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Information delivery channels and agricultural technology uptake: experimental evidence from Ghana

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Abstract

Purpose: Adequate information is necessary for investments. We use data from a randomized controlled experiment in northern Ghana to advance knowledge on which communication options are most effective for reaching farmers with a new technology (*Bradyrhizobium* inoculation) to boost adoption. Farmers received information through either video documentaries or radio listening clubs. Joint test of all treatment effects provide strong evidence that the video was effective for inducing technology uptake and increased yields; the radio listening club effects were mostly imprecise, partly due to insufficient statistical power. We conclude that barriers to learning about correct technology usage or benefits constrain adoption.

Key words: Information, Agricultural technology, Randomised controlled experiment, Adoption, Ghana

1. Introduction

The availability and adoption of modern agricultural technologies are necessary for agricultural productivity growth and economic transformation when a country has a large share of their population employed in agriculture (World Bank, 2007; Foster and Rosenzweig, 2010; Webb and Block, 2012). Yet, the adoption of productivity-enhancing technologies by smallholder farmers is low in sub-Saharan Africa (SSA), leading to low yields and thus constraining the poverty-reducing potential of smallholder farming. For example, Ghana's average inorganic fertiliser consumption is only 15 per cent and 6 per cent of the world's and East Asian averages, respectively (World Bank, 2019). While the low adoption of modern technologies may seem puzzling, a number of factors explain this, including low marginal returns in the presence of poor infrastructure (Michler *et al.*, 2018; Suri, 2011), liquidity and risk constraints (Karlan *et al.*, 2014), unavailability of complementary inputs

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(Brush, Taylor and Bellon, 1992; Feder, Just and Zilberman, 1985) and high market transaction cost (Aker, 2011; Feder, Just and Zilberman, 1985).

Aside the above constraints, lack of information about a technology could also be a binding constraint because information and knowledge about a new technology are necessary for adoption, even if not sufficient (De Janvry, Macours and Sadoulet, 2017; Feder and O'Mara, 1982; Feder and Slade, 1984; Matuschke, Mishra and Qaim, 2007; Rogers, 2003). In fact, evidence from expert opinions across 96 countries suggests that, for some technologies such as integrated pest management, information alone (if properly delivered) could be sufficient for boosting adoption in some contexts (Parsa *et al.*, 2014). The lack of information is particularly a binding constraint to adoption when the technology in question is new (at least to the potential adopter) and thus relatively unknown in the population of interest and could thus lead to underinvestment, even if the technology is profitable (Foster and Rosenzweig, 2010; Klotz, Saha and Butler, 1995).

The choice of information communication channel also matters because the extent to which information about a new technology engenders learning and precipitates investment depends on the complexity of the new technology in question, yet not all channels can convey complex technology information adequately to arouse interest and optimal use (Foster and Rosenzweig, 1996, 2010). Therefore, the central question of this article is as follows: which channel of agricultural technology information communication is most effective for increasing awareness and inducing the adoption of new technologies, and does the complexity of the technology matter? Here, we evaluate the relative impact of two alternative channels—video documentary (VDD) and radio listening club (RLC)—of disseminating information and knowledge about a new technology (*Rhizobium* inoculation) to farmers in northern Ghana. We accomplish this by setting up a field experiment involving 1,126 smallholder farm households randomly categorised into three experimental arms (two treatments and one control) in 113 communities.

The literature on the role of Information and Communications Technologies (ICTs) as a tool for enhancing agricultural processes and outcomes has grown rapidly over the past decade. For example, there is evidence that mobile phones and mobile phone applications (text messages and interactive game apps) increase technology awareness and uptake of extension recommendations (Casaburi *et al.*, 2019; Fu and Akter, 2016; Larochelle *et al.*, 2019), even after addressing issues of bias due to self-reporting and social desirability (Fabregas *et al.*, 2019; Tjernström *et al.*, 2020). Using a randomised controlled experimental design, Arouna *et al.* (2020) found that personalised extension advice to farmers via a mobile application increased yield and profit in Nigeria, even without an increase in fertiliser application rates. Adding on a fertiliser subsidy had larger impact on measured outcomes, suggesting binding liquidity constraints. A synthesis of the current state of knowledge on the impact of digital extension delivery approaches on farmer behaviour and agricultural outcomes is provided by Fabregas, Kremer and Schilbach (2019). A

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major attraction of these ICT-based approaches to agricultural extension delivery is their cost-effectiveness relative to the traditional methods that require in-person visits or face-to-face training (Fabregas, Kremer and Schilbach, 2019).

The growing literature notwithstanding, knowledge about the relative effectiveness of alternative digital and ICT-based approaches is still limited. Is short message service (SMS) better than voice messaging? Is video better than radio? Do we need a combination of ICT-based information delivery approaches? Should ICT-based approaches be combined with in-person visits? A few studies have answered some of these questions. Using experimental data from India, Cole and Fernando (2020) evaluated the effect of a toll-free hotline service among smallholder farmers, showing that the stand-alone ICT-based approach in combination with an annual in-person visit had significant impact on farming practices. However, they did not report the relative effectiveness of the ICT-based approach versus the combination with the traditional in-person visit. The experimental set-up of Fabregas *et al.* (2019) allowed a comparison of the relative effectiveness of SMS alone versus SMS in combination with phone calls from an extension agent to explain the SMS content. They found no additional impact of the calls on farmer behaviour in Kenya. To the best of our knowledge, the most comprehensive evaluation of the relative effectiveness of ICT-based agricultural interventions to date is provided by Van Campenhout, Spielman and Lecoutere (2020). Their experimental design includes short video messages, video plus an interactive voice response (IVR) service and an incremental effect of SMS reminders. They show that the video messages had significant impact on the use of recommended practices and yields, but the IVR and SMS reminders had no additional impact.

We contribute to the literature that uses field experiments to study how ICT-related agricultural advisory services could enhance the uptake of technologies and improve agricultural outcomes. We thus contribute to filling the gap in knowledge on the relative effectiveness of alternative ICT-based approaches. The first intervention uses VDD to communicate information about a new technology (legume inoculants) and another technology that is less novel (improved legume seeds). The second intervention uses interactive radio in the form of an RLC to communicate the same information. All study participants, including the control group, were exposed to mass media campaigns about the new technology via district and community radio programmes.

There is particularly a paucity of knowledge about the impact of RLCs. Radio (particularly community radio) has been used as a mass media communication channel for disseminating agricultural technology information for many decades. Yet, surprisingly, little is known about its impact on farmer knowledge and practices. But radio alone may not provide the kind of interactive means of communication required for maximising learning and adoption because it is potentially top-down in nature, albeit mobile phone penetration has greatly reduced this potential drawback (Gilberds and Myers, 2012). An RLC or a community listeners' club is one way of making radio more interactive in extension delivery (Mchakulu, 2007). Observational data suggest

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that participatory radio campaigns influence farmer knowledge and behaviour, including the adoption of agricultural technologies in Ghana, Malawi, Mali, Tanzania and Uganda (Perkins, Ward and Leclair, 2011).

Is video better than audio for increasing knowledge and for uptake of recommended practices? Recent field experiments (Van Campenhout, Spielman and Lecoutere, 2020; Van Campenhout *et al.*, 2017) show that the answer to this question is in the affirmative. The relative effectiveness of video over audio could be because the multisensory input inherent in video (audio and visual communication) improves memory and recall (Thelen, Cappe and Murray, 2012). The bimodality advantage notwithstanding, video could be a passive medium due to the lack of interactivity, such as the opportunity to ask questions. Nonetheless, because radio is largely a unimodal method of learning, one could expect the VDD mode of agricultural technology dissemination to have greater impact than radio in general. However, an RLC has the potential advantage of being interactive—being a two-way channel of communication—than the VDD. Therefore, while one would expect video to be more effective than radio in general, it is not a priori clear that the VDD channel will have greater impact on knowledge and technology uptake than the RLC.

Months after the start of the VDD and RLC interventions, we assessed impact on four outcomes: inoculant technology awareness, inoculant uptake, the use of improved legume seeds and legume yields. We found that awareness of the new technology increased dramatically, from 3 per cent of the sample at baseline to about 84 per cent at follow-up 14 months hence. Yet, the level of awareness between the control group and the treatment groups was not different from zero, suggesting that the mass media awareness campaigns performed equally well at raising awareness. However, while the VDD treatment effects were different from zero for all other outcomes, the evidence is not overwhelming that the RLC treatment effects are significantly different from zero, except, perhaps, for the use of improved legume seeds. Some of the null results regarding the RLC effect boils down to the lack of statistical power rather than the lack of effect, however. Some of our null findings may be due to the lack of statistical power rather than the absence of a significant effect. Therefore, to distinguish between 'true null results' and those that are driven by inadequate statistical power, we provide ex post minimum detectable effect (MDE) size results in Table A1 (Appendix in supplementary data at ERAE online).[1]

The rest of the article is structured as follows. Section 2 describes the intervention, research design, data collection (including measurement of variables) and baseline balance tests. Section 3 presents the estimation methods; Section 4 contains the results and discussions. In Section 5, we provide further analysis that focuses mainly on the robustness of our results, cost-effectiveness, limitations of our study and suggestions for future research. The summary of findings and conclusion is presented in Section 6.

1 The MDE we calculate is simply the ex post minimum size of the impact that we could have detected with 80 per cent statistical power at the 0.05 alpha level.

