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Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

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Information frictions limit the adoption of new agricultural technologies in developing countries. Most public-sector interventions to eliminate these frictions target information directly at select farmers. We show that an information intervention targeted at private input suppliers increases farmer-level adoption by over 50 percent compared to this public-sector approach. These newly informed suppliers become more proactive in carrying the new variety, informing potential customers, and in increasing adoption by those most likely to benefit from the technology. They do so in a long-term perspective of reputation building and business development.

JEL Classification: O30, O13

Keywords: Technology adoption, Agriculture, Privatization, learning

Kyle Emerick - kyle.emerick@tufts.edu
Tufts University and CEPR

Alain de Janvry - alain@berkeley.edu
UC Berkeley

Elisabeth Sadoulet - esadoulet@berkeley.edu
UC Berkeley, FERDI Clermont-Ferrand France

Manzoor Dar - seedmanzoor.dar@gmail.com
None

Eleanor Wiseman - ewiseman@berkeley.edu
UC Berkeley

Private Input Suppliers as Information Agents for Technology Adoption in Agriculture*

Manzoor H. Dar†
AFC India Limited

Alain de Janvry‡
University of California, Berkeley

Kyle Emerick§
Tufts University and CEPR

Elisabeth Sadoulet¶
University of California, Berkeley

Eleanor Wiseman‖
University of California, Berkeley

December 18, 2020

Abstract

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†I-107A, 3rd Floor, Kirti Nagar, New Delhi -110015. Email: seedmanzoor.dar@gmail.com

‡Department of Agricultural and Resource Economics. 207 Giannini Hall, Berkeley, CA 94720. Email: alain@berkeley.edu

§Department of Economics. 8 Upper Campus Rd., Medford, MA 02155. Email: kyle.emerick@tufts.edu

¶Department of Agricultural and Resource Economics. 207 Giannini Hall, Berkeley, CA 94720. Email: esadoulet@berkeley.edu

‖Department of Agricultural and Resource Economics. 207 Giannini Hall, Berkeley, CA 94720. Email: ewiseman@berkeley.edu

1 Introduction

A large share of the population in poor countries relies on agriculture for their livelihoods. But they most often use traditional technologies, despite the existence of more modern alternatives. This puzzle has sparked extensive research to understand which barriers constrain technology adoption. Researchers have focused most particularly on credit and insurance markets failures and on information frictions (Feder, Just, and Zilberman, 1985; Magruder, 2018). Focusing on the latter, decades of research show that knowledge indeed plays an important role for adoption (Griliches, 1957; Conley and Udry, 2010; Cole and Fernando, 2020; Fabregas, Kremer, and Schilbach, 2019; Gupta, Ponticelli, and Tesei, 2020).

Recognizing this, governments have invested heavily in dispatching agricultural extension workers throughout rural areas to transmit information from scientists to selected farmers — with the hope that information will then flow to others via social networks. But these public-sector efforts are widely recognized as having had limited impact (Farrington, 1995; Anderson and Feder, 2007). In advanced economies, by contrast, private businesses have become the main source of information about technological innovations for potential clients. Farmers rely on commercial input dealers not only to purchase inputs but also to be informed about available technological options. These dealers are in turn informed by the companies whose products they sell (Fites, 1996) and by public agricultural agencies and research institutions (Wolf, Just, and Zilberman, 2001). In developing countries, could there similarly be a greater role for private business-motivated entrepreneurs in the public agricultural extension system? The optimal boundary between government and private entities in the provision of public services is an extensively studied topic in economics (Hart, Shleifer, and Vishny, 1997; Besley and Ghatak, 2001). Empirical research has found that outsourcing public service delivery to private firms can improve provision in many sectors such as potable water, health, food distribution, and education (Galiani, Gertler, and Schargrodsky, 2005; Loevinsohn and Harding, 2005; Banerjee et al., 2019; Romero, Sandefur, and Sandholtz, 2020). Unlike these sectors, developing country agriculture has yet to see a large role for private business in delivering public services. Agriculture thus offers an opportunity to study whether private input suppliers can internalize benefits from the spread of information and become agents in the delivery of public services.

Our experiment provides information on new technologies to local input suppliers (agrodealers), rather than to selected farmers as in the business-as-usual approach. Specifically, we give agrodealers access to a new seed variety for their own learning. Taking this business-oriented approach to agricultural extension could influence farm-level adoption in different ways. On the one hand, input suppliers — unlike contact farmers — have economic incentives to spread information. They stand to reap profits when suggesting the use of new inputs to farmers. Repeated interactions with farmers over time may discipline them into providing high-quality recommendations benefiting their clients.[1] On the other hand, agrodealers might purposefully under-provide quality (Bold et al., 2017; Ashour et al., 2019). Or they may recommend products that maximize their own profits instead of customer welfare, as has been observed in numerous other sectors (Hubbard, 1998; Inderst and Ottaviani, 2009; Mullainathan, Noeth, and Schoar, 2012; Chen, Gertler, and Yang, 2016; Anagol, Cole, and Sarkar, 2017). These concerns about securing through agrodealers adoption of the right technologies by the right farmers are akin to concerns about the quality of public services provided by the private sector (Hart, Shleifer, and Vishny, 1997). Therefore, our analysis seeks to answer both whether informed agrodealers increase adoption and whether they do so for the farmers that stand to benefit the most from the innovation.

We study these questions with an experiment where information services are provided to local seed dealers. Our sample consists of 72 blocks, spread across 10 districts in the Indian state of Odisha.[2] We consider the dissemination of Swarna-Sub1, a new and profitable flood-tolerant rice variety that reduces the damage caused by flash flooding.[3] We partnered with the government extension service to support their conventional activities in 36 control blocks. This included providing seed minikits to the contact farmers on whom they rely to use the new technologies and spread information about them in their social networks. It also involved carrying out large-scale "cluster" demonstrations where many farmers grow the new variety on contiguous pieces of land, and organizing farmer field days to share results from demonstrations. These are all activities the government extension service would do with adequate resources, but we supported them as part of the experiment to make sure that they were carried out and that our control group reflects business-as-usual activities at their best.

We provided the exact same quantity of seeds and the same information to input

[1]Targeting information to a small set of agrodealers is also less costly than trying to reach hundreds of individual farmers. A potential downside is that markets for agricultural inputs in developing countries can be sparse. Aggarwal et al. (2018) show that travel costs to input suppliers play an important role in technology adoption for African smallholders. If these costs are too high, then fewer farmers will have contact with dealers and solving information frictions on the supply side could be less effective.

[2]Blocks are the relevant administrative units for agricultural extension. Blocks in the experiment have an average of 136 villages, and each block has its own local agricultural extension office.

[3]Previous work shows that this innovation is profitable for farmers. By reducing risk, it induces them to invest more in early-season inputs. Notably, it has no yield penalty in normal years (Emerick et al., 2016).

dealers in the 36 treatment blocks. We did not support any conventional extension activities as was done in the control blocks. These dealers are highly local, small-scale businesses, selling seeds and sometimes other inputs such as agro-chemicals. They were free to choose how to use the demonstration seeds. The key distinction between this treatment and the standard mode of agricultural extension (the control) is that information constraints are being relaxed on the supply side, rather than on the demand side for new technology. The treatment tries to leverage the natural incentives created by the marketplace for private businesses to transmit information to their clients.

Turning to the results one year later, we find that the dealer-based approach to the diffusion of information increases adoption of Swarna-Sub1 — the variety being introduced — by over 56 percent, i.e. from 6.3 to 9.8 percent of farmers. We further find that the average farmer in the treatment blocks is cultivating 69 percent more land with the variety, and the cultivated area of adopters increases on the intensive margin by about 9 percent.

Consistent with these results on farm-level adoption, we find that the treatment triggered a supply-side response. The magnitude of this response is similar to the increase in adoption. By the 2018 season, two years after the new seeds had been introduced, dealers in treatment blocks were about 59 percent more likely to have Swarna-Sub1 in stock. There is also some evidence that informing agrodealers causes a change in local input production. Treatment blocks were producing 40-50 percent more Swarna-Sub1 seeds during the three years after the intervention.

An immediate next question is whether farmers induced to adopt in the dealer treatment are those with relatively higher expected returns. Dealers may invest little in promoting a particular input to the right farmers if something else provides them higher profit margins. Dealers in our setting principally sell seeds that are produced by the state-run seed corporation — that fixes both wholesale and retail prices equally for all seed varieties. This aspect of our setting eliminates a differential price motivation for dealers promoting the new technology. We consider a different possibility where dealers might encourage adoption by farmers with the greatest potential benefits if they want to maintain a good reputation as providers of high-quality advice.

To consider this, we test for treatment-effect heterogeneity according to past flood exposure — an important determinant of returns.[4] We find that the treatment effects of dealer-based extension are largest for households that are expected to have the highest returns. We would not expect this type of heterogeneity if dealers were sharing informa-

[4]Flood tolerance is the key attribute of Swarna-Sub1. The technology provides higher yields when crops are flooded relative to other types of rice grown by farmers (Xu et al., 2006).

tion indiscriminately or without considering the possible benefits for farmers. This offers a first piece of evidence that dealers are concerned with their reputation when sharing information.

We then explore whether dealers proactively promote the new variety and what might explain their behavior. For this, we sent "secret shoppers" to around 300 dealers to inquire about new rice varieties in the third season of the study — two years after the seeds were introduced. We find direct evidence that the treatment changes what dealers say to farmers coming into their shops. Dealers in treatment blocks are about 25 percent more likely to mention Swarna-Sub1 when listing the new varieties to consider. When specifically asked for a recommendation, we find that dealers are less likely to recommend older types of seeds and in some cases more likely to recommend trying Swarna-Sub1. These findings suggest that relaxing information constraints on dealers leads them to share this information with farmers.

As a final piece of evidence, we ran an experiment to test whether business incentives and reputational concerns motivate dealers to spread information. In partnership with a local NGO, we revisited dealers during the fourth season (2019) to further solicit recommendations. Each dealer was randomized into one of two treatments. In the first treatment, someone visited the dealer and asked which farmers, locations, and varieties would be best for a demonstration where farmers would cultivate a new variety and then the NGO would organize a meeting with other villagers to explain its attributes. Importantly, the name of the dealer giving the recommendation would be advertised during the meeting. We refer to this treatment as the reputation treatment because the dealer's identity would be publicly displayed as part of the demonstration. In the second treatment, the program was presented as one where the NGO would not name the dealer and would collect the harvest after the demonstration and redistribute it as seeds to other farmers. This treatment reduces the salience of reputation for the dealer and lowers their business motivation by reducing the demand that would be created by the demonstration.

We find that priming dealers to consider their reputation and business motivation changes the advice they give. In particular, it causes them to suggest different locations, types of farmers, and seed varieties. Starting with location, when presented with a candidate list of villages for the program, dealers in the reputation treatment are more likely to suggest a village not on that list. Most of this is explained by treatment dealers being more likely to suggest their own villages. Dealers in the reputation treatment also spend more time thinking of which farmers to recommend. They are more likely to suggest neighbors or other people in their own village. Finally, the reputation treatment causes dealers to recommend less common seed varieties. Taken together, these findings suggest that business interests and reputational concerns play a role in the information sharing process for dealers.[5] These results help interpret the findings in our main experiment. Concerns over market share and reputation could be factors causing dealers to promote an improved seed — even if it does not provide them with higher profit margins.

In sum, we present the first evidence on how leveraging private agents on the supply side of input markets can help disseminate agricultural technology more effectively than the conventional methods used by the public sector. An important channel through which this happens is that dealers, when they gain information, spread it actively to farmers. Importantly, the gains from leveraging agrodealers are largest in areas where the technology delivers the highest benefits to farmers. Reputational concerns may be one of the factors that motivates dealers to match the technology to farmers with the highest expected returns. These results contribute to the literature on public-private partnerships by showing how using private-sector input suppliers to spread knowledge can improve the delivery of public services.[6] Moreover, it does so at a lower cost compared to expanding the current practices used in the public sector. It is cheaper to visit a small handful of dealers compared to carrying out large-scale demonstrations and training.

