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Follow the leader? A field experiment on social influence ☆,☆☆

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ABSTRACT

We conduct an artefactual field experiment in endogenously formed groups in rural Malawi to investigate social influence in risk taking. Treatments vary whether individuals observe the behavior of a formally elected leader, an external leader, or a random peer. Results show social influence in risk taking with differential influence by leader type. The decisions made by peers are most influential, followed by those made by formal leaders, and then external leaders. Exploratory analysis suggests that participants follow peers because they extract information from their choices and share risks with them; while other forms of social utility are gained from following the example of leaders.

1. Introduction

The study of how and why someone's choices are influenced by others has long been an important topic in economics. A large literature studies the role of peers in influencing behavior,[1] while other work focuses on the effectiveness of different

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[1] Theoretical papers include Bikhchandani et al. (1992), Banerjee (1992), Jones (1984), and Bernheim (1994); an early overview of the literature is provided in Bikhchandani et al. (1998). Empirical papers have studied peer effects in a wide array of environments such as education (Sacerdote 2001; Duflo et al. 2011; Carrell et al. 2013), workplace productivity (Mas and Moretti 2009; Bandiera et al., 2010), crime (Glaeser et al., 1996), charitable giving (Frey and Meier, 2004), financial decisions (Bursztyn et al., 2014; Beshears et al., 2015), and technology adoption (Foster and Rozenzweig, 1995,

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types of leaders in social groups.[2] These two strands of research are particularly important when the effective implementation of social programs depends on the ability of policy makers to influence the choices of their target populations. Such influence can be especially difficult when the adoption of a new product, technology, or behavior is seen as risky, highlighting the specific need for better understanding the best ways to influence risky decisions. In this paper we conduct an artefactual field experiment that contributes to our understanding of how different types of actors influence risky decisions. The study takes place in existing, endogenously formed groups, allowing us to compare the influence of real-life peers to that of real-life leaders.

Our experiment is conducted in rural Malawi with 1028 farmers organized in farmer clubs. The experiment provides individuals with a cash endowment and a profitable financial investment opportunity. Participants (second movers) decide how much of the endowment to invest after observing the choice made by another person (first mover). Treatments vary whether the first mover is a randomly selected individual (peer), the elected chair of the farmer club (formal leader), or a professional extension agent assigned to work with the club (external leader).[3] Comparing the responses of individuals to the example set by these three types of agents allows us to examine if leaders exert a different influence than peer group members.

Results show that individuals positively respond to the example set by others. They increase (decrease) their investment when they observe high (low) investments. Average investments are similar across treatments, but differences in influence are observed when controlling for the amount invested by first movers. Peers are more influential than external leaders, who do not on average affect the decisions of others through the amount they invest. Formal leader influence lies between the two extremes but is not statistically distinct from either. Because second movers are responding to real first mover decisions, the influence that we identify is a function of both the first mover identity and the first mover decision. In general, first mover influence does not appear to be driven by characteristics that are correlated with each first mover type, though we do find that first movers who are close friends with more club members are more influential.

Many empirical measures of peer effects include social learning (Banerjee 1992; Bikhchandani et al., 1992), in which people learn about the decision environment or expected returns from others, and social image concerns (Bursztyn and Jensen, 2017), where subjects imitate their peers because they feel a social pressure to do so. A principal contribution of our work is that the experiment elicits choices in a private, perfect information environment without liquidity constraints which shuts down social image considerations and reduces the role of social learning. This isolates other mechanisms that can generate social conformity in behavior,[4] which include learning about social norms (Cialdini et al., 1990; Kallgren et al., 2000; Krupka and Weber 2009, 2013), learning due to imperfect understanding of payoffs and probabilities, imitation due to bounded rationality and heuristic thinking (Apesteguia et al., 2007), preference conformism (Fatas et al., 2018), as well risk sharing via income pooling (Angelucci et al., 2012) and social comparison effects. The last include effects driven by payoff differentials such as social regret (Cooper and Rege, 2011), envy (Lahno and Serra-Garcia, 2015), and a desire to "keep up with the Joneses" (Abel, 1990; Gali, 1994; Campbell and Cochrane, 1999).

An additional contribution of our work is that we directly compare peers to two different types of leaders, whereas most papers focus on one or the other (e.g. Beekman et al., 2014; Kosfeld and Rustagi 2015). More closely related to our work, BenYishay and Mobarak (2019) conduct a field experiment on agricultural extension among farmers in rural Malawi. They find that peers trained in a new technology and incentivized to disseminate information were more successful in increasing technology adoption than extension workers and lead farmers.[5] Our design is different in that the intensity of influence is constant across treatments.[6] We also control the information flow in our experimental setting, and as such our results do not conflate influencer effort with influence. More importantly, we study a general risky decision instead of a particular type of technology adoption. Our study therefore speaks to the general capacity of peers and leaders to influence risky decisions, while BenYishay and Mobarak (2019) provides a careful estimate of differential influence in the Malawian agricultural extension sector.

Our paper studies these questions in a context of high interest to policy makers. We work with a large number of endogenously formed groups whose main purpose is to facilitate information diffusion and technology adoption, and which

Conley and Udry, 2010). Due to the challenges associated with identifying and measuring peer effects (Manski, 1993), recent advances in the literature use experimental techniques to overcome these challenges. A comprehensive overview of studies using field experiments to study social spillover and network effects in developing countries is provided by Breza (2016). Lab experiments investigating peer effects, herding, and informational cascades include Cason and Mui (1998); Falk and Ichino (2006), Anderson and Holt (1997), and Celen and Kariv (2004, 2005).

[2] Different types of leaders have been shown to affect many types of decisions and outcomes at the local level. Examples include the voluntary provision of public goods (Beekman et al. 2014; Jack and Recalde 2015), the conservation of forest commons (Kosfeld and Rustagi, 2015), technology adoption decisions (Miller and Mobarak, 2014), and the diffusion of microfinance (Banerjee et al., 2013).

[3] Extension agents are advisors typically employed by governments or large non-government organizations to provide agricultural assistance to households in rural areas. They are also used for the implementation of other types of development projects, such as HIV/AIDS information campaigns.

[4] A large literature in psychology has studied social conformity and social influence. Important references include Asch (1956), Festinger (1954), and Sherif (1936, 1937). Broader overviews of the literature which include discussions of social norms, compliance, and obedience to authority (Milgram, 1974) are provided by Cialdini (2007) and Cialdini and Goldstein (2004).

[5] Lead farmers are somewhat comparable to the club chairs in our sample, however they are typically systematically identified by the extension organization or government rather than elected by their peers.

[6] In BenYishay and Mobarak (2019) there are 5 peer farmers in each village, 1 lead farmer, and 1 extension agent. However, extension agents are responsible for a minimum of 15 villages, and usually serve many more.

contain both peers and formal and external leaders. As such, we are operating in a relevant real-world environment and leveraging actual interactions and relationships. Moreover, many of the recent advances in the empirical social learning literature have been made by studies that investigate technology adoption in agriculture in rural Malawi, using a similar sample of smallholder farmers and comparing the behavior of similar agents (Beaman et al., 2021; BenYishay et al., 2020; BenYishay and Mobarak 2019). These studies have found that traditional diffusion strategies such as targeting extension agents and lead farmers are not the optimal ways to diffuse technology in agriculture. Our results extend their findings by showing similar results in a more general risk environment where other barriers for diffusion, such as liquidity constraints, are absent and where social image concerns are minimized.

Finally, we show exploratory analysis that seeks to understand what drives the influence of the different movers. This analysis uses orthogonal variation in whether the first mover's choice is implemented (following the design of Bursztyn et al., 2014), and if it is, whether the second mover's outcome is determined by the same coin flip as the first mover or a different one. The treatment in which the first mover's choice is not implemented separates information effects, which include learning and preference conformism, from social utility effects comprised of joint decisions, risk, and payoffs, which require that choices are implemented. Social utility effects include risk sharing and social comparison incentives. When first mover choices are implemented, both information and social utility channels of influence are present. The results suggest that the influence of peers is driven by information effects and risk sharing, while the influence of external and formal leaders is driven by social comparison incentives.

This analysis contributes to a small literature that studies the channels underlying peer effects in risk taking in perfect information environments using lab experiments with anonymous peer interaction (e.g., Cooper and Rege, 2011; Lahno and Serra-Garcia, 2015).[7] Most closely related to our work, Bursztyn et al. (2014) study the purchase of a financial asset in Brazil in a setting where social learning matters. They find that participants are more likely to purchase the asset when informed that a peer intended to purchase the asset, even if they were not allowed to, suggesting that social learning is important. Purchase is even higher when the peer's choice was carried out, showing that social utility motives are also at play. Our work builds on this paper by examining a different context, focusing on intensive margin adjustments, and comparing across first mover types. Additionally, the variation in risk structure allows us to explore whether risk sharing plays a role.

