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DISCUSSION PAPER

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How Do Different Compensation Schemes and Loss Experience Affect Insurance Decisions? Experimental Evidence From Two Independent and Heterogeneous Samples

How do different compensation schemes and loss experience affect insurance decisions?

Experimental evidence from two independent and heterogeneous samples

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Abstract

Although natural hazard insurance is advocated as an important means of risk management, private insurance demand often remains below critical levels. Prior loss experience and the design of governmental relief schemes are two factors potentially influencing insurance decisions. We address these two elements in monetary incentivized experiments which include representations of natural hazard insurance schemes in Europe. We draw on two very different samples: First, we run a laboratory experiment with a student subject pool in Germany. In addition, we replicate the experiment as an online experiment with citizens of flood-prone areas in the city of Dornbirn (Austria). The experiment reflects two possible designs of governmental relief schemes: partial but guaranteed relief and full but non-guaranteed relief. The risk of loss is kept constant over ten consecutive rounds to analyze the effect of loss experience. In both of our samples, the design of the governmental relief scheme has no effect on insurance decisions. Furthermore, prior loss experience adversely affects insurance decisions. Uninsured subjects tend to remain uninsured after experiencing a loss, and previously insured subjects often switch to non-insurance in the rounds after the loss. These results have important policy implications, e.g., for the optimal design of flood risk communication.

JEL Codes

C91, D14, H84, Q54

Keywords

Natural hazard insurance, experiment, governmental relief, charity hazard

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1. Background and introduction

Societies and economies in developing as well as high income countries are increasingly affected by natural disasters, such as floods, hurricanes, winter storms and landslides. In Europe, hydrological and meteorological disasters have caused an average annual economic damage of more than 8 billion USD since 2000, of which almost 60 percent were associated with flooding (EM-DAT, n.d.). Due to climate change, the frequency and severity of extreme weather events are expected to increase (Hirabayashi et al. 2013), which highlights the need for developing strategies and policies to make our societies more resilient to natural disasters. Especially when it comes to flooding, recent academic literature (Merz et al. 2010, Ward et al. 2013) as well as policy approaches (e.g., Bundesregierung 2008) emphasize the need of an integrated risk management approach. Under integrated flood risk management, public protection measures, such as dikes, are complemented by protection measures implemented by private companies, households, and individuals. Integrated flood risk management also includes private insurance of residual financial risks (Surminski et al. 2015). Hence, the design of insurance markets is crucial for increasing society's resilience to natural disasters and climate change (Hudson et al. 2020).

Throughout the world, countries have developed different strategies to strengthen private insurance markets for natural disasters. In general, market penetration rates in privately organized insurance markets are relatively low (Lamond and Penning-Rowsell 2014). In this analysis, we focus on two main reasons for the reluctance of individuals to purchase insurance coverage: the anticipation of financial support by the government and the misperception of risk.

First, private actors may refrain from costly insurance because they expect financial relief payments from the government or charity organizations in case a disaster hits (which is referred to as "charity hazard", see Andor et al. 2020, Raschky et al. 2013, Raschky and Weck-Hannemann 2007). Indeed, this expectation is grounded on experience. In various countries, affected households and businesses have regularly received financial compensation for their damage (Kunreuther 2006, Schwarze and Wagner 2007). Importantly for this analysis, some countries (such as Austria) have clear-cut, *ex-ante* formulated rules in place, which set out whether and how much compensation is to be paid. In other countries (e.g., USA, Germany), policy makers decide on an *ad-hoc* basis whether an extreme weather event qualifies as a disaster and whether and how much compensation is to be paid to affected households and businesses. In the present analysis, we address the question if such fundamental differences in the design of governmental compensation affect insurance decisions of subjects in a laboratory and online experiment.

The second major reason for reluctance to insure against natural hazards is a possible misperception of risk. Individuals may neglect small probabilities (Kunreuther and Pauly 2004) and adjust their risk perception after experiencing an unexpected loss (referred to as availability bias, Gallagher 2014; Tversky and Kahneman 1973). On the other side, a counteracting process may be observed: the gambler's fallacy (Cohen et al. 2008, Tversky and Kahneman 1974). This process refers to the erroneous belief that an event is less probable after it has happened more often than expected in prior periods (and vice versa). Given this background of potential counteracting effects of prior damage experience, we contribute experimental evidence from two very different subject pools to the ongoing research on the effects of damage experience on insurance decisions.

We base our analysis on the existing literature on (1) charity hazard in natural disaster insurance markets and (2) loss experience effects in insurance decisions. Regarding charity hazard, the disincentive to privately insure when expecting relief payments has been postulated theoretically (Buchanan 1975; Coate 1995) and tested empirically (e.g., Davlasheridze et al. 2019; Kousky et al. 2018; for a comprehensive review of the empirical literature, see Andor et al. 2020). While the theoretical literature

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unanimously predicts a negative effect of compensation on insurance and other precautionary measures, the findings in the empirical literature are more ambiguous. The literature on different effects of compensation scheme designs is especially limited. So far, we are aware of only two empirical studies which focus on institutional settings regarding governmental flood relief: Raschky et al. (2013) observe flood insurance decisions of homeowners, and Robinson et al. (2019) employ an incentivized experiment, using a Dutch student sample.

The empirical study of Raschky et al. (2013) compares flood insurance decisions reported in household surveys in Germany and Austria and explains cross-country differences, by pointing to the different compensation schemes in these countries. Indeed, Germany and Austria serve as prime examples for two different archetypes of governmental relief payment schemes after floods. In both countries, private flood insurance is generally available, but the penetration rates are relatively low (Germany: 41 percent in 2018, GDV 2018, Austria: 10-25 percent in 2010, Aakre et al. 2010, or around 30% based on self-reports, Balas et al. 2015). In Austria, the government provides a tax-funded disaster fund, and affected households can safely expect a partial compensation (between 20 and 30 percent of their damage). On the contrary, in Germany there is no *ex-ante* formal regulation on governmental relief. However, in the past, the federal and some state governments have repeatedly decided to grant *ad-hoc* relief to affected households (Bundesregierung 2016). Policy makers were particularly generous if the respective weather event gained interest in the national mass media and/or happened shortly before important election dates (Bechtel and Hainmueller 2011, Garret and Sobel 2003). If granted, the magnitude of the financial relief was often almost as high as the total damage (Raschky et al. 2013). Relief payments are normally reported extensively in the media triggering expectations regarding further relief payments in the future. Indeed, survey data of 2014 show that although there is no legal obligation for the government to provide relief altogether, a considerable share of 25 percent of household heads in Germany expect to receive compensation by the government in case their premises are flooded (Osberghaus and Philippi 2015).