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2. The intervention, experimental design and data

2.1. Context and the intervention

Legumes are important for food security and income in northern Ghana (Fening and Danso, 2002). However, yields are low because the crops need high amounts of nitrogen, which most smallholder farmers in northern Ghana cannot afford (Ulzen *et al.*, 2016). Like most farmers in SSA, legume farmers either do not use nitrogen fertilisers or rarely apply the required amounts. Fortunately, legumes can form a symbiotic relationship with rhizobia (a type of bacteria) that allow atmospheric nitrogen to become available in the soil for their use and that of intercrops or crops planted in rotation with the legumes (Rurangwa, Vanlauwe and Giller, 2018). However, this biological behaviour of grain legumes is limited if the local rhizobia population in the soil is depleted or ineffective (Kermah *et al.*, 2018), which is the case in most Ghanaian soils, particularly in northern Ghana. This limitation can be overcome by introducing appropriate rhizobia into soils via inoculation of legume seeds (Ulzen *et al.*, 2018). The effective application of the inoculant technology requires not just knowledge about its potential benefits but also proper storage (of the inoculants) and inoculation procedures to ensure that experimental results about the effectiveness of the technology are replicated by farmers on their own fields. This makes the choice of media for disseminating information about the new technology even more important.

Aside the inoculant technology itself, farmers were advised to inoculate improved certified legume seeds to maximise the potential benefits of the inoculant technology. For this purpose, specific legume varieties were promoted alongside the inoculant technology, although farmers could inoculate other varieties. Clearly, the two main technologies (the inoculants and improved legume seeds) are different in both complexity and newness—the improved legume seed technology being simpler and relatively older (to the farmers) than the inoculant technology. An important question is then whether the impacts of the information dissemination channels on the uptake of these technologies depend on these inherent attributes of the technologies.

The VDD and RLC channels of dissemination were encored on legume-based farmer associations (FAs) and the existing public agricultural extension system—the various district-level Directorates of Agricultural Extension Services of Ghana's Ministry of Food and Agriculture (MoFA). It is known that working through FAs could boost adoption (Caviglia-Harris, 2003), perhaps because such groups encourage peer learning (Conley and Udry, 2010; Foster and Rosenzweig, 1995). A 20-minute-long VDD produced by scientists at the Savanna Agricultural Research Institute of the Council for Scientific and Industrial Research (abbreviated CSIR-SARI)—the inoculant project-implementing agency—was shown in selected communities across the then

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three northern regions using tricycle video vans.[2] The video was in English and in the most widely spoken local language in each of the regions. The video covered inoculant production, handling and preservation, seed inoculation, results from famer field demonstrations and how to access the inoculants. The video stressed the need to use improved legume seeds, with specific varieties recommended for inoculation. The video screening was organised by CSIR-SARI in conjunction with extension agents and hosted by a relevant community-level FA. Aside members of the FAs, other farmers in the communities were also exposed to the VDD because the video was screened at community centres.

All communities (including those in the control group) were exposed to mass media campaigns (mainly local radio broadcasts) about the technologies. The radio broadcasts took place twice a month during the off-farming season and weekly in the month preceding the planting period. The RLCs (made up of an average of 27 FA members) gathered at specific times to listen-in to 45-minute-long radio broadcasts and were provided with mobile phone call credit to enable them call into the broadcast to ask questions and to contribute to the discussions (see Appendix A in supplementary data at *ERAE* online). Given that all the 113 communities were exposed to radio broadcasts, the VDD and RLC interventions are expected to pick up impacts over and above what the mass media method alone could achieve.

2.2. Experimental design and data collection

The interventions evaluated in this article were implemented in 113 communities in northern Ghana. Since the project was implemented through FAs in each of the communities, members of the FAs served as our sample frame. From January to February 2015, we visited all the communities as part of baseline data collection activities. During these visits, we verified that the list of FA members provided by CSIR-SARI as the sample frame for the household survey was accurate. In some cases, the list was updated in collaboration with the FA leaders at the community level.

At 0.05 significance level, intra-cluster correlation of 0.11, expected standardised effect size of 0.30, and 0.15 (or 15 per cent) explained variation from the inclusion of covariates to increase precision; our calculations (Appendix B in supplementary data at *ERAE* online) showed that we needed to draw at least 8 households per community to have the minimum 80 per cent statistical power to detect an effect if indeed present. Given budget constraints, we took a random draw of 10 FA members per community, the extra two serving as a buffer against attrition. If we set the significance level at 0.03 (instead of 0.05) due to multiple hypothesis testing (see Appendix B in supplementary data at *ERAE* online), then the 10 FA members per community give just 80 per cent power rather than 84 per cent power in the absence of multiple testing adjustment. We achieved the planned sample size in all communities except four,

2 Two new regions were carved out of the Northern region following a referendum in December 2018 to create additional regions in Ghana, thus bringing the number of regions now in northern Ghana to five.

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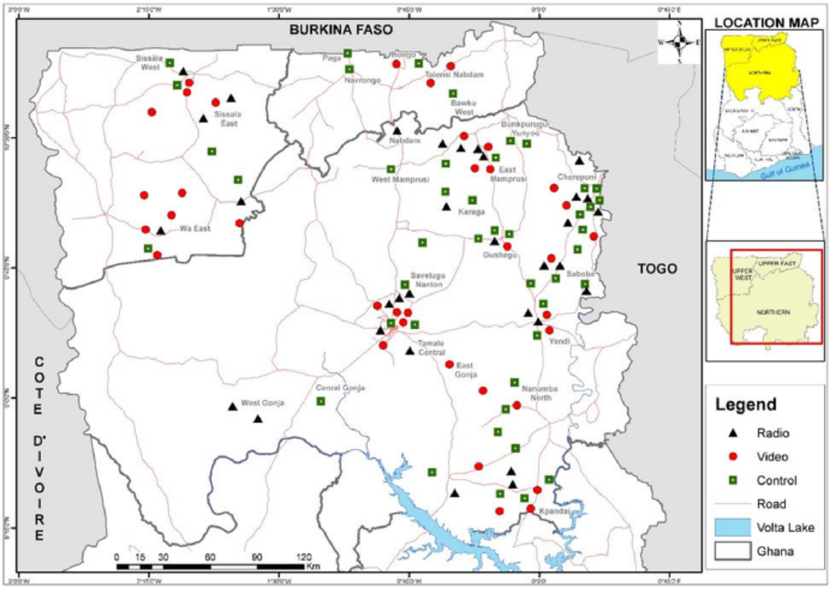

Fig. 1. Map of northern Ghana showing treatment and control communities.

where we successfully interviewed 9 FA-member households, resulting in a baseline sample size of 1,126 households. The 113 communities (Figure 1) were randomly assigned to one of the three experimental groups:

T1. Control: 39 communities were exposed to all the project interventions except the VDD and RLC.
T2. VDD: 37 communities were exposed to all the project interventions (including the VDD) except the RLC.
T3. RLC: 37 communities were exposed to all the project interventions (including the RLC) except the VDD.

Because the plan was to evaluate outcomes related to the 2016 cropping season, the VDD and RLC treatments were implemented before the 2016 planting season, which for the legumes was between early June and mid-August. The VDD and RLC interventions were implemented between November 2015 and May 2016. All the 1,126 FA-member households were successfully followed up during the endline survey carried out between March and April 2017.[3] Figure 2 provides a timeline of the experiment and data collection activities.

Although attrition was not an issue, our field experiment suffered from imperfect compliance. As shown in Table 1, compliance rates were 90 per cent for the control group (i.e. 35 out of 39), 86 per cent for the VDD group

3 We delayed the follow-up survey to allow some time after the 2016 crop harvest and marketing activities.

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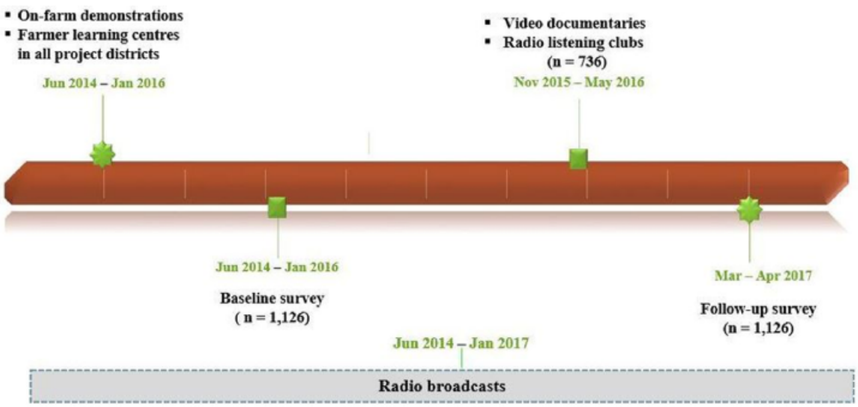

Fig. 2. Intervention and activity timeline.

(i.e. 32 out of 37) and 89 per cent for the RLC group (i.e. 33 out of 37). Thus, we have an issue of two-sided noncompliance caused by project implementers not treating some of the communities that were assigned to treatment and treating some of the communities that were assigned to the control group. The overall noncompliance rate was about 11.6 per cent. Although compliers and noncompliers are identical on observed characteristics (Table A2, Appendix B in supplementary data at ERAE online), we account for noncompliance in the impact estimation method.

2.3. Outcome indicators

We evaluate the impact of the VDD and RLC interventions on four outcomes. The first indicator is a measure of inoculant awareness or knowledge, that is, whether a farmer knows about inoculants.[4] To minimise mismeasurement bias, we only accept as valid the answer 'yes' to the question 'have you heard about inoculant' if the farmer is able to describe correctly what inoculant is as described in the content of the information dissemination channels (including the radio broadcast). Our interest in this indicator is essentially to see whether the impact (or the lack thereof) of the interventions on the other outcomes could be because of knowledge or awareness differences.

The second indicator is actual inoculant uptake, which is the main intermediate outcome of interest to the project. Ideally, we should measure this using either the proportion of land allocated to the technology or the intensity of use (quantity/expenditure per unit area). However, with a large number of nonusers, we have a skewed distribution with a large mass at zero. We therefore simply use the indicator variable of whether a farmer used inoculants during the 2016 cropping season or not as the main uptake indicator. To minimise

4 Unfortunately, we did not test farmer knowledge about how to use the inoculants.

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Table 1. Compliance with treatment assignment (number of communities/farmer organisations).

