

# Illusion of control in farmers' investment and financing decisions

Illusion of  
control

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## Abstract

**Purpose** – People's tendency to overestimate their ability to control random events, known as illusion of control, can affect financial decisions under uncertainty. This study developed an artificial field experiment on illusion of control for a farm machinery investment.

**Design/methodology/approach** – In an experiment with two treatments, the individual farmer was either given or not given a sense of control over a random outcome. After each decision, the authors elicited perceived control, and a questionnaire collected additional indirect measures of illusion of control from 78 German farmers and 10 farm advisors.

**Findings** – The results did not support preregistered hypotheses of the presence of illusion of control. This null result was robust over multiple outcomes and model specifications. The findings demonstrate that cognitive biases may be small and difficult to replicate.

**Research limitations/implications** – The sample is not representative for the German farming population. The authors discuss why the estimated treatment effect may represent a lower bound of the true effect.

**Originality/value** – Illusion of control is well-studied in laboratory settings, but little is known about the extent to which farmers' behavior is influenced by illusion of control.

**Keywords** Risk and uncertainty, Behavioral finance, Superstition, Framed field experiment, Artificial field experiment

**Paper type** Research paper

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## 1. Introduction

The objective of this study was to examine illusion of control bias of German farmers related to farm investment and financial decision making. Behavioral economists and cognitive psychologists have long argued that people violate the *homo economicus* model. People are subject to various cognitive biases (Kahneman, 2011; Kahneman *et al.*, 1982) that also influence their economic and financial decisions (De Bondt and Thaler, 1995; Thaler, 2012; Frydman and Camerer, 2016). People tend to see patterns and meaning in random events (Taleb, 2005). One of the many biases is the so-called illusion of control bias, i.e. people's tendency to overestimate their ability to control random events (Thompson, 1999). Decision makers may believe they have control over random outcomes, such as die rolls or roulette wheels, and such biases can also affect financial decisions under uncertainty (Frydman and Camerer, 2016).

Control over outcomes in different contexts has been discussed for nearly a century (Adler, 1930; Heider, 1958; Kelley, 1967; White, 1959; Presson and Benassi, 1996). In recent decades, researchers have also focused on the factors that mediate illusion of control (Fellner, 2009; Kahai *et al.*, 1998). For instance, Thompson (1999) argues that personal involvement, familiarity, experience and knowledge determine the degree of illusion of control bias. Fenton-O'Creevy *et al.* (2003) claim that illusion of control is more pronounced in situations that are perceived as stressful or competitive such as financial trading. Charness and Gneezy (2010) show that illusion of control can be alleviated by explicitly asking participants to pay for more control.

Illusion of control is well studied in laboratory settings with students or the general population (Charness and Gneezy, 2010; Ejova *et al.*, 2010; Ginakis and Ohtsuka, 2005; King *et al.*, 2011; Langer, 1975; Ohtsuka and Hyam, 2003; Sloof and von Siemens, 2017; Stephens and Ohtsuka, 2014). Less is known about illusion of control in business and financial decision making (Biais *et al.*, 2005; Fast *et al.*, 2008; Kottemann *et al.*, 1994; Meissner and Wulf, 2017; Simon *et al.*, 2000) and in specific professional contexts (Durand, 2003; Fenton-O'Creevy *et al.*, 2003). Recent studies also highlight the need within policymaking to employ behavioral experiments with farmers (Colen *et al.*, 2016; Dessart *et al.*, 2019; Viceisza, 2016), who face great risks, such as weather and price fluctuations, plant and animal disease, and changing policy environments (Harwood *et al.*, 1999). In addition, there is a need to systematically replicate experiments to arrive at realistic effect size estimates in the literature and to provide a basis for meta-analysis (Camerer *et al.*, 2016; Christensen *et al.*, 2019).

