

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 our 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 for this is that our
22 moment-by-moment subjective experiences are shaped in part by the idiosyncratic prior
23 experiences, memories, goals, thoughts, expectations, and emotions that we bring with
24 us into the present moment. These factors collectively define a *context* for our experi-
25 ences (Manning, 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 in-
36 teract 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 paradigms, partic-
42 ipants study lists of items and are instructed to recall the items in any order they choose.
43 The orders in which words come to mind can provide insights into how participants have

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

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

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

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

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

94 the words in a random order (Kahana, 2012).

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

117 From a theoretical perspective, we are interested in several core questions organized
118 around the central theme of how structure in our experiences affects how we remember

119 those experiences, as well as how we remember *future* experiences (which may or may not
120 exhibit similar structure). For example, when we distill participants' experiences down
121 to simple word lists that vary (meaningfully) along just a few feature dimensions, are
122 there important differences in these dimensions' influence on participants' memories? Or
123 are all features essentially "equally" influential? Further, are there differences in how
124 specific features influence participants' memories for ongoing versus future experiences?
125 Are there interaction effects between different features, or is the influence of each feature
126 independent of all others'? And are there individual differences in how people organize
127 their memories, or in how participants are influenced by our experimental manipulations?
128 If so, what are those differences and which aspects of memory do they affect?

129 **Materials and methods**

130 **Participants**

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

137 Participants received either course credit or a one-time \$10 cash payment for enrolling
138 in our study. We asked each participant to fill out a demographic survey that included
139 questions about their age, gender, ethnicity, race, education, vision, reading impairments,
140 medications and recent injuries, coffee consumption on the day of testing, and level of
141 alertness at the time of testing. All components of the demographics survey were optional.

142 One participant elected not to fill out any part of the demographic survey, and all other
143 participants answered some or all of the survey questions.

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

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

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

184 We dropped from our dataset the one participant who reported having abnormal color
185 vision, as well as 38 participants whose data were corrupted due to technical failures while
186 running the experiment or during the daily database merges. In total, this left usable data
187 from 452 participants, broken down by experimental condition as follows: feature-rich (67
188 participants), reduced (61 participants), reduced (early) (42 participants), reduced (late)
189 (41 participants), category (30 participants), size (30 participants), length (30 participants),
190 first letter (30 participants), color (31 participants), location (30 participants), and adaptive
191 (60 participants). The participant who declined to fill out their demographic survey

192 participated in the location condition, and we verified verbally that they had normal color
193 vision and no significant reading impairments.

194 **Experimental design**

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

209 **Stimuli**

210 The stimuli in our paradigm were 256 English words selected in a previous study (Ziman
211 et al., 2018). All words referred to concrete nouns and were chosen from 15 unique semantic
212 categories: body parts, building-related, cities, clothing, countries, flowers, fruits, insects,
213 instruments, kitchen-related, mammals, (US) states, tools, trees, and vegetables. We also
214 tagged each word according to the approximate size of the object it referred to. Words



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.

215 were labeled as “small” if the referent object was likely able to “fit in a standard shoebox”
216 or “large” if the object was larger than a shoebox. Most semantic categories comprised
217 words that reflected both “small” and “large” object sizes, but several included only one
218 or the other (e.g., all countries, US states, and cities are larger than a shoebox; mean
219 number of different sizes per category: 1.33; standard deviation: 0.49). The number of
220 words in each semantic category also varied from 12–28 (mean number of words per
221 category: 17.07; standard deviation: 4.65). We also identified lexicographic features for
222 each word, including its first letter and length (i.e., number of letters). Across all categories,
223 all possible first letters were represented except for ‘Q’ (average number of unique first
224 letters per category: 11.00; standard deviation: 2.00 letters). Word lengths ranged from
225 3–12 letters (average: 6.17 letters; standard deviation: 2.06 letters).

226 We assigned the categorized words into a total of 16 lists with several constraints. First,
227 we required that each list contain exactly four unique words from each of four unique
228 categories. Second, we required that each list contain at least one word representing each
229 of the two object sizes (“small” and “large”). On average, each category was represented in
230 4.27 lists (standard deviation: 1.16 lists). Aside from these two constraints, we randomly
231 assigned each word to a single list (i.e., such that no words appeared in multiple lists
232 or were omitted entirely). After random assignment, each list contained words with an
233 average of 11.13 unique starting letters (standard deviation: 1.15 letters) and an average
234 length of 6.17 letters (standard deviation: 0.34 letters).

235 The above assignments of words to lists was performed once across all participants,
236 such that every participant studied the same set of 16 lists. In every condition, we
237 randomized the study order of these lists across participants. For participants in most
238 conditions, on some or all of the lists, we also randomly varied two additional visual
239 features associated with each word: the presentation font color and the word’s onscreen

location. These attributes were assigned independently for each word (and for every participant). These visual features were varied for words in all lists and conditions except for the “reduced” condition (all lists), the first eight lists of the “reduced (early)” condition, and the last eight lists of the “reduced (late)” condition. In these latter cases, all words were presented in black at the center of the experimental computer’s display.

To select a random font color for each word, we drew three integers uniformly and at random from the interval $[0, 254]$, corresponding to the red (r), green (g), and blue (b) color channels for that word. To assign random presentation locations to each word, we selected two floating point numbers uniformly and at random (one for the word’s horizontal x -coordinate and the other for its vertical y -coordinate). The bounds of these coordinates were selected to cover the entire visible area of the display without cutting off any part of the words. The words were shown on 27-in (diagonal) Retina 5K iMac displays (resolution: 5120 by 2880 pixels).

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

Real-time speech-to-text processing

Our experimental paradigm incorporates the Google Cloud Speech-to-Text engine (Halpern et al., 2016) to automatically transcribe participants’ verbal recalls into text. This allows recalls to be transcribed in real time—a distinguishing feature of the experiment; in typical verbal recall experiments, the audio data must be parsed and transcribed manually. In

264 prior work, we used a similar experimental setup (equivalent to the “reduced” condition
265 in the present study) to verify that the automatically transcribed recalls were sufficiently
266 close to human-transcribed recalls to yield reliable data (Ziman et al., 2018). This real-time
267 speech processing component of the paradigm plays an important role in the “adaptive”
268 condition of the experiment, as described below.

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

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

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

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

285 Each of six *order manipulation* conditions used a different feature-based sorting procedure
286 to order words on early lists, where each sorting procedure relied on one relevant feature

dimension. All of the irrelevant features varied freely across words on early lists, in that we did not consider irrelevant features in ordering the early lists. However, we note that some features were correlated—for example, some semantic categories of words referred to objects that tended to be a particular size, which meant that category and size were not fully independent (Fig. S9). On late lists, the words were always presented in a randomized order (chosen anew for each participant). In all of the order manipulation conditions, we varied words’ font colors and onscreen locations as in the feature-rich condition.

Defining feature-based distances. Sorting words according to a given relevant feature requires first defining a distance function for quantifying the dissimilarity between the values of that feature for each pair of words. This function varied according to the type of feature under consideration. Semantic features (category and size) are *categorical*. For these features, we defined a binary distance function: two words were considered to “match” (i.e., have a distance of 0) if their labels were the same (i.e., both from the same semantic category or both of the same size). If two words’ labels were different for a given feature, we defined the words to have a distance of 1. Lexicographic features (length and first letter) are *discrete*. For these features, we defined a discrete distance function. Specifically, we defined the distance between two words as either the absolute difference between their lengths, or the absolute distance between their starting letters in the English alphabet, respectively. For example, two words that started with the same letter would have a “first letter” distance of 0, and a pair of words starting with ‘J’ and ‘A’ would have a first letter distance of 9. Because words’ lengths and letters’ positions in the alphabet are always integers, these discrete distances always take on integer values. Finally, the visual features (color and location) are *continuous* and *multivariate*, in that each “feature” is defined by multiple (positive) real values. We defined the “color” and “location” distances between two words as the Euclidean distances between their (r, g, b) color vectors and (x, y) location

312 vectors (specified as percentages of screen width and height), respectively. Therefore, the
 313 color and location distance measures always take on non-negative real values (upper-
 314 bounded at 439.94 for color, or 124.52 for location, reflecting the distances between the
 315 corresponding maximally different vectors).

316 **Constructing feature-sorted lists.** Given a list of words, a relevant feature, and each
 317 word’s value(s) for that feature, we developed a stochastic algorithm for (noisily) sorting
 318 the words. The stochastic aspect of our sorting procedure enabled us to obtain unique
 319 orderings for each participant. First, we choose a word uniformly and at random from
 320 the set of words on the to-be-presented list. Second, we compute the distances between
 321 the chosen word’s feature value(s) and the corresponding feature value(s) of all yet-to-be-
 322 presented words. Third, we convert these distances (between the previously presented
 323 word’s feature values, a , and each of the W yet-to-be-presented candidate words’ feature
 324 values, $b_{i \in 1 \dots W}$) to similarity scores:

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

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

$$\text{similarity}_{\text{normalized}}(a, b_i) = \frac{\text{similarity}(a, b_i)}{\sum_{j=1}^W \text{similarity}(a, b_j)}, \quad (2)$$

329 where in the denominator, b_j takes on the feature value of each of the W to-be-presented
 330 words. The resulting set of normalized similarity scores sums to 1.

