

1 Feature and order manipulations in a free recall task affect memory
2 for current and future lists

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4 **Abstract**

5 We perceive, interpret, and remember ongoing experiences through the lens of our prior
6 experiences. Inferring that we are in one type of situation versus another can lead us to interpret
7 the same physical experience differently. In turn, this can affect how we focus our attention,
8 form expectations about what will happen next, remember what is happening now, draw on
9 our prior related experiences, and so on. To study these phenomena, we asked participants
10 to perform simple word list-learning tasks. Across different experimental conditions, we held
11 the set of to-be-learned words constant, but we manipulated how incidental visual features
12 changed across words and lists, along with the orders in which the words were studied. We
13 found that these manipulations affected not only how the participants recalled the manipulated
14 lists, but also how they recalled later (randomly ordered) lists. Our work shows how structure
15 in our ongoing experiences can influence how we remember both our current experiences and
16 unrelated subsequent experiences.

17 **Keywords:** episodic memory, free recall, incidental features, implicit priming, temporal
18 **order**

19 Introduction

20 Experience is subjective: different people who encounter identical physical experiences
21 can take away very different meanings and memories. One reason is that our moment-by-
22 moment subjective experiences are shaped in part by the idiosyncratic prior experiences,
23 memories, goals, thoughts, expectations, and emotions that we bring with us into the
24 present moment. These factors collectively define a *context* for our experiences (Manning,
25 2020).

26 The contexts we encounter help us to construct *situation models* (Manning et al., 2015;
27 Radvansky and Copeland, 2006; Ranganath and Ritchey, 2012; Zwaan et al., 1995; Zwaan
28 and Radvansky, 1998) or *schemas* (Baldassano et al., 2018; Masís-Obando et al., 2022;
29 Tse et al., 2007) that describe how experiences are likely to unfold based on our prior
30 experiences with similar contextual cues. For example, when we enter a sit-down restau-
31 rant, we might expect to be seated at a table, given a menu, and served food. Priming
32 someone to expect a particular situation or context can also influence how they resolve
33 potential ambiguities in their ongoing experiences, including in ambiguous movies and
34 narratives (Rissman et al., 2003; Yeshurun et al., 2017).

35 Our understanding of how we form situation models and schemas, and how they
36 interact with our subjective experiences and memories, is constrained in part by substantial
37 differences in how we study these processes. Situation models and schemas are most often
38 studied using “naturalistic” stimuli such as narratives and movies (Nastase et al., 2020;
39 Zwaan et al., 1995; Zwaan and Radvansky, 1998). In contrast, our understanding of how
40 we organize our memories has been most widely informed by more traditional paradigms
41 like free recall of random word lists (Kahana, 2012, 2020). In free recall, participants study
42 lists of items and are instructed to recall the items in any order they choose. The orders
43 in which words come to mind can provide insights into how participants have organized

44 their memories of the studied words. Because random word lists are unstructured by
45 design, it is not clear if, or how, non-trivial situation models might apply to these stimuli.
46 As we unpack below, this provides an important motivation for our current study, which
47 uses free recall of *structured* lists to help bridge the gap between these two lines of research.

48 Like remembering real-world experiences, remembering words on a studied list re-
49 quires distinguishing the current list from the rest of one's experience. To model this
50 fundamental memory capability, cognitive scientists have posited a special context repre-
51 sentation that is associated with each list. According to early theories (e.g. Anderson and
52 Bower, 1972; Estes, 1955) context representations are composed of many features which
53 fluctuate from moment to moment, slowly drifting through a multidimensional feature
54 space. During recall, this representation forms part of the retrieval cue, enabling us to
55 distinguish list items from non-list items. Understanding the role of context in memory
56 processes is particularly important in self-cued memory tasks, such as free recall, where
57 the retrieval cue is "context" itself (Howard and Kahana, 2002a). Conceptually, the same
58 general processes might be said to describe how real-world contexts evolve during natural
59 experiences. However, this is still an open area of study (Manning, 2020, 2021).

60 Over the past half-century, context-based models have had impressive success at ex-
61 plaining many stereotyped behaviors observed during free recall and other list-learning
62 tasks (Estes, 1955; Glenberg et al., 1983; Howard and Kahana, 2002a; Kimball et al., 2007;
63 Polyn and Kahana, 2008; Polyn et al., 2009; Raaijmakers and Shiffrin, 1980; Sederberg
64 et al., 2008; Shankar and Howard, 2012; Sirotin et al., 2005). These phenomena include
65 the well known recency and primacy effects (superior recall of items from the end and,
66 to a lesser extent, from the beginning of the study list), as well as semantic and temporal
67 clustering effects (Howard and Kahana, 2002b; Kahana et al., 2008). The contiguity effect
68 is an example of temporal clustering, which is perhaps the dominant form of organization

69 in free recall. This effect can be seen in people’s tendencies to successively recall items that
70 occupied neighboring positions in the studied list (Kahana, 1996). There are also striking
71 effects of semantic clustering (Bousfield, 1953; Bousfield et al., 1954; Jenkins and Russell,
72 1952; Manning and Kahana, 2012; Romney et al., 1993), whereby the recall of a given
73 item is more likely to be followed by recall of a similar or related item than a dissimilar
74 or unrelated one. In general, people organize memories for words along a wide variety
75 of stimulus dimensions. According to models like the *Context Maintenance and Retrieval*
76 *Model* (Polyn et al., 2009), the stimulus features associated with each word (e.g. the word’s
77 meaning, size of the object the word represents, the letters that make up the word, font
78 size, font color, location on the screen, etc.) are incorporated into the participant’s mental
79 context representation (Manning, 2020; Manning et al., 2015, 2011, 2012; Smith and Vela,
80 2001). During a memory test, any of these features may serve as a memory cue, which in
81 turn leads the participant to recall in succession words that share stimulus features.

82 A key mystery is whether (and how) the sorts of situation models and schemas that
83 people use to organize their memories of real-world experiences might map onto the
84 clustering effects that reflect how people organize their memories for word lists. On
85 one hand, both situation models and clustering effects reflect statistical regularities in
86 ongoing experiences. Our memory systems exploit these regularities when generating
87 inferences about the unobserved past and yet-to-be-experienced future (Bower et al., 1979;
88 Momennejad et al., 2017; Ranganath and Ritchey, 2012; Schapiro and Turk-Browne, 2015;
89 Xu et al., 2023). On the other hand, the rich structures of real-world experiences and other
90 naturalistic stimuli that enable people to form deep and meaningful situation models and
91 schemas have no obvious analogs in simple word lists. Often, lists in free recall studies are
92 explicitly *designed* to be devoid of exploitable temporal structure, for example, by sorting
93 the words in a random order (Kahana, 2012).

94 We designed an experimental paradigm to explore how people organize their mem-
95 ories for simple stimuli (word lists) whose temporal properties change across different
96 “situations,” analogous to how the content of real-world experiences change across dif-
97 ferent real-world situations. We asked participants to study and freely recall a series of
98 word lists (Fig. 1). In the different conditions in our experiment, we varied the lists’
99 appearances and presentation orders in different ways. The studied items (words) were
100 designed to vary along three general dimensions: semantic (word *category* and physical
101 *size* of the referent), lexicographic (word *length* and *first letter*), and visual (font *color* and
102 the onscreen *location* of each word). We used two control conditions as a baseline; in
103 these control conditions all of the lists were sorted randomly, but we manipulated the
104 presence or absence of the visual features. In two conditions, we manipulated whether
105 the words’ appearances were fixed or variable within each list. In six conditions, we asked
106 participants to first study and recall eight lists whose items were sorted by a target feature
107 (e.g., word category), and then study and recall an additional eight lists whose items had
108 the same features, but that were sorted in a random temporal order. We were interested
109 in how these manipulations affected participants’ recall behaviors on early (manipulated)
110 lists, as well as how order manipulations on early lists affected recall behaviors on later
111 (randomly ordered) lists. Finally, in an *adaptive* experimental condition we used partici-
112 pants’ recall behaviors on early lists to manipulate, in real-time, the presentation orders
113 of subsequent lists. In this adaptive condition, we varied the agreement between how
114 participants preferred to organize their memories of the studied items versus the orders
115 in which the items were presented.

116 From a theoretical perspective, we are interested in several core questions organized
117 around the central theme of how structure in our experiences affect how we remember
118 *those* experiences, and also how we remember *future* experiences (which may or may not

119 exhibit similar structure). For example, when we distill participants' experiences down
120 to simple word lists that vary (meaningfully) along just a few feature dimensions, are
121 there important differences in which dimensions influence participants' memories? Or
122 are all features essentially "equally" influential? Further, are there differences in how
123 specific features influence participants' memories for ongoing versus future experiences?
124 Are there interaction effects between different features, or do people appear to treat each
125 feature independently? And are there individual differences in how people organize their
126 memories, or in how people are influenced by our experimental manipulations? If so,
127 what are those differences and which aspects of memory do they affect?

128 **Materials and methods**

129 **Participants**

130 We enrolled a total of 491 members of the Dartmouth College community across 11 exper-
131 imental conditions. The conditions included two controls (feature rich and reduced), two
132 visual manipulation conditions [reduced (early) and reduced (late)], six order manipula-
133 tion conditions (category, size, length, first letter, color, and location), and a final adaptive
134 condition. Each of these conditions is described in the *Experimental design* subsection
135 below.

136 Participants either received course credit or a one-time \$10 payment for enrolling in
137 our study. We asked each participant to fill out a demographic survey that included
138 questions about their age, gender, ethnicity, race, education, vision, reading impairments,
139 medications or recent injuries, coffee consumption on the day of testing, and level of
140 alertness at the time of testing. All components of the demographics survey were optional.
141 One participant elected not to fill out any part of the demographic survey, and all other

142 participants answered some or all of the survey questions.

143 We aimed to run (to completion) at least 60 participants in each of the two primary
144 control conditions and in the adaptive condition. In all of the other conditions, we set a
145 target enrollment of at least 30 participants. Because our data collection procedures en-
146 tailed the coordinated efforts of 12 researchers and multiple testing rooms and computers,
147 it was not feasible for individual experimenters to know how many participants had been
148 run in each experimental condition until the relevant databases were synchronized at the
149 end of each working day. We also over-enrolled participants for each condition to help
150 ensure that we met our minimum enrollment targets even if some participants dropped
151 out of the study prematurely or did not show up for their testing session. This led us to
152 exceed our target enrollments for several conditions. Nevertheless, we analyze all viable
153 data in the present paper.

154 Participants were assigned to experimental conditions based loosely on their date of
155 participation. (This aspect of our procedure helped us to more easily synchronize the ex-
156 periment databases across multiple testing computers.) Of the 490 participants who opted
157 to fill out the demographics survey, reported ages ranged from 17 to 31 years (mean: 19.1
158 years; standard deviation: 1.356 years). A total of 318 participants reported their gender as
159 female, 170 as male, and two participants declined to report their gender. A total of 442 par-
160 ticipants reported their ethnicity as “not Hispanic or Latino,” 39 as “Hispanic or Latino,”
161 and nine declined to report their ethnicity. Participants reported their races as White (345
162 participants), Asian (120 participants), Black or African American (31 participants), Amer-
163 ican Indian or Alaska Native (11 participants), Native Hawaiian or Other Pacific Islander
164 (four participants), Mixed race (three participants), Middle Eastern (one participant), and
165 Arab (one participant). A total of five participants declined to report their race. We note
166 that several participants reported more than one of the above racial categories. Participants

167 reported their highest degrees achieved as “Some college” (359 participants), “High school
168 graduate” (117 participants), “College graduate” (seven participants), “Some high school”
169 (five participants), “Doctorate” (one participant), and “Master’s degree” (one participant).
170 A total of 482 participants reported no reading impairments, and eight reported having
171 mild reading impairments. A total of 489 participants reported having normal color vision
172 and one participant reported that they were red-green color blind. A total of 482 partic-
173 ipants reported taking no prescription medications and having no recent injuries; four
174 participants reported having ADHD, one reported having dyslexia, one reported having
175 allergies, one reported a recently torn ACL/MCL, and one reported a concussion from
176 several months prior. The participants reported consuming 0–3 cups of coffee prior to the
177 testing session (mean: 0.32 cups; standard deviation: 0.58 cups). Participants reported
178 their current level of alertness, and we converted their responses to numerical scores as
179 follows: “very sluggish” (-2), “a little sluggish” (-1), “neutral” (0), “a little alert” (1), and
180 “very alert” (2). Across all participants, the full range of alertness levels were reported
181 (range: -2–2; mean: 0.35; standard deviation: 0.89).

182 We dropped from our dataset the one participant who reported having abnormal color
183 vision, as well as 38 participants whose data were corrupted due to technical failures while
184 running the experiment or during the daily database merges. In total, this left usable data
185 from 452 participants, broken down by experimental condition as follows: feature rich (67
186 participants), reduced (61 participants), reduced (early) (42 participants), reduced (late)
187 (41 participants), category (30 participants), size (30 participants), length (30 participants),
188 first letter (30 participants), color (31 participants), location (30 participants), and adaptive
189 (60 participants). The participant who declined to fill out their demographic survey
190 participated in the location condition, and we verified verbally that they had normal color
191 vision and no significant reading impairments.

192 Experimental design

193 Our experiment is a variant of the classic free recall paradigm that we term “*feature-rich free*
194 *recall*.” In feature-rich free recall, participants study 16 lists, each comprised of 16 words
195 that vary along a number of stimulus dimensions (Fig. 1). The stimulus dimensions include
196 two semantic features related to the *meanings* of the words (semantic category, referent
197 object size), two lexicographic features related to the *letters* that make up the words (word
198 length in number of letters, identity of the word’s first letter), and two visual features
199 that are independent of the words themselves (text color, presentation location). Each
200 list contains four words from each of four different semantic categories, with two object
201 sizes reflected across all of the words. After studying each list, the participant attempts
202 to recall as many words as they can from that list, in any order they choose. Because
203 each individual word is associated with several well defined (and quantifiable) features,
204 and because each list incorporates a diverse mix of feature values along each dimension,
205 this allows us to estimate which features participants are considering or leveraging in
206 organizing their memories.

