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Does having insurance change individuals' self-confidence?

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Abstract
Recent research in contract theory on the effects of behavioral biases implicitly assumes that they are stable, in the sense of not being affected by the contracts themselves. In this paper, we provide evidence that this is not necessarily the case. We show that in an insurance context, being insured against losses that may be incurred in a real-effort task changes subjects' self-confidence. Our novel experimental design allows us to disentangle selection into insurance from the effects of being insured by randomly assigning coverage after subjects revealed whether they want to be insured or not. We find that uninsured subjects are underconfident while those that obtain insurance have well-calibrated beliefs. Our results suggest that there might be another mechanism through which insurance affects behavior than just moral hazard.

KEYWORDS
insurance choice, overconfidence, underplacement

JEL CLASSIFICATION
C91; D82; D84

1 | INTRODUCTION

Self-assessments and beliefs matter in decision making and contract design. Optimal decisions depend on correct self-assessments and beliefs that are not systematically biased. One important example is self-confidence in own ability and performance. In particular, overconfidence has been established as a relevant aspect in individual's economic behavior. For example, overconfidence has been found to predict excess market entry of entrepreneurs (Camerer & Lovallo, 1999), risky investment decisions of CEOs (Malmendier & Tate, 2005), and speculative trading (Scheinkman & Xiong, 2003). In the context of insurance, Sandroni and Squintani (2007) consider the Rothschild and Stiglitz (1976) model in the presence of overconfident individuals. They find that if the share of overconfident types in the population is large enough, compulsory insurance is not Pareto-optimal anymore. It follows that overconfidence as a behavioral inclination has important implications for contract design in several settings (see e.g., De la Rosa, 2011; Santos-Pinto, 2008; Sautmann, 2013).

While the majority of papers focuses on the case of overconfidence, situations in which individuals are underconfident are also researched (Benoît, Dubra, & Moore, 2015; Clark & Friesen, 2009; De la Rosa, 2011; Hoelzl & Rustichini, 2005; Moore & Cain, 2007; Sandroni & Squintani, 2007; Sautmann, 2013). Imperfect self-confidence calibration relates to many effects observed in human decision making. However, a general interpretation of the literature on self-confidence is that over- or underconfidence are comparably stable traits, at least within a certain decision environment. That is, one can be overconfident when driving and underconfident with math tasks, but overconfidence when driving should not be affected by the color of the car that one drives.

This paper provides evidence for self-confidence to be malleable in a setting that has relevant implications. Our focus here is on over- and underplacement as a specific form of over-/underconfidence. Larrick, Burson, and Soll (2007) define the degree of an individual's overplacement as the difference between her perceived and actual percentile in the distribution of performance within a group. It differs from other concepts of overconfidence in that it depends on the expected performance of others. We show in a laboratory experiment that self-placement depends on whether people acquire insurance or not. While insurance in our setup partially covers potential losses from bad performance in a real-effort task, it should be unrelated to self-placement for rational decision makers. At the same time, we find no evidence for more overconfident individuals choosing more or less insurance in the first place.

More specifically, we implement an experimental design that allows us to disentangle effects from the incentives provided by the insurance contract from effects coming from selection into the contract. In the insurance context, the former is known as the problem of moral hazard and the latter as the problem of adverse selection.

Before attempting the real-effort task, individuals are given the choice to buy the insurance contract. Conditional on this choice, actual insurance status is randomized, that is, whether one obtains insurance or not is based on a random draw, and individuals are informed about their insurance status throughout the experiment. Our design is similar to the one used in a credit market field experiment by Karlan and Zinman (2009). Their idea is to attract borrowers with an advertised interest rate and, conditional on showing up in the lenders office, to randomize the actual interest rate. However, Karlan and Zinman (2009) are not able to impose an interest rate that is higher than the one advertised, as borrowers could simply walk out of the experiment. In a laboratory experiment, by design there is no attrition. This allows us to assess whether the effect of insurance on relative self-assessment only comes from feeling (un-)lucky when actually (not) receiving it—remember, insurance status is based on a random draw—or whether there is another mechanism that is consistent with the effect. A related design is used by Bó, Foster, and Putterman (2010), who let individuals vote on a policy that allows punishment for defection in a prisoners dilemma, but then randomize the actual implementation of the policy (see also Sutter, Haigner, & Kocher, 2010).

