In for a penny, in for a pound: Examining motivated memory through the lens of retrieved context models

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Running head: The effect of reward anticipation on free recall

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Abstract

When people encounter items that they believe will help them gain reward, they later remember them better than others. Our recently-proposed model of emotional memory, the emotional Context Maintenance and Retrieval model (eCMR), predicts that these effects would be stronger when stimuli that predict high and low reward can compete with each other both during encoding and retrieval. We tested this prediction in two experiments. Participants were promised £1 for remembering some pictures, but only a few pence for remembering others. Their recall of the content of the pictures they saw was tested after one minute and in Experiment 2, also after 24 hours. Memory at immediate test showed effects of list composition. Recall of stimuli that predicted high reward was greater than of stimuli that predicted lower reward, but only when high- and low-reward items were studied and recalled together, not when they were studied and recalled separately. These results, together with the dynamics of recall, confirm eCMR's predictions. More high-reward items in mixed lists were forgotten over a 24-hour retention interval compared to items studied in other conditions, but reward did not modulate the forgetting rate, a null effect which should be replicated in a larger sample.

Introduction

It is paramount to remember information that can help us attain our goals. Because memory resources are limited, it is adaptive for the brain to prioritise experiences that can be leveraged to maximise subjective utility over less-important experiences (Gershman & Daw, 2017). This intuition was expressed by Sherlock Holmes, Arthur Conan Doyle's timeless protagonist, when, justifying his ignorance of astronomy, he remarked that "It is of the highest importance… not to have useless facts elbowing out the useful ones". Holmes' intuition agrees with rational considerations, which suggests that people should allocate their limited resources to increase recall of the most valuable information (Anderson & Schooler, 1991). Yet how the opportunity to maximise utility influences the cognitive mechanisms that underlie successful recall has not been fully worked out.

The discussion of which memory resources are limited, and which are not, goes back decades (Bjork & Bjork, 1992). Classically, encoding resources were described as "heroic", and retrieval resources as "frail" (Bjork & Bjork, 1992). Despite the immense capacity of encoding, there is substantial evidence from classical and instrumental conditioning tasks that they can be biased towards subjectively-valuable information. For example, stimuli that predict reward attract preferential attention (e.g. Austin & Duka, 2010; reviewed in Le Pelley et al., 2016 and Mather, Clewett, Sakaki, & Harley, 2016). Since attention is one of the key drivers of subsequent memory, it is, perhaps, not surprising that in an experimental setting where participants anticipate that remembering some items earns more reward, operationalised through money or 'points', such items are rehearsed more frequently

(Stefanidi et al., 2018) and remembered better (e.g. Ariel & Castel, 2014; Lee, Greening, & Mather, 2015; Stefanidi, Ellis, & Brewer, 2018a; Wittmann et al., 2005). There is some debate about the degree to which this bias is under top-down control. On the one hand, there is strong evidence that reward can attract attention obligatorily, even against current goals (Bucker & Theeuwes, 2017). On the other hand, the recall advantage of high-reward information can be overcome when participants are instructed to allocate strategic, effortful resources to encoding all items, suggesting that reward recruits such strategies obligatorily (Hennessee et al., 2019).

A key point of contention is whether enhanced memory for high-value information is entirely dependent on increased encoding resources, or also benefits from modulated consolidation (Cohen et al., 2017; Lisman et al., 2011; Mather & Sutherland, 2011). The disagreement stems from the understanding that while effects at encoding enhance memory regardless of the timing of the test, memory facilitation is sometimes observed only in delayed tests (Montagrin et al., 2013; Murayama & Kitagami, 2014; Murayama & Kuhbandner, 2011; Spaniol et al., 2014; Wittmann et al., 2005). Additionally, while reward prediction error also enhances memory in immediate memory tests (e.g. Rouhani, Norman, & Niv, 2018), dopamine release triggered by reward-prediction and prediction-error signals is thought to underlie enhanced delayed memory through its influence on the consolidation of hippocampal memory traces (Adcock et al., 2006; Bunzeck et al., 2010; Lisman et al., 2011; Mason, Farrell, et al., 2017; Mason, Ludwig, et al., 2017; Patil et al., 2017a; Spaniol et al., 2014; Wittmann et al., 2005). Our reading of burgeoning literature on motivated memory suggests that the main interpretative framework concerns dopaminergic neuromodulation that allow memory to serve its adaptive

function of holding on to information that serves fitness goals. The bias reward exerts on the allocation of encoding resources clearly agrees with the broad rational analysis of memory, mentioned above. It is also reasonable to assume that fitness considerations ensured that the bias to encode high-value information is implemented through multiple routes. The effect of reward on the efficiency of memory consolidation suggest that the maintenance stage may also be usefully subjected to rational analysis. Yet while the computational and neural aspects of motivated memory have been discussed extensively, less is known about the cognitive mechanisms (Barsalou, 2017) that process signals associated with reward anticipation to support memory formation, maintenance and retrieval. Our main objective in this paper, therefore, is to investigate how reward anticipation influences memory-relevant cognitive mechanisms.

In this endeavour, it may be helpful to look to the emotional memory literature for clues. This is because the anticipation of reward and the response to reward feedback would trigger not only motivational circuits, but also an emotional response. To appreciate why, consider that theories of emotion define it as a response to signals that are appraised as goal-relevant (Lazarus, 1991; Sander et al., 2005). From this perspective, viewing a picture scene that depicts violent crime and viewing a stimulus that predicts reward are both signals that are relevant to our goals, albeit to different goals (to live in a safe society, to become wealthy, respectively). This claim notwithstanding, it should be acknowledged that the definition of emotion is controversial (Barrett, 2006; Izard, 2010), as is the boundary between emotion and motivation (Chiew & Braver, 2011). Crucially, given that reward anticipation is likely to give rise to an emotional response, and that like reward, emotionally-arousing stimuli also attract preferential attention (Pourtois et al., 2013), modulate

consolidation and enhance memory (Cahill & McGaugh, 1998), it is useful to consider whether they have similar influences on memory-relevant cognitive mechanisms.

The emotional Context Maintenance and Retrieval model (eCMR) is a variant of retrieved-context models, which was developed to account for the influence of emotion on free recall (Talmi et al., 2019). eCMR models emotion reductively through a single parameter, which was introduced to capture the preferential attention that participants typically allocate to emotional stimuli (Pourtois et al., 2013). In addition to their effect on attention, emotional and neutral stimuli are also thought to differ in the pattern of semantic associations among them. However, given that in reward experiments there is no reason to be concerned with particular structures of semantic associations, it is the influence on attention that will be key here. eCMR distinguishes between two aspects of the encoding context – the temporal and the source context (Polyn et al., 2009). The ‘temporal context’ is a core concept for retrieved-context theory (Howard & Kahana, 2002). The term does not refer to the passage of time per se (Hintzman, 2016), but rather to the sequential nature of encoding, whereby the encoded representation of each event reflects its place in the sequence of events. Thus, the temporal context of each item is a recency-weighted representation of previous items. By contrast, the ‘source context’ codes the attributes that an item shares with other items regardless of their place in the sequence. In an experiment, the source context refers to aspects of the encoding of the item which are not part of its identity. Stimuli such as emotional picture scenes trigger unique cognitive, affective and physiological changes (Bradley et al., 1992), and therefore, in emotional memory experiments, the source context of emotional items is emotional while that of neutral items is neutral. In eCMR the extra attention

that participants allocate to emotional items binds them more tightly to their source context. When memory is probed with a free recall test, the temporal context of the test allows participants to narrow their memory search to that of the most recent study set, thereby minimising intrusions (Lohnas et al., 2015). When items from the previous set compete with each other for recall, the one whose encoding context is most similar to that of the test context is the most likely to be recalled (Polyn et al., 2009), and its recall biases the test context, thereby influencing the recall likelihood of subsequent items. The tighter binding of emotional items to the emotional source context will enhance their competitive advantage during the test when they compete against neutral items (Talmi et al., 2019), while making it difficult to link them to a new context later on (Madan et al., 2012). This is how eCMR explains the robust findings that recall of distressing scenes and taboo words is better than recall of neutral scenes and words, as long as emotional and neutral stimuli are presented and recalled together, in ‘mixed’ lists (for review, see Ack-Baraly et al., 2016). eCMR also successfully predicts a less intuitive finding – that the recall advantage of emotional items is less pronounced and sometimes disappears altogether when emotional and neutral items are presented and recalled separately, in ‘pure’ lists (Barnacle et al., 2017; Hadley & MacKay, 2006; MacKay et al., 2004; Talmi et al., 2007). The joint pattern of recall of emotional and neutral stimuli in mixed and pure lists is called the emotional list composition effect. eCMR simulates the emotional list composition effect as a consequence of the interplay between encoding – where emotional items always capture attention, regardless of the composition of the list, and are therefore more tightly bound to their source context - and retrieval effects, where the increased association strength between emotional items and the source context helps them win the retrieval competition only against neutral items, but not

against equally strongly-bound emotional items. The reader is referred to previous publications for the formal definition of these ideas (Howard & Kahana, 2002; Lohnas et al., 2015; Polyn et al., 2009; Talmi et al., 2019).

Although we developed eCMR to simulate the effects of emotion, the model makes the same prediction for experiments that manipulate reward, for two reasons. First, like emotional items, high-reward items also attract preferential attention. Second, signals that certain items predict reward may also trigger unique internal operations, such as appraisal of goal-relevance, increased top-down effort, or mild positive affect. While these could vary across individuals, in each person they are likely to create a source context that differentiates the experiences of encoding high-reward and low-reward items. Crucially, the model makes the same prediction for a list-composition effect for reward just as it does for emotion. It is intuitive to think that items that predict “a pound” would always be recalled better than items that predict “a penny”. Indeed, theories that focus on the influence of reward on encoding predict that it will enhance memory across retention intervals; a rational analysis of memory would agrees, because it is manifestly adaptive to retain the most valuable information. Theories that focus on the influence of reward at the maintenance stage are silent about immediate effects, but predict that reward will enhance delayed recall. Counterintuitively, eCMR predicts that the promise for higher gain would only result in a recall advantage in the mixed list condition. In the pure list condition, by contrast, it predicts that participants who are encoding “for a penny” would recall just as well as those who are encoding “for a pound”.

Yet so far, eCMR has only been tested against results of experiments with negatively-valenced emotional materials. We are not aware of any previous studies of the list-composition effect with positively-valenced stimuli, nor with items that

predict reward explicitly and thus may also be perceived as positively-valenced. List-composition effects have been reported for other types of materials that attract special elaboration during encoding (McDaniel & Bugg, 2008), and for stimuli that are repeated multiple times within a study set (Ratcliff et al., 1990), but not for stimuli that are studied for longer durations, for mass-repeated items, and for deeply-encoded items (Malmberg & Shiffrin, 2005). Here we test the prediction of eCMR, that reward will give rise to the list composition effect, for the first time.