207 Stimuli

208 The stimuli in our paradigm were 256 English words selected in a previous study (Ziman
209 et al., 2018). The words all referred to concrete nouns, and were chosen from 15 unique se-
210 mantic categories: body parts, building-related, cities, clothing, countries, flowers, fruits,
211 insects, instruments, kitchen-related, mammals, (US) states, tools, trees, and vegetables.
212 We also tagged each word according to the approximate size of the object the word referred
213 to. Words were labeled as “small” if the corresponding object was likely able to “fit in
214 a standard shoebox” or “large” if the object was larger than a shoebox. Most semantic
215 categories comprised words that reflected both “small” and “large” object sizes, but sev-



Figure 1: Feature-rich free recall. After studying lists comprised of words that vary along several feature dimensions, participants verbally recall words in any order (microphone icon). Each experimental condition manipulates word features and/or presentation orders within and/or across lists. The rows display representative (illustrated) examples of items from the first list participants might encounter in each condition. The rectangles during the “Presentation phase” show illustrated screen captures during a series of word presentations. Each word appeared onscreen for 2 seconds, followed by 2 seconds of blank screen. The red microphone icons during the “Recall” phase denote the one minute verbal recall interval. The labels on the right (and corresponding groupings on the left) denote experimental condition labels.

216 eral included only one or the other (e.g., all countries, US states, and cities are larger than
217 a shoebox; mean number of different sizes per category: 1.33; standard deviation: 0.49).
218 The numbers of words in each semantic category also varied from 12–28 (mean number of
219 words per category: 17.07; standard deviation number of words: 4.65). We also identified
220 lexicographic features for each word, including the words’ first letters and lengths (i.e.,
221 number of letters). Across all categories, all possible first letters were represented except
222 for ‘Q’ (average number of unique first letters per category: 11; standard deviation: 2
223 letters). Word lengths ranged from 3–12 letters (average: 6.17 letters; standard deviation:
224 2.06 letters).

225 We assigned the categorized words into a total of 16 lists with several constraints. First,
226 we required that each list contained words from exactly four unique categories, each with
227 exactly four exemplars from each category. Second, we required that (across all words
228 on the list) at least one instance of both object sizes were represented. On average, each
229 category was represented in 4.27 lists (standard deviation: 1.16 lists). Aside from these
230 two constraints, we assigned each word to a unique list. After random assignment, each
231 list contained words with an average of 11.13 unique starting letters (standard deviation:
232 1.15 letters) and an average word length of 6.17 letters (standard deviation: 0.34 letters).

233 The above assignments of words to lists was performed once across all participants,
234 such that every participant studied the same set of 16 lists. In every condition we random-
235 ized the study order of these lists across participants. For participants in most conditions,
236 on some or all of the lists, we also randomly varied two additional visual features associ-
237 ated with each word: the presentation font color, and the word’s onscreen location. These
238 attributes were assigned independently for each word (and for every participant). These
239 visual features were varied for words in all lists and conditions except for the “reduced”
240 condition (all lists), the first eight lists of the “reduced (early)” condition, and the last eight

241 lists of the “reduced (late)” condition. In these latter cases, words were all presented in
242 black at the center of the experimental computer’s display.

243 To select a random font color for each word, we drew three integers uniformly and
244 at random from the interval $[0, 255]$, corresponding to the red (r), green (g), and blue
245 (b) color channels for that word. To assign random presentation locations to each word,
246 we selected two floating point numbers uniformly and at random (one for the word’s
247 horizontal x -coordinate and the other for its vertical y -coordinate). The bounds of these
248 coordinates were selected to cover the entire visible area of the display without cutting off
249 any part of the words. The words were shown on 27-in (diagonal) Retina 5K iMac displays
250 (resolution: 5120×2880 pixels).

251 Most of the experimental manipulations we carried out entailed presenting or sorting
252 the presented words differently on the first eight lists participants studied (which we call
253 *early* lists) versus on the final eight lists they studied (*late* lists). Since every participant
254 studied exactly 16 lists, every list was either “early” or “late” depending on its order in
255 the list study sequence.

256 **Real-time speech-to-text processing**

257 Our experimental paradigm incorporates the Google Cloud Speech API speech-to-text en-
258 gine (Halpern et al., 2016) to automatically transcribe participants’ verbal recalls into text.
259 This allows recalls to be transcribed in real time—a distinguishing feature of the experi-
260 ment; in typical verbal recall experiments, the audio data must be parsed and transcribed
261 manually. In prior work, we used a similar experimental setup (equivalent to the “re-
262 duced” condition in the present study) to verify that the automatically transcribed recalls
263 were sufficiently close to human-transcribed recalls to yield reliable data (Ziman et al.,
264 2018). This real-time speech processing component of the paradigm plays an important

265 role in the “adaptive” condition of the experiment, as described below.

266 **Random conditions (Fig. 1, top four rows)**

267 We used two “control” conditions to evaluate and explore participants’ baseline behaviors.
268 We also used performance on these control conditions to help interpret performance in
269 other “manipulation” conditions. In the first control condition, which we call the *feature*
270 *rich* condition, we randomly shuffled the presentation order (independently for each
271 participant) of the words on each list. In the second control condition, which we call the
272 *reduced* condition, we randomized word presentations as in the feature rich condition.
273 However, rather than assigning each word a random color and location, we instead
274 displayed all of the words in black and at the center of the screen.

275 We also designed two conditions where we varied the words’ visual appearances across
276 lists. In the *reduced (early)* condition, we followed the “reduced” procedure (presenting
277 each word in black at the center of the screen) for early lists, and followed the “feature rich”
278 procedure (presenting each word in a random color and location) for late lists. Finally, in
279 the *reduced (late)* condition, we followed the feature rich procedure for early lists and the
280 reduced procedure for late lists.

281 **Order manipulation conditions (Fig. 1, middle six rows)**

282 Each of six *order manipulation* conditions used a different feature-based sorting procedure
283 to order words on early lists, where each sorting procedure relied on one relevant feature
284 dimension. All of the irrelevant features varied freely across words on early lists, in that
285 we did not consider irrelevant features in ordering the early lists. However, we note that
286 some features were correlated—for example, some semantic categories of words referred
287 to objects that tended to be a particular size, which meant that category and size were not

288 fully independent. On late lists, the words were always presented in a randomized order
289 (chosen anew for each participant). In all of the order manipulation conditions, we varied
290 words' font colors and onscreen locations, as in the feature rich condition.

291 **Defining feature-based distances.** Sorting words according to a given relevant feature
292 requires first defining a distance function for quantifying the dissimilarity between each
293 pair of features. This function varied according to the type of feature under consideration.
294 Semantic features (category and size) are *categorical*. For these features, we defined a
295 binary distance function: two words were considered to “match” (i.e., have a distance of
296 0) if their labels were the same (i.e., both from the same semantic category or both of the
297 same size). If two words' labels were different for a given feature, we defined the words
298 to have a distance of 1 for that feature. Lexicographic features (length and first letter)
299 are *discrete*. For these features we defined a discrete distance function. Specifically, we
300 defined the distance between two words as either the absolute difference between their
301 lengths, or the absolute distance between their starting letters in the English alphabet,
302 respectively. For example, two words that started with the same letter would have a “first
303 letter” distance of 0, and a pair of words starting with ‘J’ and ‘A’ would have a first letter
304 distance of 9. Because words' lengths and letters' positions in the alphabet are always
305 integers, these discrete distances always take on integer values. Finally, the visual features
306 (color and location) are *continuous* and *multivariate*, in that each “feature” is defined by
307 multiple (positive) real values. We defined the “color” and “location” distances between
308 two words as the Euclidean distances between their (r, g, b) color or (x, y) location vectors
309 (specified in inches), respectively. Therefore, the color and location distance measures
310 always take on non-negative real values (upper-bounded at 441.67 for color, or 27 in for
311 location, reflecting the distances between the corresponding maximally different vectors).

312 **Constructing feature-sorted lists.** Given a list of words, a relevant feature, and each
 313 word’s value(s) for that feature, we developed a stochastic algorithm for (noisily) sorting
 314 the words. The stochastic aspect of our sorting procedure enabled us to obtain unique
 315 orderings for each participant. First, we choose a word uniformly and at random from
 316 the set of words on the to-be-presented list. Second, we compute the distances between
 317 the chosen word’s feature(s) and the corresponding feature(s) of all yet-to-be-presented
 318 words. Third, we convert these distances (between the previously presented word’s
 319 feature values, a , and the candidate word’s feature values, b) to similarity scores:

$$\text{similarity}(a, b) = \exp\{-\tau \cdot \text{distance}(a, b)\}, \quad (1)$$

320 where $\tau = 1$ in our implementation. We note that increasing the value of τ would amplify
 321 the influence of similarity on order, and decreasing the value of τ would diminish the
 322 influence of similarity on order. Also note that this approach requires $\tau > 0$. Finally, we
 323 computed a set of normalized similarity values by dividing the similarities by their sum:

$$\text{similarity}_{\text{normalized}}(a, b) = \frac{\text{similarity}(a, b)}{\sum_{i=1}^n \text{similarity}(a, i)}, \quad (2)$$

324 where in the denominator, i takes on each of the n feature values of the to-be-presented
 325 words. The resulting set of normalized similarity scores sums to 1.

326 As illustrated in Figure 2, we use these normalized similarity scores to construct a
 327 sequence of “sticks” that we lay end to end in a line. Each of the n sticks corresponds to a
 328 single to-be-presented word, and the stick lengths are proportional to the relative similar-
 329 ities between each word’s feature value(s) and the feature value(s) of the just-presented
 330 word. We choose the next to-be-presented word by moving an indicator along the set of
 331 sticks, by a distance chosen uniformly and at random on the interval $[0, 1]$. We select the

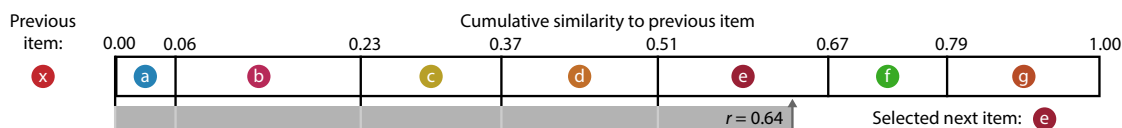


Figure 2: Generating stochastic feature-sorted lists. For a given feature dimension (e.g., color), we compute the similarity (Eqn. 1) between the feature value(s) of the previous item, x , and all yet-to-be-presented items (a – g). Next, we normalize these similarity scores so that they sum to 1. We lay, in sequence, a set of “sticks,” one for each candidate item, whose lengths are equal to these normalized similarity scores. To select the next to-be-presented item, we draw a random number, r , from the uniform distribution bounded between 0 and 1 (inclusive). The identity of the next item is given by the stick adjacent to an indicator that moves distance r (starting from 0) along the sequence of sticks. In this case, the next to-be-presented item is e . Note that each item’s chances of selection is proportional to its similarity to the previous item, along the given feature dimension (e.g., color).

word associated with the stick lying next to the indicator to be presented next. This process continues iteratively (re-computing the similarity scores and stochastically choosing the next to-be-presented word using the just-presented word) until all of the words have been presented. The result is an ordered list that tends to change gradually along the selected feature dimension (for example “sorted” lists, see Fig. 1, *Order manipulation* lists).

Adaptive condition

We designed the *adaptive* experimental condition to study the effect on memory of lists that matched (or mismatched) the ways participants “naturally” organized their memories. Like the other conditions, all participants in the adaptive condition studied a total of 16 lists, in a randomized order. We varied the words’ colors and locations for every word presentation, as in the feature rich and order manipulation conditions.

All participants in the adaptive condition began the experiment by studying a set of four *initialization* lists. Words and features on these lists were presented in a randomized order (computed independently for each participant). These initialization lists were used to estimate each participant’s “memory fingerprint,” defined below. At a high level,

347 a participant’s memory fingerprint describes how they prioritize or consider different
348 semantic, lexicographic, and/or visual features when they organize their memories.

349 Next, participants studied a sequence of 12 lists in three batches of four lists each. These
350 batches came in three types: *random*, *stabilize*, and *destabilize*. The batch types determined
351 how words on the lists in that batch were ordered. Lists in each batch were always
352 presented consecutively (e.g., a participant might receive four random lists, followed
353 by four stabilize lists, followed by four destabilize lists). The batch orders were evenly
354 counterbalanced across participants: there are six possible orderings of the three batches,
355 and 10 participants were randomly assigned to each ordering sub-condition.

356 Lists in the random batches were sorted randomly (as on the initialization lists and in
357 the feature rich condition). Lists in the stabilize and destabilize batches were sorted in ways
358 that either matched or mismatched each participant’s memory fingerprint, respectively.
359 Our procedures for estimating participants’ memory fingerprints and ordering the stabilize
360 and destabilize lists are described next.

361 **Feature clustering scores (uncorrected).** Feature clustering scores describe participants’
362 tendencies to recall similar presented items together in their recall sequences, where
363 “similarity” considers one given feature dimension (e.g., category, color, etc.). We base
364 our main approach to computing clustering scores on analogous temporal and semantic
365 clustering scores developed by Polyn et al. (2009). Computing the clustering score for
366 one feature dimension starts by considering the corresponding feature values from the
367 first word the participant recalled correctly from the just-studied list. Next, we sort all
368 not-yet-recalled words in ascending order according to their feature-based distance to the
369 just-recalled item (see *Defining feature-based distances*). We then compute the percentile rank
370 of the observed next recall. We average these percentile ranks across all of the participant’s
371 recalls for the current list to obtain a single uncorrected clustering score for the list, for the

372 given feature dimension. We repeated this process for each feature dimension in turn to
373 obtain a single uncorrected clustering score for each list, for each feature dimension.

374 **Temporal clustering score (uncorrected).** Temporal clustering describes a participant's
375 tendency to organize their recall sequences by the learned items' encoding positions. For
376 instance, if a participant recalled the lists' words in the exact order they were presented (or
377 in exact reverse order), this would yield a score of 1. If a participant recalled the words in
378 a random order, this would yield an expected score of 0.5. For each recall transition (and
379 separately for each participant), we sorted all not-yet-recalled words according to their
380 absolute lag (that is, distance away in the list). We then computed the percentile rank of
381 the next word the participant recalled. We took an average of these percentile ranks across
382 all of the participant's recalls to obtain a single (uncorrected) temporal clustering score for
383 the participant.

