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Evidence that judgments of learning are causally related to study choice

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Three experiments investigated whether study choice was directly related to judgments of learning (JOLs) by examining people's choices in cases in which JOLs were dissociated from recall. In Experiment 1, items were given either three repetitions or one repetition on Trial 1. Items given three repetitions received one on Trial 2, and those given one repetition received three on Trial 2—equating performance at the end of Trial 2, but yielding different immediate Trial 2 JOLs. Study choice followed the "illusory" JOLs. A delayed JOL condition in Experiment 2 did not show this JOL bias and neither did study choice. Finally, using a paradigm (Koriat & Bjork, 2005) in which similar JOLs are given to forward and backward associative pairs, despite much worse performance on the backward pairs, study choice again followed the mistaken JOLs. We concluded that JOLs—what people believe they know—directly influence people's study choices.

Many researchers have argued that metacognition is important because of its causal role—via people's study choices or their allocation of study time—in the control of self-determined learning. For example, T. O. Nelson and Dunlosky (1991) said, "The accuracy of JOLs (judgments of learning) is critical because if the JOLs are inaccurate, the allocation of subsequent study time will correspondingly be less than optimal" (p. 267). Benjamin, Bjork, and Schwartz (1998) put it succinctly: "Poor self-monitoring capacity necessarily entails poor selection and execution of relevant control processes: If you do not know what you do not know, you cannot rectify your ignorance" (p. 65). Despite these and many other admonitions (see Dunlosky & Hertzog, 1998; Koriat, 2002; Mazzoni & Cornoldi, 1993; Metcalfe, 2002; Pressley & Ghatala, 1990; Thiede, 1999; and see Koriat, Ma'ayan, & Nussinson, 2006, for discussion), there have been no direct tests of the hypothesis that metacognitions—and, in particular, JOLs—directly influence what people choose to study.

Is it conceivable that metacognitions do not affect control? According to Humphrey (1987), an organism can, in theory, respond adaptively and learn without even having metacognition. Most nonhuman animals conform to this template (though see Hampton, 2001, and Kornell, Son, & Terrace, 2007, for studies showing that monkeys can, under exceptional circumstances and with extensive training, exhibit limited metacognitive capabilities). Furthermore, as Bargh, Gollwitzer, Lee-Chai, Barndollar, and Trötschel (2001) demonstrated, people often do things without knowing why—apparently without the intervention of metacognition (see also Nisbett & Wilson, 1977). They may explain their behavior as though it were determined by their beliefs, but the explanations are liable to be ex post facto (Wegner, 2003). The question that we address here, then, is whether people use their metacognitions at all to control study behavior, or whether these judgments are simply epiphenomenal.

The inference made by many researchers, that JOLs determine study behavior, stems largely from a finding that people choose to study items that are judged to be more difficult (Dunlosky & Hertzog, 1997; T. O. Nelson, Dunlosky, Graf, & Narens, 1994; and see Son & Metcalfe, 2000, for a review), resulting in a negative correlation between JOLs and study time or choice. It has been suggested (T. O. Nelson et al., 1994) that this indicates metacognitive control and is an optimal strategy. However, as T. O. Nelson and Leonesio (1988) themselves demonstrated, large increases in allocated time to difficult items may sometimes have no recall benefit—the so-called *labor in vain* effect. A viable alternative strategy (Atkinson, 1972; Metcalfe & Kornell, 2005) is to choose items in one's own "region of proximal learning"—neither too easy nor too difficult. But doing so could result in positive, zero, or negative JOL to study-time correlations. There are now a number of studies showing conditions under which the negative correlation—once thought to be ubiquitous—breaks down (e.g., Metcalfe & Kornell, 2005; Son & Metcalfe, 2000; Thiede & Dunlosky, 1999). Thus, the correlational evidence—besides being merely correlational—is neither consistent nor theoretically justified.

What other evidence exists? Two studies have shown that people's performance is better when they are allowed to study their own choices rather than items they have not chosen (Kornell & Metcalfe, 2006; T. O. Nelson et al., 1994). However, the correlations between JOLs and choices were opposite in these two studies (negative and

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positive, respectively). Furthermore, neither experiment contained any direct evidence that it was the JOLs that were controlling the choices (whether efficacious or not).

