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How (un)informative are experiments with students for other social groups? A study of agricultural students and farmers

Sven Grüner, Mira Lehberger, Norbert Hirschauer and Oliver Mußhoff[†]

Experiments are often used to study individual decision-making under controlled circumstances. Due to their low opportunity costs and high availability, university students are frequently recruited as the study population. Even though they are rather untypical with regard to many characteristics (e.g. age and income) compared to the representatives of the social group of interest, the experimental behaviours of students are sometimes prematurely generalised to other social groups or even to humans in general. Given the widespread challenges in the agricultural and environmental sector, it is particularly interesting to address farmers' decision-making. We analyse whether agricultural students can be used to approximate the behaviour of farmers in simple economic experiments, which are often used to measure risk aversion, impatience, positive reciprocity, negative reciprocity, altruism and trust. Moreover, we consider the role of systematically varied monetary incentives. We find no differences between agricultural students and farmers in their risk aversion; farmers' positive reciprocity and trust are positively associated with the incentive level, which cannot be observed with agricultural students. Findings regarding altruism in the two populations are mixed and challenge the finding of earlier studies of students being less pro-social. Agricultural students are a lower boundary of impatience and negative reciprocity. These heterogeneous results suggest that scientific inference from agricultural students to farmers should be made cautiously. However, we do not deal with a representative sample of our target population (e.g. gender). Replication studies are required to evaluate the generalisability of our findings.

Key words: agricultural students, economic experiments, farmers, monetary incentives.

JEL classifications: C90 Design of Experiments General, D01 Microeconomics: Microeconomic Behavior: Underlying Principles, Q12 Agriculture: Micro Analysis of Farm Firms, Farm Households, and Farm Input Markets

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1. Introduction

To better understand human behaviour, social scientists often elicit preferences and other behavioural traits (Chapman *et al.* 2018; Falk *et al.* 2018; Snowberg and Yariv 2021). But there are no comprehensive studies with representatives of the primary sector. This is surprising because farming produces not only valuable commodities but also negative externalities in terms of harmful ecological effects on waters, soils, the air and the biosphere (Rosa-Schleich *et al.* 2019)—a profound understanding of farmers' context-dependent evaluations including their behavioural traits is a crucial prerequisite for the design of efficient regulatory strategies (Dessart *et al.* 2019). Risk aversion, impatience, reciprocity, altruism and trust are important to explain behaviour in the agricultural sector, such as entrepreneurial decisions of farmers. For example, *risk averse* small-scale and subsistence farmers from South Africa have been found to prefer traditional agriculture and are less likely to opt for farming that needs financing (Brick and Visser 2015). Holden and Quiggin (2017) show that more risk averse food insecure farmers in Malawi more often adopt drought-tolerant maize. Di Falco *et al.* (2019) have shown that *impatience* (measured as higher discount rates) is negatively associated with profitable agricultural investments in their study of farmers from rural Ethiopia. Analysing the agricultural origins of time preference, Galor and Özak (2016) argue that areas that experienced higher returns due to agricultural investment-triggered adaptation tend to be more long-term oriented. Fischer and Qaim (2014) find evidence for *reciprocity* among smallholder farmers in Kenya: receiving benefits from a group is associated with more collective actions (e.g. collective marketing and participation in group meetings). Reciprocity is also important in the relationship between local food participants (farms and food retailers) in the United States and, in turn, helps to mitigate challenges associated with the conventional system (Trivette 2017). According to Marr and Howley (2019), farmers in England and Ontario adopt pro-environmental practices at least in part due to *altruistic* reasons (i.e. doing the right thing). In their study of the Philippines, Kuroishi and Sawada (2019) find that pure altruism is most pronounced in the aftermath of a disaster but erodes over time, indicating much less altruism in non-disaster environments. Iowa farmers' *trust* in senders of climate information influences perceived risks from climate change and, in turn, adaptation behaviours (Arbuckle *et al.* 2015). Miao *et al.* (2015) identify a positive link between social trust and the propensity for collective action studying Chinese farmers (Shaanxi Province).

The above-described behavioural traits and their economic relevance are difficult to measure with real-world data. Many variables simultaneously change in reality and, in turn, interact with the behavioural traits of interest (e.g. Cohn *et al.* 2015). To overcome these concerns, economists often resort to experiments. Economic experiments are aimed at combining the advantage of revealed-preference approaches with a systematic control of the

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environment by testing how people behave in various decision environments (Smith 1976; Smith 1982). Therefore, economic experiments are also seen as a promising resource in the impact analysis of regulatory policies aimed at influencing farmers' sustainability-relevant behaviours (e.g. Colen *et al*. 2016). To become a valuable practical tool, experiments would have to be aimed at gauging farmers' would-be behaviours contingent on changes in their decision environments brought about by governments or other regulators. However, using students instead of representatives of the social group of interest is widespread in behavioural experiments (cf., Nguyen 2020).

Experiments with university students provide several advantages. The costs are low and the recruitment is relatively straightforward, which also increases the feasibility of replication studies (e.g. Belot *et al*. 2015; Fréchette 2015). Students have steeper learning curves and thus quickly penetrate an experiment (e.g. understanding abstract instructions) better than non-students. According to Belot *et al*. (2015), students are a relatively homogenous population, which also allows experimenters to gain insights at relatively low costs because low sample sizes may be sufficient to isolate findings. By contrast, while representing the benchmark from a validity point of view, conducting natural field experiments (Harrison and List 2004; Samek 2019), that is experiments with subjects from the social group of interest in their real-life environment, is often not feasible because of the considerable financial, moral and legal requirements. However, students are often younger on average, have less income and are more gender-representative than the social group of interest (Friedman and Cassar 2004; Johnson and Mislin 2011; Belot *et al*. 2015).

There are several experimental studies that find behavioural differences between students and non-students. It has been found that students tend to have less pro-social traits than professionals (Alatas *et al*. 2009; Anderson *et al*. 2013; Falk *et al*. 2013; Camerer 2015). For example, Fehr and List (2004) compare students from the University of Costa Rica and CEOs from the coffee mill sector with the help of a trust game. They find that CEOs transfer more and send more money back. Belot *et al*. (2015) use classic experimental games to analyse students and non-students. Students behave more selfishly and rationally than non-students. List and Haigh (2005) and Haigh and List (2005) find evidence that professional traders recruited from the CBOT are myopic loss averse to a greater extent than students and that traders fall prey to the Allais paradox less often. In the light of subject pool differences, Cason and Wu (2019) argue that students are appropriate to scrutinise theories with a general claim to validity and professionals are more suitable to address policy-related questions about behaviours in specific decision environments. However, there are also studies that find similar behaviours of students and non-students. In a three-person ultimatum bargaining game, Güth *et al*. (2007) observe similar behaviour of students and participants in a newspaper experiment. Fréchette (2015) finds in his

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review of 13 papers that the behaviour of students and non-students is often quite similar. Differences and, in particular, a worse performance of professionals are often in line with an experimental environment that differs from the professional environment in everyday working life. Fréchette argues that non-students may import irrelevant experiences and decision heuristics into the lab.

Experimental differences between students and non-students may rest, among others, on experience and different decision heuristics, knowledge and associated thinking, and decision context. Alatas *et al.* (2009) compare Indonesian public servants and Indonesian students in a corruption experiment. They find lower tolerance to corruption with Indonesian public servant subjects. The authors give real-life *experiences* as a reason, but find no evidence for selection effects. Carpenter and Seki (2006) compare university students from Japan and Japanese shrimp fishermen in voluntary contribution mechanism experiments. Non-students behaved more cooperatively than students. The authors emphasise that cooperation among non-students is especially pronounced if they are organised in groups that share income and operate expenses. Burns (1985) compares microeconomics undergraduates and experienced wool buyers in an auction experiment and finds that students perform better. But the experiment was not designed in such a way that professionals could benefit from their key competence. Abbink and Rockenbach (2006) examine students and employees 'from an influential German Bank in Frankfurt/Main' in an option pricing experiment with neutral language. Students perform better; professionals have more *knowledge*, but they are more intuitive, less analytical. Cooper *et al.* (1999) compare Chinese students and Chinese managers with relevant field experience in a ratchet effect game. The authors use both generic terms and context (real terms). They find similar behaviour from students and non-students, but *context* is helpful for non-students (strategic play increases for managers, little influence on students).

However, despite substantial differences between students and non-students and mixed but limited evidence from systematic comparisons, the experimental behaviour of students is sometimes prematurely generalised to other social groups or even to humans in general. For example, Fehr and Gächter (2002) use the headline 'Altruistic Punishment in Humans' in their Nature publication that rests on students only. However, the validity for a larger social group must be critically questioned on a case-by-case basis when reporting and interpreting results. From a technical point of view, using the observed behaviour of students to approximate the unobserved behaviour of another social group is critical when differing individual characteristics of the members of the respective group ('covariates') are correlated with the behavioural outcome of interest (e.g. Henrich *et al.* 2010). According to Henrich *et al.* (2010) researchers often implicitly assume that WEIRD samples ('Western, Educated, Industrialized, Rich, and Democratic') are representative for people in general. But they conclude in their literature

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review that the results found in WEIRD samples are often unsuitable to derive universal statements. For example, Herrmann *et al.* (2008) find substantial differences in experimental behaviour between Western and non-Western cultures. But also within a culture, relevant differences in experimental behaviour were found between different groups of people such as students and entrepreneurs (e.g. Abbink and Rockenbach 2006; Levitt and List 2007).

