changes in overall platform preference between baseline and endline.

For heterogeneous treatment effect analyses by key subgroups (e.g., district, market proximity, household headship), with approximately 175 women per subgroup per arm, we can detect an MDES of 0.212.

Experiment arm	**Sample size per arm**	**Power**	**Alpha**	**MDES**
Treatment 1 (SMS/Voice) vs Control	350	0.80	0.05	0.150
Treatment 2 (App) vs Control	350	0.80	0.05	0.150
Treatment 1 (SMS/Voice) vs Treatment 2 (App)	350	0.80	0.05	0.150
DCE overall (baseline to endline; DCE villages)	420 (total)	0.80	0.05	0.137
Subgroup analysis (e.g., Mchinji vs Mzimba)	175	0.80	0.05	0.212

Calculations of minimum sample size (*N*) for DCE are based on the following parameters and formula:

K = number of parameters to be estimated in the DCE = 8 if considering price as a categorical variable (or 6 if considering price as a continuous variable)

S = number of choice tasks per respondent = 4 at baseline and 4 at endline (or 8 if considering all the choice tasks each respondent is exposed too, across the two waves)

J = number of alternatives per choice task (excluding opt-out) = 2

Lmax = the largest number of levels for any attribute = 4 for the price attribute, which is modelled as a continuous variable in the main specification; otherwise 2.

Orme (1998) rule of thumb:

$N \geq 500 \times Lmax / (J \times S) = 500 \times 4 / (2 \times 4) = 250$ (or 125 if considering all 8 choice tasks together)

Assele et al. (2023):

$N \geq 150 \times K / S = 150 \times 8 / 4 = 300$ (or 150 if considering all 8 choice tasks together).

In the main analysis, the monetary category (price) will be modelled as a continuous variable, further decreasing the minimum sample size compared to the calculations shown above with conservative assumptions.

4. Empirical analysis

4.1 Treatment effects

To estimate the causal effect of treatments in this study, we perform the following empirical specification for participant i:

$$\mathbf{Y_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \theta Y_i(baseline) + \gamma X_i + \delta_e EPA_e + \varepsilon_i}$$

where we define Y_i to be the outcome variable as described in Table 1 for participant i measured at endline, T1 and T2 are binary indicators for Treatment Arm 1 (SMS/Voice) and Treatment Arm 2 (App) respectively, Y_i(baseline) is the baseline value of the outcome variable, X_i is the vector of household and individual level controls, and EPA_e represents Extension Planning Area fixed effects.

β_1 estimates the treatment effect of SMS/Voice platform exposure on women's knowledge, practices, and technology preferences relative to the control group. β_2 estimates the treatment effect of App platform exposure relative to the control group. The difference $\beta_1 - \beta_2$ (tested with an F-test) estimates the differential effectiveness of SMS/Voice versus App platforms.

For outcomes only measured at endline (such as DAT platform evaluations), we estimate the same specification without the baseline control term θY_i(baseline).

4.2 Heterogeneous effects

We will study the following dimensions of heterogeneity to understand how treatment effects vary across different subgroups of women farmers. These heterogeneity analyses are particularly important given the diversity of contexts in our sample, including variations in cultural norms (matrilineal vs patrilineal), market access, household structure, and baseline technology exposure. The specific dimensions of heterogeneity we will examine are defined in Table 4 below.

To test whether treatment effects vary heterogeneously across groups with specific individual-level and contextual characteristics, we will re-run the empirical specification, interacting the treatment with the variable of interest for heterogeneity.

Heterogeneity Specification:

$$\boldsymbol{Y_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 H_i + \beta_4 (T1_i \times H_i) + \beta_5 (T2_i \times H_i) + \gamma X_i + \delta EPA_i + \varepsilon_i}$$

where H_i represents heterogeneity dimensions. The coefficients β_4 and β_5 estimate whether treatment effects differ across subgroups defined by H_i.

Table 4

Heterogeneous treatment effects		
Variable	**Definition**	**Rationale**
Cultural context	District (Mchinji=matrilineal vs Mzimba=patrilineal)	Women in matrilineal societies may have different decision-making autonomy affecting technology adoption
Market access	Village proximity to market (close vs far)	Market access may moderate the value of market linkage information
Household structure	Female-headed vs male-headed household	FHH may have different resource constraints and decision-making patterns
Baseline digital literacy	Mobile phone ownership and type (none, feature, smart)	Prior technology exposure may affect the ability to utilize DAT platforms
Baseline knowledge	Soil knowledge index at baseline (above vs below median)	Those with lower baseline knowledge may benefit more from advisories
Age	Above vs below median age	Younger women may be more comfortable with digital technologies
Literacy	Can you read? (Yes v No)	Women who can read may be better able to absorb the information as they can read the text presented to them in the treatment, in addition to listening to the audio messages.