Our work also fits into a broader literature that examines peer effects in financial decisions. For example, Beshears et al. (2015) studies peer effects in retirement savings using a field experiment and finds a negative relationship between peer information and choices, a result that is driven by those who may feel economically constrained. Also relevant is empirical work studying financial trades in online copy trading platforms which allow investors to see the choices and returns of others and to copy their behavior with the click of a button (see for example, Pan et al., 2012; Liu et al., 2014). A principal difference between the decisions made in these platforms and our experiment is that financial decisions in these markets are more complex, and the success of the first mover investors is visible. Recent experimental evidence suggests that copy trading leads investors to follow successful first movers and make overly risky investments (Apesteguia et al., 2020). All of these studies concern financial investment products mostly traded in the developed world, so their relevance to the decisions of smallholder farmers in Malawi may be limited.

The paper proceeds as follows. Section 2 presents the experimental design. Section 3 describes the data and Section 4 presents the main results. Section 5 describes the exploratory analysis on mechanisms and Section 6 concludes.

2. Background and experimental design

Our experiment is designed to study how individual investment decisions are influenced by peers and leaders. A conceptual framework that guides this work is provided in Online Appendix 1. The timing of the experiment is summarized in Fig. 1, and additional experimental details are in Appendix A.

2.1. Background

Our study sample is composed of smallholder farmers that were part of a randomized control trial (RCT) conducted with the National Smallholder Farmers' Association of Malawi (NASFAM) to study the impacts of cash and input transfers and agricultural extension on agricultural production (Ambler et al., 2020). The area in which we work is characterized by high poverty rates and reliance on rain-fed maize farming, though the farmers with whom we work all engage in some cash-cropping.[8]

NASFAM farmers self-organize into clubs which range in size from 3 to 15 members. Within these clubs there are two natural sets of leaders. First, each club elects a club chair to coordinate crop sales and extension assistance, and more

[7] Cooper and Rege (2011) study the extent to which social regret or a taste for conformity drive peer effects in risk taking. Lahno and Serra-Garcia (2015) tease envy apart from conformity by comparing behavior when peers are randomly allocated a choice to when they make it. Bernheim and Exley (2015) and Fatas et al. (2018) are related papers that explore mechanisms underlying peer effects in the laboratory, but do not focus exclusively on risk taking. They study conformism more broadly, as do Goeree and Yariv (2015). All of these studies use university student samples in developed countries and reveal the choices of peers anonymously. Other lab experiments that study peer effects in risk taking but do not tease apart mechanisms are Bougheas et al. (2013), and Gioia (2017).

[8] To see how our sample compares to households in the area more generally, refer to Ambler et al. (2018).

Date	Activity
1 year prior	RCT baseline survey conducted
2 months prior	Randomization (using club membership listings)
5-45 days prior	External leader choices elicited
2-11 days prior	RCT follow-up survey 1 (FU1) conducted
1-3 days prior	Schedule visit
Day of	1. Arrival to community
	2. Simultaneous interview of first movers (peer + club chair)
	3. Enumerators meet to share first mover decisions
	4. Simultaneous interview of second movers
	5. Payment of first movers
1 year after	RCT follow-up survey 2 (FU2) conducted

Fig. 1. Implementation timeline.
Note: Our study sample is composed of smallholder farmers that were part of a randomized control trial (RCT) conducted with the National Smallholder Farmers' Association of Malawi (NASFAM) and evaluated by Ambler, de Brauw and Godlonton (2020). This table describes the timeline of data collection activities, including RCT survey waves. RCT surveys took approximately 3 hours to complete and included questions on many topics and modules, primarily covering a household's agricultural production practices. Club visits were scheduled via phone calls with club chairs and mentioned only a survey. No information about the artefactual field experiment or incentives was provided to respondents prior to each private one-on-one interview. Although our randomization sample included 1,325 farmers, only 1,158 individuals were active members of NASFAM at the time we conducted the study, and only 1,028 participated in the experiment. More attrition information is provided in Appendix B.

generally act as the club's representative for all NASFAM activities. Second, as part of the RCT, each club received agricultural assistance from one NASFAM extension agent explicitly tasked with providing advice to farmers.[9] Additionally, the other club members provide a natural set of peers with whom farmers interact.

2.2. Investment decision

Participants in the study were classified either as first movers or second movers. First movers are those who set an example and had their behavior observed by others. Second movers are participants who observed a choice made by a first mover. Participants of both types received a 1000 MWK endowment in cash in ten 100 MWK notes and had to decide how much, if any, of that endowment to invest in an account that paid four times the amount invested half of the time and zero half of the time. This investment decision is a modification of Gneezy and Potters (1997), and requires participants to invest the entire endowment if they wish to maximize expected earnings.[10] Individuals may find it optimal to invest less than the full endowment if they are sufficiently risk averse, derive utility from social comparisons, imperfectly understand the decision environment, and/or share risks with others. The decision participants make is meaningful: a 1000 MWK endowment was roughly equivalent to the daily wage in the study area at the time we conducted the experiment. Participants were provided full information about payoffs and probabilities when they made their decision. We used scripted protocols and visual aids with menu choices to explain the payoff consequences of every possible amount invested and minimize the role of confusion and error in the study.[11]

We elicited two decisions from participants using a procedure that was slightly different for first and second movers. Fig. 2 outlines the structure of this procedure. Both first and second movers make an initial private investment decision. They are then offered the opportunity to revise their decision as a surprise. Before they revised their decision, *first movers* were informed that their revised decision would be shared with some members of their club. Depending on their treatment group, *second movers* were provided with information about a certain first mover decision prior to revising their choice. All participants knew that their revised decision was the final one that would determine earnings. The first mover choice was revealed to second movers using the first name and last name of individuals, without labels indicating treatment status.

This two-step procedure was used to elicit decisions for several reasons. First, the initial decision provides a benchmark for what investment would look like in the absence of any revision or information about other participants (within-subject control treatment). Second, there is a large degree of heterogeneity in risk preferences that should determine how respon-

[9] As part of the RCT some farmers received standard extension services and some received intensive services. These services were provided by 15 extension agents, each assigned to a geographical cluster of clubs.

[10] A similar setup is used by Jakiela and Ozier (2016) to study social pressure to share income with kin and neighbors in rural villages in Kenya.

[11] See the conceptual framework provided in Online Appendix 1 for theoretical details about the decision environment. Refer to Online Appendix 2 for the experimental scripts we used and to Online Appendix 3 for the visual aids.

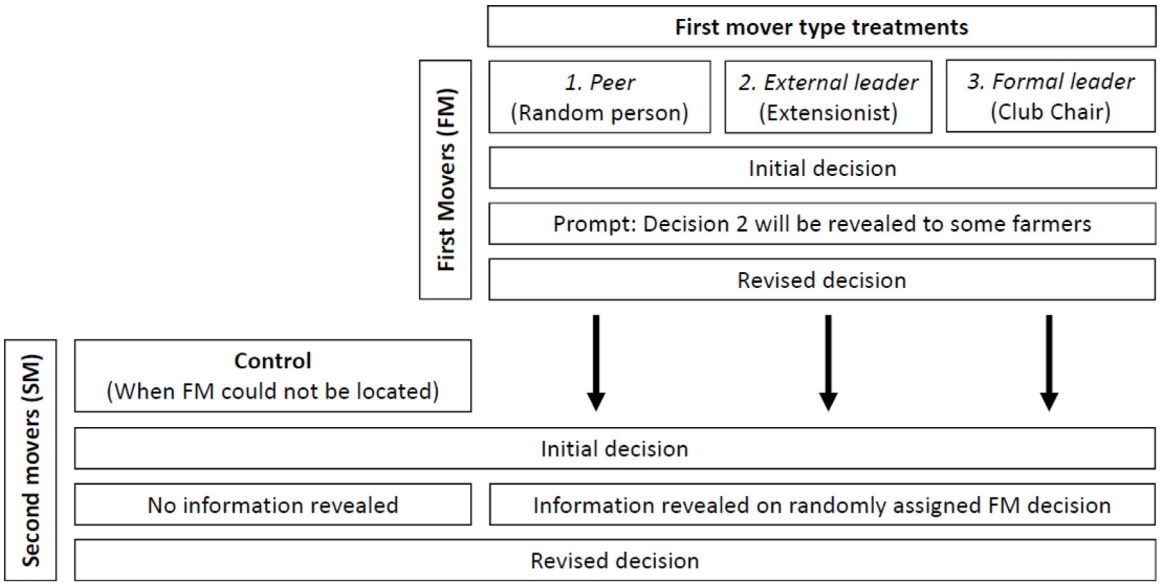

Fig. 2. Structure of the incentivized decision.

sive people are to the example set by others. Third, this design choice maximizes statistical power (McKenzie, 2012).[12] A chronology of the elicitation of decisions is presented in Fig. 1. First mover decisions were elicited first, enumerators then met to share the information on first mover choices, and then the second mover decisions were elicited.[13] Second movers learned the outcome of the coin flip that determined the return on their investment immediately following their revision decision and were subsequently paid. Finally, first movers learned their return on investment and were paid.[14] Each interview took between 20 and 30 min to complete.