Our real-effort task involves the forecasting of numbers with the help of two cue values (Brown, 1998; So, Brown, Chaudhuri, Ryvkin, & Cameron, 2017; Vandegrift & Brown, 2003). This task fulfills two requirements for our purpose of creating a realistic insurance setting. First, the ability for forecasting, which might in the present case be related to math skills, varies sufficiently in the sample to create different levels of confidence in own ability. Second, the participant's effort can influence the precision of their forecasts and thus their relative performance. Schram and Sonnemans (2011) also consider insurance choice by varying various parameters such as the number of available contracts. However, in their setting, losses occur without a subject's influence, which may not be realistic for some insurance contracts such as car insurance. Previous experiments studied insurance choice with exogenous loss in various settings, see for example Ganderton, Brookshire, McKee, Stewart, and Thurston (2000) and Laury, McInnes, and Swarthout (2009). Our design naturally exhibits features of insurance markets outside the laboratory such as adverse selection and moral hazard, but of course it has to abstract from some features of real-world insurance markets.

Self-placement is measured as the difference in an individual's self-assessed and true performance rank among all participants within an experimental session, regardless of whether they had insurance or not. The elicitation of the self-assessed rank is incentivized by rewarding accuracy. We find that, on average, insured individuals have well-calibrated beliefs about their ability relative to others, while those individuals that do not have insurance underplace themselves. These results are in line with experiments by Clark and Friesen (2009) and Murad, Sefton, and Starmer (2016), who argue that the use of real-effort tasks and incentivized confidence elicitation leads to a lack of overconfidence which is generally observed in "better-than-average" predictions. Moore and Cain (2007) and Hoelzl and Rustichini (2005) find that subjects tend to underplace themselves in tasks that are perceived as difficult and where performance is low in absolute terms, which is also true for our setup. Meub and Proeger (2015) study socially derived anchors, and they show that the social context increases biased behavior.

There is a second interpretation of our results that is also possible.[1] Participants could be unaware of or have miscalibrated beliefs regarding the effect of insurance on their relative performance. More precisely, this could be relatively miscalibrated beliefs about own moral hazard or miscalibrated beliefs about the performance of others without insurance. While we have no way of formally disentangling the two possibilities, given the relative measure of confidence, the specifics of our setup tend to favor the miscalibration of beliefs about own moral hazard when being insured.

The contribution of our paper is threefold. First, we show that individuals' self-confidence can be affected by contracts. While in its generality, this result is probably not too surprising, its impact on our insurance application bears relevant implications—just imagine that drivers become relatively more overconfident after being insured. While contract design has started to take behavioral biases into account (Kőszegi, 2014), we are not aware of any existing model that would be consistent with our main finding. Second, we experimentally study assumptions made

[1]We are very grateful to the editor for pointing out this interpretation.

on the selection mechanism into contracts based on presumably stable traits such as self-confidence calibration (see e.g., Sandroni & Squintani, 2007, 2013). This paper thus speaks to a broader literature that studies sorting into contracts based on behavioral biases and preferences (Dohmen & Falk, 2011; Larkin & Leider, 2012). Finally, we add experimental evidence to decision making in a behavioral insurance context in which own effort instead of a random device determines losses to a significant extent (Browne, Knoller, & Richter, 2015), and we think that thus our contribution is also methodological.

2 | EXPERIMENTAL DESIGN

The experiment was conducted at MELESSA, the economics laboratory of the University of Munich. It was computerized with the help of z-tree (Fischbacher, 2007), and participants were invited with the organizational software ORSEE (Greiner, 2015).

Monetary payoff was based on points, converted to euros at a fixed and pre-announced exchange rate. Participants received an endowment of 100 points, equal to EUR 10. The show-up fee for participants was an additional EUR 4.