Hence, when it comes to the design of governmental disaster relief, countries in Europe and elsewhere pursue very different policies and practices. In this analysis, we focus on the guarantee and completeness of the compensation and design two extreme archetypes of relief schemes. The first type entails guaranteed but partial damage compensation by the government (as the disaster fund in Austria). The second kind of scheme, on the contrary, refers to a government discretely deciding on whether to fully compensate for damage or not at all, similar to the current practice in Germany. Given the necessity to motivate private actors to take precautionary measures, including insurance, it is relevant which of these two different compensation schemes affects private insurance decisions to a greater degree. Raschky et al. (2013) analyze this question based on household surveys in Austria and Germany and conclude that the guaranteed but partial compensation scheme in Austria has higher disincentives for private insurance. Note that a causal interpretation of their finding is complicated as the assignment of households to different compensation schemes was not random but dependent on their country of residence. To overcome these methodological challenges, we present an experiment which allows subjects to insure against the risk of loss, after they have been randomly assigned to one of two different compensation schemes. The experimental literature on insurance decisions is extensive (for a review, see Jaspersen 2016), but the effect of compensation schemes has rarely been analyzed. Closest to our study is the analysis of Robinson et al. (2019), basing on an incentivized experiment with 200 Dutch students. In their experiment, participants are confronted with either "certain half" or "risky full" compensation, or no compensation at all. While the insurance decision negatively depends on the general availability of compensation (hence, there is a charity hazard effect), the type of compensation does not play a major role. We expand the work of Robinson et al. (2019) in two dimensions: First, we test the robustness of the results using two very different subject samples – besides data from students in Mannheim, Germany, we collect experimental data of household heads in Dornbirn, Austria. Second, we add the time dimension. By keeping risk and compensation parameters constant over the run of the experiment, we can analyze how damage and compensation experiences affect subsequent insurance decisions.

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Regarding the loss experience effects on insurance decisions, there are empirical and experimental studies supporting both potential effects: the availability bias, implying a positive effect of prior experience on the willingness to insure, and the gambler's fallacy, postulating the opposite effect. The experimental literature on repeated insurance decisions with constant damage probabilities (as in our setting) reports a slight tendency towards behavior consistent with the gambler's fallacy. For example, Jaspersen and Aseervatham (2017) show that (a) insurance demand decreases following rounds with a loss, and (b) this effect is dependent on the emotional activation of subjects. The gambler's fallacy is also found by Ganderton et al. (2000), in the experimental analysis of Turner et al. (2014), and to a less significant degree by Papon (2008). In contrast, Kunreuther & Michael-Kerjan (2015) report higher insurance demand if the subjects experienced a loss event at an early stage in the experiment, and Kunreuther & Pauly (2018) find a positive effect in a hypothetical insurance experiment. Camerer and Kunreuther (1989) find no effects of prior losses on insurance demand, concluding that subjects understood that losses were statistically independent. In light of these partly contrasting findings, we contribute further experimental evidence from students in the lab (as many of the mentioned studies do) but add a replication of the study in a very different context (household heads in their homes, in a flood-prone area, via an online-experiment). While in the non-experimental literature, most empirical studies report an increasing insurance demand after experiencing major damaging events (hence consistent with availability bias, e.g. Browne & Hoyt 2000, Hudson et al. 2017, Petrolia et al. 2013), there is also evidence which may be interpreted as consistent with gambler's fallacy. Kamiya & Yanase (2019) find both kinds of reaction after two major earthquakes in Japan. In directly affected districts, insurance demand increased, but it decreased in neighboring districts. In the context of flooding, some studies for Germany find that insurance penetration amongst flood-experienced households decreased after the major flood in 2002 (Bubeck et al. 2013; Kreibich et al. 2011). While the authors speculate that contracts might have been cancelled by the insurance companies, the data are generally also compatible with the gambler's fallacy interpretation.

With respect to the effect of specific compensation schemes, we confirm the finding of Robinson et al. (2019) and find that experiment participants in both treatment groups do not differ in their insurance decisions. Hence, the type of relief payment does not affect insurance behavior in the experiment. Regarding the effect of previous loss experience, we find a highly significant and robust negative effect of previous loss experience on insurance purchase, hence an indication of the gambler's fallacy. Participants tend to opt for insurance after experiencing several rounds without loss and show a high tendency not to insure directly after suffering a loss. Both of these main findings were obtained independently from each other in both samples, which indicates that the socio-demographic characteristics of participants do not drive the results.

In the remainder of the paper, we present the design and procedure of the experiment, and summarize and interpret the main results. The final section concludes and presents some preliminary policy implications.

2. The Game

2.1 Underlying experiment

In the experiment, subjects decide whether to purchase insurance or not against a probably occurring damage event. If purchased, the insurance will cover the whole damage. Only in case of non-insured damage, the government can step in. In a between-subjects design, the treatments reflect two different governmental relief mechanisms, which differ in two ways: (i) the payment of compensation is either guaranteed or non-guaranteed, and (ii) the amount of compensation by the government is either full or

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partial.[1] Nevertheless, the expected payoff is the same in both treatments. Subjects form their decisions based on the same treatment conditions in ten independent consecutive rounds. Note that we deliberately do not test for charity hazard in general, hence we do not include a "control" treatment without compensation. First, the analysis of a general charity hazard is feasible in empirical settings (e.g., Davlasheridze et al. 2019; Kousky et al. 2018) which questions the need for an experiment; second, based on recent empirical and experimental studies (e.g., Robinson et al. 2019), we are quite confident of the existence of charity hazard, and opt to focus on research questions which are less settled, such as the stability of charity hazard over time, across different compensation schemes and across different samples.

In the following, we describe the payoff functions for the different conditions applicable for an individual in one round.

Apart from the governmental relief mechanism, the setting is the same in both treatments. Subjects get an initial endowment of EUR 20 (E) in each independent round. From this endowment EUR 0.50 (F) are automatically deducted – this is to roughly mirror the individual contribution needed to finance the governmental relief fund. Thus, the remaining amount at stake is EUR 19.50. In each of the rounds, a damage event (D) occurs with a 20% chance resulting in a loss of EUR 15.[2] Subjects can protect themselves against this damage by purchasing insurance coverage for a premium of EUR 3.50 (P).

For insured subjects, the guaranteed payoff in both treatments is the following:

$$\pi = E - F - P$$

The insurance is designed as an actuarially unfair insurance with a premium (P) comprising the probability of the damage (p), the insured damage and a moderate premium loading factor of about 1.17 (L).[3]

$$P = pDL$$

In the following, we explain the two treatments that differ in their governmental relief mechanism in more detail.

2.2 Non-guaranteed full compensation (NFC)

In the first treatment, the governmental compensation in case of non-insured damage is not guaranteed. However, the government would compensate the damage fully (non-guaranteed full compensation, *NFC)*. In case of damage, the probability of a relief is $r = 1/3$. The expected earning for a subject deciding against an insurance in treatment NFC is:

$$\pi^{NFC} = E - F - p(D - rD)$$

The expected damage $p(D - rD)$ consists of the probability of the occurrence of damage times the loss and the compensation of the total damage.

[1] We use the term 'non-guaranteed' for those cases where governmental relief cannot be taken as given. We design this as a probability that governmental relief takes place.

[2] The 20% probability is meant to ensure that losses take place in the experiment to observe loss experience behavior.

[3] Although the loading factor is higher when governmental relief is taken into account, the insurance itself is non-prohibitive.

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2.3 Guaranteed partial compensation (GPC)

In the alternative treatment, the governmental compensation is guaranteed but covers the damage only partially (guaranteed partial compensation, *GPC*). The share of the damage which is covered by the relief is $c = 1/3$. The expected earning for an uninsured subject in treatment GPC is:

$$\pi^{GPC} = E - F - p(D - cD)$$

The expected residual damage $p(D - cD)$ consists of the probability of the occurrence of damage times the loss and the compensation of a third of the total damage.