384 **Permutation-corrected feature clustering scores.** Suppose that two lists contain unequal
385 numbers of items of each size. For example, suppose that list *A* contains all "large" items,
386 whereas list *B* contains an equal mix of "large" and "small" items. For a participant
387 recalling list *A*, any correctly recalled item will necessarily match the size of the previous
388 correctly recalled item. In other words, successively recalling several list *A* items of the
389 same size is essentially meaningless, since *any* correctly recalled list *A* word will be large.
390 In contrast, successively recalling several list *B* items of the same size *could* be meaningful,
391 since (early in the recall sequence) the yet-to-be-recalled items come from a mix of sizes.
392 However, once all of the small items on list *B* have been recalled, the best possible next
393 matching recall will be a large item. All subsequent correct recalls must also be large
394 items—so for those later recalls it becomes difficult to determine whether the participant
395 is successively recalling large items because they are organizing their memories according

396 to size, or (alternatively), whether they are simply recalling the yet-to-be-recalled items
397 in a random order. In general, the precise order and blend of feature values expressed
398 in a given list, the order and number of correct recalls a participant makes, the number
399 of intervening presentation positions between successive recalls, and so on, can all affect
400 the range of clustering scores that are possible to observe for a given list. An uncorrected
401 clustering score therefore conflates participants’ actual memory organization with other
402 “nuisance” factors.

403 Following our prior work (Heusser et al., 2017), we used a permutation-based cor-
404 rection procedure to help isolate the behavioral aspects of clustering that we were most
405 interested in. After computing the uncorrected clustering score (for the given list and
406 observed recall sequence), we compute a “null” distribution of n additional clustering
407 scores after randomly shuffling the order of the recalled words (we use $n = 500$ in the
408 present study). This null distribution represents an approximation of the range of cluster-
409 ing scores one might expect to observe by “chance,” given that a hypothetical participant
410 was *not* truly clustering their recalls, but where the hypothetical participant still studied
411 and recalled exactly the same items (with the same features) as the true participant. We
412 define the *permutation-corrected clustering score* as the percentile rank of the observed un-
413 corrected clustering score in this estimated null distribution. In this way, a corrected score
414 of 1 indicates that the observed score was greater than any clustering score one might
415 expect by chance—in other words, good evidence that the participant was truly clustering
416 their recalls along the given feature dimension. We applied this correction procedure to
417 all of the clustering scores (feature and temporal) reported in this paper.

418 **Memory fingerprints.** We define each participant’s *memory fingerprint* as the set of their
419 permutation-corrected clustering scores across all dimensions we tracked in our study,
420 including their six feature-based clustering scores (category, size, length, first letter, color,

421 and location) and their temporal clustering score. Conceptually, a participant’s memory
422 fingerprint describes their tendency to order in their recall sequences (and, presumably,
423 organize in memory) the studied words along each dimension. To obtain stable estimates
424 of these fingerprints for each participant, we averaged their clustering scores across lists.
425 We also tracked and characterized how participants’ fingerprints changed across lists (e.g.,
426 Figs. 6, S8).

427 **Online “fingerprint” analysis.** The presentation orders of some lists in the adaptive
428 condition of our experiment (see *Adaptive condition*) were sorted according to participants’
429 *current* memory fingerprint, estimated using all of the lists they had studied up to that point
430 in the experiment. Because our experiment incorporated a speech-to-text component, all
431 of the behavioral data for each participant could be analyzed just a few seconds after the
432 conclusion of the recall intervals for each list. We used the Quail Python package (Heusser
433 et al., 2017) to apply speech-to-text algorithms to the just-collected audio data, aggregate
434 the data for the given participant, and estimate the participant’s memory fingerprint
435 using all of their available data up to that point in the experiment. Two aspects of our
436 implementation are worth noting. First, because memory fingerprints are computed
437 independently for each list and then averaged across lists, the already-computed memory
438 fingerprints for earlier lists could be cached and loaded as needed in future computations.
439 This meant that our computations pertaining to updating our estimate of a participant’s
440 memory fingerprint only needed to consider data from the most recent list. Second, each
441 element of the null distributions of uncorrected fingerprint scores (see *Permutation-corrected*
442 *feature clustering scores*) could be estimated independently from the others. This enabled
443 us to make use of the testing computers’ multi-core CPU architectures by considering (in
444 parallel) elements of the null distributions in batches of eight (i.e., the number of CPU
445 cores on each testing computer). Taken together, we were able to compress the relevant

446 computations into just a few seconds of computing time. The combined processing time for
447 the speech-to-text algorithm, fingerprint computations, and permutation-based ordering
448 procedure (described next) easily fit within the inter-list intervals, where participants
449 paused for a self-paced break before moving on to study and recall the next list.

450 **Ordering “stabilize” and “destabilize” lists by an estimated fingerprint.** In the adap-
451 tive condition of our experiment, the presentation orders for *stabilize* and *destabilize* lists
452 were chosen to either maximally or minimally (respectively) comport with participants’
453 memory fingerprints. Given a participant’s memory fingerprint and a to-be-presented set
454 of items, we designed a permutation-based procedure for ordering the items. First, we
455 dropped from the participant’s fingerprint the temporal clustering score. For the remain-
456 ing feature dimensions, we arranged the clustering scores in the fingerprint into a template
457 vector, f . Second, we computed $n = 2500$ random permutations of the to-be-presented
458 items. These permutations served as candidate presentation orders. We sought to select
459 the specific order that most (or least) closely matched f . Third, for each random permu-
460 tation, we computed the (permutation-corrected) “fingerprint,” treating the permutation
461 as though it were a potential “perfect” recall sequence. (We did not include temporal
462 clustering scores in these fingerprints, since the temporal clustering score for every per-
463 mutation is always equal to 1.) This yielded a “simulated fingerprint” vector, \hat{f}_p for each
464 permutation p . We used these simulated fingerprints to select a specific permutation, i ,
465 that either maximized (for stabilize lists) or minimized (for destabilize lists) the correlation
466 between \hat{f}_i and f .

467 **Computing low-dimensional embeddings of memory fingerprints**

468 Following some of our prior work (Heusser et al., 2021, 2018; Manning et al., 2022),
469 we use low-dimensional embeddings to help visualize how participants’ memory fin-

gerprints change across lists (Figs. 6A, S8A). To compute a shared embedding space across participants and experimental conditions, we concatenated the full set of across-participant average fingerprints (for all lists and experimental conditions) to create a large matrix with number-of-lists (16) \times number-of-conditions (10, including the adaptive condition) rows and seven columns (one for each feature clustering score, plus an additional temporal clustering score column). We used principal components analysis to project the seven-dimensional observations into a two-dimensional space (using the two principal components that explained the most variance in the data). For two visualizations (Figs. 6B, and S8B), we computed an additional set of two-dimensional embeddings for the *average* fingerprints across lists within a given list grouping (i.e., early or late). For those visualizations, we averaged across the rows (for each condition and group of lists) in the combined fingerprint matrix prior to projecting it into the shared two-dimensional space. This yielded a single two-dimensional coordinate for each *list group* (in each condition), rather than for each individual list. We used these embeddings solely for visualization. All statistical tests were carried out in the original (seven-dimensional) feature spaces.

Factoring out the effects of temporal clustering

For a given list of words, if the values along two feature dimensions (e.g., category and size) are correlated, then the clustering scores for those two dimensions will also be correlated. When lists are sorted along a given feature dimension, the sorted feature values will also tend to be correlated with the serial positions of the words in the list. This means that the temporal clustering score will *also* tend to be correlated with the clustering scores for the sorted feature dimension. These correlations mean that it can be difficult to specifically identify when participants are using one feature versus another (or a manipulated feature versus temporal information) to organize or search their memories.

494 We developed a permutation-based procedure to factor out the effects of temporal
495 clustering from the clustering scores for each feature dimension. For a given set of recalled
496 items (whose presentation positions are given by $x_1, x_2, x_3, \dots, x_N$), we circularly shift the
497 presentation positions by a randomly chosen amount (between 1 and the list length) to
498 obtain a new set of items. Since the new set of items will have the same (average) temporal
499 distances between successive recalls, the temporal clustering score for the new set of items
500 is equal (on average) to the temporal clustering score for the original recalls. However,
501 we can then re-compute the feature clustering score for those new items. Finally, we
502 can compute a “temporally corrected” feature clustering score by computing the average
503 percentile rank of the observed (raw) feature clustering score within the distributions of
504 circularly shifted feature clustering scores, across $N = 500$ repetitions of this procedure.
505 This new temporally corrected score provides an estimate of the observed degree of feature
506 clustering over and above what could be accounted for by temporal clustering alone.

507 While these temporally corrected clustering scores are useful for identifying when
508 feature clustering cannot be accounted for by temporal clustering alone, they are *not*
509 necessarily valid estimates of the “true” degree to which participants are organizing their
510 memories along a given feature dimension. For example, on a list where the presentation
511 order and feature values (along the given feature dimension) are perfectly correlated, the
512 temporally corrected score will have an expected value of 0.5 no matter which words (or
513 in what order) are recalled. Therefore these temporally corrected clustering scores are
514 interpretable only to the extent that presentation order and feature values are decoupled.

515 **Analyses**

516 **Probability of n^{th} recall curves**

517 Probability of first recall curves (Atkinson and Shiffrin, 1968; Postman and Phillips, 1965;
518 Welch and Burnett, 1924) reflect the probability that an item will be recalled first, as a
519 function of its serial position during encoding. To carry out this analysis, we initialized
520 (for each participant) a number-of-lists (16) by number-of-words-per-list (16) matrix of 0s.
521 Then, for each list, we found the index of the word that was recalled first, and we filled
522 in that position in the matrix with a 1. Finally, we averaged over the rows of the matrix
523 to obtain a 1 by 16 array of probabilities, for each participant. We used an analogous
524 procedure to compute probability of n^{th} recall curves for each participant. Specifically,
525 we filled in the corresponding matrices according to the n^{th} recall on each list that each
526 participant made. When a given participant had made fewer than n recalls for a given
527 list, we simply excluded that list from our analysis when computing that participant's
528 curve(s). The probability of first recall curve corresponds to a special case where $n = 1$.

529 **Lag-conditional response probability curve**

530 The lag-conditional response probability (lag-CRP) curve (Kahana, 1996) reflects the prob-
531 ability of recalling a given item after the just-recalled item, as a function of their relative
532 encoding positions (lag). In other words, a lag of 1 indicates that a recalled item was
533 presented immediately after the previously recalled item, and a lag of -3 indicates that a
534 recalled item came three items before the previously recalled item. For each recall tran-
535 sition (following the first recall), we computed the lag between the just-recalled word's
536 presentation position and the next-recalled word's presentation position. We computed
537 the proportions of transitions (between successively recalled words) for each lag, normaliz-
538 ing for the total numbers of possible transitions. In carrying out this analysis, we excluded

all incorrect recalls and repetitions (i.e., recalling a word that had already appeared previously in the current recall sequence). This yielded, for each list, a 1 by number-of-lags (−15 to +15; 30 lags in total, excluding lags of 0) array of conditional probabilities. We averaged these probabilities across lists to obtain a single lag-CRP for each participant. Because transitions at large absolute lags are rare, these curves are typically displayed using range restrictions (Kahana, 2012).

Serial position curve

Serial position curves (Murdock, 1962) reflect the proportion of participants who remember each item as a function of the items' serial positions during encoding. For each participant, we initialized a number-of-lists (16) by number-of-words-per-list (16) matrix of 0s. Then, for each correct recall, we identified the presentation position of the word and entered a 1 into that position (row: list; column: presentation position) in the matrix. This resulted in a matrix whose entries indicated whether or not the words presented at each position, on each list, were recalled by the participant (depending on whether the corresponding entries were set to 1 or 0). Finally, we averaged over the rows of the matrix to yield a 1 by 16 array representing the proportion of words at each position that the participant remembered.

Identifying event boundaries

We used the distances between feature values for successively presented words (see *Defining feature-based distances*) to estimate “event boundaries” where the feature values changed more than usual (DuBrow and Davachi, 2016; Ezzyat and Davachi, 2011; Manning et al., 2016; Radvansky and Copeland, 2006; Swallow et al., 2011, 2009). For each list, for each feature dimension, we computed the distribution of distances between the feature values

for successively presented words. We defined event boundaries (e.g., Fig. 3B) as occurring between any successive pair of words whose distances along the given feature dimension were greater than one standard deviation above the mean for that list. Note that, because event boundaries are defined for each feature dimension, each individual list may contain several sets of event boundaries, each at different moments in the presentation sequence (depending on the feature dimension of interest).

Data and code availability

All of the data analyzed in this manuscript, along with all of the code for carrying out the analyses may be found at <https://github.com/ContextLab/FRFR-analyses>.

Results

While holding the set of words (and the assignments of words to lists) constant, we manipulated two aspects of participants' experiences of studying each list. We sought to understand the effects of these manipulations on participants' memories for the studied words. First, we added two additional sources of visual variation to the individual word presentations: font color and onscreen location. Importantly, these visual features were independent of the meaning or semantic content of the words (e.g., word category, size of the referent, etc.) and of the lexicographic properties of the words (e.g., word length, first letter, etc.). We wondered whether this additional word-independent information might facilitate recall (e.g., by providing new or richer potential ways of organizing or retrieving memories of the studied words; Davachi et al., 2003; Drewnowski and Murdock, 1980; Hargreaves et al., 2012; Madan, 2021; Meinhardt et al., 2020; Slamecka and Barlow, 1979; Socher et al., 2009) or impair recall (e.g., by distracting or confusing participants with irrelevant information Lange, 2005; Marsh et al., 2012, 2015; Reinitz et al., 1992).

585 Second, we manipulated the orders in which words were studied (and how those orderings
586 changed over time). We wondered whether presenting the same list of words with different
587 appearances (e.g., by manipulating font size and onscreen location) or in different orders
588 (e.g., sorted along one feature dimension versus another) might serve to influence how
589 participants organized their memories of the words (e.g., Manning et al., 2015; Polyn and
590 Kahana, 2008). We also wondered whether some order manipulations might be temporally
591 “sticky” by influencing how *future* lists were remembered (e.g., Baddeley, 1968; Darley
592 and Murdock, 1971; Lohnas et al., 2010; Sirotin et al., 2005; Whitely, 1927).