We rotated which enumerator was assigned to elicit the choices made by the different first and second movers. We also controlled the flow of information during interviews in several ways. First, we elicited decisions simultaneously and in private at each participants' home. Second, we collected data from an entire club in a few hours, and targeted several clubs located in close geographical proximity during the same day. Third, we had a field coordinator present in a centrally located village to provide logistical support and avoid any interview interruptions that may otherwise have happened. Fourth, opportunity for cell phone communication was limited due to poor signal, and low ownership and phone use. Appendix A provides additional implementation details.

2.3. First mover type treatments

The goal of this paper is to understand whether and how people are differently influenced by peers, leaders from within the community, and external leaders. To address this question, we varied the identity of the first mover using a between-subject design with three treatments.

- *Peer treatment:* Second movers learned the choice made by a randomly selected peer from their club (who was never the club chair).
- *Formal leader treatment:* Second movers learned the choice made by their club chair.

[12] While one may be concerned about experimenter demand effects generated by this design choice, it is a priori unclear whether two consecutive decisions provide a lower or upper bound of the social influence we can observe in this experiment. If participants anchor their choices based on their first decision, then they may be less influenced than they would be if we had provided the information before they made a first decision. On the other hand, if participants think that we want them to be influenced by others, they may overreact to social information. We did everything possible to minimize experimenter demand effects in the study, but nevertheless assume that any anchoring or experimenter demand effects present are constant across treatments and do not affect the internal validity of our results.

[13] Our enumerators accidentally mixed up the first mover amounts in 3 clubs (12 cases). This resulted in 12 second movers being shown the wrong amount. Our results are consistent if we drop these 12 respondents from our analysis.

[14] Because Okeke and Godlonton (2014) find, in the context of a field experiment in Nigeria, that pro-social preferences led enumerators to deviate from field protocols relying on the roll of a die to determine treatment assignment, the coin toss used to determine return on investments was simulated electronically and could not be manipulated by the enumerator.

- *External leader treatment:* Second movers learned the choice made by the extension agent who worked with them on a regular basis as part of NASFAM activities. Extension agents made a separate revised choice for each club with which they worked. The choices of extension service workers were elicited first, in a meeting organized for this purpose.[15]

Treatment randomization was conducted by a computer prior to implementation of the experiment. All treatment assignments were preloaded into the tablets used to conduct the experiments. Second movers were randomized at the individual level into a treatment group, stratified by second mover gender and club.

Some of the first movers were unavailable when we visited them to conduct the experiment. To address this, we randomly selected a replacement peer first mover at the randomization stage. The replacement peer made decisions as a first mover if the original peer could not be located. In the cases where no first mover information was available, we simply allowed farmers to revise their decision without providing any additional information. These cases provide us with a quasi-random control group that can be used in our analysis.[16]

The randomization of first mover type allows us to test the null hypothesis that famers respond equally to social influence from different types of actors. We can examine whether revision decisions and the response to the first mover decision vary across different types of first movers. Online Appendix 1 presents a conceptual framework that describes in detail the decisions made by second movers in the experiment and describes how influence may vary across first mover types. Based on BenYishay and Mobarak (2019) we would expect peers may be more influential than extension agents. However, other literature on the importance of leaders and their location in the social network (e.g. Banerjee et al., 2013; Miller and Mobarak, 2014), indicates that we may expect peers to be less influential than leaders. As such the direction of these differences remains an empirical question. The quasi-random control group also allows us to differentiate social influence of any type (peer effects) from revision due to something else, such as deliberation. A second orthogonal randomization, affecting the risk structure experienced by second movers will allow for exploratory analysis of the channels of social influence and will be described in Section 5.

3. Data

3.1. Sample description

All farmers who were registered members of the 122 clubs participating in the RCT at the time of the first follow-up survey are included in our sample.[17] A total of 1028 individuals participated in our experiment: 110 peer first movers, 94 formal leaders, 14 external leaders, and 810 second movers. Among the second movers, 239 are in the peer group, 246 in the external leader group, 209 in the formal leader group, and 116 in the quasi-random control group. There were two main types of treatment reassignment. The first type occurred when a peer first mover could not be interviewed in time and a replacement peer who was initially intended to be a second mover became a first mover. This occurred for 17 peer first movers, associated with 39 second movers.[18] The second source of treatment reassignment occurred when a first mover other than a peer could not be interviewed, or both the peer first mover and the replacement could not be found in time, and second movers matched to that first mover could thus not be treated, resulting in the administration of the control treatment. Of those in the control group, 25 were initially assigned to be second movers in the peer group, 16 in the external leader group, and 62 in the formal leader group.[19,20]

Table 1 presents the mean characteristics of first and second movers. Column 1 shows summary statistics for all second movers, and columns 2 through 4 for first movers by type. Because external leaders are not part of the RCT sample, they were interviewed separately using an abbreviated survey instrument. We also include the p-value associated with the test that group means of second movers and peer first movers are equal. Across all characteristics second movers and peer first movers are similar. By design, external and formal leaders are different than second movers and peers, and as such we do not report statistical tests on the differences between these groups. External leaders are less likely to be female, are younger,

[15] One concern is that the treatments may be differentially diluted because participants may be more likely to recognize the name of one type of first mover than another. Using survey data collected one year after the experiment we can verify that there are no such differences; between 94 and 95 percent of participants report knowing the different types of first movers.

[16] Although one might worry about selection problems affecting the types of first movers we observe in our data, the fact that second movers were randomized into the first mover type treatments in advance ensures that this quasi-random control treatment is free of selection problems for second movers.

[17] Additional details pertaining to the sample frame and attrition are discussed in Appendix B. Appendix Table B.1 provides detail on the number of participants (first and second movers) by first mover type.

[18] Appendix Table 1 tests whether the replacement peer first movers are systematically different with respect to observable characteristics from those peer leaders who were initially assigned as first movers and were interviewed. Replacement peer first movers are similar to the peer first movers though they are less likely to be female and to have only some primary schooling. Our main results are robust to excluding second movers that observed a decision made by a replacement peer first mover.

[19] There are also few cases of reassignment of first movers to the control group. This occurred for 7 peer first movers, who were found but not in time to have their decisions revealed to second movers, and for 6 formal leaders who were no longer club chairs when we conducted the experiment. Both the former club chairs and their associated second movers were administered the control treatment in this last scenario.

[20] Appendix Table 2 examines whether those first movers who were administered their pre-assigned treatment differ from those who were not. In general, the two groups are similar with a few exceptions. These differences do not affect the internal validity of our experiment but do affect the composition of the first mover group.

Table 1
Differences in participant characteristics.

		First movers			
	Second movers $N = 810$ (1)	Peer $N = 110$ (2)	External leader $N = 14$ (3)	Formal leader $N = 94$ (4)	*t*-test p-value: (1)=(2) (5)
Demographics and Household Characteristics					
Female	0.663	0.591	0.429	0.516	0.120
	(0.473)	(0.494)	(0.514)	(0.502)	
Age	42.019	40.818	26.429	44.077	0.391
	(14.162)	(13.440)	(3.458)	(11.772)	
No schooling	0.189	0.173	0.000	0.099	0.667
	(0.392)	(0.380)	(0.000)	(0.300)	
Some primary schooling	0.563	0.509	0.000	0.495	0.309
	(0.496)	(0.502)	(0.000)	(0.503)	
Completed at least primary schooling	0.248	0.318	1.000	0.407	0.142
	(0.432)	(0.468)	(0.000)	(0.494)	
Household size	5.630	5.427	1.692	6.000	0.322
	(2.172)	(1.923)	(0.855)	(2.011)	
Land owned	3.781	3.724	1.104	4.200	0.812
	(3.339)	(2.005)	(1.282)	(1.862)	
GVAO (in USD)	576.480	526.619		624.909	0.481
	(1234.511)	(564.603)		(495.404)	
GVAO p/acre (in USD)	127.338	122.210		119.871	0.636
	(209.106)	(107.927)		(82.022)	
Value of assets (in USD)	118.389	118.098		187.171	0.991
	(206.848)	(251.183)		(279.744)	
Social Status					
Absolute status rank*		0.500	0.227	0.203	
		(0.168)	(0.112)	(0.154)	
Number of leadership positions		0.491	0.000	1.277	
		(0.763)	(0.000)	(0.932)	
Social Network Centrality					
Absolute friend rank*		0.411	0.589	0.274	
		(0.143)	(0.093)	(0.135)	
Frequently see*		1.962	3.798	1.674	
		(0.878)	(0.796)	(0.617)	

Note: GVAO stands for gross value of agricultural output. *indicates that a variable is constructed using answers provided by members of the NASFAM club. Ranks are normalized by club size such that values range from 0 to 1, where a lower value indicates a higher ranking. Frequently see uses a scale ranging from 1 to 6, where 1=every day, 2=multiple times per week, 3=once per week, 4=several times per month, 5=once per month, 6=less than once per month.

more educated, have smaller households, and own less land than peers, formal leaders, and second movers. Formal leaders are older than peers and second movers, are more educated, have larger households, and own more land and assets. Formal leaders also produce more, a pattern driven by land holdings, not productivity.

