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Choice or No Choice: Is the Langer Effect Evidence Against Simulation?

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Abstract: The discussion of whether people understand themselves and others by using theories of behaviour (theory theory) or by simulating mental states (simulation theory) lacks conclusive empirical evidence. Nichols et al. (1996) have proposed the Langer effect (Langer, 1975) as a critical test. From people's inability accurately to predict the difference in the subjective value of lottery tickets in choice and no-choice conditions, they argued that people do not simulate behaviour in such situations.

In a series of four experiments, we consistently failed to replicate the original difference between choice and no-choice under the conditions used by Nichols et al. We conclude that the replicability of the effect depends on an unknown combination of factors. As long as the target effect is not better understood and under better experimental control, it is difficult to use it as a yardstick against which the accuracy of simulation can be assessed.

1. Introduction

Under the influence of Wittgenstein's critique of the introspectability and privacy of internal states the standard theory in philosophy of mind has become that mental states are theoretical terms in a theory of behaviour (Fodor, 1985; Sellars, 1956). Within this framework our understanding of the mind is on a par with our understanding of all other domains of knowledge: they all are theories.

This view has recently been challenged drawing on the older tradition of the *'Verstehens-Psychologie'* (Stone and Davies, 1996; Gordon, 1986; Heal, 1986). It is held that we are able to understand other people and ourselves by simulating their mental states. We activate our own mental apparatus through imagination of their situation. The mind is thus the only domain that is accessible to us by this special method. For only mental states can be mentally simulated, nothing else.

Attempts to provide empirical evidence for and against these two fundamental positions have focused on the intuition that theories can be false and,

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therefore, can go astray in their predictions of people's behaviour. In contrast, simulation ought to yield correct predictions provided that the imagined situation captures the relevant features of the simulated person's actual situation (*imaginative adequacy*). Of the psychological data originally adduced (see *Mind and Language*, 1992, Special Issue on Simulation) only a few satisfy this latter criterion to any degree. There is some developmental evidence (Perner and Howes, 1992) that ensured imaginative adequacy by demonstrating children's ability to simulate one mental state induced by the situation but failing to simulate the person's conscious reflection on that mental state. Although these data pose a serious difficulty for introspective versions of simulation theory (Goldman, 1993; Harris, 1991) they may be compatible with Gordon's (1995) anti-introspectionist version based on ascent routines (Perner, 1994).

Problematic data for any version of the simulation theory are discussed by Nichols et al. (1996). They compared people's actual behaviour in a shortened version of Langer's (1975) experiment and people's ability to simulate this behaviour. Langer (1975, Study 2) sold 50 lottery tickets to 50 people. The price of a ticket was $1.00 and the jackpot that could be won was $50.00. Two groups were formed. Participants in one group were given a choice; they could choose the one ticket they wanted out of an urn. Participants in the second group could not choose but were assigned their ticket without being given a choice. In either case, participants owned their ticket for roughly a week and then—on the pretext that another person desperately wanted to participate—were asked to state the price for selling back their ticket. Participants who had been given a choice requested a significantly higher resale price ($8.67) than participants in the no-choice condition ($1.96). Note the big difference between the two groups. Langer interpreted her finding as an instance of the *Illusion of Control* (1975, p. 313): 'An illusion of control is defined as an expectancy of a personal success probability inappropriately higher than the objective probability would warrant'. The illusion may be invoked when factors that are usually related to skilled behaviour (a choice usually makes sense when the person making the choice has the skill or information to make a reasonable decision) are introduced in chance contexts. This illusion works in the choice condition, where the fact that one could choose the ticket heightened the expectation that this ticket might win.

This Langer-type situation was used by Nichols et al. (1996) to test simulation theory against theory theory. Their experiment involved lottery tickets of which participants (N = 30) were either given a particular ticket or could choose one of three tickets as a reward for participating in a 5-minute filler task. After the filler task participants were asked for how much they would sell their ticket back to the experimenter. Participants who had a choice of ticket asked for about 6 times its original value ($6.29) while participants without a choice only asked for 1½ times its value ($1.60). A second group of participants (N = 77) did not do the real experiment, but, instead, simulated the role of a subject in the real experiment. Despite ensuring imaginat-

ive adequacy by showing video tapes of the target participants' choice situation, simulators did not come up with differential predictions for the two conditions ($9.37 in the no-choice and $7.82 in the choice condition). Nichols et al. noted many 'unreasonably high values' in the simulator group. They corrected their data by eliminating the outliers, but, again, could not find a significant difference between choice ($4.62) and no-choice conditions ($3.47).