What one might like to do is eliminate the JOLs entirely and determine whether doing so affects choice. Unfortunately, simply telling people not to make metacognitive judgments does not ensure that they will not make them covertly. And to date, no humans have been documented to lack metacognition (although in some patients it may be distorted). Thus, no experiments eliminating JOLs have been conducted.

A different strategy would be to manipulate the JOLs in an illusory way and determine what—if anything—happens to choice. We adopted this strategy in the present study. In the first two experiments, we capitalized on our own previous finding (Finn & Metcalfe, 2008) of illusorily biased JOLs. In our paradigm, item pairs were given either one repetition on Trial 1 and three repetitions on Trial 2 (1–3) or three repetitions on Trial 1 and one on Trial 2 (3–1). At the end of the second trial, the total number of repetitions was the same and recall performance was equated. However, the second-trial JOLs that were made immediately upon studying the pairs for the last time were systematically higher in the 3–1 condition than in the 1–3 condition. We have argued and presented data that this illusion occurs (see Finn & Metcalfe, 2007, 2008, and Dunlosky & Serra, 2006, for supporting evidence) because people make their second-trial immediate JOLs, in part, by remembering whether they got a particular item right or wrong on the previous test. If they got it right, then they give it a high JOL. If they got it wrong, then they give it a low JOL. Test performance on the first trial was higher in the 3–1 condition than in the 1–3 condition, of course, and the second-trial JOLs (incorrectly) reflected this difference. Our present question was whether people's choices would follow this mistaken belief about their knowledge. If people's choices were directly related to their metacognition, then they should choose to study more items in the low JOL condition than in the high JOL condition, despite equivalent recall.

In the second experiment, we also included a delayed JOL condition, which erased the above-described metacognitive illusion while keeping first-trial study and test the same. People based their delayed JOLs on an attempt to retrieve the target at the time of making the judgments (rather than on past test performance). People in the delayed JOL condition did better on the first test in the 3–1 condition, of course. However, their JOLs on Trial 2 did not reflect this difference. Thus, we could determine whether choice was made on the basis of this past test performance (a possibility that we needed to rule out as the cause of the choice in Experiment 1) or on the JOLs themselves.

Finally, in the third experiment, to counter the criticism that the linkage between JOLs and choice might only be found in the multitrial test procedure, we used a different paradigm in which illusory JOLs were found (Koriat & Bjork, 2005). Forward and backward directional pairs elicited similar JOLs but resulted in very different levels of recall. The JOLs in this paradigm rely on either perceived study fluency or the perceived association between the cue and target—an heuristic that appears to be insensitive to the direction of the association. Our hypothesis was that people's study choices, again, would follow their (incorrect) JOLs.

The JOLs themselves in these three experiments are generally agreed to be based on different heuristics: memory for past test, an attempt at retrieval, and an assessment of study fluency. We were thus also able to investigate whether people's choices were determined by their metacognitions, even when those metacognitive assessments themselves stemmed from different heuristics.

EXPERIMENT 1

Method

In all experiments, Columbia University and Barnard College students participated for course credit or cash and were treated in accordance with APA ethical guidelines. Experiment 1 was a 2 (trial: 1 or 2) $\times$ 2 (repetition condition: 1–3 or 3–1, indicating the number of repetitions on the first and second trials, respectively) within-participants design with 12 pairs of items, drawn from the Toronto Word Pool (Friendly, Franklin, Hoffman, & Rubin, 1982), per repetition condition.

Twenty-two participants were instructed that they would be learning a list of 24 word pairs (some of which presented more than once) and making JOLs—that is, judgments of their confidence on a scale from 0% to 100% that they would be able to recall the second word when given the first on the upcoming test when the computer requested such a judgment. At the beginning of Trial 2, participants were also told that after making each JOL, they would indicate whether they would like to study that pair again. During study, the pairs slated for three presentations were shown one at a time for 3 sec each in one random order; then, they were reshuffled and shown again in a different random order, before all pairs—both those in the three- and in the one-repetition condition for that trial—were randomized, shown for 3 sec each, and followed immediately by a request for a JOL that was self-paced. On Trial 2 only, there was a request for a self-paced choice of whether the participant wanted to restudy the pair. A cued recall test, but no restudy, occurred after each trial.