Our findings contribute to the literature on agricultural technology adoption in the developing world.[7] Several studies consider different methods to improve the provision of extension services. For instance, cell-phone-based monitoring of extension workers can help solve agency problems and increase the number of farmers reached (Dal Bó et al., 2020). Alternatively, researchers have considered efforts to better select, incentivize, and train the farmers that the extension service partners with (Beaman et al., 2020; BenYishay and Mobarak, 2018; Kondylis, Mueller, and Zhu, 2017; Beaman and Dillon, 2018; BenYishay et al., 2020). Finally, field days to share information from demonstrations is a commonly used technique (Maertens, Michelson, and Nourani, 2020; Emerick and Dar, 2020).

This literature has focused on ways of reaching more farmers via the traditional channels — either through direct outreach or by learning from the contact farmers that were trained by extension workers. None of these studies consider whether engaging the supply side of the market can increase adoption by farmers. Survey data in agricul-

[5] The importance of reputational concerns for suppliers has been studied extensively in other contexts (Banerjee and Duflo, 2000; List, 2006; Bai, 2018).

[6] Our intervention stands apart from fully outsourcing public service delivery — which is the topic most commonly debated and studied in the literature. Instead, we experiment with adopting a new strategy that engages the private sector in knowledge dissemination as a complement to public-sector programs.

[7] de Janvry, Sadoulet, and Suri (2017), Magruder (2018), and Macours (2019) all offer reviews of this literature. It has focused on finding solutions to unlock the credit, risk, information, and other external constraints that farmers face.

ture suggest that input suppliers act as the second most popular source of information for Indian farmers (Glendenning et al., 2010). Informing private input suppliers about technological benefits is one way to take advantage of their intrinsic motivation to spread information. Yet, it has not been looked at as source of potential information agents with well-aligned incentives in promoting the adoption of new technologies. Our paper is the first to implement and test this as a new approach to doing agricultural extension.[8]

The rest of this paper is organized as follows. Section 2 gives more background information on the setting and outlines the experiment. Section 3 describes the data collection. Section 4 presents the main results on how targeting agricultural extension to input dealers increases technology adoption by farmers, particularly those with the highest potential benefit. Section 5 turns to analyzing a potential explanation for this result. Particularly, it focuses on whether dealers spread information to their customers and what motivates them to do so. Section 6 concludes.

2 Background and Design of Main Experiment

This section starts by providing background information on the standard methods used in agricultural extension. It also gives a description of how the public sector delivers information to farmers in our particular study area. We then outline the design of our main experiment to compare these standard methods with the more business-oriented approach of using agrodealers as information agents.

2.1 Public-Sector Agricultural Extension

Governments all over the world support agricultural extension services as a mode of information delivery. Ministries of agriculture typically have entire departments dedicated to providing these services. These departments oversee local administrative offices that hire frontline extension agents whose role is to diffuse information about new agricultural technologies and practices to farmers. The specific techniques used by agents vary across contexts, but the basic methods are largely consistent, especially in poor countries. Agents usually work with selected "contact farmers" who are keen on trying new approaches and are ideally able to transmit knowledge to others in their social networks. They also organize farmer field days with cluster demonstration plots, where new seeds are grown by multiple farmers, to boost the diffusion of information.

[8] As such, our work is related to Maitra et al. (2020) who show that agricultural credit is more beneficial when private traders select loan recipients — perhaps due to incentive effects where private traders benefit themselves when loans cause farmers to harvest more output.

The public sector provides agricultural extension services for at least two reasons. First, markets do not exist for many new innovations. For instance, a new planting method may only be promoted by government agents because there is no scope for profiting from its sale in private markets. This contrasts with new seed varieties or material inputs that are produced and sometimes marketed by private firms. Second, many agricultural innovations are not developed, and hence not marketed, by private firms. Public entities such as national agricultural research systems or international research organizations frequently develop new seeds, inputs, and agricultural management practices. The public extension service then transmits information about these developments to farmers.

In the context of our experiment, agricultural extension workers use many of these standard techniques. Each of the 10 districts in the sample is organized into blocks, where a block has an average of 135 villages. Each block has an agricultural office that is led by a Block Agricultural Officer (BAO). The BAO employs Assistant Agricultural Officers (AAO) and Village Agricultural Workers (VAW) who work in the field with farmers.

2.2 Experiment on Dealer-Based Extension

Our sample consists of 72 blocks in 10 flood-prone districts of Odisha.[9] We selected these areas because the technology being promoted — a flood-tolerant rice variety called Swarna-Sub1 — is most suitable for flood-affected areas.[10] The blocks in the sample represent around 20 percent of the blocks in the state.

We randomly assigned 36 of these blocks to the treatment group where agrodealers were targeted to receive seeds and information. This randomization was stratified by district. The remaining 36 blocks serve as a comparison group where we supported the government extension service to carry out normal extension activities.

Figure 1 displays the timeline of these interventions. Starting in May 2016 — about 6-8 weeks before planting time — we partnered with the government's extension service to introduce Swarna-Sub1 into control blocks. We did this in a way that mirrors three common practices in agricultural extension. First, field staff provided 10 seed minikits of 5 kilograms each to the BAO, who then helped identify contact farmers to use the kits. The BAO chose 2 villages and 5 farmers in each village. Each kit contained only seeds for testing and some basic information about Swarna-Sub1. Our field staff then delivered the kits to the recommended farmers. Second, we provided another 150 kg of seeds to

[9]The districts are Bhadrak, Balasore, Cuttack, Ganjam, Kendrapara, Khorda, Jagatsinghpur, Jajpur, Nayagarh, and Puri.

[10]The technology has been shown to benefit farmers by reducing both yield losses when flooding occurs and downside risk in any year, thereby increasing investment (Dar et al., 2013; Emerick et al., 2016).

the BAO so that he could set up a cluster demonstration where the seeds would be used by several farmers on a contiguous set of plots. Based on seeding rates in the region, 150 kg allows for cultivation of 5-10 acres. The BAO chose where to do the demonstration and which farmers to target. Official government guidelines for organizing these clusters suggest that they be carried out in sites that are easily accessible to be viewed by many farmers. Moreover, sites should be representative of average conditions in the area. Third, we helped the BAO carry out a farmer field day in November — at the time right before harvest. The BAO selected the location of the field day and whom to invite. The purpose of the field day was for extension staff to train farmers about Swarna-Sub1, share information from the demonstrations, and hope that information will spread throughout the block.

The objective of such an active control group is twofold. First, it ensures that each block is endowed with the same quantity of seeds. Therefore, the dealer-based treatment only differs on *who* received the new seeds and information. Second, the demonstrations and partnerships with contact farmers may not have taken place without our involvement. Forcing these activities to happen makes the treatment-control comparison more meaningful. Most importantly, it sets a higher bar for the dealer-based treatment by eliminating any possibility that the new technology would not be promoted by the government extension service.

Turning to the 36 treatment blocks, we obtained a list of 2,087 seed suppliers from the state Department of Agriculture. These include suppliers of two types: private seed dealers and Primary Agricultural Cooperative Societies (PACS). PACS are farmer groups that handle credit, seed supply, and procurement of output for farmers. We did not include them in the intervention because their incentives are not the same as those of private dealers. Seed sales are usually handled by a member that is not the residual claimant on any profits from the sale. Despite being fewer in number relative to PACS, private dealers account for almost 60 percent of the seeds sold to farmers. The sample consists of 666 private dealers, 327 of which were located in the treatment blocks.

Armed with this list, our field staff entered each treatment block and located five dealers interested in receiving seed minikits and an informational pamphlet about Swarna-Sub1. In some blocks fewer than 5 dealers were available. We provided additional seed to each dealer in these cases to guarantee that a full 200 kilograms (the same amount as control blocks) were introduced. The list provided by the Department of Agriculture did not have enough locatable dealers in some cases. In these circumstances, our field staff provided the seeds to other local agrodealers.[11] Overall, seeds and information were pro-

[11]The list of 666 dealers includes only those that are registered with the state, a prerequisite for selling

vided to 151 dealers across the 36 treatment blocks.[12] 119 of these were from the original list.

Once provided with seeds and information, the dealers were left alone to decide how to use them. We asked dealers about their intended uses. They overwhelmingly stated that they would use the seeds for testing on their own farms and would provide them to good customers for testing.[13] Our intervention did not include any additional assistance to dealers. This differs from standard methods in agricultural extension where agents continually revisit their contact farmers. We allowed dealers to learn on their own because in theory they should be motivated to learn about a new product that could enhance their business. The goal of our treatment is to measure whether this motivation causes information to flow to farmers and ultimately increases adoption. Not intervening further ensures that our treatment effect is driven by any real-world incentives dealers have to learn, rather than heavy monitoring by our partners.

Dealers in our sample are small business entrepreneurs. Some operate out of their homes, while others maintain shops in rural towns. 44 percent of dealers sell only seeds, with fertilizers and pesticides being the most common inputs sold by the other dealers. Dealers are highly local. The median dealer sells enough rice seed to cover roughly 400 acres, which amounts to the rice area cultivated by 150 farmers.[14] Importantly, dealers tend to serve the same customers from year to year.

Another important feature of our context is that 84% of the seeds sold by dealers in our sample are produced by the state-run Odisha State Seed Corporation (OSSC). Government subsidies explain this. Seeds produced by the state are subsidized at a rate of approximately 40 percent. No subsidies exist for seeds produced by private companies. As licensed agents, dealers receive a fixed commission that amounts to about 8 percent of the pre-subsidy price. All varieties have the same final price for farmers. Thus, the margin for dealers is the same across all types of varieties. Hence dealers have no direct financial incentive to sell one variety over another.

Turning to the second season (2017), we ran an SMS messaging experiment to compare our intervention with this "lighter touch" information treatment. The random de-

seeds from the state seed corporation. The dealers not included in our list could have been in the process of renewing their license or only selling seeds produced by private companies.

[12]Two dealers in one control block were provided seeds by mistake. All our analysis uses only the original random treatment assignment.

[13]Around 83 percent of dealers indicated they would try some of the seeds on their own, while 63 percent indicated that some of the seeds would be provided to their good customers. Other less common responses were to provide them to family members (9 percent) and friends (24 percent).

[14]A dealer's actual customer base is larger than this for two reasons. First, rice farmers do not buy new seed every year. It is common to reuse seeds for several years before replacing it. Second, farmers use new seed for only a portion of their land each year.

livery of SMS messages allows us to test whether our dealer treatment substitutes (or complements) basic knowledge that can be easily transmitted via ICT technology. Furthermore, it allows us to compare the direct effects of the two approaches.

The messaging was simple. It informed farmers that Swarna-Sub1 is a new variety that is suitable for medium-low land in terms of elevation, matures in 145 days, and can tolerate up to two weeks of flooding. The message also stated that it was being produced by OSSC and could be available at local dealers. As a sampling frame, we obtained mobile numbers for 75,616 farmers that had registered for the state government's Direct Benefit Transfer (DBT) scheme to obtain seed subsidies.[15] These farmers are located across the 261 gram panchayats that cover our main estimation sample, as outlined below. The SMS treatment was randomized at the gram panchayat level, resulting in messages being delivered to 37,783 of the names on the list.

3 Data Collection for Main Experiment

This section describes the experimental data for testing if involving agrodealers in the diffusion of information increases adoption by farmers. It also discusses the satellite data used to test whether this treatment led to greater adoption by farmers with higher expected benefits from using the technology. We focus only on the data from the first two years of the study. We save the discussion on the additional data and the experiment on mechanisms for Section 5.