Nichols et al. consider this to be strong evidence against simulation, provided two conditions are satisfied: (a) simulators and real subjects share similar cognitive structures, and (b) the relevant boundary conditions for simulation (imaginative adequacy) are met. Condition (a) is met since both groups belong to the same population; condition (b) is met since experimenters provided simulators with all relevant information by showing them videotapes of the target persons, making conditions for producing pretend inputs about as good as they could possibly be. Thus, the simulation should be correct. If not, simulation cannot account for the results. People must be doing something else, namely, using a theory. Use of a theory can explain the data since a theory can be false in many respects, resulting in a range of different predictions not related to actual performance.

Harris (1992) had objected to an earlier informal study by Stich and Nichols (1992) based on the same paradigm, that the time lag between receiving the ticket and setting a resale value, which in Langer's original experiment was a whole week, could be critical and that such an effect of time might be difficult, if not impossible to simulate. For that reason it is crucial that Nichols *et al.* were able to find Langer's effect even with an intervening period as short as five minutes. However, even five minutes may be critical for developing a sense of belongingness, so that participants feel that the ticket is truly theirs and start to attach a specifically personal value to it. And even five minutes of belonging might be difficult to simulate. So we set out to investigate the factors operating in Langer's paradigm more thoroughly. In what follows, we present results obtained with slight variations of the paradigm. In our view, these results are apt to evoke strong reservations about the generalizability of Langer's finding, its interpretation as illusion of control, and about the adequacy of this finding as evidence for or against simulation.

2. *Experiment 1: Real Lottery*

Our main goal was to first replicate the findings of Nichols et al., with the view of later trying a zero-delay condition. To ensure success we attempted to make the task more realistic than Nichols et al. by using real commercial lottery tickets.

2.1 Task

Participants were given the opportunity to take part in a lottery. The lottery tickets were official tickets from a government lottery ('Brieflos'), which were

Table 1 Results of Experiments 1–4

	Choice condition				No-choice condition			
Experiments	N	Mean	Anchoring at price	Anchoring at jackpot	N	Mean	Anchoring at price	Anchoring at jackpot
1: Real lottery	10	38.2	3	variable	12	125, 105.8	3	variable
2: Fixed jackpot	10	141.0	0	4	13	166.9	1	9
3: Manipulation of anchor	12	143.3	1	1	17	131.2	0	5
4: Personal interaction	13	70.8	3	1	14	82.0	2	3
	45		7	6	56		6	17

Anchoring at price shows the number of participants stating the ticket price as resale value, anchoring at jackpot shows the number of participants stating the jackpot value as resale price.

bought by the experimenters and given to participants as reward for participating in a five-minute task. This lottery is currently run in Austria, with prizes between 10 Austrian Schillings (ATS) and ATS 1,000.000. On the cover, all these tickets look the same. The price of a ticket was ATS 10 (approx. $1.00). The filler task, which was presented to participants as the main task of the experiment, was a framing task, similar to the Asian disease problem (Tversky and Kahneman, 1981), which lasted 5 minutes. Participants were divided into two groups, one group could choose their ticket (choice group), the other was simply given a ticket (no-choice group). After having finished the framing task, participants were asked for how much they would sell their ticket back, on the pretext that the experimenter needed more participants for the experiment. The selling price was noted on the backside of their sheets. After having collected the sheets, participants were debriefed and opened their tickets to see whether they had won or not.

2.2 *Participants*

22 students from an introductory psychology course at the University of Salzburg volunteered for this experiment, 10 in the choice and 12 in the no-choice condition. There were 15 women and 7 men, with ages ranging from 19 to 30 years.