Figure 1

We tested this hypothesis by examining recall of picture stimuli whose later recall explicitly predicts high or low reward, and which were encoded in pure and mixed lists. In two experiments, our participants viewed lists of neutral pictures that were presented framed or unframed. They knew that they could gain larger monetary reward by recalling framed items on later test. The critical manipulation was the arrangement of framed and unframed items in the list, such that some lists included both item types (mixed) and some only one (pure). The frame provides a clear signal of which individual stimulus should be prioritised (Mather et al., 2016; Mather & Sutherland, 2011).

In Experiment 2, we additionally asked whether a similar pattern can be observed after a delay. We situate this question and introduce our hypothesis for the manipulation of the retention interval in the introduction to Experiment 2.

Experiment 1

Methods

Participants

In two previous studies, using a similar number of lists/items per list and emotional/neutral pictures (Barnacle et al., 2016, 2018), we found that the list-type-by-emotion interaction was large (the partial η^2 was 0.33 and .61, respectively; Richardson, 2011 defines a partial η^2>0.1379 as 'large'). Using Gpower (Faul et al., 2007), we calculated that *N*=23 participants were required to achieve 95% statistical power to detect an interaction with a large effect size (assuming a correlation between repeated measures of 0.2). Twenty-nine participants took part in Experiment 1 (28 women, 1 man, age range 18-21). This final sample size allows us to detect a medium effect size with >0.8 statistical power. All were students, who received course credits for participation. To be eligible participants were required to have normal or corrected-to-normal vision. Participants could take part if they were between 18-35 years old, but not if they had past or current neurological or psychiatric problem, were taking centrally-acting medications, or any non-prescription recreational drugs. The project gained ethical approval from the University of Manchester Research Ethics Committee.

Materials

Stimuli consisted of pictures retrieved from freely-available databases, including the Bank of Standardized Stimuli (Brodeur et al., 2010, 2014) and from Food-Pics (Blechert, Meule, Busch, & Ohla, 2014; scores for pictures drawn from the Food-pic

database were converted from a 1-100 scale to the BOSS 1-5 scale). 48 pictures of clothing and 48 pictures of stationery were used, for example: a green handbag, a tweed jacket, a high-heel black shoe with a buckle; a metal filing cabinet, a red paper folder. Pictures from these two semantic categories were matched for familiarity (clothing: M=4.30, SD=.32; stationery: M=4.39, SD=.35) and complexity (clothing: M=2.29, SD=.37; stationery: M=2.30, SD=.49) and adjusted in size to 280x210 pixels.

The experiment included 6 lists: 2 pure lists where all pictures were framed (pure lists, high reward), 2 pure lists where none of the pictures were framed (pure lists, low reward), and 2 mixed lists, where in each, half the pictures were framed and the other half unframed. Each list included 16 pictures, with 8 pictures from each semantic category. In the pure-list condition they were all either framed with a grey square, or unframed. In the mixed list condition, 4 pictures from each category were framed and 4 unframed, for a total of 8 framed and 8 unframed in the list. The assignment of pictures to conditions was randomised for every participant.

The experiment was implemented using Cogent200 on a MATLAB platform. In addition, the experiments used 9 printed sheets with 36 easy arithmetic subtraction and addition problems for the distractor task, with one sheet assigned randomly to each block.

Procedure

Participants were tested individually, in a quiet room, in the presence of a single experimenter. After providing informed consent, the experiment began with instructions and practice and ended with reward delivery. It comprised of 6 blocks, with one of the experimental lists assigned to each block. The allocation of

experimental condition to block, the allocation of picture lists to the picture task in each block, the order of pictures in each list, and the order of blocks were randomised for each participant. Each block included a picture task, a distractor task, and a free recall test. These are described below.

Picture task. In each block participants viewed 16 pictures which could all be framed, all unframed, or half framed and half unframed. The condition of each upcoming list (pure or mixed frames) was never declared to participants. Participants did not know in advance how many framed pictures they would see in any individual block. Each picture was individually presented at the centre of the screen on a white background. It was presented for 2s followed by a blank screen for 3.5, 4 or 4.5s (randomised ISI) before the next picture was presented.

Distractor task. Immediately following the picture task, the screen displayed the words ‘math task’ and participants were given 60s to complete as many arithmetic questions as possible on the sheet in front of them.

Free recall test. At the end of the distractor task a soft beep was sounded and the words “Free Recall” were displayed. Participants were given 3 minutes to describe in writing each of the pictures they remembered at any order. They were prompted to keep trying if they stopped before the time. A beep signalled the start and end of the memory task.

Instructions. Before the experiment began participants were given instructions about each of the tasks. For the picture task, they were asked to view the pictures and try to commit them to memory. Participants were told that they will earn £1 for recalling framed pictures, and 10 pence for recalling unframed pictures, but they were not given any information about the proportion of framed pictures. Thus, the

gain in the high-reward lists was 10 times higher than the gain in the low-reward lists. Participants were explicitly told that their goal was to maximise their monetary reward. For the Distractor task, they were informed that to qualify for reward they must (1) complete at least eight questions and (2) answer correctly two questions that are randomly selected for checking. For the free recall test, participants were asked to describe each of the pictures they could recall by writing three details about the picture, separated by a comma. They were shown two pictures and given the following examples: “A plant, spikey green leaves, in green pot” and “plastic elephant, purple, facing right”. What constituted a ‘detail’ was not defined further.

In previous studies of recall of complex picture scenes we did not mandate a particular number of details, but this requirement was introduced here after piloting, because of the nature of the stimuli we used, which were simpler and more inter-related than those we used in previous experiments. Indeed, some stimuli could be considered subordinates of the same basic-level exemplars (e.g. two types of shirt; Rosch, Mervis, Gray, Johnson, & Boyes-Braem, 1976), so that without this instruction, a participant might have responded with the single word “shirt” to refer to a number of list items (e.g. a folded, purple-checked shirt and a short-sleeve, blue T-shirt on a hanger). The requirement to report three details helped minimise responses that could not be confidently assigned to a particular picture. Yet we did not wish to be overly prescriptive with this instruction and therefore told participants that anything at all about the picture counted as a ‘detail’, including perceptual (the colour or orientation of an object), or semantic (an old-fashioned or an expensive-looking object). We assumed that most participants who really did recall the specific picture of the blue T-shirt would be able to describe it as a “blue (detail 1) T (detail 2) shirt (detail 3)” rather than just a “shirt”.

Practice. After the instructions were delivered, participants practiced each of the tasks using a set of 16 additional pictures (3 framed). During the practice block a lower reward rate was in effect (10 pence vs. 1 penny). At the end of the practice block participants were given feedback about their performance and paid any money they earned.

Reward delivery. At the end of the session, one block was selected randomly, and participants who qualified for reward, based on their performance on the distractor task, were paid according to their free recall performance (no participant failed to comply with the reward eligibility criteria on the distractor task). They were debriefed and thanked.

Results

Model simulations. Because our predictions are based directly on simulations using eCMR, we begin by using eCMR to simulate Experiment 1. The simulations reported here use the same formal structure described previously (Simulation 2, Talmi et al., 2019) with the main difference being that that instead of simulating a single ‘emotional’ category and a single ‘neutral’ category, the simulations follow the design of the current Experiment 1, where there are two semantic categories in each list. The middle panel of Figure 1 depicts the semantic associations in mixed lists in Experiment 1. As in Simulation 2 of Talmi et al., here semantic associations within category are higher than those between categories (we use the values 0.08 and 0.02 here, respectively).

The parameter used previously to model the increased attention to distressing emotional scenes was used here to model the increased attention to framed pictures that promised higher reward, where a value of $\varphi_{emot} = 1$ represents a null effect of reward on attention; all parameter values are reported in Tables 1 and 2. Simulation 1 used $\varphi_{emot} = 1.25$ (the value of this parameter in Simulation 2 of Talmi et al., 2019). Simulation 2 used a higher value of $\varphi_{emot} = 2.25$. We report both simulations in order to expose the effect that differential motivational investments might have on recall.

eCMR assumes that high-value increases the strength of the relevant association in the context-to-item association matrix; formally, φ_{emot} modulates $\boldsymbol{L}_{sw}^{CF}$. As such, eCMR implements a form of massed rehearsal. Another way to operationalise massed rehearsal is to assume that emotional items update the temporal context more than neutral items. This could be implemented by having φ_{emot} modulate $\boldsymbol{\beta}_{enc}^{temp}$ instead of $\boldsymbol{L}_{sw}^{CF}$. We simulated this model variant for the same two values of φ_{emot} in Simulations 1b and 2b.

The bottom panel of Figure 1 depicts the results of the simulations. Simulations 1a and 2a suggest that the composition of the list (pure vs. mixed) interacts with reward even when the list comprises of two semantic categories. Despite this change, the model still predicts that reward anticipation will produce a list composition effect. Increasing the potency of the reward manipulation only influenced the mixed list condition, where it increased the recall of high-value items and decreased the recall of low-value items. Simulations 1b and 2b did not capture the emotional list composition effect because the increased drift between items led to *decreased* recall of the in mixed lists in comparison to pure lists. When the effect of

emotion was made stronger, the increase in contextual drift between items did not decrease the recall of emotional items as much as that of neutral items.

Experimental results. As described above, picture recall was scored as ‘correct’ if participants recalled at least 3 correct details about a picture. Two raters coded the recall data independently. Inter-rater reliability was analysed using Cronbach’s alpha which showed high consistency between researchers results (α = .96). Discrepancies in their scoring and they were resolved through discussion. Given that here, participants knew they would only be scored as correct if they retrieved 3 details, some of them may not have even attempted recall if they only had a single detail. Therefore, a score of 0 for any recall could mean either no memory at all, or memory that is not strong enough. The proportion of correct recalls was calculated for each condition (out of 16 in the two pure list conditions, and out of 8 in the mixed list condition).

The statistical analysis of average recall follows exactly on the analytic approach we have used in previous work. That work led to the prediction that we would find an interaction between list type and reward. Therefore, the four comparisons that ensue – within lists (high vs. low reward, in pure and mixed lists separately) and across lists (high and low reward in mixed vs. pure lists) are all planned contrasts. To be conservative we corrected these for familywise error by using a Bonferroni-corrected p-value threshold of $p<.0125$.