		Ex post assignment			
		Control	VDD	RLC	Total (ex-ante)
Ex ante assignment	Control	35	1	3	39
	VDD	5	32	0	37
	RLC	0	4	33	37
	Total (ex-post)	40	37	36	113

Note: This table shows the number of villages that adhered to the treatment assignment rules and those that did not.

possible self-reporting bias, we asked farmers to describe the inoculant or to show the sachet (as the inoculants were sold in 100-g sachets).

The third outcome is the use of improved varieties of legumes. This indicator is motivated by the fact that the content of the messages delivered through all the information dissemination channels included the need to use improved legume seeds. It must be noted that this indicator could suffer from the problem of misclassification—either false negatives or false positives (Wossen *et al.*, 2018)—because we did not test farmers' knowledge of what they report as improved legume seeds.

Fourth, we evaluate the impact of the intervention on legume yields. A number of studies have reported significant increases in yields and profits from the application of the inoculant technology on farmer fields. For example, the use of inoculants more than doubled legume yields in Rwanda (Rurangwa, Vanlauwe and Giller, 2018). The application of inoculants did not only increase yields in Kaduna and Kano (northern Nigeria), but was also economically more profitable than the application of inorganic fertilisers (Ronner *et al.*, 2016). Similarly in western Kenya, inoculants increased soybean yields by about 26 per cent, with an average gross margin of about $278/ha (Mutuma *et al.*, 2014). In northern Ghana, large increases in yields (between 22 and 200 per cent) and significant economic returns due to inoculation of soybean and cowpea have been reported (Asei, Ewusi-Mensah and Abaidoo, 2015; Ulzen *et al.*, 2016). In spite of the yield and economic gains of inoculant use, the unavailability of the product and lack of information about its usefulness have long been identified as major hindrances to adoption (Woomer *et al.*, 1997). The dissemination of information about the use and potential benefits of the inoculants using the VDD and RLC channels could boost adoption, yields and potential economic gains because the inoculant technology is relatively new to smallholder farmers in Ghana.

2.4. Baseline orthogonality tests

Table 2 reports sample mean values of the outcome variables and other relevant characteristics of the sample at baseline. Since the assignment of farmers to control and treatment groups was random by design, it is more useful to

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present the baseline mean comparison and orthogonality tests by the ex post treatment groups. What is striking (although not surprising) is the very low level of inoculant awareness among farmers prior to the CSIR-SARI inoculant project: only about 3 per cent of the farmers could correctly tell what inoculants are. Given the potential benefits of inoculants, particularly for poor farmers who cannot afford inorganic fertilisers, it seems clear that the information dissemination campaigns to promote this new technology were justified. As the results show, inoculant awareness is identical between the treated and control groups at baseline. We find that, while inoculant use was not entirely new to some farmers in the study areas, only about 1.4 per cent used the technology at baseline. This is most likely explained by the activities of the N2Africa project in some of the communities in the past (Ulzen *et al.*, 2018).

At baseline, only about 15 per cent of farmers reported using any improved legume seeds, and yields averaged about 0.681 ton/ha, which is much lower than the 1.3 tons/ha reported by Ghana's Ministry of Food and Agriculture for the same year (MoFA, 2016).[5] The low yields are not surprising because of low and declining soil quality and low adoption of productivity-enhancing inputs. For example, only about 18 per cent of farmers applied inorganic fertiliser to legumes at baseline.

Although the focus of the interventions was the three legumes, the sample of farmers involved have a diversified portfolio of crops—only about a third of total cultivated land was devoted to the legumes at baseline. Other key baseline characteristics include the following: about 80 per cent of the farm households can be described as small scale (below 5 ha); about 61 per cent live below the international poverty line of $1.90 per person per day at purchasing power parity (PPP) exchange rates for private consumption; about 52 per cent participate in off-farm rural employment; and only about 21 per cent reported access to agricultural input credit (mostly from informal sources).

As Table 2 shows, the null hypothesis that the difference-in-means for each variable between the treatment and control groups equal zero cannot be rejected at conventional levels. We also carried out omnibus tests of joint orthogonality, which returned a chi-squared statistic of 56.91, with 56 degrees of freedom (p value = 0.441) based on the ex ante experimental assignment. Using the ex post experimental groups, however, returned a higher chi-squared statistic of 66.78 (p value = 0.153). These mean that the randomisation was successful at balancing observed baseline characteristics for our sample.

3. Estimation and inferential methods

3.1. Model specification

Since the randomised assignment of clusters (the intervention communities) ensured that households had similar observed mean characteristics at baseline

5 As noted by Dzanku and Udry (2017), farmer reported yields from survey data tend to be much lower than what the Ministry of Food and Agriculture reports.

Table 2. Baseline mean values and orthogonality tests, by ex-post treatment.

	Mean			p value of difference			
	Control n = 390	VDD n = 369	RLC n = 367	VDD—control	RLC—control	VDD—RLC	Joint test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome variables							
Aware of inoculants	0.03	0.03	0.04	0.89	0.63	0.60	0.82
Used inoculants	0.01	0.02	0.01	0.45	0.60	0.24	0.49
Used improved legume seeds	0.13	0.15	0.18	0.72	0.34	0.56	0.63
Legume yield (ton/ha)	0.69	0.66	0.69	0.64	0.97	0.69	0.87
Other characteristics							
Female farmer (yes = 1)	0.35	0.38	0.41	0.51	0.12	0.44	0.30
Age of farmer	46.63	45.92	46.66	0.63	0.99	0.60	0.84
Household size	7.25	7.19	7.15	0.86	0.73	0.89	0.94
Years of schooling	5.91	5.43	5.53	0.55	0.56	0.89	0.79
Total farm size (ha)	3.36	3.74	3.61	0.54	0.58	0.81	0.79
Legume farm size (ha)	1.17	1.39	1.23	0.57	0.83	0.63	0.85
Share of land cultivated to legumes	0.35	0.36	0.34	0.57	0.97	0.54	0.79
Inorganic fertiliser use on any crop	0.55	0.54	0.49	0.88	0.28	0.40	0.50
Quantity of fertiliser on any crop (kg/ha)	62.87	63.44	42.87	0.97	0.16	0.14	0.23
Inorganic fertiliser use on legumes	0.17	0.20	0.18	0.43	0.75	0.65	0.74
Quantity fertiliser use on legumes (kg/ha)	44.64	52.28	26.05	0.72	0.26	0.15	0.25
Soil quality index	0.43	0.49	0.40	0.27	0.42	0.07	0.20

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Table 2. (Continued)

	Mean			*p* value of difference			
	Control n = 390	VDD n = 369	RLC n = 367	VDD—control	RLC—control	VDD—RLC	Joint test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Improved seeds on crops other than legumes	0.23	0.28	0.24	0.40	0.92	0.48	0.67
Number of legumes produced	0.97	0.96	1.05	0.96	0.48	0.45	0.71
Access to agricultural credit	0.17	0.25	0.22	0.18	0.42	0.61	0.39
Per capita income (PPP $)	725.28	807.27	765.99	0.40	0.66	0.68	0.69
Poverty headcount ratio at $1.90 PPP $	0.61	0.61	0.60	0.92	0.75	0.83	0.95
Normalized asset index	0.20	0.20	0.17	0.92	0.11	0.17	0.24
Participation in off-farm employment	0.51	0.51	0.54	0.86	0.52	0.59	0.78
Distance to market (km)	6.76	6.68	6.76	0.95	1.00	0.96	1.00
Distance to all-weather road (km)	1.89	2.40	1.74	0.56	0.86	0.45	0.74
Distance to agro-dealership (km)	9.35	8.78	8.05	0.79	0.48	0.70	0.77
Owns radio	0.51	0.54	0.53	0.58	0.67	0.86	0.84
Owns mobile phone	0.72	0.72	0.68	0.89	0.37	0.38	0.60

Note: The *p* values are from community-level clustered standard errors after using baseline data to estimate: $y_{ij} = \alpha + \beta_1 VDD_{ij} + \beta_2 RLC_{ij} + \varepsilon_{ij0}$.

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(Table 2), we could estimate the impact of the interventions using the single-difference model, which is a comparison of the treatment and control groups at follow-up:

$$y_{ij1} = \alpha + \tau_1 VDD_j + \tau_2 RLC_j + \varepsilon_{ij1}, \tag{1}$$

where y_{ij1} is the outcome variable of interest for farm household i in community j at follow-up; τ_1 and τ_2 are the average intention-to-treat (ITT) or as-treated (AST) effects associated with the VDD and RLC interventions, respectively; and ε_{ij1} is the random disturbance term. Comparing the difference between the two effects (i.e. τ_1 and τ_2) is straightforward. Because the correlation of outcomes between baseline and follow-up is low (less than 0.5) for all outcomes except yield (0.77), analysis of covariance (ANCOVA) could be used to increase precision and efficiency (McKenzie, 2012). That is, we can include the baseline value of the outcome, y_{ij0}, in equation (1):

$$y_{ij1} = \alpha + \tau_1 VDD_j + \tau_2 RLC_j + y_{ij0} + \varepsilon_{ij1}. \tag{2}$$

Additionally, we can also specify a pooled difference-in-differences (DD) model (Angrist and Pischke, 2009):

$$y_{ijt} = \alpha + \gamma_1 VDD_j + \gamma_2 RLC_j + \lambda time_t + \tau_1 (VDD_j \cdot time_t) + \tau_2 (RLC_j \cdot time_t) + \varepsilon_{ijt}, \tag{3}$$

where y_{ijt} is the outcome variable of interest for household i in intervention community j at time t, $time_t$ is the time dummy that equals 1 at endline and 0 at baseline; all other variables and parameters are as described for equation (1).

These equations are estimated by both Ordinary least squares (OLS), and by instrumental variable (IV) methods.[6] For the IV estimates, we instrument actual programme participation by the randomised assignment. The OLS regressions provide the ITT estimates that compare outcomes by assignment to treatment, ignoring actual treatment receipt. On the other hand, the IV estimates provide the AST average effects that compare outcomes by actual receipt of treatment, accounting for double-sided noncompliance.