Results

Recall. Although there was an effect of repetition condition [$F(1,21) = 16.01$, $MS_e = .02$, $p = .001$, $ES = .43$], it was entirely due to Trial 1, which showed higher recall in the 3–1 condition (.56), than in the 1–3 condition (.30) [$t(21) = 7.50$, $p < .001$]. As is shown in Figure 1, by Trial 2, recall was equated [$M(1–3) = .72$; $M(3–1) = .69$, $t < 1$]. The interaction between condition and trials was significant [$F(1,21) = 39.44$, $MS_e = .01$, $p < .001$, $ES = .65$]. Recall improved from Trial 1 to Trial 2 [$F(1,21) = 307.39$, $MS_e = .01$, $p < .001$, $ES = .94$].

JOLs. As shown in Figure 1, the Trial 2 JOLs were higher in the 3–1 condition ($M = .78$) than in the 1–3 condition ($M = .70$) [$t(21) = 5.29$, $p < .001$], replicating our previous findings. On Trial 1, the JOLs were higher in the 3–1 condition ($M = .73$) than in the 1–3 condition ($M = .50$) [$t(21) = 6.06$, $p < .001$].

Study choice. Significantly more items were chosen for study at the end of the second trial in the 1–3 (.51) condition than in the 3–1 condition (.38) [$t(21) = 4.33$, $p < .001$], as shown in Figure 1. This is the main result of interest, and it provides evidence for the hypothesis that changes in JOLs, even without changes in recall, result in changes in choice.

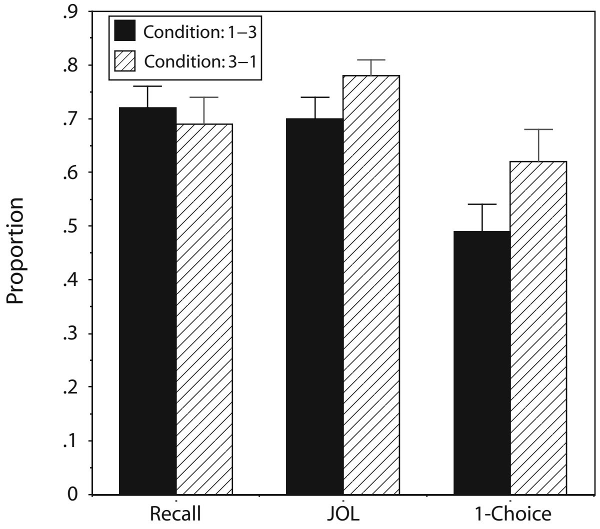

Figure 1. Recall, JOL, and 1-choice on Trial 2 in Experiment 1, for the condition given one repetition on the first trial and three repetitions on the second (1–3), and for the condition given three repetitions on the first trial and three repetitions on the second trial (3–1). Standard errors of the mean are shown. The ordinate plots the mean proportion correct and the mean proportional JOL ratings for the first two dependent variables. Choice data are plotted in this and subsequent figures as 1-choice, rather than as choice, with the ordinate again in proportions. We inverted the choice data here because we predicted that people would choose more items for restudy when their JOLs were lower rather than higher—that is, when they thought they had not yet learned the answer rather than when they thought they had already learned it. To analyze whether choices and JOLs went up and down together in this predicted manner, we needed to invert the scale.

Gammas. Table 1 shows mean gamma correlations that were computed for each participant, for all three experiments. These correlations are a nonparametric statistic, indicating predictive metacognitive accuracy (sometimes called *resolution*) of the JOLs with respect to recall. Gammas are also given between JOLs and choice. These data indicate that in all cases, people chose to restudy items that were given low JOLs preferentially to those given high JOLs.

EXPERIMENT 2

The results of Experiment 1 suggest that people were using their metacognitions to determine their study choices. However, it is possible that Trial 1 test results were responsible for the observed choice difference in Experiment 1. The JOLs in the 1–3 and 3–1 condition were different, but so too were the Trial 1 test results. To determine whether it was the JOLs or the past test results that were responsible for the choice difference, a method of eliciting JOLs in this situation that would not result in illusory JOLs was needed.

There is general agreement that delayed JOLs are mainly based on people's attempts to retrieve the target (rather than remember past test performance) at the time of judgment. Furthermore, Finn and Metcalfe (2007) showed that only immediate and not delayed JOLs show a second-trial bias. If the choices in Experiment 1 had been due to either the difference in the number of presentations in the first trial or the first-trial test-performance difference, but not to people's current metacognitive assessments of their JOLs, then the study choice difference was expected to persist even when the JOLs were delayed. However, if the choice differences were a function of the JOLs themselves, then we expected that choices favoring the 1–3 condition would be found only in the immediate JOL condition, in which the JOLs were expected to be different from those in the 3–1 condition, but not in the delayed JOL condition, in which the JOLs were expected to be the same in the two conditions.