2.4 Hypotheses

The NFC and the GPC treatment only differ in the design of the governmental compensation. The expected payoff is the same in both cases. Therefore, we can test whether the different relief mechanisms affect the insurance decision by comparing insurance decisions between subjects in the two treatments. Compared to the guaranteed compensation in the GPC treatment, the NFC treatment appears to the subject as a compound lottery, with uncertainty on the damage event as well as on the availability of compensation. As shown i.a. by Elabed and Carter (2015) and Dean and Ortoleva (2019), compound lotteries are often dismissed relative to simple lotteries with equivalent outcomes. In case of compound aversion, the compensation offer in the NFC treatment has therefore a lower value than in the GPC treatment, and incentives to privately insure will be higher in the NFC treatment. This is compatible with findings by Raschky et al. (2013) suggesting that under relief schemes with guaranteed compensation, the tendency to insure is lower than in schemes with non-guaranteed compensation. In our setting, this would imply the following:

Hypothesis 1: The tendency to insure is lower in case of a guaranteed partial compensation scheme (treatment GPC) than with a non-guaranteed full compensation scheme (treatment NFC).

Besides testing the effect of the design of governmental relief, the setting allows us to observe whether subjects base their insurance decision on the experience in previous rounds. The subjects make their decisions, remaining in the same treatment condition, in ten consecutive rounds. The risk of damage is the same in each round and therefore independent from the occurrence of previous damage. Thus, purely rational subjects should not change their preferences for or against an insurance when experiencing damage. However, previous experimental and empirical findings with similar settings of constant damage properties show that experience of damage might alter subjects' perception of risk of damage and subjects seem to make inconsistent decisions. On one side, they could perceive the risk of damage as higher, which might be explained by availability bias. On the other side, the gambler's fallacy may induce that the risk of damage is considered to be lower than before the damage event. In both cases, damage experience influences individual risk perceptions in one way or the other. This leads us to the following hypothesis:

Hypothesis 2: The tendency to insure is influenced by previous loss experience.

3. Experimental Procedure, Subject Samples and Statistical Methods

We conducted the laboratory experiment at the mLab of the University of Mannheim in October and November 2018. Overall 128 students participated, of which 63 were assigned to the NFC and 65 to the GPC treatment. We paid a show-up fee of EUR 5.00.[4] Subjects were informed that they would be paid for a single round randomly selected from the 10 rounds in the session. The average payoff was EUR 16.58 plus the show-up fee of EUR 5.00. We randomly assigned the subjects to treatments in which the

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[4] We offered a relatively generous participation fee of EUR 5.00 to encourage participation of household heads in Dornbirn. The show-up fee was also paid in the laboratory experiment to keep all parameters equal between the two samples.

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subjects remained during the ten rounds. The subjects were seated in separate cubicles. The instructions, the experiment as well as a post-questionnaire were programmed with LimeSurvey and provided to each participant at a personal computer. The instructions were written in neutral language, i.e., they referred to an 'aid fund' instead of governmental relief (see appendix A.1). Subjects made an insurance decision in each of the 10 independent rounds where a reminder explaining the decision situation ensured a sufficient understanding of the design (see appendix A.1). After each round they were informed whether damage had occurred and were also shown the outcome of the respective round. As the ten rounds were finished, subjects got an overview on the outcomes of all ten rounds. In a post-questionnaire, relevant socio-demographics were collected. Risk preferences were elicited by a widely used single-item risk measure tested in Dohmen et al. (2011). The payment-relevant round was randomly chosen for each subject after finishing the questionnaire.

Note that students might not have made an actual insurance decision before and therefore may differ in their insurance decision in the lab from real household heads. Therefore, in November and December 2018, we ran an online experiment with household heads residing in flood-prone areas in the city of Dornbirn, Austria, who likely had made actual insurance decisions before and who presumably face more real risks to their homes and personal belongings than students. The experiment was only differing in the recruiting and payment procedure. While the students from the laboratory experiment were recruited via the standard software Orsee (Greiner 2004), the households in Dornbirn had been participants of a former survey. We contacted these households via email, including a link to the LimeSurvey-based experiment and an individual access code (the invitation email is included in appendix A.2). Overall 47 households took part in this online experiment: 18 were randomly assigned to the NFC and 29 to the CPC treatment. A schematic diagram for the laboratory and online experiment is provided in appendix A.3.

In addition, the payment procedure differed between the samples. The students received their payment in cash directly after the experiment. Household heads in Dornbirn were paid via shopping cards charged with the individual payoff including the participation fee of EUR 5.00. This procedure was chosen to have an incentive compatible payment without sharing sensible data, such as bank account details. The shopping cards are usable in many local shops and large supermarkets in Dornbirn. They were sent out by mail after the conclusion of the experiment in January 2019. The participants were aware of the specifics of the payment procedure. Table 1 presents a summary of the two samples' characteristics and how they differ. The descriptive data show significant differences between the two samples in terms of education, age, gender, having German as a mother tongue, and duration of the experiment. While we did not measure the monthly income of the student subjects, we can safely assume that the average income of the household heads is substantially higher than the income of the students. Average insurance decisions, pay-out, and stated risk aversion, however, do not differ significantly.

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Table 1: Descriptive statistics of subject pools and key variables.

Variable group	Variable name	Variable description	Mean (std. dev.)	Mean (std. dev.)	Difference of means [a]
			Student sample	**Household sample**	
Information on sample	---	Number of participants	128	47	---
	---	- thereof in treatment NFC	63 (49%)	18 (38%)	---
	---	- thereof in treatment GPC	65 (51%)	29 (62%)	---
Experimental outcomes	---	Duration in minutes	9.37 (3.49)	14.71 (8.47)	-5.34***
	---	Payoff in EUR (exclusive show-up fee)	16.68 (1.07)	16.58 (0.89)	0.10
Insurance variables	*insall*	Average insurance decision in all rounds (1=insured)	0.61 (0.38)	0.56 (0.35)	0.05
	insfirst	Insurance decision in the first round (1=insured)	0.60 (0.49)	0.47 (0.50)	0.13
	inscat	Categorical measure of insurance behavior (1=never insured, 2=sometimes insured, 3=always insured)	2.15 (0.69)	2.02 (0.61)	0.13
	ins	Insurance decision per round (1=insured)	0.61 (0.49)	0.56 (0.50)	0.05**
	insnew	Switching to insurance (1=insured in current round and not insured in previous round)	0.26 (0.44)	0.22 (0.41)	0.04
	insleave	Switching to non-insurance (1=not insured in current round and insured in previous round)	0.16 (0.36)	0.15 (0.36)	0.01
Other variables	*fem*	Gender (1=female)	0.52 (0.50)	0.15 (0.36)	0.37***
	age	Age in years	22.54 (3.34)	47.47 (12.75)	24.93***
	educ [b]	Education level (1=college or higher)	1.00 (0.00)	0.40 (0.50)	0.60***
	german	Mother tongue German	0.80 (0.40)	0.98 (0.15)	-0.18***
	risk	Stated level of risk seeking on an ordinal scale from 0 to 10	4.59 (2.13)	4.21 (2.12)	0.38
	switcher	1=Subject changes insurance behavior in the course of the experiment	0.51 (0.50)	0.64 (0.49)	-0.13

a) Mean of student sample minus mean of household sample. Significance levels are based on the Wilcoxon rank-sum test. *, **, *** indicate significance levels of 10, 5, and 1 percent, respectively.

b) The variable *educ* has no variance in the student sample, hence we do not use it as a control variable in the main analysis. It is reported to illustrate the differences between the two samples.

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We analyze the experimental data, using the following statistical methods: First, we define a series of variables, capturing different aspects of the insurance decisions. Table 1 describes these and other variables used in the empirical analysis, along with their descriptive statistics.