3.1 Survey on farmer technology adoption

We anticipated that dealers and contact farmers would use the demonstration minikits for learning in 2016 and any possible treatment effects could first be detected during year two (the 2017 season). Our main followup survey therefore took place in August-September 2017 — around 15 months after the interventions. Its purpose was to measure adoption of seed varieties by rice farmers. To minimize measurement error, we timed the survey to be right after planting.

Our sample consists of 7,200 farmers. These farmers were drawn from a random sample of 261 gram panchayats — an administrative unit usually consisting of around 8

[15]Beginning in 2016 the state government started providing seed subsidies in the form of payments back to farmers. Farmers were required to register, provide bank account details, and pay the full price at the time of seed purchase. The subsidy was then credited to their bank account after the transaction details had been entered into a mobile phone app by the seed dealer.

villages.[16] Before drawing this sample, we excluded gram panchayats that had any village within 1.5 kilometers of the block boundary.[17] We removed these areas to reduce any interference caused by farmers possibly obtaining seeds from other blocks. The 261 sample gram panchayats had 75,616 farmers registered for the DBT program for seed subsidies. Using this database as a sampling frame, we randomly drew 100 farmers from each block (amongst the sample gram panchayats). These farmers are spread across 1,333 villages. Figure A1 shows their geographic dispersion across the 10 districts in the experiment.

Survey teams succeeded in locating and surveying 6,653 (92 percent) of the farmers. Of these, 93 percent were currently cultivating rice. Table A1 shows no significant differences in the probabilities of being surveyed or growing rice between treatment and control groups.

The survey focused on which seed varieties were currently being used for rice cultivation. Surveyors went through a list of 30 varieties and asked farmers which ones they were currently using and the amount of land being grown.[18] In addition to these adoption data, we obtained information on contacts with agricultural extension agents during the last year, topics discussed during these conversations, whether the farmer had seen any seed demonstrations, and whether they had recently learned about Swarna-Sub1.

3.2 Data on supply responses

Any treatment effects on farmer-level uptake might occur simultaneously with supply responses by dealers.[19] To measure this, we surveyed seed dealers around the same time as the farmer survey. We timed the survey to be in September so that seed purchases would be recently completed and easier to recall for dealers. Dealers were asked which varieties they carried for the 2017 season, how much of each was sold, and whether they were selling seeds from private companies or from the state's seed corporation.

Our sample consists of 613 dealers from the list of dealers obtained prior to the experiment.[20] A large fraction could not be located or were no longer selling rice seeds.

[16] We limited our data collection to a sample of gram panchayats to lower transportation costs for survey teams. The gram panchayats were identified using the 2011 Population Census of India.

[17] Approximately 17.5 percent of the villages across the 72 blocks are within 1.5 km of another village in a different block.

[18] Swarna-Sub1 — the variety introduced in the treatments — was 24th in this ordering. Asking about uptake in this way makes it less likely that responses reflect experimenter demands. Furthermore, farmers surveyed were not informed about the interventions that were carried out in their block a year earlier.

[19] This need not be the case if there was already excess capacity of Swarna-Sub1 seeds or if farmers obtain seeds outside of formal markets, such as from friends or relatives.

[20] There were 53 dealers on our list of 666 that had no contact details and thus we did not attempt to locate them.

Specifically, 22.8 percent of them could not be reached. Of the 473 dealers located, 274 (58 percent) were selling rice seeds in the 2017 season. In results that follow, we show effects both for all dealers that were reached and those that remained in the seed business. Table A2 shows that the likelihood of being located and the probably of selling rice seeds during the 2017 season are uncorrelated with treatment. Focusing on the treatment blocks, about 42 percent of the dealers surveyed received the intervention.

In addition to these dealer sales, we obtained data on the physical location of seed production. Seeds are grown by registered farmers that contract with the state to produce seeds that meet minimum certification standards. OSSC then collects, processes, and bags these seeds before selling them to farmers (via dealers and cooperatives) during the next season. The average block in our study had 32 seed growers per season from 2014 to 2019. We use records from a publicly available database that gives the location of each seed grower, the contracted area, the variety they produced, and the amount that was collected and processed.

Seed growers tend to be large farmers. They have incentives to produce the most profitable varieties for their land — just like farmers.[21] As such, their production of a new variety depends on them being convinced of its potential. We therefore aggregate seed production at the block-season level and estimate the effect of the dealer treatment on the amount of Swarna-Sub1 produced in the block.

3.3 Flooding exposure for individual farmers

Returning to farmer-level information, we use remote sensing data to approximate flooding risk. These data help us predict which farmers are expected to benefit the most from Swarna-Sub1. Being able to observe a key determinant of returns makes it possible to test for heterogeneous treatment effects according to a proxy for predicted benefits. More simply, is there a tradeoff between intervening with private-sector agents and a technology reaching the right people? Or, does involving input suppliers in the diffusion of information cause technology to diffuse to high-return individuals?

We have GPS coordinates of the houses for 83 percent of the farmers that we surveyed in 2017.[22] These coordinates are matched to daily images of flooded areas from June to October for the period 2011 to 2017. We consider a household as exposed to flooding on a given day if their house is within one kilometer of any flooded area.[23] We then

[21] The contracts with OSSC are on an acreage basis. OSSC and the grower agree on the variety and OSSC purchases the output at a pre-determined price. The grower pays for all the inputs.

[22] The likelihood of missing GPS coordinates is uncorrelated with treatment. A regression of observing GPS coordinates on treatment has a coefficient estimate of -.018 and a t-statistic of 0.4.

[23] A different study in one of the same districts collected GPS coordinates of both houses and rice plots

aggregate the total number of days of flood exposure across the 7 years as a measure of flooding risk — and hence as a proxy for the return to Swarna-Sub1.

The online appendix shows three characteristics of this variable. First, it varies substantially across the sample (Figure A3). About 30 percent of households were not exposed to flooding. In contrast, 10 percent of households had flooding for 40 days or more. Second, this variation is partly driven by geographic characteristics. Particularly, Figure A4 shows that flooding is more frequent in lower-elevation areas that are closer to rivers. These correlations provide verification that our measure at least partly reflects underlying determinants of flooding risk — not just recent flood shocks. Third, farmers exposed to more flooding tend to be smaller, poorer, and belong to low-caste social groups (Table A3).

3.4 Descriptive Statistics

Table 1 shows descriptive statistics and verifies that we have a balanced randomization. Panel A shows block-level characteristics, derived mostly from the 2011 Census. Most notably, the blocks have around 136 villages and an average population of 110,000. Beyond these, treatment and control blocks look similar on a number of other characteristics, including local Swarna-Sub1 seed production, caste distribution of the population, and elevation.

Panel B shows characteristics of the respondents from our 2017 survey. These characteristics were collected after the treatment, but are time invariant. Observables are mostly balanced for this sample that we use to estimate our main regressions.

4 Results of Dealer Extension Experiment

This section presents the results of the agrodealer experiment. After outlining the estimation strategy in Section 4.1, Section 4.2 shows that using dealers as information agents increases adoption by farmers. This finding is robust to different ways of measuring adoption and to including a battery of control variables. Section 4.3 tests whether this treatment effect varies by exposure to flooding risk — which is highly correlated with expected returns. We turn to effects on the supply side in Section 4.4. Particularly, we show effects on both dealer-level seed inventories and block-level production of Swarna-Sub1 seeds.

(Emerick and Dar, 2020). These data show that rice plots are within one kilometer of the household almost 90 percent of the time (Figure A2).

4.1 Estimation

Our main analysis consists of farmer- or dealer-level regressions of outcomes on the block-level treatment indicator:

$$y_{ibd} = \beta * Treatment_{bd} + \alpha_d + \varepsilon_{ibd}, \tag{1}$$

where i indexes farmers (or dealers), b indexes blocks, and d indexes districts. We include district fixed effects in all specifications because the dealer treatment was stratified by districts. We cluster all standard errors by block. The analysis uses only the random variation we generated, but the online appendix shows that our results are robust to controlling for the covariates in Table 1.

4.2 Effect on Technology Adoption

Informing private input dealers and providing them with seeds to test leads to greater adoption by farmers when compared to conventional extension approaches used by the public sector. Table 2 shows this result. Starting with Column 1, farmers in treatment blocks are 3.5 percentage points more likely to have adopted Swarna-Sub1 a year after the treatment, compared to farmers in control blocks. Given an adoption rate of 6.3% in the control group, this implies the treatment leads to a 56% increase in uptake. The treatment also caused acreage cultivated to increase: farmers in treatment blocks planted an average of 0.06 more acres with Swarna-Sub1 compared to farmers in control blocks, a 69% increase (Column 2). This adoption effect operates on both the extensive and intensive margins: private agrodealers also caused cultivated area of adopters to increase. Focusing specifically on the 329 adopters in treatment blocks, they cultivated 10% more of their land with Swarna-Sub1 compared to the 210 adopters in control blocks (Column 3). Table A4 shows that decomposing the intensive and extensive margins more formally with a tobit model leads to the same conclusions.

Our specifications in Table 2 use only the random variation created in the experiment. Table A5 verifies that controlling for the large set of covariates included in the balance test does not change the result. The point estimates stay similar when including these additional explanatory variables.

Table A6 shows that the level of contact with extension agents or with cluster demonstrations is very low, even with our reinforced extension service in control blocks and that farmers in treatment blocks were no less likely to be in contact with extensions workers, or to have observed a demonstration of Swarna-Sub1, compared to control farmers.[24] In other words, we do not find evidence of displacement at the expense of other traditional channels.

Following up on the idea of displacement, we look at whether the treatment displaced other new varieties, potentially lowering welfare if it caused a shift away from high-quality seeds. We find no such evidence. Table A7 shows that the treatment had a negative effect on adoption of only two seed varieties — both of which were released over three decades ago. It does not appear that the increase in adoption caused by agrodealers corresponds to a shift away from newly released technologies.

Finally, we find no evidence that the SMS messages increased adoption (Table A8). They also did not change the effectiveness of the dealer treatment. The adoption gains from the dealer treatment cannot be obtained with a "lighter touch" SMS messaging intervention, at least in our context.

4.3 Heterogeneity

The evidence on average adoption rates shows that helping private agrodealers learn is more effective than conventional approaches used in the public sector. A concern may be that, as private agents, dealers optimize behavior based on their own expected sales and profits; in contrast with government extension agents who can factor in equity and may be better at targeting farmers who have high expected returns to adoption. It is however not obvious whether profit maximizing dealers will deliver inferior targeting. Profit maximization strategies and farmers benefiting from adoption could coincide and may lead to similar outcomes, especially if we consider the repeated interactions between dealers and farmers over time.

In our context, being exposed to frequent flooding gives an easy-to-observe measure of potential returns — given the flood tolerance property of the variety.[25] We show that treatment dealers were successful at targeting Swarna-Sub1 to farmers who could benefit the most from the new technology, i.e. farmers who live in flood prone areas.

Figure 2 separates the sample by the satellite-based measure of past flooding and shows that treatment effects only exist in approximately half the sample where there were at least 3 flood days from 2011 to 2017. Conversely, the dealer treatment had little or no

[24] Only 5.7% of farmers report contact with the agricultural extension worker during the last year. This number is in line with other studies that showed low levels of contact between extension workers and farmers.

[25] Our analysis focuses on flooding risk as one determinant of returns because it is measurable in our data. However, we acknowledge that the actual benefits to a given farmer depend on a number of factors, some of which are harder to observe, such as risk aversion.

effect on adoption in the bottom half of the sample.