To examine illusion of control bias of German farmers, we applied and compared a measure of illusion of control from a preregistered artificial field experiment (Harrison and List, 2004) with ex-post stated perceived control over outcomes. We also used psychological scales to assess the magnitude of superstition and illusion of control. Previous research on farmers' financial decision making has largely focused on the elicitation of risk and ambiguity attitudes (e.g. Bocquého *et al.*, 2014; Bougherara *et al.*, 2017; Cerroni, 2020; Herberich and List, 2012; Iyer *et al.*, 2020; Menapace *et al.*, 2016; Vollmer *et al.*, 2017). We add to this emerging literature on farmers' behavior under risk and uncertainty by investigating how cognitive biases are a potential driver of farmers' financial decision making.

The remainder of this paper is organized as follows. In Section 2, we briefly review the theoretical and empirical literature on illusion of control bias and its measurement. Section 3 introduces our study design of an artificial field experiment on farmers' loan allocation and the complementary measures. Section 4 describes the socio-economic background and farm characteristics of participants. In Section 5, we present and discuss the results of illusion of control bias, as well as some conclusions and areas for future research.

## 2. Illusion of control – definitions and measurement

Langer (1975, p. 113) first coined the term illusion of control, which she defined as “an expectancy of a personal success probability inappropriately higher than the objective

probability would warrant.” Cognitive psychologists have defined illusion of control as a cognitive bias that belongs to the group of positive illusions (Kahneman *et al.*, 1982). Presson and Benassi (1996) note that much of the early work in the field used broad definitions that would be better covered by the term illusion of judgment. They also point out that much of the early empirical work on illusion of control was concerned with the illusion of predictions rather than outcomes. In this study, we applied the rather narrow definition by Thompson (1999), who describes illusion of control as an overestimation of a person’s ability to control random events for which the probability distribution is known, e.g. the roll of a die. No matter who rolls the die, where and how, the probability distribution of outcomes is unaffected. If a person believes they have influence over the outcomes of a die roll by rolling the die themselves, it is fair to say that this person is susceptible to illusion of control bias (Charness and Gneezy, 2010; Fellner, 2009; Langer, 1975).

Experiments and surveys are distinct methods to measure illusion of control. In a typical experiment, subjects would be asked to perform different tasks, such as placing a bet or making a hypothetical investment decision, under different treatments which manipulate the subjects’ perception of randomness (Charness and Gneezy, 2010; Fellner, 2009; Grou and Tabak, 2008). Survey measures either directly ask respondents about their perceived control over a certain event or use more sophisticated psychological scales to elicit a more general measure of illusion of control (Moodie, 2008; Ohtsuka and Hyam, 2003; Stephens and Ohtsuka, 2014; Wood and Clapham, 2005). Experimental and survey methods are also often combined (Biais *et al.*, 2005; Cowley *et al.*, 2015; Ejova *et al.*, 2010, 2015; Fenton-O’Creavy *et al.*, 2003; Kwak *et al.*, 2010; Meissner and Wulf, 2017; Simon *et al.*, 2000; Yarritu *et al.*, 2014).

### 3. Study design, data collection and summary statistics







Most of the experimental literature on illusion of control has been produced in psychology, but there are also studies in economics and finance. Experiments in economics use incentives contingent on performance and abstain from deception (Hertwig and Ortmann, 2001). Our experiment was based on a simple paper and pen task during a workshop with farmers. Farmers had to make a choice under risk involving salient monetary incentives. We elicited participants’ perceived control in the situation shortly before the die roll. Participants also responded to psychometric tests on superstition and illusion of control, parts of which were adapted to fit the context of decision making in agriculture. A survey asked for demographic and financial information, as well as farm characteristics. Finally, subjects received cash payments.

#### 3.1 Experimental design to measure illusion of control

Our experimental design was inspired by Fellner (2009), but we adapted it to the context of a German farm manager. An explorative study with Swedish farmers (Labajova, 2018) and exploratory interviews with German farmers guided the experimental design, specifically with respect to framing the decision in a familiar context to enhance external validity and comprehension (cf. Meraner *et al.*, 2018; Rommel *et al.*, 2017, 2019). In an artificial field experiment, non-student subjects are recruited for participation in an experiment which may help to identify differences in behavior across subject pools and the sensitivity of behavior to context (Cason and Wu, 2019; Rahwan *et al.*, 2019). In a framed field experiment, context is added to the task, because it “is not the case that abstract, context-free experiments provide more general findings if the context itself is relevant to the performance of subjects” (Harrison and List, 2004, p. 1,028). Although we added farming context to the task, the task remained fairly abstract and did not involve unusual rewards. Hence, one may define our experiment as an artificial field experiment in the typology of Harrison and List (2004), although the experiment also carries some characteristics of a framed field experiment.