Hypothesis 1, regarding the potential effect of compensation scheme designs, is tested with Wilcoxon rank-sum tests, comparing the means of insurance variables between subjects in the NFC treatment and subjects in the GPC treatment. If the compensation scheme design significantly affects insurance decisions, the test will yield significant differences. We expand this analysis by regression analyses, using data of all single insurance decisions in the ten rounds of the experiment. For the analyses, we use a random effects probit model to make use of the longitudinal structure of the data. In this step, we also add control variables such as gender and risk seeking of participants, and round-fixed effects to increase the overall fit of the model.

When analyzing potential effects of prior losses on insurance, i.e. hypothesis 2, we again start with simple mean comparisons, using the Wilcoxon rank-sum test, comparing insurance decisions with and without previous loss experience. We thereby test several definitions of the terms “loss” and “previous experience”, as in our setting damage does not necessarily lead to loss if either the insurance or the governmental relief scheme comes into operation. We also define a variable capturing each participant’s number of previous rounds without loss experience, and analyze how insurance decisions evolve with this variable. Finally, we use a fixed effects logit model of insurance behavior, including the time lagged loss experience as explaining variable(s). This model includes participant- and round-fixed effects to control for any participant- and round-specific effects (observable and unobservable).

In the remainder of the paper we focus on the results from the laboratory (student) sample due to the larger sample size. If any of the main results presented in the following are not confirmed by the online experiment conducted with household heads, we explicitly mention that discrepancy.

4. Results

Effect of compensation scheme designs

The results of the mean comparisons of insurance variables between participants from the two treatment groups are depicted in Table 2.

Table 2: Wilcoxon rank-sum tests of insurance variables between the two treatment groups, for both samples

Insurance variable	**Treatment NFC**	**Treatment GPC**	**Difference** [a]
Student sample (laboratory)			
insall	0.63	0.60	0.03 (p=0.56)
insfirst	0.67	0.54	0.13 (p=0.14)
inscat	2.16	2.14	0.02 (p=0.81)
Household head sample (online)			
insall	0.53	0.58	-0.05 (p=0.49)
insfirst	0.44	0.48	-0.04 (p=0.80)
inscat	2.00	2.03	-0.03 (p=0.85)

a) Mean of insurance variables in NFC treatment minus mean in GPC treatment. P-values are based on the Wilcoxon rank-sum test.

In the student sample, all the insurance variables exhibit slightly higher values for the NFC treatment than for the GPC treatment, which supports hypothesis 1. However, the differences between the two treatment groups are so small that they are not statistically discernable from zero. This serves as a first hint that the design of the relief scheme did not influence the insurance decisions of participants in a significant way. In the following, as far as the main results are very similar across both samples, we present detailed results only for the student sample.

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The variables *insall* and *inscat* are based on the aggregate of all periods in the experiment. One may speculate that the treatment effect evolves or diminishes over the course of the ten rounds in the experiment. This may be tested with a mean comparison for each period, as illustrated in Figure 1. The graph shows that the mean values of the insurance decision slightly vary over time, but there is no significant treatment effect in any of the ten rounds and no obvious time trend in neither the insurance decisions nor the difference between the treatment groups.

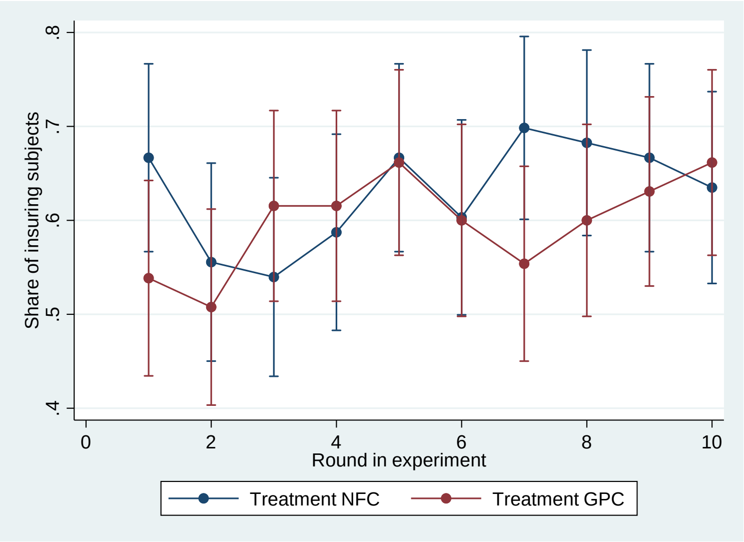

Figure 1: Share of insuring subjects per round and treatment, with 90% confidence intervals. Only data of student sample included.

The panel structure of the data enables us to estimate the single insurance decisions with a random effects probit model. Table 3 summarizes the results of two different regression specifications. The estimated coefficients for the treatment effect confirm that the treatment has no significant effect on insurance decisions. In the student sample, participants are significantly more willing to purchase insurance if they are younger, more risk-averse, and if they have a mother tongue other than German. The insurance willingness is also generally higher amongst participants who never changed their insurance decisions in the course of the ten rounds. None of these correlations, however, are confirmed by the results from the household head sample.

Table 3: Regression results (coefficients of the random effects probit model) of insurance decisions.

Dependent variable	**Insurance purchased (*ins*)**	**Insurance purchased (*ins*)**
Treatment effect	-0.179	0.069
fem	---	0.280
age	---	-0.141***
german	---	-0.796**
risk	---	-0.570***
switcher	---	-0.875***
Number of observations	1,280	1,280
Number of subjects	128	128

Round-fixed effects are always included. *, **, *** indicate significance levels of 10, 5, and 1 percent, respectively. Only data of student sample included.

Possibly, the treatment showed a more pronounced effect after the subjects experienced a loss or a relief payment, hence after they actually got confronted with the respective compensation rules. Therefore, we assess the treatment effect by mean comparisons after the respective subjects experienced a loss or were compensated. We also interact the treatment effect with loss experience in the probit regressions.

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We use different definitions of "loss experience", including (a) the experience of a damage event with or without monetary consequences in any round before, (b) the same event in the immediately previous round, (c) a damage event with monetary consequences (hence no insurance coverage and no full compensation) in any round before, and (d) a damage event with monetary consequences in the immediately previous round. Regardless of the definition of loss experience and the statistical procedure, the treatment effect remains insignificant in sub-samples of subjects who experienced loss or compensation events, and the interactions of treatment with experience variables are insignificant in the regressions.

We also test whether there is a treatment effect amongst subjects who never change their insurance behavior or amongst those who sometimes change their decisions. We do not find a robust result which would suggest that the treatment has an effect in any of these sub-samples.[5]

Taken together, we do not find evidence for an effect of the relief scheme design, hence for hypothesis 1. Whether full but non-guaranteed compensation (treatment NFC) is provided or whether partial but guaranteed compensation can be expected (treatment GPC) seems to be of no relevance for the participants' decisions to insure against an uncertain monetary loss.

Effect of damage experience

As in the preceding subsection, we first compare the means of insurance variables of subjects with differing damage experience to assess any effect of prior damage. To assess different aspects of damage experience, we define prior damage experience in different ways as detailed out in Table 4.