In Table 3, we show how the treatment effect is heterogeneous based on the number of flood days. Two results stand out in the table. First, control farmers who live in flood prone areas are less likely to adopt Swarna-Sub1. This negative relationship is true whether flood risk is measured in days of flooding (Column 1) or as a binary variable separating the sample into high- and low-risk farmers based on the median number of flood days (Column 2). Indeed, being a high-risk control farmer is associated with a 6% lower likelihood of adoption compared to low-risk control farmers. But it is important to emphasize that this estimate is merely a correlation. Farmers exposed to flooding are different in a number of ways that might directly influence adoption. Second, and more importantly, the dealer treatment was only effective in flood-prone areas, i.e. the interaction between treatment and flooding exposure is positive. The interaction term in Column 1 is less precise, likely because the heterogeneity in Figure 2 did not appear to be linear. But Column 2, which corresponds most closely with the figure, shows that the dealer treatment targets high-risk farmers increasing their adoption by 6.4%, while the effect of the treatment is only 0.8% for low-risk farmers (and not significant). The difference between the two treatment effects (the interaction term) is statistically significant at the 10% level. As another piece of evidence, Table A9 shows that the average adopter in treatment blocks is more exposed to flooding. Specifically, they are more than twice as likely to be above the median in terms of flood exposure.[26]

There is no evidence that informing dealers prioritizes adoption by the wealthiest farmers, which might have been expected if agrodealers cater more to larger and wealthier farmers. In particular, Table A10 shows that there is no treatment-effect heterogeneity according to farm size. Adoption is more likely by larger farmers, but this is equally true in treatment and control blocks. We also find no heterogeneity according to being below the poverty line or in a marginalized caste group.

4.4 Supply-side responses to the treatment

Recall that we only treated a fraction of the dealers in each block. More precisely, 42% of sample dealers in treatment blocks received seeds and information (Table A11). These

[26] One possible concern is that we failed to obtain GPS coordinates for all farmers — they are missing for about 17 percent of the sample (1,110 respondents). In the online appendix we impute the locations of these houses using village locations in one of two ways. If we observe other households in that village, then we use the average latitude and longitude values from the observed households (603 farmers). If we observe no other households in the village, then we try to match the village to the 2011 Census and use the village centroid as an approximate household location (323 farmers). Figure A5 shows that results are robust to including these observations in the flood-heterogeneity analysis.

dealers were not randomized. Hence, our dealer-level analysis compares all private dealers in treatment blocks to those in control blocks. We therefore capture any direct effect of receiving the seeds and information and any spillovers — which of course could be either negative or positive.

There is some evidence that the treatment caused dealers to increase the *availability* of Swarna-Sub1. Columns 1-4 in Table 4 show results from one year after the treatment (year 2). Focusing on all dealers — including those that were no longer operating — the treatment has a small positive effect on the likelihood of carrying Swarna-Sub1 at any time during the season (Column 1) and the total amount the dealer reported selling throughout the year (Column 2). But both of these estimates are very imprecise, partly due to some dealers no longer being in business. Amongst the subset of active dealers, those in treatment blocks were 6.2 percentage points more likely to carry Swarna-Sub1, a 17 % increase (Column 3). Column 4 shows that dealers in treatment blocks sold 3.7 additional quintals (1 quintal = 100 kg), which represents a 59% increase in volume sold. But again, while larger, neither of these results are close to statistically significant.

Anticipating on an intervention done in year 3 (and described below), we find large and precise effects on stocking behavior (Column 5). 19.3% of dealers in control blocks had Swarna-Sub1 in stock when visited by the secret shopper.[27] This increases by 11.4 percentage points (59%) in treatment blocks. This large effect is being observed two years after the treatment. It also comes from a direct observation of what the dealer had available on a certain day, rather than a noisy estimate from what they recalled after the season.

This result could be driven by a number of things. First, it could come directly from the dealers that were treated and had their information sets updated. Second, dealers talk to farmers. Any increase in knowledge of farmers could spread to other dealers, not only those that were treated. Third, dealers were provided with several minikits for testing. They could have shared those in a way that increased local knowledge. We cannot distinguish between these effects in the analysis.

We next test whether the treatment changed the extent of local seed *production*. Our data here amount to six observations per block: three from the period before our treatment could have triggered a production response (2014-2016) and three from the post-treatment period (2017-2019). We therefore estimate block-level regressions of the amount of Swarna-Sub1 seed produced on treatment and district and year fixed effects.

We first verify that seed production from 2014 to 2016 is not related to the treatment.

[27]The probability of having Swarna-Sub1 available appears lower in year 3 for a couple of reasons: 1) availability was observed on a specific day when the shopper visited and not across the entire season and 2) visits by the secret shopper occurred later in the season when varieties were no longer in stock for some dealers.

Columns 1 and 2 of Table 5 show that performing the analysis in the "pre period" yields a negative but statistically insignificant coefficient on the treatment indicator. This verifies that there were not large pre-existing differences between treatment and control blocks prior to the experiment.

We find evidence that treatment blocks produced more Swarna-Sub1 seeds during years after the experiment. Columns 3 and 4 show regressions using the total amount of seed production (in quintals) and its log. The point estimate is more precise in the log regression of Column 4. It shows that the treatment led to a 47.9 percent increase in the amount of seed production. Columns 5 and 6 show that results become more precise when conditioning on average annual production during the 2014 to 2016 period. In column 5, treatment increased production by 79 quintals, or about 38%. Again the result is more precise in the log regression, as Column 6 shows that treatment blocks produced an average of 57 percent more Swarna-Sub1 seeds during the three years after the treatment. The online appendix helps visualize this result by giving the cumulative distribution functions of seed production by treatment (Figure A6). They show a noticeable rightward shift in the distribution for the treatment blocks.

These results should not necessarily be interpreted as local production increasing to meet growing demand of farmers. In fact, seeds are often processed outside of the block where they are grown and can be sold anywhere in the state after processing. It seems more likely that intervening with agrodealers caused more people to know enough about Swarna-Sub1 to cultivate it. This group includes seed producers, who are often large landholders, and can rely on some of the same sources of information as smaller farmers.[28]

5 Mechanisms and motivations behind dealers' role in increasing technology adoption

Our analysis up to this point shows that the information treatment targeted at agrodealers causes more farmers to adopt new technology. That is, the approach of informing private agents on the supply side of technology can outperform standard approaches where frontline government workers interact directly with farmers. This section explores the mechanisms that are at play. More specifically, we first want to understand **how** dealers increase the adoption of Swarna-Sub1 amongst farmers in treatment blocks and the

[28]Table A12 shows that there is a positive correlation between farmer-level adoption (from our survey) and block-level seed production. This is evidence that seed producers select varieties that are best suited for local conditions — and hence selected by farmers.

role they play in that process (Section 5.1). Secondly, we look into **why** treating dealers led to farmers being more likely to use Swarna-Sub1 and what are dealers' underlying motivations for increasing farmers' adoption (Section 5.2).

5.1 How do dealers increase adoption amongst farmers?

There are different ways dealers could have increased farmers' adoption of Swarna-Sub1. For example, they may advise clients towards purchasing this variety; playing an active and directed information-sharing role, that goes beyond the more traditional information sharing approach practiced by extension agents. Alternatively, they may have played more of an indirect role by informing people who are well connected, or even by giving demonstration seeds to better connected farmers.

We used the third year of the experiment (2018 season) to test whether dealers actively advised farmers about the new technology. Eliciting advice is not easy. Simply asking dealers whether they informed farmers and/or recommended Swarna-Sub1 likely suffers from experimenter demand effects.

We alleviate this concern with a unique strategy to elicit advice using "secret shoppers". First, an enumerator visited dealers in both treatment and control blocks during the time when farmers usually buy seeds. The enumerator was someone the dealer had not seen before and who did not identify himself as part of the research. Then, the enumerator followed a specific script to obtain advice from the dealer. Specifically, the enumerator mentioned that his father from a nearby village was planning to cultivate rice and was looking for information on possible varieties to grow. The enumerator asked the dealer which varieties to consider, without mentioning the name of any particular variety.[29] Dealers usually mentioned several varieties - which we describe as the dealers "listing" varieties. If the dealer did not make a specific recommendation the shopper asked him which one he would recommend. We refer to this outcome as a dealer recommendation. If the dealer asked about type of land, he was told medium-low in terms of elevation and hence risk of flooding. We also asked which varieties the dealer currently had in stock. These data allow us to test whether dealers were acting as information agents two years after some of them were informed about, and given access to Swarna-Sub1 seeds for their own learning.[30]

Given the costs of these visits, and the scattered nature of our sample, we focused

[29]We phrased the question in these general terms to avoid priming the dealers to think about any particular variety.

[30]Right after the conversation with the dealer, the enumerator filled in a survey to record the answers given by the dealer.

on the dealers that we reached during the previous year and were selling rice seeds. The sample consists of 310 dealers, 15 of which were not from our list obtained at the beginning of the study, and 15 of which we did not reach. The sample for analysis therefore includes 280 dealers.

To assess whether dealers actively advised farmers, we look at how they interact with the secret shopper who visited their shops. If dealers do play an active role in promoting Swarna-Sub1 to farmers, dealers located in treatment blocks should be more likely to list and/or recommend Swarna-Sub1 compared to control blocks. If instead, dealers do not play this active role, we should not expect to see differential recommendations from dealers between control and treatment blocks.

Table 6 shows regression results for separate outcomes (as rows) on the block-level dealer treatment and district fixed effects. Again, these results are "intention to treat" since not all dealers in treatment blocks were informed. While the shoppers had a clear script to follow, it was impossible for the conversation to follow the same path for every single dealer. We therefore show results with and without fixed effects for the different shoppers.

Most dealers list Swarna as a popular variety and this is not different between treatment and control blocks. However, dealers in treatment blocks were 12-13 percentage points more likely to list Swarna-Sub1 as a possibility to consider, a 25% increase given that control dealers list Swarna-Sub1 51% of the time. Furthermore, treatment dealers were about 4-7 percentage points more likely to recommend Swarna-Sub1 - a large albeit non significant effect. Not surprisingly, the increase in Swarna-Sub1 recommendations comes at the expense of recommendations for Swarna — the variety that is otherwise similar, but does not offer flood tolerance. Indeed, being located in the treatment block reduces the likelihood dealers recommend Swarna by 13 percentage points, a 31% decrease in recommendations for this older variety.

We previously showed that dealers in treatment blocks were more likely to be carrying Swarna-Sub1 at the time of these visits. The last four rows of the table show that listing and recommending Swarna-Sub1 go hand in hand with stocking it. In other words, the treatment causes dealers to both suggest the new variety to farmers and carry it in their shops. This is evidence that treatment dealers play a direct role in the increase of Swarna-Sub1 adoption by mentioning it to farmers who enter their shops.

5.2 What motivates dealers to actively encourage adoption amongst farmers?

We conducted an additional small experiment during the fourth year (2019) to learn more about what motivates dealers to expend effort in making recommendations. Again, dealers in our study did not have an obvious financial incentive to recommend any specific seed variety. Their margins were the same across varieties. But we might expect dealers to expend effort to make good recommendations because they are motivated by business prospects, beyond standard pro-social preferences for other farmers' well-being and/or usual norms expected in a supplier-client relationship. In particular, making good recommendations would be in their best interest if maintaining a solid reputation keeps farmers coming to their shop.

To test whether treatment dealers' incentives to advise farmers come from the fact that they are business motivated individuals who care about their reputation, we partnered with a local NGO that visited the dealers in our sample and asked for recommendations for a program they were planning on launching. More specifically, the NGO informed the dealers that they would organize a seed demonstration in a village (located in the dealer's block) where farmers would cultivate a new variety and villagers would be invited to learn about it. They asked dealers for recommendations on the type of variety and which village to choose for the program as well as who would be the best farmers within the chosen village to partner with for cultivation. Dealers were randomly allocated to one of two groups and received slightly different introductions from the NGO. In the first group, the NGO made more salient the fact that the dealers' reputation may be at play. Specifically, in the first group — referred to as the reputation treatment group —, dealers were explicitly told that their name would be displayed and advertised during the demonstration; whereas in the second group, no mention was made of displaying the dealer's name and the NGO informed the dealers that the harvest would be collected after the demonstration and redistributed as seeds to other farmers in the village. We recorded the responses to our four main types of outcomes of interest: dealers' effort invested in providing recommendations to the NGO, the varieties they suggest using, the village they recommend for the program, and lastly which farmers they suggest partnering with to carry out the demonstration.