331 As illustrated in Figure 2, we use these normalized similarity scores to construct a

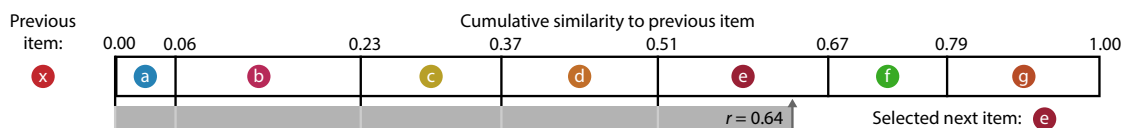


Figure 2: Generating stochastic feature-sorted lists. For a given feature dimension (e.g., color), we compute the similarity (Eqn. 1) between the feature value(s) of the previous item, x , and all yet-to-be-presented items (a – g). Next, we normalize these similarity scores so that they sum to 1. We 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).

sequence of “sticks” that we lay end to end in a line. Each of the n sticks corresponds to a single to-be-presented word, and the stick lengths are proportional to the relative similarities between each word’s feature value(s) and the feature value(s) of the just-presented word. We choose the next to-be-presented word by moving an indicator along the set of sticks, by a distance chosen uniformly and at random on the interval $[0, 1]$. We select the 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 examples of “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

word 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 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," which we define below. At a high level, a participant's memory fingerprint describes how they prioritize or consider different semantic, lexicographic, and/or visual features when they organize their memories.

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

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

Feature clustering scores (uncorrected). Feature clustering scores describe participants' tendencies to recall similar presented items together in their recall sequences, where "similarity" considers one given feature dimension (e.g., category, color, etc.). We based our main approach to computing clustering scores on analogous temporal and semantic clustering scores developed by Polyn et al. (2009). Computing the clustering score for

one feature dimension starts by considering the corresponding feature values from the first word the participant recalled correctly from the just-studied list. Next, we sort all not-yet-recalled words in ascending order according to their feature-based distance to the just-recalled item (see *Defining feature-based distances*). We then compute the percentile rank of the observed next recall. We average these percentile ranks across all of the participant's recalls for the current list to obtain a single uncorrected clustering score for the list, for the given feature dimension. We repeated this process for each feature dimension in turn to obtain a single uncorrected clustering score for each list, for each feature dimension.

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

Permutation-corrected feature clustering scores. Suppose that two lists contain unequal numbers of items of each size. For example, suppose that list *A* contains all "large" items, whereas list *B* contains an equal mix of "large" and "small" items. For a participant recalling list *A*, any correctly recalled item will necessarily match the size of the previous correctly recalled item. In other words, successively recalling several list *A* items of the same size is essentially meaningless, since *any* correctly recalled list *A* word will be large.

396 In contrast, successively recalling several list *B* items of the same size *could* be meaningful,
397 since (early in the recall sequence) the yet-to-be-recalled items come from a mix of sizes.
398 However, once all of the small items on list *B* have been recalled, the best possible next
399 matching recall will be a large item. All subsequent correct recalls must also be large
400 items—so for those later recalls it becomes difficult to determine whether the participant
401 is successively recalling large items because they are organizing their memories according
402 to size, or (alternatively), whether they are simply recalling the yet-to-be-recalled items
403 in a random order. In general, the precise order and blend of feature values expressed
404 in a given list, the order and number of correct recalls a participant makes, the number
405 of intervening presentation positions between successive recalls, and so on, can all affect
406 the range of clustering scores that are possible to observe for a given list. An uncorrected
407 clustering score therefore conflates participants’ actual memory organization with other
408 “nuisance” factors.

409 Following our prior work (Heusser et al., 2017), we used a permutation-based cor-
410 rection procedure to help isolate the behavioral aspects of clustering that we were most
411 interested in. After computing the uncorrected clustering score (for the given list and
412 observed recall sequence), we constructed a “null” distribution of n additional clustering
413 scores by repeatedly randomly shuffling the order of the recalled words and recomputing
414 the clustering score for these shuffled recall sequences (we use $n = 500$ in the present
415 study). This null distribution represents an approximation of the range of clustering
416 scores one might expect to observe by “chance,” given that a hypothetical participant was
417 *not* truly clustering their recalls, but where the hypothetical participant still studied and
418 recalled exactly the same items (with the same features) as the true participant. We define
419 the *permutation-corrected clustering score* as the percentile rank of the observed uncorrected
420 clustering score in this estimated null distribution. In this way, a corrected score of 1

421 indicates that the observed score was greater than any clustering score one might expect
422 by chance—in other words, good evidence that the participant was truly clustering their
423 recalls along the given feature dimension. We applied this correction procedure to all
424 of the clustering scores (feature and temporal) reported in this paper. In Figure S4, we
425 report how participants’ clustering scores along different feature dimensions (in the order
426 manipulation conditions) are correlated, and how clustering scores change across lists.

427 **Memory fingerprints.** We define each participant’s *memory fingerprint* as the set of their
428 permutation-corrected clustering scores across all dimensions we tracked in our study,
429 including their six feature-based clustering scores (category, size, length, first letter, color,
430 and location) and their temporal clustering score. Conceptually, a participant’s memory
431 fingerprint describes their tendency to order in their recall sequences (and, presumably,
432 organize in memory) the studied words along each dimension. To obtain stable estimates
433 of these fingerprints for each participant, we averaged their clustering scores across lists.
434 We also tracked and characterized how participants’ fingerprints changed across lists (e.g.,
435 Figs. 6, S8).

436 **Online “fingerprint” analysis.** The presentation orders of some lists in the adaptive con-
437 dition of our experiment (see *Adaptive condition*) were sorted according to each individual
438 participant’s *current* memory fingerprint, estimated using all of the lists they had studied
439 up to that point in the experiment. Because our experiment incorporated a speech-to-text
440 component, all of the behavioral data for each participant could be analyzed just a few
441 seconds after the conclusion of the recall intervals for each list. We used the Quail Python
442 package (Heusser et al., 2017) to apply speech-to-text algorithms to the just-collected audio
443 data, aggregate the data for the given participant, and estimate the participant’s memory
444 fingerprint using all of their available data up to that point in the experiment. Two aspects

of our implementation are worth noting: First, because memory fingerprints are computed independently for each list and then averaged across lists, the already-computed memory fingerprints for earlier lists could be cached and retrieved as needed in future computations. This meant that updating our estimate of a participant’s memory fingerprint required computing feature and temporal clustering scores only for the single most recent list. Second, the clustering scores for each dimension of a participant’s memory fingerprint could be estimated independently from the others, as could each element of the null distributions of uncorrected clustering scores computed for each dimension (see *Permutation-corrected feature clustering scores*). This enabled us to aggressively parallelize the fingerprint-updating procedure and compress the relevant computations into just a few seconds of computing time. The combined processing time for the speech-to-text algorithm, fingerprint computations, and permutation-based ordering procedure (described next) easily fit within the inter-list intervals, where participants paused for a self-paced break before moving on to study and recall the next list.

Ordering “stabilize” and “destabilize” lists by an estimated fingerprint. In the adaptive condition of our experiment, the presentation orders for *stabilize* and *destabilize* lists were chosen to either maximally or minimally (respectively) comport with participants’ memory fingerprints. Given a participant’s memory fingerprint and a to-be-presented set of items, we designed a permutation-based procedure for ordering the items. First, we dropped from the participant’s fingerprint the temporal clustering score. For the remaining feature dimensions, we arranged the clustering scores in the fingerprint into a template vector f . Second, we computed $n = 2500$ random permutations of the to-be-presented items. These permutations served as candidate presentation orders. We sought to select the specific order that most (or least) closely matched f . Third, for each random permutation, we computed the (permutation-corrected) “fingerprint,” treating the permutation

as though it were a potential “perfect” recall sequence. (We did not include temporal clustering scores in these fingerprints, since the temporal clustering score for every permutation is always equal to 1.) This yielded a “simulated fingerprint” vector \hat{f}_p for each permutation p . We used these simulated fingerprints to select a specific permutation i that either maximized (for stabilize lists) or minimized (for destabilize lists) the correlation between \hat{f}_i and f .

Computing low-dimensional embeddings of memory fingerprints

Following some of our prior work (Fitzpatrick et al., 2023; Heusser et al., 2021, 2018; Manning et al., 2022), we used low-dimensional embeddings to help visualize how participants’ memory fingerprints 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, excluding 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, 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.

494 **Factoring out the effects of temporal clustering**

495 For a given list of words, if the values along two feature dimensions (e.g., category and size)
496 are correlated, then the clustering scores for those two dimensions will also be correlated
497 (Figs. S4, S9). When lists are sorted along a given feature dimension, the sorted feature
498 values will also tend to be correlated with the serial positions of the words in the list.
499 This means that the temporal clustering score will *also* tend to be correlated with the
500 clustering scores for the sorted feature dimension. These correlations mean that it can be
501 difficult to specifically identify when participants are using one feature versus another (or
502 a manipulated feature versus temporal information) to organize or search their memories.

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

517 While these temporally corrected clustering scores are useful for identifying when
518 feature clustering cannot be accounted for by temporal clustering alone, they are *not*

necessarily valid estimates of the “true” degree to which participants are organizing their memories along a given feature dimension. For example, on a list where the presentation order and feature values (along the given feature dimension) are perfectly correlated, the temporally corrected score will have an expected value of 0.5 no matter which words a participant recalls, or the order in which they recall them. Therefore these temporally corrected clustering scores are interpretable only to the extent that presentation order and feature value are decoupled.