In Table 1 we also report measures of first mover social status and social network centrality, collected in a survey conducted one year after this experiment. Participants ranked all club members (including the extension agent) by: (1) who is most highly regarded in the community (social status); and (2) who is their closest friend (network centrality). Average rankings provided by other club members are computed for each first mover and are normalized by club size. We also collect a self-reported history of past leadership positions (social status), and club members were asked how frequently they see other members and the extension agent (network centrality).

Examining the measures of social status, formal leaders occupy more leadership positions than peers. Additionally, formal and external leaders rank higher than peers in terms of their social status: formal leaders are ranked in the top 20.3 percent of club members in terms of how highly regarded they are by club members, external leaders in the top 22.7 percent, and peers on average in the middle of the distribution. The two measures of network centrality show that formal leaders are more central in the social network than peers, who are in turn more central than external leaders. This is expected given that extension service workers do not live in the same communities as the club members they work with. In sum, the survey data shows that formal leaders have higher social status than peers and are more central in their networks. External leaders also have higher status based on the one measure that is relevant for them (status rank).

Appendix Table 3 provides evidence that second mover characteristics are largely balanced across first mover type treatments. Even though we find no evidence that group means systematically differ, our analysis will present estimates with and without individual-level controls. The table also tests whether second movers in the quasi-random control group are similar to second movers who were not re-assigned (i.e. the rest of the second movers), and we find no evidence that the control group is systematically different than the treatment groups.

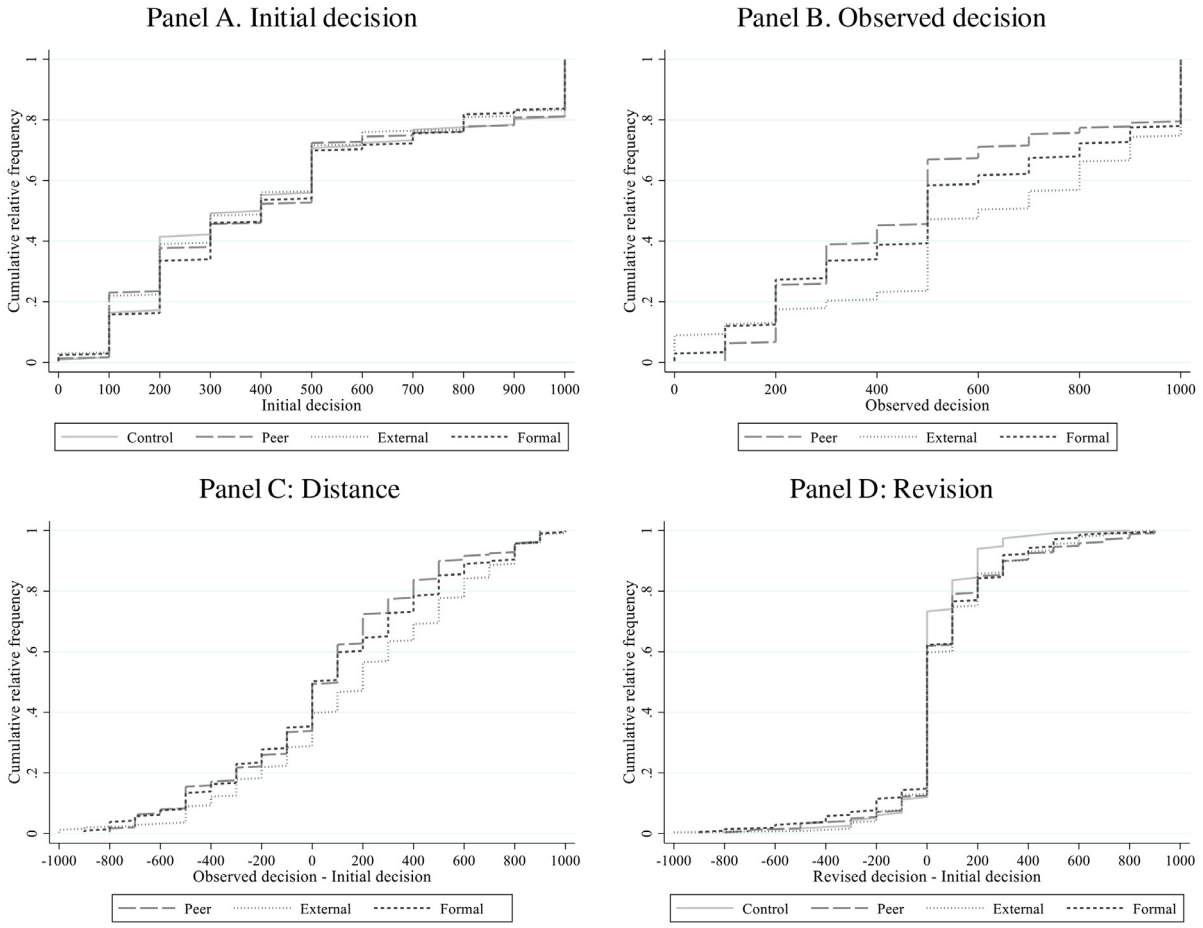

Fig. 3. CDFs of investment decisions.
Note: The distributions of investment decisions are not statistically different in Panels A and D. Differences in Panels B and C are statistically significant; Kolmogorov Smirnov $p<0.05$ when comparing the external leader treatment to the other two types of first mover treatments, Kruskal-Wallis $p<0.01$ when conducting omnibus tests across all treatments within each panel.

3.2. Investment decisions

Before moving to our main empirical models, we summarize the investment decisions made by both first and second movers, presenting cumulative distribution functions in Fig. 3. Panel A shows the second mover initial decision, separately by treatment group. As expected, there are no differences in initial investment decisions by first mover type.[21] Second movers invest on average 456 MWK (median 400 MWK) in their initial decision. Furthermore, only 18% of second movers initially invest their entire endowment across all treatments. Panel B presents the distribution of the revised (second) investment decisions made by first movers and observed by second movers, referred to hereafter as the "observed decision."[22] The observed decisions were largest in the external leader treatment, followed by the formal leader treatment, and finally the peer treatment (mean external=623 MWK, mean formal=548 MWK, mean peer=514 MWK). Panel C shows the distance of the observed decision from the second mover initial decision by treatment group, referred to hereafter as the "distance." Second movers are responding to decisions which are below, equal to, and above their initial choices. The average distance is 55 MWK in the peer group, 82 MWK in the formal leader group, and 179 MWK in external leader group. The averages of the absolute value of these differences are 315 MWK, 349 MWK, and 380 MWK in the peer, formal leader, and external leader groups respectively. This data indicates that controlling for both the initial decision made by second movers and the

[21] In results not shown we find that none of the leader characteristics presented in Table 1 consistently predict initial decisions.

[22] Appendix Fig. 1 shows how different leaders revise their decision when they know it will be observable. On average, peer and formal leader first movers revise their decision upwards, while external leaders revise their decision downwards. Formal leader first movers are least likely to revise their decisions, while external leader first movers are most likely—but they also make a different revision decision for each club they work with.