Table 7 shows the results of the experiment. Results are divided into five groups. First, we find that dealers in the reputation treatment group spent a non-significant 10 percent more time on making recommendations (row 1). The second row shows that this increase in effort appears mostly when, in the conversation, dealers are asked which specific farmers to rely upon for the demonstration: a 14% increase in time invested on an average of just over two minutes picking farmers to recommend. Second, the reputation treatment seemed to shift dealers away from suggesting popular seed varieties and toward selecting them based on land type. The fourth row shows that when asked why they suggested a particular variety, 16 percent of dealers report doing so because it is locally popular; and this falls by 7.1 percentage points, or 44% in the treatment group.[31] While the estimate in the next row is not significant, the treatment seems to cause dealers to recommend varieties based on their agronomic characteristics, suitability to the land, and how long they take to grow (duration).

Third, we find some evidence that knowing that their name would be revealed changed how dealers recommend villages. Dealers were presented with a list of three randomly selected nearby villages to choose from. They were also given an option of recommending a village not on that list. The treatment increased the likelihood that dealers took this option by 12.7 percentage points (a 34 percent effect). Most dealers taking this option picked their own village. Amongst dealers that wanted to recommend a village not on their randomly selected list, 63 percent of the control group picked their own village. This increased by 13.5 percentage points for the treatment group, although the difference is not quite statistically significant.

Fourth, dealers were asked to identify three farmers to grow the seeds as part of the demonstration. About 81 percent of dealers report that they selected existing clients. This falls by 9.2 percentage points with the treatment. Linking a dealer's name to the recommendation seems to instead cause them to suggest farmers that are villagers or close neighbors for them. The ability of observing the demonstration plot when it is cultivated by a close neighbor might explain this result. Dealers may put more value on having this ability when their reputation is linked to the demonstration.

Finally, dealers at the end of the visit were asked if they felt the demonstration would affect their business. Around 67% of the control group reported it would. This suggests that many dealers thought the demonstration would increase seed demand — even though their names would not have been identified. Making the dealer's name visible increased the perception that the demonstration would affect business by 8.6 percentage points, a 13 percent effect. This effect is modest, but it aligns with the other results showing how this subtle treatment changed some of the advice given by dealers.

Overall, we argue that these findings help interpret the results of our main experiment. They provide suggestive evidence that the advice given by dealers is motivated at least partly by their concerns about how it will affect their reputation and future business

[31] Popular varieties mostly correspond to older seeds that farmers have been growing for a long time.

opportunities. This offers one explanation as to why dealers advise farmers about a new seed variety even when prices do not give them a direct incentive to do so.

While helping to understand the incentives at play, this additional small experiment has some clear limitations. First, it does not mimic an everyday conversation between dealers and customers as closely as our secret shoppers did the previous year. Framing the conversation around an on-farm demonstration gave a simple way of randomizing some dealers to have their business interests and reputation linked to the demonstration. But it has the downside of not being the same type of conversation that would happen between a farmer and dealer. Second, the sample size is small, as it was limited to the active dealers from our original list. As such, some of the estimates are imprecisely estimated.

6 Discussion and conclusions

This article provides evidence on how private sector business practices can be more effective than public agricultural extension services in promoting the adoption of new agricultural technologies. Relying on private input suppliers, i.e. agrodealers, would be a substantial deviation from the standard methods currently used. Indeed, government workers most often try to spread knowledge via direct contacts with selected farmers, expecting those to in turn diffuse information in their social networks. Much of current research on information constraints tries to identify ways of optimizing this approach. Our paper instead provides an empirical test of using an entirely different approach where information is transmitted to private input suppliers.

We find that informing private agrodealers about a new and profitable seed variety, and giving them small amounts of demonstration seeds to test, causes farm-level adoption to increase by over 50 percent, compared to the business-as-usual approach where government workers focus on outreach with selected farmers. Using the private-sector approach increases adoption most among farmers with higher expected benefits from the technology. This improvement in targeting suggests that there is an alignment of incentives between dealers and farmers: dealers benefit in some way from inducing farmers to adopt the right technology. We also found that our treatment triggers a supply response. It causes dealers to be more likely to keep the seed in stock and it increases local production of the seed.

Further evidence shows that these effects can be at least partly explained by dealers actively advising farmers. Dealers in treated locations are more likely to mention the new seed variety when asked what to grow by a "secret shopper". Unpacking the incentives of agrodealers is difficult and not something we can do perfectly with our experiments.

But we find some evidence in our second experiment that agrodealers may consider their reputation and business prospects when giving advice. Reputational and business concerns can discipline agrodealers to put effort in recommending the right technologies to farmers.

Our findings thus show potential for a different approach to agricultural extension in developing countries: delivering information on the supply rather than the demand side of technology. Of course, there might be drawbacks to this type of approach in other contexts. Particularly, if agrodealer incentives are not well aligned with those of farmers, then using them as information agents may not increase adoption of the optimal technologies for farmers. But it appears that when incentives are aligned, as in the case of our experiment, making input suppliers better informed can improve the practice of agricultural extension.

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Tables

Table 1: Summary Statistics and Covariate Balance

	Means		
	Control	Treatment	p-value
Panel A: Block Characteristics (N=72)			
Number Villages	136.0	135.4	0.878
	(64.12)	(60.52)	
Population	110681.4	120997.2	0.296
	(38998.4)	(49184.3)	
Annual Swarna-Sub1 Seed Production	298.9	191.9	0.289
	(421.2)	(264.6)	
Share Scheduled Caste	0.209	0.214	0.618
	(0.0492)	(0.0522)	
Share Scheduled Tribe	0.0462	0.0286	0.136
	(0.0767)	(0.0441)	
Elevation (Meters)	23.51	19.72	0.174
	(24.43)	(19.35)	
Literacy Rate	0.727	0.737	0.424
	(0.0726)	(0.0475)	
Share Agricultural	0.636	0.651	0.537
	(0.141)	(0.0931)	
Child Sex Ratio, 0-6 yrs	1.072	1.069	0.746
	(0.0228)	(0.0214)	
Panel B: Farmer Characteristics (N=6653)			
Age	49.42	49.83	0.401
	(11.12)	(11.38)	
Years Education	7.930	7.818	0.303
	(4.467)	(4.533)	
Below Poverty Line Card	0.471	0.510	0.245
	(0.499)	(0.500)	
Female Farmer	0.0794	0.0809	0.714
	(0.270)	(0.273)	
Scheduled Tribe	0.0160	0.00569	0.0336
	(0.125)	(0.0752)	
Scheduled Caste	0.150	0.153	0.853
	(0.357)	(0.360)	

Panel A shows means and standard deviations of block-level characteristics from the 2011 Census of India (with exceptions of elevation and annual Swarna-Sub1 seed production). Elevation is calculated from satellite data and the annual Swarna-Sub1 seed production is the average annual amount of seed processed from registered growers in the block from 2014 to 2016. It is measured in quintals (1 quintal = 100 kg). The literacy rate is defined as the number of literate individuals divided by the population older than 6 years old. The share agricultural is defined as the number of people working in agriculture divided by the working population. The child sex ratio is the numer of male children 0-6 years old divided by the number of female childen 0-6 years old. Panel B shows means and standard deviations of characteristics from our household survey. The p values in column 3 are for the treatment variable in regressions of each characteristic on treatment and district (strata) fixed effects. Panel A uses robust standard errors while Panel B clusters errors at the block level.

Table 2: Treatment Effects on Technology Adoption

	(1) Adoption (Y/N)	(2) Acres	(3) Acres - SS1 adopters
Treatment	0.035*	0.064**	0.136*
	[0.019]	[0.031]	[0.079]
Dependent Variable Control Mean	0.063	0.093	1.470
R-Squared	0.028	0.026	0.045
District Fixed Effects	X	X	X
Observations	6653	6653	539

The table shows the impact of aiding agrodealers' learning on technology adoption by farmers. All regressions use the data from the follow-up survey with farmers in August/September of 2017. The dependent variables are whether the farmer was currently using Swarna-Sub1 (column 1), the acreage cultivated with Swarna-Sub1 (column 2), and the acreage cultivated with Swarna-Sub1 conditional on adopting it (column 3). The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 3: Heterogeneous effects by flooding risk

	(1)	(2)
Treatment	0.0263	0.00788
	(0.0216)	(0.0251)
Treatment * Flood Days	0.00290	
	(0.00299)	
Flood Days (divided by 10)	-0.00238*	
	(0.00143)	
Treatment * Above Median Risk		0.0563*
		(0.0330)
Above Median Risk		-0.0595***
		(0.0218)
District FE	Yes	Yes
Mean in Control	0.063	0.063
Number of Observations	5536	5536
R squared	0.031	0.036

The table shows heterogeneous treatment effects by flooding exposure. The dependent variable in both columns is an indicator for adopting Swarna-Sub1. The variable “Flood days” in Column 1 is the total number of days (2011-2017 June-October) where satellite imagery detected a flood within 1 km of the farmer’s house. Column 2 separates the sample into high-risk and low-risk households based on the median number of flood days (equal to 3) and introduces an interaction between this binary variable and the treatment. Standard errors are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 4: Treatment Effects on Supplying Seeds by Dealers

	Year 2: All		Year 2: In business		Year 3
	(1)	(2)	(3)	(4)	(5)
	Carried (0/1)	Quantity	Carried (0/1)	Quantity	In Stock (0/1)
Treatment	0.026	1.633	0.062	3.662	0.114**
	[0.052]	[1.676]	[0.083]	[2.656]	[0.057]
Dependent Variable Control Mean	0.242	3.793	0.397	6.231	0.193
R-Squared	0.045	0.037	0.114	0.117	0.097
District Fixed Effects	X	X	X	X	X
Observations	473	472	274	273	280

The table shows treatment effects on Swarna-Sub1 inventories from a survey of dealers (Columns 1-4) and the secret shopper visit (Column 5). Columns 1 and 2 are for the sample of dealers that were located and surveyed during year 2 (September 2017). Columns 3 and 4 are for the subset of those dealers that were actively in the seed business during that same season. Column 5 is for dealers that were visited in the secret shopper sample during year 3 (June 2018). The standard errors in each regression are clustered at the block level. The dependent variables are an indicator for whether the dealer reported carrying Swarna-Sub1 at any time during the season (columns 1 and 3), the total quantity sold throughout the season (columns 2 and 4), and an indicator for whether the dealer had Swarna-Sub1 in stock when visited by the secret shopper (column 5). Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 5: Treatment Effects on Local Seed Production

	Pre Period (2014-2016)		Post Period (2017-2019)			
	(1) Amount	(2) Log	(3) Amount	(4) Log	(5) Amount	(6) Log
Treatment	-88.914	-0.066	14.014	0.479*	79.494	0.568***
	[79.308]	[0.239]	[81.938]	[0.262]	[52.494]	[0.209]
2014-2016 Production					0.736***	0.002***
					[0.122]	[0.000]
Dependent Variable Control Mean	298.877		209.511		209.511	
R-Squared	0.175	0.246	0.149	0.391	0.435	0.532
District Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
Observations	216	129	216	124	216	124

The table shows the effects of the dealer treatment on block-level seed production of Swarna-Sub1. Publicly available data on producer-level production of certified Swarna-Sub1 seeds were matched to the blocks in the experiment. The unit of observation in each regression is the block-season, where years range from 2014 to 2019. Columns 1 and 2 are for the years where seed production was set prior to the experiment (2014-2016) and columns 3-6 are for the post-experiment years. The dependent variables are the annual amount of Swarna-Sub1 seed processed from growers in the block (columns 1, 3, and 5) and its logged value (columns 2, 4, and 6). Columns 5 and 6 control for the average annual production from 2014-2016 (the pre-period outcome). The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 6: Treatment Effects on Providing Advice to Secret Shoppers