2.3 *Results*

The results for all four experiments are presented in Table 1. Inspection of Table 1 yields an unexpected picture for Experiment 1. The mean selling

price in the choice condition (ATS 38.2) was considerably lower than that in the no-choice condition (ATS 125, 105.8). This is noteworthy in two respects. First, the overall mean (about 68.000) is much higher (approx. 7.000 times the ticket price) than reported in the literature (4-5 times the ticket price). Second, the striking difference between groups indicated that something had gone wrong. Inspection of the data showed that in the no-choice condition, 3 participants had given very high selling prices (1.000, 500.000, 1,000.000). These may be judged unreasonably high but, on the other hand, they represent possible outcomes in this lottery. We compared the prices of the two groups using the nonparametric U-statistic and found that choice and no-choice conditions did not differ from each other ($U = 52.5$, n.s.), due to the vast variation within each group.

2.4 Discussion

With standard lottery tickets we could not replicate Langer's effect. This failure may be due to the fact, that in Langer's and in Nichols et al.'s experiments the jackpot amount was fixed, whereas in our experiment, it was variable. Perhaps the nature of the lottery is more crucial for obtaining the Langer effect than hitherto thought.

3. Experiment 2: Fixed Jackpot

In the second experiment we tested whether the use of a fixed jackpot is crucial for Langer's effect.

3.1 Task

The task was very similar to the first experiment. The only difference concerned the lottery tickets and the use of a fixed jackpot. In Experiment 2 we used tickets that were specifically made for the experiment. Each ticket was a folded fiche with a number printed inside, which was not visible from the outside. The jackpot of the lottery was ATS 200. This jackpot was fixed and the jackpot money was made visible to participants.

3.2 Participants

23 students from an introductory psychology course at the University of Salzburg volunteered for this experiment. 10 participants were in the choice and 13 in the no-choice condition. There were 16 women and 7 men, with ages ranging from 18 to 32 years.

3.3 Results

The means for the choice- and no-choice conditions are presented in Table 1. Statistical analysis showed that, again, groups did not differ from each

other ($t(21)=0.75$; $U = 44.0$, n.s.). Note again, that despite a fixed jackpot, resale prices are in both conditions considerably higher (approx. 15 times the ticket price) than reported in earlier experiments (4–5 times the ticket price).

3.4 Discussion

Experiment 2, like Experiment 1, did not succeed in replicating Langer's effect under the 5-minutes conditions used by Nichols et al.

Instead of Langer's effect we started to obtain a curious effect, further substantiated in the subsequent studies. A substantial number of participants stated as selling price the value of the jackpot, i.e. ATS 200 in Experiment 2. More of these jackpot values tended to occur in the no-choice than in the choice condition, resulting in a slight 'inverse Langer effect' (see Table 1). This can be seen as an anchor effect, which is a common finding in decision experiments (e.g. Tversky and Kahneman, 1974). In fact, Langer (1982) herself, in a redescription of her original experiment, reported that 3 participants asked for the jackpot value. Unfortunately, she did not report under which condition this value occurred. Such an anchor effect could be responsible for Langer's original effect. Since Langer actually sold her tickets it is conceivable that a number of participants in the no-choice condition anchored on the ticket price rather than on the jackpot. That would lead to lower averages in the no-choice than in the choice condition. In our experiment we found no group differences for anchoring at ticket prices (see Table 1), but ticket price may emerge as an anchor when participants have to pay real money for their tickets, as was the case in Langer's experiment (though not in Nichols et al.'s). In our experiment, participants did not have to pay, thus the anchor of the jackpot may have been more salient.

4. Experiment 3: Manipulation of Anchor

In our third experiment, we tested the possibility that we tend to get an inverse Langer effect because our participants anchored on the jackpot value, while Langer's procedure induced participants to anchor on the original price of the ticket.

4.1 Task

The task was practically identical to Experiment 2, except that we emphasized the price of the ticket to make it more salient compared to the jackpot. In contrast to Experiment 2 the jackpot money was now kept invisible, whereas the ticket price was made prominent by displaying an ATS 10 coin. The jackpot value was ATS 250, since we had more participants.

4.2 *Participants*

29 students from the University of Salzburg participated. There were 19 women and 10 men; ages ranged from 19–31 years.