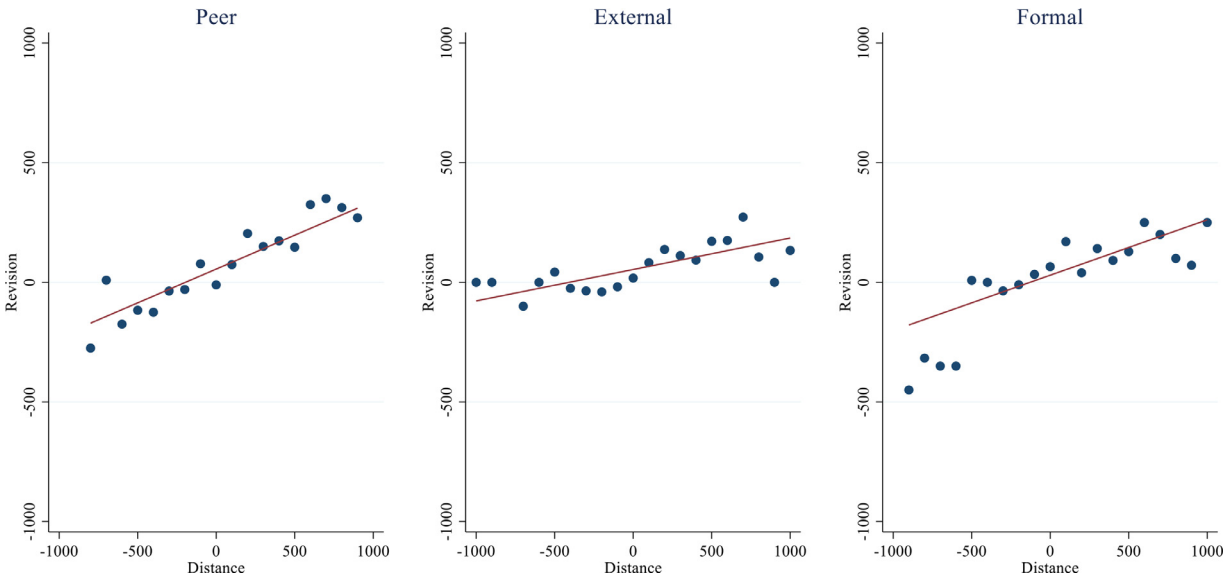

Fig. 4. Revisions as a function of the distance from the observed decision.
Note: Binned scatter plot of the raw data. Each graph plots the mean revision per distance value. Distance is defined as the observed decision minus the initial decision, while revision is defined as the revised decision minus the initial decision. The slope of the revision function is positive in all cases: 0.2827 for Peer first movers, 0.1314 for External leaders, and 0.2314 for Formal leaders. Tests of equality in slopes with standard errors clustered at the club level reveal significant differences between the Peer first mover and External leader treatments ($p = 0.015$), but not between the External and Formal leader treatments ($p = 0.130$) or between the Peer and Formal leader treatments ($p = 0.392$).

first mover choice is important if we want to understand how and why second movers respond to the observed choice made by first movers.

Panel D plots the CDF of the second mover revision (revised decision minus initial decision) in each treatment, referred to hereafter as the "revision." This figure shows that a large fraction of second movers do not revise their decision in the experiment (62 percent in the control treatment and 48 percent in the three other first mover treatments). The CDF also shows that exposure to the behavior of others causes second movers to revise their investment decisions up on average in all first mover treatments (mean revision control=31 MWK, mean revision all first mover treatments=67 MWK). The behavior observed in the three first mover type treatments cannot be explained by the fact that participants make two decisions in our experiment, otherwise, we should not see differences between the distribution of revisions for first mover type treatments and the control treatment.

We also examine the relationship between the distance (as shown in Fig. 3, Panel C) and the revision (as shown in Fig. 3, Panel D) using binned scatterplots which plot the mean revision per distance value, separately by treatment. These scatterplots are shown in Fig. 4 and show that second movers do tend to react to the decision made by first movers, and suggest that the relationship is largely linear.

4. Results

4.1. Empirical strategy

We now turn to the regression analysis that we will use to examine our primary research questions. To compare the behavior of second movers in the randomly assigned first mover type treatments to the quasi-random control treatment, we estimate the following model using ordinary least squares:

$$revision_{ic} = \beta_0 + \beta_1 Peer_{ic} + \beta_2 External_{ic} + \beta_3 Formal_{ic} + \beta_4 d_{ic} + \gamma_e + \delta_c + X'\theta_{ic} + \epsilon_{ic} \quad (1)$$

where i indexes individuals, c indexes clubs and e indexes enumerators. Our primary outcome is the revision, defined as the revised investment decision minus the initial investment decision of second mover i in club c. $Peer_{ic}$, $External_{ic}$, and $Formal_{ic}$ are indicator variables for the different treatment groups. d_{ic} is the initial decision made by each second mover, γ_e are enumerator fixed effects and δ_c are RCT treatment fixed effects. We include the initial decision in the model because the extent to which participants may want to revise their decision and their ability to do so (both in direction and magnitude) is determined partly by their first decision. The initial decision also provides a control for the risk preferences of each participant. We present results with and without controls, represented by vector X including indicator variables for gender, level of completed schooling, age, and household size. Standard errors are clustered at the club level. Our preferred outcome of interest is the revision, because it captures the size of the response to the information provided through the revelation of

Table 2
Second mover decisions by first mover type treatment.

	Dependent variable =					
	Revised		Revised decision		Revision	
	(1)	(2)	(3)	(4)	(5)	(6)
Peer	0.125**	0.115**	41.510*	40.332*	41.510*	40.332*
	(0.049)	(0.050)	(24.359)	(23.174)	(24.359)	(23.174)
External	0.148***	0.148***	44.431*	46.121**	44.431*	46.121**
	(0.053)	(0.053)	(23.208)	(22.306)	(23.208)	(22.306)
Formal	0.138**	0.133**	17.965	19.096	17.965	19.096
	(0.057)	(0.057)	(26.202)	(25.145)	(26.202)	(25.145)
Decision 1	−0.000***	−0.000***	0.739***	0.737***	−0.261***	−0.263***
	(0.000)	(0.000)	(0.026)	(0.026)	(0.026)	(0.026)
P-values from the following tests:						
Peer=External	0.609	0.462	0.888	0.782	0.888	0.782
Peer=Formal	0.735	0.660	0.351	0.401	0.351	0.401
External=Formal	0.849	0.762	0.264	0.252	0.264	0.252
Mean control	0.379	0.379	490.517	490.517	31.034	31.034
N	810	810	810	810	810	810
Includes:						
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
RCT controls		Yes		Yes		Yes
Individual controls		Yes		Yes		Yes

Note: Revision = revised decision - initial decision. Standard errors in parentheses. *$p<0.1$, **$p<0.05$, and ***$p<0.01$.

the first movers' investment decision. We also present results for two alternative outcomes: the revised decision itself and an indicator variable equal to 1 if the second mover's second decision is different from their first decision. Estimates of β_1, β_2, and β_3 are mechanically the same when the dependent variable is the revised decision rather than the revision.

Eq. (1) allows us to analyze whether second mover decisions are differentially impacted by receiving information about first mover choices, relative to the quasi-random control group and to one another. However, these comparisons do not hold constant the information provided to second movers across the different treatment groups because the distributions of investment decisions made by first movers differs (Fig. 3B and 3C). Instead, they capture the combination of differential response per additional dollar invested by the first mover and the higher or lower average investment decisions made by first mover types. Thus, we additionally analyze how individuals respond to the amount invested by first movers across the different treatment arms, holding constant the average level of first mover investment. This specification does not allow us to use the control group but does allow us to compare how farmers would respond to the same investment decision if it were made by a peer, formal leader, or external leader. Specifically, we estimate the following model separately for each first mover type:

$$revision_{ic} = \beta_0 + \beta_1 (obs_{FM} - d)_{ic} + \beta_2 d_{ic} + \gamma_e + \delta_c + X'\theta_{ic} + \epsilon_{ic} \quad (2)$$

$(obs_{FM} - d)_{ic}$ measures the difference between the decision second mover i observes (observed decision) and their initial investment decision. All other notation is as in Eq. (1).[23] For this set of results, we restrict our analysis to the revision as our outcome of interest. β_1 is thus a measure of how the second mover's decision changes with the distance from the observed decision. Our analysis will also test for equality of β_1 across first mover types, obtained from the equivalent fully interacted, pooled model. As in the previous model, estimates of β_1 are mechanically the same if the revised decision is instead used as the outcome variable. We show this in robustness checks in Appendix Table 4 (columns 7 and 8).

4.2. Regression results

We first examine whether and to what extent each of the three types of first movers influence the decisions of others, estimating Eq. (1). The results are presented in Table 2. Three outcomes are considered: do individuals revise their decision (columns 1 and 2), the revised investment decision made by individuals (columns 3 and 4), and the size of the revision (columns 5 and 6). Columns 1 and 2 show that participants are more likely to revise their decisions in all three treatment groups than in the control group, which is the omitted category. This allows us to reject the possibility that revisions are due only to the fact that participants make two consecutive decisions. At the bottom of the table, we present the p-values for the tests that the coefficients on the different first mover types are equal. Across all three comparisons we find no economically or statistically significant differences in the probability of revision.