Our experiment differs from the study of [Fellner \(2009\)](#) in at least one important aspect. We have used a simplified design and refrained from using multiple price lists to ensure that farmers are risk-averse. Note that the decision that farmers were confronted with in the task should in theory be relevant only for risk-averse decision makers (a higher expected value goes along with higher risks). Rather than using multiple price lists, we have learned about farmers' risk attitudes from the scale developed by [Dohmen et al. \(2011\)](#). Although this scale cannot provide a classification based on economic theory, it can provide some hints on risk profiles which we discuss in the results and conclusions sections below.

In the experimental task, participating farmers had to decide on a hypothetical financing decision that involved a loan of 100,000 Euro for the purchase of a new tractor with a repayment period of five years (see supplementary material for all instructions used in the study). The task was not only developed from a pre-study ([Labajova, 2018](#)) but also involved informal qualitative interviews with two German farmers. The scenario is realistic in that it uses familiar amounts for the investment and interest rates, although a number of simplifying assumptions were made. The task of the farmer was to allocate the 100,000 Euro between a risky stochastic and a safe deterministic option (see [Table 1](#) for examples of the associated payoffs). The riskier option yielded a higher expected cost saving than the safe option. The decision was incentivized, and 100 Euro of cost saving in the task was associated with a monetary gain of one Euro. The interest rate in the risky option was determined by a die roll ([Table 1](#)). Cash payments were implemented double-blind and were calculated based on a simplified formula in Excel [loan cost = (100,000 – amount placed in risky option) × 2.5% + amount placed in risky option × randomly determined interest rate]. Note that this calculation was highly stylized for the purpose of clear communication. The calculation was common knowledge and demonstrated using examples and a table that allowed for linear interpolation (see [Table 1](#) for examples and supplementary material for a

	Distribution between fixed and flexible interest rate						
	Fixed (2.5%)	100,000	80,000	60,000	40,000	20,000	0
	Flexible	0	20,000	40,000	60,000	80,000	100,000
Outcome of die roll	Flexible interest rate	Cost saving contingent on distribution and interest rate outcome					
	<b>1.90%</b>	2,500	2,860	3,220	3,580	3,940	4,300
	<b>2.10%</b>	2,500	2,740	2,980	3,220	3,460	3,700
	<b>2.30%</b>	2,500	2,620	2,740	2,860	2,980	3,100
	<b>2.50%</b>	2,500	2,500	2,500	2,500	2,500	2,500
	<b>2.70%</b>	2,500	2,380	2,260	2,140	2,020	1,900
	<b>2.90%</b>	2,500	2,260	2,020	1,780	1,540	1,300

**Table 1.** Overview on possible outcomes for six possible loan allocations as shown to participants

calculator tool). Payments were always rounded up to the next full Euro amount. The lowest and highest possible payments were 13 and 43 Euro, respectively.

Our experiment had two treatments, implemented between subjects. The experimental treatments differed with respect to the person who rolled the die. In one treatment, participants rolled the die themselves; in the other treatment, the researcher rolled the die. The treatments were balanced within each of the two days of data collection. In other words, approximately half of all participants received one of the two treatments on each of the two days. This practice ensured that differences in the oral instructions, the day of data collection or the composition of the participant group were not confounded with the treatment. The die roll was mentioned only in general terms in the oral instructions: it was never mentioned orally *who* would roll the die. To minimize the influence of experimenter bias and to avoid demand effects (Zizzo, 2010), this information was only part of the written instructions which were placed in envelopes and randomly distributed to the seats. The written instructions explicitly mentioned who would roll the die. To increase the salience of the die roll treatments, participants had to tick a box on the decision sheet (see supplementary material), stating explicitly that they understood who would roll the die (which is the only information that differed between treatments). After participants had ticked the box, they were asked to decide on their fictitious loan allocation. Anonymity was ensured for all decisions, and small booths were used to ensure privacy (see supplementary material for pictures of the setup). After the decision was made, participants were asked to go to another room to meet a researcher to determine the interest rate by means of the die roll (implemented as indicated in the treatment on the respective sheet).