Average recall across experimental conditions was just under 50%, the level expected based on computational models of recall (Naim et al., 2019). The average recall of mixed lists, collapsing across reward (M=41.27, SD=12.48) was numerically lower than the average recall of unrewarded pure lists (M=43.86, SD=13.79). The

average free recall data from each of the conditions are depicted in Figure 2. They were analysed with a 2 (list: pure, mixed) x 2 (reward: high, low) repeated-measures ANOVA. The effect of list type was not significant, but the interaction with reward was significant and large, $F(1,28)=20.09$, $p<.001$, $\eta_p^2 =.42$, qualifying the significant main effect of reward, $F(1,28)=10.71$, $p<.001$, $\eta_p^2 =.28$. These results reveal a reward-dependent list-composition effect. Planned paired t-tests showed that the effect of reward was only significant in the mixed list condition, $t(28)=4.38$, $p<.001$, Cohen's d = .81. There was no significant difference between recall of pure lists, $t<1$ or between memory for high-rewarded items in mixed and pure lists, $t(28)=1.69$, $p>.1$, Cohen's d =.38 Low-rewarded items were recalled less well in mixed lists, $t(28)=4.72$, $p<.001$, Cohen's d =.88.

Figure 3 depicts recall probability as a function of output position. If the first item that all participants recall is a framed item, then the probability of recalling a high reward item would be 1 and the probability of recalling a low reward item would be 0. The figure suggests that participants recalled high-rewarded items earlier than low-rewarded items. We analysed the data with a reward (high, low) x output position (16) repeated-measures ANOVA. The interaction between reward and output position did not reach significance, $F(15,855)=1.49$, $p=.10$.

Items which could not be confidently assigned to the preceding list were coded as intrusions (combining unstudied items and items from other lists). Three paired t-tests compared the average intrusions for pure low-reward and pure high-reward, and each pure list to the mixed list. No significant differences were found, $t<1$.

To further understand the dynamics of recall in the mixed list condition, we analysed contiguity effects in mixed lists using the temp_fact script of the

Computational Memory Lab Behavioural Toolbox (https://github.com/vucml/EMBAM). For each recall transition, the scoring considered the absolute lag of that transition against the distribution of absolute lags from not-yet-recalled items, assigning it a score from 0 to 1, where 1 reflects that the transition was the smallest absolute lag possible. Any recall attempt with fewer than 2 recalled items was not considered in this analysis. Category-based clustering scores were defined as the proportion of transitions made to the same semantic category (clothing or stationery) out of all transitions. Reward-based clustering scores were defined as the proportion of transitions made to the same reward category (high or low) out of all transitions. Thus, overall higher temporal, category-based or reward-based clustering scores reflect recall organisation with greater influence from temporal or reward-based associations, where 0.5 was the baseline level expected by chance. Recall clustered based on temporal contiguity, $t(28)=3.62$, $p=0.001$, but not based on reward, $t=1$, or semantic category, $t<1$.

Discussion

This is the first demonstration of a list composition effect using an explicit reward manipulation. They replicate the emotional list composition effect, this time with emotional value operationalised as prediction of high monetary reward, and confirm the prediction of eCMR (Talmi et al., 2019).

Testing memory with free recall is more straightforward when the stimuli are words, because then it is more obvious when participants did or did not recall a target. Scoring recall of picture stimuli is more open-ended. Regardless of the specific instructions participants receive, we cannot be sure that they have retrieved

more than a gist of the picture. The requirement, here, to provide three details about each picture in order for the response to be scored as correct is more stringent than usual, and removes instances where participants only have a very weak memory of item gist. It is unlikely that this instruction had a large impact on the results, because the instructions we used previously – to describe a picture with a short sentence in order to distinguish it from all others – also induced participants to provide a number of details. The benefit of the current instructions is that they are clearer to participants and thus decrease measurement error and inter-individual variance.

To ensure the robustness of these results, we decided to replicate the study. Experiment 2 provides a conceptual replication but also investigates another question of interest – whether the list-composition effect persists in delayed memory.

Experiment 2

Experiment 2 was conducted as a conceptual replication of Experiment 1. It used a very similar methodology in the test of immediate memory, but added a delayed memory test after 24 hours. We first explain the rationale for this delayed test, and then list the methodological difference that this addition introduced here compared to Experiment 1.

How emotional and motivational value changes memories across time is an important topic for research on the neurobiological basis of memory, where the focus is on catecholamines neuromodulation of the consolidation of hippocampal memory traces (Adcock et al., 2006; Lisman et al., 2011; McGaugh et al., 2000; Yamasaki & Takeuchi, 2017). In rodents, more intense experiences – such as more painful

electric shocks or a larger food rewards – trigger dopaminergic and noradrenergic effects that support the persistence of LTP as well as behavioural measures of memory (Yamasaki & Takeuchi, 2017). This literature is the main interpretative framework for demonstrations that in humans, also, effects of emotion (reviewed in Cahill & McGaugh, 1998; Craik & Blankenstein, 1975; Yonelinas & Ritchey, 2015) and reward (Montagrin et al., 2013; Murayama & Kitagami, 2014; Murayama & Kuhbandner, 2011; Spaniol et al., 2014; Wittmann et al., 2005) are stronger, or only significant, after a prolonged delay. For example, in a study that used a between-group manipulation, where some participants were promised a monetary reward for answering trivia questions, but others were not (Murayama & Kuhbandner, 2011), reward enhanced delayed, but not immediate, memory. Crucially for present purposes, if high-reward items persist while low-reward items are forgotten more quickly, then the list composition effect would be attenuated after a delay.

Yet it should be acknowledged that learning a large number of items is very different than the setup prevalent in the animal literature, where animals are typically exposed only to a single emotional experience. An influential theory, the synaptic tagging and capture hypothesis, suggests that neuromodulation triggered by emotionally-intense experiences would strengthen all active synapses (Redondo & Morris, 2011). Therefore, it is possible that in the list-composition task the neural effects of reward anticipation and the receipt of reward at the end of the session could slow the forgetting of all of the traces established during the session, rather than selectively slowing the forgetting rate of reward-predictive traces. This possibility is supported by experiments using post-encoding manipulations, where emotional experiences within 30 minutes of encoding slowed forgetting, whether induced by positive or negative film clips (Nielson & Powless, 2007) or monetary

reward (Nielson & Bryant, 2005). If this prediction is correct, we would not see any changes in the magnitude of the list-composition effect after a delay. Finally, it is possible that reward only influence the traces activated within a limited time window. In that case, we may observe a slower forgetting of mixed lists compared to pure low-reward lists.

Retrieved-context theories offer little guidance here. On the one hand, “offline” consolidation effects have been considered to be a challenge for retrieved-context models, which lack an operationalisation of consolidation (Davelaar et al., 2005; Usher et al., 2008). On the other hand, consolidation effects have been understood as consequences of interference, which that class of models simulate naturally (Kahana et al., 2008; Yonelinas et al., 2019). In our previous work we offered a range of methods to simulate forgetting in eCMR, but we did not have access to datasets that would allow us to decide which of them is most appropriate.

Despite decade-long interest in the effect of list composition on recall (Malmberg & Shiffrin, 2005; McDaniel & Bugg, 2008; Ratcliff et al., 1990), to our knowledge there has been no investigations of the effect after a prolonged retention interval. Part of the reason for the dearth of findings is that it is difficult to test free recall after a 24-hour delay, because memory for items that participants only view once is often very poor, risking floor effects. Consequently, most of the studies that investigate delayed memory probe memory with cued recall or recognition tests. But there is another challenge, specific to the list-composition task. Consider a participant who studies and recalls list A (pure list of items that predict high reward) and then studies and recalls list B (pure list of items that predict low reward) in the first session. In the delayed session, it would be difficult for them to recall list A without considering items from list B in some way (recall-to-reject, source memory monitoring). But any

consideration of both lists together would be detrimental for the list-composition paradigm, because instead of recalling only from a set of items that predicted high reward (A or B), participants would now recall from a set of items that predict either high or low reward (lists A and B). In effect, a within-subject design renders delayed recall of pure lists more comparable to the recall of mixed lists.

To summarise, while the influence of reward on forgetting is of intense research interest, the neurobiological literature leads to opposing predictions for the reward-based list-composition effects in delayed tests. Whether retrieved-context models can simulate delayed memory appropriately is a topic of some contention, which could be illuminated by data from the recall of pure and mixed lists, but such data is not presently available, and obtaining these data is empirically challenging. We included a delayed recall condition for all of these reasons, but treat the results as exploratory, and secondary to the main question of whether we can detect a reward-based list-composition effect, and whether it persists over time.

To overcome the aforementioned empirical challenge here we provided participants with cues that should help them, during the delayed test, to constrain their memory search to a single study list. We reasoned that participants may be confused if we ask them to recall the first, second, or third list, it is natural to address lists by their content. Therefore, in Experiment 2 each study list included items from a single semantic category, with the category label presented before the first item of each list. Participants were tested only on half of the lists immediately. During the delayed test they were given the label of each list in turn, e.g. they were asked to recall “the list of animals”. The added manipulation of time delay decreased the number of items participants could recall in each condition. In Experiment 1 there were 2 data points in each condition (2 pure lists in each reward condition - a total of

32 items per condition; and 2 mixed lists - a total of 16 items per condition). To avoid halving the number of items per condition and the concomitant potential increase in measurement error, we increased the number of lists from six to eight. This also allowed us to equate the number of items participants studied in each condition (now 16 in each cell). Finally, in order to decrease rehearsal during the retention interval, we introduced a cover story and a manipulation check. Other than these changes, the methodology of Experiment 2 resembled that of Experiment 1.

In summary, Experiment 2 provided a conceptual replication of Experiment 1 with small changes in methodology. Those allowed us to ask, for the first time, whether the list-composition effect will be observed after a delay. We expected that the findings will advance understanding of the selectivity of the influences of reward on forgetting, and add constraints to modelling forgetting in retrieved-context models.

Methods

Participants

Participants in Experiment 2 were recruited from the local community through advertising. They took part in two experimental sessions, and received £14 for their time and effort. We told participants explicitly that they can only qualify for reward if they do not make mistakes in the distractor task, and have excluded 2 participants who made more than 2 errors on average across all lists (they were statistical outliers, with error numbers that exceeded the inter-quartile range by more than 3 SDs). Of the remaining sample, 9 participants indicated they expected a memory test in session 2, and three of these 9 stated they have rehearsed the pictures in

preparation for this (see “Session 2”, below). These 3 participants were excluded from the analysis to avoid contaminating the forgetting rate data. The final sample included 22 women and 8 men who were 19-35 years old. Our sample size calculations were based on the same considerations as for Experiment 1. The final sample size allows us to detect a medium effect size with >0.8 statistical power.

Materials

Stimuli consisted of 16*9 pictures from 9 different categories: clothing, animals, cityscapes, food, household objects (non-kitchen), kitchen objects, landscapes, office items, and people. For example, a stream in an autumnal field; a pigeon on a low brick fence; a black swivel chair. They were drawn from the same sources as in Experiment 1, as well as the Nencki Affective Picture System (Marchewka et al., 2014), ImageNet (image-net.org), and internet sources. None had an overt emotional tone. While categories and pictures differed on multiple attributes, they were randomly assigned to conditions for each participant, eliminating the danger of systematic error. The Clothing list was always used in the practice block; otherwise, allocation of list to condition was randomised for each participant.