Analyses

Probability of n^{th} recall curves

Probability of first recall curves (Atkinson and Shiffrin, 1968; Postman and Phillips, 1965; Welch and Burnett, 1924) reflect the probability that an item will be recalled first, as a function of its serial position during encoding. We used an analogous approach to compute the proportion of trials on which each item (as a function of its presentation position) was recalled at each output position n (Hogan, 1975; Howard and Kahana, 1999; Polyn et al., 2009; Zhang et al., 2023). To carry out this analysis, we initialized (for each participant) a number-of-lists (16) by number-of-words-per-list (16) matrix of 0s. Then, for each list, we found the presentation index of the word that was recalled first, and filled in that position in the matrix with a 1. Finally, we averaged over the rows of the matrix to obtain a 1 by 16 array of probabilities, for each participant. We used an analogous procedure to compute probability of n^{th} recall curves for each participant. Specifically, we filled in the corresponding matrices according to the n^{th} recall on each list that each participant made. When a given participant had made fewer than n recalls for a given list, we simply excluded that list from our analysis when computing that participant’s curve(s). The probability of first recall curve corresponds to a special case where $n = 1$.

543 We note that several other studies have used a slightly different approach to compute
544 these curves, by correcting for the “availability” of a given word to be recalled. For
545 example, if a participant recalls item 1, then item 2 on a given list, our approach places a
546 0 into the item 1 column for that list when computing the “probability of second recall”
547 curve. However, accounting for the fact that the participant had already recalled item
548 1, an alternative approach (e.g., Farrell, 2010) would be to count the item 1 column as
549 “unobserved” (i.e., missing data). Ultimately we chose to use the simpler variant of this
550 approach in our work, but we direct the reader to further discussion of this issue in other
551 work (Farrell, 2014; Moran and Goshen-Gottstein, 2014).

552 **Lag-conditional response probability curve**

553 The lag-conditional response probability (lag-CRP) curve (Kahana, 1996) reflects the prob-
554 ability of recalling a given item after the just-recalled item, as a function of the items’
555 relative encoding positions (lag). In other words, a lag of 1 indicates that a recalled item
556 was presented immediately after the previously recalled item, and a lag of -3 indicates
557 that a recalled item came three items before the previously recalled item. For each re-
558 call transition (following the first recall), we computed the lag between the presentation
559 positions of the just-recalled word and the next-recalled word. We then computed the
560 proportions of transitions (between successively recalled words) for each lag, normalizing
561 for the total numbers of possible transitions. In carrying out this analysis, we excluded
562 all incorrect recalls and repetitions (i.e., recalling a word that had already appeared in
563 the current recall sequence). This yielded, for each list, a 1 by number-of-lags (-15 to
564 +15; 30 lags in total, excluding lags of 0) array of conditional probabilities. We averaged
565 these probabilities across lists to obtain a single lag-CRP for each participant. Because
566 transitions at large absolute lags are rare, these curves are typically displayed using range

567 restrictions (Kahana, 2012).

568 **Serial position curve**

569 Serial position curves (Murdock, 1962) reflect the proportion of participants who remember
570 each item as a function of the items' serial positions during encoding. For each participant,
571 we initialized a number-of-lists (16) by number-of-words-per-list (16) matrix of 0s. Then,
572 for each correct recall, we identified the presentation position of the word and entered a
573 1 into that position (row: list; column: presentation position) in the matrix. This resulted
574 in a matrix whose entries indicated whether or not the words presented at each position,
575 on each list, were recalled by the participant (depending on whether the corresponding
576 entries were set to 1 or 0). Finally, we averaged over the rows of the matrix to yield a
577 1 by 16 array representing the proportion of words at each position that the participants
578 remembered.

579 **Identifying event boundaries**

580 We used the distances between feature values for successively presented words (see *Defin-*
581 *ing feature-based distances*) to estimate "event boundaries" where the feature values changed
582 more than usual (DuBrow and Davachi, 2016; Ezzyat and Davachi, 2011; Manning et al.,
583 2016; Radvansky and Copeland, 2006; Swallow et al., 2011, 2009). For each list, for each
584 feature dimension, we computed the distribution of distances between the feature values
585 for successively presented words. We defined event boundaries (e.g., Fig. 3B) as occurring
586 between any successive pair of words whose distances along the given feature dimension
587 were greater than two standard deviations above the mean for that list. Note that, because
588 event boundaries are defined for each feature dimension, each individual list may contain
589 several sets of event boundaries, each at different moments in the presentation sequence

590 (depending on the feature dimension of interest).

591 **Transparency and openness**

592 All of the data analyzed in this manuscript, along with all of the code for carrying out the
593 analyses, may be found at <https://github.com/ContextLab/FRFR-analyses>. Code for run-
594 ning the non-adaptive experimental conditions may be found at [https://github.com/Con-](https://github.com/ContextLab/efficient-learning-code)
595 [textLab/efficient-learning-code](https://github.com/ContextLab/efficient-learning-code). Code for running the adaptive experimental condition
596 may be found at <https://github.com/ContextLab/adaptiveFR>. We have also released an as-
597 sociated Python toolbox for analyzing free recall data, which may be found at [https://cdl-](https://cdl-quail.readthedocs.io/en/latest)
598 [quail.readthedocs.io/en/latest](https://cdl-quail.readthedocs.io/en/latest).

599 **Results**

600 While holding the set of words (and the assignments of words to lists) constant, we
601 manipulated two aspects of participants' experiences of studying each list. We sought to
602 understand the effects of these manipulations on participants' memories for the studied
603 words. First, we added two additional sources of visual variation to the individual word
604 presentations: font color and onscreen location. Importantly, these visual features were
605 independent of the meaning or semantic content of the words (e.g., word category, size
606 of the referent, etc.) and of the lexicographic properties of the words (e.g., word length,
607 first letter, etc.). We wondered whether this additional word-independent information
608 might facilitate recall (e.g., by providing new or richer potential ways of organizing or
609 retrieving memories of the studied words; Davachi et al., 2003; Drewnowski and Murdock,
610 1980; Hargreaves et al., 2012; Madan, 2021; Meinhardt et al., 2020; Slamecka and Barlow,
611 1979; Socher et al., 2009) or impair recall (e.g., by distracting or confusing participants
612 with irrelevant information; Lange, 2005; Marsh et al., 2012, 2015; Reinitz et al., 1992).

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	-0.290	126	-0.051	0.772	0.772	-2.387	1.768
Temp clust	10.632	126	1.882	< 0.001	< 0.001	7.786	14.386
Cat clust	10.148	126	1.796	< 0.001	< 0.001	7.324	13.778
Sz clust	12.033	126	2.129	< 0.001	< 0.001	9.030	15.918
Len clust	10.720	126	1.897	< 0.001	< 0.001	7.442	15.174
1 st ltr clust	6.679	126	1.182	< 0.001	< 0.001	4.490	9.611

Table 1: Comparing memory in the feature-rich versus reduced conditions (all lists). The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all lists from each participant. Abbreviations used in this table are defined in Table S1.

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

To obtain a clean preliminary estimate of the consequences on memory of randomly varying the font colors and locations of presented words (versus holding the font color fixed at black, and the words’ locations fixed at the center of the screen), we compared participants’ performance on the *feature-rich* and *reduced* experimental conditions (see *Random conditions*, Fig. S1, Tab. 1). In the feature-rich condition, the words’ colors and locations varied randomly, and in the reduced condition, words were always presented in black, at the center of the display. Aggregating across all lists for each participant, we found no difference in recall accuracy (i.e., the proportions of words successfully recalled) for feature-rich versus reduced lists. However, participants in the feature-rich condition clustered their recalls substantially more along every dimension we examined (see *Permutation-corrected feature clustering scores* for more information about how we quantified each participant’s

632 clustering tendencies.) Taken together, these comparisons suggest that adding new fea-
633 tures changes how participants organize their memories of studied words, even when
634 those new features are independent of the words themselves and vary randomly across
635 words. We found no evidence that those additional uninformative features were distract-
636 ing (in terms of their impact on memory performance), but they did affect participants’
637 recall dynamics (measured via their clustering scores).