The experiment consisted of three parts, and participants were aware of this from the start of the experiment. They were not informed about the content of the next part before the end of the previous part. In this paper, we analyze only the data from Part I of the experiment.[2] Experimental instructions for all parts are provided in the Supporting Information Appendix B.

In the following, we first explain the experimental procedure of Part I. We then discuss three features of the experimental design in more detail: belief elicitation, the real-effort task, and the insurance decision.

2.1 | Experimental procedure

Part I of the experiment consisted of seven stages that are illustrated in Figure 1, along with the variables that were generated at each stage. Screenshots of the key Stages 2–6 can be found in the Supporting Information Appendix C.

In Stage 1, subjects received a sheet of paper with 10 examples of solutions in the real-effort task (see the section on the details of the task below). The real-effort task was a forecasting task, and the examples could be studied for 5 min. A pen was provided, and participants were allowed to take notes, which was actually done frequently.

Stage 2 consisted of five practice rounds (five forecasts) with feedback on individual performance. These practice rounds were not incentivized, but there was an implicit incentive in the form of a potential information gain regarding one's own ability in this task.

In Stage 3, individuals had to decide whether they wanted to buy the insurance for the upcoming payoff-relevant rounds or not. The insurance covered 65% of losses in the real-effort task and came at a cost of 22.5 points. An on-screen calculator could be used at this point.

In Stage 4, actual insurance receipt was randomized, and the choice made in Stage 3 was realized with 70% probability. Thus, if a subject did not want to buy insurance, there was still a

[2]Part II consisted of a set of lottery decisions; Part III was a short survey on subjects' experience with insurance. The payoffs reported here are only those that subjects achieved in Part I. Subjects' total earnings from the experiment were the sum of the payoffs in Parts I and II, while the questionnaire in Part III was not incentivized.

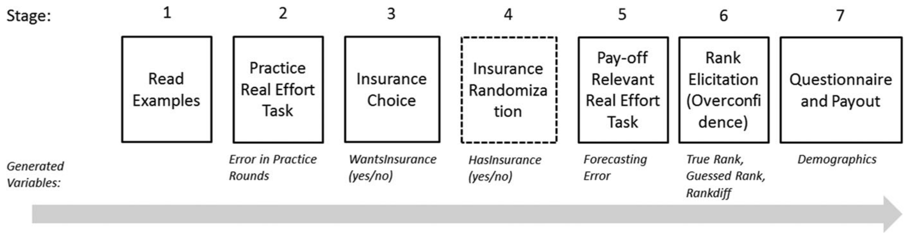

FIGURE 1 Experimental procedure and definition of variables

30% probability that she got the insurance and that she had to pay the premium. Conversely, there was an equally large chance to not receive insurance, although the subject wanted to buy it. This creates a 2×2 matrix of possible outcomes shown in Table 1. The probability of 70% was chosen trading-off incentive-compatibility and statistical power. A message informed participants about the realized insurance status.

Stage 5 comprised the 10 rounds of the real-effort forecasting task. The message showing the realized insurance status stayed on the screen throughout this stage.

After the 10 rounds of the real-effort task were completed, we elicited self-assessed performance in Stage 6. Individuals were asked to think about their average performance in the previous 10 rounds and should indicate which rank they think that they hold in their respective session, consisting either of 23 (one session) or of 24 (six sessions) participants. The person with the lowest average forecasting error would take the first rank, the one with the second-lowest the second rank, and so on. At this point, subjects had not received any feedback on their or other participants' performance. Guessing the rank correctly earned 10 additional points, and a deviation of ± 1 from the realized rank earned 5 additional points.

Between Stages 6 and 7, the second part and the third part of the experiment took place. In Stage 7, one of the 10 real-effort task rounds was randomly drawn by the computer to be payoff-relevant, and subjects were informed about their performance and earnings in this round. They also learned how much they earned from the ranking guess. At the end of the experiment, individuals answered a standard demographic questionnaire and were paid out in private.