[23] As in model 1, we control for the initial decision because participants' revision choices will be determined partly by their initial decision and because the initial decision serves as a control for personal risk preferences.

Table 3
Second mover response to the observed decision by first mover type.

	Dependent variable = Revision					
	Peer		External		Formal	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from observed decision	0.233***	0.246***	0.045	0.054	0.156***	0.160***
	(0.062)	(0.063)	(0.047)	(0.047)	(0.056)	(0.055)
Decision 1	−0.107*	−0.106	−0.193***	−0.195***	−0.163***	−0.138**
	(0.063)	(0.065)	(0.055)	(0.057)	(0.058)	(0.060)
P-values from the following tests:						
Peer X dist. = Ext X dist.			0.015	0.015		
Peer X dist. = Formal X dist.					0.316	0.267
Ext X dist. = Formal X dist.					0.157	0.174
N	239	239	246	246	209	209
Includes:						
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
RCT controls		Yes		Yes		Yes
Individual controls		Yes		Yes		Yes

Note: Revision = revised decision - initial decision. P-values from tests between treatments are obtained by estimating a fully interacted, pooled model which is presented in Appendix Table 4. Standard errors in parentheses. *$p<0.1$, **$p<0.05$, and ***$p<0.01$.

Columns 3 and 4 show how the revised second mover decision varies by first mover type. Second movers in the peer treatment group invest approximately 40 MWK more than the control group, which represents an 8 percent relative increase. The external leader treatment generates a similar response, the coefficient is 46 MWK (9 percent relative to the control treatment), while the formal leader treatment generates a smaller response of 4 percent that is not statistically different from the amount invested by participants in the control treatment.

Columns 5 and 6 present the results using the size of the revision as the outcome. Because the specification controls for the first decision, the estimated coefficients are the same as in columns 3 and 4. However, the coefficients on the first mover types in these regressions represent the difference in the average revision in that group relative to the average revision in the control group. We find that the size of the revision increases relative to the control treatment by 130 percent in the peer treatment, 149 percent in the external leader treatment, and by 62 percent in the formal leader treatment (not statistically different from zero). Nevertheless, the Wald tests presented at the bottom of Table 2 show that the differences between first mover treatments are not statistically distinguishable in any specification.

We next examine how individuals respond to the distance between their initial decision and the observed choice of the first mover. This allows us to examine the relative influence of the first mover types for the same investment decision. Table 3 presents estimates of Eq. (2) by first mover type. Estimates for peers are in columns 1 and 2, external leaders in columns 3 and 4, and formal leaders in columns 5 and 6. P-values for the test that the response to this distance is equal across treatment groups are reported at the bottom of the table. Estimates of the equivalent fully interacted pooled model used to compute these p-values are presented in Appendix Table 4 (columns 1 and 2). In the peer group, we find that, on average, for every 100 MWK increase in the difference between the second mover's initial decision and the investment decision of the first mover, second movers increase their investment by 24.6 MWK in the specification with control variables. Columns 3 and 4 show that second movers do not respond to the observed decision in the external leader treatment, coefficients are small and not statistically different from zero. Finally, we find that second movers respond to the decisions made by formal leaders; they increase their investment by approximately 16 MWK for every 100 MWK increase in the difference variable (Column 5 and 6). This response is smaller than the response in the peer group, but the difference is not statistically significant. The response to peers is however statistically different than the response to external leaders. The difference in response between external and formal leaders is economically significant, but not statistically significant at conventional levels (p-value = 0.157 or 0.174 depending on the specification).

These results show that peer first movers appear to be the most influential, followed by formal leaders (though we cannot reject that they are equal). The influence of external leaders is not statistically distinguishable from zero in this specification. These results vary from the results presented in Table 2 which suggest that external leaders are *more* influential than formal leaders. The results in Table 2 show the combined effect of the first mover identity and the endogenous first mover decision, while the results in Table 3 isolate the impact of identity holding constant first mover decisions. Because the distance between the second mover initial decision and the observed decision is on average greater for the external leader treatment group, the larger revision observed in Table 2 does not translate to the distance between the second mover decision and the observed choice falling as much as it does for the formal leaders and peers.

These results are robust to several specification checks which are presented in Appendix Table 4. First, these results may be driven by club characteristics that are correlated with the second mover responses. To address this we control for club

Table 4
Second mover response to the observed decision by first mover type and characteristic.

	First mover characteristic=									
	None	Age	Female	No education	Household size	Land size	Absolute status rank	Absolute friend rank	Frequently see	# Leadership positions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance from observed decision	0.214***	0.191**	0.237***	0.202***	0.160*	0.251***	0.267**	0.387***	0.195**	0.206***
	(0.051)	(0.096)	(0.060)	(0.057)	(0.096)	(0.081)	(0.102)	(0.095)	(0.077)	(0.056)
Decision 1	−0.149***	−0.150***	−0.143***	−0.154***	−0.154***	−0.151***	−0.149***	−0.147***	−0.147***	−0.150***
	(0.035)	(0.035)	(0.037)	(0.036)	(0.036)	(0.035)	(0.035)	(0.036)	(0.035)	(0.035)
External	6.608	6.803	3.570	2.415	31.827	−3.759	20.237	4.476	−14.146	6.975
	(18.902)	(21.368)	(19.985)	(20.442)	(29.361)	(27.148)	(31.686)	(21.991)	(32.207)	(19.536)
Formal	−23.732	−23.124	−26.576	−25.488	−24.840	−22.684	−8.428	−21.403	−21.786	−23.592
	(25.796)	(26.134)	(26.157)	(25.387)	(26.182)	(26.507)	(39.218)	(28.403)	(26.114)	(31.539)
Ext X dist.	−0.146**	−0.138**	−0.146**	−0.134**	−0.113	−0.182**	−0.175**	−0.087	−0.181*	−0.137**
	(0.058)	(0.062)	(0.060)	(0.062)	(0.090)	(0.088)	(0.081)	(0.064)	(0.099)	(0.064)
Formal X dist.	−0.048	−0.049	−0.054	−0.045	−0.049	−0.044	−0.078	−0.107*	−0.049	−0.062
	(0.060)	(0.059)	(0.061)	(0.064)	(0.061)	(0.061)	(0.084)	(0.061)	(0.057)	(0.070)
FM characteristic		−0.020	−15.922	−23.263	6.506	−2.927	46.947	9.097	10.574	−0.447
		(0.900)	(19.929)	(35.654)	(5.202)	(6.192)	(80.666)	(73.759)	(12.610)	(15.131)
FM charac X dist.		0.001	−0.027	0.062	0.010	−0.011	−0.092	−0.381**	0.015	0.017
		(0.002)	(0.053)	(0.139)	(0.016)	(0.020)	(0.187)	(0.184)	(0.036)	(0.035)
P-values from the following tests:										
Peer X dist. = Ext X dist.	0.014	0.028	0.016	0.033	0.209	0.042	0.033	0.180	0.071	0.034
Peer X dist. = Formal X dist.	0.427	0.409	0.383	0.487	0.425	0.475	0.356	0.079	0.391	0.376
Ext X dist. = Formal X dist.	0.126	0.153	0.146	0.139	0.552	0.171	0.127	0.781	0.157	0.403
N	694	694	694	687	694	694	694	694	692	694
Includes:										
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
RCT controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Dependent variable is Revision. Revision = revised decision - initial decision. Standard errors in parentheses. *$p<0.1$, **$p<0.05$, and ***$p<0.01$.

fixed effects, and our findings remain unchanged (Appendix Table 4, columns 3 and 4).[24] Second, because the distribution of first mover choices varies by first mover type, a lack of common support across the three treatments could be driving our results. We note that the area of common support across leader types covers 98 percent of the observations. We also restrict analysis only to the area of common support and find similar results (Appendix Table 4, columns 5 and 6). Third, our results are not driven by a mechanical correlation between the magnitude of the revision, our main dependent variable of interest, and the distance from the observed decision. To demonstrate this, we use the revised decision as an alternative outcome variable (Appendix Table 4, columns 7 and 8). As noted in Section 4.1 and in our discussion of Table 2, the coefficients of interest are in fact mechanically the same as when using the revision (columns 1 and 2). We also show that our results are not driven by strong non-linearities in the relationship between the distance and the revision, as demonstrated in Fig. 4. Finally, Fig. 4 also shows that an alternative model which does not control for the initial decision made by second movers reveals the same comparative statics as the results displayed in Table 3. [25]

Our results suggest that the identity of the first mover matters. However, because different types of first movers have different characteristics by design, it is interesting to consider whether the differential influence is attributable to the individuals' type or to these differences in characteristics across types. To explore this question, we examine a pooled model and sequentially control for different first mover characteristics and interact this characteristic with the distance variable. These results are presented in Table 4. For presentational purposes, we show results only with individual controls. We limit the set of characteristics examined to those characteristics which are available for all three first mover types. Importantly, we find our main results to be quite consistent when adding these additional controls. For the most part, variation in these first mover characteristics does not appear to drive revision behavior. There is one notable exception, absolute friendship

[24] We do not include club fixed effects in our preferred specifications because there is orthogonal variation in channel treatments (discussed and analyzed in Section 5). In Appendix C we briefly describe how results vary by gender and exposure to intensive extension. We find few differences.