593 To obtain a clean preliminary estimate of the consequences on memory of randomly
594 varying the font colors and locations of presented words (versus holding the font color
595 fixed at black, and holding the display locations fixed at the center of the display) we
596 compared participants’ performance on the *feature rich* and *reduced* experimental conditions
597 (see *Random conditions*, Fig. S1). In the feature rich condition the words’ colors and
598 locations varied randomly across words, and in the reduced condition words were always
599 presented in black, at the center of the display. Aggregating across all lists for each
600 participant, we found no difference in recall accuracy (i.e., the proportions of correctly
601 recalled words) for feature rich versus reduced lists ($t(126) = -0.290, p = 0.772$). However,
602 participants in the feature rich condition clustered their recalls substantially more along
603 every dimension we examined (temporal clustering: $t(126) = 10.624, p < 0.001$; semantic
604 category clustering: $t(126) = 10.077, p < 0.001$; size clustering: $t(126) = 11.829, p < 0.001$;
605 word length clustering: $t(126) = 10.639, p < 0.001$; first letter clustering: $t(126) = 7.775, p <$
606 0.001 ; see *Permutation-corrected feature clustering scores* for more information about how we
607 quantified each participant’s clustering tendencies.) Taken together, these comparisons
608 suggest that adding new features changes how participants organize their memories of
609 studied words, even when those new features are independent of the words themselves

610 and even when the new features vary randomly across words. We found no evidence
611 that those additional uninformative features were distracting (in terms of their impact on
612 memory performance), but they did affect participants' recall dynamics (measured via
613 their clustering scores).

614 We also wondered whether adding these incidental visual features to later lists (after
615 the participants had already studied impoverished lists), or removing the visual features
616 from later lists (after the participants had already studied visually diverse lists) might affect
617 memory performance. In other words, we sought to test for potential effects of changing
618 the "richness" of participants' experiences over time. All participants studied and recalled
619 a total of 16 lists; we defined *early* lists as the first eight lists and *late* lists as the last eight lists
620 each participant encountered. To help interpret our results, we compared participants'
621 memories on early versus late lists in the above feature rich and reduced conditions.
622 Participants in both conditions remembered more words on early versus late lists (feature
623 rich: $t(66) = 4.553, p < 0.001$; reduced: $t(60) = 2.434, p = 0.018$). Participants in the feature
624 rich (but not reduced) conditions exhibited more temporal clustering on early versus
625 late lists (feature rich: $t(66) = 2.318, p = 0.024$; reduced: $t(60) = 0.929, p = 0.357$). And
626 participants in both conditions exhibited more semantic (category and size) clustering
627 on early versus late lists (feature rich, category: $t(66) = 3.805, p < 0.001$; feature rich,
628 size: $t(66) = 2.190, p = 0.032$; reduced, category: $t(60) = 2.856, p = 0.006$; reduced, size:
629 $t(60) = 2.947, p = 0.005$). Participants in the reduced (but not feature rich) conditions
630 exhibited more lexicographic clustering on early versus late lists (feature rich, word length:
631 $t(66) = 0.161, p = 0.872$; feature rich, first letter: $t(66) = 0.410, p = 0.683$; reduced, word
632 length: $t(60) = 3.528, p = 0.001$; reduced, first letter: $t(60) = 2.275, p = 0.026$). Taken
633 together, these comparisons suggest that even when the presence or absence of incidental
634 visual features is stable across lists, participants still exhibit some differences in their

635 performance and memory organization tendencies for early versus late lists.

636 With these differences in mind, we next compared participants' memories on early ver-
637 sus late lists for two additional experimental conditions (see *Random conditions*, Fig. S1).
638 In a *reduced (early)* condition, we held the visual features constant on early lists, but al-
639 lowed them to vary randomly on late lists. In a *reduced (late)* condition, we allowed
640 the visual features to vary randomly on early lists, but held them constant on late lists.
641 Given our above findings that (a) participants tended to exhibit stronger clustering ef-
642 fects on feature rich (versus reduced) lists, and (b) participants tended to remember more
643 words and exhibit stronger clustering effects on early (versus late) lists, we expected
644 these early versus late differences to be enhanced in the reduced (early) condition and
645 diminished in the reduced (late) condition. However, to our surprise, participants in *nei-*
646 *ther* condition exhibited reliable early versus late differences in accuracy (reduced (early):
647 $t(41) = 1.499, p = 0.141$; reduced (late): $t(40) = 1.462, p = 0.152$), temporal clustering (re-
648 duced (early): $t(41) = 0.998, p = 0.324$; reduced (late): $t(40) = 1.099, p = 0.278$), nor feature-
649 based clustering (reduced (early), category: $t(41) = 0.753, p = 0.456$; reduced (early), size:
650 $t(41) = 0.721, p = 0.475$; reduced (early), length: $t(41) = 0.493, p = 0.625$; reduced (early),
651 first letter: $t(41) = 0.780, p = 0.440$; reduced (late), category: $t(40) = -0.086, p = 0.932$;
652 reduced (late), size: $t(40) = 0.746, p = 0.460$; reduced (late), length: $t(40) = 1.476, p = 0.148$;
653 reduced (late), first letter: $t(40) = 0.966, p = 0.340$). We hypothesized that adding or remov-
654 ing the variability in the visual features was acting as a sort of "event boundary" between
655 early and late lists (e.g., Clewett et al., 2019; Radvansky and Copeland, 2006; Radvansky
656 and Zacks, 2017). In prior work, we (and others) have found that memories formed just
657 after event boundaries can be enhanced (e.g., due to less contextual interference between
658 pre- and post-boundary items; Flores et al., 2017; Gold et al., 2017; Manning et al., 2016;
659 Pettijohn et al., 2016).

660 We found that *adding* incidental visual features on later lists that had not been present
 661 on early lists (as in the reduced (early) condition) served to enhance recall performance
 662 relative to conditions where all lists had the same blends of features (accuracy for feature
 663 rich versus reduced (early): $t(107) = -2.230, p = 0.028$; reduced versus reduced (early):
 664 $t(101) = -2.045, p = 0.043$; also see Fig. S3A). However, *subtracting* irrelevant visual fea-
 665 tures on later lists that *had* been present on early lists (as in the reduced (late) condition) did
 666 not appear to impact recall performance (accuracy for feature rich versus reduced (late):
 667 $t(106) = -0.638, p = 0.525$; reduced versus reduced (late): $t(100) = -0.407, p = 0.685$).
 668 These comparisons suggest that recall accuracy has a directional component: accuracy is
 669 affected differently by removing features later that had been present earlier versus adding
 670 features later that had *not* been present earlier. In contrast, we found that participants
 671 exhibited more temporal and feature-based clustering when we added incidental visual
 672 features to *any* lists (comparisons of clustering on feature rich versus reduced lists are
 673 reported above; temporal clustering in reduced versus reduced (early) and reduced ver-
 674 sus reduced (late) conditions: $ts \leq -9.780, ps < 0.001$; feature-based clustering in reduced
 675 versus reduced (early) and reduced versus reduced (late) conditions: $ts \leq -5.443, ps$
 676 < 0.001). Temporal and feature-based clustering were not reliably different in the feature
 677 rich, reduced (early), and reduced (late) conditions (temporal clustering in feature rich
 678 versus reduced (early) and feature rich versus reduced (late) conditions: $ts \geq -1.434, ps$
 679 ≥ 0.154 ; feature-based clustering in feature rich versus reduced (early) and feature rich
 680 versus reduced (late) conditions: $ts \geq -1.359, ps > 0.177$).

681 Taken together, our findings thus far suggest that adding item features that change
 682 over time, even when they vary randomly and independently of the items, can enhance
 683 participants' overall memory performance and can also enhance temporal and feature-
 684 based clustering. To the extent that the number of item features that vary from moment

to moment approximates the “richness” of participants’ experiences, our findings suggest that participants remember “richer” stimuli better and organize richer stimuli more reliably in their memories. Next, we turn to examine the memory effects of varying the temporal ordering of different stimulus features. We hypothesized that changing the orders in which participants were exposed to the words on a given list might enhance (or diminish) the relative influence of different features. For example, presenting a set of words alphabetically might enhance participants’ attention to the studied items’ first letters, whereas sorting the same list of words by semantic category might instead enhance participants’ attention to the words’ semantic attributes. Importantly, we expected these order manipulations to hold even when the variation in the total set of features (across words) was held constant across lists (e.g., unlike in the reduced (early) and reduced (late) conditions, where variations in visual features were added or removed from a subset of the lists participants studied).

Across each of six order manipulation conditions, we sorted early lists by one feature dimension but randomly ordered the items on late lists (see *Order manipulation conditions*; features: category, size, length, first letter, color, and location). Participants in the category-ordered condition showed an increase in memory performance on early lists (accuracy, relative to early feature rich lists; $t(95) = 3.034, p = 0.003$). Participants in the color-ordered condition also showed a trending increase in memory performance on early lists (again, relative to early feature rich lists: $t(96) = 1.850, p = 0.067$; Fig. 5A). Participants’ performances on early lists in all of the other order manipulation conditions were indistinguishable from performance on the early feature rich lists ($|t|s < 1.013, ps > 0.314$). Participants in both of the semantically ordered conditions exhibited stronger temporal clustering on early lists (versus early feature rich lists; category: $t(95) = 8.508, p < 0.001$; size: $t(95) = 2.429, p = 0.017$; Fig. 5B). Participants in the length-ordered condition

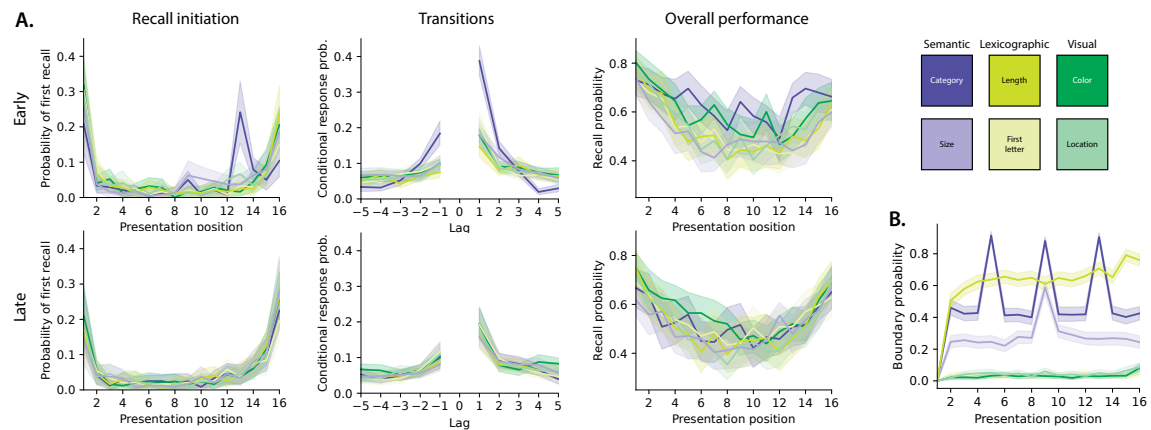


Figure 3: Recall dynamics in feature rich free recall (order manipulation conditions). **A.** Behavioral plots. **Left panels.** The probabilities of initiating recall with each word are plotted as a function of presentation position. **Middle panels.** The conditional probabilities of recalling each word are plotted as a function of the relative position (Lag) to the words recalled just-prior. **Right panels.** The overall probabilities of recalling each word are plotted as a function of presentation position. **All panels.** Error ribbons denote bootstrap-estimated 95% confidence intervals (calculated across participants). Top panels display the recall dynamics for early (order manipulation) lists in each condition (color). Bottom panels display the recall dynamics for late (randomly ordered) lists. See Figures S1 and S2 for analogous plots for the random and adaptive conditions. **B.** Proportion of event boundaries (see *Identifying event boundaries*) for each condition's feature of focus, plotted as a function of presentation position.

710 tended to exhibit *less* temporal clustering on early lists relative to early feature rich lists
 711 ($t(95) = -1.666, p = 0.099$), whereas participants in the first letter-ordered condition ex-
 712 hibited stronger temporal clustering on early lists ($t(95) = 2.587, p = 0.011$). Participants
 713 in the visually ordered conditions exhibited more similar performance on early lists, rel-
 714 ative to early feature rich lists (color: $t(96) = -1.064, p = 0.290$; we found a trending
 715 enhancement for participants in the location-ordered condition: $t(95) = 1.682, p = 0.096$).
 716 We also compared feature-based clustering on early lists across the order manipulation
 717 and feature rich conditions. Since these results were similar across both semantic con-
 718 ditions (category and size), both lexicographic conditions (length and first letter), and
 719 both visual conditions (color and location), here we aggregate data from conditions that
 720 manipulated each of these three feature groupings in our comparisons, to simplify the
 721 presentation. On early lists, participants in the semantically ordered conditions exhibited
 722 stronger semantic clustering relative to participants in the feature rich condition (category:
 723 $t(125) = 2.524, p = 0.013$; size: $t(125) = 3.510, p = 0.001$), but showed no reliable differences
 724 in lexicographic (length: $t(125) = 0.539, p = 0.591$; first letter: $t(125) = -0.587, p = 0.558$)
 725 or visual (color: $t(125) = -0.579, p = 0.564$; location: $t(125) = -0.346, p = 0.730$) clustering.
 726 Similarly, participants in the lexicographically ordered conditions exhibited stronger (rela-
 727 tive to feature rich participants) lexicographic clustering (length: $t(125) = 3.426, p = 0.001$;
 728 first letter: $t(125) = 3.236, p = 0.002$) on early lists, but showed no reliable differences in
 729 semantic (category: $t(125) = -1.078, p = 0.283$; size: $t(125) = -0.310, p = 0.757$) or visual
 730 (color: $t(125) = -0.209, p = 0.835$; location: $t(125) = -0.004, p = 0.997$) clustering. And
 731 participants in the visually ordered conditions exhibited stronger visual clustering (again,
 732 relative to feature rich participants, and on early lists; color: $t(126) = 2.099, p = 0.038$;
 733 location: $t(126) = 4.392, p < 0.001$), but showed no reliable differences in semantic (cate-
 734 gory: $t(126) = 0.204, p = 0.839$; size: $t(126) = -0.093, p = 0.926$) or lexicographic (length:

735 $t(126) = 0.714, p = 0.476$; first letter: $t(126) = 0.820, p = 0.414$) clustering. Taken together,
 736 these order manipulation results suggest several broad patterns (Figs. 3A, 4). First, most
 737 of the order manipulations we carried out did *not* reliably affect overall recall perfor-
 738 mance. Second, most of the order manipulations increased participants' tendencies to
 739 temporally cluster their recalls. Third, all of the order manipulations enhanced partici-
 740 pants' clustering of each condition's target feature (i.e., semantic manipulations enhanced
 741 semantic clustering, lexicographic manipulations enhanced lexicographic clustering, and
 742 visual manipulations enhanced visual clustering; Fig. 5C) while leaving clustering along
 743 other feature dimensions roughly unchanged (i.e., semantic manipulations did not affect
 744 lexicographic or visual clustering, and so on). Although it is not possible to fully separate
 745 feature versus temporal clustering when considering sorted lists, we used a permutation-
 746 based procedure to identify the degree of feature clustering over and above what could
 747 be accounted for by temporal clustering alone (see *Factoring out the effects of temporal clus-*
 748 *tering*). When we carried out this analysis (Fig. 5D), we found that participants exhibited
 749 more semantic clustering on semantically sorted lists than on randomly ordered lists (cat-
 750 egory: $t(XXX) = XXX, p = XXX$; size: $t(XXX) = XXX, p = XXX$), but the effects of the
 751 other order manipulations could not reliably be separated from temporal clustering alone
 752 ($ts > XXX, ps > XXX$).