All stages were known in advance and common knowledge among participants, with the exception of Stage 6. This is a key feature of the experiment: So as not to influence the insurance choice and performance in the earlier stages, we announced that we elicit subjects' assessment of their own relative performance (Stage 6) only after the insurance decisions and the real-effort task had been completed in the earlier stages.

TABLE 1 Sample distribution

		Actual		
Insurance status		**Yes**	**No**	**Total**
	Yes	68	41	109
Choice		41%	25%	
	No	13	45	58
		8%	27%	
	Total	81	86	167

2.2 | Remarks on belief elicitation

There are several aspects of the elicitation that warrant a short discussion. We chose to measure confidence in performance after the task, instead of before the task, to avoid hedging behavior and possible priming or goal-setting effects. Asking individuals about their relative performance to others before the task could give the wrong impression of a competitive environment, which we neither consider in this paper, nor is it common in an insurance context outside the laboratory.

We are well aware of the fact that linear incentives when eliciting beliefs have their limitations (see, Gächter & Renner, 2010; Trautmann & van de Kuilen, 2015). Obviously, a linear scoring rule is not a proper scoring rule, but even proper scoring rules do not account, for instance, for respondents risk attitudes that might matter in this context. Offerman, Sonnemans, Van de Kuilen, and Wakker (2009) discuss potential adjustments for risk attitudes and for non-expected utility models. Nonetheless, it is important to notice that approximately risk-neutral respondents have an incentive to report the event they think is most likely to occur under a linear scoring rule (see Schlag, Tremewan, & Van der Weele, 2015). Hence, under mild assumptions, our measure of confidence should not be systematically biased, on average, compared to a quadratic scoring rule.

Unfortunately, there is little empirical evidence in experimental economics that provide a comparison between different elicitation methods regarding beliefs. Thus, all forms of elicitation exist in experimental economics papers: unincentivized elicitations, linearly incentivized elicitations, elicitations based on quadratic scoring rules, and elicitations based on quadratic scoring rules with adjustments for risk attitudes. In other words, the trade-off between straightforward implementation (easy to explain, easy to understand) and desirable theoretical properties (full incentive compatibility) has not been solved by the profession. Schlag et al. (2015) provide an extensive discussion on belief elicitation. They conclude that "given the limited time and resources that we can invest in belief elicitation, simple mechanisms that yield more imprecise information, [...], may be preferable to more complicated ones that yield very precise beliefs" (p. 485). However, they also argue that it depends on the context, on the participant pool, and on the research questions at hand when one wants to determine the behaviorally optimal scoring rule. They continue in stating that more empirical research is needed that compares different scoring rules in action to determine behaviorally proper ones.

2.3 | More information on the real-effort task

We used the forecasting task by Brown (1998), Vandegrift and Brown (2003), and So et al. (2017). Participants are asked to enter the price Y of a fictitious stock whose price they had to predict from two cue values W_1 and W_2. The true relationship of Y and the two cues was given by

$$Y = 50 + 0.3W_1 + 0.7W_2 + e, \tag{1}$$

where $W_1, W_2 \sim U(0, 250)$ and $e \sim N(0, 5)$. Y was rounded to the nearest integer. Individuals knew that there was a potential constant, but did neither know that the function was linear, that the weights added to one, nor that there was a random error term e. During the task, individuals where shown W_1 and W_2 on the screen and had 60 s every round to enter their forecast $\hat{Y}$ into a box and click OK (see Figure C.4 in the Supporting Information Appendix). The remaining time was always displayed on screen. Individuals could not earn more points by entering an answer faster than in 60 s, but after 60 s without any input, the program would skip to the next round, automatically creating a no-input. We introduced a penalty to avoid this, and the details are described in the next section. From the forecasting input we derived the error in each forecast, which is given by the absolute difference between the true and the predicted value of Y:

$$\text{Error} = |Y - \hat{Y}|. \tag{2}$$

2.4 | Insurance

Based on a pilot of the real-effort task, we set the insurance premium to 22.5 points, with a coverage rate of 65%. Remember that only one round was payoff-relevant, that is, the insurance was valid for all rounds. Earnings from the task are

$$\text{Earnings}_{\text{no}} = 100 - \text{error} \tag{3}$$

for individuals that did not get the insurance and

$$\text{Earnings}_{\text{in}} = 100 - \text{error} \times (1 - 0.65) - 22.5 \tag{4}$$

for those that did. Thus, insurance covered 65% of the loss from the absolute difference between the true and the predicted value of Y. Notice that we capped losses at the zero earnings boundary. As a consequence, there were no losses from this part of the experiment unless a participant had not entered any forecast at all for the randomly chosen round and was insured. In that case, the participant would have to pay the insurance premium of 22.5 points from her show-up fee. This happened only once.

2.5 | Experimental participants

We conducted seven sessions in the MELESSA laboratory at the University of Munich. Six sessions involved 24 participants and one session 23 participants. In total, 167 subjects participated and earned on average EUR 12.50 in a bit more than 1 hr. Participants were mainly students from various backgrounds, with 33% studying economics or business, 18% life sciences or engineering and 13% humanities. Almost 60% of participants were female, and age ranged from 18 to 43, with an average of 22.

3 | RESULTS

3.1 | Descriptive results on self-placement and insurance choice

We first look at a set of descriptive results. Our variable of interest is *rankdiff*, the difference between the individual's actual and guessed ranks as entered in Stage 6 of the experiment:

$$\text{Rankdiff} = \text{TrueRank} - \text{GuessedRank}. \tag{5}$$

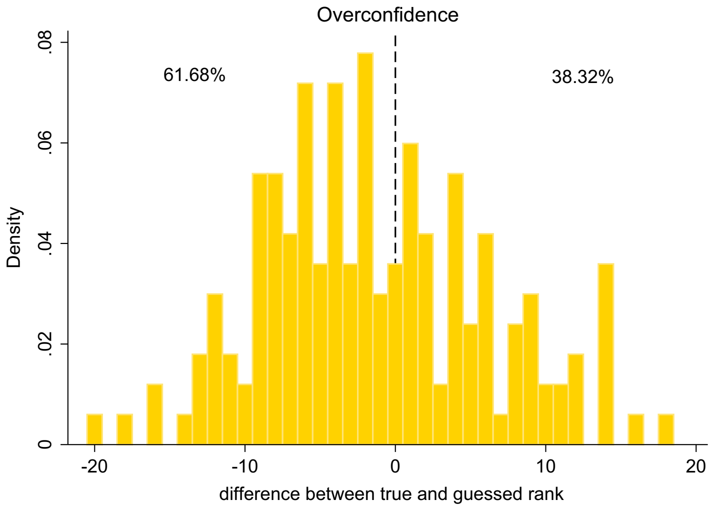

FIGURE 2 Distribution of variable rankdiff [Color figure can be viewed at wileyonlinelibrary.com]

A positive value indicates overplacement, where higher values imply stronger overplacement. A similar variable has been applied by Sautmann (2013), who uses the difference between predicted and actual scores in trivia quizzes as her measure for overconfidence. The mean of *rankdiff* in our study is −1.37 (which is significantly different from zero at the 5% level), indicating slight underplacement, on average. The distribution of *rankdiff* is shown in Figure 2.[3] The average underconfidence result is in line with Hoelzl and Rustichini (2005) and their task-specific explanation. However, there exists considerable variation of *rankdiff* in our sample on the individual level and when comparing treatments. An alternative measure is a simple indicator variable for overconfidence. It takes on the value one if *rankdiff* is larger than zero, and the value zero otherwise. The entire sample has a share of 38.32% overconfident individuals according to this measure.