	Control Mean (1)	Treatment Effect (2)	Treatment Effect w/ Shopper FE (3)
Listed Swarna	0.834	-0.0157	0.0129
		(0.0353)	(0.0337)
Listed Swarna-Sub1	0.510	0.120	0.130**
		(0.0740)	(0.0634)
Recommended Swarna-Sub1	0.297	0.0425	0.0701
		(0.0517)	(0.0484)
Recommended Swarna	0.421	-0.116**	-0.131**
		(0.0467)	(0.0495)
Listed Swarna-Sub1 & No Stock	0.352	-0.00892	0.00120
		(0.0576)	(0.0476)
Listed Swarna-Sub1 & Stocked	0.159	0.129**	0.129**
		(0.0514)	(0.0550)
Recommended Swarna-Sub1 & No Stock	0.179	-0.0166	-0.00249
		(0.0376)	(0.0419)
Recommended Swarna-Sub1 & Stocked	0.117	0.0591	0.0726*
		(0.0395)	(0.0406)

The table shows treatment effects on the type of information provided by dealers when they were visited by a secret shopper during year three (June 2018). Each row shows results from a separate regression (N=280) of that outcome variable on the block-level treatment indicator and strata fixed effects. Column 1 shows the control mean, while the coefficient estimate on the treatment indicator, and its standard error, are presented in column 2. Column 3 shows results when also including a shopper fixed effect. Listing Swarna and Swarna-Sub1 (rows 1 and 2) are binary variables for whether the dealer included that variety when listing good varieties to try. Recommending Swarna and Swarna-Sub1 (rows 3 and 4) are binary variables for whether the dealer recommended that variety when asked to make a specific recommendation. Rows 5-8 show effects on listing / recommending the varieties and whether or not they were currently in stock with the dealer. The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 7: Effects of priming business and reputation concerns on dealer recommendations

	Control Mean	Estimate
Timing		
Total time spent (minutes)	5.093	0.526 (0.406)
Time spent picking farmers	2.200	0.303* (0.166)
Variety Selection		
Number varieties recommended	2.893	-0.269 (0.212)
Selected popular	0.157	-0.071* (0.039)
Selected for duration/land type	0.179	0.078 (0.048)
Village Selection		
Chose outside of list	0.371	0.127** (0.056)
Picked own village	0.493	0.069 (0.059)
Picked own village if chose outside	0.635	0.135 (0.086)
Farmer Selection		
Chose because client	0.807	-0.092* (0.047)
Chose because villager/neighbor	0.121	0.091** (0.041)
Business Perception		
Felt treatment would affect business	0.671	0.086* (0.052)

The table shows effects of suggesting that a dealer's name be affiliated with an on-farm demonstration on their suggestions to an NGO for carrying out that demonstration in 2019. The treatment was informing dealers that their name would be affiliated with the demonstration and the control group is not providing this information and explaining that the seeds from the demonstration would be distributed to other villagers. Column 1 shows the mean of the outcome in the control group, while column 2 shows the point estimate and standard error from a regression of the outcome on the treatment and district fixed effects. The unit of randomization is the dealer. Therefore, robust standard errors are in parentheses for column 2. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Figures

Figure 1: Timeline of interventions and data collection

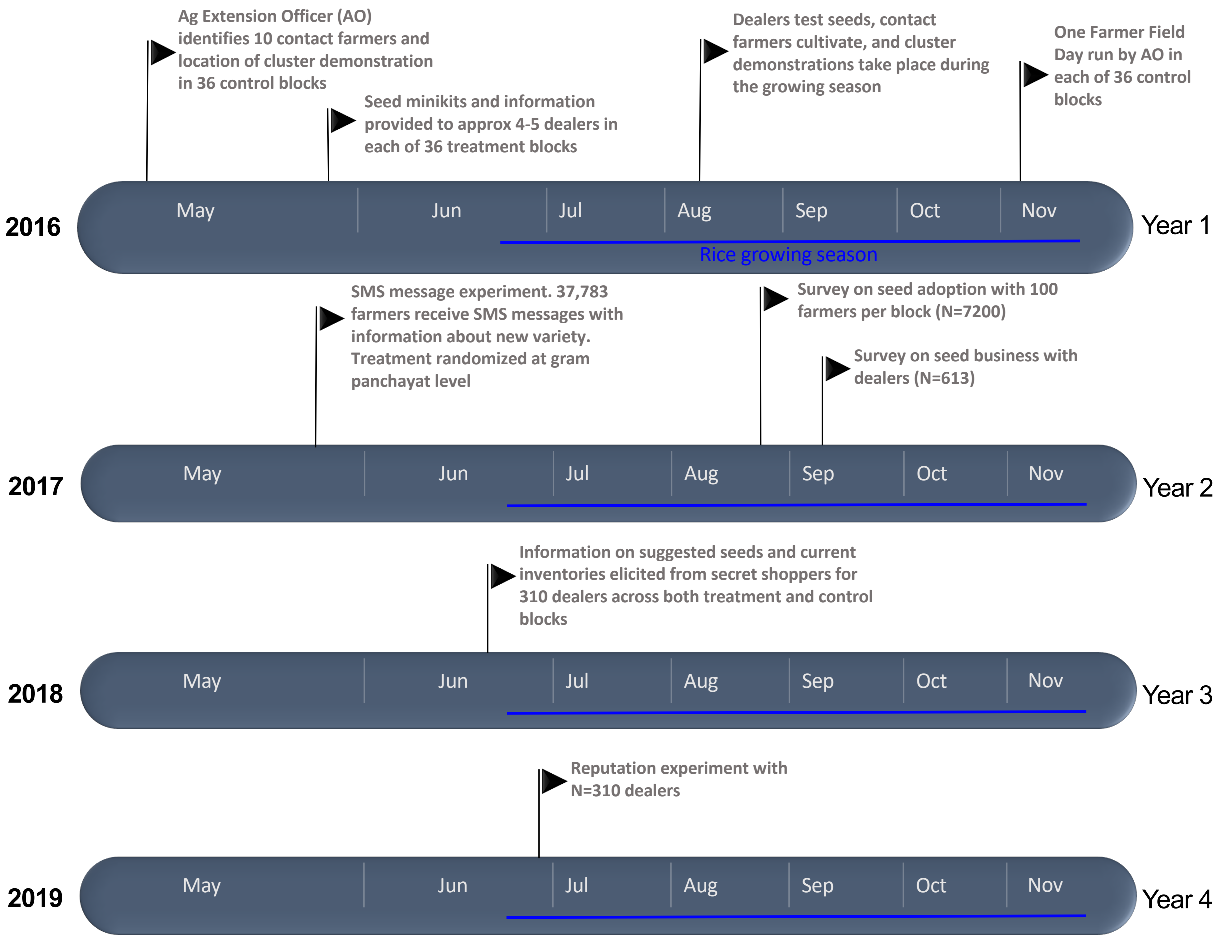
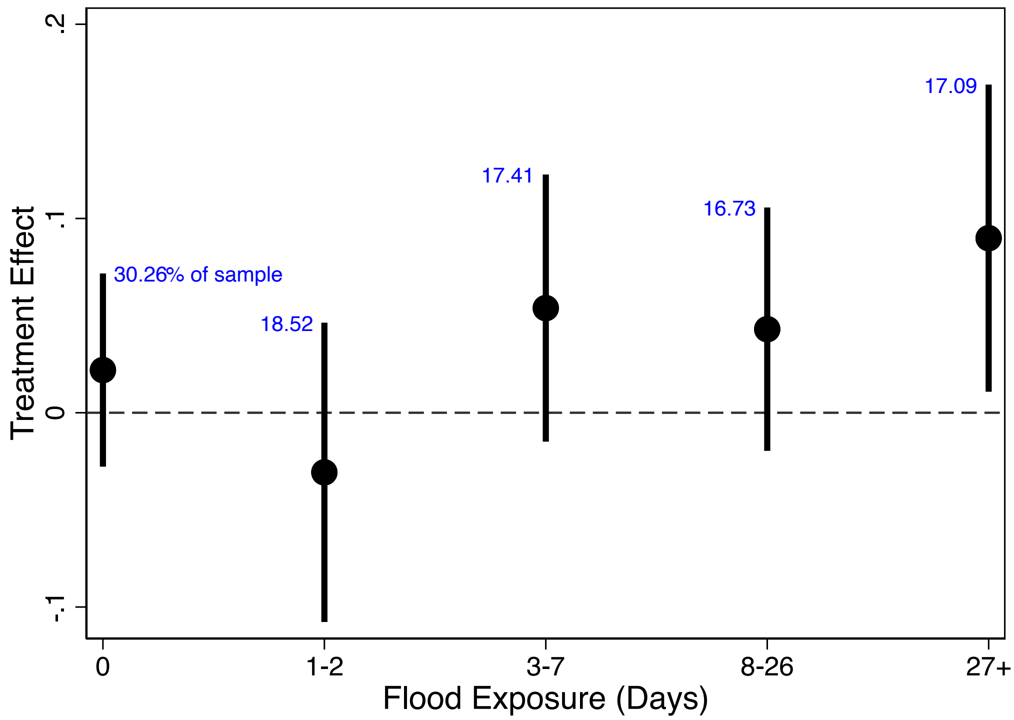


Figure 2: Treatment effects by flood exposure



Notes: The figure shows treatment effects from a single regression of adoption on separate treatment indicators for different levels of flood exposure and district fixed effects. The 5 bins of flood exposure correspond to households with no exposure from 2011-2017 and then an approximately equal division of households with at least one day of exposure. The percentage of observations in each bin is denoted in blue. The dots are the treatment effects of dealer-based extension and the vertical lines denote 95 percent confidence intervals.

Appendix: For Online Publication

Table A1: Relationship between treatment assignment, non-response, and growing rice among the sample of farmers

	(1) Not Surveyed	(2) Grows Rice
Treatment	-0.010	0.024
	[0.013]	[0.015]
Dependent Variable Control Mean	0.079	0.920
R-Squared	0.043	0.011
District Fixed Effects	X	X
Observations	7200	6653

The table shows the difference in the rate of non-response and currently growing rice across treatment and control groups. All regressions use the data from the follow-up survey with farmers in August/September 2017. The dependent variables are indicator variables for not being surveyed (column 1) and an indicator for growing rice during the 2017 season (column 2). The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A2: Relationship between treatment assignment, non-response, and selling seeds among the sample of dealers

	(1) Located	(2) In Business
Treatment	0.035	-0.041
	[0.046]	[0.055]
Dependent Variable Control Mean	0.745	0.610
R-Squared	0.316	0.050
District Fixed Effects	X	X
Observations	613	473

The table shows the difference in the rate that dealers were not surveyed (column 1) and the difference in being in the rice seed business (column 2) across treatment and control groups. All regressions use the data from the follow-up survey with dealers around September 2017. The dependent variables are indicator variables for not being surveyed (column 1) and an indicator for currently selling rice seeds among those surveyed (column 2). The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A3: Correlation between flood exposure and socioeconomic characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Area Cultivated (Acres)	-1.104	-0.593**				
	[0.666]	[0.242]				
Below Poverty Line Card			3.269	1.987		
			[2.016]	[1.451]		
Scheduled Tribe or Caste					4.183*	5.136**
					[2.113]	[2.366]
Dependent Variable Control Mean	16.075	16.075	17.374	17.374	17.353	17.353
R-Squared	.004	.129	.002	.142	.001	.144
District Fixed Effects		X		X		X
Observations	5134	5134	5521	5521	5529	5529

The table shows the relationship between flood exposure and household characteristics from the 2017 survey. The dependent variable in all regressions is the total number of days of flood exposure during the growing seasons from 2011-2017, measured by matching satellite data to the GPS coordinates of the household. All standard errors are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A4: Tobit estimates of extensive and intensive margins of adoption