[25] Because Table 3 presents our main results, we also address the issue of multiple hypothesis testing by calculating sharpened q-values to control for the false discovery rate for the results in Table 3 (Anderson 2008). We calculate these separately for the model without and with controls and consider the following six tests: the coefficient on the distance is equal to zero for each first mover type and the three tests for whether these coefficients are different from each other. In all cases the sharpened q-values fall within the same category of significance as the corresponding p-value.

rank (column 8). Recall that this is a measure of social network centrality. All participants ranked the club members and extension officers, indicating the relative closeness of their friendships. This measure is the average of the rankings of all participants in the group of that first mover. A lower value indicates that the individual is more socially central within the club. In other words, the lower the value, the more "popular" the first mover. Our results indicate that individuals that are more socially central induce a larger response by the second movers. In general, being more "popular" appears to make first movers more influential. However, this result does not drive the pattern of differential responses across first mover types, which remains even in this specification. In analysis not shown, we also control for all first mover characteristics jointly and find consistent results.

In our setting peer effects can play an important role in investment decisions, even though we study a perfect information environment and social image channels of influence are minimized. Importantly for policy makers we find that peers appear to be the most influential, while external leaders, whose job it is to provide advice to farmers, are the least. Formal leaders fall in between, and we cannot reject that they are equal to peers or external leaders. This is consistent with BenYishay and Mobarak (2019), who find that peers are more influential than extension agents and lead farmers when they receive incentives to spread information, but is extended to a more general risk environment where the intensity of influence is constant across treatments. Our work suggests that looking within communities for people, and not necessarily leaders, to spread advice may be an effective strategy to influence behavior in environments that involve risk.

5. Channels of social influence

Though our study design limits social learning and shuts down social image considerations, two different channels may still drive peer effects, and these channels may vary by first mover type.[26] The first is information; people observe the actions of others and may condition their behavior on that information. Information effects include learning about social norms, social learning due to imperfect understanding of payoffs and probabilities, and imitation due rational inattention, heuristic thinking, and preference conformism. The second is social utility, or effects that are driven by preferences over joint decisions, risk, and payoffs. Social utility includes both risk sharing and social comparison incentives. Due to sample size limitations, this analysis is considered exploratory.[27]

5.1. Experimental variation in risk structure

We implemented three treatments designed to differentiate between these channels of influence:

- *Pure information:* First movers made an investment decision, but their choice was not carried out (by chance) and they instead kept their endowment. Second movers in this treatment learned both the intended choice of the first mover, and that the choice was not realized. This treatment follows the methodology used in Bursztyn et al. (2014). Second movers extract information from the intended choice of another person but cannot derive utility from experiencing the same risk or outcome, isolating information effects as the only channel of social influence.
- *Idiosyncratic risk (IID)*: The investment made by the first mover was carried out. Second movers learned the first mover investment choice and were informed that different coin flips would determine the outcome for the second mover and the first mover. Social utility motives are present through social comparison incentives, because second movers can derive social utility by experiencing the same risk and/or outcome as the first mover.
- *Perfectly correlated risk (PCR)*: The investment made by the first mover was carried out. Second movers learned the first mover investment choice and were informed that the same coin flip would determine the outcome for the first mover and the second mover. In addition to the presence of social comparison incentives, the PCR treatment also allows us to identify if risk sharing is an important form of social utility underlying first mover influence. If second movers share risks with the first mover, they may insure against the possibility that both receive a negative shock by negatively responding to the example set by first movers in the PCR treatment.[28]

We test for the differences between these channel treatments by estimating Eq. (2) separately for each channel treatment and first mover type. If the response to first mover decisions is positive in the pure information treatment, then information effects are important. If the response in the IID treatment is larger (smaller) than that in the pure information treatment, then there is a positive (negative) social utility effect present when risk is idiosyncratic. Finally, if the second mover response in the PCR treatment is positive and larger than in the IID treatment, we ascribe that to stronger positive social utility from social comparisons generated by the perfectly correlated risk structure. If, however, the response in the PCR treatment is smaller than in the IID treatment, we take that as evidence that risk sharing is important.

First movers (including the extension officers) were randomized into a channel treatment, stratified by first mover type and treatment group from the RCT. Extension workers received a different channel treatment assignment for each club for which they made a revision decision. Second movers experience the channel treatment of their assigned first mover. Balance

[26] See Online Appendix 1 for a theoretical exposition of these channels and possible confounders.

[27] There was a high degree of attrition relative to the randomization sample. See Appendix B.

[28] Since social utility from social comparisons is likely stronger in the PCR than in the IID treatment, given that second movers experience the same luck as first movers, risk sharing is identified only if it outweighs any positive social utility effects generated by the perfectly correlated risk structure.

Table 5
Second mover response to the observed decisions by channel type.

Panel A - Peer	Pure Information		IID		PCR	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from observed decision	0.292**	0.244	0.326***	0.362***	0.058	0.061
	(0.129)	(0.164)	(0.091)	(0.099)	(0.083)	(0.079)
Decision 1	−0.083	−0.145	−0.193	−0.133	−0.114	−0.099
	(0.124)	(0.154)	(0.119)	(0.110)	(0.091)	(0.091)
P-values from the following tests:						
Info X dist. = IID X dist.			0.444	0.534		
Info X dist. = PCR X dist.					0.036	0.318
IID X dist. = PCR X dist.					0.001	0.018
N	82	82	68	68	89	89
Panel B - External leader	Pure Information		IID		PCR	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from observed decision	−0.009	0.021	0.044	0.079	0.168**	0.102
	(0.053)	(0.049)	(0.103)	(0.097)	(0.072)	(0.081)
Decision 1	−0.279***	−0.233***	−0.143	−0.141	−0.110	−0.201*
	(0.083)	(0.083)	(0.101)	(0.100)	(0.069)	(0.111)
P-values from the following tests:						
Info X dist. = IID X dist.			0.847	0.592		
Info X dist. = PCR X dist.					0.237	0.392
IID X dist. = PCR X dist.					0.243	0.855
N	85	85	85	85	76	76
Panel C - Formal leader	Pure Information		IID		PCR	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from observed decision	0.075	0.088	0.133*	0.121*	0.268**	0.219*
	(0.102)	(0.121)	(0.077)	(0.070)	(0.112)	(0.121)
Decision 1	−0.135	−0.094	−0.302***	−0.314***	−0.029	−0.071
	(0.139)	(0.161)	(0.084)	(0.090)	(0.068)	(0.085)
P-values from the following tests:						
Info X dist. = IID X dist.			0.156	0.816		
Info X dist. = PCR X dist.					0.256	0.447
IID X dist. = PCR X dist.					0.935	0.481
N	71	71	62	62	76	76
Includes:						
Enumerator dummies	Yes	Yes	Yes	Yes	Yes	Yes
RCT controls		Yes		Yes		Yes
Individual controls		Yes		Yes		Yes

Note: Dependent variable is Revision. Revision = revised decision - initial decision. Estimates from separate regressions are presented in each panel. P-values from tests between treatments are obtained by estimating a fully interacted, pooled model. Standard errors in parentheses. $^{*}p<0.1$, $^{**}p<0.05$, and $^{***}p<0.01$.

tests for the three channel treatment groups are shown in Appendix Table 5, separately for each first mover type. Given the small sample size in each group, there are several differences across groups, some small in magnitude, while others are larger. However, for all analyses we present results with and without control variables and find that our estimates do not seem to be driven by imbalances in second mover characteristics.

5.2. Results

The results are presented in Table 5, with peer first movers in Panel A, external leaders in Panel B, and formal leaders in Panel C. The first two columns show results for the pure information group, columns 3 and 4 for the IID group, and columns 5 and 6 for the PCR group. P-values testing for differences between the channel treatments are also reported at the bottom of each panel.