Each list included 16 pictures, which all belonged to a single semantic category. In the pure list condition all pictures were either framed or unframed; in the mixed list condition 8 pictures were framed and 8 unframed.

Procedure

Participants in Experiment 2 took part in two Sessions, 24 hours apart. The procedure of Session 1 was almost identical to the procedure of Experiment 1 other than the differences noted below. Session 2 included a delayed free recall test of each of the blocks encoded in Session 1.

Session 1 included eight blocks. Each block began with a screen stating the semantic category of the pictures in that block, e.g. “landscapes”, which participants could view as long as they wished. Four blocks included pure lists (half high-reward and half low-reward), and four included mixed lists. As in Experiment 1, the list condition (pure, mixed, high or low reward) was never declared to participants. Only half of the blocks in each condition ended with a free recall test.

Cover story in Session 1. Participants received the same instructions as participants in Experiment 1, and the same example pictures, but the details given with the second picture were slightly altered to: “plastic toy, elephant, purple” because the detail ”facing right” led to some participants using object orientation in their recall which were sometimes tricky to verify. Additional instructions were appended at the end. The purpose of the additional instructions was to equate the nature of encoding across blocks allocated to the immediate and delayed recall condition, and to decrease the likelihood of rehearsal between Session1 and Session 2. Participants were told that in some blocks, the immediate free recall test will be omitted. Furthermore, they were told that one of these blocks would be selected for a delayed free recall test at the end of Session 1. To maintain credibility, Session 1 always began with a bogus, additional practice block, which appeared to participants to be part of the experiment, and ended with a delayed free recall test of pictures from that block. The practice block always used the same Clothing list. In the delayed test at the end of Session 1 participants were reminded they saw a list of

“clothing” and asked to recall the items from that list. Data from this test were not analysed. The session ended with reward delivery as in Experiment 1. We hoped that the inclusion of a delayed memory test in Session 1 would discourage participants from expecting another one in Session 2. Finally, participants were told that Session 2 will include different tasks in Session 2.

Session 2. The session began with a delayed free recall test of each of the 8 blocks presented in Session 1. Participants were shown the title of the category of a list from one block, and given 4 minutes to recall the pictures from that block, before the next title was presented. 8 titles were presented in total. Note that 4 of the tests referred to lists that were never tested before. The other 4 tests referred to lists that were already tested in Session 1, immediately after the distractor task that followed list presentation. Thus, the delayed test in Session 2 was the first test of half of the blocks, but the second test of the other half. Participants were informed that they would be rewarded for their recall according to the same schedule and rate. When participants completed the experimental tasks, they were given an additional task of picture rating. Data from this task, which are available on bioRxiv, were collected to help control stimuli in future experiments, and are not reported here. In the picture rating task, participants were asked to rate the similarity between picture pairs on a 1-7 scale. For this rating they were asked to consider picture content, rather than visual similarity, and to respond quickly. Participants were shown each category in turn (the order of categories was counterbalanced) and, within each category, all possible pairs between the 16 pictures (the order of pairs was random). In all, participants rated 120 pairs in each category, a total of 1000 ratings. At the end of the session participants were asked whether they expected a memory test, and if

they answered in the affirmative, whether they made any notes or rehearsed pictures in their head. Then they were payed, debriefed and thanked.

Results

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Picture recall was scored by the experimenter as ‘correct’ if participants recalled at least 3 correct perceptual or semantic details about a picture. The proportion of correct recalls was calculated for each condition. Average immediate recall across experimental conditions was lower than that we observed in Experiment 1, potentially indicative of decreased motivation during encoding on part of this sample. Recall averaged around 26% in the immediate recall, 20% in the delayed recall of pictures already recalled in session 1, and only 9% of pictures that were only recalled for the first time in session 2. The average free recall data were analysed with a 2 (delay: immediate, delayed) x 2 (list: pure, mixed) x 2 (reward: high, low) repeated-measures ANOVA. This analysis only includes lists that were tested once – either immediately (session 1) or only after a delay (only tested in session 2), not lists that were tested in both sessions. The three-way interaction was significant and large, $F(1,29)=13.83$, $p<.001$, $\eta_p^2=.32$. Using the same analysis, but including only includes lists that were tested twice – both immediately (session 1) and after a delay (only tested in session 2), we found that as for the once-recalled data, the three-way interaction was again large and significant, $F(1,29)=5.13$, $p<.001$, $\eta_p^2=.15$. We unpacked these interactions to examine whether the list composition effect can be detected immediately and whether it persists after a delay.

Immediate recall. The first analysis was a list-by-reward repeated-measures ANOVA which focused on average recall in the immediate condition; these data are depicted in Figure 2. Its aim was to examine whether this experiment replicated Experiment 1. The effect of list type was not significant, F<1, but the interaction with reward was significant and large, $F(1,29)=30.05$, $p<.001$, $\eta_p^2=.51$, qualifying the significant main effect of reward, $F(1,29)=20.06$, $p<.001$, $\eta_p^2=.41$, which was also observed in Experiment 1. Planned paired t-tests showed that as in Experiment 1, the effect of reward was only significant in the mixed list condition, t(29)=6.60, $p<.001$, Cohen's d= 1.20, where participants again recalled high-rewarded items earlier than low-rewarded items (Figure 2). By contrast, there was no difference between the recall of the two pure lists, $t<1$. To increase sensitivity to effects that may take place at the beginning of the pure list, we analysed the recall of pure lists as a function of serial position (Figure 4), but found no significant effects (reward: $F<1$; serial position: $F(1,15)=1.43$, $p=.13$; interaction: $F<1$).

As in Experiment 1, low-rewarded items were recalled less well in mixed compared to pure lists, $t(29)=4.44$, $p<.001$. However, while in Experiment 1 high reward did not increase memory, here recall of high-rewarded items was better than their recall in pure lists $t(29) = 4.32$, $p<.001$. The average recall of mixed lists, collapsing across reward, $M=26.45$, SD=10.10, was equivalent to the average recall of unrewarded pure lists, $M=26.25$, SD=9.34, t<1.

Further analysis of the immediate recall in the mixed list condition, using the same methodology as in Experiment 1, showed that in this experiment, the interaction between reward and output position was significant, $F(15,1035)=13.90$, $p<.001$ $\eta_p^2 =.17$. Recall did not cluster based on temporal contiguity, $t<1$, but did cluster based on reward, $t(28)=2.08$, $p=.047$, such that participants had a greater-

than-chance propensity to recall high-rewarded (and low-rewarded) items together. Clustering based on reward predicted the recall advantage of high-reward over low-reward items, r=.74, p<.001.

Delayed recall – tested once. The second analysis was a list-by-reward repeated-measures ANOVA which focused on average recall in the delayed condition for pictures that were only recalled once (Figure 1 and 2). The list composition effect was less pronounced in this condition. Indeed, after a delay the interaction between list type and reward was small in size and not statistically significant, $F(1,25)=3.40$, $p=.08$, $\eta_p^2=.12$, while the effect of reward was significant and large, $F(1,25)=17.39$, $p<.001$, $\eta_p^2=.41$, and the effect of list was not significant, $F<1$. Nevertheless, planned paired t-tests showed that the effect of reward was only significant in the mixed list condition, $t(25)=3.54$, $p<.01$, Cohen's d = .69, with a moderate-to-large effect size, but the same trend was not significant in the pure list condition, $t(25)=1.22$, $p=0.23$, Cohen's d =.23, and had a small effect size. The average recall of mixed lists, collapsing across reward, M=9.61, SD=5.98, was equivalent to the average recall of unrewarded pure lists, M=7.93, SD=6.97, $t(25)=1.09$, $p=.29$.

Recall as a function of output position is depicted in Figure 3. As in the immediate condition, we analysed the data from the mixed list condition with a reward (high, low) x output position (16) repeated-measures ANOVA. Although this was the first recall of these lists, and although this test took place 24 hours after encoding, the interaction between reward and output position was, again, significant, $F(15,375)=8.76$, $p<.001$, $\eta_p^2=.26$.

Delayed recall – tested twice. The results of the delayed recall test in this condition mimicked the results of the immediate recall, showing that the list-composition effect persisted over 24 hours. The effect of list type was not significant, $F<1$, but the interaction with reward was significant and large, $F(1,29)=22.89$, $p<.001$, $\eta_p^2=.44$, qualifying the significant main effect of reward, $F(1,29)=14.07$, $p<.001$, $\eta_p^2=.33$.

Linear effect of forgetting. The significant three-way interaction between delay, reward and list type suggests that the number of items 'lost' over 24 hours differed among the 4 list types. We refer to this result as the influence of reward on linear forgetting. In order to understand this influence better, we unpack the three-way interaction with 4 separate delay-by-reward ANOVAs. The results of the analysis that included items that participants recalled once, either in session 1 or 2, and the analysis that included items that participants recalled twice, both immediately (session 1) and after a delay (only tested in session 2), yielded very similar results, and will therefore be reported together.

The analysis of pure lists found a very large, significant effect of delay, (once-recalled: $F(1,29)=130.07$, $p<.001$, $\eta p^2=.82$, twice-recalled: $F(1,29)=48.52$, $p<.001$, $\eta_p^2=.63$), but no other significant effects (all $F<1$). The analysis of mixed lists produced a different pattern. While the main effect of delay was again significant and large, (once-recalled: $F(1,29)=115.08$, $p<.001$, $\eta_p^2=.80$, twice-recalled: $F(1,29)=52.47$, $p<.001$, $\eta p^2=.64$), the main effect of reward was also significant, (once-recalled: $F(1,29)=48.69$, $p<.001$, $\eta_p^2=.63$, twice-recalled: $F(1,29)=46.28$, $p<.001$, $\eta_p^2=.61$), as was the interaction, (once-recalled: $F(1,29)=13.80$, $p<.001$, $\eta_p^2=.32$, twice-recalled: $F(1,29)=6.49$, $p=.016$, $\eta_p^2=.18$). In both cases, the significant

interaction stemmed from a greater loss of high-reward items than low-reward items in mixed lists (Figure 6).