638 A core assumption of our approach is that each participant organizes their memo-
639 ries in a unique way. We defined each participant’s *memory fingerprint* as the set of their
640 permutation-corrected clustering scores across all dimensions we tracked in our study,
641 including their six feature-based clustering scores (category, size, length, first letter, color,
642 and location) and their temporal clustering score. Conceptually, a participant’s memory
643 fingerprint describes their tendency to order, in their recall sequences (and presumably,
644 organize in memory), the studied words along each dimension. If these memory fin-
645 gerprints are truly unique to each participant, then we would expect that the estimated
646 fingerprints computed for a given participant, on different lists, should be more similar
647 than the estimated fingerprints computed for different participants. We reasoned that the
648 feature-rich condition would provide the best opportunity to test this assumption, since
649 the clustering scores would not be potentially confounded by order manipulations. To test
650 our “unique memory fingerprint” assumption, we compared the similarity (correlation)
651 between the fingerprint from a single list (from one participant) and (a) the average finger-
652 print from all other lists from the same participant versus (b) the average fingerprint from
653 each other participant (across all of their lists). Repeating this procedure for all lists and
654 participants, we found that participants’ fingerprints for a held-out list were reliably more
655 similar to their fingerprints for other lists than they were to other participants’ fingerprints
656 ($t(70280) = 5.077$, $p < 0.001$, $d = 0.162$, $CI = [3.086, 6.895]$). That within-participant fin-

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	4.553	66	0.233	< 0.001	< 0.001	2.427	7.262
Temp clust	2.268	66	0.181	0.027	0.053	0.437	4.425
Cat clust	3.684	66	0.220	< 0.001	0.001	1.733	5.732
Sz clust	1.629	66	0.100	0.108	0.162	-0.207	3.905
Len clust	-0.100	66	-0.010	0.921	0.921	-2.217	1.899
1 st ltr clust	-0.412	66	-0.045	0.681	0.818	-2.461	1.645

Table 2: Comparing memory for early versus late lists in the feature-rich condition. The *t*-tests reported in the table were carried out across-participants. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	2.434	60	0.134	0.018	0.027	0.493	4.910
Temp clust	0.986	60	0.061	0.328	0.328	-0.897	3.348
Cat clust	2.755	60	0.177	0.008	0.016	0.761	5.189
Sz clust	3.081	60	0.201	0.003	0.009	1.210	5.326
Len clust	3.762	60	0.261	< 0.001	0.002	1.604	6.821
1 st ltr clust	1.721	60	0.175	0.090	0.109	-0.138	4.098

Table 3: Comparing memory for early versus late lists in the reduced condition. The *t*-tests reported in the table were carried out across-participants. Abbreviations used in this table are defined in Table S1.

gerprint similarity (across lists) was greater than across-participant fingerprint similarity suggests that participants' memory fingerprints are relatively stable across lists, and that each participant's fingerprint is unique to them.

Beyond affecting participants' memories for individual lists, we wondered how studying early lists (with versus without incidental visual features) might affect how participants remembered later lists (again, with versus without incidental visual features). In other words, we sought to test for potential effects of changing the "richness" of participants' experiences over time. All participants studied and recalled a total of 16 lists; we defined *early* lists as the first eight lists and *late* lists as the last eight lists each participant encountered. To help interpret our results, we compared participants' memories on early versus late lists in the above feature-rich (Tab. 2) and reduced (Tab. 3) conditions. Participants in both conditions remembered more words on early versus late lists. Participants in the feature-rich (but not reduced) condition exhibited more temporal clustering on early versus late lists. And participants in both conditions tended to exhibit more semantic

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	1.499	41	0.098	0.141	0.580	-0.345	3.579
Temp clust	0.857	41	0.068	0.396	0.580	-1.012	2.896
Cat clust	0.707	41	0.068	0.484	0.580	-1.314	2.830
Sz clust	0.803	41	0.079	0.427	0.580	-1.142	2.953
Len clust	0.461	41	0.060	0.648	0.648	-1.545	2.462
1 st ltr clust	0.781	41	0.101	0.439	0.580	-1.039	2.881

Table 4: Comparing memory for early versus late lists in the reduced (early) condition. The *t*-tests reported in the table were carried out across-participants. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	1.462	40	0.121	0.152	0.441	-0.376	2.993
Temp clust	1.244	40	0.128	0.221	0.441	-0.894	3.088
Cat clust	-0.101	40	-0.009	0.920	0.920	-2.307	1.776
Sz clust	0.555	40	0.058	0.582	0.873	-1.444	2.274
Len clust	1.482	40	0.126	0.146	0.441	-0.444	3.743
1 st ltr clust	-0.143	40	-0.017	0.887	0.920	-2.204	1.830

Table 5: Comparing memory for early versus late lists in the reduced (late) condition. The *t*-tests reported in the table were carried out across-participants. Abbreviations used in this table are defined in Table S1.

clustering on early versus late lists. Participants in the reduced (but not feature-rich) conditions tended to exhibit more lexicographic clustering on early versus late lists. Taken together, these comparisons suggest that even when the presence or absence of incidental visual features is stable across lists, participants still exhibit some differences in their performance and memory organization tendencies for early versus late lists.

With these differences in mind, we next compared participants' memories on early versus late lists for two additional experimental conditions (see *Random conditions*, Fig. S1). In a *reduced (early)* condition, we held the visual features constant on early lists, but allowed them to vary randomly on late lists. In a *reduced (late)* condition, we allowed the visual features to vary randomly on early lists, but held them constant on late lists. Given our above findings that (a) participants tended to exhibit stronger clustering effects on feature-rich (versus reduced) lists, and (b) participants tended to remember more words and exhibit stronger clustering effects on early (versus late) lists, we expected these early versus late differences to be enhanced in the reduced (early) condition and

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	-2.230	107	-0.439	0.028	0.167	-4.252	-0.229
Temp clust	-1.379	107	-0.271	0.171	0.512	-3.319	0.474
Cat clust	0.013	107	0.003	0.989	0.989	-2.003	2.102
Sz clust	-0.349	107	-0.069	0.728	0.873	-2.244	1.641
Len clust	-0.581	107	-0.114	0.563	0.844	-2.328	1.291
1 st ltr clust	0.636	107	0.125	0.526	0.844	-1.291	2.940

Table 6: Comparing memory in the feature-rich versus reduced (early) conditions (all lists). The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	-2.045	101	-0.410	0.043	0.043	-3.826	0.112
Temp clust	-10.689	101	-2.143	< 0.001	< 0.001	-13.479	-8.512
Cat clust	-9.538	101	-1.912	< 0.001	< 0.001	-12.332	-7.457
Sz clust	-12.222	101	-2.451	< 0.001	< 0.001	-15.311	-9.954
Len clust	-10.620	101	-2.129	< 0.001	< 0.001	-13.902	-8.239
1 st ltr clust	-5.213	101	-1.045	< 0.001	< 0.001	-7.290	-3.403

Table 7: Comparing memory in the reduced versus reduced (early) conditions (all lists). The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all lists from each participant. Abbreviations used in this table are defined in Table S1.

diminished in the reduced (late) condition. However, to our surprise, participants in *neither* condition exhibited reliable early-versus-late differences in accuracy, temporal clustering, nor feature-based clustering (Tabs. 4, 5). We hypothesized that adding or removing the variability in the visual features was acting as a sort of “event boundary” between early and late lists (e.g., Clewett et al., 2019; Radvansky and Copeland, 2006; Radvansky and Zacks, 2017). In prior work, we (and others) have found that memories formed just after event boundaries can be enhanced (e.g., due to less contextual interference between pre- and post-boundary items; Flores et al., 2017; Gold et al., 2017; Manning et al., 2016; Pettijohn et al., 2016).

We found that *adding* incidental visual features on later lists that had not been present on early lists (as in the reduced (early) condition) served to enhance recall performance relative to conditions where all lists had the same blends of features (Tabs. 6, 7; also see Fig. S3A). However, *subtracting* irrelevant visual features on later lists that *had* been present on early lists (as in the reduced (late) condition) did not appear to impact recall

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	-0.638	106	-0.126	0.525	0.593	-2.720	1.362
Temp clust	-0.535	106	-0.106	0.593	0.593	-2.552	1.237
Cat clust	-1.345	106	-0.267	0.181	0.420	-3.525	0.660
Sz clust	-1.441	106	-0.286	0.153	0.420	-3.557	0.382
Len clust	-1.261	106	-0.250	0.210	0.420	-3.611	0.669
1 st ltr clust	0.939	106	0.186	0.350	0.525	-1.018	2.949

Table 8: Comparing memory in the feature-rich versus reduced (late) conditions (all lists). The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Acc	-0.407	100	-0.082	0.685	0.685	-2.477	1.626
Temp clust	-9.885	100	-1.996	< 0.001	< 0.001	-14.701	-6.499
Cat clust	-10.436	100	-2.107	< 0.001	< 0.001	-15.607	-6.940
Sz clust	-12.413	100	-2.507	< 0.001	< 0.001	-18.413	-8.398
Len clust	-9.672	100	-1.953	< 0.001	< 0.001	-14.476	-6.437
1 st ltr clust	-4.555	100	-0.920	< 0.001	< 0.001	-7.332	-2.538

Table 9: Comparing memory in the reduced versus reduced (late) conditions (all lists). The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all lists from each participant. Abbreviations used in this table are defined in Table S1.

performance (Tabs. 8, 9) These comparisons suggest that recall accuracy has a directional component: accuracy is affected differently by removing features that had initially been present versus adding features that had initially been absent. In contrast, we found that participants exhibited more temporal and feature-based clustering when we added incidental visual features to *any* lists (feature-rich versus reduced: Tab. 1; reduced versus reduced (early): Tab. 7; reduced versus reduced (late): Tab. 9). Temporal and feature-based clustering were not reliably different in the feature-rich versus reduced (early) or reduced (late) conditions (Tabs. 6, 8).