4.3 *Results*

Again, we found no differences between the two groups ($t(27)=0.24$; $U=102.5$, n.s.). Inspection of the data showed, similar to Experiment 2, more jackpot values in the no-choice condition than in the choice condition. The intended manipulation of the anchor was not successful, as only 1 participant stated the ticket price as resale value. However, anchoring at the jackpot was less frequent than in experiment 2 ($\chi^2 = 7.10$, $p < 0.01$). Thus, while being unsuccessful in making the ticket price more salient, we were successful in making the jackpot value less salient. Yet, by and large, the overall means in Experiments 2 and 3 were very similar.

4.4 *Discussion*

Langer's effect again could not be replicated. Obviously we were not successful in manipulating the anchor, since again the no-choice condition showed more jackpot anchors, while anchoring on the ticket price occurred only once in each condition. For some reason, the ticket price does not appeal as an anchor as much as the jackpot value. Maybe the only way to get participants to anchor on the ticket price would be to make them pay the ticket price in cash. But since Nichols et al. obtained Langer's effect without this we did not pursue this possibility.

Another interesting difference between our and both Langer's and Nichols et al.'s results is that the resale values in our experiments were generally higher than in earlier work. This alerted us to the fact that in our experiments, participants stated their resale value anonymously, whereas in Langer's and in Nichols et al.'s experiments resale values had to be stated in personal interaction with the experimenter. It is easily conceivable that resale values would be lower when given in personal interaction with the experimenter rather than anonymously, since participants feel under more pressure of potentially having to justify their price. Moreover, when we had participants simulate in a pilot study, our simulator participants and those in Nichols et al.'s experiment made similar predictions, close to the actual resale values obtained in our experiments. This observation suggests that people in Nichols et al.'s study failed to predict Langer's effect because their simulation may have missed the critical point that participants had to stipulate the resale value of their ticket in personal interaction with the experimenter. They may have accurately simulated the condition where participants set the resale value anonymously on their work sheet.

5. *Experiment 4: Personal Interaction*

Our fourth experiment tested whether personal interaction is critical for obtaining Langer's effect. We predicted lower resale values with personal interaction compared to anonymous stipulation and were hoping that this personal interaction would also revive Langer's difference between the choice and no-choice conditions.

5.1 Task

This time participants were interviewed individually. Another filler task (judgment of probability) was used to see whether the filler task was influential. The jackpot was ATS 300. Moreover, special care was taken to ensure that participants believed that they were to sell their ticket. Resale values were taken personally, i.e. participants had to verbalize the resale value, which was noted by the experimenter, together with their names and addresses. This procedure was to emphasize the personal nature of the situation.

5.2 Participants

27 persons were approached individually and asked to participate. They were students and non-students of different professions. 14 were women and 13 were men; ages ranged from 18 to 53 years.

5.3 Results

Table 1 depicts the results. It is the fourth time that we failed to replicate the Langer effect: groups did not differ from each other ($t(25)=0.33$; $U=85$, n.s.). This time however, the resale values were much lower than in Experiments 2 and 3, even though the jackpot was higher ($U = 992$, which, due to the large sample size, can be approximated by a normal distribution yielding: $z = 2.81$, $p < 0.01$).

5.4 Discussion

Our expectation that personal interaction lowers the resale value was confirmed. Under this condition the overall means (7–8 times the ticket price) were now more similar to the results by Langer (5 times) and Nichols et al. (4 times). Nevertheless, despite this rapprochement in overall means, we still failed to obtain Langer's difference between conditions. We consider this a serious challenge to the generalizability of her results, and thus we hold that the use of this elusive effect by Nichols et al. to argue against simulation is problematic.

6. General Discussion

Four experiments were run in order to replicate the findings of Langer and Nichols et al. None of them was successful. Three significant points result from our experiments.