The question of how well we can approximate the behaviour of the respective professional group through the behaviour of students may also depend on other features of the research design, in particular, the incentivisation (i.e. task-related payments) of the experiment. The economic experimental literature considers salient incentivisation as crucial to make subjects' choices in the experiment 'realistic' by attaching real consequences to them (Azrieli *et al.* 2018). By making pay-offs salient, economists intend to avoid, or at least mitigate, hypothetical bias in which people overstate their willingness to pay (e.g. Penn and Hu 2018). Consequently, incentivisation is often regarded as a methodological precondition for revealing people's preferences and, therefore, for publishing experimental work in a major economic journal (Bardsley *et al.* 2010). The argumentation of economists is well illustrated by the labour theory (Smith and Walker 1993) and the capital–labour–production theory (Camerer and Hogarth 1999). Smith and Walker (1993) assume that cognitive effort is a scarce resource. They argue that higher incentives are correlated with higher cognitive effort and, in turn, reduce errors. To put it differently, salient pay-offs reduce performance variability. Camerer and Hogarth (1999) extend Smith and Walker (1993) by considering the 'cognitive capital' of the individuals (i.e. skills, knowledge and experience) at the time of the task.

In incentivised studies that compare non-students with students, some researchers argue that non-students should receive higher monetary incentives than students because the former are likely to have higher opportunity costs (Haigh and List 2005; Alevy *et al.* 2009). Others emphasise the importance of identical decision environments for both groups and use identical monetary incentives (e.g. Dyer *et al.* 1989; Falk *et al.* 2013). By contrast, psychologists criticise the economists' focus on task-related payments (Voslinsky and Azar 2021). For example, Kahneman and Tversky (1979) argue that experimental subjects have both an idea of how they would behave in real situations and no reason to conceal their true preferences. According to Hertwig and Ortmann (2001), there are situations in life in which monetary incentives do not necessarily lead to a greater approximation of reality. These include social interactions, memory tasks and problem-solving. Moreover, psychologists argue that experimental subjects are intrinsically motivated to perform well (Camerer and Hogarth 1999). Monetary incentives can, however, have unintended consequences and reduce individuals' performance if they are perceived as low (Gneezy and Rustichini 2000).

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Given the relevance of behavioural traits for entrepreneurial decisions in the agricultural sector and the above-described methodological challenges associated with subject pools and incentives in economic experiments, the following two research questions emerge:

1. Can the behaviour of farmers in simple economic experiments be approximated by agricultural students?
2. Does the level of monetary incentives influence the behaviour of these two groups in the experiments?

To address these questions, we adopt the categorisation of preferences of Falk *et al.* (2018) in our experimental study (risk aversion, impatience, positive and negative reciprocity, altruism and trust). However, our study differs from Falk *et al.* (2018) in two dimensions both of which put scepticism on their idea to compare highly aggregated data (e.g. average risk aversion) between countries. First, instead of collecting data on hypothetical choice tasks (i.e. stated preferences), we conduct incentivised experiments for each of the preferences. We add value to the literature by systematically changing the level of the monetary incentives considering that some decision situations are more important than others. Second, we compare proponents of the two subpopulations students of agricultural sciences and farmers (non-students), and add value to the academic discourse on the generalisability of experimental findings with university students.

Although both research questions address methodological issues, they are of considerable practical relevance for agricultural economists who conduct experiments. If the experimental behaviour of the convenience group of students approximates the behaviour of farmers, financial and time resources could be saved by recruiting university students. Similarly, researchers can reduce their expenses if experimental subjects show similar behaviours independently of the level of monetary incentives. The preserved resources can, for example, be used to increase the sample size. If, however, there is a behavioural gap between both populations and/or the level of monetary incentives, upper and lower boundaries can be used as corrective for the population of interest. Let us assume, for illustration purposes, that agricultural students were, in general, less pro-social in their experimental behaviour than farmers. Researchers, who conduct studies with agricultural students, might then make inferences to farmers by qualitatively claiming that their results represent a lower bound of the behaviour of farmers. Note that it is often neither possible nor necessary to disentangle all drivers of behaviour. For many relevant questions, analysing a population as a whole with its unique characteristics (e.g. farmers with their specific job experiences, male–female ratio, age) provides valuable information.

Our paper extends a study of Maart-Noelck and Musshoff (2014), which compares the subject pools of German farmers, German students and

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Kazakhstani farmers. Instead of being limited to one behavioural driver, we systematically address several preferences that are relevant for entrepreneurial decision-making in the agricultural sector. Our study is also related to Snowberg and Yariv (2021) who compare students at Caltech, MTurkers and a representative sample of the United States. They find that students serve as a lower boundary for risk aversion, impatience and generosity and as an upper boundary for strategic behaviour and cognitive skills. However, their findings are limited by the fact that the monetary payments for Caltech students (a rather untypical population, e.g. highly selective school) were almost three times higher than the payments for non-students. Our study contributes to the literature by systematically analysing the relevance of monetary incentives. Moreover, being especially interested in the behaviour of individuals from the agricultural sector, we recruit farmers and, somewhat relatedly, agricultural students.

The rest of the paper is structured as follows: in Section 2, we describe the basic study design, the recruiting procedure and the characteristics of experimental subjects. Section 3 contains details on the experimental design and the respective findings. In Section 4, we discuss our findings and make concluding remarks.

2. Basic study design and experimental subjects

2.1 Basic study design

We conduct an experiment for each of the following behavioural constructs: risk aversion, impatience, positive reciprocity, negative reciprocity, altruism and trust (cf., Falk *et al.* 2018). This study has been approved by the German Association for Experimental Economic Research e.V. (No. 1VjNTvM3). As there is a replication crisis in the social sciences (including environmental and resource economics; Ferraro and Shukla 2020), we pre-registered our basic design, research questions and approach to data analysis before collecting data (AsPredicted #45697; Appendix S3) to increase the credibility of our findings.

To measure the above-mentioned behavioural constructs, we use standard tasks from the literature that have been used frequently used to elicit these constructs. We use the Holt and Laury procedure (2002) to elicit risk aversion, the procedure according to Laury *et al.* (2012) to measure impatience, a gift exchange game (cf., Charness *et al.* 2004) to capture positive reciprocity, an ultimatum bargaining game (cf., Güth *et al.* 1982) to assess negative reciprocity, a dictator experiment (cf., Engel 2011) to gauge altruism, and a trust game (cf., Kosfeld *et al.* 2005) to assess trust in others (cf., Table 1). All constructs are measured in one-shot (non-iterated)

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Table 1 Theoretical constructs and their experimental measurement

Theoretical constructs	Risk aversion	Impatience	Positive Reciprocity	Negative Reciprocity	Altruism	Trust
Experimental design	Holt and Laury (2002)	Laury *et al.* (2012)	Gift exchange game (cf., Charness *et al.* 2004)	Ultimatum bargaining game (cf., Güth *et al.* 1982)	Dictator experiment (cf., Engel 2011)	Trust game (cf., Kosfeld *et al.* 2005)

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experiments. Rational choice predictions, if available, were not communicated to the subjects.[1]

Two subject pools, agricultural students and farmers, were recruited. The payment of the subjects comprises a show-up fee and monetary incentives. Agricultural students received a show-up fee of €5, farmers received €20. With the exception of the show-up fee, the whole experimental design is identical for both populations to make the environment as similar as possible. Show-up fees are not linked to any actions of interest in the experimental study. They only serve the goal of recruiting a sufficient number of subjects (Bardsley *et al.* 2010). It would have been very difficult to recruit farmers with a lower show-up fee. But we also refrain from a higher show-up fee for students due to budget constraints. This practice is in line with other studies that analyse different subject pools (e.g. Peth and Mußhoff 2020). All subjects were randomly assigned to one of three monetary incentive levels (between-subject design) and monetary incentives were provided to 10% of the subjects who were randomly selected. In the following, we refer to these categories as treatment 1 (lowest monetary incentive level), treatment 2 (medium monetary incentive level) and treatment 3 (highest monetary incentive level). However, depending on the type of the experiment, different incentive levels are used to account for the fact that complexity, and therefore, the required time and mental effort differ considerably.

2.2 Recruitment of experimental subjects

2.2.1 Recruitment of agricultural students

Our starting point was a website (agrarstudieren.de/universitaeten) aimed at providing young people with information about universities where they can study agriculture in Germany. A total of 10 universities are listed there.[2] From each university, we contacted the dean of the agricultural and environmental sciences faculty, a randomly selected professor from the agricultural department, and the agricultural and food sciences student council with the request to advertise the study. All contacted individuals received a link from us that allowed agricultural students to attend the Internet-based study. We were told that the people are happy to support our

[1] We did not randomize the order of the tasks. Similarly, Belot *et al.* (2015) did also present the tasks in the same order in each of their sessions. In our study, the respective tasks are independent of each other (no practice effects). Moreover, we did not provide subjects with information on their performance in the respective tasks. They were paid after all tasks had been completed. The time lag between making decisions and getting paid (subjects were paid via bank transfer since cash payments are not allowed at the institution where the study has been carried out) helps to avoid house money effects (cf., Thaler and Johnson 1990). In addition, the number of possible combinations is very large.

[2] The list reads as follows: Christian-Albrechts-Universität zu Kiel, Georg-August-Universität Göttingen, Humboldt-Universität zu Berlin, Justus-Liebig-Universität Gießen, Martin-Luther-Universität Halle-Wittenberg, Rheinische-Friedrich-Wilhelms-Universität Bonn, Technische Universität München, Universität Hohenheim, Universität Kassel and Universität Rostock.

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project and forward the link to the study. Several students from different universities also expressed interest in the results of the study. Thus, we have agricultural students from different German universities in our sample.