753 When we closely examined the sequences of words participants recalled from early
 754 order-manipulated lists (Fig. 3A, top panel), we noticed several differences from the dy-
 755 namics of participants' recalls of randomly ordered lists (Figs. S1, S7). One difference is
 756 that participants in the category condition (dark purple curves, Fig. 3) most often initiated
 757 recall with the fourth-from-last item (*Recall initiation*, top left panel), whereas participants
 758 who recalled randomly ordered lists tended to initiate recall with either the first or last list
 759 items (Fig. S1, top left panel). We hypothesized that the participants might be "clumping"

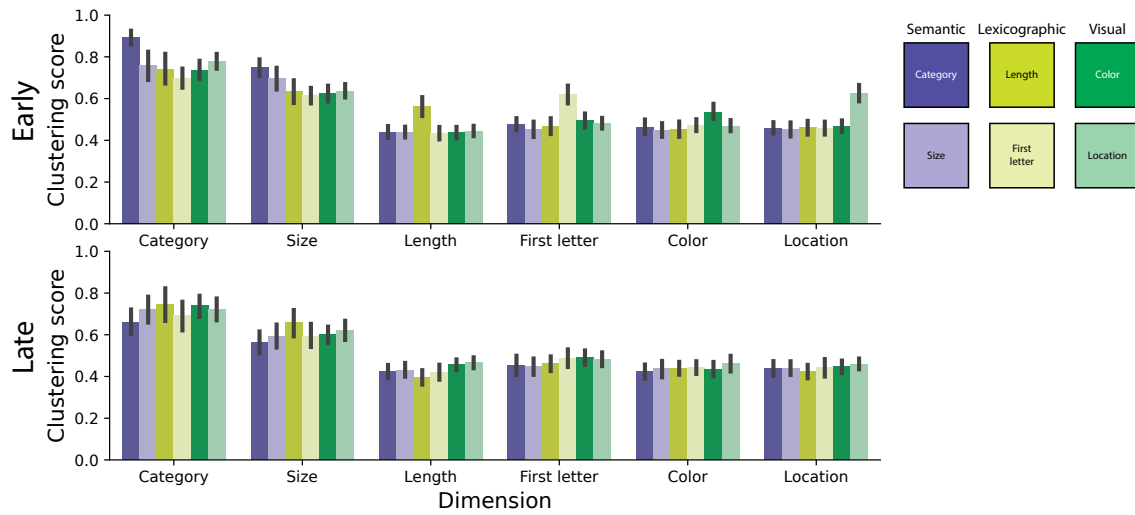


Figure 4: Memory “fingerprints” (order manipulation conditions). The across-participant clustering scores for each feature type (x -coordinate) are displayed for each experimental condition (color), separately for order manipulation (early, top) and randomly ordered (late, bottom) lists. Error bars denote bootstrap-estimated 95% confidence intervals. See Figures S5 and S6 for analogous plots for the random and adaptive conditions.

760 their recalls into groups of items that shared category labels. Indeed, when we com-
 761 pared the positions of feature changes in the study sequence (Fig. 3B; see *Identifying event*
 762 *boundaries*) with the positions of items participants recalled first, we noticed a striking
 763 correspondence in both semantic conditions. Specifically, on category-ordered lists, the
 764 category labels changed every four items on average (dark purple peaks in Fig. 3B), and
 765 participants also seemed to display an increased tendency (relative to other order manipu-
 766 lation and random conditions) to initiate recall of category-ordered lists with items whose
 767 study positions were integer multiples of four. Similarly, for size-ordered lists, the size la-
 768 bels changed every eight items on average (light purple peaks in Fig. 3B), and participants
 769 also seemed to display an increased tendency to initiate recall of size-ordered lists with
 770 items whose study positions were integer multiples of eight. A second striking difference
 771 is that participants in the category condition exhibited a much steeper lag-CRP (Fig. 3A,

top middle panel) than participants in other conditions. (This is another expression of participants' increased tendencies to temporally cluster their recalls on category-ordered lists, as we reported above.) Taken together, these order-specific idiosyncrasies suggest a hierarchical set of influences on participants' memories. At longer timescales, "event boundaries" (to use the term loosely) can be induced across lists by adding or removing incidental visual features. At shorter timescales, "event boundaries" can be induced across items (within a single list) by adjusting how item features change throughout the list.

The above comparisons between memory performance on early lists in the order manipulation versus feature rich conditions highlight how sorted lists are remembered differently from random lists. We also wondered how sorting lists along each feature dimension influenced memory relative to sorting lists along the other feature dimensions. Participants trended towards remembering early lists that were sorted semantically better than lexicographically sorted lists ($t(118) = 1.936, p = 0.055$). Participants also remembered visually sorted lists better than lexicographically sorted lists ($t(119) = 2.145, p = 0.034$). However, participants showed no reliable differences in recall for semantically versus visually sorted lists ($t(119) = 0.113, p = 0.910$). Participants temporally clustered semantically sorted lists more strongly than either lexicographically ($t(118) = 5.572, p < 0.001$) or visually ($t(119) = 6.215, p < 0.001$) sorted lists, but did not show reliable differences in temporal clustering on lexicographically versus visually sorted lists ($t(119) = 0.189, p = 0.850$). Participants also showed reliably more semantic clustering on semantically sorted lists than lexicographically (category: $t(118) = 3.492, p = 0.001$, size: $t(118) = 3.972, p < 0.001$) or visually (category: $t(119) = 2.702, p = 0.008$, size: $t(119) = 4.230, p < 0.001$) sorted lists; more lexicographic clustering on lexicographically sorted lists than semantically (length: $t(118) = 3.112, p = 0.002$; first letter: $t(118) = 3.686, p < 0.001$) or visually (length: $t(119) = 3.024, p = 0.003$; first letter: $t(119) = 2.644, p = 0.009$) sorted lists; and more visual

797 clustering on visually sorted lists than semantically (color: $t(119) = -2.659, p = 0.009$;
798 location: $t(119) = -4.604, p < 0.001$) or lexicographically (color: $t(119) = -2.366, p = 0.020$;
799 location: $t(119) = -4.265, p < 0.001$) sorted lists. In summary, sorting lists by different
800 features appeared to have slightly different effects on overall memory performance and
801 temporal clustering. Participants also tended to cluster their recalls along a given fea-
802 ture dimension more when the studied lists were (versus were not) sorted along that
803 dimension.

804 Beyond affecting how we process and remember *ongoing* experiences, what is happen-
805 ing to us now can also affect how we process and remember *future* experiences. Within
806 the framework of our study, we wondered: if early lists are sorted along different feature
807 dimensions, might this affect how people remember later (random) lists? In exploring this
808 question, we considered both group-level effects (i.e., effects that tended to be common
809 across individuals) and participant-level effects (i.e., effects that were idiosyncratic across
810 individuals).

811 At the group level, there seemed to be almost no lingering impact of sorting early
812 lists on memory for later lists. To simplify the presentation, we report these null results
813 in aggregate across the three feature groupings. Relative to memory performance on
814 late feature rich lists, participants' memory performance in all six order manipulation
815 conditions showed no reliable differences (semantic: $t(125) = 0.487, p = 0.627$; lexico-
816 graphic: $t(125) = 0.878, p = 0.382$; visual: $t(126) = 1.437, p = 0.153$). Nor did we observe
817 any reliable differences in temporal clustering on late lists (relative to late feature rich
818 lists; semantic: $t(125) = 0.146, p = 0.884$; lexicographic: $t(125) = 0.923, p = 0.358$; visual:
819 $t(126) = 0.525, p = 0.601$). Aside from a slightly increased tendency for participants to
820 cluster words by their length on late visual order manipulation lists (more than late fea-
821 ture rich lists; $t(126) = 2.199, p = 0.030$), we observed no reliable differences in any type of

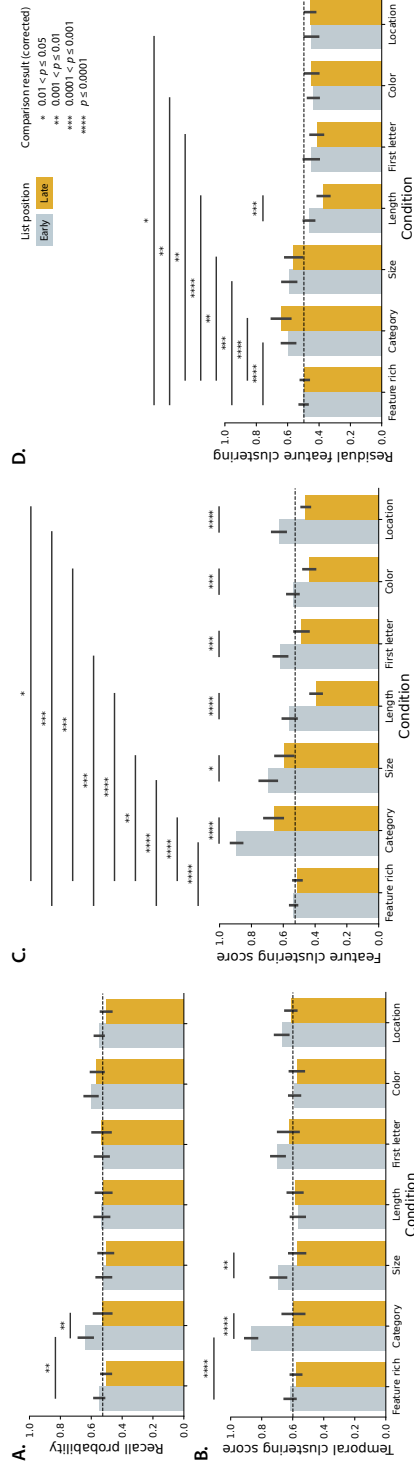


Figure 5: Recall probability and clustering scores on early and late lists. The bar heights display the average (across participants) recall probabilities (A.), temporal clustering scores (B.), feature clustering scores (C.), and residual feature clustering scores (after factoring out temporal clustering effects; D.) for early (gray) and late (gold) lists. For the feature rich bars (left), the feature clustering scores are averaged across all feature dimensions. For the order manipulation conditions, feature clustering scores are displayed for the focused-on feature for each condition (e.g., category clustering scores are displayed for the category condition, and so on). All panels: error bars denote bootstrap-estimated 95% confidence intervals. The horizontal dotted lines denote the average values (across all lists and participants) for the feature rich condition. The bars denote t -tests between the corresponding bars, and the asterisks denote the Benjamini-Hochberg-corrected p -values. Comparisons for which corrected $p \geq 0.05$ are not shown.

feature clustering on late order manipulation condition lists versus late feature rich lists ($\|t\|_s \leq 1.234, p_s \geq 0.220$).