### 3.2 Hypotheses, outcome variables, preregistration and sample size

If there was no illusion of control bias, it should not have mattered for farmers' decisions who rolled the die. Under the assumption of illusion of control, our main hypothesis was that farmers took greater risks and felt more in control if they rolled the die. We compared three different outcome variables by treatments. First, we were interested in how much farmers allocated to the risky option. Second, we were interested in the proportions of the extremes, i.e. the share of participants that allocated the full 100,000 Euro to either the risky or the safe option. Third, we elicited participants' perceived control in the situation shortly before the die roll which was assessed by a commonly applied single-item rating (Ejova *et al.*, 2010). Participants were asked whether they feel in no to full control over the die roll outcome on a nine-point scale. In principle, perceived control should be zero over purely random outcomes. Thus, any positive value would indicate illusion of control. We also elicited participants' beliefs on the die outcome. They could choose any number or state that all numbers are equally likely (the correct answer).

We preregistered all hypotheses and outcomes before the start of the data collection, and we report all preregistered analyses below (see time-stamped preregistration document in supplementary material). Our sample size calculations were guided by practical limitations, as it is often difficult to recruit a very large number of farmers (Weigel *et al.*, 2021). An expedite *ex ante* power calculation for the loan allocation outcome and the final design revealed that the study is sufficiently powered to detect large effects (Cohen's  $d = 0.8$ ) at the usual level of power  $p = 0.8$  and alpha = 5% (see time-stamped preregistration document) for a one-sided  $t$ -test. In the absence of suitable priors for the power calculations, we assumed two scenarios. The optimistic scenario powers for a difference of 20,000 Euro and a standard deviation of 20,000. For a one-sided  $t$ -test, 13 observations per treatment are needed. The pessimistic scenario assumes a difference of only 15,000 Euro and a higher standard deviation of 25,000 Euro, resulting in a minimum sample size of 35 participants.

Note that the standard deviation in the main outcome of the sample was very close to the 25,000 scenario.

3.3 Recruitment and data collection

Participants were recruited for a workshop on financial decision making in agriculture in the German federal state of Hesse. The workshop lasted half a day and was free of charge. Information on the workshop was spread through social networks and advisory services. Catering and drinks were offered in addition to the lectures and training sessions. The invitations did not mention the experiment. More than 80 people registered for the workshop (almost everyone who registered also participated) which took place in the premises of the Justus-Liebig-Universität in the German city of Gießen. Participation was split between two consecutive days (February 1 and 2, 2019). The experiment was embedded in the broader context of the workshop. After all decisions were made, the aggregated data were presented to participants, and a short debriefing and discussion took place. Participants received oral instructions supported by a power point presentation (see supplementary material). A strict script was applied to ensure consistency across both days of the workshop. The oral instructions used various calculation examples to enhance comprehension, but we carefully selected diverse examples to avoid priming (see supplementary material). In addition, there were written instructions (see supplementary material). Participants were allowed to ask questions in private.

3.4 Additional survey instruments to elicit illusion of control

There are different psychological scales within psychology to study illusion of control. For instance, Kwak *et al.* (2010) and Moore and Ohtsuka (1999) have developed scales with a limited number of items to evaluate illusion of control in the context of gambling. Here, we opted for the more elaborate Drake Beliefs about Chance (DBC) inventory, which was also originally developed in the context of gambling but includes more items (Wood and Clapham, 2005). The original scale consists of 22 items and explicitly distinguishes superstition and illusion of control dimensions. Some of the items were developed in the context of gambling and did not suit our context very well. We carefully evaluated the items and used a shortened list of 18 items, 14 from the original scale and 4 adapted to an agricultural context (see supplementary material and Table 5 below). One item was reversed, and we recoded it for the analysis.