Method

The experiment was similar to Experiment 1, including the 3–1 and 1–3 conditions, except that we added a between-participants variable—whether the judgments were made immediately or at a delay. A total of 64 cue–target pairs were used. Participants in the immediate condition followed the same procedure as that in Experiment 1. Participants in the delayed JOL condition made their JOLs after all of the to-be-remembered pairs had been presented. Then, all of the cues were rerandomized and presented individually with a request for the JOL. On Trial 2, in both conditions, study choices were requested following each JOL. We allowed people to study their chosen items in this experiment prior to Trial 2 recall, because Trial 2 recall, following the choice at issue, was not focal to the present hypothesis (and we wanted to avoid participants telling other participants that the choices in our experiments were never honored). Fifty people participated.

Results

Recall. Overall, the 3–1 items showed higher recall than did the 1–3 items, but this effect was qualified by a trial × repetition condition interaction [$F(1,48) = 114.36$,

Table 1
Gamma Correlations

Condition	JOL–Recall Trial 1 *M*	Trial 1 *SE*	Trial 2 *M*	Trial 2 *SE*	JOL–Study Choice *M*	*SE*
			Experiment 1			
1–3	.17	.16	.54*	.11	−.78*	.11
3–1	.14	.14	.59*	.12	−.97*	.02
			Experiment 2			
Immediate						
1–3	−.01	.17	.51*	.11	−.63*	.11
3–1	.50*	.07	.63*	.05	−.75*	.10
Delayed						
1–3	.87*	.06	.50*	.10	−.85*	.08
3–1	.84*	.06	.68*	.05	−.86*	.07
			Experiment 3			
Forward	.46*	.09			−.48*	.19
Backward	.13	.11			−.48*	.16

Note—The asterisk denotes a result significantly different from zero. In Experiment 1, only the increase in JOL–recall gammas from Trial 1 to Trial 2 in the 3–1 condition was significant ($p < .05$). In Experiment 2, in the immediate condition, the JOL–recall gamma difference between the 1–3 and 3–1 conditions on Trial 1 was significant ($p < .05$). There was a significant increase in the immediate condition in the JOL–recall gammas in the 1–3 condition from Trial 1 to Trial 2 ($p < .05$). In the delayed condition, there was a significant decrease in JOL–recall gammas from Trial 1 to Trial 2 for the 1–3 condition ($p < .05$). In Experiment 3, the JOL–recall gamma was significantly higher in the forward condition than in the backward condition ($p < .05$).

$MS_e = .01$, $p < .001$, $ES = .70$], showing that superior recall in the 3–1 condition was restricted to the first trial (.37 vs. .12 for the 3–1 and 1–3 conditions, respectively) [$t(49) = 13.66$, $p < .001$; see Figure 2]. There was no difference between the 3–1 and 1–3 conditions on Trial 2 recall ($t < 1$). Recall improved from Trial 1 (.25) to Trial 2 (.62) [$F(1,48) = 231.01$, $MS_e = .03$, $p < .001$, $ES = .83$]. There was no effect of JOL type (delayed or immediate) on any measure of recall (all Fs < 1).

JOLs. As shown in Figure 2, on Trial 2, there was an interaction between repetition condition and JOL type; JOLs were lower in the 1–3 condition than in the 3–1 condition with immediate JOLs (.49 vs. .56), but they were the same with delayed JOLs (.60 vs. .61) [$F(1,48) = 5.42$, $MS_e = .01$, $p = .02$, $ES = .10$]. Post hoc comparisons showed no difference for delayed JOLs ($t < 1$), whereas the immediate JOLs were significantly different [$t(21) = 3.75$, $p = .001$].

Study choice. The interaction between repetition condition (1–3 vs. 3–1) and JOL type (immediate or delayed) was significant [$F(1,48) = 5.34$, $MS_e = .01$, $p = .03$, $ES = .10$]. When people made immediate JOLs, more items were chosen for restudy in the 1–3 condition ($M = .42$), the condition in which JOLs were lower, than in the 3–1 condition ($M = .36$) [$t(21) = 2.38$, $p = .03$], the condition showing higher JOLs. There was no difference in choice (M: 1–3 = .43; 3–1 = .44) when JOLs were made at a delay ($t < 1$). See Figure 2.