We also looked for more subtle group-level patterns. For example, perhaps sorting early lists by one feature dimension could affect how participants cluster *other* features (on early and/or late lists) as well. We defined participants' *memory fingerprints* as the set of their temporal and feature clustering scores (see *Memory fingerprints*). A participant's memory fingerprint describes how they tend to retrieve memories of the studied items, perhaps searching in parallel through several feature spaces (or along several representational dimensions). To gain insights into the dynamics of how participants' clustering scores tended to change over time, we computed the average (across participants) fingerprint from each list, from each order manipulation condition (Fig. 6). We projected these fingerprints into a two-dimensional space to help visualize the dynamics (top panels; see *Computing low-dimensional embeddings of memory fingerprints*). We found that participants' average fingerprints tended to remain relatively stable on early lists, and exhibited a "jump" to another stable state on later lists. The sizes of these jumps varied somewhat across conditions (the Euclidean distances between fingerprints in their original high dimensional spaces are displayed in the bottom panels). We also averaged the fingerprints across early and late lists, respectively, for each condition (Fig. 6B). We found that participants' fingerprints on early lists seem to be influenced by the order manipulations for those lists (see the locations of the circles in Fig. 6B). There also seemed to be some consistency across different features within a broader type. For example, both semantic feature conditions (category and size; purple markers) diverge in a similar direction from the group; both lexicographic feature conditions (length and first letter; yellow markers) diverge in a similar direction; and both visual conditions (color and location; green) also diverge in a similar direction. But on late lists, participants' fingerprints seem to return

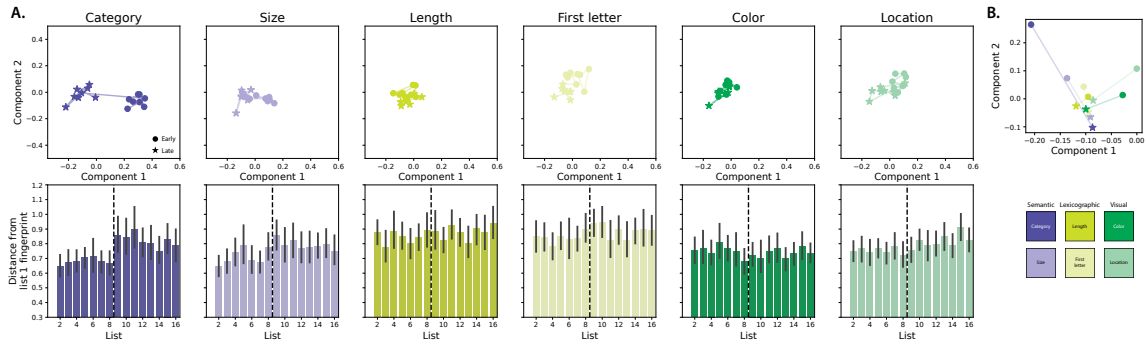


Figure 6: Memory fingerprint dynamics (order manipulation conditions). **A.** Each column (and color) reflects an experimental condition. In the top panels, each marker displays a 2D projection of the (across-participant) average memory fingerprint for one list. Order manipulation (early) lists are denoted by circles and randomly ordered (late) lists are denoted by stars. All of the fingerprints (across all conditions and lists) are projected into a common space. The bar plots in the bottom panels display the Euclidean distances of the per-list memory fingerprints to the list 0 fingerprint, for each condition. Error bars denote bootstrap-estimated 95% confidence intervals. The dotted vertical lines denote the boundaries between early and late lists. **B.** In this panel, the fingerprints for early (circle) and late (star) lists are averaged across lists and participants before projecting the fingerprints into a (new) 2D space. See Figure S8 for analogous plots for the random conditions.

847 to a common state that is roughly shared across conditions (i.e., the stars in that panel are
848 clumped together).

849 When we examined the data at the level of individual participants (Figs. 7 and 8), a
850 clearer story emerged. Within each order manipulation condition, participants exhibited
851 a range of feature clustering scores on both early and late lists (Fig. 7A, B). Across every
852 order manipulation condition, participants who exhibited stronger feature clustering (for
853 their condition's manipulated feature) recalled more words. This trend held overall across
854 conditions and participants (early: $r(179) = 0.537, p < 0.001$; late: $r(179) = 0.492, p < 0.001$)
855 as well as for each condition individually for early ($r_s \geq 0.386$, all $p_s \leq 0.035$) and late
856 ($r_s \geq 0.462$, all $p_s \leq 0.010$) lists. We found no evidence of a condition-level trend; for
857 example, the conditions where participants tended to show stronger clustering scores
858 were not correlated with the conditions where participants remembered more words
859 (early: $r(4) = 0.526, p = 0.284$; late: $r(4) = -0.257, p = 0.623$; see insets of Fig. 7A and B).

860 We observed carryover associations between feature clustering and recall performance
 861 (Fig. 7C, D). Participants who showed stronger feature clustering on early lists tended to
 862 recall more items on late lists (across conditions: $r(179) = 0.492, p < 0.001$; all conditions
 863 individually: $r_s \geq 0.462$, all $p_s \leq 0.010$). Participants who recalled more items on early lists
 864 also tended to show stronger feature clustering on late lists (across conditions: $r(179) =$
 865 $0.280, p < 0.001$; all non-visual conditions: $r_s \geq 0.445$, all $p_s \leq 0.014$; color: $r(29) = 0.298, p =$
 866 0.103 ; location: $r(28) = 0.354, p = 0.055$). Neither of these effects showed condition-level
 867 trends (early feature clustering versus late recall probability: $r(4) = -0.299, p = 0.565$;
 868 early recall probability versus late feature clustering: $r(4) = 0.400, p = 0.432$). We also
 869 looked for associations between feature clustering and temporal clustering. Across every
 870 order manipulation condition, participants who exhibited stronger feature clustering also
 871 exhibited stronger temporal clustering. For early lists (Fig. 7E), this trend held overall
 872 ($r(179) = 0.924, p < 0.001$), for each condition individually (all $r_s \geq 0.822$, all $p_s < 0.001$),
 873 and across conditions ($r(4) = 0.964, p = 0.002$). For late lists (Fig. 7F), the results were more
 874 variable (overall: $r(179) = 0.348, p < 0.001$; all non-visual conditions: $r_s \geq 0.382$, all p_s
 875 ≤ 0.037 ; color: $r(29) = 0.453, p = 0.011$; location: $r(28) = 0.190, p = 0.314$; across-conditions:
 876 $r(4) = -0.036, p = 0.945$). While less robust than the carryover associations between feature
 877 clustering and recall performance, we also observed some carryover associations between
 878 feature clustering and temporal clustering (Fig. 7G, H). Participants who showed stronger
 879 feature clustering on early lists trended towards showing stronger temporal clustering
 880 on later lists (overall: $r(179) = 0.301, p < 0.001$; for individual conditions: all $r_s \geq 0.297$,
 881 all $p_s \leq 0.111$; across conditions: $r(4) = 0.107, p = 0.840$). And participants who showed
 882 stronger temporal clustering on early lists trended towards showing stronger feature
 883 clustering on later lists (overall: $r(179) = 0.579, p < 0.001$; all non-visual conditions: r_s
 884 ≥ 0.323 , all $p_s \leq 0.082$; visual conditions: $r_s \geq 0.089$, all $p_s \leq 0.632$; across conditions:

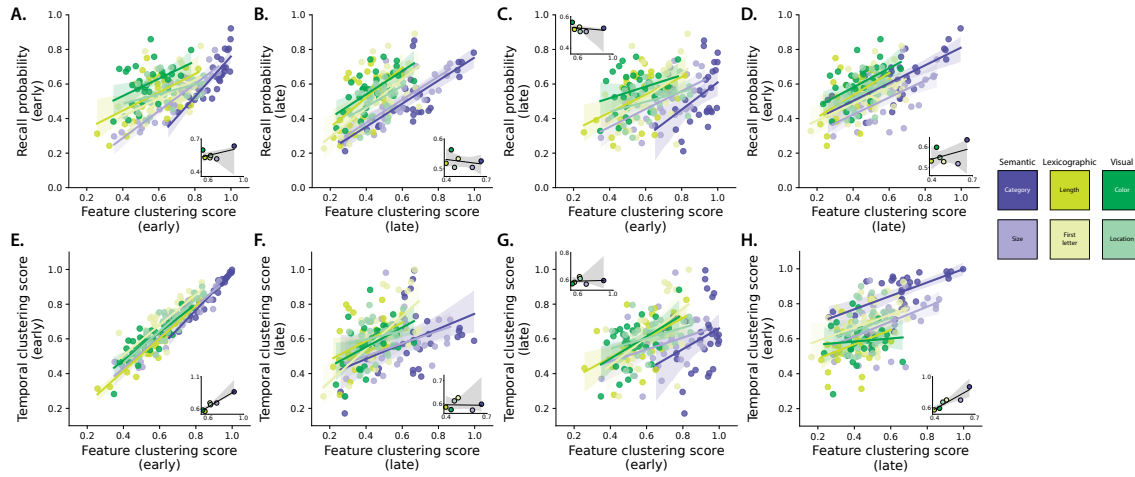


Figure 7: Interactions between feature clustering, recall probability, and contiguity. **A.** Recall probability versus feature clustering scores for order manipulation (early) lists. **B.** Recall probability versus feature clustering for randomly ordered (late) lists. **C.** Recall probability on late lists versus feature clustering on early lists. **D.** Recall probability on early lists versus feature clustering on late lists. **E.** Temporal clustering scores (contiguity) versus feature clustering scores on early lists. **F.** Temporal clustering scores versus feature clustering scores on late lists. **G.** Temporal clustering scores on late lists versus feature clustering scores on early lists. **H.** Temporal clustering scores on early lists versus feature clustering scores on late lists. **All panels.** Each dot in the main scatterplots denotes the average scores for one participant. The colored regression lines are computed across participants. The inset displays condition-averaged results, where each dot reflects a single condition and the regression line is computed across experimental conditions. All error ribbons denote bootstrap-estimated 95% confidence intervals.

885 $r(4) = 0.916, p = 0.010$). Taken together, the results displayed in Figure 7 show that
 886 participants who were more sensitive to the order manipulations (i.e., participants who
 887 showed stronger feature clustering for their condition's feature on early lists) remembered
 888 more words and showed stronger temporal clustering. These associations also appeared
 889 to carry over across lists, even when the items on later lists were presented in a random
 890 order.

891 If participants show different sensitivities to order manipulations, how do their be-
 892 haviors carry over to later lists? We found that participants who showed strong fea-
 893 ture clustering on early lists often tended to show strong feature clustering on late lists

(Fig. 8A; overall across participants and conditions: $r(179) = 0.592, p < 0.001$; non-visual feature conditions: all $r_s \geq 0.350$, all $p_s \leq 0.058$; color: $r(29) = -0.071, p = 0.704$; location: $r(28) = 0.032, p = 0.868$; across conditions: $r(4) = 0.934, p = 0.006$). Although participants tended to show weaker feature clustering on late lists (Fig. 6) on *average*, the associations between early and late lists for individual participants suggests that some influence of early order manipulations may linger on late lists. We found that participants who exhibited larger carryover in feature clustering (i.e., continued to show strong feature clustering on late lists) for the semantic order manipulations (but not other manipulations) also tended to show a smaller decrease in recall on early versus late lists (Fig. 8B; overall: $r(179) = 0.378, p < 0.001$; category: $r(28) = 0.419, p = 0.021$; size: $r(28) = 0.737, p < 0.001$; non-semantic conditions: all $r_s \leq 0.252$, all $p_s \geq 0.179$; across conditions: $r(4) = 0.773, p = 0.072$) on late lists, relative to early lists. Participants who exhibited larger carryover in feature clustering also tended to show stronger temporal clustering on late lists (relative to early lists) for all but the category condition (Fig. 8C; overall: $r(179) = 0.434, p < 0.001$; category: $r(28) = 0.229, p = 0.223$; all non-category conditions: all $r_s \geq 0.448$, all $p_s \leq 0.012$; across conditions: $r(4) = 0.598, p = 0.210$).

We suggest two potential interpretations of these findings. First, it is possible that some participants are more “malleable” or “adaptable” with respect to how they organize incoming information. When presented with list of items sorted along *any* feature dimension, they will simply adopt that feature as a dominant dimension for organizing those items and subsequent (randomly ordered) items. This flexibility in memory organization might afford such participants a memory advantage, explaining their strong recall performance. An alternative interpretation is that each participant comes into our study with a “preferred” way of organizing incoming information. If they happen to be assigned to an order manipulation condition that matches their preferences, then they will appear to be

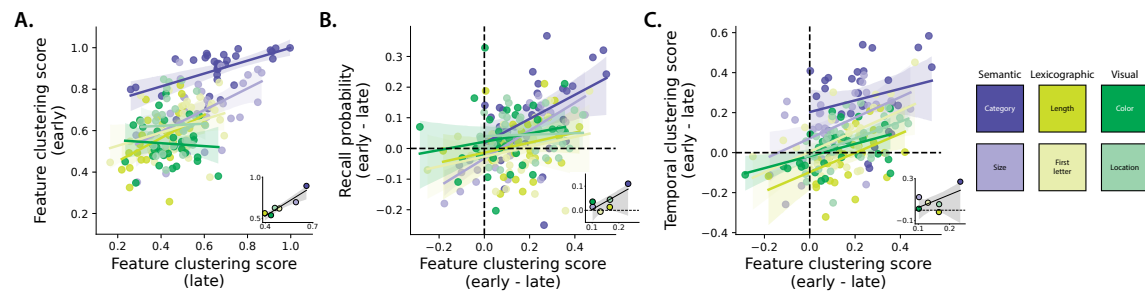


Figure 8: Feature clustering carryover effects. **A.** Feature clustering scores for order manipulation (early) versus randomly ordered (late) lists. **B.** Accuracy differences (on early versus late lists) versus feature clustering “carryover” (defined as the differences between the average clustering scores on early and late lists). **C.** Temporal clustering differences (on early versus late lists) versus feature clustering carryover. **All panels.** Each dot in the main scatterplots denotes the average scores for one participant. The colored regression lines are computed across participants. The inset displays condition-averaged results, where each dot reflects a single condition and the regression line is computed across experimental conditions. All error ribbons denote bootstrap-estimated 95% confidence intervals.

919 “sensitive” to the order manipulation and also exhibit a high degree of carryover in feature
 920 clustering from early to late lists. These participants might demonstrate strong recall per-
 921 formance not because of their inherently superior memory abilities, but rather because the
 922 specific condition they were assigned to happened to be especially easy for them, given
 923 their pre-experimental tendencies. To help distinguish between these interpretations, we
 924 designed an *adaptive* experimental condition (see *Adaptive condition*). The primary ma-
 925 nipulation in the adaptive condition is that participants each experience three key types
 926 of lists. On *random* lists, words are ordered randomly (as in the feature rich condition).
 927 On *stabilize* lists, the presentation order is adjusted to be maximally similar to the current
 928 estimate of the participant’s memory fingerprint (see *Online “fingerprint” analysis*). Third,
 929 on *destabilize* lists, the presentation order is adjusted to be *minimally* similar to the current
 930 estimate of the participant’s memory fingerprint (see *Ordering “stabilize” and “destabilize”*
 931 *lists by an estimated fingerprint*). The orders in which participants experienced each type
 932 of list were counterbalanced across participants to help reduce the influence of potential

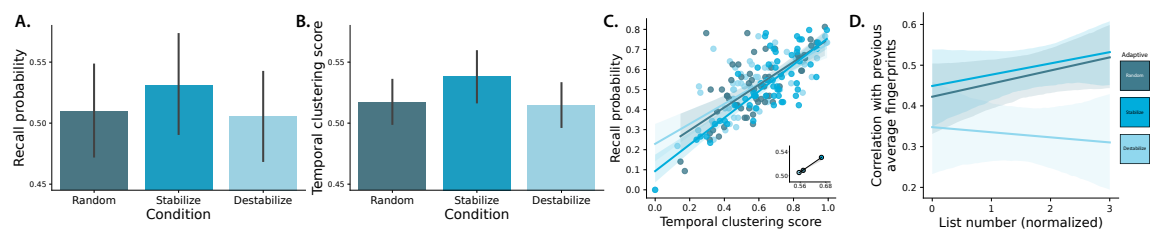


Figure 9: Adaptive free recall. **A.** Average probability of recall (taken across words, lists, and participants) for lists from each adaptive condition. **B.** Average temporal clustering scores for lists from each adaptive condition. **C.** Recall probability versus temporal clustering scores by participant (main panel; each participant contributes one dot per condition) and averaged within condition (inset; each dot represents a single condition). **D.** Per-list correlations between the current list’s fingerprint and the average fingerprint computed from all previous lists. The normalized list numbers (x-axis) denote the number of lists of the same type that the participant had experienced at the time of the current list. All panels: Colors denote the sorting type (condition) for each list. Error bars and ribbons denote bootstrap-estimated 95% confidence intervals. For additional details about participants’ behavior and performance during the adaptive conditions, see Figure S2.

list-order effects. Because the presentation orders on stabilize and destabilize lists are adjusted to best match each participant’s (potentially unique) memory fingerprint, the adaptive condition removes uncertainty about whether participants’ assigned conditions might just “happen” to match their preferred ways of organizing their memories.