3.5 Summary statistics

Table 2 displays summary statistics of the participants. Participants were predominately male with an average age of approximately 45 years. The majority of participants were active

	Mean	Median	Standard deviation	N	Minimum	Maximum
Gender (1 = female)	0.06			88	0	1
Age (years)	44.83	47	14.93	88	18	74
Farming (1 = active farmer)	0.89			88	0	1
Main income (1 = agriculture main income)	0.42			78	0	1
Livestock units	17.57	0	38.00	77	0	180
Arable land (hectares)	88.27	80	75.91	77	1.50	438
Economics courses (= 1 has training in economics)	0.72			78	0	1
Risk attitude (11-point scale following Dohmen <i>et al.</i> , 2011)	5.43	6	1.80	88	2	8

**Table 2.**  
Summary statistics

farmers; the ten other participants were farm advisors whom we include for the main analysis. Note that we have run all major analysis also with the subsample of farmers and did not find major differences. For approximately 42% of the farming participants, farming was their main income. Farms were quite diverse in terms of livestock units and cultivated area with a range of 0–180 livestock units and 1.5–438 hectares, respectively. A majority of the farming participants had received at least some economic training. We also elicited risk attitudes, using the simple 11-point scale question from [Dohmen \*et al.\* \(2011\)](#). The distribution is similar to the [Dohmen \*et al.\*](#) study, but the modal respondent is somewhat more willing to take risks in our sample (modal respondent chose 8 in our sample but 5 in the [Dohmen](#) study).

The average, median, minimum and maximum cash payment farmers received were 26.69, 25, 15.40 and 43 Euro, respectively. The standard deviation was 6.23 Euro.

### *3.6 How representative are the sampled farms for the German farming population?*

Farmers are difficult to recruit for the participation in experiments which increases the risks of biased samples ([Weigel \*et al.\*, 2021](#)). To become aware of potential biases, it is useful to compare the sample to the population of all German farms as well as farms in the federal state of Hesse. According to the [Federal Ministry of Food and Agriculture \(2018, p. 24\)](#), conventional farms in Germany utilized approximately 91.9 hectares on average in 2017/18 (86.7 hectares for organic farms). Conventional farms made an average profit of 39,107 Euro (40,641 Euro for organic farms). While conventional farms had on average 145.3 livestock units per 100 hectare, with 82.7 livestock units per 100 hectare this figure was much lower for organic farms. The farms in our sample have a similar size but fewer livestock units than the average German farm. However, it must be noted that farms in the federal state of Hesse are typically smaller than in other parts of Germany (see [Statistik Hessen, 2011](#) for the latest detailed available figures from 2010). In 2020, there were approximately 15,130 farms in the state of Hesse, farming a total of 764,700 ha. Hence, the average farm size was approximately 50.5 hectares ([Destatis, 2021](#)), indicating a bias toward large farms in our sample. There were only few women in our sample. Population statistics on the farmers with vocational training show that approximately 17 percent were women in 2016 ([German Farmers' Association, 2021](#)).

It is also useful to compare the risk attitudes of our sample to population data. [Dohmen \*et al.\* \(2011\)](#) found an average score of 4.42 (SD = 2.38) which was substantially higher for men (mean = 4.91). In a sample of German farmers, [Maart-Noelck and Musshoff \(2014\)](#) find that farmers are slightly more risk-seeking than the [Dohmen](#) sample which is similar to our sample, displaying a value of 5.43 (which is also highly skewed toward male participants). In conclusion, our sample cannot be considered representative for the German farming population or farms in the federal state of Hesse for the mentioned characteristics. However, under the assumption of homogeneous treatments effects, i.e. treatment effects do not interact with characteristics of participants, we can still obtain an unbiased estimate of the treatment effect.