Discussion

Experiments 1 and 2 indicated that when people's metacognitions were biased, as in the immediate JOL conditions, their choices followed the biased metacognitions. When JOLs were unbiased, because JOLs were delayed, people did not choose to study more in the 1–3 condition than in the 3–1 condition, despite similar repetition conditions in the first trial and similar differences on the first trial test performance.

EXPERIMENT 3

Insofar as it was possible that the first trial test might have influenced people's choices, at least in the immediate JOL condition, we thought that it was crucial not only to conduct the delayed JOL manipulation, in which the judgments are not attributable to people's memory for past test, but also to include a different paradigm that produces illusory JOLs, but in which there was no previous test. We also sought to investigate the relation between JOLs and choice in a situation in which the heuristics that people used to make their JOLs were different from either of the two different heuristics that were used in the previous experiments. Accordingly, in the third experiment, we used a metacognitive illusion that was discovered by Koriat and Bjork (2005). People making immediate first trial JOLs—judgments that are based on an assessment of encoding or study fluency—are metacognitively insensitive to associative directionality. For example, it is easier to recall *cat* when given *kitten* than it is to recall *kitten* when given *cat*. People can be told

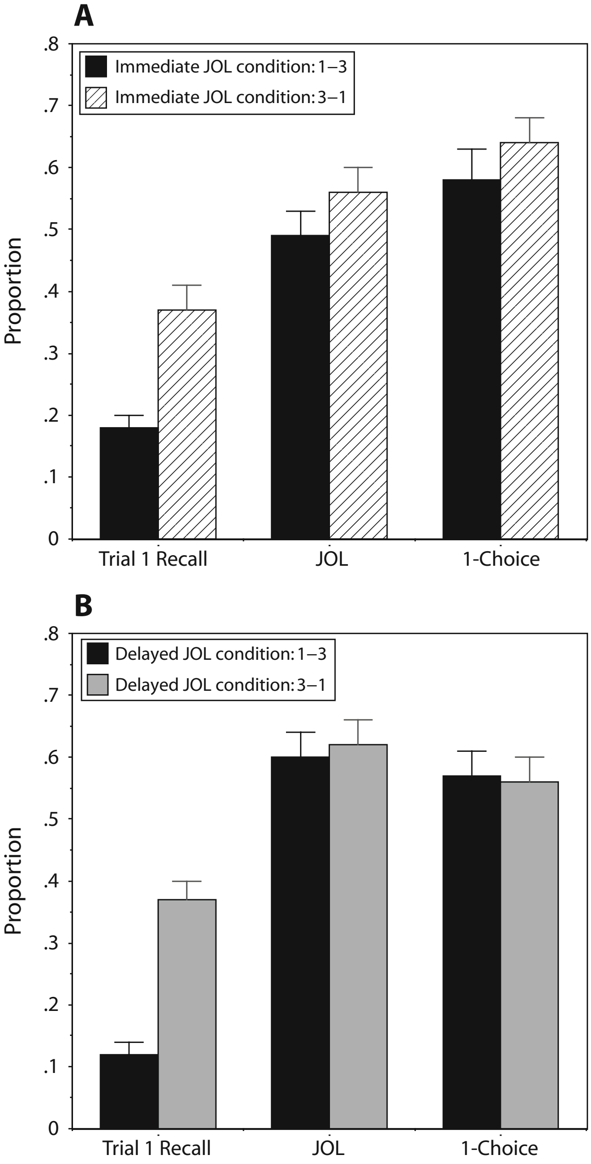

Figure 2. Trial 1 recall, Trial 2 JOL, and 1-choice for the 1–3 and 3–1 conditions, in Experiment 2. The ordinate plots the mean proportion correct and the mean proportional JOL ratings for the first two dependent variables. Choice data are plotted as 1-choice rather than as choice, with the ordinate again in proportions. Data for the immediate JOL condition are shown in panel A; data for the delayed JOL condition are given in panel B.

when given *cat–kitten* that the left item will later be the cue for recall. But even so, their JOLs are very similar to those for *kitten–cat*, regardless of the fact that the directionality of the associations has a large effect on subsequent recall.