Proportional forgetting rates. The number of items lost over time is a simple, intuitive metric, but it had been criticised (Loftus, 1985; Slamecka, 1985), and a variety of forgetting scores have been proposed (Wixted, 1990). Because here, immediate recall performance differed between conditions, it was imperative to examine whether reward modulated forgetting rates, taking immediate performance into account. Proportional forgetting scores were computed for each list-by-reward condition. This calculation was carried out both for lists that were tested once - immediately or after a delay, i.e. in session 1 or 2, but not both, and for lists that were tested twice, in both sessions 1 and 2. The calculation followed the formula:

(average immediate recall – average delayed recall)/

(average immediate recall + average delayed recall)

As in the d2 discrimination index in animals' novel-object recognition tests (Ennaceur & Delacour, 1988), the denominator is a sum of both immediate and delayed recall. The benefit of this computation here is that it allowed us to avoid excluding some participants who had not recalled anything in one of these conditions. However, it still necessitated excluding participants who did not recall anything both immediately and after a delay in one of the conditions. Forgetting scores were analysed with two separate list-by-reward repeated-measures ANOVA, for the once-recalled and twice-recalled data. In total, N=26 and N=24 were included in these analyses, respectively. The results of both analyses were very similar: none of the factors were significant, all F<1.

Discussion

Experiment 2 replicated the immediate list composition effect observed in Experiment 1. In Experiment 2, but not Experiment 1, the average recall of items that predicted high reward in mixed lists was higher than the average recall of pure lists. Simulations 1 and 2 show that one of the reasons for this difference could be increased subjective value of the reward in the sample used in Experiment 2. This interpretation is supported by the tendency to begin recall with high-reward items, and in significant clustering based on reward, both of which were significant in Experiment 2 but not in Experiment 1.

Average recall in the immediate test here was lower than in Experiment 1, which was around 50%. Indeed, we have often observed a 50% recall rate in experiments such as this, which align well with the expectations based on a computational model (Naim et al., 2019). The poorer memory in the immediate test could explain the even lower memory in the delayed tests. Unfortunately, the low level of recall in the delayed test meant that many participants recalled too few items to compute various clustering scores, which could have helped test some of the more subtle aspects of eCMR, and possibly differentiate between the 4 operationalisations of delay in Simulations 3-6. It also meant that the any conclusions from the delayed once-recalled data are vulnerable to floor effects. However, conclusions from the once-recalled data are supported by the twice-recalled data, where floor effects are not a concern.

The list composition effect was less pronounced after a delay, an effect that, at face value, aligns best with the hypothesis that reward attenuates forgetting.

However, more high-reward items than low-reward items were lost in the mixed-list condition, and there was no difference between the number of items lost in the high- and the low-reward pure lists. Moreover, neither reward, nor the type of list, modulated the proportional forgetting rate. The null linear and proportional effects were the same both when participants recalled lists for the first time in Session 2, and when they recalled lists they have already recalled once in Session 1. In interpreting the null effect of reward on the forgetting rate, we note that our sample size was powered to detect large effect sizes with 0.95 power (the final sample size allows us to detect a medium effect size with >.8 statistical power).

General discussion

In two experiments, participants recalled significantly more high-value items than low-value items only when they were studied and recalled together, in a mixed list, but not when they were studied and recalled separately, in pure lists. The reward-based list-composition effect aligns with a core prediction of eCMR, depicted in simulations reported here and by Talmi et al. (2019).

The immediate mixed-list results, where the effect of reward was large, are not new. They corroborate many previous reports of enhanced immediate free recall advantage for reward-predicting items (e.g. Ariel & Castel, 2014; Stefanidi et al., 2018). In eCMR this effect is due to the increased association strengths between high-reward items and their source context, which boosts their likelihood of being recalled when they compete against low-reward items. eCMR also uniquely predicts that these items will have a higher probability of being the first to be recalled. There is evidence for this prediction in experiments with emotional stimuli as well as with

reward-predictive stimuli (Stefanidi et al., 2018; Talmi et al., 2007). The two experiments reported here provide additional evidence for this prediction, and Experiment 2 shows that it extends to delayed recall, a finding that decreases the likelihood that preferential early recall of high-reward items is a result of rehearsal during the distractor task before the immediate test. eCMR also uniquely predicts that each high-reward item that is recalled will help promote the recall of additional items that share its source context (and similarly for low-reward items). Again, there is evidence for this in experiment with emotional stimuli (Long et al., 2015; Siddiqui & Unsworth, 2011; Talmi et al., 2007). Experiment 2 confirmed this prediction in the significant clustering of recall that was based on the reward category, although these were not significant in Experiment 1. While there are other frameworks that can account for the increased recall of high-reward items, some of which we review below, the ability of eCMR to make novel predictions, and the confirmatory evidence we provide here, help support to this model more broadly.

The immediate pure-list results, where reward did not increase free recall, replicate previous experiments where reward, manipulated between subjects, failed to increase immediate free recall, cued recall or source memory (Murayama & Kuhbandner, 2011; Ngaosuvan & Mantila, 2005; Nilsson, 1987). Our findings suggest that those failures may be due to the between-subject nature of the manipulation of reward in those experiments, which is similar to our pure-list condition. The list-composition paradigm, which was here used with a reward manipulation for the first time, is superior to between-subject designs because it allows us to document a large, replicable influence of reward anticipation on immediate memory in mixed lists in the same participants. The entire pattern therefore extends the results of Ngaosuvan and Mantila and of Nilsson, where the

evidence for the success of the manipulation of reward was only documented in subjective ratings of motivation. This significant interaction was replicated in both of the present experiments, and in Experiment 2 also in the twice-recalled delayed data, another advantage over a replication of null effects in between-subject designs.

Like Murayama and Kuhbandner (2011), we also found increased recall of pure high-reward lists after a delay, although here the effect was not statistically significant. Reward increased the number of items lost after a delay, but had no effect on the forgetting rate. There are other reports where either emotional value or reward did not modulate the forgetting rate (Koster et al., 2015; Murayama & Kitagami, 2014), and evidence that most cognitive manipulations that increase immediate memory do not attenuate proportional forgetting rates (Loftus, 1985; Slamecka, 1985), but no conclusive meta-analysis. Murayama and Kuhbandner had double our sample size (*N*=45) and report the size of the delayed effect to be small. Our more modest sample size in Experiment 2, due to the exclusion of participants who suspected a memory test, would not have allowed us to detect a small effect, and the delayed once-recalled data of these remaining participants was low, risking floor effects. Although the delayed twice-recalled data – those that depict memory for pictures tested both immediately and after a delay – replicates both the linear and the proportional effect of forgetting in the once-recalled data, we refrain from interpreting the null effect of reward on forgetting in Experiment 2. Tentatively, we conclude that reward does not have a large effect on forgetting in the list-composition task.

An interesting aspect of our data is the comparison between the average recall in mixed lists and in what could be considered a ‘baseline’ condition, the low-reward, pure-list condition. In agreement with previous findings (Stefanidi et al., 2018), the

presence of reward in mixed lists, while serving to prioritise memory for high-value items, did not increase memory for the mixed list overall, compared to baseline. This is because increased memory for high-value items was accompanied by decreased memory for low-value items in both experiments. This result has implications for the rational analysis of memory. Taken simplistically, it is always rational for an individual to recall information that has high value for them than any other information. Yet a rational agent should not slavishly recall everything that was important in the past, but rather, adapt their retrieval to present goals. Our results suggest that the goal to maximise reward may not increase retrieval capacity, but rather bias retrieval to best serve the agent's current priorities. Retrieved-context models implement the goal to maximise reward by using the temporal context of the recall test to narrow the focus of the memory search to the most-recent list. With this narrow focus of the memory search, high-reward items stand out against low-reward items from the same mixed list, but have no particular advantage against other high-reward items. Similar ideas about the importance of the relative, rather than the absolute, distinctiveness of emotional items during memory retrieval have been reviewed authoritatively by Schmidt (Schmidt, 1991), and resemble findings from choice experiments using pain and monetary reward (Nieuwenhuis et al., 2005; Vlaev et al., 2009, 2011). These ideas could be further tested in settings where the reward landscape changes prior to retrieval.

The remarkable similarity between the list-composition effects obtained with emotional pictures in our previous work (summarised in Table 1 in Talmi et al., 2018) and high-reward items here is compatible with the suggestion that they impact upon the same cognitive mechanism, through prioritisation of goal-relevant stimuli for resource allocation (Mather et al., 2015; Mather & Sutherland, 2011) that then has

varied influences on memory as a function of the context of recall. This is also reminiscent of the recent finding that unsigned prediction error, possibly reflecting the initial spike of dopamine released when animals experience stimuli with either appetitive or aversive motivational salience (Schultz, 2016), drives memory enhancement (Kalbe & Schwabe, 2019). Yet despite the impressive performance of the eCMR model to date, we suspect that future work will reveal differences between the impact of the prospect of instrumental positive reinforcement on cognition (current manipulation) and the impact of other manipulations of emotional value, which will necessitate revisions of the model in future. For example, there are indications that recognition memory is affected differently by the influence of monetary reward compared to pain (Dunsmoor et al., 2015; Murty et al., 2016; Patil et al., 2017b), and by actions taken to gain reward, compared to avoidance of action for the same purpose (Koster et al., 2015). Even within the domain of positive instrumental reinforcement, where participants act to gain reward that depends on their performance in recall tests, there is already evidence for a dissociation between actually winning and the mere possibility of winning (Mather & Schoeke, 2011). It is thus important to acknowledge that even if the same model parameter is discovered to provide the best account of the impact of reward anticipation and that of negative, intense emotion, this does not imply equivalence either at the level of cognitive mechanism or at the level of neural implementation.

A caveat of the work reported here is that we did not monitor how much attention was allocated to each item during encoding. Although we hypothesize that framed items will attract more attention than unframed items regardless of the composition of the list, and this assumption is reflected in the simulations, we did not collect evidence for this hypothesis here. Our data allows us to test one hypothesis – that

reward prediction error and attention were greater at the beginning of the high-reward pure list, compared to later in the list, conferring a special memory advantage on the early serial positions. This hypothesis was not confirmed: when we examined recall as a function of the serial position of the item in pure lists, there was no evidence for increased memory for early list items in the high-reward condition. Another possibility is that participants devoted maximal processing resources to framed items but not to unframed items in the mixed list, but in the low-reward pure-list condition they devoted maximal resources to unframed items because they had no reason to do anything else, increasing memory in that condition. If correct, this implies that the value of reward is extremely sensitive to its local list context (Nieuwenhuis et al., 2005). Further research would be needed to test this alternative hypothesis by examining attention to high- and low-reward items during encoding. Finally, recognition performance could be used to test eCMR's prediction that high-value items are attended well regardless of the list composition. Allocation of increased encoding resources to high-value items during encoding could increase their processing fluency in an item recognition memory test, and result in increased recognition accuracy if participants then attribute increased fluency to memory (Westerman et al., 2002). Increased recognition accuracy of high-value items regardless of list composition, together with list-composition effects, would align with eCMR's predictions. Although such a study has not been conducted with a reward manipulation, the list composition effect triggered by other manipulations if often abolished in recognition tests (Bugg & McDaniel, 2008).