2.2.2 Recruitment of farmers

While the recruitment of the agricultural students was straightforward, recruiting professional farmers was quite difficult.[3] In anticipation of that, our recruitment procedure for farmers was more comprehensive and entailed the sequential use of four different recruitment channels. First, we tried to get a quota-representative sample with regard to age, gender, region and income for the farmers in Germany. Therefore, in a very early stage of the study (during writing the grant application), we contacted professional online-access-panel providers if this would be feasible. We were told that it is not possible and, as a consequence, we switched to a more practical solution. Second, we contacted the heads of all 16 farmer's associations of the federal states (*Landesbauernverbände*) as well as local farmer's associations within the federal states (*Kreisbauernverbände*) with the plea to support and advertise our study (e.g. via the newsletter to their members). We provided a link with access to the study to the farmers. Third, since this did not suffice to recruit a sufficient number of subjects, we also contacted the head of the Initiative of Domestic Agriculture (*Initiative Heimische Landwirtschaft*) with a similar request. They were also given a link to the study. Fourth, we approached farms providing professional agricultural training (*Ausbildungsbetriebe*) in Germany to participate in the study. Similarly, we also provided them with a link to the study.

2.2.3 Remark on inference

Statistical inferential procedures are based on probability theory and a formal chance model that links a randomly generated data set to a broader context. Therefore, they presuppose either random sampling or randomisation and are limited to dealing with uncertainty caused by random error. In our study, standard errors (and *P*-values) can be used to assess the randomisation-induced uncertainty surrounding the estimation of the incentive treatment effect. This is because the subjects in the respective subject pools were randomly assigned to various incentive levels. However, both students and farmers represent convenience samples that were not randomly selected from a population. These assumption violations preclude using inferential statistics to assess the random error-induced uncertainty in the estimation of population effect sizes. For a meaningful use of standard errors and *P*-values, one would have to assume that these convenience samples represent

[3] In general, recruiting non-students often poses a challenge. For example, Belot *et al.* (2015) use different types of recruiting schemes (e.g. emailing non-students and placing advertisements in newspapers and local pubs). This is not an ideal solution but is often necessary for practice (cf., Cooper 2007).

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approximately random samples. While it should be observed that the informative value of *P*-values is quite limited in all the widespread cases where data are non-random, we nonetheless display them in the student–farmer comparisons. For one thing, this still seems to be the required practice that is adopted by many researchers (e.g. Snowberg and Yariv 2021). Furthermore, some researchers may find *P*-values genuinely informative as summary statistics of a given data set without explicitly questioning whether there is a chance model upon which to base statistical inference in the first place. Whether *P*-values are helpful in the light of data collection is thus left a transparent issue open for judgement by the reader.

2.3 Description of experimental subjects

The study was conducted Internet-based from 14.09.2020–13.11.2020. We refrained from carrying out the study in the laboratory, which would have led to high transaction costs for both populations and unnecessary health risks during the COVID-19 pandemic. Overall, we recruited 150 agricultural students and 150 farmers in Germany. As expected, farmers are on average older than agricultural students (cf., Table 2). The vast majority of the farmers are male, whereas the majority of agricultural students are female. The differences in gender are not surprising. The agricultural sector in Germany is overwhelmingly represented by men, whereas only 10% of the farm managers in Germany are female. Similarly, the fraction of female farmers amounts to 20.2% in Maart-Noelck and Musshoff (2014), 0% in Mußhoff *et al.* (2018) and 7.4% in Peth and Mußhoff (2020). By contrast, various studies have shown that the proportion of male and female

Table 2 Description of the subjects

		Farmers (N = 150)		Agricultural students (N = 150)		Difference	
		M	*SD*	*M*	*SD*	*M*	*SD*
Age (in years)		40.86	11.79	23.20	3.28	17.66	8.51
Gender	Male (in %)	84.00	-	25.33	-	58.67	
	Female (in %)	16.00	-	74.00	-	–58.00	
	Other (in %)	0.00	-	0.67	-	– 0.67	
Financial situation†		3.33	0.97	3.28	0.93	0.04	0.03
Happiness‡		6.78	2.12	7.21	2.29	–0.42	–0.16
Political attitude§		5.64	1.72	3.54	1.59	2.10	0.13
Religious group¶		0.82	-	0.59	-	0.23	-

†How would you rate your current financial situation? (1 = bad, ..., 5 = very good).
‡How satisfied are you, all in all, with your life at present? (0 = completely dissatisfied, . . ., 10 = completely satisfied).
§In politics people often talk about 'left' and 'right' to mark different political attitudes. If you think about your own political attitude: Where would you place yourself? (0 = very left, . . ., 10 = very right).
¶Do you belong to a church or religious community? (1 = yes, 0 = no).

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agricultural students is roughly about equal (Lehberger and Hirschauer 2014; Grüner *et al.* 2021).[4] It must be noted, however, that the gender differences across the two samples (both overrepresenting women compared to the respective population of Germany) limit the generalisability of our findings. Both subject pools were equally satisfied with their financial situation on average. Students indicated themselves a little bit happier than farmers did. There are differences in the political attitudes between the subject pools: students were on average more left-oriented than farmers. The majority of subjects indicated to be a member of a religious group. The latter was more pronounced with the farmers.[5] Moreover, the agricultural students were asked whether they want to become a farmer after graduation. Overall, about one-third of the respondents indicated that becoming a farmer is either planned or that they can well imagine it. One-fifth of the students stated that they are unsure and the rest of the respondents do not intend to become a farmer at the moment or do not consider it as an option.

3. Behaviour of students and farmers in six experiments

In this section, we present the behaviour (i.e. decisions) of the subjects in the six behavioural experiments. After a short description of the respective design (Appendix S1 contains the data and code, the translated instructions can be found in the Appendix S2), the main findings with regard to the research questions are presented and discussed. We provide figures and mean values to illustrate the effect sizes of the subject pool and incentive schemes. Both research questions outlined above are also analysed by assuming the null of having no differences between the populations and no effect of the various incentive levels. To analyse the null of no population effects, we resort to rank sum tests, and one-way analysis of variance (ANOVA) is used to address the null of no monetary incentive effects. Since the paper presents a total of six experiments, two populations and three incentive schemes, we selectively provide test statistics. A more comprehensive presentation can be found in Appendix S5.

3.1 Experiment 1: Risk aversion

3.1.1 Basic design

To measure risk aversion, we use the Holt and Laury (2002) risk elicitation procedure. In this procedure, subjects are shown 10 pairs of lotteries in each

[4] A similar pattern has been found by Grüner and Khassine (2022) who compare the behaviour of chess players and students in a deception game. While only 9.44% of the non-students are female, 63.3% of the student sample indicated themselves as female. However, it is left to future research to find out why more women attended.

[5] Baseline comparisons to show that there are no systematic differences between the treatments (i.e., that differences between the subjects are not influencing the treatment) can be found in Appendix S4.

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of which they have to decide between an option A and an option B. For example, in lottery pair 1, participants must decide between an option A, with probabilities 1/10 and 9/10 of either winning €2.00 or €1.60, and an option B, with probabilities 1/10 and 9/10 of either winning €3.85 or €0.10. The difference between the winning amounts (pay-offs) are relatively low in option A ('safer option') but relatively high in option B ('riskier option'). The absolute pay-offs of either option remain unchanged over all 10 lottery pairs. However, the probabilities of the lower (higher) pay-offs are systematically decreased (increased). As a consequence, the difference in the expected pay-offs between A and B decreases from lottery pair 1 to 10. The number of safer A-choices is understood to indicate the individual risk attitude (0 to 3 risk loving, 4 risk neutral and 5 to 9/10 risk averse).

3.1.2 Monetary incentivisation

The payees (i.e. 10% of all subjects that were randomly selected) are monetarily rewarded following a two-step procedure: first, one of the ten decision situations (lottery pairs) is randomly selected; second, the subject's preferred lottery, A or B, for the randomly selected lottery pair is played for real and the resulting amount is paid out to the subject. The level of the payment depends on the scenario. The pay-offs shown in Table 3 (A: €2.00 vs. €1.60; B: €3.85 vs. €0.10) reflect treatment 1. The pay-offs of treatment 2 were scaled up by the factor 10 (A: €20.00 vs. €16.00; B: €38.50 vs. €1.00); and the pay-offs of treatment 3 were scaled up by the factor 50 (A: €100.00 vs. €80.00; B: €192.50 vs. €5.00).

3.1.3 Experimental findings and discussion

Figure 1 shows the proportions of safer choices that students and farmers made in each of the ten decisions of the Holt and Laury (2002) procedure. These proportions are (near) identical in both groups across all decision situations within the respective incentive treatments. Consequently, the average number of safer A-choices M is also very similar between farmers and agricultural students within the respective incentive treatment ($M_S = 4.84$ and $M_F = 5.02$ in treatment 1, P-value = 0.7927; $M_S = M_F = 5.90$ in treatment 2, P-value = 0.8475; and $M_S = 5.74$ and $M_F = 5.62$ in treatment 3, P-value = 0.8289). This finding contradicts Maart-Noelck and Musshoff (2014) who find agricultural students to be more risk averse than farmers. The authors use relatively high monetary stakes in their study but monetarily rewarded only 1 in 100 subjects. Moreover, we see that in both groups, the level of the monetary incentives influences the average number of safer choices (P-value = 0.0113 for students; P-value = 0.0929 for farmers): both students and farmers exhibit the lowest number of safe choices (lowest risk aversion) in treatment 1, and the highest risk aversion in treatment 2. In both groups, the difference between treatments 2 and 3 was minor, however. This is interesting because earlier studies found a generally positive relationship between monetary incentives and individual risk aversion measured as

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Table 3 Paired lottery-choice decisions (Holt and Laury 2002): treatment 1[†]