Taken together, our findings thus far suggest that adding item features that change over time, even when they vary randomly and independently of the items, can enhance participants' overall memory performance and can also enhance temporal and feature-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

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat	3.034	95	0.667	0.003	0.019	1.048	5.113
Sz	-1.013	95	-0.223	0.314	0.627	-3.055	0.865
Len	-0.550	95	-0.121	0.584	0.700	-2.368	1.363
1 st ltr	-0.690	95	-0.152	0.492	0.700	-2.663	1.119
Clr	1.850	96	0.402	0.067	0.202	-0.010	3.712
Loc	0.043	95	0.010	0.966	0.966	-1.598	1.729

Table 10: Comparing accuracy on early lists in the order manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all early lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat	8.813	95	1.936	< 0.001	< 0.001	6.793	11.751
Sz	2.630	95	0.578	0.010	0.020	0.831	4.866
Len	-1.547	95	-0.340	0.125	0.150	-3.693	0.341
1 st ltr	2.858	95	0.628	0.005	0.016	1.031	4.886
Clr	-1.339	96	-0.291	0.184	0.184	-3.238	0.394
Loc	1.705	95	0.374	0.092	0.137	-0.155	3.521

Table 11: Comparing temporal clustering on early lists in the order manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all early lists from each participant. Abbreviations used in this table are defined in Table S1.

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*;

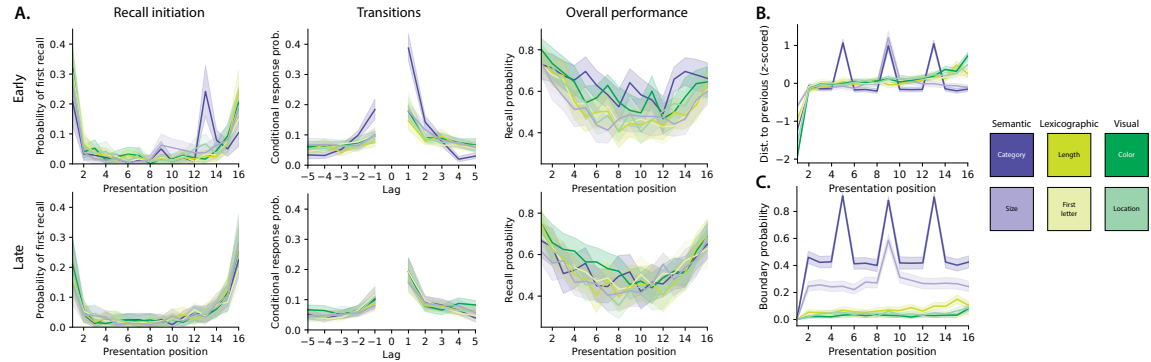


Figure 3: Recall dynamics in feature-rich free recall (order manipulation conditions). **A.** Behavioral plots. **Left panels.** The probabilities of initiating recall with each word are plotted as a function of presentation position. **Middle panels.** The conditional probabilities of recalling each word are plotted as a function of the relative position (lag) to the word recalled just-prior. **Right panels.** The overall probabilities of recalling each word are plotted as a function of presentation position. 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.** Distances between successively presented words (z-scored within condition) computed based on each condition’s feature of focus, and plotted as a function of presentation position. See *Defining feature-based distances* for additional information. **C.** Proportion of event boundaries (see *Identifying event boundaries*) for each condition’s feature of focus, plotted as a function of presentation position. **All panels.** Error ribbons denote bootstrap-estimated 95% confidence intervals (calculated across participants in Panel A, and across lists in Panels B and C).

	<i>t</i> -value	df	Cohen’s <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat	2.722	125	0.484	0.007	0.022	0.827	4.932
Sz	3.866	125	0.687	< 0.001	0.001	2.020	5.983
Len	0.521	125	0.093	0.603	0.724	-1.311	2.333
1 st ltr	-0.842	125	-0.150	0.401	0.724	-2.825	1.095
Clr	-0.650	125	-0.116	0.517	0.724	-2.680	1.249
Loc	-0.251	125	-0.045	0.802	0.802	-2.257	1.524

Table 12: Comparing feature-based clustering on early lists in the semantic order manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all early lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat	-1.040	125	-0.185	0.301	0.601	-3.095	1.092
Sz	0.006	125	0.001	0.995	0.995	-1.933	1.952
Len	3.682	125	0.655	< 0.001	0.001	1.890	5.569
1 st ltr	5.134	125	0.912	< 0.001	< 0.001	3.251	7.258
Clr	0.092	125	0.016	0.927	0.995	-1.834	1.867
Loc	0.407	125	0.072	0.685	0.995	-1.655	2.463

Table 13: Comparing feature-based clustering on early lists in the lexicographic order manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all early lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat	0.012	126	0.002	0.991	0.991	-1.988	1.871
Sz	-0.104	126	-0.018	0.917	0.991	-2.166	1.847
Len	0.592	126	0.105	0.555	0.991	-1.361	2.420
1 st ltr	0.040	126	0.007	0.968	0.991	-1.791	1.863
Clr	2.022	126	0.358	0.045	0.136	0.056	3.965
Loc	4.390	126	0.777	< 0.001	< 0.001	2.730	6.199

Table 14: Comparing feature-based clustering on early lists in the visual order manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all early lists from each participant. Abbreviations used in this table are defined in Table S1.

726 features: category, size, length, first letter, color, and location). When we compared partic-
727 ipants' memories for early lists in each of these conditions to their memories for early lists
728 in the feature-rich condition (Tab. 10), we found that participants in the category-ordered
729 condition remembered more words than participants in the feature-rich condition. Partic-
730 ipants' performances on early lists in all of the other order manipulation conditions were
731 indistinguishable from performance on the early feature-rich lists. We also compared
732 participants' temporal clustering on early lists in each of these conditions to their tem-
733 poral clustering on early lists in the feature-rich condition (Tab. 11). Participants in both
734 of the semantically ordered conditions and the first letter-ordered condition all exhibited
735 stronger temporal clustering on early lists (vs. early feature-rich lists). Participants in the
736 other order manipulation conditions all showed similar temporal clustering on early lists
737 relative to early feature-rich lists. We also also compared feature-based clustering on early
738 lists across the order manipulation and feature-rich conditions. Since these results were

739 similar across both semantic conditions (category and size; Tab. 12), both lexicographic
740 conditions (length and first letter; Tab. 13), and both visual conditions (color and loca-
741 tion; Tab. 14), here we aggregate data from conditions that manipulated each of these
742 three feature groupings in our comparisons, to simplify the presentation. On early lists,
743 participants in the semantically ordered conditions exhibited stronger semantic clustering
744 relative to participants in the feature-rich condition, but showed no reliable differences in
745 lexicographic or visual clustering. Similarly, participants in the lexicographically ordered
746 conditions exhibited stronger (relative to feature rich participants) lexicographic cluster-
747 ing on early lists, but showed no reliable differences in semantic or visual clustering. And
748 participants in the visually ordered conditions exhibited stronger visual clustering (again,
749 relative to feature-rich participants, and on early lists), but showed no reliable differences
750 in semantic or lexicographic clustering. Taken together, these order manipulation results
751 suggest several broad patterns (Figs. 3A, 4). First, most of the order manipulations we
752 carried out did *not* reliably affect overall recall performance. Second, most of the order
753 manipulations increased participants' tendencies to temporally cluster their recalls. Third,
754 all of the order manipulations enhanced participants' clustering of each condition's target
755 feature (i.e., semantic manipulations enhanced semantic clustering, lexicographic manip-
756 ulations enhanced lexicographic clustering, and visual manipulations enhanced visual
757 clustering; Fig. 5C) while leaving clustering along other feature dimensions roughly un-
758 changed (i.e., semantic manipulations did not affect lexicographic or visual clustering, and
759 so on). Although it is not possible to fully separate feature-based versus temporal clus-
760 tering when considering sorted lists, we used a permutation-based procedure to identify
761 the degree of feature clustering over and above what could be accounted for by temporal
762 clustering alone (see *Factoring out the effects of temporal clustering*). When we carried out
763 this analysis (Fig. 5D), we found that participants exhibited more semantic clustering on

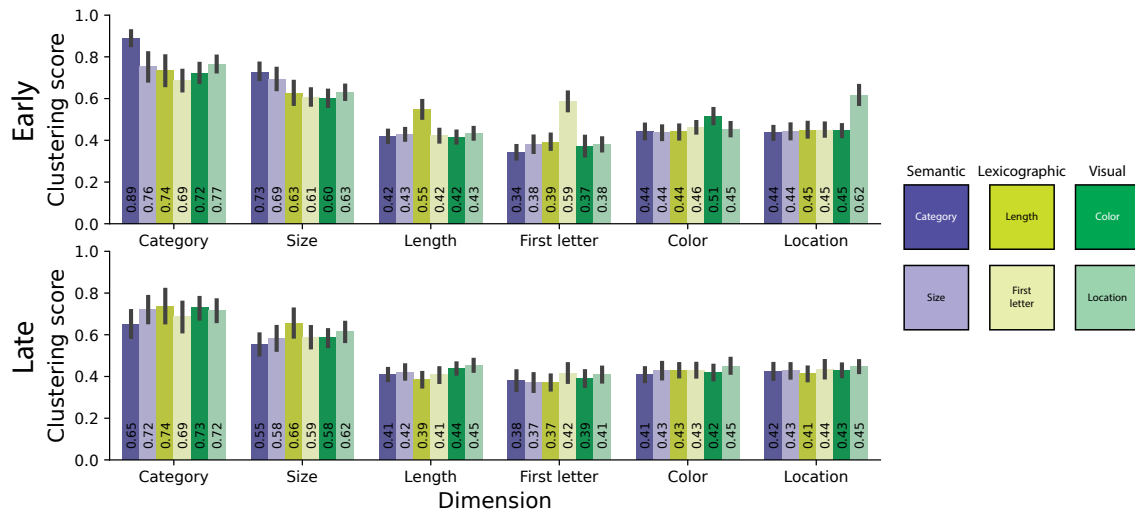


Figure 4: Memory “fingerprints” (order manipulation conditions). The across-participant average clustering scores for each feature type (*x*-axis) are displayed for each experimental condition (color), separately for order-manipulated (early, top) and randomly ordered (late, bottom) lists. Error bars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

semantically sorted lists than on randomly ordered lists, but the effects of the other order manipulations could not reliably be separated from temporal clustering alone (reliable comparisons are reported in the figure).