6.1 Replicability

Langer's effect is not easily replicated. Inspection of the literature shows that most references to the illusion of control in the risky choice paradigm relate to Langer's original work and that there are no replications of her Study 2 in the social psychology and decision theory literature. This is quite surprising, since the illusion of control is one of the more important instances of cognitive illusions (see Nisbett and Ross, 1980) mentioned in most textbooks. As noted by Dunn and Wilson (1990, p. 306): 'there has not been much research following up on the original illusion-of-control effect'. Our data also indicate that participants' responses do not cluster regularly around a mean subjective value of ticket prices. Rather, there seems to be a large individual variation in how participants approach the task, leading to a multimodal distribution of values: resale values cluster on zero, on the ticket price, on slightly more than the ticket price, and on the jackpot value. Nichols et al. do not give any details about the distribution of responses, but Langer (1982) herself, in a redescription of the original experiment, does report some cases of anchoring at the jackpot value. George Botterill (personal communication) ran a small pilot study and was also struck by the vast individual differences in responses. Great individual differences thus do not seem to be atypical. Therefore, reporting mean group differences may project a more reliable picture of the data than is warranted. Given the great variation in response patterns and the dearth of replications of Langer's effect, it is an open question whether the effect does actually exist. The two positive reports in the literature (Langer, 1975, and Nichols et al., 1996) may reflect a reporting bias of positive findings in a sea of unreported failures to replicate.

Perhaps this is too extreme a view to take and Langer's effect does exist. However, in that case our results suggest that it is highly volatile. It leads to statistically reliable findings only on occasion, and we do not yet know which combination of factors is responsible for it.

6.2 Critical Factors

It is not yet clear what contributes to the Langer effect, if anything. It is not unlikely that the effect results from basic processes of perception and judgment, triggered by surface characteristics of text problems. In decision-making research, for instance, it is generally found that seemingly minor changes in task and context lead to sizeable differences in judgment and decision, differences that are not warranted from a rational point of view (Payne, Bettman and Johnson, 1992).

One such process could be that the tendency to rely on anchors (which, by the way, is one of the most prominent judgmental heuristics; for examples see Tversky and Kahneman, 1974; Hogarth, 1981) is different for choice and no-choice conditions. If you have the possibility to choose, you may process information more thoroughly, you may be more involved, may feel more responsibility for your behaviour. Hence your thought processes are less susceptible to the surface structure of situations, which makes you less prone to look for anchors. This can explain why the no-choice condition is more apt to induce the jackpot as an anchor for reselling prices than the choice condition. This is in line with our findings (see Table 1): if we add all instances of presumed anchoring at the jackpot-value in Experiments 2, 3, and 4 (Experiment 1 has no clear jackpot value), we find a significant difference: 6 cases out of 35 in the choice, and 17 cases out of 44 in the no-choice condition ($\chi^2 = 4.36$, $p < 0.05$).

A second potential anchor is the price of the ticket. This anchor was nearly never used in our experiments. Maybe buying a ticket in cash would make this anchor more salient and more influential. If payment of the ticket's price is apt to enhance the saliency of the price sufficiently, the same process that produces something like our inverse Langer effect may have produced the effect in Langer's Study 2. Even though Nichols et al. obtained the Langer effect without selling tickets, we consider this a serious possibility.

The decision-making and economic literature provides a further finding, that may be critical for the Langer effect to occur. The disparity between buying and selling prices (Coursey, Hovis and Schulze, 1987; Irwin, 1994). Median selling prices are typically more than twice the median buying prices. Kahneman, Knetsch, and Thaler (1990) relate this to an *endowment effect*, which means that simply being endowed with a good gives it added value. This is directly related to Harris' (1992) argument that even five minutes of possession of a good may alter a person's evaluations. Most experiments on the endowment effect deal with endowments of short duration (some minutes) and find significant effects even for this relatively short duration.

From these considerations another possible explanation for the failure to get the Langer effect emerges. Langer had no filler task but people had to pay for their ticket and owned it for a week. Nichols et al. asked participants to judge the grammaticality of sentences. We had two different decision problems as filler tasks. Maybe the nature of this filler task is important. A difference between our tasks and Nichols et al.'s task is that our tasks seemed interesting to participants, whereas Nichols et al.'s grammaticality judgments were probably perceived as boring. This may change the subjective feelings toward the ticket: after an interesting task the ticket may be seen as a gift (for which one has no right to ask much money when asked to sell it), whereas after a boring task it may be considered a well-deserved payment. A feeling of having truly earned or paid for the ticket (in Nichols et al. and Langer) could interact with the conditions such that the choice condition enhances the personal value attached to the ticket.