Lottery pair	Option A	Option B	Expected pay-off difference between A and B in €	Number of safe A-choices	Range of relative risk aversion[‡]	Risk preference classification
1	P(€2.00) = 10% P(€1.60) = 90%	P(€3.85) = 10% P(€0.10) = 90%	1.17	0–1	$r < -0.95$	Highly risk loving
2	P(€2.00) = 20% P(€1.60) = 80%	P(€3.85) = 20% P(€0.10) = 80%	0.83	2	$-0.95 < r < -0.49$	Very risk loving
3	P(€2.00) = 30% P(€1.60) = 70%	P(€3.85) = 30% P(€0.10) = 70%	0.50	3	$-0.49 < r < -0.15$	Risk loving
4	P(€2.00) = 40% P(€1.60) = 60%	P(€3.85) = 40% P(€0.10) = 60%	0.16	4	$-0.15 < r < 0.15$	Risk neutral
5	P(€2.00) = 50% P(€1.60) = 50%	P(€3.85) = 50% P(€0.10) = 50%	−0.18	5	$0.15 < r < 0.41$	Slightly risk averse
6	P(€2.00) = 60% P(€1.60) = 40%	P(€3.85) = 60% P(€0.10) = 40%	−0.51	6	$0.41 < r < 0.68$	Risk averse
7	P(€2.00) = 70% P(€1.60) = 30%	P(€3.85) = 70% P(€0.10) = 30%	−0.85	7	$0.68 < r < 0.97$	Very risk averse
8	P(€2.00) = 80% P(€1.60) = 20%	P(€3.85) = 80% P(€0.10) = 20%	−1.18	8	$0.97 < r < 1.37$	Highly risk averse
9	P(€2.00) = 90% P(€1.60) = 10%	P(€3.85) = 90% P(€0.10) = 10%	−1.52	9–10	$1.37 < r$	Stay in bed
10	P(€2.00) = 100% P(€1.60) = 0%	P(€3.85) = 100% P(€0.10) = 0%	−1.85			

[†]The last four columns were not shown to the subjects.
[‡]Assuming $U(x) = x^{1-r} / (1-r)$.

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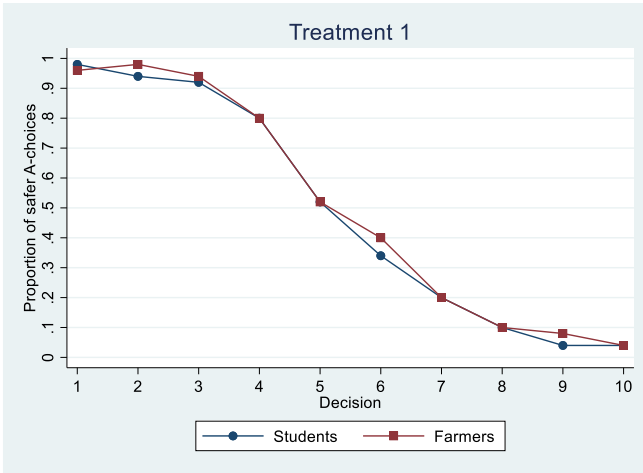


Average number (M) and standard deviation (SD) of safer choices

Students (N=50)

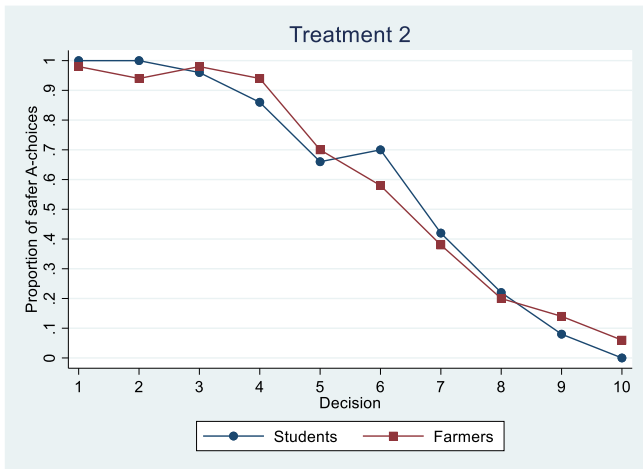
M_S=4.84

SD_S=1.82

Farmers (N=50)

M_F=5.02

SD_F=1.88



Average number (M) and standard deviation (SD) of safer choices

Students (N=50)

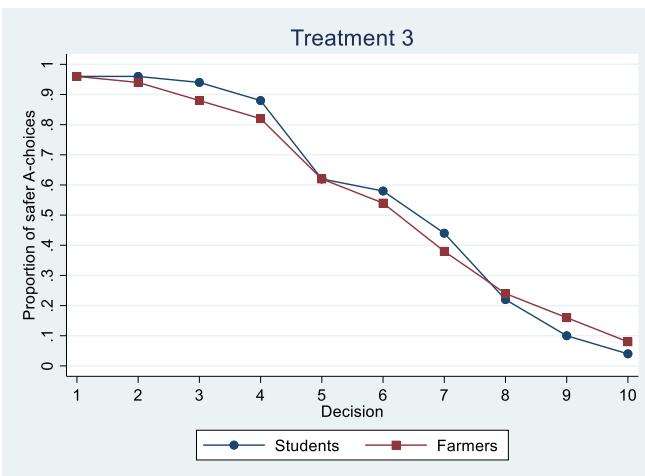
M_S=5.90

SD_S=1.84

Farmers (N=50)

M_F=5.90

SD_F=1.91



Average number (M) and standard deviation (SD) of safer choices

Students (N=50)

M_S=5.74

SD_S=1.96

Farmers (N=50)

M_F=5.62

SD_F=2.30

Figure 1 Risk aversion of students opposed to farmers as assessed in the experiment. [Colour figure can be viewed at wileyonlinelibrary.com]

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number of safer choices (i.e. stake size effect; Holt and Laury 2002, Holt and Laury 2005). The change in behaviour from treatment 2 to treatment 3 might be explained by the following heuristic: "I have started with €0 and can now earn almost €400—so, I go for it".

3.2 Experiment 2: Impatience

3.2.1 Experimental design

To measure time preferences, we use the multiple price list format of Laury *et al.* (2012). Subjects choose between a 'sooner' lottery A and a 'later' lottery B for a total of 20 decision situations. The sooner lottery offers a 50% chance to win €X in 3 weeks. The later lottery offers the same pay-off, but it is not paid out until 12 weeks after finishing the experiment. To compensate for waiting, the chances to win the prize €X in the later lottery B are systematically increased from row 1 to 20. In decision row 1, the chances between lottery A and B are equal, corresponding to an interest rate of 0%. Starting with decision row 2, the probability to win €X in lottery B continuously increases. In each row, individuals must make a choice. The number of A-choices (sooner choices) is understood to describe the individual's impatience: the more A-choices, the more impatient.

3.2.2 Monetary incentivisation

A total of 10% of the subjects are paid according to the following two-step procedure: first, one of the 20 decision situations (lottery pairs) is randomly selected; second, the subject's preferred lottery, A or B, for the randomly selected lottery pair is played for real and the resulting amount, if any, is paid out to the subject—either after 3 weeks (option A) or 12 weeks (option B). As a consequence, the payment of the subjects was organised in a manner that the bank transfer to the subjects was carried out within either the former or latter period of time. The three incentive treatments under study differ in the absolute lottery pay-off X. The pay-off amounts to X = 2 in treatment 1, to X = 20 in treatment 2 and to X = 200 in treatment 3 (Table 4).

3.2.3 Experimental findings and discussion

Figure 2 describes the proportion of students and farmers who chose the sooner option in each of the 20 decision situations. The proportions of farmers choosing the sooner option A are consistently higher across all decision situations and treatments, indicating that they are more impatient than agricultural students. As a consequence, the average number M of sooner choices is considerably higher for farmers than for students within all three treatments ($M_F = 11.52 > M_S = 7.56$ in treatment 1, P-value = 0.0060; $M_F = 10.22 > M_S = 8.14$ in treatment 2, P-value = 0.0941; $M_F = 8.98 > M_S = 4.92$ in treatment 3, P-value = 0.0051). This finding is not in line with Cohen's *et al.* (2020) literature review on time preferences in which the authors find that average interest rate used for discounting later pay-offs is

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Table 4 Experimental design to elicit time preferences[†]

Row	Option A Chance of €X in 3 weeks (percent)	Option B Chance of €X in 12 weeks (percent)	Annual effective interest rate (per cent)	Range of (annual effective) interest rate demanded for waiting if switching in this row (per cent)
1	50.0	50.0	0.00	$\delta \leq 0.00$
2	50.0	50.1	1.01	$0.00 \leq \delta \leq 1.01$
3	50.0	50.2	2.02	$1.01 \leq \delta \leq 2.02$
4	50.0	50.4	4.08	$2.02 \leq \delta \leq 4.08$
5	50.0	50.5	6.18	$4.08 \leq \delta \leq 6.18$
6	50.0	50.7	8.33	$6.18 \leq \delta \leq 8.33$
7	50.0	50.9	10.52	$8.33 \leq \delta \leq 10.52$
8	50.0	51.1	12.75	$10.52 \leq \delta \leq 12.75$
9	50.0	51.2	15.02	$12.75 \leq \delta \leq 15.02$
10	50.0	51.4	17.35	$15.02 \leq \delta \leq 17.35$
11	50.0	51.6	19.72	$17.35 \leq \delta \leq 19.72$
12	50.0	51.8	22.13	$19.72 \leq \delta \leq 22.13$
13	50.0	52.0	25.22	$22.13 \leq \delta \leq 25.22$
14	50.0	52.2	28.39	$25.22 \leq \delta \leq 28.39$
15	50.0	52.7	34.97	$28.39 \leq \delta \leq 34.97$
16	50.0	53.6	49.15	$34.97 \leq \delta \leq 49.15$
17	50.0	54.5	64.82	$49.15 \leq \delta \leq 64.82$
18	50.0	56.9	111.54	$64.82 \leq \delta \leq 111.54$
19	50.0	59.4	171.46	$111.54 \leq \delta \leq 171.46$
20	50.0	64.7	346.79	$171.46 \leq \delta \leq 346.79$

[†]The last two columns were not shown to the subjects.

about the same between university students and 'representative samples'. However, we found considerable differences between the subject pools, with university students being more patient than farmers. In other words, agricultural students are a lower boundary of the behaviour of the farmers. Moreover, we find that in both groups the average number of sooner choices is considerably lower in the highest incentive level compared to the very similar low and medium incentive levels (behavioural monetary incentive effects seem to be more pronounced with students, P-value = 0.0377, than with farmers, P-value = 0.1823). In other words, subjects are more ready to wait when the stakes are high. This is in line with Ericson *et al.* (2015), who use psychological principles to account for the negative association of scaling up monetary rewards and decreasing impatience (e.g. the Intertemporal Choice Heuristics Model).