As indicated earlier, the overall success of the randomisation means that we do not need to include covariates in any of the equations in order to estimate the average effects of the interventions (Athey and Imbens, 2017). However, the estimates may benefit from baseline covariate adjustment to remove any bias that may be present due to the noncompliance. Even in the absence of this randomisation compromise, including covariates could increase the precision of the estimates if the covariates are sufficiently correlated with an outcome (Athey and Imbens, 2017), but which covariates should we include in the regressions? We answer this question using the least absolute shrinkage

6 The IVs are used in a control function approach as follows. In the first stage, we regress the ex post treatment receipts on the ex ante treatment assignments (i.e. the instrumental variables) and other covariates selected by the PDS lasso procedure using probit models. We then calculate the inverse Mills ratios, which then enter the second-stage equations as *additional* covariates to account for possibly nonrandom compliance to assignment rules. The second-stage standard errors are bootstrapped using 2000 draws.

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and selection operator (LASSO or lasso) procedure, specifically, the post-double-selection (PDS) lasso procedure (Belloni, Chernozhukov and Hansen, 2014). This is accomplished in three steps and is superior to the ordinary lasso (see Appendix C in supplementary data at *ERAE* online). In the first step, we include the list of potential baseline covariates (39 in all, including 14 unpenalised district dummies) and then use the lasso to pick out those that are sufficiently correlated with treatment status. In the second step, the lasso is used to select covariates that may be correlated with the outcome, and then in the final step, the treatment effects (τ_1 and τ_2) are estimated, with the covariates being the union of controls that the lasso picked out in the first two steps.

3.2. Multiple hypothesis testing and randomisation inference

There is the risk of over-rejection of the null hypothesis of no intervention impact (false positives or type I error) due to multiple testing. With inference involving multiple outcomes as well as tests of impact heterogeneity across subgroups, the probability of wrongly rejecting at least one true null hypothesis increases with the number of hypotheses tested. We address this issue by controlling for false discovery rate (FDR)—the proportion of null hypotheses rejections that are actually false positives. Here, this is accomplished using the Benjamini, Krieger and Yekutieli (2006) sharpened two-stage q values approach as described in Anderson (2008). The FDR approach could be viewed as a balance between the risk of false positives and false negatives because it allows a small number of type I errors in return for more statistical power, unlike the family-wise error rate (FWER) correction (Anderson, 2008). With three experimental arms (including the control group) and four main outcomes, we consider 12 tests in each family of hypotheses for each model specification. Thus, we perform the FWER adjustment for 4×3 outcome-treatment pairs for each specification. We consider each regression specification in a separate family of hypotheses because they are alternative specifications for testing the same hypothesis. Following Young (2019), we used random sampling with replacement (sampling via bootstrapping), which enables the calculation of exact p value under fairly standard assumptions.

Finally, in addition to sampling-based inference, we report our key hypothesis tests using randomisation-based inference (Young, 2019). In this case, uncertainty in the estimates result from the random assignment of the treatments rather than from sampling, which then allows estimation of exact p values under the sharp null hypothesis that all treatment effects equal to zero. The exact test of the sharp null is thus constructed by calculating all possible realisations of a test statistic and rejecting if that which is actually observed in the experiment is more extreme than 0.05, say. The advantage of this approach is that it provides exact finite sample test statistics without relying on asymptotic results or the characteristics of the regression and is superior to all other methods when the sharp null is true without being inferior if the opposite is the case (Young, 2019). Besides, this approach is reliable under the influence of outlier treatment values and under multiple testing.

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Table 3. Effect of VDD and RLC on inoculant technology awareness.

	(1)	(2)	(3)	(4)	(5)
	ITT: OLS	AST: OLS	IV 1: first stage	IV 2: First stage	AST: IV second stage
	Awareness	Awareness	VDD_AST	RLC_AST	Awareness
Panel (A)					
VDD impact	–0.001	0.019			–0.001
	(0.973)	(0.519)			(0.990)
	[0.492]	[0.166]			[0.329]
	(0.975)	(0.527)			(0.989)
RLC impact	0.023	0.034			0.027
	(0.398)	(0.240)			(0.482)
	[0.249]	[0.092]			[0.303]
	(0.376)	(0.234)			(0.402)
Assignment to VDD			0.406		
			(0.000)		
Assignment to RLC				0.348	
				(0.000)	
Observations	1126	1126	1126	1126	1126
Control mean	0.826	0.816			0.825
VDD = RLC (p value)	0.438	0.628			0.511
	[0.249]	[0.187]			[0.303]
	(0.429)	(0.608)			(0.504)
Panel (B)					
VDD impact	–0.001	0.028			0.003
	(0.972)	(0.413)			(0.948)
	[0.480]	[0.304]			[0.477]
	(0.975)	(0.421)			(0.954)

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Table 3. (Continued)

	(1)	(2)	(3)	(4)	(5)
	ITT: OLS	AST: OLS	IV 1: first stage	IV 2: First stage	AST: IV second stage
	Awareness	Awareness	VDD_AST	RLC_AST	Awareness
RLC impact	–0.008	0.012			–0.012
	(0.810)	(0.711)			(0.734)
	[0.456]	[0.422]			[0.477]
	(0.788)	(0.713)			(0.736)
Assignment to VDD			0.411		
			(0.000)		
Assignment to RLC				0.354	
				(0.000)	
Observations	1126	1126	1126	1126	1126
Control mean	0.836	0.820			0.836
VDD = RLC (*p* value)	0.842	0.591			0.686
	[0.456]	[0.368]			[0.477]
	(0.838)	(0.588)			(0.570)

Note: This table presents results from the single-difference regressions, with covariate adjustment (Panel (A)) and without covariate adjustment (Panel (B)). ITT and AST denote intention-to-treat and as-treated, respectively. Column (1) shows the results from regressing the inoculant awareness outcome on dummies for assignment to the VDD and RLC treatments. Column (2) is the regression of inoculant awareness on the dummies for actual VDD and RLC treatments received. Columns (3) and (4) are the first stages of the control function regressions, where actual VDD and RLC treatments are regressed on ex-ante VDD and RLC treatment assignments. Column (5) is the second stage, which regresses awareness on actual VDD and RLC treatments received and the inverse Mills ratios (IMRs) calculated from the first stage to correct for possibly non-random compliance. The choice of which baseline covariate to include in the regressions was determined by the PDS lasso procedure. All the *p* values are based on community-level clustered standard errors. Naïve *p* values (in parentheses) are unadjusted for multiple testing; *q* values [in brackets] control for false discovery rate (FDR) using the sharpened two-stage approach of Benjamini, Krieger and Yekutieli (2006). The randomization-*t* *p* values (in diamond brackets) follow Young (2019) and are produced using his Stata code, with 2,000 draws.

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4. Results

Here, we report and discuss estimates of the average impacts of the interventions on the four outcomes in separate subsections. In all cases, inference is based on community-level clustered standard errors and *p* values that are adjusted for multiple testing. Each table also contains randomisation inference (RI) *p* values calculated using codes provided by Young (2020). In the interest of space and coherence, we present estimates from the single-difference ITT and AST regressions here; the results from the DD specification can be found in Appendix C (Appendix in supplementary data at *ERAE* online). Figures A3–A6 (Appendix C in supplementary data at *ERAE* online) provide the graphical results.

4.1. Awareness effects

The potential mechanism through which the VDD and RLC could have differential effects on the outcomes of interest include differing effects on awareness, retention, and quality of knowledge gained, which could manifest through the right application of information. Given that the inoculant technology was relatively new in the population, we first evaluate the impact of the VDD and RLC on the inoculant technology awareness. We note that awareness of the inoculant technology (i.e. correctly describing the inoculant) increased astronomically, from only about 3 per cent of the sample at baseline to 83 per cent at endline.

Turning to the regressions, first, the PDS lasso procedure did not pick up any baseline covariate for inclusion in the awareness equations (see Table A3, Appendix C in supplementary data at *ERAE* online). Therefore, only the unpenalised district fixed effects are included. Table 3 report the single-difference ITT and AST estimates with covariate adjustment (Panel (A)) and without covariate adjustment (Panel (b)); the corresponding DD results are reported in Table A4 (Appendix C in supplementary data at *ERAE* online). The results show that, at the 0.05 level of significance, neither the VDD nor the RLC average effects are different from zero across all model specifications. This result is robust to choice of estimator (Table A4, Appendix in supplementary data at *ERAE* online). This shows that there is no evidence of an incremental effect of the VDD or RLC on awareness creation, meaning that the radio broadcasts and the other awareness creation activities did a good job at raising awareness about the inoculant technology.

These results are probably not surprising because the advantage of video over other mass media information dissemination methods depends on the learning outcome, and when the outcome is simply to create awareness about the availability of a technology, the video may not have much advantage over audio (Schwartz and Hartman, 2007). Nonetheless, the absence of detectable impact could be related to how the outcome was measured; Van Campenhout, Spielman and Lecoutere (2020) conducted a multiple-choice knowledge tests

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among Ugandan farmers and found that those who viewed short video messages about maize production technologies had significantly higher knowledge scores than those who were reached through IVR and SMS.

4.2. Effect on inoculants uptake

From the foregoing, is it reasonable to expect differential impact of the VDD and RLC interventions on inoculant uptake in the absence of observable awareness gaps? The answer to this question is in the affirmative because both the theoretical and empirical literatures show that technology adoption is not just a two-stage process whereby awareness leads to adoption (Dimara and Skuras, 2003; Feder, Just and Zilberman, 1985; Sunding and Zilberman, 2001); the quality of information matters, and the source of information and how it is delivered influences a transition from awareness, to being convinced that it actually works and then the decision to try it (Genius *et al.*, 2014; Leathers and Smale, 1991; Lee, Barrett and McPeak, 2006).