Table 4: Wilcoxon rank-sum tests of insurance decisions between samples with differing damage experience

Damage experience definition	**Sample**	**Mean of *ins* with damage experience**	**Mean of *ins* without damage experience**	**Difference [a]**
Round after a loss versus all other rounds	Students	0.53 (N=202)	0.63 (N=950)	-0.10 (p=0.01)
	Household heads	0.36 (N=88)	0.63 (N=335)	-0.27 (p<0.01)
Round after a real monetary loss[b] versus all other rounds	Students	0.19 (N=68)	0.64 (N=1,084)	-0.45 (p<0.01)
	Household heads	0.11 (N=36)	0.61 (N=387)	-0.50 (p<0.01)
Round after the first loss versus the rounds before	Students	0.52 (N=107)	0.59 (N=641)	-0.07 (p=0.16)
	Household heads	0.28 (N=39)	0.63 (N=207)	-0.35 (p<0.01)
Round after the first real monetary loss[b] versus the previous rounds	Students	0.15 (N=47)	0.68 (N=1,020)	-0.53 (p<0.01)
	Household heads	0.04 (N=24)	0.63 (N=319)	-0.59 (p<0.01)

a) Mean of *ins* with damage experience minus mean of *ins* without damage experience. P-values are based on the Pearson test of independence corrected for complex survey data, to account for possible correlations of multiple observations per subject.

b) Real monetary losses are defined as damage events with monetary consequences, hence no insurance coverage and no full compensation.

The mean comparisons presented in Table 4 serve as a first hint that prior damage experience influences insurance behavior negatively. In the household head sample, all four differences are even more negative

[5] While there is a significant negative interaction effect of treatment interacted by *switcher* (p<0.05) in one specification of the probit regressions, we do not deem this result as robust. It does not hold if control variables are omitted, if the household head sample is used, or if we compare the means of insurance decisions by the Wilcoxon rank-sum test.

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and more significant than in the student sample. A similar conclusion can be drawn from Figure 2 and Figure 3. These figures depict the average insurance decisions and their confidence intervals per number of previous rounds without damage experience (“safe rounds”, Figure 2), and number of previous rounds without real monetary damage experience (“real safe rounds”, Figure 3), respectively. The patterns show a relatively low insurance willingness directly after the damage experience (“safe rounds” = 0), which increases considerably after a few rounds without damage experience.

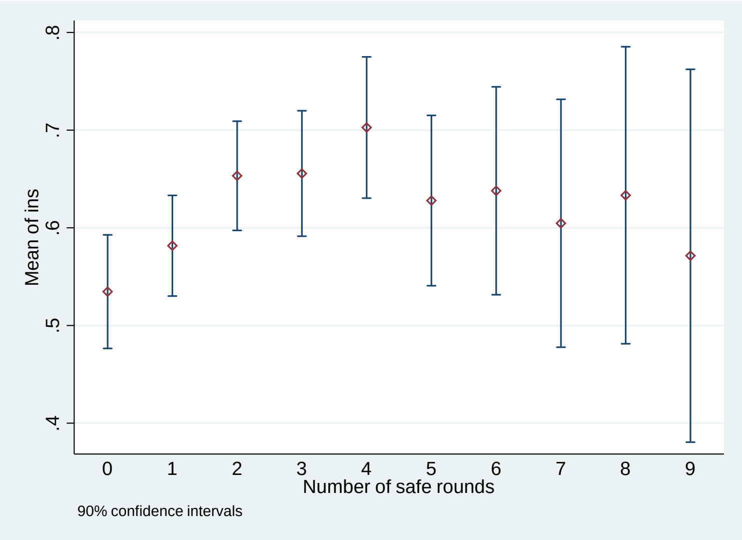

Figure 2: Means and 90% confidence intervals of ins per number of rounds without previous damage experience. Only data of student sample included.

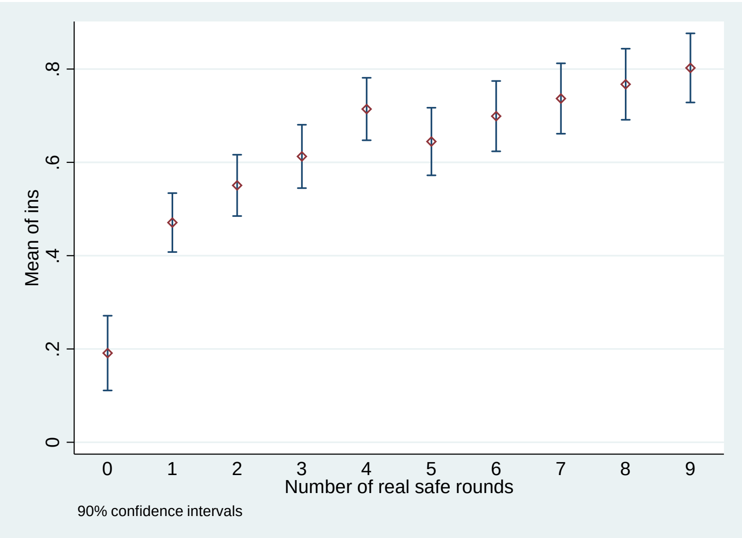

Figure 3: Means and 90% confidence intervals of ins per number of rounds without previous real monetary damage experience. Only data of student sample included.

Next, we evaluate whether the negative damage experience effect persists in a multi-variate regression setting. Based on a test of over-identifying restrictions, the fixed effects model should be preferred over the random effects model, hence we estimate a fixed effects logit regression. Beside damage experience in the previous rounds, we include round-fixed effects. Any observable and unobservable factors at the subject-level which remain constant over the rounds of the experiment, such as personal characteristics as well as the treatment assignment, are absorbed by the subject-fixed effects. Again, we differentiate between general damage experience and damage experience with real monetary consequences. In Table

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5, we present the regression results in case of damage experience with real monetary consequences. Both damage definitions as well as the data from both samples basically yield the same results.

Table 5: Regression results (coefficients of fixed effects logit model) of insurance decisions.

Dependent variable	**Insurance purchased (*ins*)**	**Insurance purchased (*ins*)**	**Insurance purchased (*ins*)**
Loss in round t-1	-1.146***	-1.032**	-0.817*
Loss in round t-2	---	-0.138	0.037
Loss in round t-3	---	---	0.400
Number of observations	558	480	406
Number of subjects	62	60	58

Round-fixed effects and fixed effects at the subject level are always included. *, **, *** indicate significance levels of 10, 5, and 1 percent, respectively. Loss is defined as a damage with real monetary consequences (hence without insurance coverage or full compensation). The number of subjects is lower than in previous analyses because subjects without variation of *ins* are omitted in the fixed effects model. Only data of student sample included.

The results of the fixed effects regressions confirm the impression of the graphical analysis (Figure 2 and Figure 3) that damage experience, particularly in the immediate previous round, affects insurance demand negatively. In subsequent rounds, the willingness to insure quickly recovers to pre-damage levels.

One may pose the question whether damage experience decreases the willingness of previously uninsured subjects to purchase insurance, whether it decreases the willingness of previously insured subjects to remain insured, or whether both processes contribute to the effect. We may assess this by estimating the insurance decision in a more disaggregated way. As dependent variables, we are now using both possible switching behaviors – the decision to newly insure compared to the previous round (*insnew*, there are 703 missing observations with insurance in the previous round), and the decision to leave the insurance (*insleave*, there are 449 missing observations without insurance in the previous round). Table 6 presents the results of the respective logit estimations.

Table 6: Regression results (coefficients of fixed effects logit model) of insurance switching decisions.