3.3 Experiment 3: Positive reciprocity

3.3.1 Experimental design

To measure positive reciprocity, we rely on the two-person gift exchange game of Charness *et al.* (2004). The game is framed as an employment situation in which an employer makes a wage offer to an employee. This wage offer can be varied in equal steps of 10 between 0 and 100 monetary units

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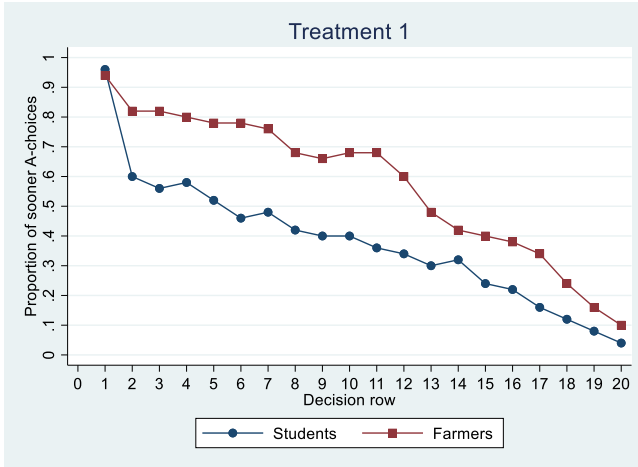


Average number (M) and standard deviation (SD) of sooner choices

Students (N=50)

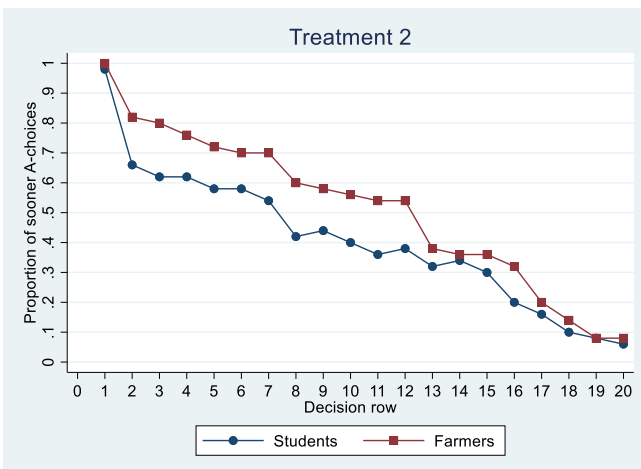
M_S=7.56

SD_S=6.91

Farmers (N=50)

M_F=11.52

SD_F=6.59



Average number (M) and standard deviation (SD) of sooner choices

Students (N=50)

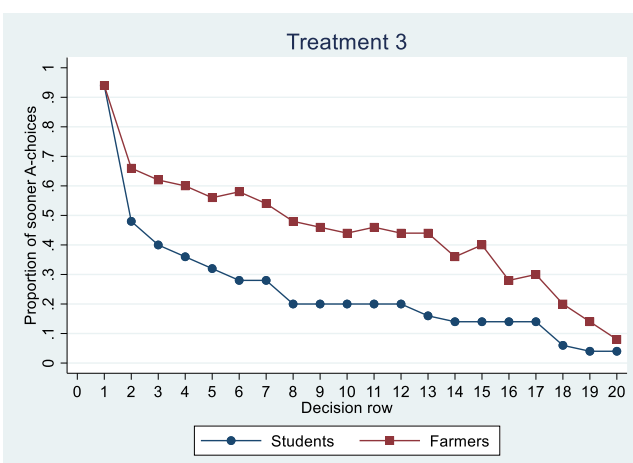
M_S=8.14

SD_S=6.83

Farmers (N=50)

M_F=10.22

SD_F=6.50



Average number (M) and standard deviation (SD) of sooner choices

Students (N=50)

M_S=4.92

SD_S=6.11

Farmers (N=50)

M_F=8.98

SD_F=7.39

Figure 2 Impatience of students opposed to farmers as assessed in the experiment. [Colour figure can be viewed at wileyonlinelibrary.com]

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Table 5 The employee's effort levels and costs of effort

Effort level	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
Cost	0	1	2	4	6	8	10	12	15	18

(MU). After seeing the employer's wage offer, the employee has the choice between costly effort levels. The effort levels to choose from and their respective costs for the employee are shown in Table 5.

The pay-offs of the employer and the employee are mutually dependent on each other's choice. Their respective pay-off functions (in MU) are mutually known and read as follows:

$$\text{Pay}-\text{off (employer)} = (100-\text{wage}) \cdot \text{employee's effort}$$

$$\text{Pay}-\text{off (employee)} = \text{wage}-\text{cost of effort}$$

Aiming to measure positive reciprocity, we are primarily interested in how subjects in the position of employee respond to (reciprocate) more or less generous wage offers. We, therefore, confront all subjects in a multiple price list with systematically varied wage offers (0, 10, 20, ..., 100) and ask which effort level (0.1, 0.2, 0.3, . . ., 1) they would choose in response, respectively.

3.3.2 Monetary incentivisation

To determine the real pay-offs, we additionally asked all subjects to indicate the wage level they would choose in the position of employer. Next, we randomly and anonymously matched subjects with each other (i.e. by conducting virtual pairs of their decision behaviour in the experiment) and determined subjects' pay-offs in the position of employee according to the pay-off function and the choices of the matched pair. Finally, 10% of all subjects were randomly selected as payees. Again, three different incentive levels (treatments) were studied. They differ in the exchange rate from MU in Euro. In treatment 1, subjects received €0.10 for 1 MU. In treatments 2 and 3, the respective amounts were €0.50 and €2.00.

3.3.3 Experimental findings and discussion

Axiomatic game theory based on the conventional rational choice paradigm predicts rational subjects to choose the lowest possible effort level since the employer cannot punish such a behaviour in a one-shot game (Falk and Heckman 2009). However, what we found was different. Figure 3 shows the average effort of the two groups, students and farmers, for each of the wage levels in the gift exchange game. The average group effort is near-identical for farmers and students across all wage offers in treatment 1, slightly higher for farmers in treatment 2 (especially near the endpoints of wages), and systematically but only slightly higher with farmers in treatment 3. Because

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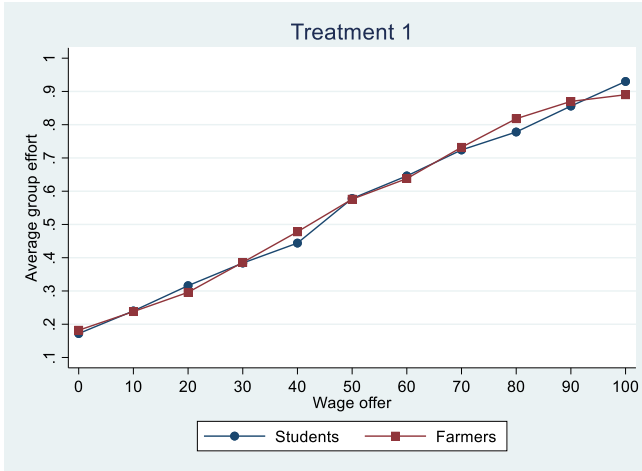


Average individual effort across all wage offers (M) and standard deviation (SD)

Students (N=50)

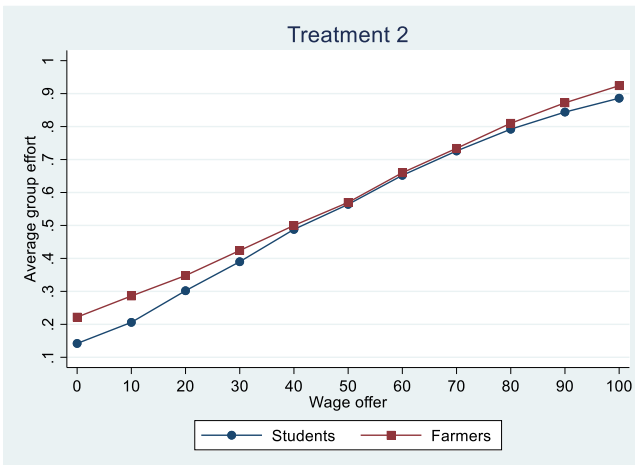
M_S=0.551

SD_S=0.142

Farmers (N=50)

M_F=0.554

SD_F=0.157



Average individual effort across all wage offers (M) and standard deviation (SD)

Students (N=50)

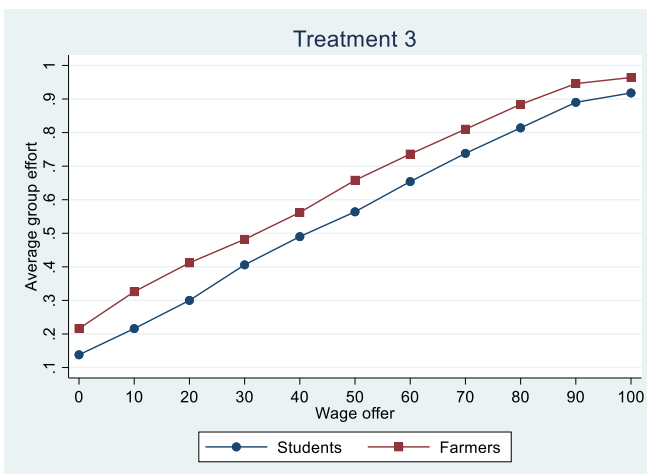
M_S=0.544

SD_S=0.163

Farmers (N=50)