Education	Primary or less vs Secondary or more	Education may affect the ability to understand and apply technical information
Land size	Above vs below median cultivated area	Larger farmers may have more to gain from precision agriculture
Decision-Making Autonomy Index	Standardized index of participation in household agricultural decisions	Women with greater autonomy may be better positioned to implement recommendations

4.3 Standard error adjustments

Randomization is done at the household level for the sample. We will use robust standard errors to correct for heteroskedasticity for all specifications.

We will perform multiple hypothesis test adjustments using False Discovery Rate (FDR) correction. Following Benjamini et al. (2006) and Anderson (2008), we will report sharpened q values over the set of p values associated with each hypothesis. Thus, in our analysis, we will report p values and sharpened q values.

4.4 Analysis of the Discrete Choice Experiment

To estimate preferences for specific characteristics of DATs, we analyse the responses to the choice tasks in the DCE using a random parameter logit (also known as mixed logit) model. Coefficients will be estimated in willingness to pay (WTP) space so to be directly interpretable as WTP for the presence or absence of each characteristic (Hole and Kolstad, 2012; Scarpa et al., 2008). If convergence is not achieved in the estimation process, then coefficients will be estimated in preference space and willingness to pay calculated subsequently. Further robustness checks may be conducted using the conditional logit model, which relies on more restrictive assumptions but is less computationally intensive.

To estimate the impact of the exposure to DAT advisories (treatment arms), interaction terms will be included between the treatment status and the coefficients we hypothesize to have been affected. We will test in particular whether the treatments increase the probability / WTP for the phone-based service as opposed to the opt-out option (radio programme), and whether the treatments change the probability / WTP for the in-person support as opposed to the hotline and for the audio message as opposed to written message. Further interactions may be tested as exploratory analysis, as the channel through which the treatments may affect the role of other attributes is less clear.

As an additional analysis to understand the effect of the treatment, the main model specification will be estimated on the endline data separately for the control group and each treatment group.

To understand heterogeneity based on observable characteristics (as listed in Table 4), specifications with interaction terms and separate specifications by sub-group will be used.

5. Fieldwork

5.1 Data Collection

Prior to the experiment, we will organize a two-day training of 15 enumerators, which will be followed by a one-day pilot to ensure that the modules and the experimental protocol run smoothly. After the pilot, the team will evaluate the exercise and adjust based on the issues arising from the pilot. The pilot will be done in Lilongwe district, and NASFAM will facilitate the identification of the village for the pilot. The data from the pilot will not be used in the main analysis.

We expect the data collection process to take 8-9 weeks, including the pilot. Enumerators will be trained for two days before the pilot, then the pilot will take one day, and then one day after the pilot will be reserved for reflection and adjustments. We are expected to spend about 120 minutes (2 hours) in one village, as one enumerator will be assigned to one respondent at the village level. Thus, we expect to cover 2 to 3 villages per day. Since we have 70 villages, we expect to spend about 15 days in each district, making a total of 30 days of actual data collection, plus the 4 days of the training and pilot, we expect to spend a total of 34 active days in the field. These will be spread across 8-9 weeks from 12th January to 13th March. The weekends are reserved for rest. We will also have a period of rest between the two districts as travel between villages is difficult, especially in wet weather. We have another 2 weeks until March 13th, in case we are unable to visit some villages due to weather-related or other reasons. So, the fieldwork activities are expected to run from January 12th 2026, to 13th March 2026.

Data from the experiment will be sent directly from enumerators' tablets to the research teams' computers through an online server. We will use Survey Solutions for data collection. Data protection procedures have been approved by the UCD ethics committee (Ref: HS-CO-25-033-Garikipati). Ethical approval from Malawi's National Committee on Research in the Social Sciences and Humanities (NCRSH) was obtained before fieldwork commencement (Ref:NCST/RTT/2/6).

5.2 Data Processing

Data processing will involve cleaning the data, managing the data, and analyzing the data. Data will be reviewed after lab-in-the-field sessions each day to check for errors, if any. We anticipate data processing to start after all the lab sessions are done and the data is transferred to the research team anonymously. The research team will share ownership of the processed data, which will be used and stored in the cloud and on local computers. Data processing is expected to finish within 8 to 10 weeks, starting 23rd March 2026.

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