Dependent variable	**Insurance newly purchased (*insnew*)**			**Insurance not maintained (*insleave*)**		
Loss in round t-1	-1.016**	-1.154**	-1.324**	1.171***	1.073***	1.306***
Loss in round t-2	---	-1.454**	-1.641**	---	0.430	0.210
Loss in round t-3	---	---	-0.504	---	---	-1.191*
Number of observations	207	177	135	282	248	214
Number of subjects	43	41	36	50	50	48

Round-fixed effects and fixed effects at the subject level are always included. *, **, *** indicate significance levels of 10, 5, and 1 percent, respectively. Loss is defined as damage with or without real monetary consequences. Only data of student sample included.

The main results presented in Table 6, hence a negative effect of damage experience on *insnew*, and a positive effect on *insleave*, are also obtained when using the household head sample. This reconfirms that the effect of damage experience on the insurance decision is driven by both processes – uninsured subjects are hesitant to purchase insurance directly after suffering a loss, and insured subjects are prone to dismiss their insurance coverage after experiencing damage.

These results show that many subjects in both samples – students at the University of Mannheim, Germany, and household heads in flood-prone areas in the city of Dornbirn, Austria – change their behavior after experiencing a damage event, although they were explicitly instructed that the ten rounds

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are independent from each other, and all parameters and probabilities do not change over the course of the experiment. Hence, the data support hypothesis 2. Previous research provides two main explanations for inconsistent behavior in such settings, which, however, are opposed to each other. While the availability bias would lead to an increase of insurance demand, our results rather confirm the gambler's fallacy. Apparently, after a damage event has occurred, subjects misperceive the probability of a damage event as lower relative to the rounds before. This change in risk perception has been documented in other experiments (Jaspersen and Aseervatham 2017, Ganderton et al. 2000, Turner et al. 2014) but we show that the characteristics of the subject sample are not decisive for the presence of the gambler's fallacy. Remember that the two subject samples differ heavily in terms of education level, age, nationality, income, and gender. Moreover, the students are mainly enrolled in subjects prone to economics, such as business administration. Still, in terms of the gambler's fallacy, their behavior is very similar to those of the household head sample without such a high level of prior economic education.

5. Summary and conclusions

We contribute insights from a monetary incentivized experiment on individual insurance decisions in the context of different compensation scheme designs and recurrent damage events. In two independent experiments with two very different subject pools (students in Germany and household heads in Austria), we obtain the following two main findings: First, the decision to insure an uncertain monetary loss does not depend on whether the government (or another charitable donor) provides non-guaranteed but full compensation or whether the expected compensation is guaranteed but partial. This result is unexpected as compound aversion suggests that the latter compensation scheme design would affect private insurance more negatively than the former design. However, our results confirm the respective finding of Robinson et al. (2019) who only use a student sample and do not regard the time dimension in their experiment. Second, we find robust and significant evidence for the gambler's fallacy in the experimental setting, as subjects tend to forego insurance after they have experienced a loss. Although we cannot safely conclude that household heads in the field are prone to gambler's fallacy when it comes to actual flood risk management decisions, we make an important and innovative contribution to the literature by finding this result in our two independent subject samples, which differ drastically in terms of practical insurance experience, education, age, gender, income and nationality.

This research provides important implications for flood risk management and communication strategies. Flood risk at the property level is usually communicated in the form of expected recurrence intervals or annual occurrence probabilities. Both formats highlight the probabilistic component of flood risk and may have unintended consequences after a flood has occurred. Flood experienced households may misperceive the risk of flooding in the near future as relatively low because the recent flood event is expected to occur only once in a relatively long time span (Pidot 2013, Pielke Jr. 1999). Hence, flood awareness campaigns could consider alternative ways of communicating risk levels. For example, past events could serve as illustrative examples of the concrete impacts of floods, or risk levels could be communicated as non-numerical categories, such as "low", "medium", and "high". However, developing alternative communication strategies clearly exceeds the scope of this study. We show that the gambler's fallacy is an issue when risks are communicated in a probabilistic way, and that this is relevant for different subject samples, ranging from well educated, younger students in Germany to higher income household heads in flood-prone areas in Austria.

We also show that the concrete design of relief schemes, in particular, the certainty and completeness of the compensation payment, does not decisively affect the impact of the scheme on private insurance demand. Importantly, we do not assess the direct effect of the existence of a relief scheme *per se* on insurance demand (which is referred to as charity hazard). The existing literature on charity hazard shows that people indeed reduce their private insurance and precautionary behavior. In practice, relief schemes are especially relevant in the context of flooding. As prime examples of two different

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archetypes of relief schemes, we refer to the systems in Germany and Austria. While the German government sometimes has provided full but non-guaranteed damage compensation to uninsured flood victims, Austrian households can count on partial flood damage compensation from a public disaster fund. Switching from one system to the other may be perceived as a more or less cost-neutral way of redesigning a flood relief scheme, with the intention to eventually increase private insurance demand. However, our results suggest that switching from one system to the other will probably not effectively and sustainably trigger private precaution. When fostering private flood insurance and protective measures, policy makers should consider alternative strategies, such as credibly announcing limits and restrictions of public relief, making the relief conditional on private measures, or even introducing mandatory insurance schemes.

Our study opens some avenues for further research. Experimental analysis of the effects of relief schemes on private precaution is still in its infancy. If the experimental design includes subject groups, one may model the cost of public relief schemes more endogenously, or let people vote for different compensation rules. Regarding the effects of risk communication on risk perception in a multi-period setting with constant damage probabilities, future research could more rigorously test different risk communication formats, such as recurrence intervals versus less numerical formats. This would also provide the opportunity to separate pure information deficits from cognitive biases. As such, future research could inform policy makers about how risk as well as relief schemes should adequately be communicated to different subgroups of interest.

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Appendix

A.1: Instructions and Questionnaire

The following instructions and the questionnaire were identical in the laboratory experiment with students and in the online experiment with household heads. However, a few additional questions were added to the online experiment, which were not relevant for the students (for example, concerning the address to which the shopping voucher was to be sent).

Text highlighted in grey indicates text which differed according to treatments and random draws.

- Screen 1 - T1/T2

Welcome!

Thank you very much for participating. Please do not talk to each other and do not use any electronical devices like mobile phones or smartphones during the entire event. Please read the instructions carefully and if you have questions, signal it by raising your hand.

At the end of the event, you will receive an individual payment in cash. Your payment depends on your decisions and a random component.

Here, you can find the key points of the experiment:

- The experiment comprises a task with 10 rounds and a questionnaire.
- The task is explained to you in detail in the course of the study.
- The amounts of money mentioned are real amounts in EUR, which can be actually paid out in cash at the end.
- You will only receive a remuneration for your participation if you complete the task and fill in the questionnaire completely.
- The remuneration for your participation is explained to you in detail on the following pages.

Please note: To navigate from one screen to the next, always use the buttons provided on the screen and **not** your internet browser as otherwise successful completion of the survey cannot be guaranteed.

Now click on “Next“ to learn more about your task and remuneration.

- Screen 2 - T1/T2

How is my remuneration determined?

Your remuneration comprises two parts:

- If you **complete** the task and the questionnaire, you will receive a **participation remuneration** of **EUR 5** in any case.
- The task consists of 10 rounds in which additional pay-outs arise for you. The amount of these pay-outs depends on your decisions and a random component (similar to a coin toss).
- For the pay-out, one of these 10 rounds is randomly selected, every round being equally likely. That means that every round can be relevant for your pay-out.

Your complete remuneration for participating in the study is thus determined as follows:

Your complete pay-out = EUR 5 + pay-out from a randomly selected round

On the following screens, your task is explained in detail. Please read them carefully. On the respective decision screen, you can find a summary of the task and pay-out rules.