On inoculants uptake, we observe that the proportion of farmers in the entire sample who used the technology increased from about 1.4 per cent at baseline to about 15.1 per cent at endline. While this rate is much lower than anticipated before the start of the intervention, it is not surprising because of delays in the production and distribution of the inoculants. This is an important constraint because the new technology was not readily available on the market, although production and distribution were a component part of the programme. Specifically, the inoculant production laboratory was not completed until December 2016, and so CSIR-SARI imported approximately 702 kg of inoculants from Brazil for the 2016 crop year.[7] Yet, inoculant uptake rate at endline topped 22 per cent among the VDD treatment group, compared with about 15 per cent and 9 per cent among the RLC treatment and the control groups, respectively.

Table 4 reports the single-difference ITT and AST model results; the corresponding DD estimation results are presented in Table A5 (Appendix C in supplementary data at *ERAE* online). Here, too, PDS lasso did not select any baseline covariates, so we included only the unpenalised district fixed effects. In terms of statistical significance, the VDD treatment effects are robust to model specification and the inclusion/exclusion of covariates (district fixed effects), but the RLC treatment effects are not. On average, the VDD increased inoculants uptake by between 15.8 percentage points [Table 4, Panel (A) column (1)] and 16–18 percentage points (for the AST estimates), all of which are statistically different from zero at the 0.001 level, even after multiple inference correction; the RI-based p-values are also zero to three decimal places. The VDD impact magnitudes are smaller by up to 3 percentage points when district fixed effects are excluded (Panel (B)), and in the DD model (Tables A5, Appendix C in supplementary data at *ERAE* online).

Conversely, the RLC treatment effects are not statistically different from zero in any of the specifications except in the single-difference models with

7 This quantity was enough to supply inoculants to about 2,500 farmers, assuming that all legumes under cultivation in our sample were inoculated.

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Table 4. Effect of VDD and RLC on inoculant technology uptake (single-difference).

	(1)	(2)	(3)	(4)	(5)
	ITT: OLS	AST: OLS	IV 1: first stage	IV 2: first stage	AST: IV second stage
	Inoculant use	Inoculant use	VDD_AST	RLC_AST	Inoculant use
Panel (A)					
VDD impact	0.158	0.160			0.180
	(0.000)	(0.000)			(0.000)
	[0.001]	[0.001]			[0.001]
	(0.000)	(0.000)			(0.000)
RLC impact	0.107	0.061			0.104
	(0.003)	(0.060)			(0.035)
	[0.008]	[0.037]			[0.059]
	(0.005)	(0.069)			(0.023)
Assignment to VDD			0.406		
			(0.000)		
Assignment to RLC				0.348	
				(0.000)	
Observations	1126	1126	1126	1126	1126
Control mean	0.064	0.079			0.059
VDD = RLC (p value)	0.220	0.014			0.185
	[0.160]	[0.017]			[0.115]
	(0.235)	(0.018)			(0.209)
Panel (B)					
VDD impact	0.135	0.162			0.159
	(0.000)	(0.000)			(0.000)
	[0.003]	[0.001]			[0.002]
	(0.000)	(0.000)			(0.001)

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Table 4. (Continued)

	(1)	(2)	(3)	(4)	(5)
	ITT: OLS	AST: OLS	IV 1: first stage	IV 2: first stage	AST: IV second stage
	Inoculant use	Inoculant use	VDD_AST	RLC_AST	Inoculant use
RLC impact	0.060	0.033			0.054
	(0.099)	(0.356)			(0.215)
	[0.153]	[0.304]			[0.186]
	(0.107)	(0.357)			(0.221)
Assignment to VDD			0.411		
			(0.000)		
Assignment to RLC				0.354	
				(0.000)	
Observations	1126	1126	1126	1126	1126
Control mean	0.087	0.087			0.082
VDD = RLC (*p* value)	(0.086)	(0.002)			(0.040)
	[0.153]	[0.006]			[0.069]
	(0.085)	(0.002)			(0.049)

Note: This table presents results from the single-difference regressions, with covariate adjustment (Panel (A)) and without covariate adjustment (Panel (B)). ITT and AST denote intention-to-treat and as-treated, respectively. Column (1) shows the results from regressing the inoculant uptake outcome on dummies for assignment to the VDD and RLC treatments. Column (2) is the regression of inoculant uptake on the dummies for actual VDD and RLC treatments received. Columns (3) and (4) are the first stages of the control function regressions, where actual VDD and RLC treatments are regressed on ex ante VDD and RLC treatment assignments. Column (5) is the second stage, which regresses inoculant uptake on actual VDD and RLC treatments received and the inverse Mills ratios (IMRs) calculated from the first stage to correct for possibly nonrandom compliance. The choice of which baseline covariate to include in the regressions was determined by the PDS lasso procedure. All the p values are based on community level clustered standard errors. Naïve p values (in parentheses) are unadjusted for multiple testing; q values [in brackets] control for false discovery rate (FDR) using the sharpened two-stage approach of Benjamini, Krieger and Yekutieli (2006). The randomization-t p-values (in diamond brackets) follow Young (2019) and are produced using his Stata code, with 2,000 draws.

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district fixed effects (Panel (A) of Table 4), where the ITT and AST IV estimates show that the RLC treatment increased inoculants uptake by 10.7 and 10.4 percentage points, respectively [columns (1) and (5)]; both the q-values and the RI-based p-values are less than 0.05. The RLC effect sizes reduce by almost half and then lose statistical significance in the absence of the district fixed effects (Panel (b) of Table 4), and in the DD models with or without district fixed effects (Tables A5, Appendix C in supplementary data at *ERAE* online). The fact that we could not detect statistically significant RLC effects in these regressions is thus because our study is underpowered to detect an RLC treatment effect size that is less than 10 percentage points for inoculant uptake (Table A1, Appendix A in supplementary data at *ERAE* online).

It is therefore not surprising that whereas the results in Panel (A) of Table 4 show insufficient evidence to reject the hypothesis that the VDD and RLC treatment effects are equal$(\hat{\tau}_1 - \hat{\tau}_2 = 0)$, this hypothesis is mostly rejected elsewhere for the AST estimates (Panel (b) of Table 4 and Table A5, Appendix in supplementary data at *ERAE* online). The magnitude of the VDD ITT effect, approximately 16-percentage point increase, relative to a control group mean of about 6 percentage points, is practically meaningful.

The fact that the content of the audio messages were similar across the VDD and RLC treatments, and that awareness of the inoculant technology was identical across the experimental arms (Table 3) suggest that the VDD did a better job at convincing farmers. This is probably because the VDD better displayed the benefits of the inoculant technology, implying that barriers to learning about correct technology usage or benefits of the technology constrain adoption (Foster and Rosenzweig, 2010). While it appears that listening without seeing is less effective in spite of the interactivity offered by the RLC, such a conclusion needs to be tempered by the low statistical power to detect a meaningful effect of the RLC intervention. This notwithstanding, our finding about the effectiveness of video is consistent with the agricultural technology adoption literature suggesting that adoption decisions depend on channel of information dissemination (Gervais, Lambert and Boutin-Dufresne, 2001; Wozniak, 1993), and that video-mediated agricultural extension services tend to be superior (Abate *et al.*, 2019; Hörner *et al.*, 2019; Van Campenhout, Spielman and Lecoutere, 2020).

4.3. Effect on the use of improved seeds

Together with the inoculants, CSIR-SARI also promoted and made available three improved varieties of the legumes: *Jeguma, Songotra*, and *Chinesse* varieties of soybean, cowpea, and groundnuts, respectively. We observe that the use of the improved seeds increased from 15.3 per cent at baseline to 23.7 per cent at endline for the entire sample. Here, we ask whether choice of information dissemination channel matters.

First, the PDS lasso procedure selected only one covariate (the baseline value of the dependent variable) for inclusion in the models. Therefore, the

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single-difference results presented in Panel (A) of Table 5 are ANCOVA estimates with district fixed effects. The results without covariate adjustment are presented in Panel (B). Table A6 (Appendix C in supplementary data at *ERAE* online) presents results from the corresponding DD model. We find that the VDD treatment statistically significantly increased the use of the modern varieties across all model specifications, but the RLC treatment effect is not statistically different from zero at the 0.05 level in the DD model. However, because of the moderately low autocorrelation coefficient of this outcome (about 0.33), the ANCOVA model results, which show that the RLC significantly increased the use of the modern seed technology, should be preferred; the ANCOVA model provided gains in statistical power as Table A1 (Appendix A in supplementary data at *ERAE* online) demonstrates, which is consistent with the data in McKenzie (2012).

We note that the AST effects are only slightly larger than their corresponding ITT estimates, particularly when we instrument treatment receipt with treatment assignment. In the ITT model, the estimated uptake of the modern seeds among the VDD treatment group was 30 per cent, relative to 17 per cent among the control group; the effect size of 13 percentage points is significant at the 0.01 level (column 1 of Table 5). The corresponding estimate of uptake is 32 per cent versus 16 per cent in the instrumented AST model, with the effect size of 16 percentage points being significant at the 0.001 level (column 5 of Table 5, Panel (A)). While the RLC effect size is smaller than the VDD effect size by about 4–7 percentage points (Table 5), our study is not adequately powered to reject the null that the difference between the VDD and RLC treatment effects is equal to zero at the 0.05 level (MDE = 10.9 percentage points, Table A1 of Appendix A in supplementary data at *ERAE* online).

It is intriguing that the RLC seemed to do a better job at encouraging the use of the improved seed than the inoculants, which could be because the two technologies have some fundamental differences. First, the inoculant technology is very new to the population while improved seeds are much less so. Second, the inoculant technology appears more complex to farmers than the improved seeds as the former requires behavioural changes in terms of handling, preparation and use. It thus appears that the relative complexity of a technology should influence the choice of channel for reaching farmers with information in order to boost adoption, all else remaining the same.