We selected directional pairs that were based on the constraints detailed in the Koriat and Bjork (2005) experiments (which were in Hebrew). We also included unrelated pairs in the experiment so that people would not give uniformly high JOLs to all pairs. We were focally interested in the contrast of the forward and backward pairs. We expected to replicate the finding that JOLs were similar for the forward and backward pairs, but that recall would be much lower for the backward than for the forward pairs. The question of interest was what would happen with study choice. If study choice were determined by people's metacognitions, then we would find that the choices mirrored the metacognitions.

Method

The experiment was a 2 (direction of association: forward or backward) × 3 (measure: recall, JOL, study choice) within-participants design, with 24 pairs in each of the forward and backward conditions. Each list also included a control condition of 20 unrelated pairs. The word lists consisted of 68 cue–target pairs that were selected from D. L. Nelson, McEvoy, and Schreiber's (1998) word-association norms so that for the directional pairs, the Word 1–Word 2 association strength was between .45 and .85, whereas Word 2–Word 1 strength was 0. When pairs were presented in the order Word 1–Word 2, it was a forward association; if they were presented in the order Word 2–Word 1, then it was a backward association. Both forward and backward associational strengths for the unrelated word pairs were 0. Randomly intermixed forward, backward, and unrelated pairs were presented for study for 3 sec each. Immediately following the study of each pair, participants made a JOL and a decision about whether they wanted to restudy that pair. Following the list presentation, all of the pairs were reshuffled and the cues were presented (without restudy) for the final test. There were 24 participants.

Results

Two ANOVAs were conducted. The first, comparing recall and choice, revealed a main effect of association direction [$F(1,23) = 46.83$, $MS_e = .01$, $p < .001$, $ES = .67$], no main effect of measure ($F < 1$), and, crucially, a significant interaction between measure and association direction [$F(1,23) = 23.64$, $MS_e = .01$, $p < .001$, $ES = .51$]. A post hoc test showed that the difference between

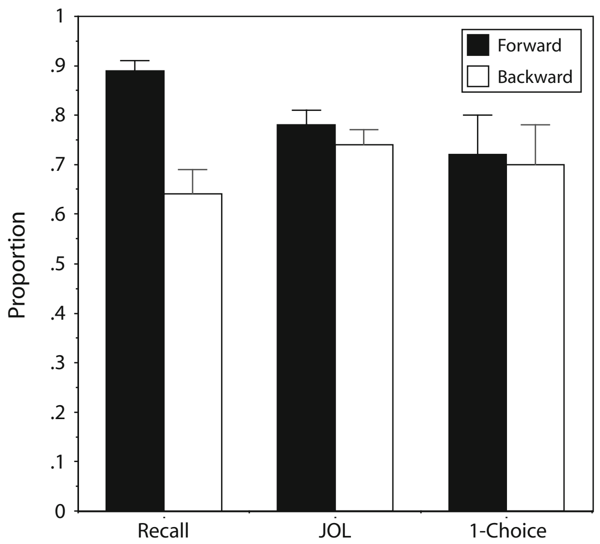

Figure 3. Recall, JOL, and 1-choice for forward and backward directional associates in Experiment 3. The ordinate plots the mean proportion correct and the mean proportional JOL ratings for the first two dependent variables. Choice data are plotted as 1-choice rather than as choice, with the ordinate again in proportions.

the forward and backward associations was significant only in the recall condition [$t(23) = 6.98$, $p < .001$] and not in the choice condition [$t(23) = 1.10$, $p > .05$].

By way of contrast, an ANOVA comparing JOLs and choice showed only a main effect of associative direction [$F(1,23) = 4.97$, $MS_e = .01$, $p = .04$, $ES = .18$]. There was no effect of JOLs as compared with choice ($F < 1$), and notably, there was no interaction between JOLs and choice ($F < 1$). In summary, people's choices followed the pattern of their JOLs and did not follow the pattern of their recall. These results are shown in Figure 3.

GENERAL DISCUSSION

These experiments showed that under conditions in which people's metacognitions were manipulated independently of their memory performance, their study choices were influenced not by their performance or by differences in their previous test performance, but rather by their JOLs—regardless of the heuristics that were used to generate those JOLs. Thus, these experiments provide converging evidence that people's metacognitions are directly used in determining what they choose to study. Given that the previous evidence that has been rallied to support the idea that JOLs are directly related to choice is open to criticism, this series of experiments provides crucial evidence that people's metacognitions are not epiphenomenal. These results also bolster the justification for studying people's metacognitions, not only because they are intrinsically interesting mental phenomena, but also because people act on them.

AUTHOR NOTE

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