M_F=0.577

SD_F=0.191



Average individual effort across all wage offers (M) and standard deviation (SD)

Students (N=50)

M_S =0.557

SD_S=0.156

Farmers (N=50)

M_F=0.636

SD_F=0.179

Figure 3 Positive reciprocity of students opposed to farmers as assessed in the experiment. [Colour figure can be viewed at wileyonlinelibrary.com]

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of the very similar efforts that were chosen by both groups in response to each wage offer, the average individual effort across all wage offers M (positive reciprocity) is also comparable. It is identical in treatment 1 ($M_S = 0.551$, $M_F = 0.554$, P-value = 0.7745), slightly higher for farmers in treatment 2 ($M_S = 0.544$, $M_F = 0.577$, P-value = 0.5364), and somewhat higher for farmers in treatment 3 ($M_S = 0.557$, $M_F = 0.636$, P-value = 0.1297). To put it differently, although the mean values indicate agricultural students to be a lower boundary of the behaviour of the farmers (with the exception of treatment 1), the null of no population effects cannot be rejected in either of the three treatments. Our results contradict Cappelen *et al.* (2015) who compare students with a nationally representative sample of the adult population in Norway. The authors find fundamental differences in reciprocity between both populations. Contrary to agricultural students (P-value = 0.9229), we find that farmers' positive reciprocity is positively associated with the incentive level (P-value = 0.0635). More precisely, positive reciprocity is similar across the monetary incentive levels for both groups, except for a slight increase with farmers. In contrast to theoretical predictions, both groups clearly rewarded kind initial behaviour by choosing higher efforts to reciprocate more generous wage offers. Our findings are related to Fehr *et al.* (2014) who find in a series of labour market experiments that stakes have little impact on behaviour and do not undermine fair behaviour.

3.4 Experiment 4: Negative reciprocity

3.4.1 Experimental design

To measure negative reciprocity, we resort to the ultimatum bargaining game introduced by Güth *et al.* (1982). In the ultimatum bargaining game, two players bargain about the division of a given sum of money €X (initial endowment). The size of the pie X is common knowledge of both the proposer (player 1) and the responder (player 2). The proposer suggests a split of the pie. The action space is quasi continuous; that is, the proposer can give any amount of integer Euros she/he deems fit. The responder can either accept or reject the offer. If the responder accepts, the money is split the way the proposer has suggested. If the responder rejects, none of the players receives anything. To measure negative reciprocity, we are interested in the minimum amount of money player 2 accepts ('minimum accepted offer'). To put it the other way round, we measure negative reciprocity through the amount of money that subjects are prepared to forego for punishing offers that they do not consider to be fair.

3.4.2 Monetary incentivisation

To determine the real pay-offs, we matched 10% of the subjects with each other (i.e. by conducting virtual pairs of their decision behaviour in the experiment) and selected them as payees. No information on the identity of

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the subjects was communicated. Their roles, either as proposer or responder, were randomly assigned to them. The pay-offs for both players were determined according to the choices of the matched pair. If player 2 rejects the offer of player 1, none of the players receives anything; if player 2 accepts the offer of player 1, both receive the amount proposed by player 1. The three different incentive levels (treatments) under study differ in the size of the initial endowment X. In treatment 1, the pie to be split was X = €5; in treatment 2, it was X = €30; and in treatment 3, it was X = €80.

3.4.3 Experimental findings and discussion

Rational choice predicts the proposer (player 1) to offer the smallest possible, positive amount of money and the responder (player 2) to accept this amount. Being interested in measuring negative reciprocity, we focus on subjects in the position of the responder. Figure 4 depicts negative reciprocity in the experiment: the more the minimum accepted offer exceeds one Euro, the higher the negative reciprocity in terms of money subjects are ready to forego to punish 'unfair' offers. Our findings are largely in line with the experimental literature that finds that subjects punish unfair behaviours (e.g. Fehr and Gächter 2002; Kuwabara and Yu 2017). The great majority of students and farmers were ready to punish 'unfair' offers. What is more, they were ready to do so at considerable costs, indicating a high degree of negative reciprocity. Negative reciprocity (i.e. answering unkind behaviours with unkind behaviour) is more prevalent with farmers. On average, the minimum accepted offer in terms of the initial pie was consistently higher in the farmer group compared to the students across all three treatments ($M_F = 36.80\% > M_S = 33.60\%$ in treatment 1, P-value = 0.0749; $M_F = 38.60\% > M_S = 32.60\%$ in treatment 2, P-value = 0.0090; $M_F = 37.50\% > M_S = 32.82\%$ in treatment 3, P-value = 0.2157). This finding is not in line with the three-person ultimatum bargaining game of Güth *et al.* (2007) who observe similar behaviour of students and participants in a newspaper experiment. Negative reciprocity is similar across the monetary incentive levels for both agricultural students (P-value = 0.9358) and farmers (P-value = 0.8281). Our results are in line with literature reviews and meta-analyses on ultimatum bargaining experiments according to which that stake size effects are generally found to be small (Güth and Kocher 2014; Larney *et al.* 2019).

3.5 Experiment 5: Altruism

3.5.1 Experimental design

To measure altruism, we resort to the dictator experiment (cf., Engel 2011). In this two-person *game*, one subject (player 1) is initially endowed with a certain amount of €X. This subject, the 'dictator', can decide to pass on a certain share of the endowment to a second, passive 'player'. The action space is quasi continuous; that is, dictators can pass on nothing or any positive

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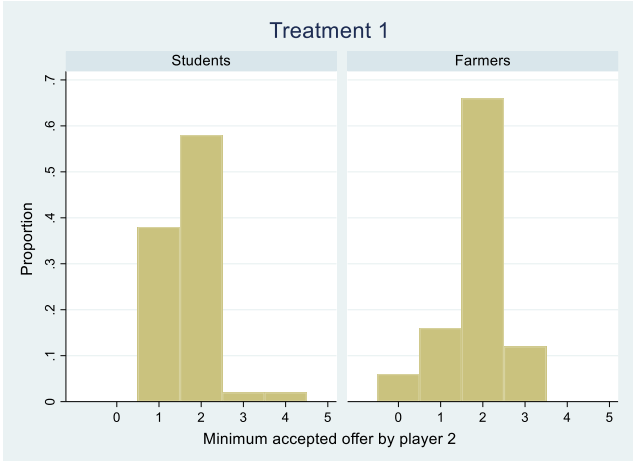


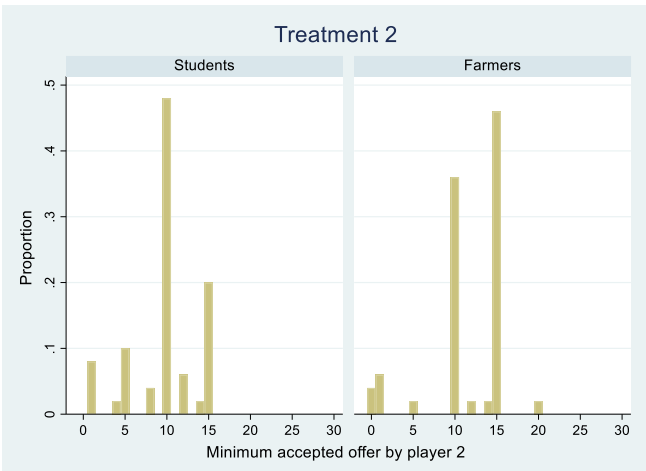
Average (M) and standard deviation (SD) of the minimum accepted offer

Students (N=50)
M_S =1.68 (=33.60% of the initial pie)

SD_S=0.62

Farmers (N=50)
M_F=1.84 (=36.80% of the initial pie)

SD_F=0.71



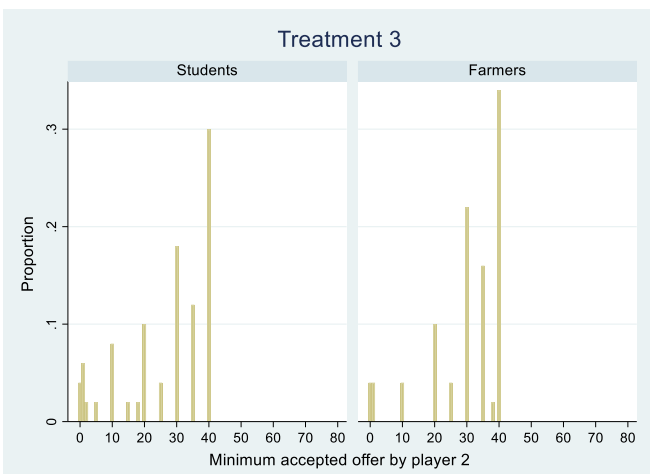
Average (M) and standard deviation (SD) of the minimum accepted offer

Students (N=50)
M_S=9.78 (=32.60% of the initial pie)

SD_S=3.96

Farmers (N=50)
M_F=11.58 (=38.60% of the initial pie)

SD_F=4.60



Average (M) and standard deviation (SD) of the minimum accepted offer

Students (N=50)

M_S=26.26 (=32.82% of the initial pie)

SD_S=13.70

Farmers (N=50)

M_F=30.00 (=37.50% of the initial pie)

SD_F=11.77

Figure 4 Negative reciprocity of students opposed to farmers as assessed in the experiment. [Colour figure can be viewed at wileyonlinelibrary.com]

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amount of integer Euros they deem fit. In contrast to the Ultimatum bargaining game, player 2 has no veto power.