4.4. Effect on yields

Finally, we assess the impact of the interventions on legume yields. There are a number of channels through which the interventions could affect yields. The obvious channels are increased inoculant and improved seed uptake. Additionally, the radio broadcast, video documentaries and listening clubs discussed the need to complement the inoculant and improved seed technologies with good agronomic practices (GAPs), such as timely weeding and integrated soil and water management practices, which are known to be important for increasing yields (Munialo *et al.*, 2020).

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Table 5. Effect of VDD and RLC on the use of improved legume seeds.

	(1)	(2)	(3)	(4)	(5)
	ITT: OLS	AST: OLS	IV 1: first stage	IV 2: first stage	AST: IV second stage
	Inoculant use	Inoculant use	VDD_AST	RLC_AST	Inoculant use
Panel (A)					
VDD impact	0.130	0.161			0.156
	(0.001)	(0.000)			(0.003)
	[0.003]	[0.001]			[0.009]
	(0.000)	(0.000)			(0.001)
RLC impact	0.087	0.080			0.085
	(0.005)	(0.011)			(0.054)
	[0.009]	[0.017]			[0.066]
	(0.005)	(0.011)			(0.034)
Assignment to VDD			0.407		
			(0.000)		
Assignment to RLC				0.347	
				(0.000)	
Observations	1126	1126	1126	1126	1126
Control mean	0.171	0.164			0.163
VDD = RLC (p value)	0.278	0.036			0.156
	[0.183]	[0.026]			[0.109]
	(0.280)	(0.043)			(0.114)
Panel (B)					
VDD impact	0.142	0.178			0.181
	(0.000)	(0.000)			(0.000)
	[0.004]	[0.001			[0.002]
	(0.001)	(0.000)			(0.000)

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Table 5. (Continued)

	(1)	(2)	(3)	(4)	(5)
	ITT: OLS	AST: OLS	IV 1: first stage	IV 2: first stage	AST: IV second stage
	Inoculant use	Inoculant use	VDD_AST	RLC_AST	Inoculant use
RLC impact	0.095	0.097			0.100
	(0.001)	(0.001)			(0.005)
	[0.004]	[0.003			[0.018]
	(0.001)	(0.001)			(0.003)
Assignment to VDD			0.411		
			(0.000)		
Assignment to RLC				0.354	
				(0.000)	
Observations	1126	1126	1126	1126	1126
Control mean	0.164	0.152			0.150
VDD = RLC (*p* value)	0.209	0.024			0.072
	[0.248]	[0.041]			[0.106]
	(0.205)	(0.031)			(0.061)

Note: This table presents results from the single-difference regressions, with covariate adjustment (Panel (A)) and without covariate adjustment (Panel (B)). ITT and AST denote intention-to-treat and as-treated, respectively. Column (1) shows the results from regressing the improved seed uptake outcome on dummies for assignment to the VDD and RLC treatments. Column (2) is the regression of improved seed uptake on the dummies for actual VDD and RLC treatments received. Columns (3) and (4) are the first stages of the control function regressions, where actual VDD and RLC treatments are regressed on ex ante VDD and RLC treatment assignments. Column (5) is the second stage, which regresses improved seed uptake on actual VDD and RLC treatments received and the inverse Mills ratios (IMRs) calculated from the first stage to correct for possibly nonrandom compliance. The choice of which baseline covariate to include in the regressions was determined by the PDS lasso procedure. All the *p* values are based on community level clustered standard errors. Naïve *p* values (in parentheses) are unadjusted for multiple testing; *q* values [in brackets] control for false discovery rate (FDR) using the sharpened two-stage approach of Benjamini, Krieger and Yekutieli (2006). The randomization-*t* p-values (in diamond brackets) follow Young (2019) and are produced using his Stata code, with 2,000 draws.

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That inoculants increase legume yields under natural science experimental conditions is not in question (Asei, Ewusi-Mensah and Abaidoo, 2015; Ulzen *et al.*, 2016), but whether it does so under farmer conditions could depend on appropriate handling, storage and utilisation of the technology (Martey *et al.*, 2016). In the overall sample, legume yields increased by about 10 per cent between baseline and follow-up (from 0.681 to 0.748 ton/ha). Here, we ask whether the VDD and the RLC interventions were effective in raising yields. Figure 3 presents the results graphically, showing that the distribution of yields in the VDD treatment communities stochastically dominates those in the RLC and control communities. For the regression models, the only covariate selected by the PDS lasso is the value of yields at baseline. Thus, the single-difference results presented in Panel (A) of Table 6 are ANCOVA estimates with district fixed effects.

The ANCOVA ITT effect of the VDD on yields is 186 kg/ha, which represents an average increase of about 28 per cent over the control group mean [Panel (A) column (1) of Table 6]. The corresponding instrumented AST effect of the VDD is larger, about 231 kg/ha, representing a 36 per cent increase over the control group mean [Panel (A) column (5) of Table 6]. Given the high autocorrelation of yields (0.77), the DD model results (Table A7, Appendix C in supplementary data at *ERAE* online) could be preferred and show a VDD ITT effect size of 173 kg/ha, which represents a 25 per cent increase, relative to the baseline mean [column (1)]. Here, too, the instrumented AST VDD effect size is larger, 216 kg/ha, representing an increase of about 31 per cent over

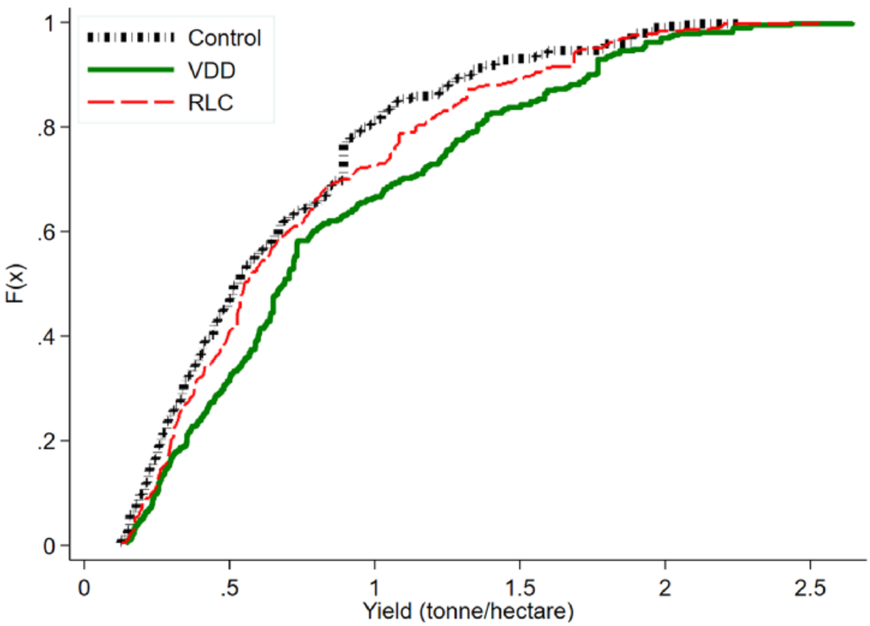

Fig. 3. Distribution of legume yield by treatment arms.

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Table 6. Single-difference effect of VDD and RLC on legume yield (ton/ha).

	(1)	(2)	(3)	(4)	(5)
	ITT: OLS	AST: OLS	IV 1: First stage	IV 2: first stage	AST: IV second stage
	Inoculant use	Inoculant use	VDD_AST	RLC_AST	Inoculant use
Panel (A)					
VDD impact	0.186	0.213			0.231
	(0.000)	(0.000)			(0.000)
	[0.001]	[0.001]			[0.001]
	(0.000)	(0.000)			(0.000)
RLC impact	0.106	0.098			0.106
	(0.005)	(0.011)			(0.042)
	[0.009]	[0.017]			[0.060]
	(0.005)	(0.011)			(0.020)
Assignment to VDD			0.407		
			(0.000)		
Assignment to RLC				0.348	
				(0.000)	
Observations	1126	1126	1126	1126	1126
Control mean	0.653	0.647			0.639
VDD = RLC (*p* value)	0.039	0.003			0.012
	[0.035]	[0.007]			[0.028]
	(0.041)	(0.005)			(0.012)
Panel (B)					
VDD impact	0.185	0.161			0.237
	(0.009)	(0.031)			(0.010)
	[0.024]	[0.047]			[0.023]
	(0.015)	(0.031)			(0.012)
RLC impact	0.068	0.047			0.082
	(0.364)	(0.504)			(0.351)
	[0.256]	[0.339]			[0.243]
	(0.372)	(0.491)			(0.333)

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Table 6. (Continued)

	(1)	(2)	(3)	(4)	(5)
	ITT: OLS	AST: OLS	IV 1: First stage	IV 2: first stage	AST: IV second stage
	Inoculant use	Inoculant use	VDD_AST	RLC_AST	Inoculant use
Assignment to VDD			0.411		
			(0.000)		
Assignment to RLC				0.354	
				(0.000)	
Observations	1126	1126	1126	1126	1126
Control mean	0.666	0.681			0.645
VDD = RLC (p value)	0.116	0.136			0.091
	[0.153]	[0.132]			[0.106]
	(0.127)	(0.152)			(0.123)

Note: This table presents results from the single-difference regressions, with covariate adjustment (Panel (A)) and without covariate adjustment (Panel (B)). ITT and AST denote intention-to-treat and as-treated, respectively. Column (1) shows the results from regressing the legume yield outcome on dummies for assignment to the VDD and RLC treatments. Column (2) is the regression of legume yield on the dummies for actual VDD and RLC treatments received. Columns (3) and (4) are the first stages of the control function regressions, where actual VDD and RLC treatments are regressed on ex ante VDD and RLC treatment assignments. Column (5) is the second stage, which regresses legume yield on actual VDD and RLC treatments received and the inverse Mills ratios (IMRs) calculated from the first stage to correct for possibly nonrandom compliance. The choice of which baseline covariate to include in the regressions was determined by the PDS lasso procedure. All the p values are based on community level clustered standard errors. Naïve p values (in parentheses) are unadjusted for multiple testing; q values [in brackets] control for false discovery rate (FDR) using the sharpened two-stage approach of Benjamini, Krieger and Yekutieli (2006). The randomization-t p-values (in diamond brackets) follow Young (2019) and are produced using his Stata code, with 2,000 draws.