- Screen 3 - T1/T2

Explanation and task execution

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Your task consists in making a decision in each of **10 rounds**. In all of these 10 rounds, the decision situation is **identical**, however, the **individual rounds are independent from each other**. That is, you make a new decision in each of these rounds under the same conditions.
On the next screen, the decision situation is explained.

- Screen 4 - [T1] [T2]

Execution of a round
In every round, you receive an initial endowment of EUR 20, from which EUR 0.50 are subtracted and contributed to an aid fund. You are left with a remaining credit of EUR 19.50 at your free disposal.
In every round, damage can occur. This possible damage amounts to EUR 15. Lots are drawn every time to determine if damage occurs. The likelihood that **damage occurs** is always **one fifth (20%)**; the likelihood that **no damage** occurs is **four fifths (80%)**.
You decide whether you want to take up insurance against possible damage. If you do, EUR 3.50 are subtracted from your remaining credit and you are completely insured against damage.
After you have decided on insurance, lots are drawn to determine if damage occurs or not.

- If you have decided **in favor of obtaining insurance**, your result for this round is the following:
  Initial endowment – contribution to aid fund – insurance premium = result of round
  20 – 0.50 – 3.50 = EUR 16.00
- If you have decided **against obtaining insurance** and **no damage** occurs, your result for this round is the following:
  Initial endowment – contribution to aid fund = result of round
  20 – 0.50 = EUR 19.50
- If you have decided **against obtaining insurance** and **damage occurs**, damage first amounts to EUR 15. [T1: Your result of this round, however, depends on a decision by the aid fund. The decision on whether you receive full compensation or not is taken randomly. The likelihood of **receiving compensation is one third (about 33%)**; the likelihood of **not receiving compensation is two thirds (about 67%).** This means that your result in this round depends on you receiving or not receiving compensation.
  - If **damage occurs** and you receive **compensation**, your result for this round is the following:
    Initial endowment – contribution to aid fund – damage + compensation = result of round
    20 – 0.50 – 15.00 + 15.00 = EUR 19.50
  - If **damage occurs** and you receive **no compensation**, your result for this round is the following:
    Initial endowment – contribution to aid fund – damage = result of round
    20 – 0.50 – 15.00 = EUR 4.50
    ]

  [T2: You, however, receive compensation for part of the damage from the aid fund. The aid fund compensates you for **a third of the damage (EUR 5)**. The remaining **two thirds of the damage (EUR 10) are not compensated for.** Hence, your result for this round is the following:
  Initial endowment – contribution to aid fund – damage + compensation = result of round
  20 – 0.50 – 15.00 + 5.00 = EUR 9.50]

- Screen 5 - T1/T2

Which pay-out rules are there?
On the following screens, you are asked to make your decisions. In this part, you can earn money in addition to your participation remuneration. Please note that the participation remuneration of EUR 5 is independent from the decisions made in this part and is given to you in any case.

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At the beginning of every round, you can see in which of the 10 rounds you are. For pay-out, **one** of these 10 rounds is randomly selected, every round being equally likely. That means that every round can be relevant for your pay-out.

- Screen 6 - T1/T2

Decision round 1

Please decide now if you want to obtain insurance: Yes/No

Reminder:

Decision situation

In every round, you receive an initial endowment of EUR 20, from which EUR 0.50 are subtracted and contributed to an aid fund. That means you are left with a remaining credit of EUR 19.50 at your free disposal. In each of these 10 rounds, damage can occur (with a likelihood of one fifth, or 20%). This possible damage amounts to EUR 15. Lots are drawn to determine if damage occurs. In every round, you have two options:

- You obtain insurance against damage. That means you pay EUR 3.50, using your remaining credit and are completely insured against possible damage in this round.
  Or:
- You do not obtain insurance against damage. That means you are not insured against damage in this round. If damage does not occur, you keep your entire remaining credit. [T1: If damage occurs, the aid fund might compensate you fully (with a likelihood of one third, about 33%). However, you could also receive no compensation in case of damage (with a likelihood of two thirds, about 67%).] [T2: If damage occurs, you receive a compensation of EUR 5 from the aid fund (a third of the damage). The remaining damage is subtracted from your remaining credit.] [T2: If damage occurs, you receive a compensation of EUR 5 from the aid fund (a third of the damage). The remaining damage is subtracted from your remaining credit.]

- Screen 7 - T1/T2

You have decided in favor of/ against obtaining insurance. Lots are drawn to determine if damage occurs. The likelihood that **damage occurs** is **one fifth (20%)**; the likelihood that **no damage** occurs is **four fifths (80%)**.

- Screen 8 - T1/T2

It has been drawn by lot that damage/ no damage occurs.

- Screen 9 – only T1: no insurance and damage

Damage has occurred. Damage first amounts to EUR 15. Your result in round 1, however, depends on the compensation by the aid fund. A random decision is now taken on you receiving compensation for the **full amount of damage** or not. The likelihood of you **receiving compensation is one third (about 33%)**; the likelihood of you **receiving no compensation is two thirds (about 67%).**

- Screen 10 – only T1: no insurance and damage

It has been randomly decided that you receive **compensation** from the aid fund for the full amount of damage. /**no** compensation from the aid fund.

- Screen 11 -

[T1/T2 no damage without insurance:

Your result in round 1:

Initial endowment – contribution to aid fund = result of round

20 – 0.50 = **EUR 19.50]**

[T1/T2 no damage with insurance:

Your result in round 1:

Initial endowment – contribution to aid fund – insurance premium = result of round

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20 – 0.50 – 3.50 = **EUR 16.00**]
[T1/T2 damage with insurance:
Your result in round 1:
Initial endowment – contribution to aid fund – insurance premium = result of round
20 – 0.50 – 3.50 = **EUR 16.00**]
[T1 damage without insurance with aid fund:
Your result in round 1:
Initial endowment – contribution to aid fund – damage + compensation = result of round
20 – 0.50 – 15.00 + 15.00 = **EUR 19.50**]
[T1 damage without insurance without aid fund:
Your result in round 1:
Initial endowment – contribution to aid fund – damage = result of round
20 – 0.50 – 15.00 = **EUR 4.50**]
[T2 damage without insurance with aid fund:
Your result in round 1:
Initial endowment – contribution to aid fund – damage + compensation = result of round
20 – 0.50 – 15.00 + 5.00 = **EUR 9.50**]
Round 1 is finished now, round 2 follows.

- Screen 12 - T1/T2

Decision round 2
See above, repetition for 10 rounds (screens 12 to 65)

- Screen 66 - T1/T2

Results for all 10 rounds: (Table)

Thank you very much for your decisions in the 10 rounds of the task.
Finally, we ask you for information about yourself and your attitudes in the following questionnaire. It contains selected questions from the field of social research and we would like to ask you to answer them carefully. Of course, all your information is saved anonymously and not passed on to third persons. After the questionnaire, one of the rounds is randomly selected for your pay-out with each round being equally likely. The pay-out does not depend on your answers in the questionnaire. However, the pay-out can only take place if you fill in the following questionnaire completely.

- Screen 67 - T1/T2

1. Please indicate your gender:
(0) Male
(1) Female
(20) No information

2. How old are you?
(20) No information

3. How well do you speak German?
(0) Badly
(1) A little
(2) Well
(3) German is my mother tongue
(20) No information

4. In which study program are you enrolled? _______________

5. Are you volunteering in a non-profit association or non-profit organization?
(1) Yes

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(2) No
(20) No information
If yes:
In which organization or association are you volunteering?