Because list-composition effects fall so naturally from retrieved-context theories, and because there are so many list composition effects (McDaniel & Bugg, 2008; Ratcliff et al., 1990), it may be useful to expand on the theoretical contribution, as

well as the limitations, of the present study. First, it is important to note that eCMR is the first retrieved-context model variant that can account for the list composition effect, because it is the first to include a parameter that differentiated some of the items (here, $\emptyset_{emot}$ modulated item-context associations for high-reward compared to low-reward items). Second, it may be useful to contrast eCMR to SAM (Raaijmakers & Shiffrin, 1981). The “one shot” variant of SAM can account for list composition effects that stem from strengthening some items through spaced repetition, but does not predict a list-composition effects that depend on increased study time, massed repetition, or deeper levels of processing (Malmberg & Shiffrin, 2005). Indeed, Malmberg and Shiffrin reported that such operations did not result in list-composition effects. Yet rewarded items, which are known to capture processing resources during encoding (Hennessee et al., 2019), emotional items, which also capture encoding resources (Hadley & MacKay, 2006; Talmi et al., 2007; Talmi & McGarry, 2012), and experimental manipulations that increase item elaboration during encoding (McDaniel & Bugg, 2008), all give rise to list-composition effects even without overt repetition during study. Taken together, it may be the case that either spaced repetition or intra-item elaboration could give rise to two “flavour” of the list-composition effect, which SAM and eCMR account for, respectively. Alternatively, it is possible that manipulations that increase intra-item elaboration also induce covert spaced rehearsal, if participants choose to rehearse elaborated items throughout list presentation. Future research can test this fascinating possibility directly, either using neuroimaging to quantify attention to items during their encoding and test the extent to which it predicts subsequent memory, using multivariate approaches to detect post-item rehearsal, or using overt rehearsal protocols (Stefanidi et al., 2018; Ward & Tan, 2004). Rehearsal patterns could vary between participants, with massed vs.

spaced rehearsal then leading to different predictions. Each of these forms of rehearsal can then also be modelled in a number of competing ways, and contrasted against the empirical memory data. eCMR implements a form of massed rehearsal, through increasing the context-to-item associations, that captures the list-composition effect better than one other form that increases the rate of context update.

In summary, our experiments show that in some situations, people recall just as much for a penny as they do for a pound. We found that memory for reward-predicting information was not any better than memory for any other information - unless the two competed during recall. eCMR predicted this pattern, and explained it as the result of the interplay between encoding and retrieval. eCMR also predicted aspects of the dynamics of recall, including the modulation of output order by reward and clustering based on reward. If eCMR captures the working of participants' minds, Sherlock Holmes may have been wrong to attribute enhanced memory for useful facts to what "the skilful workman…takes into his brain-attic" – namely, only to biased encoding. Given that retrieval capacity is more limited than storage capacity, the genius detective may have benefited also from an expert ability to select the most appropriate cues to narrow the search of his brain-attic, so that the most important facts would come readily to mind.

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TABLE 1. Fixed parameter values in simulations.

Note. Superscripts to β: temp= temporal context, emot=emotional context. Subscripts to β: enc = encoding. dist = distractor.

TABLE 2. Variable parameter values in Simulations 1-5.

Parameter	*Talmi et al. 2019*	*1*	*2*	*3*	*4*	*5*	*6*
ϕ_{emot}	*1.25*	*1.25*	*2.25*	*2.25*	*2.25*	*2.25*	*2.25*
β_{rec}^{temp}	*.51*	*.51*	*.51*	*.51*	*.05*	*.05*	*.05*
β_{rec}^{emot}	*.588*	*.588*	*.588*	*.588*	*.588*	*.05*	*.588*
L_{tw}^{CF}	*.3*	*.3*	*.3*	*.3*	*.3*	*.3*	*.1*
L_{wt}^{FC}	*.898*	*.898*	*.898*	*.898*	*.898*	*.898*	*.1*

Note. Superscripts to L: CF = context to item; FC: item to context; Subscripts to L: tw = temporal context to item; wt = item to temporal context. emot = emotional, rec = recall, dist = distractor.

Figure captions

Figure 1. The list-composition task.

Top. Schematic of the list composition task. The list composition task comprises of 2 types of lists: pure lists with items of only one type, and mixed lists of items of two types. Traditionally, one type of items is ‘stronger’ in some way. Here the two item types were framed pictures that predicted £1 if recalled and unframed pictures that predicted £0.10 if recalled. These are depicted as squares with thicker and thinner frames in the figure. At the end of list presentation participants recall all of the items they studied in any order. Here, a brief distractor task intervened between encoding the recall.

Middle. Graphical representation of the hypothetical strength of semantic associations between stimuli in the mixed list conditions in the experiments reported here, compared to typical such experiment, e.g. Talmi et al., 2007, which we have previously simulated using eCMR. Each matrix represents a hypothetical list in one of the experiments. Each row/column represents a single item in the list. Darker shades of grey represent stronger semantic associations. In the leftmost matrix the first half of the stimuli are negative, arousing picture scenes and the second half, neutral domestic scenes. In the middle matrix, the first quarter of the stimuli are the framed clothing pictures, followed by framed stationery pictures, unframed clothing, and unframed stationery.

Bottom. Results of two simulations of Experiment 1 using eCMR.

Figure 2. Average free recall as a function of reward and list composition. High and low reward are presented in dark and light grey, respectively. Error bars correspond to the standard error.

A: Immediate memory test in Experiments 1.

B: Immediate memory test in Experiment 2.

C: Delayed memory test in Experiment 2 for lists that were only tested once.

D: Delayed memory test in Experiment 2 for lists that were only tested twice.

Figure 3. Recall probability as a function of output order. The shaded area represents standard errors.

Top: Experiment 1

Middle: Experiment 2, immediate recall

Bottom: Experiment 2, delayed recall

Figure 4. Recall probability as a function of output order. The shaded area represents standard errors.

Left. Immediate recall of pure lists in Experiment 2.

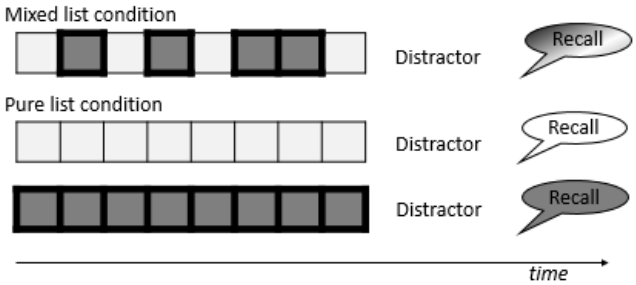
Right. Immediate recall of mixed lists in Experiment 2.

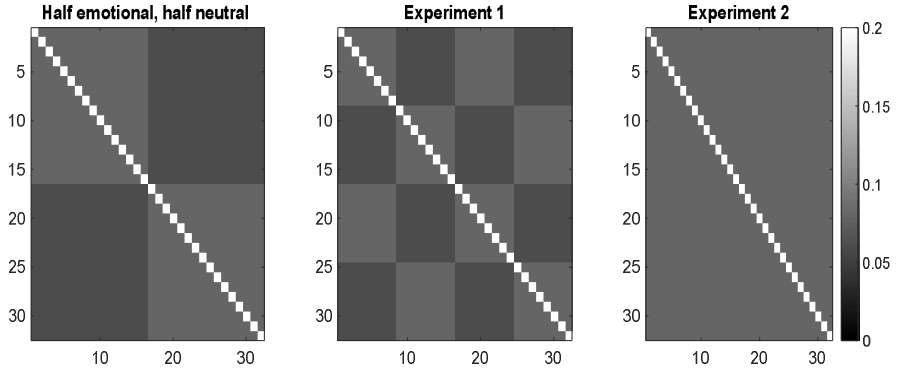
Figure 5. Results of four simulations of Experiment 2 using eCMR. The leftmost simulation models immediate recall. The nested simulations to the right add additional facets of delayed memory, in order: decreased ability to retrieve the temporal context, decreased ability to retrieve the source context, and a decreased weighting of the temporal context.

Figure 6. Proportional forgetting scores of high and low-reward items over 24 hours. Separate forgetting scores are depicted for items only tested once, after 24 hours, or twice, both immediately and after a delay. Error bars represent the standard error.

FIGURES

Figure 1.