## **4. Results**

### *4.1 Illusion of control measures by treatments*

Our main outcome variable of interest was the loan amount allocated to the risky option. We compared this variable between subjects by treatments. Results are displayed in [Table 3](#).

Participants allocated about the same amounts to the risky and safe options across all possible outcomes and tests. In most instances, and in contrast to our hypothesis, participants allocated *less* to the risky option when they were in control of the die roll. Yet, none of the differences are statistically significant.

**Table 3.**  
Major outcomes by  
treatments

Outcome measure	Average outcome if participant rolls the die	Average outcome if researcher rolls the die	Test	Test statistic	<i>p</i> -Value
Amount allocated to risky option (Euro)	54934.09 (SD = 24917.64)	60454.55 (SD = 23614.35)	<i>t</i> -Test (one-sided)	<i>t</i> = -1.0667	0.86
Proportion of participants who allocated everything to risky option (variable interest rate)	0.09	0.11	Mann-Whitney <i>U</i> test	<i>z</i> = -0.983	0.33
Proportion of participants who allocated everything to safe option (fixed interest rate)	0.05	0.02	Two-sample test of proportions (one-sided)	<i>z</i> = -0.35	0.36
Perceived control of outcome of the die before die roll on 9-point scale	3.02 (SD = 1.93)	2.72 (SD = 2.00)	Two-sample test of proportions (one-sided)	<i>z</i> = 0.59	0.72
Proportion of participants who believe probability distribution of die is uniform	0.59	0.55	<i>t</i> -Test (one-sided)	<i>t</i> = 0.71	0.24
			Mann-Whitney <i>U</i> test	<i>z</i> = 0.85	0.40
			Two-sample test of proportions (one-sided)	<i>z</i> = 0.43	0.33



4.2 Controlling for heterogeneity

Table 4 displays coefficient estimates of three Ordinary Least Squares (OLS) models with the amount allocated to the risky option as the dependent variable. The model in column (1) only includes the experimental variables, that is, a treatment dummy and the day of data collection (recall that data collection took place on Friday and on Saturday). Column (2) also includes gender, age and risk attitude. Column (3) only reports outcomes for the farmer sample and includes four farm level variables: farming is main income, livestock units, farm area and economic training.

The regressions confirm the analysis from the previous section. Treatment effects are in the opposite of the expected direction, small and statistically insignificant. Note that the gender coefficient is large in absolute terms, with women being substantially more risk-averse. However, the effect is not statistically significant, and there were very few women in the sample. There is a fairly large effect of the risk attitude in the expected direction (risk-seeking farmers place more money in the risky option), albeit it is statistically significant at the 5% level only in the model presented in column (2). A one-point increase in the self-assessed willingness to accept risks leads to a greater willingness to allocate money to the risky option in the experimental task of approximately 2,800 Euro. The 95% confidence interval was the estimate and is 5.9 to 5758.7 in column (2) and -157.0 to 6537.4 in column (3).

The estimated models have small explanatory power as indicated by the small *F*-statistics and adjusted *R*<sup>2</sup> values. Predicted values for all models are within the defined interval of 0–100,000 Euro, and there are few censored observations (<15%). Hence, we decided not to estimate Tobit models.

As noted above, our sample may have been biased towards risk-seeking farmers, whereas the theoretical framework required risk-averse farmers. We have performed additional analysis with an interaction effect of treatment and risk attitude. The estimated coefficient was positive but statistically not significant. We have also repeated the analysis for the subsample that had Dohmen scores of 6 or less (30 observations with scores 7 or 8 were removed). The results do not change qualitatively, but the treatment effect estimates increased (albeit the treatment effect was still fairly small and statistically not significant) which could indicate that a more risk-averse sample would indeed show a larger treatment effect, although the