When we closely examined the sequences of words participants recalled from early order-manipulated lists (Fig. 3A, top panel), we noticed several differences from the dynamics of participants’ recalls of randomly ordered lists (Figs. S1, S7). One difference is that participants in the category condition (Fig. 3, dark purple curves) most often initiated recall with the fourth-from-last item (*Recall initiation*, top left panel), whereas participants who recalled randomly ordered lists tended to initiate recall with either the first or last list items (Fig. S1, top left panel). We hypothesized that the participants might be “clumping” their recalls into groups of items that shared category labels. Indeed, when we compared the positions of feature changes in the study sequence (Fig. 3C; see *Identifying*

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Sem vs. lex	1.936	118	0.353	0.055	0.083	0.057	3.916
Sem vs. vis	0.113	119	0.021	0.910	0.910	-1.987	2.097
Lex vs. vis	-2.145	119	-0.390	0.034	0.083	-4.254	-0.208

Table 15: Comparing accuracy on early lists in different order manipulation conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all early lists from each participant. Abbreviations used in this table are defined in Table S1.

event boundaries) with the positions of items participants recalled first, we noticed a striking correspondence in both semantic conditions. Specifically, on category-ordered lists, the category labels changed every four items on average (dark purple peaks in Figs. 3B, C), and participants also seemed to display an increased tendency (relative to other order manipulation and random conditions) to initiate recall of category-ordered lists with items whose study positions were integer multiples of four. Similarly, for size-ordered lists, the size labels changed every eight items on average (light purple peaks in Figs. 3B, C), and participants also seemed to display an increased tendency to initiate recall of size-ordered lists with items whose study positions were integer multiples of eight. A second striking difference 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 and feature-rich conditions highlight how sorted lists are remembered differ-

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Sem vs. lex	5.620	118	1.026	< 0.001	< 0.001	3.486	8.010
Sem vs. vis	6.613	119	1.202	< 0.001	< 0.001	4.481	9.464
Lex vs. vis	0.589	119	0.107	0.557	0.557	-1.336	2.539

Table 16: Comparing temporal clustering on early lists in different order manipulation conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all early lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat: sem vs. lex	3.667	118	0.670	< 0.001	< 0.001	1.822	5.942
Sz: sem vs. lex	4.043	118	0.738	< 0.001	< 0.001	2.145	6.296
Len: sem vs. lex	-3.390	118	-0.619	< 0.001	0.002	-5.661	-1.499
1 st ltr: sem vs. lex	-5.705	118	-1.042	< 0.001	< 0.001	-7.790	-3.841
Clr: sem vs. lex	-0.767	118	-0.140	0.444	0.533	-2.744	1.154
Loc: sem vs. lex	-0.658	118	-0.120	0.512	0.576	-2.595	1.171
Cat: sem vs. vis	3.114	119	0.566	0.002	0.004	1.052	5.737
Sz: sem vs. vis	4.692	119	0.853	< 0.001	< 0.001	2.620	7.024
Len: sem vs. vis	-0.068	119	-0.012	0.946	0.946	-1.897	1.907
1 st ltr: sem vs. vis	-0.842	119	-0.153	0.401	0.516	-2.944	1.089
Clr: sem vs. vis	-2.673	119	-0.486	0.009	0.014	-4.567	-0.848
Loc: sem vs. vis	-4.499	119	-0.818	< 0.001	< 0.001	-6.399	-2.721
Cat: lex vs. vis	-1.186	119	-0.216	0.238	0.329	-3.010	0.891
Sz: lex vs. vis	0.118	119	0.021	0.906	0.946	-1.778	2.271
Len: lex vs. vis	3.399	119	0.618	< 0.001	0.002	1.500	5.527
1 st ltr: lex vs. vis	4.859	119	0.883	< 0.001	< 0.001	2.860	6.849
Clr: lex vs. vis	-1.988	119	-0.361	0.049	0.074	-3.894	-0.102
Loc: lex vs. vis	-3.966	119	-0.721	< 0.001	< 0.001	-5.862	-2.099

Table 17: Comparing feature-based clustering on early lists in different order manipulation conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all early lists from each participant. The feature used to compute clustering is shown before the colon in each row, and the conditions being compared are shown after the colon. Abbreviations used in this table are defined in Table S1.

796 ently from random lists. We also wondered how sorting lists along each feature dimension
797 influenced memory relative to sorting lists along the other feature dimensions (accuracy:
798 Tab. 15; temporal clustering: Tab. 16; feature-based clustering: Tab 17). Participants
799 trended towards remembering early lists that were sorted visually better than lexico-
800 graphically sorted lists. However, participants showed no reliable differences in recall for
801 semantically versus lexicographically or visually sorted lists. Participants temporally clus-
802 tered semantically sorted lists more strongly than lists sorted either lexicographically or
803 visually, but did not show reliable differences in temporal clustering on lexicographically
804 versus visually sorted lists. Participants also showed reliably more semantic clustering on
805 semantically sorted lists than lexicographically or visually sorted lists; more lexicographic
806 clustering on lexicographically sorted lists than semantically or visually sorted lists; and
807 more visual clustering on visually sorted lists than semantically or lexicographically sorted
808 lists. In summary, sorting lists by different features appeared to have slightly different
809 effects on overall memory performance and temporal clustering. Participants also tended
810 to cluster their recalls along a given feature dimension more when the studied lists were
811 (versus were not) sorted along that dimension.

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

819 At the group level, there seemed to be almost no lingering impact of sorting early
820 lists on memory for later lists. To simplify the presentation, we report these null results

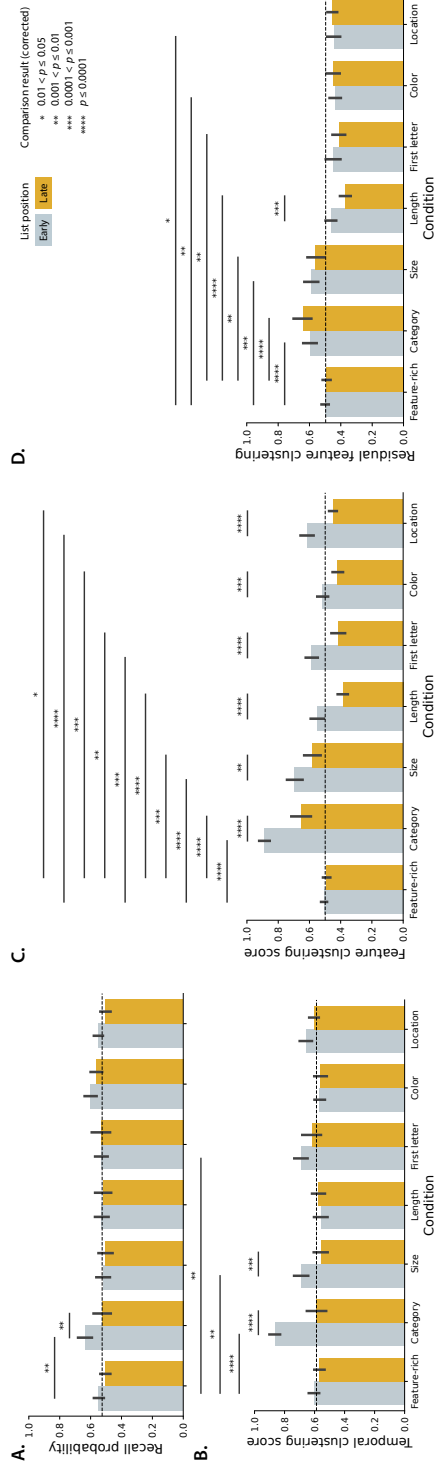


Figure 5: Recall probability and clustering scores on early and late lists. The bar heights display the average (across participants) recall probabilities (A.), temporal clustering scores (B.), feature clustering scores (C.), and residual feature clustering scores (after factoring out temporal clustering 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.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Sem	0.487	125	0.087	0.627	0.627	-1.661	2.323
Lex	0.878	125	0.156	0.382	0.573	-1.226	3.044
Vis	1.437	126	0.254	0.153	0.460	-0.447	3.519

Table 18: Comparing accuracy on late lists in order-manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all late lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Sem	0.157	125	0.028	0.875	0.875	-1.859	1.974
Lex	0.998	125	0.177	0.320	0.875	-0.902	2.920
Vis	0.548	126	0.097	0.585	0.875	-1.450	2.365

Table 19: Comparing temporal clustering on late lists in order-manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all late lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat	-0.041	125	-0.007	0.967	0.967	-2.088	1.861
Sz	-0.989	125	-0.176	0.324	0.967	-3.100	0.948
Len	-0.045	125	-0.008	0.964	0.967	-1.959	1.870
1 st ltr	-0.369	125	-0.066	0.713	0.967	-2.338	1.630
Clr	-0.602	125	-0.107	0.548	0.967	-2.541	1.273
Loc	-0.521	125	-0.093	0.603	0.967	-2.592	1.565

Table 20: Comparing feature-based clustering on late lists in semantic order-manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all late lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat	0.678	125	0.121	0.499	0.655	-1.240	2.608
Sz	0.915	125	0.163	0.362	0.655	-1.137	2.756
Len	-1.200	125	-0.213	0.233	0.655	-3.499	0.737
1 st ltr	0.606	125	0.108	0.546	0.655	-1.390	2.553
Clr	0.094	125	0.017	0.925	0.925	-1.955	1.966
Loc	-0.619	125	-0.110	0.537	0.655	-2.672	1.270

Table 21: Comparing feature-based clustering on late lists in lexicographic order-manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all late lists from each participant. Abbreviations used in this table are defined in Table S1.