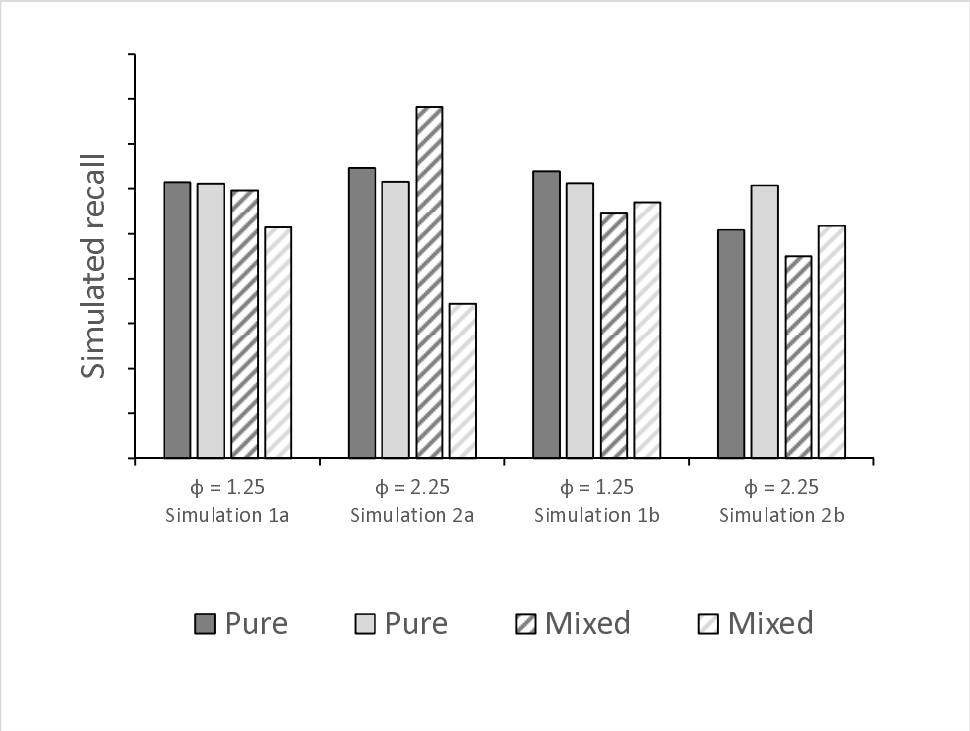
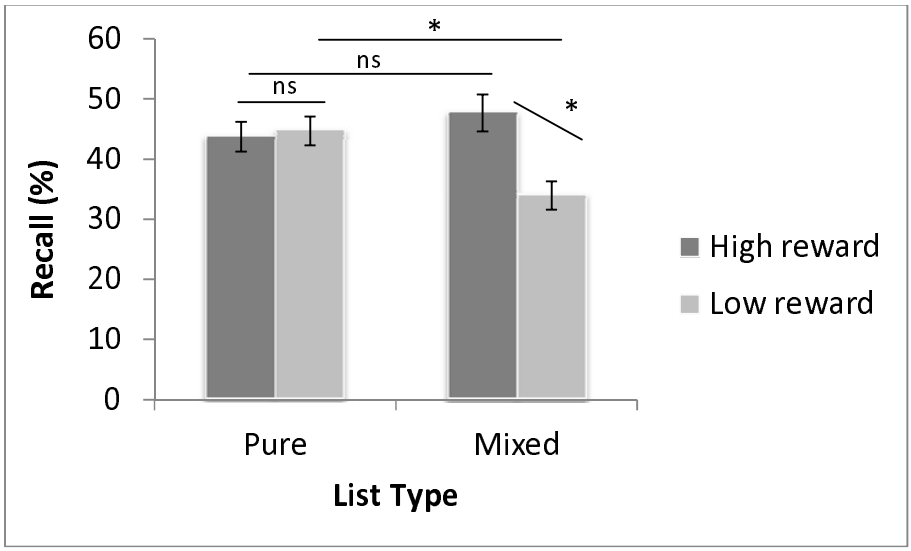
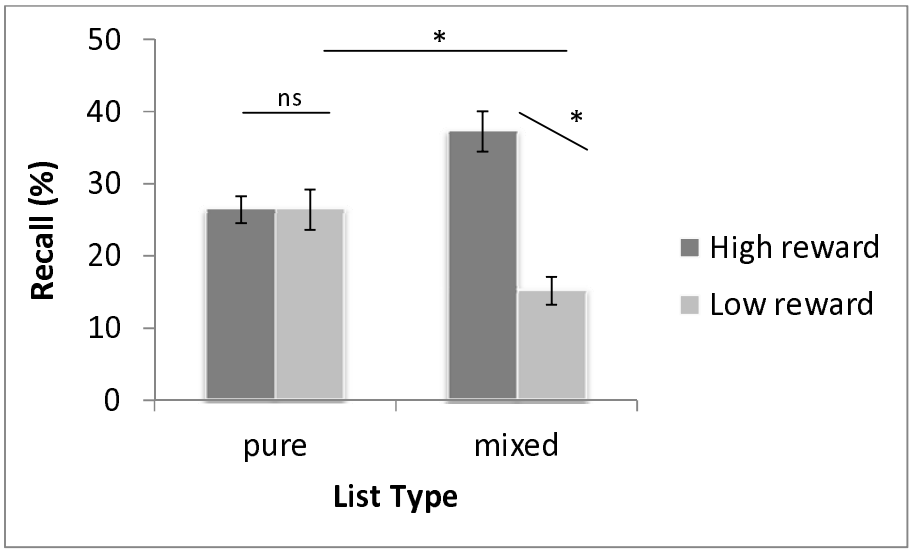

Simulated recall
ϕ = 1.25
Simulation 1a
ϕ = 2.25
Simulation 2a
ϕ = 1.25
Simulation 1b
ϕ = 2.25
Simulation 2b
Pure
Pure
Mixed
Mixed

Figure 2.

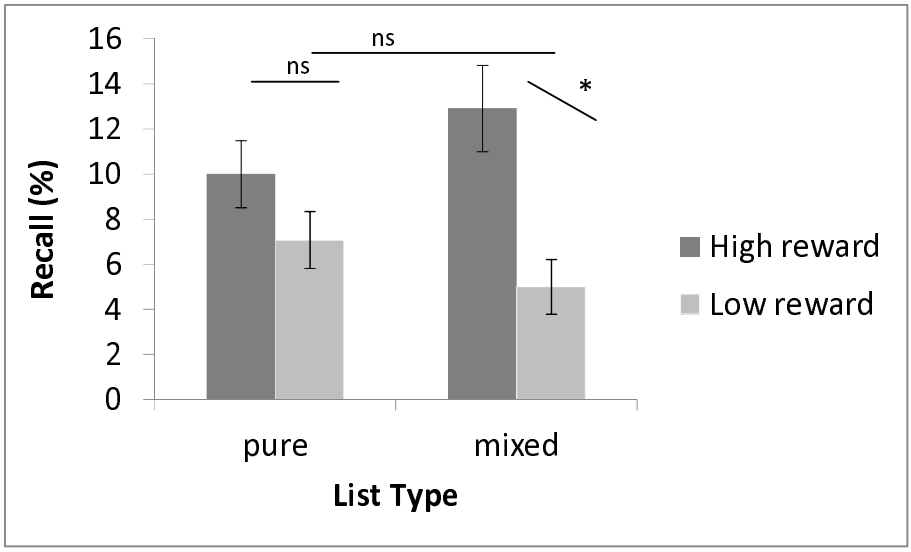
A


60
50
40
30
20
10
0
Recall (%)
*
ns
ns
*
Pure
Mixed
List Type
High reward
Low reward

B


50
40
30
20
10
0
Recall (%)
*
ns
*
pure
mixed
List Type
High reward
Low reward

C


16
14
12
10
8
6
4
2
0
Recall (%)
ns
ns
*
pure
mixed
List Type
High reward
Low reward

D

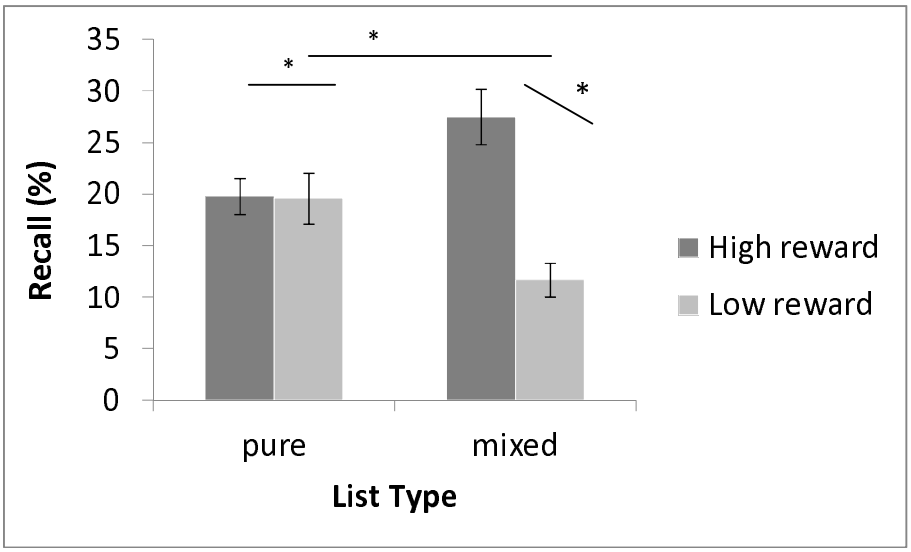

35
30
25
20
15
10
5
0
Recall (%)
*
*
*
pure
mixed
List Type
High reward
Low reward

Figure 3.

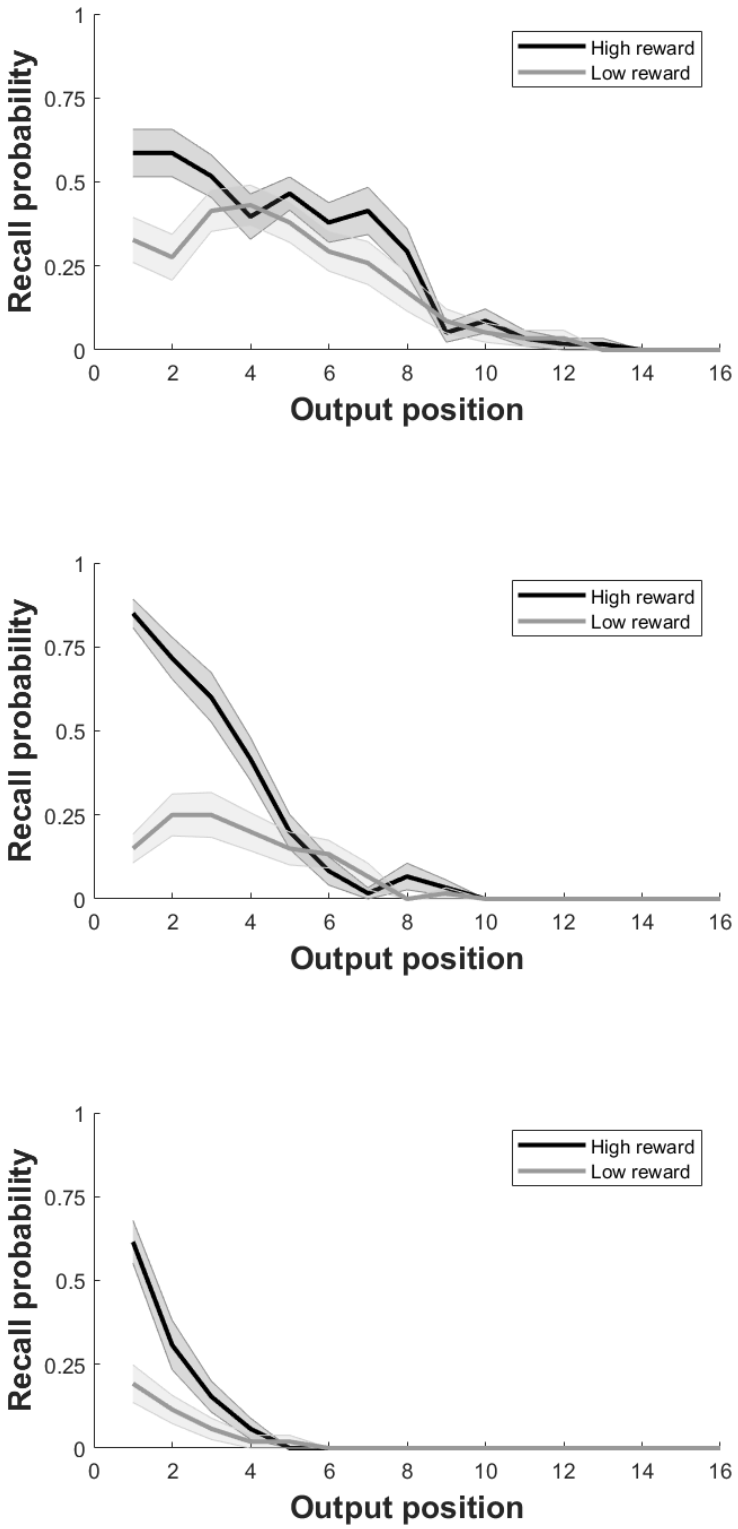

Recall probability
Output position
High reward
Low reward
0
0.25
0.5
0.75
1
0
2
4
6
8
10
12
14
16

Figure 4.