6.3 Testing Simulation Theory

Our persistent failure to replicate Langer's effect raises questions about its suitability as a critical test to decide between simulation and theory theory. Maybe the Langer effect does not exist; in this case it is no wonder that simulators do not show it in their simulation. More likely, the effect is weak, depends on a subtle, yet unknown combination of factors, and is easily marred by great individual differences in approach to the task of deciding on a resale value.

With these features it is very difficult to ensure that simulator participants are provided with sufficient information about exactly the right combination of factors that produces the Langer effect. Nichols et al. did a very thorough job in trying to satisfy this requirement. They showed videos depicting a confederate participating in the entire procedure: choice (no-choice) of ticket, filler task, and stipulation of resale value. So, whichever combination of factors may be responsible for the Langer effect, Nichols et al.'s simulator participants ought to have had the relevant information.

Yet, even so, there may be problems. All we know is that *some*, perhaps just a few, participants must have had the correct combination of factors, so that a group difference between choice and no-choice emerged. Since not every target participant's situation may have contained the critical combination of factors, and since (due to the large individual differences) not every simulator participant is one who would show the effect even when confronted with the right combination of factors, it becomes very difficult to make sure that enough of the right simulators are shown the right combination of factors. To make sure this requirement is met simulator participants would have to be shown a *representative* sample of target situations. Nichols et al.'s procedure did certainly not achieve this. They showed all simulators the same situation of their confederate as target participant.

Nichols et al.'s otherwise thorough approach to providing simulators with the needed information may yet have contained another problematic feature. Their videos did not only show the situation of a target person (confederate)—as would be typically the case with verbal descriptions—but also showed the target person in that situation. This method may actually suppress the use of simulation and induce reliance on behavioural cues instead. To illustrate take an opponent chess player. What will be his next move? Simulation theory suggests that we make this prediction by figuring out what move we would make, e.g. move the king out of check. However, if we see our opponent's hand hovering over the bishop, we will predict that he might protect the king with his bishop. This is not pure simulation anymore but prediction on the basis of behavioural cues. Similarly in Nichols et al.'s videos direct behavioural cues from the confederate may have interfered with what simulator participants might have predicted purely on the basis of simulation. For instance, the confederate might have looked perky and in the mood of taking risks which pushed simulator subjects to predict that he may ask a lot for his ticket. Since the same behavioural cues were

shown to all simulator participants in both conditions (video tapes differed only in the first two minutes) the influence of these behavioural cues may have levelled any differences between conditions if based on simulation alone.

7. Conclusion

Although we were not able to find the critical factor responsible for Langer's effect, the mere fact that this factor is not known poses a problem for interpreting Nichols et al.'s results as evidence against simulation. The general methodological lesson is that for Nichols et al.'s project of testing simulation theory one needs to employ effects whose generalizability across situations has been shown, so that one can make sure that the critical variables can be made available to simulators. If, as it seems to be the case with Langer's effect, the target effect depends on an idiosyncratic combination of unknown variables, then failure to simulate it accurately can be accounted for by failure to show a representative sample of situations (so that enough do contain the combination of critical factors) to enough simulators who are people who would show the effect as target subjects. Although such a procedure is possible, it has not been used by Nichols et al. and it is unlikely to be useful for further research because of its sheer complexity and required number of participants.

So, to where should we turn from here to look for evidence to adjudicate between simulation theory and theory theory? One possibility is to pursue the research for the critical factors of the Langer effect. Should a time factor (e.g. long possession of the ticket) prove essential, then simulation theorists can argue that it need not be accurately simulable since time effects pose a problem for simulation (e.g. Harris, 1992). Should time not be essential, then Nichols et al.'s project can be resumed by making sure that the then established critical factors are prominently included in the video tapes shown to simulators (without having to ensure representative samples of situations shown). Alternatively one might use some of the effects suggested by our experiments that do not depend on time (e.g. the difference between resale values set in personal interaction and those set anonymously), establish that they can be reliably replicated under different conditions, and then check whether they can be accurately simulated or not.

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