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Cat	1.209	126	0.214	0.229	0.526	-0.700	3.136
Sz	0.202	126	0.036	0.840	0.869	-1.832	2.163
Len	2.005	126	0.355	0.047	0.283	0.211	3.722
1 st ltr	1.124	126	0.199	0.263	0.526	-0.846	3.260
Clr	0.278	126	0.049	0.781	0.869	-1.710	2.084
Loc	0.165	126	0.029	0.869	0.869	-1.779	2.004

Table 22: Comparing feature-based clustering on late lists in visual order-manipulation versus feature-rich conditions. The *t*-tests reported in the table were carried out across-participants, and reflect data aggregated across all late lists from each participant. Abbreviations used in this table are defined in Table S1.

in aggregate across the three feature groupings (accuracy: Tab. 18; temporal clustering: Tab. 19; feature-based clustering: Tabs. 20, 21, and 22). Relative to memory performance on late feature-rich lists, participants' memory performance in all six order manipulation conditions showed no reliable differences. Nor did we observe any reliable differences in temporal clustering on late lists (relative to late feature-rich lists). Aside from a slightly increased tendency for participants to cluster words by their length on late visual order manipulation lists (more than late feature-rich lists), we observed no reliable differences in any type of feature clustering on late order manipulation condition lists versus late feature-rich lists.

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. As described above, a participant's memory fingerprint characterizes 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'

840 average fingerprints tended to remain relatively stable on early lists, and exhibited a
 841 “jump” to another stable state on later lists. The sizes of these jumps varied somewhat
 842 across conditions (the Euclidean distances between fingerprints in their original high di-
 843 mensional spaces are displayed in the bottom panels). We also averaged the fingerprints
 844 across early and late lists, respectively, for each condition (Fig. 6B). We found that par-
 845 ticipants’ fingerprints on early lists seem to be influenced by the order manipulations
 846 for those lists (see the locations of the circles in Fig. 6B). There also seemed to be some
 847 consistency across different features within a broader type. For example, both semantic
 848 feature conditions (category and size; purple markers) diverge in a similar direction from
 849 the group; both lexicographic feature conditions (length and first letter; yellow markers)
 850 diverge in a similar direction; and both visual conditions (color and location; green) also
 851 diverge in a similar direction. But on late lists, participants’ fingerprints seem to return
 852 to a common state that is roughly shared across conditions (i.e., the stars in that panel are
 853 clumped together).

854 When we examined the data at the level of individual participants (Figs. 7 and 8), a
 855 clearer story emerged. Within each order manipulation condition, participants exhibited
 856 a range of feature clustering scores on both early and late lists (Fig. 7A, B). Across ev-
 857 ery order manipulation condition, participants who exhibited stronger feature clustering
 858 (for their condition’s manipulated feature) recalled more words. This trend held overall
 859 across conditions and participants (early: $r(179) = 0.492$, $p < 0.001$, $CI = [0.352, 0.606]$;
 860 late: $r(179) = 0.403$, $p < 0.001$, $CI = [0.271, 0.517]$) as well as for each condition indi-
 861 vidually for early ($rs \geq 0.331$, all $ps \leq 0.069$) and late ($rs \geq 0.404$, all $ps \leq 0.027$) lists.
 862 We found no evidence of a condition-level trend; for example, the conditions where
 863 participants tended to show stronger clustering scores were not correlated with the con-
 864 ditions where participants remembered more words (early: $r(4) = 0.511$, $p = 0.300$, $CI =$

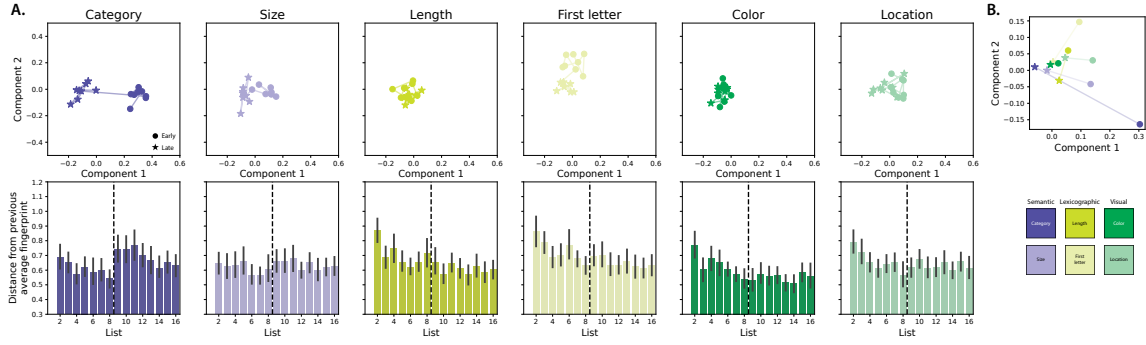


Figure 6: Memory fingerprint dynamics (order manipulation conditions). **A.** Each column (and color) reflects an experimental condition. In the top panels, each marker displays a 2D projection of the (across-participant) average memory fingerprint for a single list. Lines connect successive lists. Order 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 between each per-list memory fingerprint and the average fingerprint across all prior lists, 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.

865 $[-0.999, 0.996]$; late: $r(4) = -0.304$, $p = 0.559$, $CI = [-0.833, 0.748]$; see insets of Fig. 7A
866 and B). We observed carryover associations between feature clustering and recall perfor-
867 mance (Fig. 7C, D). Participants who showed stronger feature clustering on early lists
868 in the non-visual conditions tended to recall more items on late lists (across conditions:
869 $r(179) = 0.230$, $p = 0.002$, $CI = [0.072, 0.372]$; all non-visual conditions individually: r s
870 ≥ 0.405 , all p s ≤ 0.027 ; color: $r(29) = 0.212$, $p = 0.251$, $CI = [-0.164, 0.532]$; location:
871 $r(28) = 0.320$, $p = 0.085$, $CI = [0.011, 0.584]$). Participants who recalled more items on
872 early lists also tended to show stronger feature clustering on late lists (across conditions:
873 $r(179) = 0.464$, $p < 0.001$, $CI = [0.321, 0.582]$; individual conditions: all r s ≥ 0.377 , all p s
874 ≤ 0.040). Neither of these effects showed condition-level trends (early feature clustering
875 versus late recall probability: $r(4) = -0.338$, $p = 0.512$, $CI = [-0.971, 0.634]$; early recall
876 probability versus late feature clustering: $r(4) = 0.451$, $p = 0.369$, $CI = [-0.986, 0.998]$). We

877 also looked for associations between feature clustering and temporal clustering. Across
 878 every order manipulation condition, participants who exhibited stronger feature cluster-
 879 ing also exhibited stronger temporal clustering. For early lists (Fig. 7E), this trend held
 880 overall ($r(179) = 0.916$, $p < 0.001$, $CI = [0.893, 0.936]$), for each condition individually
 881 (all $rs \geq 0.822$, all $ps < 0.001$), and across conditions ($r(4) = 0.964$, $p = 0.002$). For late
 882 lists (Fig. 7F), the results were more variable (overall: $r(179) = 0.348$, $p < 0.001$; all non-
 883 visual conditions: $rs \geq 0.382$, all $ps \leq 0.037$; color: $r(29) = 0.453$, $p = 0.011$; location:
 884 $r(28) = 0.190$, $p = 0.314$; across-conditions: $r(4) = -0.036$, $p = 0.945$). While less ro-
 885 bust than the carryover associations between feature clustering and recall performance,
 886 we also observed some carryover associations between feature clustering and temporal
 887 clustering (Fig. 7G, H). Participants who showed stronger feature clustering on early lists
 888 showed stronger temporal clustering on later lists (overall: $r(179) = 0.464$, $p < 0.001$, $CI =$
 889 $[0.321, 0.582]$; for individual conditions: all $rs \geq 0.377$, all $ps \leq 0.040$; across conditions:
 890 $r(4) = 0.451$, $p = 0.369$, $CI = [-0.986, 0.998]$). And participants who showed stronger
 891 temporal clustering on early lists trended towards showing stronger feature clustering on
 892 later lists (overall: $r(179) = 0.266$, $p < 0.001$, $CI = [0.129, 0.396]$; for individual conditions:
 893 all $rs \geq 0.298$, all $ps \leq 0.110$; across conditions: $r(4) = 0.064$, $p = 0.903$, $CI = [-0.972, 0.965]$).
 894 Taken together, the results displayed in Figure 7 show that participants who were more
 895 sensitive to the order manipulations (i.e., participants who showed stronger feature clus-
 896 tering for their condition's feature on early lists) remembered more words and showed
 897 stronger temporal clustering. These associations also appeared to carry over across lists,
 898 even when the items on later lists were presented in a random order.