Participants’ fingerprints on stabilize and random lists tended to become (numerically) slightly more similar to their average fingerprints computed from the previous lists they had experienced, and their fingerprints on destabilize lists tended to become numerically less similar (Fig. 9D). Overall, we found that participants tended to be better at remembering words on stabilize lists relative to words on both random ($t(59) = 1.740, p = 0.087$) and destabilize ($t(59) = 1.714, p = 0.092$) lists (Fig. 9A). Participants showed no reliable differences in their memory performance on destabilize versus random lists ($t(59) = -0.249, p = 0.804$). Participants also exhibited stronger temporal clustering on stabilize lists, relative to random ($t(59) = 3.554, p = 0.001$) and destabilize ($t(59) = 4.045, p < 0.001$) lists (Fig. 9B). We found no reliable differences in temporal clustering for items on random versus destabilize lists ($t(59) = -0.781, p = 0.438$).

948 As in the other experimental manipulations, participants in the adaptive condition
949 exhibited substantial variability with respect to their overall memory performance and
950 their clustering tendencies (Fig. 9C). We found that individual participants who exhibited
951 strong temporal clustering scores also tended to recall more items. This held across
952 subjects, aggregating across all list types ($r(178) = 0.721, p < 0.001$), and for each list type
953 individually (all $r_s \geq 0.683$, all $p_s \leq 0.001$). Taken together, the results from the adaptive
954 condition suggest that each participant comes into the experiment with their own unique
955 memory organization tendencies, as characterized by their memory fingerprint. When
956 participants study lists whose items come pre-sorted according to their unique preferences,
957 they tend to remember more and show stronger temporal clustering.

958 Discussion

959 We asked participants to study and freely recall word lists. The words on each list (and
960 the total set of lists) were held constant across participants. For each word, we considered
961 (and manipulated) two semantic features (category and size) that reflected aspects of the
962 *meanings* of the words, along with two lexicographic features (word length and first letter),
963 which reflected characteristics of the words' *letters*. These semantic and lexicographic
964 features are intrinsic to each word. We also considered and manipulated two additional
965 visual features (color and location) that affected the *appearance* of each studied item, but
966 could be varied independently of the words' identities. Across different experimental
967 conditions, we manipulated how the visual features varied across words (within each
968 list), along with the orders of each list's words. Although the participants' task (verbally
969 recalling as many words as possible, in any order, within one minute) remained constant
970 across all of these conditions, and although the set of words they studied from each list
971 remained constant, our manipulations substantially affected participants' memories. The

972 impact of some of the manipulations also affected how participants remembered *future*
973 lists that were sorted randomly.

974 **Recap: visual feature manipulations**

975 We found that participants in our feature rich condition (where we varied words' ap-
976 pearances) recalled similar proportions of words to participants in a reduced condition
977 (where appearance was held constant across words). However, varying the words' ap-
978 pearances led participants to exhibit much more temporal and feature-based clustering.
979 This suggests that even seemingly irrelevant elements of our experiences can affect how
980 we remember them.

981 When we held the within-list variability in participants' visual experiences fixed across
982 lists (in the feature rich and reduced conditions), they remembered more words from early
983 lists than from late lists. For feature rich lists, they also showed stronger clustering for early
984 versus late lists. However, when we *varied* participants' visual experiences across lists (in
985 the "reduced (early)" and "reduced (late)" conditions), these early versus late accuracy
986 and clustering differences disappeared. Abruptly changing how incidental visual features
987 varied across words seemed to act as a sort of "event boundary" that partially reset how
988 participants processed and remembered post-boundary lists. Within-list clustering also
989 increased in these manipulations, suggesting that the "within-event" words were being
990 more tightly associated with each other.

991 When we held the visual features constant during early lists, but then varied words'
992 appearances in later lists (i.e., the reduced (early) condition), participants' overall memory
993 performance improved. However, this impact was directional: when we *removed* visual
994 features from words in late lists that had been present in early lists (i.e., the reduced (late)
995 condition), we saw no memory improvement.

996 **Recap: order manipulations**

997 When we (stochastically) sorted early lists along different feature dimensions, we found
998 several impacts on participants' memories. Sorting early lists semantically (by word cat-
999 egory) enhanced participants' memories for those lists, but the effects on performance of
1000 sorting along other feature dimensions were inconclusive. However, each order manipu-
1001 lation substantially affected how participants *organized* their memories of words from the
1002 ordered lists. When we sorted lists semantically, participants displayed stronger semantic
1003 clustering; when we sorted lists lexicographically, they displayed stronger lexicographic
1004 clustering; and when we sorted lists visually, they displayed stronger visual clustering.
1005 Clustering along the unmanipulated feature dimensions in each of these cases was un-
1006 changed.

1007 The order manipulations we examined also appeared to induce, in some cases, a
1008 tendency to "clump" similar words within a list. This was most apparent on semantically
1009 ordered lists, where the probability of initiating recall with a given word seemed to follow
1010 groupings defined by feature change points.

1011 We also examined the impact of early list order manipulations on memory for late
1012 lists. At the group level, we found little evidence for lingering "carryover" effects of
1013 these manipulations: participants in the order manipulation conditions showed similar
1014 memory performance and clustering on late lists to participants in the corresponding
1015 control (feature rich) condition. At the level of individual participants, however, we
1016 found several meaningful patterns.

1017 Participants who showed stronger feature clustering on early (order-manipulated) lists
1018 tended to better remember late (randomly ordered) lists. Participants who remembered
1019 early lists better also tended to show stronger feature clustering (along their condition's
1020 feature dimension) on late lists (even though the words on those late lists were presented

1021 in a random order). We also observed some (weaker) carryover effects of temporal cluster-
1022 ing. Participants who showed stronger feature clustering (along their condition's feature
1023 dimension) on early lists tended to show stronger temporal clustering on late lists. And
1024 participants who showed stronger temporal clustering on early lists also tended to show
1025 stronger feature clustering on late lists. Essentially, these order manipulations appeared to
1026 affect each participant differently. Some participants were sensitive to our manipulations,
1027 and those participants' memory performance was impacted more strongly, both for the
1028 ordered lists and for future (random) lists. Other participants appeared relatively insen-
1029 sitive to our manipulations, and those participants showed little carryover effects on late
1030 lists.

1031 These results at the individual participant level suggested to us that either (a) some
1032 participants were more sensitive to *any* order manipulation, or (b) some participants might
1033 be more (or less) sensitive to manipulations along *particular* (e.g., preferred) feature dimen-
1034 sions. To help distinguish between these possibilities, we designed an adaptive condition
1035 whereby we attempted to manipulate whether participants studied words in an order that
1036 either matched or mismatched our estimate of how they would cluster or organize the
1037 studied words in memory (i.e., their idiosyncratic memory fingerprint). We found that
1038 when we presented words in orders that were consistent with participants' memory fin-
1039 gerprints, they remembered more words overall and showed stronger temporal clustering.
1040 This comports well with the second possibility described above. Specifically, each partici-
1041 pant seems to bring into the experiment their own idiosyncratic preferences and strategies
1042 for organizing the words in their memory. When we presented the words in an order
1043 consistent with each participant's idiosyncratic fingerprint, their memory performance
1044 improved. This might indicate that the participants were spending less cognitive effort
1045 "reorganizing" the incoming words on those lists, which freed up resources to devote to

1046 encoding processes instead.

1047 **Memory consequences of feature variability**

1048 Several prior studies have examined how varying the richness or experiences, or the
1049 extensive of encoding, can affect memory. Although specific details differ (Bonin et al.,
1050 2022), in general these studies have found that richer and more deeply or extensively
1051 encoded experiences are remembered better (Hargreaves et al., 2012; Madan, 2021; Mein-
1052 hardt et al., 2020). Our findings help to elucidate an additional factor that may contribute
1053 to these phenomenon. For example, our finding that participants better remember “fea-
1054 ture rich” lists (where words’ appearances are varied) than “reduced” lists (where words’
1055 appearances are held constant) only when those feature rich lists are presented *after* re-
1056 duced lists suggests that some factors that influence the richness or depth of encoding
1057 may be relative, rather than absolute. In other words, *increases* in richness (e.g., relative
1058 to a recency-weighted baseline) may be more important than the overall complexity or
1059 numbers of features.

1060 Some prior studies have suggested that people can “cue” their memories using different
1061 “strategies” or “pathways” for searching for the target information. For example, modern
1062 accounts of free recall typically posit that memory search typically begins by matching
1063 the current state of mental context with the contexts associated with other items in mem-
1064 ory (Kahana, 2020). Since context is the defining hallmark of episodic memory (Tulving,
1065 1983), context-based search can be described as an “episodic” pathway to recall. When
1066 episodic cueing fails to elicit a match, participants may then search for items that are simi-
1067 lar to the current mental context or mental state along other dimensions, such as semantic
1068 similarity (Davachi et al., 2003; Socher et al., 2009). These multiple pathways accounts of
1069 memory search also provide a potential explanation of why participants might have an

1070 easier time remembering richer stimuli (or experiences): richer stimuli and experiences
1071 might have more features that could be used to cue memory search. Our work suggests
1072 that there may be some additional factors at play with respect to the *dynamics* of these pro-
1073 cesses. In particular, we only observed memory benefits for “richer” stimuli when they
1074 were encountered after more “impoverished” stimuli (in the reduced (early) condition).
1075 This suggests that the pathways available to recall a given item may also depend on recent
1076 prior experiences.

1077 We did *not* find any evidence that changing words’ appearances *harmed* memory per-
1078 formance, e.g., by distracting them with irrelevant information (Lange, 2005; Marsh et al.,
1079 2012, 2015; Reinitz et al., 1992). Nor did we find any evidence that *changes* in the presence
1080 of potentially “distracting” features adversely affected memory. For example, when we
1081 increased or decreased the variability in words’ appearances on late versus early lists (as in
1082 the reduced (early) and reduced (late) conditions), we found no evidence that this harmed
1083 participants’ memories. One potential interpretation under the “multiple pathways to
1084 recall” framework is that the availability of multiple pathways to recall do not appear to
1085 specifically interfere with each other.

1086 **Context effects on memory performance and organization**

1087 In real-world experience, each moment’s unique blend of contextual features (where we
1088 are, who we are with, what else we are thinking of at the time, what else we experience
1089 nearby in time, etc.) plays an important role in how we interpret, experience, and re-
1090 member that moment, and how we relate it to our other experiences (e.g., for review see
1091 Manning, 2020). What are the analogues of real-world contexts in laboratory tasks like
1092 the free recall paradigm employed in our study? In general, modern formal accounts of
1093 free recall (Kahana, 2020) describe context as comprising a mix of (a) features pertaining

1094 to or associated with each item and (b) other items and thoughts experienced nearby in
1095 time, e.g., that might still be “lingering” in the participant’s thoughts at the time they
1096 study the item. Item features can include semantic properties (i.e., features related to the
1097 item’s meaning), lexicographic properties (i.e., features related to the item’s letters), sen-
1098 sory properties (i.e., feature related to the item’s appearance, sound, smell, etc.), emotional
1099 properties (i.e., features related to how meaningful the item is, whether the item evokes
1100 positive or negative feelings, etc.), utility-related properties (e.g., features that describe
1101 how an item might be used or incorporated into a particular task or situation), and more.
1102 Essentially any aspect of the participant’s experience that can be characterized, measured,
1103 or otherwise described can be considered to influence the participant’s mental context at
1104 the moment they experience that item. Temporally proximal features include aspects of
1105 the participant’s internal or external experience that are *not* specifically occurring at the
1106 moment they encounter an item, but that nonetheless influence how they process the item.
1107 Thoughts related to percepts, goals, expectations, other experiences, and so on that might
1108 have been cued (directly or indirectly) by the participant’s recent experiences prior to the
1109 current moment all fall into this category. Internally driven mental states, such as thinking
1110 about an experience unrelated to the experiment, also fall into this category.

1111 Contextual features need not be intentionally or consciously perceived by the partic-
1112 ipant to affect memory, nor do they need to be relevant to the task instructions or the
1113 participant’s goals. Incidental factors such as font color (Jones and Pyc, 2014), background
1114 color (Isarida and Isarida, 2007), inter-stimulus images (Chiu et al., 2021; Gershman et al.,
1115 2013; Manning et al., 2016), background sounds (Sahakyan and Smith, 2014; ?), secondary
1116 tasks (Masicampo and Sahakyan, 2014; Oberauer and Lewandowsky, 2008; Polyn et al.,
1117 2009), and more can all impact how participants remember, and organize in memory, lists
1118 of studied items.

1119 Consistent with this prior work, we found that participants were sensitive to task-
1120 irrelevant visual features. We also found that changing the dynamics of those task-
1121 irrelevant visual features (in the reduced (early) and reduced (late) conditions) *also* affected
1122 participants' memories. This suggests that it is not only the contextual features themselves
1123 that affect memory, but also the *dynamics* of context—i.e., how the contextual features
1124 associated with each item change over time.