Remember that we can distinguish between four insurance outcomes, indicated by the variables *HasInsurance* and *WantsInsurance*. The variable *HasInsurance* describes the true insurance status of an individual in the real-effort task, and it is randomized. The variable *WantsInsurance* describes the individual's initial choice for or against insurance, and it is endogenous in the sense that it may correlate with any observed or unobserved individual characteristics such as gender, age, and risk attitude. Conditional on insurance choice (=*WantsInsurance*), *HasInsurance* identifies the incentive effects of the insurance contract. Conditional on actual insurance status (=*HasInsurance*), *WantsInsurance* identifies selection effects, that is, differences between individuals who wanted insurance and those who did not.

Table 2 displays means and standard deviations of *rankdiff* by insurance outcome. Table A.1 in the Supporting Information Appendix contains *p* values of *t* tests within every cell of Table 2 whether the mean of *rankdiff* is significantly different from zero. In addition, Supporting Information Table A.2 displays *p* values of pairwise, two-sided Wilcoxon–Mann–Whitney tests for differences in *rankdiff* between all experimental groups. We observe strong and highly significant underplacement without insurance. There is, however, also significant underplacement

[3]The distribution of *rankdiff* by insurance choice and actual insurance status is shown in the Supporting Information Appendix Figure A.2.

TABLE 2 Mean and standard deviation of rankdiff

	WantsInsurance = 1	WantsInsurance = 0	Total
HasInsurance = 1	0.088	−0.46	−2.01
	(7.39)	(6.00)	(7.67)
HasInsurance = 0	−2.88	−2.46	−1.03
	(6.99)	(7.96)	(7.41)
Total	−2.66	0.00	−1.37
	(7.56)	(7.23)	(7.50)

for those who did not want insurance, when we pool observations for those who ended up with insurance and those who did not.

Two-thirds (109 of 167) of individuals wanted to buy the insurance. We can investigate which individual characteristics predicted insurance choice. Table 3 shows mean values of these variables by insurance choice status and in the full sample. Individuals who made larger errors in the practice rounds were more likely to prefer insurance, which is in line with standard predictions of adverse selection models. *Insurance pays off* is a dummy equal to one if the forecasting error in a practice round was larger than 22.5/0.65 = 34.62, which is the break-even point (error) of the insurance for a fully rational risk-neutral decision maker. There is a large difference of 20 percentage points between those who wanted insurance and those who did not. However, buying insurance would still have paid off in 40% of rounds for those that did not want to buy insurance. On average, in 53% of the practice rounds insurance would have paid off. This is a feature that does not necessarily show up in settings outside the laboratory in which insurance is often used in a trade-off between reducing risk and lowering expected payoff. However, there is considerable heterogeneity across individuals in this respect in our data. Females more frequently wanted insurance than males and so did younger individuals.

3.2 | Regression analysis

We now turn to the effect of insurance on self-confidence and selection into insurance based on self-confidence by using parametric models. All regressions in Table 4 use ordinary least squares estimations and include session fixed effects.[4] We start with performance in the real-effort task in the first column. We find that having the insurance increases the absolute forecasting error by 4 points (or 0.15 standard deviations). The same difference is found between individuals who wanted and not wanted insurance. The first effect is moral hazard and the second adverse selection, two classic elements in insurance markets (Rothschild & Stiglitz, 1976; Shavell, 1979). Column 2 shows the direct consequence of a lower performance in the task: both incentive and selection effects lead to a higher (i.e., worse) ranking within a session. Column 3 concerns the rank that individuals guessed they are taking. Individuals who ultimately got the insurance do not rank themselves worse or better than those who did not. In contrast, the pure selection effect in guessed ranks equals the one in true ranks. It follows in column 4 that insurance increases the difference between individual's guessed and actual

[4]Ordered logit (for rank outcomes) and logit (for the overconfident dummy) models yield very similar results. The results are available on request.