	(1) Adoption	(2) Acres for Adopters
Treatment	0.034*	0.117*
	[0.019]	[0.061]
Dependent Variable Control Mean	0.063	1.470
District Fixed Effects	X	X
Observations	6653	6653

The table shows marginal effects from Tobit regressions of area cultivated with Swarna-Sub1 on strata fixed effects and treatment. All regressions use the data from the follow-up survey with farmers in August/September of 2017. Both columns show average marginal effects and delta-method standard errors. Column 1 shows the marginal effect on the probability of adoption, while column 2 shows the marginal effect on acreage cultivated, conditional on adoption. The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A5: Effects on Technology Adoption with household- and block-level controls

	(1) Adoption (Y/N)	(2) Acres	(3) Acres - SS1 adopters
Treatment	0.032**	0.061**	0.105
	[0.015]	[0.025]	[0.091]
Dependent Variable Control Mean	0.063	0.093	1.467
R-Squared	0.063	0.045	0.092
District Fixed Effects	X	X	X
Controls	X	X	X
Observations	6599	6599	535

The table shows the treatment effects on technology adoption when including all of the block- and household-level covariates in the balance table (Table 1). All regressions use the data from the follow-up survey with farmers in August/September of 2017. The dependent variables are whether the farmer was currently using Swarna-Sub1 (column 1), the acreage cultivated with Swarna-Sub1 (column 2), and the acreage cultivated with Swarna-Sub1 conditional on adopting it (column 3). The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A6: Effects on learning-related outcomes

	(1) Extension Contact	(2) Saw Demonstration	(3) Learned during last 24 months
Treatment	0.013	0.003	0.018
	[0.010]	[0.012]	[0.017]
Dependent Variable Control Mean	0.057	0.043	0.090
R-Squared	0.016	0.031	0.191
District Fixed Effects	X	X	X
Observations	6120	6653	6653

The table shows treatment effects on contact with extension workers and learning about Swarna-Sub1. All regressions use the data from the follow-up survey with farmers in August/September of 2017. The dependent variables are an indicator for whether the farmer had any contact with the Village Agricultural Worker during the last year (column 1), whether the farmer had seen a demonstration of a new seed variety (column 2), and whether the farmer had learned about Swarna-Sub1 in the last 24 months (column 3). The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A7: Effects on adoption of different rice varieties

	Control Mean	Estimate
Pooja	0.376	0.011
		(0.048)
CR 1018	0.053	0.010
		(0.021)
MTU 1001	0.053	0.010
		(0.026)
Swarna	0.433	-0.041
		(0.049)
Sarala	0.099	-0.047
		(0.030)
Hybrid Rice	0.052	0.004
		(0.014)
Other Modern Seeds	0.065	0.024
		(0.026)
Local Varieties	0.304	0.052
		(0.040)

The table shows separate regressions for adoption of the rice varieties in each row on the treatment and district fixed effects. All regressions use the data from the follow-up survey with farmers in August/September of 2017. The first column shows mean adoption in the control group while the second column shows the coefficient estimate and its standard error (in parentheses). The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A8: Comparing Treatment Effects with an SMS messaging intervention

	(1) Adoption	(2) Acres	(3) Adoption	(4) Acres
SMS	-0.007	-0.012	-0.007	0.012
	[0.016]	[0.028]	[0.019]	[0.031]
Treatment			0.035	0.089*
			[0.026]	[0.046]
Treatment * SMS			-0.000	-0.049
			[0.032]	[0.055]
Dependent Variable Control Mean	0.063	0.093	0.063	0.093
R-Squared	0.024	0.023	0.028	0.027
District Fixed Effects	X	X	X	X
Observations	6653	6653	6653	6653

The table shows the treatment effects of the dealer-based extension, SMS message, and their combined effect. All regressions use the data from the follow-up survey with farmers in August/September of 2017. The dependent variables are whether the farmer was currently using Swarna-Sub1 (columns 1 and 3), and the acreage cultivated with Swarna-Sub1 (columns 2 and 4). The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A9: Average risk level of adopters by treatment group

	(1) Above-Median Risk	(2) Days Flood
Treatment	0.259*	6.742
	(0.144)	(4.322)
Mean in Control	0.239	6.273
Number of Observations	441	441
R squared	0.068	0.046

The regressions show average exposure to flood risk between Swarna-Sub1 adopters in treatment and control blocks. The dependent variable in column 1 is the binary indicator for above-median risk (exposure to flooding for four or more days). The dependent variable in column 2 is the days of exposure across all monsoon seasons (June-October) from 2011 to 2017. Standard errors are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A10: Heterogeneous Treatment Effects on Adoption

	(1)	(2)	(3)	(4)	(5)
Treatment	0.032	0.027	0.033*	0.023	0.013
	[0.020]	[0.018]	[0.019]	[0.018]	[0.017]
Scheduled Tribe or Caste	0.023*		0.018		0.018
	[0.012]		[0.016]		[0.016]
Below Poverty Line Card	0.030***			0.013	0.019
	[0.011]			[0.012]	[0.013]
Area Cultivated (Acres)	0.015***	0.013***			0.013***
	[0.003]	[0.004]			[0.003]
Treatment * Area Cultivated		0.002			0.003
		[0.006]			[0.006]
Treatment * Scheduled Tribe or Caste			0.013		0.011
			[0.025]		[0.026]
Treatment * Below Poverty Line Card				0.023	0.021
				[0.019]	[0.021]
Dependent Variable Control Mean	0.069	0.069	0.063	0.063	0.069
R-Squared	0.046	0.042	0.029	0.031	0.047
District Fixed Effects	X	X	X	X	X
Observations	6177	6193	6642	6628	6177

The table shows heterogeneous effects of the dealer treatment by farm size, caste, and being below the poverty line (columns 2-5). Column 1 shows the correlations between these characteristics and adoption, across both treatment and control blocks. All regressions use the data from the follow-up survey with farmers in August/September of 2017. The dependent variable in all regressions is whether the farmer was currently using Swarna-Sub1. The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A11: Share of dealers in estimation sample that received seeds and information

	(1) All	(2) In Business
Treatment	0.423***	0.404***
	[0.052]	[0.057]
Dependent Variable Control Mean	0.000	0.000
R-Squared	0.329	0.324
District Fixed Effects	X	X
Observations	473	274

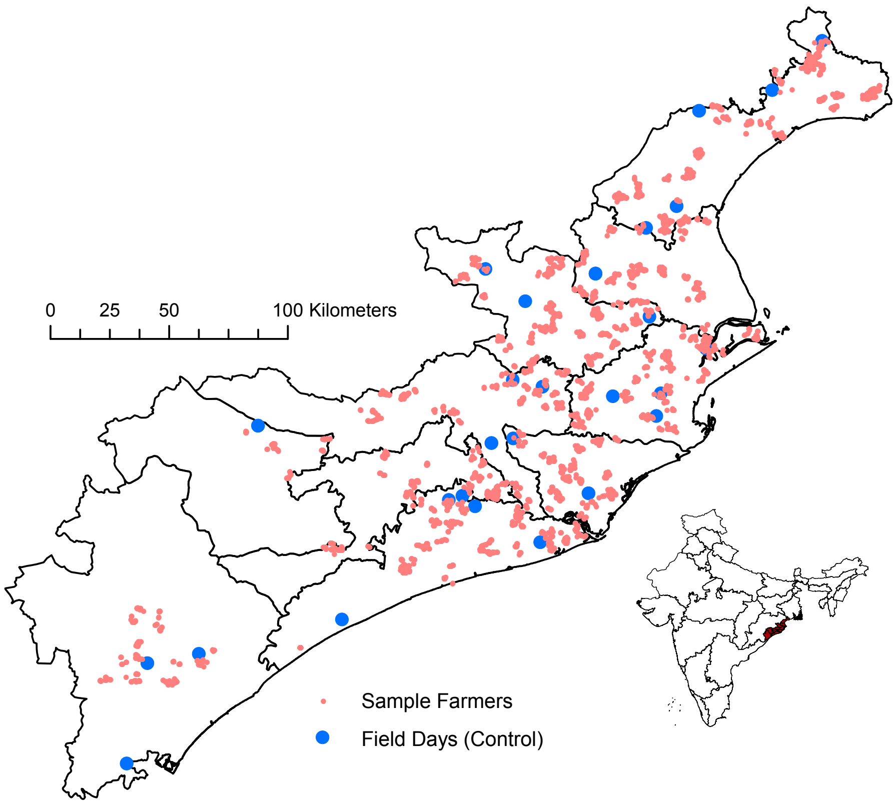
The table shows the "first-stage impact" of a dealer being located in a treatment block on the probability that they were provided Swarna-Sub1 seeds and information. Column 1 is for all dealers that were reached during the year 2 survey, while column 2 is only for the dealers that were still selling rice seeds. The dependent variable in both regressions is an indicator for whether the dealer received seeds and information. The standard errors are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A12: Correlation between farmer-level adoption of Swarna-Sub1 in 2017 and local seed production

	(1)	(2)
2014-2016 Seed Production	0.007***	0.010***
	[0.002]	[0.003]
Dependent Variable Mean	0.081	0.081
R-Squared	0.030	0.059
District Fixed Effects	X	X
Control Variables		X
Observations	6653	6599

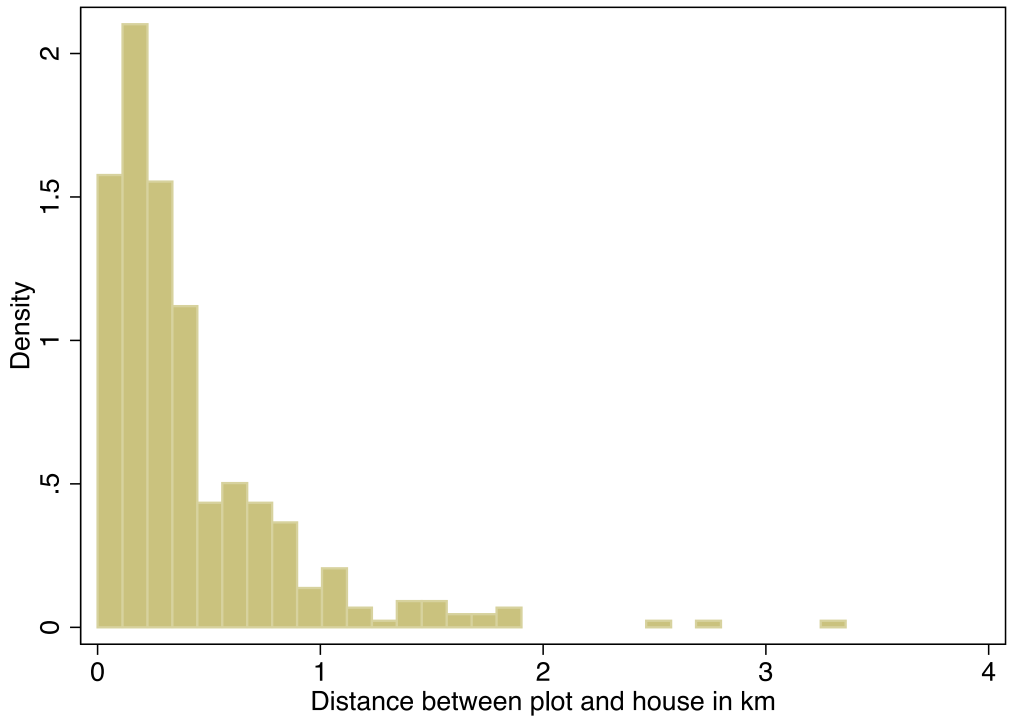
The table shows the within-district correlation between Swarna-Sub1 seed adoption by farmers and the amount of seed produced locally by growers. The estimates come from the 2017 survey with farmers where Swarna-Sub1 adoption is regressed on the average annual Swarna-Sub1 seed production in the block from 2014-2016. Seed production is measured in hundreds of quintals (1 quintal=100 kg). The dependent variable in both regressions is an indicator variable for adopting Swarna-Sub1. The control variables in column 2 are all of the covariates in Table 1 of the main text. The standard errors in each regression are clustered at the block level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Figure A1: Location of the sample