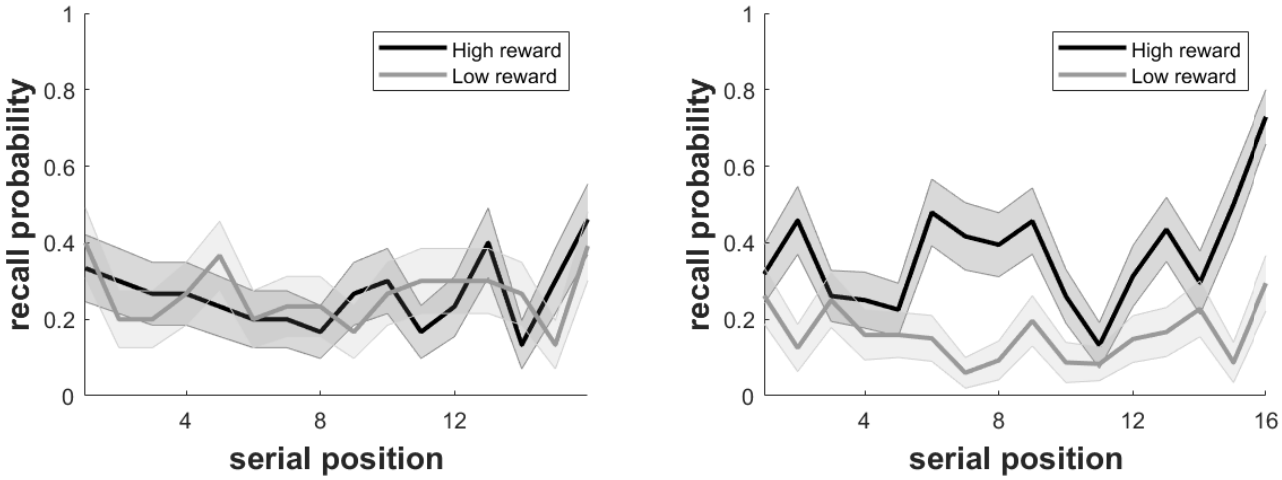

recall probability
serial position
High reward
Low reward
0
0.2
0.4
0.6
0.8
1
4
8
12
16

Figure 5

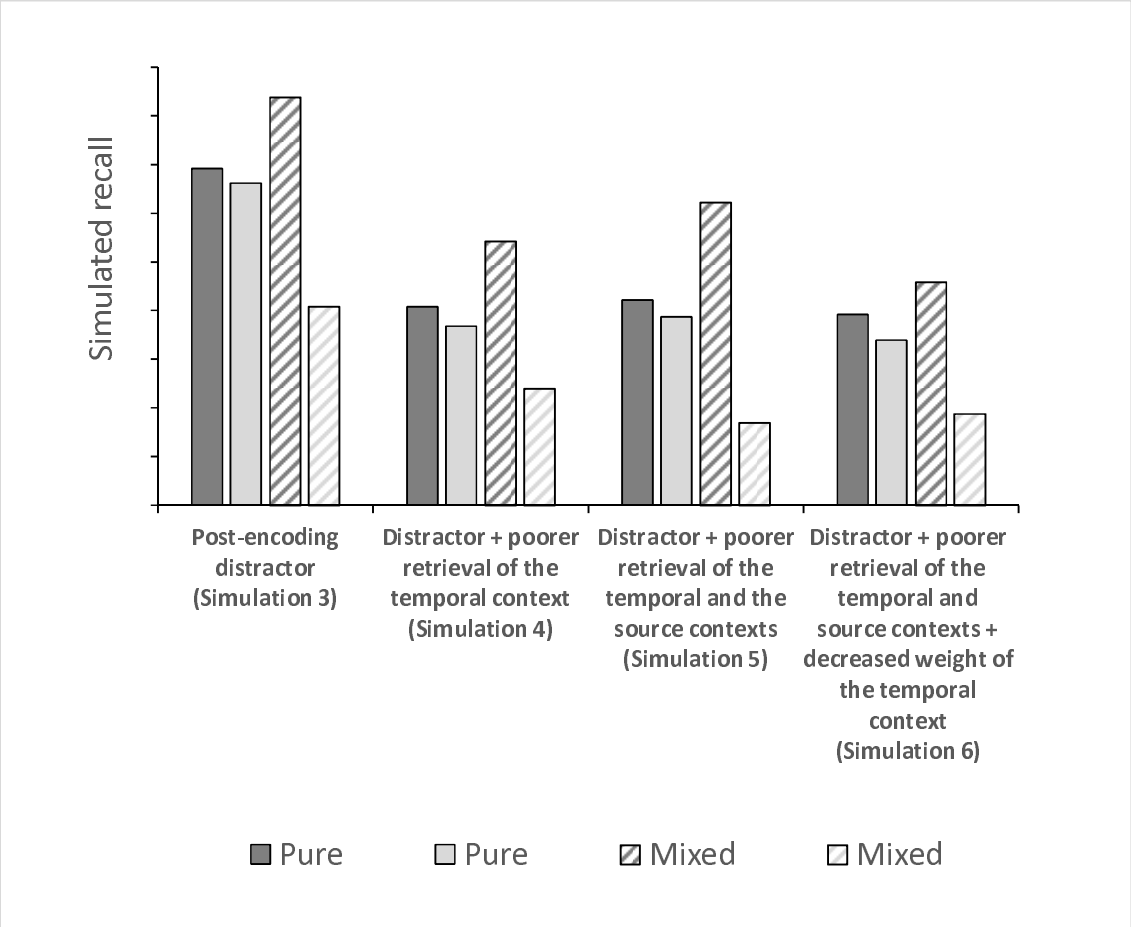
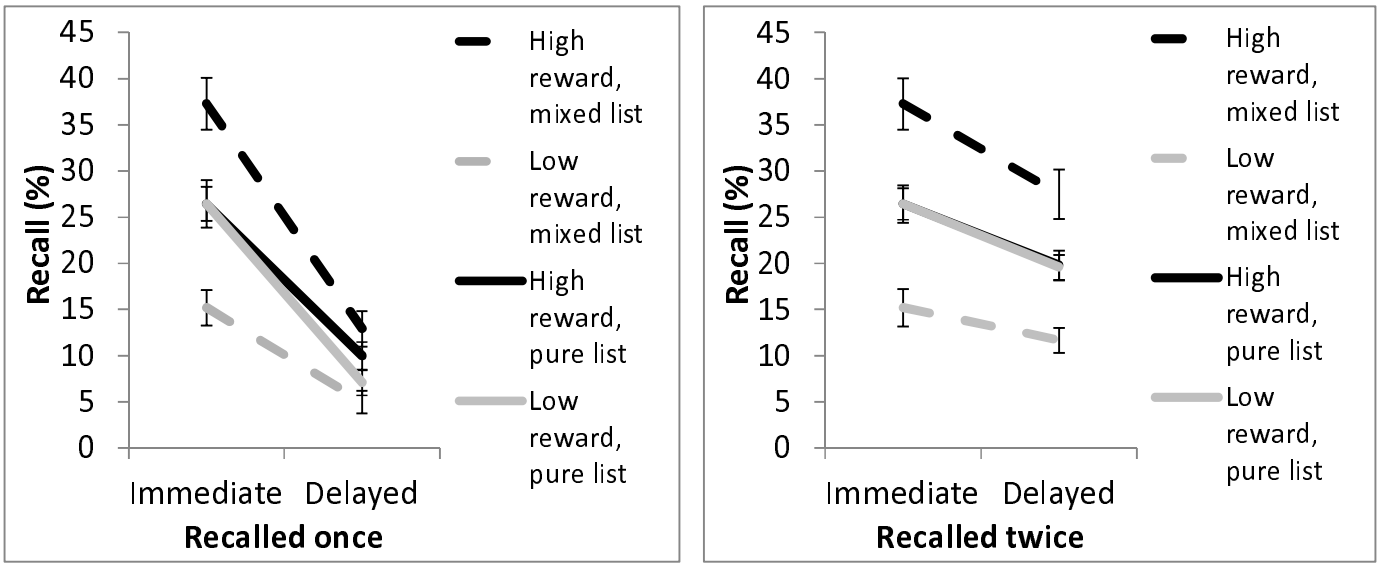
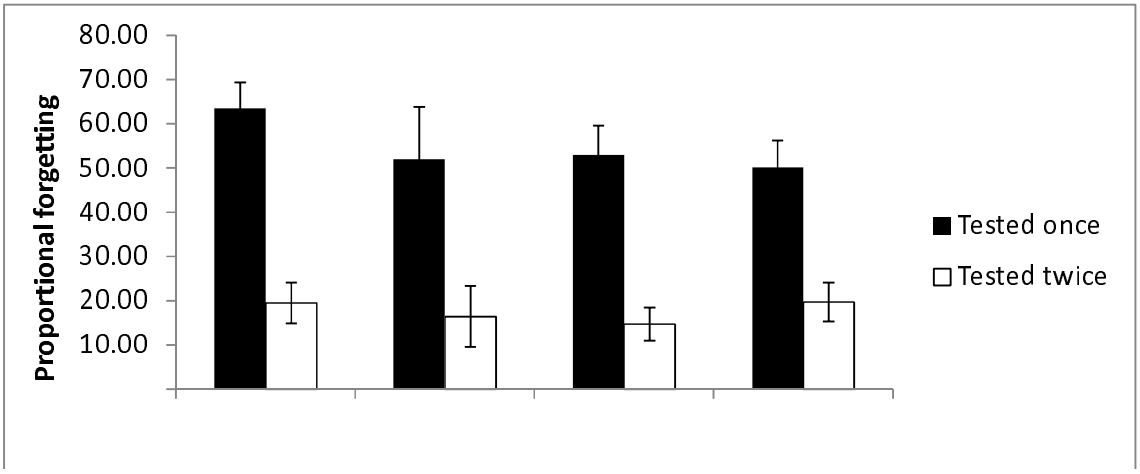

Simulated recall
Post-encoding distractor (Simulation 3)
Distractor + poorer retrieval of the temporal context (Simulation 4)
Distractor + poorer retrieval of the temporal and the source contexts (Simulation 5)
Distractor + poorer retrieval of the temporal and source contexts + decreased weight of the temporal context (Simulation 6)
Pure
Pure
Mixed
Mixed

Figure 6.


45
40
35
30
25
20
15
10
5
0
Recall (%)
Immediate
Delayed
Recalled once
High reward, mixed list
Low reward, mixed list
High reward, pure list
Low reward, pure list
Recalled twice


80.00
70.00
60.00
50.00
40.00
30.00
20.00
10.00
Proportional forgetting
Tested once
Tested twice