Panel A shows that the positive influence of peers is driven by information. We observe a strong, similarly sized response to the distance from the observed decision in the pure information treatment and the IID treatment. Because the information channel operates in both treatments, while social utility is present in the IID treatment only, this suggests that information is the important channel for the influence of peers. In the PCR group there is no statistically significant response to peer first movers, and this coefficient is statistically different from the coefficients in the other groups. This suggests that risk sharing also matters in the peer first mover treatment. In sum, the results for peer first movers suggest that information is their primary channel of positive influence, but that there is also a negative response driven by risk sharing in the PCR treatment.

Panel B shows different results for external leaders. The amount invested by external leaders in the pure information treatment and the IID treatment does not affect the decisions of second movers. Instead, we find suggestive evidence that second movers positively respond to the behavior of extension service workers in the PCR treatment. This pattern of results suggests that participants do not act on the information provided by the external leader's choice. However, the coefficients in the PCR treatment provide evidence that a positive social utility channel may be important. It is important to add the caveat that we are unable to reject that the coefficients in the three treatment groups are equal, and as such the patterns must be interpreted as suggestive.

The results in Panel C for formal leaders are similar. The coefficients on the distance variable in the pure information treatment are not statistically significant. The coefficients are larger (and statistically significant) for the IID treatment, and larger still in the PCR treatment. As for external leaders this pattern suggests that social utility is the primary channel of influence for the club chairs. However, due to the small sample, we again cannot definitively reject that these coefficients are equal across treatment groups.

These results provide evidence that is useful for those designing social programs that promote the flow of information and the adoption of new technologies or behaviors. Programs that rely on peers can focus on the information channel, while those employing leaders must be cognizant of social comparison effects. It may not be enough for leaders to provide information, people must see that they have actually done something or possess something in order for the influence to be effective. At the same time, it is important to note that peers may not always be well placed as purveyors of information, especially if they themselves are not well informed or trained. Similarly, when considering risky decisions, the role of risk sharing in social influence must not be ignored.

6. Conclusion

This paper carefully investigates social influence in risk taking in an experiment that studies the influence of three types of agents (peers, external leaders, and formal leaders). Results show that individuals positively respond to the behavior of others. They invest more (less) in response to high (low) investments by others. Although investment decisions are similar across first mover treatments, the response to the observed decision is different. When first mover decisions are held constant, decisions made by peers are the most influential, while extension agents are the least, with formal leaders falling suggestively in between. This extends the findings of BenYishay and Mobarak (2019) and Beaman et al. (2021), who study technology adoption in agriculture in a comparable sample of farmers, but in a setting where informational asymmetries and liquidity constraints play a major role. We additionally find suggestive evidence that farmers follow their peers because of information effects, while they follow external and formal leaders because they derive social utility from imitating their actions. Furthermore, risk sharing appears to influence the responses of those second movers who observe randomly selected peers.

These results illustrate the importance of carefully considering the identity of the opinion leaders used to influence the behavior of target populations. Despite the fact that extension officers occupy positions created for the transmission of information, peers and formal leaders may in fact be the most trusted agents. However, the exploratory analysis of mechanisms suggests that while peers may be more influential than formal and external leaders in the pure information and the IID risk channel treatments, they are less influential in the perfectly correlated risk scenario. Leaders may be the optimal agents to target in environments where risk taking involves common risk scenarios such as insurance products for extreme weather events, while peers may be the ideal injection points for other types of information and technologies that deal with idiosyncratic risks.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2021.05.022.

Appendix A: Additional experimental design details

A1. First mover decisions

As outlined in Fig. 2, all first movers made an initial decision. Thereafter, they were informed that their revised investment decision would be revealed to some of the members of their club. First movers were not informed which group members would see their investment choice but knew that not all group members would see their decision. First movers in the formal leader and peer treatments knew that this revised decision would determine their final investment choice and therefore their earnings from the investment decision. The revision decision made by external leaders was implemented in a slightly different manner because we only had 15 extension agents in our sample, each one working with several clubs.[29]

[29] One extension service worker withdrew from participation after making the first investment decision, which is why we have only 14 extension workers in our data. This extension service worker terminated their participation because they refused to make a decision that would be observed by others, and was the only person who chose not to participate in the study after the decision and incentives were explained. They were allowed to keep their endowment.

Appendix Table B.1
Number of participants by first mover type treatment.

Treatment	Control	Peer	External leader	Formal leader	All
Panel A: First movers only					
N in sample		122	15	122	259
N in FU1 sample		110	15	106	231
N in experiment		110	14	94	218
% of assigned sample		0.902	0.933	0.770	0.920
Panel B: Second movers only					
N in sample	0	353	353	360	1066
N in FU1 sample	0	313	304	310	927
N in experiment	116	239	246	209	810
% of assigned sample		0.677	0.697	0. 580	0.760

We elicited several revision decisions from extension agents, one for each of the clubs they worked with. Clubs were presented in random order, and one revised choice was randomly selected to be paid.[30]

We chose to conduct the experiment in this way to avoid using deception and to inform first movers that their actions would be observed by others. Indeed, a situation in which first movers are aware that their choices and actions are observed is most relevant to situations when behavior is easily observable or advice is directly given.

Each extension worker was interviewed in private by one enumerator. Communication between extension service workers was prevented before they made their decisions. Extension service workers were not immediately paid after they made their decisions. They were contacted to be paid after decisions from all individuals in the study were elicited, and were informed of this delay in payment before they made their decisions. When they were paid, they learned the outcome of the coin flip and which revision decision was randomly selected to count for payment.

As is described in Section 5 of the main text, first movers were randomized into three different channel treatments that varied whether their choice was implemented and the structure of the underlying risk. Whether or not the first mover's choice was realized was implemented through the roll of a die. First movers were informed that the roll of a die would determine whether their choice could be carried out or not. If the outcome of the die roll was 1 or 2, the money invested was returned to the first movers and their investment choice was not carried out. If the outcome was between 3 and 6, then the choice was carried out.[31]

Appendix B. Sample frame and resulting experimental sample

This study uses the sample of smallholder farmers who, at the time of the first follow-up survey of the evaluation of the cash transfer and extension program (Ambler et al., 2020), had been a registered member of the 122 participating farmer clubs at any point in the preceding two years. We refer to this sample frame as the "randomization sample." We randomly assigned smallholder farmers to the first mover type treatments and the channel treatments using this sample. Appendix Table B.1 describes the allocation to the first mover treatment conditions for first movers (in Panel A) and second movers (in Panel B). Because we integrated this study into the impact evaluation RCT discussed in Section 2, we only attempted to conduct the artefactual field experiment with those smallholder farmers that the RCT field team was able to make contact with during the first follow-up survey (FU1). Because this experiment was conducted just a few days following FU1 (see Fig. 1), it is unlikely we would have been able to locate additional farmers who were not surveyed in FU1. We refer to this sample as the "FU1 sample."

As Appendix Table B.1 shows, our experimental sample includes a total of 218 first movers, and 810 second movers. Only 14 (5%) of first movers are extension service workers, because each extension worker employed by NASFAM works with multiple groups. Attrition relative to the randomization sample frame is quite high for several reasons. First, the randomization sample list included all farmers who had been listed as NASFAM members in the last two years, including those who had never been located by our team, even during the project baseline. Second, to ensure that this experiment did not interfere with standard NASFAM activities, we did not mention incentives when scheduling artefactual field experiment visits. Third, to prevent information sharing between participants across time, all participants in a club had to be located and interviewed in a short time horizon on the same day. Importantly, individual farmers and club chairs were not aware of their treatment status for this artefactual field experiment. This leaves between 209 and 239 second mover observations in each first mover type treatment that we use to analyze the research questions investigated in this study.

[30] Clubs have names that are used for various NASFAM activities, so it was easy to explain to extension agents that they could adapt their revision decision according to the audience that would see their choice.

[31] First movers were informed that the die roll would determine whether their choice was realized or not. They were not informed that the roll of the die would also determine the structure of the underlying risk. We chose not to provide first movers this information to keep first mover behavior comparable across risk treatments. Like coin flips, die rolls were executed electronically on the tablets used for data collection.

Appendix C. Heterogeneity

To further investigate the dynamics behind the results for the external leaders, we examine how our results differ for those exposed to an intensive extension program conducted in one of the RCT treatment arms (Ambler et al., 2020). These second movers would have had much more one-on-one contact with the extensionist, which could impact the extent to which they are influenced by the first mover choice. In results not shown, we find little difference in influence in the external leader treatment among those who received the intensive extension and those who did not.

Because treatment assignment was stratified on the gender of the second movers, we also examine how results vary by gender. We find the women invest less than men, are more likely to revise their decisions, and the size of the revision is larger. However, there is no difference in the response to the first mover decision by gender in the peer or formal leader treatments. There is evidence that female second movers to do not react to external leaders, while males do.

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