TABLE 3 Insurance choice

	Did not want insurance	Wanted insurance	Total
Error in practice rounds	41.52	57.81***	52.15
Insurance pays off	0.40	0.60***	0.53
Female	0.36	0.67***	0.56
Age	23.33	21.42***	22.08

Note: Insurance pays off is a dummy equal to one if the forecasting error in a practice round was larger then 22.5/0.65 = 34.62, which is the break-even point (error) of the insurance for a fully rational risk-neutral decision maker. *, **, and *** indicate mean differences are significant at 10%, 5%, and 1% level, respectively. Standard errors clustered at individual level in rows 1 and 2.

rank by 2.96 ranks. Conditional on actual receipt, there exists no significant difference in self-confidence between those subjects that wanted and did not want the insurance. This is in contrast to Sandroni and Squintani (2007), who assume that overconfident individuals are less likely to buy insurance, because they perceive their risk to be lower than is actually the case. We find that, on

TABLE 4 Insurance and overconfidence

Outcome	(i) Error	(ii) True rank	(iii) Guessed rank	(iv)	(v) Rank-diff	(vi)	(vii)	(viii) 𝟙{Rankdiff >0}
HasInsurance	4.088**	2.311**	−0.649	2.960**	2.443	3.157**	0.240***	0.251***
	(1.729)	(1.147)	(0.872)	(1.235)	(2.137)	(1.254)	(0.082)	(0.083)
WantsInsurance	4.032***	3.081***	3.303***	−0.222	−0.473	0.925	−0.016	0.042
	(1.544)	(1.177)	(0.893)	(1.262)	(1.710)	(1.400)	(0.084)	(0.091)
Has × Wants					0.729			
Insurance					(2.709)			
Female						−1.651		−0.016
						(1.329)		(0.080)
Age						0.391**		0.031***
						(0.171)		(0.010)
Constant	18.171***	9.368***	11.341***	−1.974	−1.943	−11.268**	0.296**	−0.475*
	(2.407)	(1.730)	(1.118)	(2.174)	(2.187)	(4.793)	(0.114)	(0.263)
Session f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,670	167	167	167	167	167	167	167
Adj. R^2	0.017	0.056	0.053	0.000	−0.006	0.028	0.032	0.074

Note: Rankdiff is the difference between the true and guessed rank of performance in the task. Individuals were incentivized to guess their rank among all participants in their session with respect to their average performance in the 10 payoff-relevant rounds of the forecasting task. No feedback on performance was provided. Robust or clustered (column 1) standard errors in parentheses.

***$p < .01$.

**$p < .05$.

*$p < .10$.

average, individuals anticipate their performance in the task based on their skill level and adjust their rank accordingly, but independent of the actual insurance status.

In the following, we investigate if other biases specific to the experimental environment drive our results. One explanation could be that not getting the insurance despite wanting it leads to what is called "choking," a sudden decline of concentration and performance when individuals feel under pressure (Baumeister, 1984). This could lead to a severe underestimation of own performance, independent of its true level. Conversely, individuals receiving the insurance might feel lucky and thus rank themselves better than they actually are. These two confounding factors imply that the effect of the insurance on self-confidence should be larger among those individuals who also wanted it. In our 2×2 design, we can test for this possibility. Column 5 of Table 4 shows that the interaction term between wanting and actually receiving the insurance is positive, but far from significant. The main effect of the insurance is not significant anymore, but the point estimate is similar to that in the columns before.[5] Column 6 includes gender and age as explanatory variables to check if these explain the nonsignificant selection effect. Although the coefficient turns positive, it is not statistically significant and only one-third of the insurance effect. The negative coefficient of the gender dummy indicates that females are underconfident compared to males, which is in line with findings from Bannier and Schwarz (2018) in the domain of financial decisions. However, we found no statistically significant differences with respect to performance in the task between males and females, conditional on insurance status.

Columns 7 and 8 replicate columns 4 and 6 with a dummy equal to one if *Rankdiff* is positive as outcome variable and we get qualitatively similar results. The occurrence of overconfidence in ranking is increased by one-quarter under the insurance contract.