	(1)	(2)	(3)
Treatment (1 = researcher rolls the die)	5520.5 (5165.5)	3831.2 (5102.2)	1653.4 (5752.7)
Friday (1 = Friday)	-5956.8 (5165.5)	-6433.2 (5085.3)	-6204.9 (5849.4)
Gender (1 = female)		-18845.7 (11,147.2)	-6150.8 (32,506.6)
Age (years)		-170.3 (177.1)	-152.2 (215.4)
Risk attitude (11-point scale following Dohmen <i>et al.</i> , 2011)		2882.4* (1445.9)	3190.2 (1677.0)
Main income (1 = agriculture main income)			9158.4 (8292.3)
Livestock units			-46.62 (80.33)
Arable land (hectares)			-25.37 (63.54)
Economics courses (= 1 has training in economics)			7011.9 (6564.3)
Constant	52392.0*** (8566.0)	48212.0*** (12925.5)	43811.7*** (14732.8)
Observations	88	88	77
<i>F</i>	1.236	1.855	1.187
Adj. <i>R</i> <sup>2</sup>	0.0054	0.0469	0.0217

Note(s): Standard errors in parentheses

\**p* < 0.05, \*\**p* < 0.01, \*\*\**p* < 0.001

**Table 4.** Regressions adjusting for socio-economic heterogeneity (dependent variable is the amount allocated to the risky option)

Item	Mean	Standard deviation	N	Minimum	Maximum
<i>Superstition (Cronbach's alpha = 0.75)</i>					
I like to carry a lucky coin, charm or token when I'm doing something important	2.17	0.99	88	1	4
I can improve my chances of winning by performing special rituals	2.03	1.04	88	1	4
There may be magic in certain numbers	2.14	0.90	88	1	4
When I take a test (or took them in the past) I use a lucky pen or pencil	1.77	0.82	87	1	4
There is useful information in my daily horoscope	1.70	0.82	88	1	4
When I need a little luck I wear lucky clothes or jewelry	1.85	0.95	87	1	5
A game of chance is a contest of wills between the game and the player	2.85	1.14	88	1	5
I believe that fate is against me when I lose	2.25	1.06	88	1	5
I do not consider myself to be a superstitious person. (Reverse scored)	2.29	1.24	73	1	5
<i>Illusion of control (Cronbach's alpha = 0.74)</i>					
It is good advice to stay with the same pair of dice on a winning streak	2.46	1.04	87	1	5
One should pay attention to lottery numbers that often win	2.43	0.98	87	1	4
If a coin is tossed and comes up heads ten times in a row, the next toss is more likely to be tails	2.41	1.14	87	1	5
Some people are just born lucky	3.10	0.93	86	1	5
The longer I've been losing, the more likely I am to win	2.48	0.96	87	1	4
<i>Contextually framed statements (Cronbach's alpha = 0.58)</i>					
One can learn to pick a good interest rate	3.44	0.87	87	1	5
The more I know about loans, the earlier I can get a low interest rate	3.59	0.91	87	1	5
I will save money by using a system to predict the interest rate	3.39	0.94	87	1	5
If my farm yield was low for several years, the probability of a high yield increases	2.46	0.96	87	1	4

**Table 5.** Summary statistics for items on superstition and illusion of control

sample was too small to make such a statement with great confidence. Detailed results are presented in the supplementary material.

#### 4.3 Drake Beliefs about chance inventory

Table 5 displays summary statistics for the 18 survey items on superstition and illusion of control. Overall, there are low levels of superstition. The average across all items is not correlated with the amount allocated to the risky option in the experiment (Pearson's  $r = -0.05, p = 0.67$ ). If we apply a factor analysis, extracting two factors for the superstition and illusion of control items or three factors if we include the contextually framed items, factor loadings indicate a poor fit (in spite of relatively high scale reliability).

### 5. Discussion and conclusions

Farmers face high risks of seasonal production, changing weather conditions, price fluctuations and volatile policy environments. To measure illusion of control in farmers' financial decision making, we conducted an artificial field experiment in which a sample of

German farmers had to allocate a loan to a risky and a safe option. Our treatments were manipulations of who rolls a die in the experiment (researcher or farmer) to determine random outcomes with respect to interest rates. We complemented our data with questionnaire measures of perceived control and elicited illusion of control, using a psychological scale. A caveat of our approach was that farmers were not representative of the German or regional farming population for several important characteristics. In addition, our study attracted a greater than expected number of risk-seeking farmers for whom the benchmark theoretical model of a risk-averse decision maker would not necessarily hold.