3.5.2 Monetary incentivisation

To determine the real pay-offs, we matched 10% of the subjects with each other and selected them as payees. No information on the identity of the subjects was communicated. Their roles, either as 'dictator' or passive player 2, were randomly assigned to them. The pay-offs for both players were determined by the decision of the 'dictator'. Again, three different incentive levels (treatments) were studied. They differ in the size of the dictator's initial endowment X. In treatment 1, the endowment was X = €5; in treatment 2, it was X = €30; and in treatment 3, it was X = €80.

3.5.3 Experimental findings and discussion

Rational choice (i.e. money maximising) predicts the active player 1 to pass nothing on to the passive player 2. What we find is different. Figure 5 depicts altruism (i.e. unconditional kindness) in the experiment: the more the transferred money exceeds zero Euro, the higher the altruism. The great majority of students and farmers were ready to transfer money to a second player, indicating a high degree of altruism. The findings for altruism are mixed across the subject groups: on average, the transferred money in terms of the initial pie was slightly higher in the student group in the treatments 1 (M_S = 42.80% vs. M_F = 37.20%, P-value = 0.1756) and treatment 3 (M_S = 43.85% vs. M_F = 39.62%, P-value = 0.1907), whereas it was somewhat higher in the farmer group in treatment 2 (M_S = 41.00% vs. M_F = 46.00%, P-value = 0.1414). Although the null of no population differences cannot be rejected, students transfer more than farmers in two of the three treatment conditions. To sum up, our findings are not in line with the studies that argue that students behave as if they are less pro-social than other social groups (e.g. Carpenter *et al.* 2008; Belot *et al.* 2015). Contrary to agricultural students (P-value = 0.7401), we find that farmers' altruism is slightly associated with the incentive level (P-value = 0.0739). However, there does not seem to be a clear pattern. Our findings are partly in line with Larney *et al.* (2019) who find small effects of the stake size.

3.6 Experiment 6: Trust

3.6.1 Experimental design

To elicit the individuals' propensity to trust others, we adapt the trust game from Kosfeld *et al.* (2005).[6] Each of two players receive an initial endowment

[6] We used neutral language (i.e. person 1 and person 2) in this experiment. By contrast, Kosfeld *et al.* (2005) speak of investors and trustees. Moreover, our design deviates from Kosfeld *et al.* (2005) in the initial endowment and the number of options both players have. The latter was done to carry out comparable statistical methods throughout this study.

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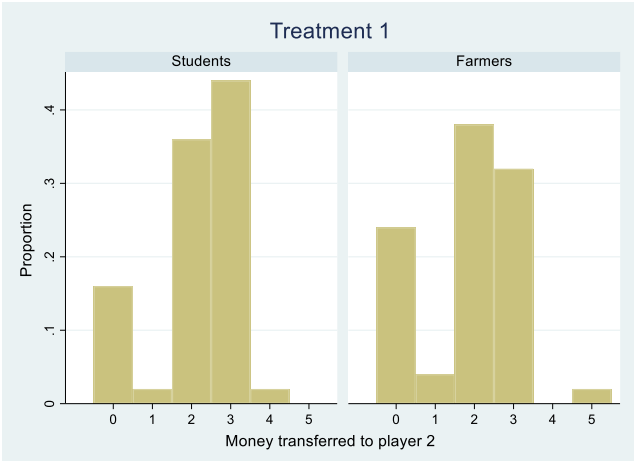


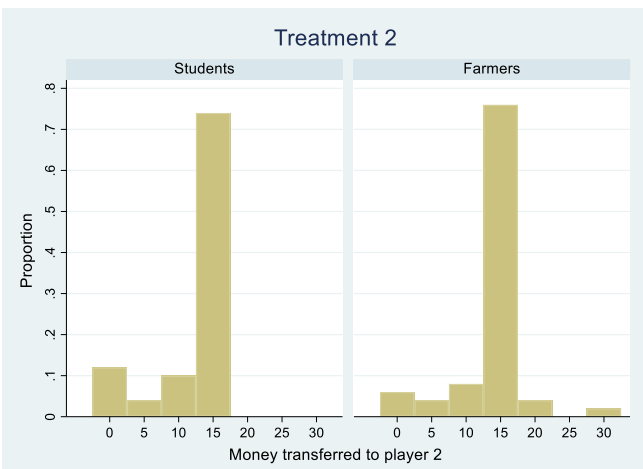
Average (M) and standard deviation (SD) of transferred money

Students (N=50)
M_S=2.14 (=42.80% of the initial pie)

SD_S=1.08

Farmers (N=50)
M_F=1.86 (=37.20% of the initial pie)

SD_F=1.22



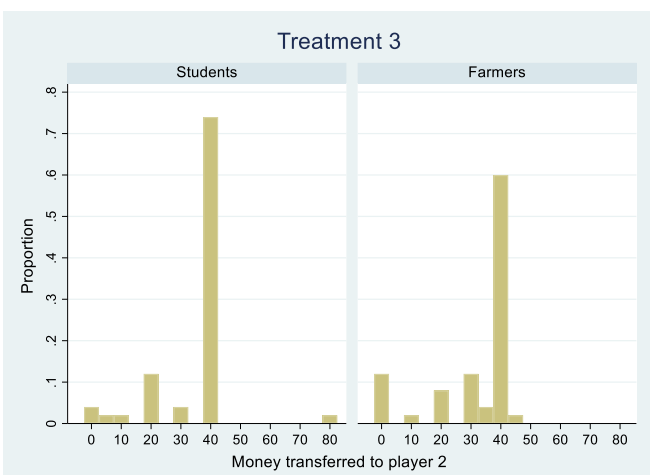
Average (M) and standard deviation (SD) of transferred money

Students (N=50)
M_S=12.30 (=41.00% of the initial pie)

SD_S=5.17

Farmers (N=50)
M_F=13.8 (=46.00% of the initial pie)

SD_F=4.90



Average (M) and standard deviation (SD) of transferred money

Students (N=50)
M_S=35.08 (=43.85% of the initial pie)

SD_S=12.96

Farmers (N=50)
M_F=31.7 (=39.62% of the initial pie)

SD_F=13.79

Figure 5 Altruism of students opposed to farmers as assessed in the experiment. [Colour figure can be viewed at wileyonlinelibrary.com]

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of 28 monetary units (MU). Player 1 has the option to transfer 0, 4, 8, 12, 16, 20, 24 or 28 MU to player 2. The gaming authority triples each MU that player 1 transfers. Player 2 can transfer any amount from zero to the total available amount back to player 1. Therefore, the final pay-offs are determined as follows:

$$\text{Payoff player 1 (MU)} = (\text{initial endowment of 28}) - (\text{transfer to player 2}) + (\text{back transfer from player 2})$$

$$\text{Payoff player 2 (MU)} = (\text{initial endowment of 28}) + (3 \cdot \text{transfer from player 1}) - (\text{back transfer to player 1})$$

Figure 6 visualises the transfers that are feasible for player 1 and the back transfers that are feasible for player 2 contingent on the transfers made by player 1.

For example, player 1 sends 28 MU to player 2. Player 2 then possesses 112 (=28 + 28 · 3) monetary units and can, therefore, send any amount from 0 to 112 back to player 1. To measure trust, we are interested in the amount of money player 1 sends to player 2. If player 1 transfers money, he/she trusts that player 2 rewards such a behaviour.

3.6.2 Monetary incentivisation

To determine the real pay-offs, we matched 10% of the subjects with each other (i.e. by conducting virtual pairs of their decision behaviour in the experiment) and selected them as payees. No information on the identity of the subjects was communicated. Their roles, either as player 1 or player 2, were randomly assigned to them. For reasons of practicability, a multiple price list format was used to capture the choice behaviour of player 2. The

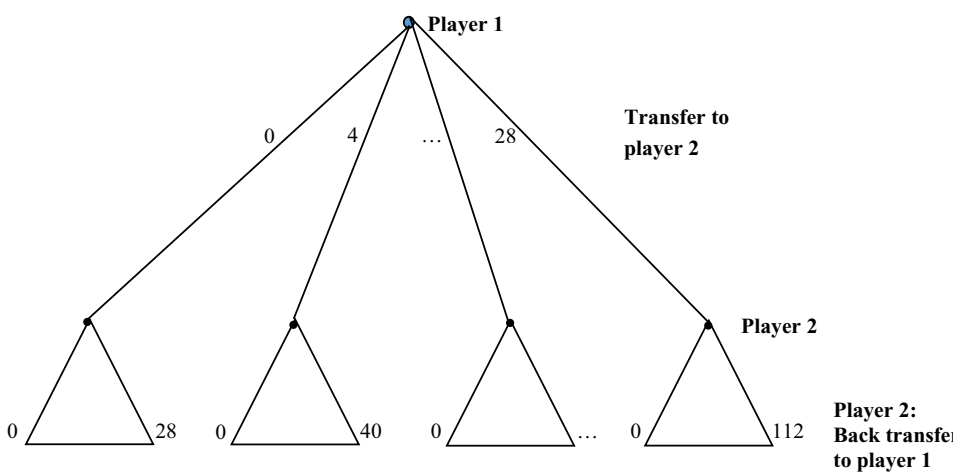

Figure 6 Feasible transfers for player 1 and player 2 in the trust game. [Colour figure can be viewed at wileyonlinelibrary.com]

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pay-offs for both players were determined according to the choices of the matched pair. Depending on the incentive level, the obtained MU were exchanged into real EUR according to the following rates: in treatment 1, subjects received €0.05 for 1 MU. In treatments 2 and 3, the respective amounts were €0.50 and €2.00.