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the baseline mean [column (5)]. The VDD treatment effect sizes are similar when no covariate is included in the models (Table A7, Appendix C in supplementary data at *ERAE* online). These effects are all statistically different from zero at the 0.01 level and remain so using RI or after correcting for multiple hypotheses testing.

Per contra, we cannot reject the null hypothesis that the RLC effect on yield equals zero at the 0.05 level, except in the ANCOVA regression, with district fixed effects reported in Panel (A) of Table 6, where the RLC ITT effect size is 106 kg/ha or a 16 per cent increase relative to the control group mean (column 1); the instrumented AST effect size is also 106 kg/ha, representing a 17 per cent increase, relative to the control group average (column 5). It must be noted that some of the RLC null results are driven by the lack of statistical power rather than the lack of effect. This is because the effect sizes are small, about 50–82 kg/ha (Panel (b) of Table 6 and Table A7, Appendix C in supplementary data at *ERAE* online), relative to MDE of 105 kg/ha for the ANCOVA model and 88 kg/ha for the DD model (Table A1, Appendix A in supplementary data at *ERAE* online). However, even where there is sufficient power to detect an RLC effect as the case for the ANCOVA model (Panel (A) of Table 6), the VDD treatment effects are significantly larger than the RLC effects by about 11–17 per cent—these effects survive multiple hypotheses test adjustments and are significant at the 0.05 and 0.001 levels, for the ITT and AST effects, respectively.

While the VDD effect sizes are not as large as the up to 200 per cent increase reported from natural science experiments in northern Ghana (Asei, Ewusi-Mensah and Abaidoo, 2015; Ulzen *et al.*, 2016), they are economically meaningful nonetheless—about \$206 (PPP) from the ANCOVA ITT model and \$232 (PPP) from the corresponding AST model.[8] The, perhaps, less than expected impact magnitude is not surprising because of the lower than anticipated inoculant uptake rate even among the treated group. Even in our sample, those who actually used the inoculants increased their yields by 51.3 per cent, on average, compared with only 6.5 per cent for those who did not use the inoculants at all.

4.5. Further discussion

One of the main aims of the intervention was to raise yield using the inoculants. If the observed yield effect is driven partly by the use of the inoculant and partly by the use of the improved seeds, then the fact that the RLC increased the use of improved seeds but not yield for most of the specifications may require further scrutiny beyond the lack of statistical power. If we simply regress yield on treatment status, inoculant use, improved seed use, district fixed effects and other factors picked up by the PDS lasso procedure, we see that the inoculants increased yield by about 158 kg/ha (p value = 0.000), but the effect associated with the improved seeds, about 39 kg/ha, is not significant at even the 0.10 level (p value = 0.268). Relatedly, aside the lack of statistical power, the fact

8 The yields are valued at village median producer price.

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that RLC households improved their modern legume seed uptake but did not change their behaviour vis-à-vis the use of the inoculants for most of the model specifications indicate that the ability to view the correct usage of the inoculants, which the VDD provided, was important. Thus, the videos seemed to be more effective in relating the process of inoculation and GAPs to the famers than the RLC did, all of which show up in the higher yields obtained by the VDD treatment group, all else being equal.

Aside the individual RI-based *p* values provided in all the regression tables, we also carried out RI-based joint tests of all treatment effects. That is, we test the following hypotheses: (i) the null that all the VDD treatment effects across the four outcomes jointly equal zero, (ii) the null that all the RLC treatment effects across the four outcomes jointly equal zero, (iii) the null of equality of all the VDD and RLC treatment effects across the four outcomes, and (iv) the null that all the treatment effects across the four outcomes jointly equal zero. We accomplish this following Young's (2019) omnibus RI-based test, which we implement using Young (2020). Table A8 (Appendix C in supplementary data at *ERAE* online) reports RI *p* value for Westfall-Young (Westfall and Young, 1993) multiple testing of statistical significance of all treatment effects. For all model specifications, hypothesis (i) is rejected at the 0.01 level, at worse; hypothesis (ii) is rejected in less than half of the model specifications, which in some cases reflect the lack of statistical power; hypothesis (iii) is rejected in 9 out of 12 cases (75 per cent of the time) and hypothesis (iv) is always rejected at the 0.001 level. These results suggest that when taken together, the VDD had statistically significant impact on the outcomes, but the RLC effect is not always so, and that the VDD treatment was more effective than the RLC treatment even in cases where the RLC null results are not driven by the lack of statistical power.

While the question of whether the average effects of the interventions conceal important variations in response to the VDD and RLC treatments across key characteristics is an important one, our study is generally underpowered for carrying out a meaningful analysis of impact heterogeneity. We use the ANCOVA models, which have relatively more power, to provide some exploratory analysis of impact heterogeneity by pre-treatment wealth, farm size and sex of farmer. The results (Table A9, Appendix C in supplementary data at *ERAE* online) show that most of the observable impact differences that are statistically significant at conventional levels relate to the uptake of the modern legume seeds (column 3). The impact of the VDD treatment on improved seeds uptake is significantly increasing with wealth and farm size. For example, the ITT effect of the VDD on uptake of the improved seed varieties among the poor (those living below \$1.90 a day) is 6 percentage points (p value $= 0.185$), but 25 percentage points among the non-poor (p value $= 0$); the 19 percentage point difference is statistically significant at the 0.01 level (RI-based p value $= 0.008$). Panel A of Figure A7 (Appendix C in supplementary data at *ERAE* online) shows the heterogeneous impact of the VDD intervention on modern legume seed uptake as pre-treatment wealth

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is increased. This result probably reflects the fact that farmers face multiple investment constraints, including binding liquidity (Karlan *et al.*, 2014; Shiferaw *et al.*, 2015). Panel B of Figure A7 (Appendix C in supplementary data at *ERAE* online) shows the farm size–associated heterogeneity of the VDD treatment. We observe, for instance, that the VDD effect is 8 percentage points (q value = 0.170) at the 10th percentile (mean of 0.28 ha) but rises to 10 percentage points at the median farm size (q value = 0.035) and then to 18 percentage points (q value = 0) at the 90th percentile; the 10 percentage point effect size difference between the 10th and the 90th percentiles is statistically significant at the 0.01 level (q value = 0.008).

We also find that both the VDD and RLC treatment responses to improved seed uptake are lower for women than for men. Figure 4 illustrates the gendered differences in response to treatment for the improved seed uptake outcome. For example, the VDD ITT effect on improved seeds uptake is only about 3 percentage points among female farmers (p value = 0.641) but 18 percentage points among male farmers (p value = 0); the RI-based p value associated with the approximately 16 percentage point difference is 0.018. Similarly, the RLC increased uptake of the modern varieties among male farmers by about 17 percentage points more than it did among female farmers (RI-based p value = 0.004).

The null results regarding the impact of treatment among female farmers may be due to the lack of power rather than the lack of effect—the MDEs among female farmers equal 17.1 and 13.1 percentage points for the VDD and RLC treatments, respectively; yet the impacts we observed are much lower than these. The lack of power notwithstanding, we observed during our process monitoring activities that although women are the key producers of legumes, some of those who participated in the programme were not the primary agricultural decision makers within their households. Yet, in some cases, their spouses did not participate in the programme.[9] These results call for further research to unravel, first, whether the econometric results hold under adequate statistical power, and, second, whether providing agricultural information to couples in such patriarchal societies engenders greater impact than when women alone are involved.

5. Robustness checks, cost-effectiveness and limitations

5.1. Are the treatment effects driven by the reported scarcity of inoculants?

If the reported scarcity of inoculants is not identical across the treatment assignments, the estimated treatment effects could be driven by the differing availability of the technology. We verify this in two ways. First, we compare the availability of agro-dealerships and distance to an agro dealer across the treatment arms. Second, we asked non-adopters directly during the endline

9 Most of the women live in male-headed households. Only 7.7 per cent of our sample were female-headed households.

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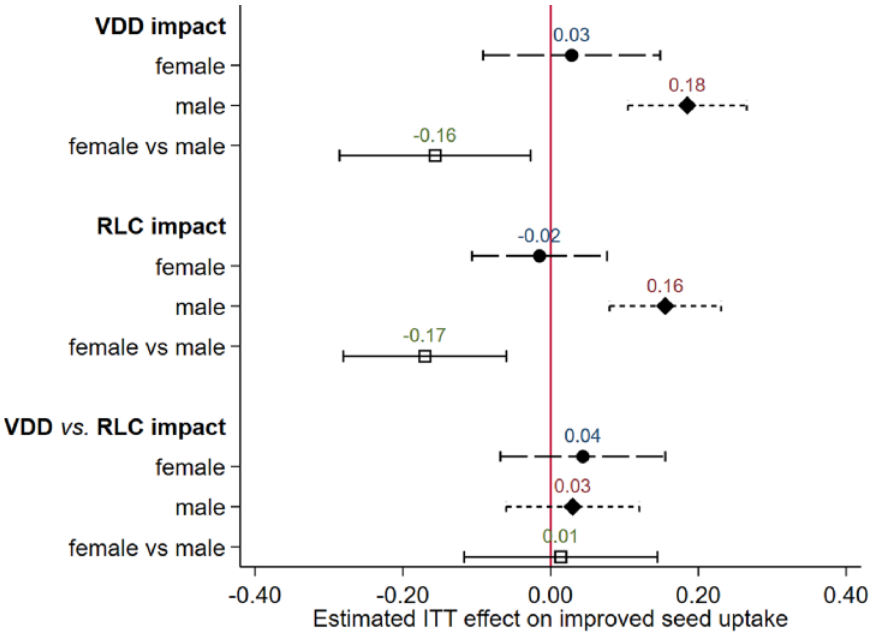

Fig. 4. Gender-associated heterogeneous impact on improved seed uptake.
Note: These are estimates of the average effects of the interventions together with their 95 per cent confidence intervals for females and males, as well as the associated impact differences between the genders. This was done by estimating $Seed_{ij1} = \alpha + \theta_1 VDD_j + \theta_2 RLC_j + \pi Female_{ij0} + \delta_1(VDD_j \times Female_{ij0}) + \delta_2(RLC_j \times Female_{ij0}) + \gamma X_{ij0} + \varepsilon_{ij1}$.. The impact of exposure to the VDD for females is given by $\delta_1 + \theta_1$and that for males is θ_1. Similarly, the impacts of exposure to the VDD for females and males are given by $\delta_2 + \theta_2$ and θ_2, respectively. The impact of exposure to the VDD versus RLC for females is given by $(\delta_1 + \theta_1) - (\delta_2 + \theta_2)$and that for males is $\theta_1 - \theta_2$. Calculating the impact differences between the two groups for each treatment exposure is straightforward from these. We do not correct for multiple testing here because these are considered exploratory only (Olken, 2015).

survey the primary reason why they did not use the technology and compared their responses across the treatment assignments.