1125 **Priming effects on memory performance and organization**

1126 When our ongoing experiences are ambiguous, we can draw on our past experiences,
1127 expectations, and other real, perceived, or inferred cues to help resolve these ambiguities.
1128 We may also be overtly or covertly “primed” to influence how we are likely to resolve
1129 ambiguities. For example, before listening to a story with several equally plausible inter-
1130 pretations, providing participants with “background” information beforehand can lead
1131 them towards one interpretation versus another (Yeshurun et al., 2017). More broadly, our
1132 conscious and unconscious biases and preferences can influence not only how we interpret
1133 high-level ambiguities, but even how we process low-level sensory information (Katabi
1134 et al., 2023).

1135 In more simplified scenarios, like list-learning paradigms, the stimuli and tasks partic-
1136 ipants encounter before studying a given list can influence what and how they remember.
1137 For example, when participants are directed to suppress, disregard, or ignore “distracting”
1138 stimuli early on in an experiment, participants often tend to remember those stimuli less
1139 well when they are re-used as to-be-remembered targets later on in the experiment (Tip-
1140 per, 1985). In general, participants' memories can be influenced by exposing them to
1141 a wide range of positive and negative priming factors before they encounter the to-be-
1142 remembered information (Balota et al., 1992; Clayton and Chattin, 1989; Donnelly, 1988;

1143 Flexser and Tulving, 1982; Gotts et al., 2012; Huang et al., 2004; Huber, 2008; Huber et al.,
1144 2001; McNamara, 1994; Neely, 1977; Rabinowitz, 1986; Tulving and Schacter, 1991; Watkins
1145 et al., 1992; Wiggs and Martin, 1998).

1146 The order manipulation conditions in our experiment show that participants can also be
1147 primed to pick up on more subtle statistical structure in their experiences, like the dynamics
1148 of how the presentation orders of stimuli vary along particular feature dimensions. These
1149 order manipulations affected not only how participants remembered the manipulated
1150 lists, but also how they remembered *future* lists with different (randomized) temporal
1151 properties.

1152 **Free recall of blocked versus random categorized word lists**

1153 A large number of prior studies have compared participants' memories for categorized
1154 word lists that are presented in blocked versus random orders. In "blocked" lists, all
1155 of the words from a given semantic category (e.g., animals) are presented consecutively,
1156 whereas in "random" lists, the words from different categories are intermixed. Most of
1157 these studies report that participants tend to better remember blocked (versus random)
1158 lists (Bower et al., 1969; Cofer et al., 1966; D'Agostino, 1969; Dallett, 1964; Kintsch, 1970;
1159 Luek et al., 1971; Puff, 1974; Shapiro, 1970; ?; ?). Other studies suggest that these order
1160 effects may also be modulated by factors like list length and the numbers of exemplars in
1161 each category (e.g., Borges and Mangler, 1972).

1162 Although we did not directly manipulate "blocking" in our order manipulation condi-
1163 tions, our sorting procedures in those conditions (see *Constructing feature-sorted lists*) have
1164 *indirect* effects on the lists' blockiness. For example, lists that are stochastically sorted by
1165 semantic category will tend to contain runs of several same-category words in succession.
1166 Consistent with the above work on blocked versus random categorized lists, we found

1167 that participants tended to better remember lists that were sorted semantically (Fig. 5B).
1168 However, this memory improvement did not appear to extend to the other order ma-
1169 nipulation conditions we considered (e.g., to lexicographically or visually sorted lists).
1170 One possibility is that the memory benefits of blocked versus random lists are specific to
1171 semantic categories, and do not generalize to other feature dimensions. Another possi-
1172 bility is that the memory benefits are due to the presence of infrequent “jumps” between
1173 successive items (e.g., from different categories). Because the features we manipulated in
1174 the lexicographic and visual conditions were less categorical than the semantic features,
1175 feature values across words in those conditions tended to vary more gradually. Relatively
1176 stable features that are punctuated by infrequent large changes (e.g., as words transition
1177 from a same-category sequence to a new category) may also relate to perceived “event
1178 boundaries,” which can have important consequences for memory (DuBrow and Davachi,
1179 2013, 2016; DuBrow et al., 2017; Radvansky and Zacks, 2017).

1180 **Expectation, event boundaries, and situation models**

1181 Our findings that participants’ current and future memory behaviors are sensitive to
1182 manipulations in which features change over time, and how features change across items
1183 and lists, suggest parallels with studies on how we form expectations and predictions,
1184 segment our continuous experiences into discrete events, and make sense of different
1185 scenarios and situations. Each of these real-world cognitive phenomena entail identifying
1186 statistical regularities in our experiences, and exploiting those regularities to gain insight,
1187 form inferences, organize or interpret memories, and so on. Our past experiences enable
1188 us to predict what is likely to happen in the future, given what happened “next” in our
1189 previous experiences that were similar to now (Barron et al., 2020; Brigard, 2012; Chow
1190 et al., 2016; Eichenbaum and Fortin, 2009; Gluck et al., 2002; Goldstein et al., 2021; Griffiths

1191 and Steyvers, 2003; Jones and Pashler, 2007; Kim et al., 2014; Manning, 2020; Tamir and
1192 Thornton, 2018; Xu et al., 2023).

1193 When our expectations are violated, such as when our observations disagree with our
1194 predictions, we may perceive the “rules” or “situation” to have changed. *Event boundaries*
1195 denote abrupt changes in the state of our experience, for example, when we transition
1196 from one situation to another (Radvansky and Zacks, 2017; Zwaan and Radvansky, 1998).
1197 Crossing an event boundary can impair our memory for pre-boundary information and en-
1198 hance our memory for post-boundary information (DuBrow and Davachi, 2013; Manning
1199 et al., 2016; Radvansky and Copeland, 2006; Sahakyan and Kelley, 2002). Event bound-
1200 aries are also tightly associated with the notion of *situation models* and *schemas*—mental
1201 frameworks for organizing our understanding about the rules of how we and others are
1202 likely to behave, how events are likely to unfold over time, how different elements are
1203 likely to interact, and so on. For example, a situation model pertaining to a particular
1204 restaurant might set our expectations about what we are likely to experience when we
1205 visit that restaurant (e.g., what the building will look like, how it will smell when we enter,
1206 how crowded the restaurant is likely to be, the sounds we are likely to hear, etc.). Similarly,
1207 as mentioned in the *Introduction*, we might learn a schema describing how events are likely
1208 to unfold *across* any sit-down restaurant—e.g., open the door, wait to be seated, receive a
1209 menu, decide what to order, place the order, and so on. Situation models and schemas can
1210 help us to generalize across our experiences, and to generate expectations about how new
1211 experiences are likely to unfold. When those expectations are violated, we can perceive
1212 ourselves to have crossed into a new situation.

1213 In our study, we found that abruptly changing the “rules” about how the visual
1214 appearances of words are determined, or about the orders in which words are presented,
1215 can lead participants to behave similarly to what one might expect upon crossing an event

1216 boundary. Adding variability in font color and presentation location for words on late
1217 lists, after those visual features had been held constant on early lists, led participants to
1218 remember more words on those later lists. One potential explanation is that participants
1219 perceive an “event boundary” to have occurred when they encounter the first “late” list.
1220 According to contextual change accounts of memory across event boundaries (e.g., Flores
1221 et al., 2017; Gold et al., 2017; Pettijohn et al., 2016; Sahakyan and Kelley, 2002), this could
1222 help to explain why participants in the reduced (early) condition exhibited better overall
1223 memory performance. Specifically, their memory for late list items could benefit from less
1224 interference from early list items, and the contextual features associated with late list items
1225 (after the “event boundary”) might serve as more specific recall cues for those late items
1226 (relative to if the boundary had not occurred).

1227 **Theoretical implications**

1228 Although most modern formal theories of episodic memory have been developed and
1229 tested to explain memory for list-learning tasks (Kahana, 2020), a number of recent studies
1230 suggest some substantial differences between memory for lists versus naturalistic stim-
1231 uli (e.g., real-world experiences, narratives, films, etc.; Heusser et al., 2021; Lee et al., 2020;
1232 Manning, 2021; Nastase et al., 2020). One reason is that naturalistic stimuli are often much
1233 more engaging than the highly simplified list-learning tasks typically employed in the
1234 psychological laboratory, perhaps leading participants to pay more attention, exert more
1235 effort, and stay more consistently motivated to perform well (Nastase et al., 2020). Another
1236 reason is that the temporal unfoldings of events and occurrences in naturalistic stimuli
1237 tend to be much more meaningful than the temporal unfoldings of items on typical lists
1238 used in laboratory memory tasks. Real-world events exhibit important associations at a
1239 broad range of timescales. For example, an early detail in a detective story may prove to

1240 be a clue to solving the mystery later on. Further, what happens in one moment typically
1241 carries some predictive information about what came before or after (Xu et al., 2023). In
1242 contrast, the lists used in laboratory memory tasks are most often ordered randomly, by
1243 design, to *remove* meaningful temporal structure in the stimulus (Kahana, 2012).

1244 On one hand, naturalistic stimuli provide a potential means of understanding how our
1245 memory systems function in the circumstances we most often encounter in our everyday
1246 lives. This implies that, to understand how memory works in the “real world,” we should
1247 study memory for stimuli that reflect the relevant statistical structure of real-world expe-
1248 riences. On the other hand, naturalistic stimuli can be difficult to precisely characterize or
1249 model, making it difficult to distinguish whether specific behavioral trends follow from
1250 fundamental workings of our memory systems, from some aspect of the stimulus, or from
1251 idiosyncratic interactions or interference between participants’ memory systems and the
1252 stimulus. This challenge implies that, to understand the fundamental nature of memory
1253 in its “pure” form, we should study memory for highly simplified stimuli that can pro-
1254 vide relatively unbiased (compared with real-world experiences) measures of the relevant
1255 patterns and tendencies.

1256 The experiment we report in this paper was designed to help bridge some of this gap
1257 between naturalistic tasks and more traditional list-learning tasks. We had people study
1258 word lists similar to those used in classic memory studies, but we also systematically var-
1259 ied the lists’ “richness” (by adding or removing visual features) and temporal structure
1260 (through order manipulations that varied over time and across experimental conditions).
1261 We found that participants’ memory behaviors were sensitive to these manipulations.
1262 Some of the manipulations led to changes that were common across people (e.g., more
1263 temporal clustering when words’ appearances were varied, enhanced memory for lists
1264 following an “event boundary,” more feature clustering on order-manipulated lists, etc.).

1265 Other manipulations led to changes that were idiosyncratic (especially carryover effects
1266 from order manipulations; e.g., participants who remembered more words on early order-
1267 manipulated lists tended to show stronger feature clustering for their condition's feature
1268 dimension on late randomly ordered lists, etc.). We also found that participants remem-
1269 bered more words from lists that were sorted to align with their idiosyncratic clustering
1270 preferences. Taken together, our results suggest that our memories are susceptible to ex-
1271 ternal influences (i.e., to the statistical structure of ongoing experiences), but the effects of
1272 past experiences on future memory are largely idiosyncratic across people.

1273 **Potential applications**

1274 Every participant in our study encountered exactly the same words, split into exactly the
1275 same lists. But participants' memory performance, the orders in which they recalled the
1276 words, and the effects of early list manipulations on later lists all varied according to how
1277 we presented the to-be-remembered words.

1278 Our findings raise a number of exciting questions. For example, how far might these
1279 manipulations be extended? In other words, might there be more sophisticated or clever
1280 feature or order manipulations that one could implement to have stronger impacts on
1281 memory? Are there limits to how much impact (on memory performance and/or or-
1282 ganization) these sorts of manipulations can have? Are those limits universal across
1283 people, or are there individual differences (based on prior experiences, natural strate-
1284 gies, neuroanatomy, etc.) that impose person-specific limits on the potential impact of
1285 presentation-level manipulations on memory?

1286 Our findings indicate that the ways word lists are presented affects how people re-
1287 member them. To the extent that word list memory reflects memory processes that are
1288 relevant to real-world experiences, one could imagine potential real-world applications of

1289 our findings. For example, we found that participants remembered more words when the
1290 presentation order agreed with their memory fingerprints. If analogous fingerprints could
1291 be estimated for classroom content, perhaps they could be utilized manually by teachers,
1292 or even by automated content-presentation systems, to optimize how and what students
1293 remember.

1294 **Concluding remarks**

1295 Our work raises deep questions about the fundamental nature of human learning. What
1296 are the limits of our memory systems? How much does what we remember (and how we
1297 remember) depend on how we learn or experience the to-be-remembered content? We
1298 know that our expectations, strategies, situation models learned through prior experiences,
1299 and more collectively shape how our experiences are remembered. But those aspects of
1300 our memory are not fixed: when we are exposed to the same experience in a new way, it
1301 can change how we remember that experience, and also how we remember, process, or
1302 perceive *future* experiences.

1303 **Author contributions**

1304 Conceptualization: JRM and ACH. Methodology: JRM and ACH. Software: JRM, PCF,
1305 CEF, and ACH. Analysis: JRM, PCF, and ACH. Data collection: ECW, PCF, MRL, AMF,
1306 BJB, DR, and CEF. Data curation and management: ECW, PCF, MRL, and ACH. Writing
1307 (original draft): JRM. Writing (review and editing): ECW, PCF, MRL, AMF, BJB, DR, CEF,
1308 and ACH. Supervision: JRM and ACH. Project administration: ECW and PCF. Funding
1309 acquisition: JRM.

Author note

All of the data analyzed in this manuscript, along with all of the code for carrying out the analyses may be found at <https://github.com/ContextLab/FRFR-analyses>. Code for running the non-adaptive experimental conditions may be found at <https://github.com/ContextLab/efficient-learning-code>. Code for running the adaptive experimental condition may be found at <https://github.com/ContextLab/adaptiveFR>. We have also released an associated Python toolbox for analyzing free recall data, which may be found at <https://cdl-quail.readthedocs.io/en/latest/>. Note that this study was not preregistered. Some of the ideas and data presented in this manuscript were also presented at the Annual Meeting of the Society for Neuroscience (2017).

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