3.6.3 Experimental findings and discussion

Game theory predicts rational agents to mistrust each other in one-shot interactions. The argument runs as follows: player 1 anticipates that player 2 is a self-interested economic agent who abuses any trust. As a consequence, he/she will not transfer any money to player 2. Figure 7 depicts trust in the experiment: the more the subjects send to player 2, the higher their level of trust. Contradicting rational choice prediction, we find a considerable willingness to trust the other player.

On average, students compared to farmers transferred slightly more money to player 2 in treatment 1 ($M_S = 18.56$, $M_F = 18.24$, P-value = 0.7968) but less money in treatment 2 ($M_S = 18.88$, $M_F = 20.32$, P-value = 0.6118) and treatment 3 ($M_S = 19.76$, $M_F = 21.84$, P-value = 0.2470). In other words, although the mean values indicate agricultural students to be a lower boundary of the behaviour of the farmers (with the exception of treatment 1), the null of no population effects cannot be rejected in either of the three treatments. The direction of the findings is roughly in line with Johnson and Mislin (2011) and Belot *et al.* (2015), who find that students are less trusting than non-students. However, the difference is small and our P-values are quite large. Contrary to agricultural students (P-value = 0.7720), we find that farmers' trust is positively associated with the incentive level (P-value = 0.0517). More precisely, trust is similar across the monetary incentive levels for both groups, except for a slight increase with farmers. Johnson and Mislin (2011) argue, however, that stakes do not influence trust in their meta-analysis.

4. Discussion and concluding remarks

The goal of this paper was to shed further light on the generalisability of the experimental behaviour of university students to other social groups. Given the widespread challenges in the agricultural and environmental sector, it is particularly interesting to address farmers' decision-making. We analyse whether agricultural students can be used to approximate the behaviour of farmers in simple economic experiments, which are often used to measure risk aversion, impatience, positive reciprocity, negative reciprocity, altruism and trust. In addition, we consider the role of systematically varied monetary incentives.

We find the magnitude of individual risk aversion to be nearly identical between agricultural students and farmers. Risk aversion is associated with the incentive level. With the exception of high-stake sizes, we find that risk

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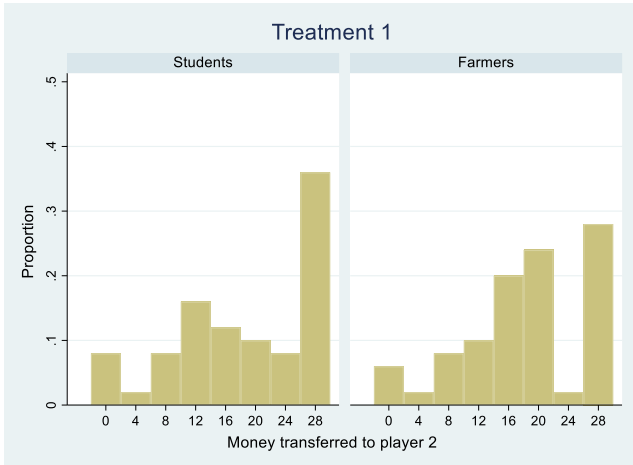


Average amount (M) and standard deviation (SD) of transferred money

Students (N=50)

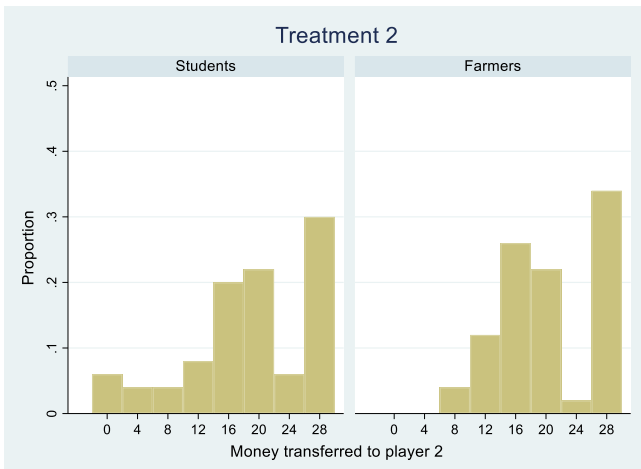
M_S=18.56

SD_S=9.20

Farmers (N=50)

M_F=18.24

SD_F=8.16



Average amount (M) and standard deviation (SD) of transferred money

Students (N=50)

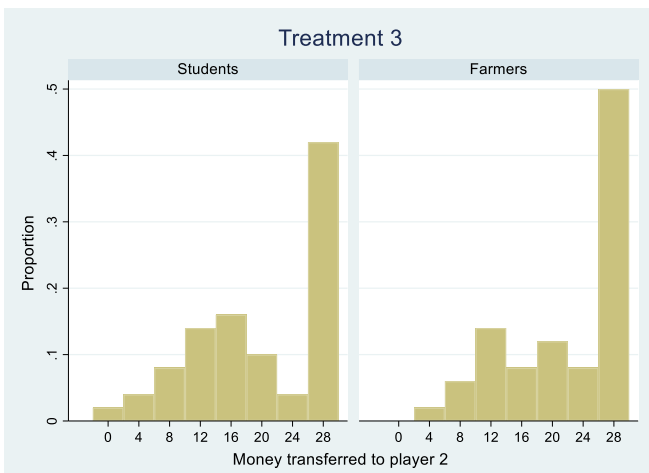
M_S=18.88

SD_S=8.28

Farmers (N=50)

M_F=20.32

SD_F=6.35



Average amount (M) and standard deviation (SD) of transferred money

Students (N=50)

M_S=19.76

SD_S=8.37

Farmers (N=50)

M_F=21.84

SD_F=7.41

Figure 7 Trust of students opposed to farmers as assessed in the experiment. [Colour figure can be viewed at wileyonlinelibrary.com]

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aversion increases with the monetary incentives (stake size effect). Moreover, we find that farmers' positive reciprocity and trust are positively associated with the incentive level. We cannot find such a pattern with agricultural students. To put it differently, agricultural students underestimate the dynamics of the monetary incentives. Knowledge about such boundaries is important. For example, trust in people, policy measures and new technologies help to reduce transaction costs. This might be important when it comes to adaptations of new production technologies against climate change (e.g. stricter fertiliser regulations). Findings regarding altruism in the two populations are mixed and challenge the finding of earlier studies of students being less pro-social. Moreover, we find that agricultural students are a lower boundary of farmers' impatience and negative reciprocity. Such boundaries are helpful to avoid inadequate generalisations. Otherwise, studies with agricultural students could miss that farmers might be more likely to forgo worthwhile investments due to impatience (i.e. lack of long-term orientation). Path dependency seems to be more prevalent with farmers and, as a consequence, the farmers' willingness to adopt could be overestimated if studies with students were taken for granted without considering boundaries. This has policy implications. For example, human-capacity building measures could be used to hint at beneficial investments and more long-term thinking. There was also some evidence in our study that subjects are more patient when it comes to higher financial stakes. The good news is that individuals seem to be more long-term oriented when they deal with more important decisions. However, the bad news is that this effect was more prevalent with agricultural students. Thus, it could be overestimated if we uncritically infer it to farmers. Agricultural students being a lower bound of negative reciprocity means that uncritically (i.e. without thinking in terms of boundaries) inferring findings from agricultural students to farmers does not adequately capture the farmers' willingness to punish 'unfair' actions. For example, farmers could react with reactance to policy measures perceived as unfair. This could, in turn, crowd out voluntary actions to protect the environment beyond required measures by law. These heterogeneous results suggest that scientific inference from agricultural students to farmers should be made cautiously: unquestioned generalisations cause the risk of erroneous conclusions and harmful consequences. This is not restricted to but includes research contexts that deal with policies aimed at influencing the sustainability-related behaviours of economic agents such as farmers.

There are several limitations to our study which might be fruitful for further research. We used standard experiments to measure the behavioural constructs. But it is an empirical question whether other experiments which could also have been used to measure the constructs lead to identical or at least similar findings. For example, there is a huge variety of procedures to measure individual risk attitude (e.g. Harrison and Rutström 2008; Charness *et al.* 2013), which could have been used within this study. Further studies should also elaborate on other incentive mechanisms (e.g. paying more

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subjects with less money or paying fewer subjects with more money; Charness *et al.* 2016). More work is also required to explain the underlying mechanism behind the differences and similarities between agricultural students and farmers. For example, in our study, we found that agricultural students and farmers were similar in their risk aversion. However, it is well known in the literature that women tend to be more risk averse than men (Croson and Gneezy 2009; Charness and Gneezy 2012). Since there are many more women in the student groups, there must be other determinants that override the gender influence and make risk aversion similar in both groups. However, it could also be that there are fewer differences between students and non-students in our study because they came from the same sector. Similarly, Croson and Gneezy (2009) find in their literature review that women tend to trust less than men. If both populations would only differ in gender, we would expect farmers to trust more than students. We find, however, that in the low-incentive treatment that the difference between both populations was quite small—in contrast to the treatments with higher incentives. More research on this and other behavioural traits is required to disentangle possible reasons behind differences and similarities between populations. Moreover, the subjects self-selected themselves into the study. Therefore, we cannot rule out the possibility that there are systematic differences between the experimental subjects and those who did not participate in the study. Replication studies are also required to address the relevance of order effects in different subject pools. In line with several other studies that address population differences, we did not control for them. But future research should systematically analyse its behavioural relevance.

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Data availability statement

The data that support the findings of this study can be found in the online version of this article.

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Supporting Information

Additional Supporting Information may be found in the online version of this article:

Appendix S1 Data and Code.
Appendix S2 Experimental instructions.
Appendix S3 Pre-registration.
Appendix S4 Balance across treatments.
Appendix S5 Subject pool and monetary incentives: behavioural drivers?

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