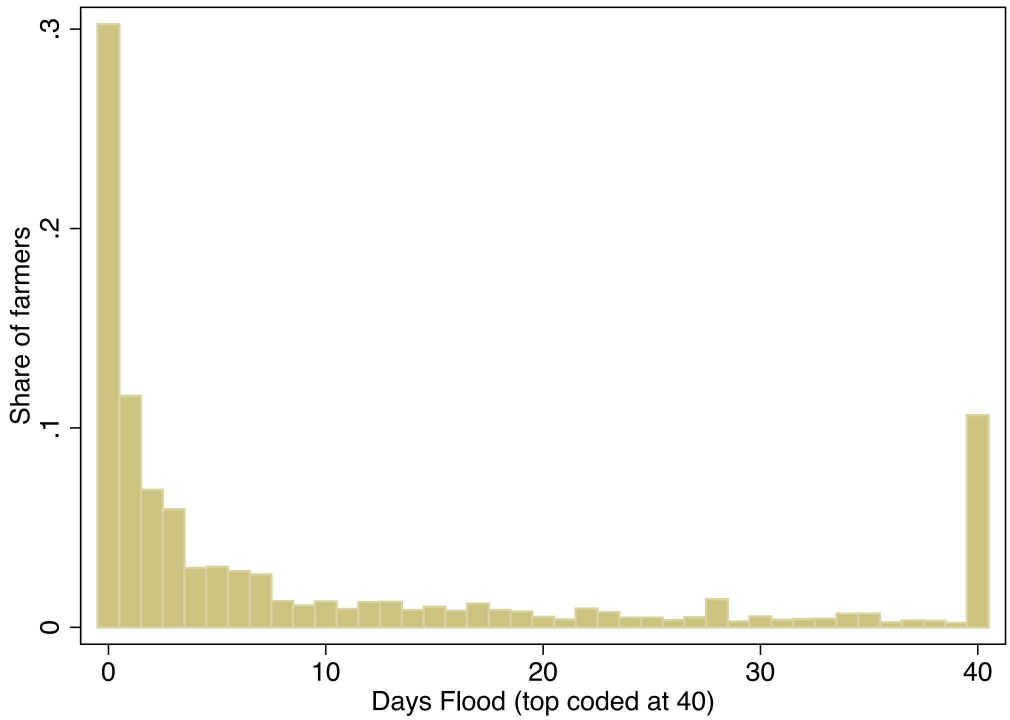
Notes: The figure shows the location for 5,536 of the 7,200 same farmers where we obtained GPS coordinates (light red dots) and the location of the farmer field days in the control blocks (blue dots). The map of India in the lower right shows the location of the sample area in the coastal belt of Odisha state.

Figure A2: Distance between plots and houses



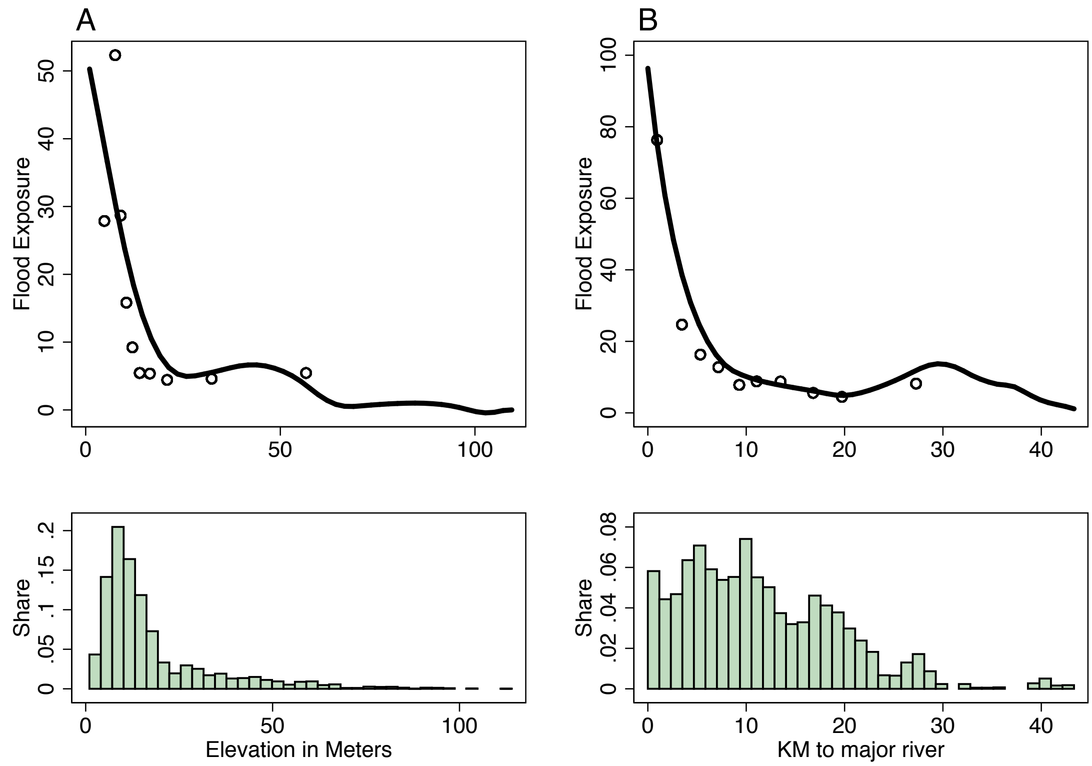
Notes: Figure shows the distribution of distances between houses and the rice plots (in km) for farmers in Emerick and Dar (2020). The district in this study is one of the 10 districts in the current paper. 92 percent of fields are within 1 km of the house.

Figure A3: Distribution of measure of flood exposure



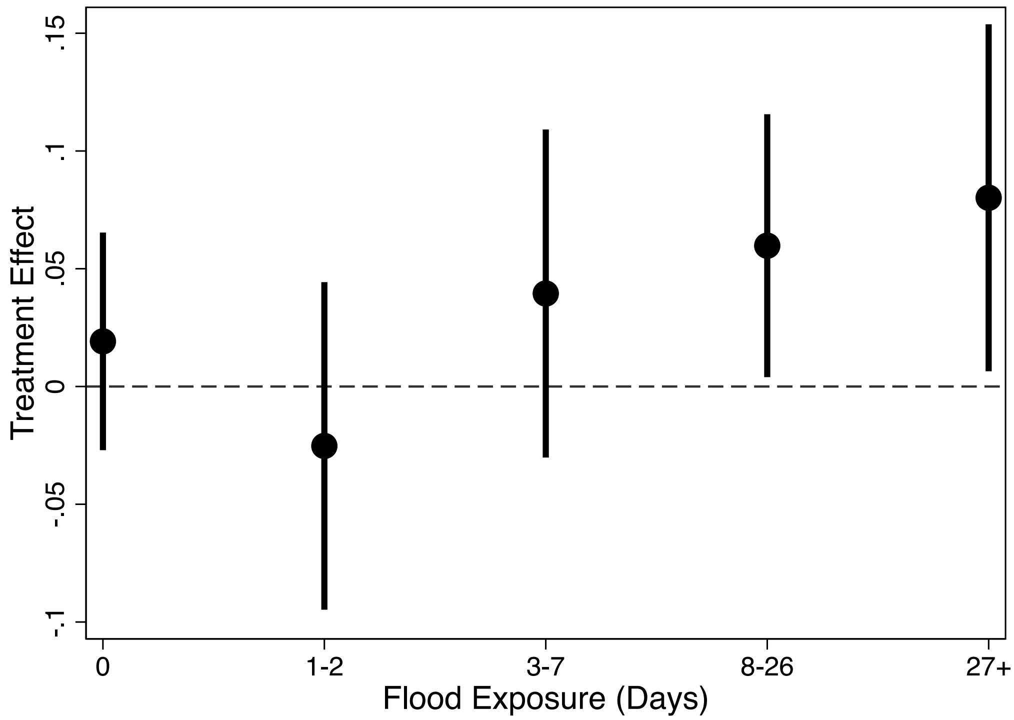
Notes: Figure shows the distribution of the days flooded from 2011 to 2017 for 5,536 households. The height of each bar displays the share of farmers with the corresponding number of days of exposure. All farmers with more than 40 days of exposure are included in the last bin at 40 days.

Figure A4: Correlation between 2011-2017 flood exposure, elevation, and proximity to rivers



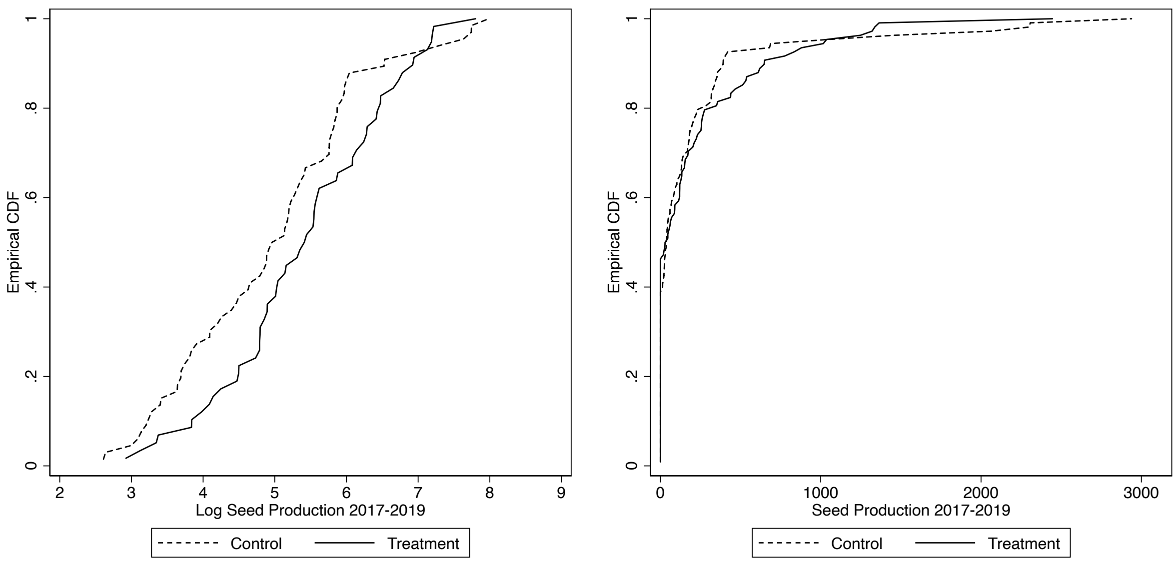
Notes: Panel A shows a non-parametric fan regression of flood exposure on elevation (heavy black line) and the average exposure levels for 10 equal-sized bins of elevation. The distribution of elevation is displayed at the bottom of the panel. Panel B shows a similar figure where flood exposure is regressed on proximity to major rivers.

Figure A5: Treatment effects by flood exposure with imputing locations for households with missing GPS coordinates



Notes: The figure shows treatment effects from a single regression of adoption on separate treatment indicators for different levels of flood exposure and district fixed effects. It is identical to Figure 2 in the main text with the one exception being that household locations are imputed from village locations for 926 observations with missing GPS coordinates. The 5 bins of flood exposure correspond to households with no exposure from 2011-2017 and then an approximately equal division of households with at least one day of exposure. The dots are the treatment effects of dealer-based extension and the vertical lines denote 95 percent confidence intervals.

Figure A6: Cumulative Distribution Functions of seed production by treatment, 2017-2019



Notes: The figure shows the cumulative distribution functions of block-year level seed production for the years 2017, 2018, and 2019. The left panel uses the log of seed production while the right panel uses the level (measured in quintals where 1 quintal = 100kg).