4 | DISCUSSION

One major concern when trying to elicit self-assessment biases is to detect what Benoît and Dubra (2011) call apparent overconfidence. If individuals are Bayesian updaters and receive only a limited number of noisy signals on their performance, they might rationally rank themselves better than others, while this is interpreted as overconfidence by the researcher. This is less of a concern in our experiment, as individuals do not receive any signal on their (or others') performance in the payoff-relevant rounds. Their ranking should therefore solely be based on the perceived difficulty of the task over the ten rounds and an idiosyncratic component, which on average is the same between those that get and do not get the insurance, conditional on choice reflecting individual preferences over insurance. Furthermore, Merkle and Weber (2011) demonstrate that the extent to which apparent overconfidence poses a problem in the laboratory is limited.

Another concern may be an insurance-induced change in a potential hedging motive when confidence levels are elicited. Since insurance reduces the downside risk in the real-effort task, the hedging motive in the elicitation loses importance. As a result, insured individuals could understate their performance less strongly than noninsured. However, this would imply that the insured place themselves at better ranks than the noninsured, which is not the case, as can

[5]This could also be due to lack of power, as the main coefficient of *HasInsurance* now refers to the insurance effect in the group that did not want the insurance and this group comprises only one-third of the sample. The insurance effect in the group that wanted the insurance is still significant at the 10% level.

be seen in column 3 of Table 4. Another change in placement behavior arises if participants anticipate the lower performance of others, potentially induced by having insurance. Knowing that others will perform worse, they can place themselves better in the confidence elicitation. However, such higher order thinking applies to both treatment groups and should therefore be averaged out.

5 | CONCLUSION

In this paper, we reported results of a laboratory experiment in which losses from a real-effort task could be reduced by purchasing an insurance. After subjects revealed whether they want to be insured or not, insurance coverage was randomized. This novel design allows us to disentangle selection from incentive effects.

Self-confidence in the form of self-placement is measured as the difference between an individual's true and self-assessed performance rank. We find that, on average, uninsured individuals underplace themselves, while those individuals that obtain insurance have well-calibrated beliefs about their ability relative to others. Although the previous literature is concerned about selection, we believe that we are the first to demonstrate that incentives that should be irrelevant in standard economic models can change self-confidence ex-post. Moreover, we find no evidence for selection into insurance based on self-confidence.

Why does insurance coverage make individuals relatively less underconfident in their ability than uninsured individuals? One possible explanation suggested by our regression analysis is that individuals do not anticipate the moral hazard that is introduced by the insurance. Subjects do however anticipate their skill level and adjust their rank estimate accordingly. Put differently, the effect of the insurance is not reflected in an adjusted ranking, while the selection effect is. This relates to the second interpretation of our results mentioned in the introduction. Another explanation involves the perception of the difficulty of the task. Under insurance, the task could appear easier, although in fact only the loss that subjects can incur in the real-effort task is lowered. As a consequence, underplacement is reduced. One can imagine alternative psychological explanations: for instance, insurance could let individuals focus more strongly on potential gains and thus the expected performance could appear more gloomy.

Our results have implications for insurance markets. Take car insurance as an example. Outside the laboratory, it is next to impossible to distinguish between potential moral hazard effects and potential self-confidence effects. If both are present, the optimal policy of the insurer should take both into account. Remedies against moral hazard would not be enough to minimize unwanted behavioral tendencies when we assume that biased self-confidence has negative consequences on driving.

The experiment in this paper also has its limitations. For reasons explained above, we do not have measures of self-confidence before randomization of the insurance. Further, we have no information on whether the induced self-confidence translates to other tasks and situations without insurance or on whether it is persistent or not. Ultimately answering this puzzle will require further research on why individuals become overconfident in the first place. It would also be very interesting to vary the attractiveness of the insurance to see whether the effect persists more generally. In addition, studying the impact of different performance beliefs would be relevant, for instance by asking for the expected absolute performance, rather than by asking for the ranking. Moreover, one could exogenously manipulated performance beliefs to study potential miscalibration. Necessarily, some of the design issues in a laboratory experiment are a bit artificial or have to pertain to smaller groups than outside the laboratory. A field experiment with confidence elicitation outside the laboratory could claim a higher level of external validity and would be an extremely interesting option for future research.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section.

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