Panels A and B of Figure A8 (Appendix C in supplementary data at *ERAE* online) are the graphs produced from regressing availability of agro-dealerships and distance to an agro dealer on the treatment assignments. In both cases, we cannot reject the nulls that both variables are identical across the treatment arms. Non-adopters provided four main reasons for not using the inoculants: ‘I could not find it’ (62 per cent), ‘I don’t know how to use it’ (13 per cent), ‘I prefer inorganic fertiliser’ (10 per cent) and ‘I can’t afford it’ (15 per cent). Figure A9 (Appendix C in supplementary data at *ERAE* online) plots these responses by the treatment assignments, showing no visible differences in reported scarcity across the treatments. More concretely, we regress the reported scarcity rate on treatment assignment, variables picked up by the PDS lasso procedure and district fixed effects. The results (Table A10, Appendix C in supplementary data at *ERAE* online) provide assurance that the

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reported treatment effects are not driven by differing scarcity of the inoculants across the treatment groups.

5.2. Cost-effectiveness

Detailed and quality cost data is essential for conducting cost-effectiveness analysis. Because we do not have such detailed data, what is provided here is only indicative. While reaching farmers through the VDD using the tricycle van may be more expensive than using mobile phones, for example (if farmers can access the videos via their own phones), an additional advantage of the video van is that, aside targeting FA members, other farmers in the communities also get exposed, which likely leads to 'spillover effects'; the design of our experiment does not allow us to evaluate this possibility, however. Aside the cost of producing the documentary itself, which could be considered as sunk cost, the cost of reaching farmers through the VDD using the tricycle video vans was approximately $452 per community (about $16,724 for the 37 communities). On the other hand, it costs approximately $339 to run a listening club (about $12,543 in total for the intervention communities). The VDD and RLC reached 5,780 and 952 farmers, respectively, which translate to an average per farmer cost of $2.89 and $13.17, respectively, excluding overhead cost.[10] The cost of the VDD treatment was thus much lower than that of the RLC, which is what one would expect (Abate *et al.*, 2019; Bello-Bravo, Olana and Pittendrigh, 2015).

Given the above, coupled with the fact the RLC had lower impact on legume yield than the VDD, it is self-evident that the VDD is more cost-effective than the RLC. In value terms, the estimated mean value of yields due to the VDD treatment was about $206 (PPP) per farmer compared with $39 for the RLC treatment. Although VDD treated farmers also spent about $4.29 and $2.27 more on inoculants and seeds per hectare, bringing the total extra cost of the VDD treatment to $9.45, these extra expenditures are low relative to the expected gains from adoption.[11] Taken together, and assuming that other production costs are identical across treatments, it seems evident that although both treatments yield benefit–cost ratios that are greater than unity (excluding overhead cost), the short-term impact of the VDD intervention was much higher than that of the RLC treatment. Besides, these estimates do not include spillover effects in the VDD communities.

5.3. Challenges and limitations

There are a number of challenges and limitations that should be kept in mind when interpreting the results reported in this article. We have highlighted most of these limitations throughout the article, but we provide a summary here. The first is the inoculant project implementation challenges that spilled over to the impact evaluation. The manufacturing of the inoculants, upon which

10 With the implementing agency being a Government of Ghana Subvented institution, an overhead cost of approximately 10–15 per cent of total cost can be added.

11 The average cost of inoculant was GHC 30/100 g sachet (or $21 PPP).

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the intervention hinged, delayed, leading to the scarcity of the technology. Yet, analyses of the impacts of the communication channels are based on the supposition that the technologies are available to all farmers. Although we showed in subsection 5.1 that the scarcity of the inoculants was identical across treatments, this limitation should be kept in mind.

The second challenge, which is closely related to the first, is that although enough agro-input dealers were georeferenced and linked to the project for the distribution of the inoculants, some of them were not keen on selling the product because of delicate storage and handling requirements such as storing the inoculants below 25°C to maintain the living bacteria and the efficacy of the product. The dealers considered this a high-risk venture in the presence of storage costs and unreliability of electricity supply in some of the communities. This could hamper availability at the community level even when the production challenges are overcome. However, our recent follow-ups suggest that the project implementers are finding innovative ways of storing the inoculants under the required temperature without the need for refrigeration; inoculants are also released to dealerships only during the growing season.

Third, most of our indicators are self-reported, and yet it has been shown that relying on self-reported data could lead to the overestimation of treatment effects due to social desirability bias (Fabregas *et al.*, 2019). Furthermore, such measures are prone to measurement error, with farmers' responses producing substantial false positives and false negatives (Wossen *et al.*, 2018). While we attempted remedying these by asking enumerators to verify, for example, by farmers showing the inoculant sachets, the impact of misclassification on our results is not known and cannot be ruled out, and so must be taken as a limitation.

Lastly, when it comes to ICT-based interventions, even small effect sizes could make a meaningful difference because of their relatively low cost (Fabregas, Kremer and Schilbach, 2019). Yet, such small effect sizes require larger samples or higher statistical power to be detected. The fact that our study is underpowered to detect such small but possibly meaningful effect sizes as shown by the ex post MDEs should be considered an important limitation of our study. This is an investigation of ongoing research, in addition to exploring the presence of spillover effects, particularly in the VDD communities.

6. Conclusion

Adequate information about a new technology is necessary, even if insufficient, for investments. Therefore, the question of which channel of information dissemination about a new technology is effective for boosting adoption and improving agricultural outcomes is important for both policy and practice. While a variety of agricultural extension communication methods are available, and there is a growing literature on the effectiveness of ICT-based channels of reaching smallholder farmers with technological information, there is still a relative paucity of knowledge about the relative effectiveness

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of alternative ICT-based approaches, particularly about the relative effectiveness of audio (radio) vis-à-vis audio-visual (video) channels of agricultural information dissemination. This article complements recent work such as Van Campenhout, Spielman and Lecoutere (2020) aimed at addressing this gap in knowledge by setting up a randomised controlled experiment in 113 farming communities in northern Ghana. By randomly assigning farming communities to receive information about a new technology (*Bradyrhizobium* inoculation), improved legume seeds and other good agronomic practices through video documentaries and radio listening clubs, this article provides lessons for policy makers and practitioners involved in agricultural information dissemination and advisory services.

Our results provide evidence that, for all the outcomes evaluated individually and together, the audiovisual (hearing and seeing) option that the video documentaries provided for communicating agricultural technology information was effective for technology uptake and improving yields. Unfortunately, however, for some of the outcomes evaluated, our study was not adequately powered statistically to detect the relatively small radio listening club effects. Nevertheless, for the outcomes and model specifications where there was adequate power to detect both the video and radio listening club effects, we observed that the audiovisual channel (video) was more effective for precipitating technology uptake and increasing crop productivity than the audio channel, in spite of the enhanced interactivity provided by the radio listening clubs. This finding is consistent with the findings in previous studies showing that audiovisuals have the potential to change attitude and behaviour (La Ferrara, Chong and Duryea, 2012; Van Campenhout, Spielman and Lecoutere, 2020). The outcome of *doing* is thus most effectively achieved by hearing and seeing. The fact that video captures information that could elude the naked eye even when seen in person but becomes clearer upon review helps influence behaviour. Even if not all the information contained in the video itself is new, hearing and seeing again helps bring to bear relevant knowledge, raises more interest and induces technology uptake. This is particularly so for a technology that is relatively complex, such as the inoculants that require careful handling and application. Indeed, considering all the outcomes together, Young (2019) omnibus randomisation-based inference test shows that while we can easily reject the null that viewing the video had no overall impact across all outcomes, we cannot reject the null of no listening club treatment effect across all outcomes in 50 per cent of the specifications tested, albeit some of the null results were due to the lack of statistical power.

Unfortunately, our results were also tempered by implementation challenges that delayed production and distribution of the inoculants. This challenge in itself is an important lesson for policy and practice, highlighting how project and programme impacts could be derailed due to poor implementation. Our cautious conclusion is that the video documentary intervention in particular has potential for scaling up. A larger sample that provides adequate statistical power as well as more (qualitative) research is required to unravel contexts within which the radio listening clubs could be more effective.

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Lastly, it is important to note that the assessment provided here only captures short-term impacts of the project; an enhanced experimental design over more than one follow-up survey is required to capture long-term and possible spillover effects of the intervention, and this is the focus of ongoing research.

Supplementary data

Supplementary data are available at *ERAE* online.

Conflict of interest statement

We (the authors of this article) certify that we have are not affiliated to the Alliance for a Green Revolution in Africa (AGRA), the institution that funded the inoculants project. The subject matter discussed in this article and the conclusions do not necessarily represent the views of AGRA or any institution; they are the authors' based on an objective analysis of the data collected. The usual caveats apply.

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