We did not find treatment effects in the experiment. In our study, there was no evidence of illusion of control. Note that we pre-registered our hypotheses and analyses. Our experiment did have sufficient statistical power ( $\alpha = 0.05$ ;  $\beta = 0.2$ ) to detect large effects (Cohen's  $d = 0.8$ ). Our analysis of loan allocations suggested that farmers' decisions were not influenced by illusion of control bias. Note that a similar result was obtained in a small study of Swedish farmers (Labajova, 2018). Although there could still be type II error, one may ask how practically relevant the many small cognitive biases identified in the psychological literature are in the day-to-day decisions of farmers. Our sample may be biased toward educated farmers with high financial literacy, but we did not find an effect of economic education or other covariates on risk-taking in the experiment. Given potential biases in the sample on observed and unobserved characteristics, our treatment effect estimate must be generally interpreted with care. Under the assumption that the treatment effect interacts with education and financial literacy, it should be viewed as a lower bound, i.e. the effect of illusion of control could be higher in the population.

Our experimental measures of illusion showed no significant correlation with the more general psychological scales developed to measure superstition and illusion of control. The experiment and the survey might have measured different latent constructs. More research is needed on how to operationalize and contextualize the measurement of cognitive biases in general and of illusion of control in particular. Existing validated scales could be adapted and extended for this purpose (see Hansson and Lagerkvist, 2012 for a discussion on the use of psychometric scales).

Real-life economic decisions are complex, and we need to develop methods to understand this type of decision making where it happens. Policymakers invest a great deal of effort in taking farmers' financial decisions into account. Our analysis did not find evidence for irrational decision making. However, other research points out that farmers may still not be easily treated as following a *homo economicus* model of rationality as typically assumed in simulation models for agricultural policy analysis (Dessart *et al.*, 2019; Howley, 2015). Taking into account cognitive biases in farmers' financial decision making for policy analysis remains an important route for future research.

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Appendix

	(1) (Full sample with interaction effect)	(2) (Only if Risk attitude smaller than 7)	(3) (Only if Risk attitude smaller than 7)	(4) (Only if Risk attitude smaller than 7)
Treatment	-3940.1 (7521.4)	6785.3 (5761.9)	7376.7 (5649.5)	10173.0 (7121.2)
Friday	-6568.3 (5120.1)	-7831.0 (5,731.0)	-6341.4 (5777.8)	-5983.7 (7216.5)
Dohmen interact (Treatment × Risk attitude)	1582.8 (923.7)			
Gender			-22686.9 (13236.4)	
Age			-352.5 (206.5)	-347.5 (276.1)
Risk attitude			3181.3 (2225.6)	2935.0 (2861.9)
Main income				8224.5 (11426.8)
Livestock units				-38.98 (92.97)
Arable land				-19.57 (99.87)
Econ courses				4878.6 (7804.2)
Constant	53848.9*** (8512.6)	49383.3*** (9276.8)	50613.0*** (14273.9)	42589.9* (19525.1)
Observations	88	58	58	49
F	1.822	1.627	1.869	0.843
r2_a	0.0275	0.0215	0.0708	-0.0269

Note(s): Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Column (1) presents regression coefficient estimates with an interaction effect of the treatment dummy and the risk attitude to test whether the treatment effect is mediated by risk. The coefficient is rather small and statistically not significant

Columns (2) to (4) re-estimate the models presented in Table 4 of the main text for the sub-sample of risk-averse participants (with risk attitude < 7). This results in a smaller sample (30 participants are removed in (2) and (3); due to missing observations for covariates in (4), 9 additional participants drop out). Note that the gender coefficient cannot be estimated for (4), as there are no women in the sub-sample

**Table A1.** Additional regressions adjusting for socio-economic heterogeneity (dependent variable is the amount allocated to the risky option)

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