____
(20) No information

6. How do you see yourself: Are you ready to take risks generally or do you try to avoid risks?
Explanation of scale: 0 (not at all ready to take risks) to 10 (very ready to take risks)
0…..10
(20) No information

- Screen 68 - T1/T2

7. What is your opinion on the following three statements?

	Not true at all	Rather not true	Partly	Rather true	Completely true	No information
In general, you can trust other people.						
Today there is no one you can rely on anymore.						
When interacting with strangers, it is better to be careful before you start trusting them.						
My plans are often thwarted by fate.						

8. Do you think that most people...
(1) ...would take advantage of you if they had a chance?
(2) ...or would try to be fair to you?
(20) No information

9. Would you say that most of the time people...
(1) ...try to be helpful?
(2) ...or rather pursue their own interests?
(20) No information

10. What is your opinion on the following statements? :

	Not true at all	Rather not true	Partly	Rather true	Completely true	No information
I am in control of my life.						
If I make an effort, I will be successful.						
Both my private and professional life are in large part controlled by others.						
My plans are often thwarted by fate.						

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11. In principle, do you think it is the state's job to …
Explanation of scale: 0 (I don't agree at all) to 10 (I agree completely)

1. … guarantee an adequate standard of living for senior citizens. 0-----10
   No information
2. … support industries financially so that they can grow. 0-----10
   No information
3. … guarantee an adequate standard of living for unemployed people. 0-----10
   No information
4. … reduce differences in income between the poor and the rich. 0-----10
   No information

- Screen 69 - T1/T2

In addition to the participation remuneration of EUR 5, one of the 10 rounds is randomly selected for pay-out.

- Screen 70 - T1/T2

Round xx has been randomly selected.
Hence, your pay-out is:
EUR 5 participation remuneration + EUR xx additional pay-out = EUR xx total pay-out

- Screen 71 - T1/T2

Please fill in your first name and your last name and also your seat number for the pay-out. This information is used for pay-out only and treated confidentially.
First name ___________________
Last name ___________________

- Screen 72 - T1/T2

Thank you very much for participating in the experiment. Please remain seated until we call you.

A.2: Invitation Email sent out to the sample of households in Dornbirn (Austria) (translated from German)

Invitation to participate in a scientific study

Dear citizens of Dornbirn,

A couple of months ago, you answered a questionnaire on flood protection and stated that you were willing to participate in another interview. Now we would like to take you up on that.

We invite you to be part of the next step of this project, which is currently being carried out jointly by the city of Dornbirn and the Centre for European Economic Research. We would kindly like to ask you to participate in this survey. By doing so, you are making a significant contribution to social science. For your participation, you are going to receive a shopping voucher of at least EUR 5. Your decisions in the online survey will further increase this amount.

No prior knowledge is required to participate in the survey. Please note that only one person per household can participate in the study and this person needs to be of age. If possible, it should be the same person that answered the questionnaire a couple of months ago.

Participation takes 20 minutes. You can participate as of now till 30.11.2018. To do so, please click on the following link:

Link to the survey

Your personal access code (valid until 30.11.2018) is: *5-digit code*

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Your personal data will, of course, be treated confidentially and will be anonymously analyzed for scientific purposes only. In case you have any questions, do not hesitate to contact Dr. Daniel Osberghaus via phone 0049-621-1235205 or email umfrage2018dornbirn@zew.de.

We are looking forward to your participation in this survey, which certainly will also be interesting for you. Finally, we would kindly like to thank you for supporting our research project.

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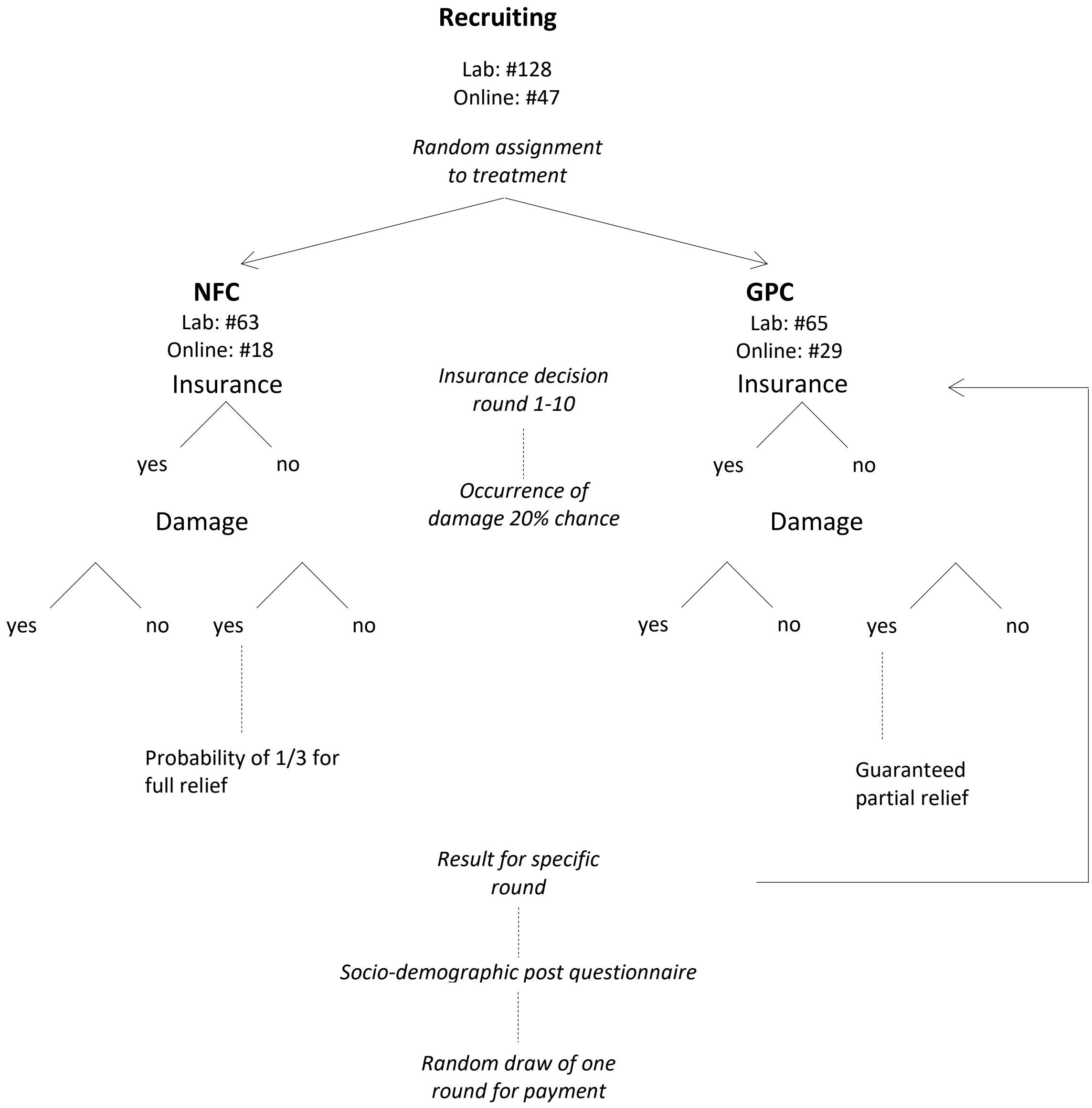
A.3: Design and Procedures

Figure A. 1: Schematic diagram experimental procedures



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