899 If participants show different sensitivities to order manipulations, how do their be-
 900 haviors carry over to later lists? We found that participants who showed strong feature
 901 clustering on early lists often tended to show strong feature clustering on late lists (Fig. 8A;

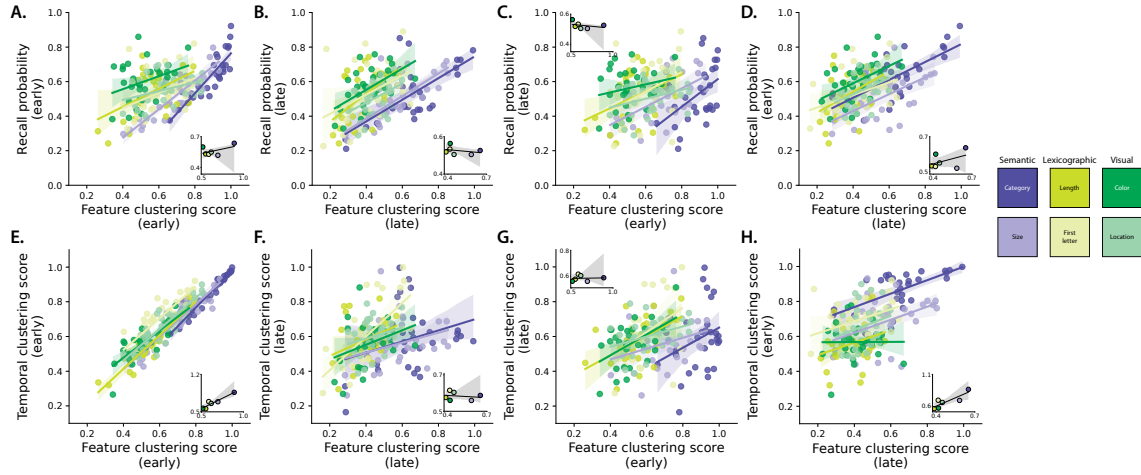


Figure 7: Interactions between feature clustering, recall probability, and contiguity. A. Recall probability versus feature clustering scores for order manipulation (early) lists. B. Recall probability versus feature clustering for randomly ordered (late) lists. C. Recall probability on late lists versus 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.

902 overall across participants and conditions: $r(179) = 0.591$, $p < 0.001$, $CI = [0.472, 0.682]$;
 903 category: $r(28) = 0.590$, $p < 0.001$, $CI = [0.354, 0.756]$; size: $r(28) = 0.488$, $p =$
 904 0.006 , $CI = [0.134, 0.732]$; length: $r(28) = 0.384$, $p = 0.036$, $CI = [0.040, 0.681]$; first
 905 letter: $r(28) = 0.202$, $p = 0.284$, $CI = [-0.273, 0.620]$; color: $r(29) = -0.183$, $p = 0.325$, $CI =$
 906 $[-0.562, 0.258]$; location: $r(28) = 0.031$, $p = 0.870$, $CI = [-0.240, 0.296]$; across condi-
 907 tions: $r(4) = 0.942$, $p = 0.005$, $CI = [0.442, 1.000]$). Although participants tended to
 908 show weaker feature clustering on late lists (Figs. 4, 5) on *average*, the associations be-
 909 tween early and late lists for individual participants suggests that some influence of early
 910 order manipulations may linger on late lists. We found that participants who exhibited
 911 larger carryover in feature clustering (i.e., continued to show strong feature clustering
 912 on late lists) for the semantic order manipulations (but not other manipulations) also
 913 tended to show a smaller decrease in recall on early versus late lists (Fig. 8B; overall:
 914 $r(179) = 0.307$, $p < 0.001$, $CI = [0.148, 0.469]$; category: $r(28) = 0.350$, $p = 0.058$, $CI =$
 915 $[0.050, 0.642]$; size: $r(28) = 0.708$, $p < 0.001$, $CI = [0.472, 0.862]$; length: $r(28) = 0.205$, $p =$
 916 0.276 , $CI = [-0.109, 0.492]$; first letter: $r(28) = 0.081$, $p = 0.672$, $CI = [-0.433, 0.597]$;
 917 color: $r(29) = 0.155$, $p = 0.406$, $CI = [-0.174, 0.541]$; location: $r(28) = 0.052$, $p =$
 918 0.787 , $CI = [-0.307, 0.360]$; across conditions: $r(4) = 0.635$, $p = 0.176$, $CI = [-0.924, 0.981]$.
 919 Participants who exhibited larger carryover in feature clustering also tended to show
 920 stronger temporal clustering on late lists (relative to early lists) for all but the category
 921 condition (Fig. 8C; overall: $r(179) = 0.426$, $p < 0.001$, $CI = [0.285, 0.544]$; category:
 922 $r(28) = 0.110$, $p = 0.564$, $CI = [-0.284, 0.442]$; all non-category conditions: all $rs \geq 0.406$,
 923 all $ps \leq 0.023$; across conditions: $r(4) = 0.649$, $p = 0.163$, $CI = [-0.856, 0.988]$).

924 We suggest two potential interpretations of these findings. First, it is possible that
 925 some participants are more “malleable” or “adaptable” with respect to how they organize
 926 incoming information. When presented with list of items sorted along *any* feature dimen-

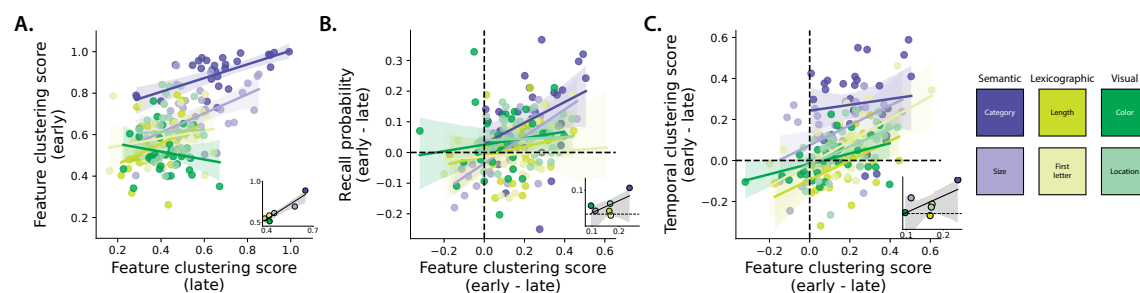


Figure 8: Feature clustering carryover effects. **A.** Feature clustering scores for order manipulation (early) versus randomly ordered (late) lists. **B.** Accuracy differences (on early versus late lists) versus feature clustering “carryover” (defined as the differences between the average clustering 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.

927 sion, they will simply adopt that feature as a dominant dimension for organizing those
 928 items and subsequent (randomly ordered) items. This flexibility in memory organization
 929 might afford such participants a memory advantage, explaining their strong recall perfor-
 930 mance. An alternative interpretation is that each participant comes into our study with
 931 a “preferred” way of organizing incoming information. If they happen to be assigned to
 932 an order manipulation condition that matches their preferences, then they will appear to
 933 be “sensitive” to the order manipulation and also exhibit a high degree of carryover in
 934 feature clustering from early to late lists. These participants might demonstrate strong
 935 recall performance not because of their inherently superior memory abilities, but rather
 936 because the specific condition they were assigned to happened to be especially easy for
 937 them, given their pre-experimental tendencies. To help distinguish between these inter-
 938 pretations, we designed an *adaptive* experimental condition (see *Adaptive condition*). The
 939 primary manipulation in the adaptive condition is that participants each experience three
 940 key types of lists. On *random* lists, words are ordered randomly (as in the feature-rich

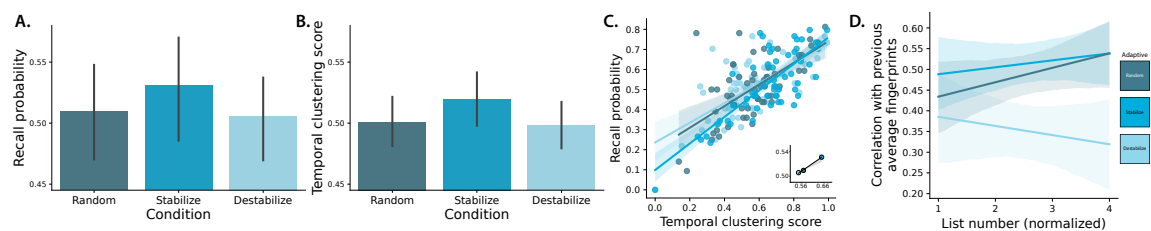


Figure 9: Adaptive free recall. **A.** Average probability of recall (taken across words, lists, and participants) for each batch of four lists in the adaptive condition. **B.** Average temporal clustering scores for each batch of lists. **C.** Recall probability versus temporal clustering scores by participant (main panel; each participant contributes one dot per batch) and averaged within batch (inset; each dot represents a single batch). **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 policy (batch) for each list. Error bars and ribbons denote bootstrap-estimated 95% confidence intervals. For additional details about participants’ behavior and performance during the adaptive condition, see Figure S2.

condition). On *stabilize* lists, the presentation order is adjusted to be maximally similar to the current estimate of the participant’s memory fingerprint (see *Online “fingerprint” analysis*). Third, on *destabilize* lists, the presentation order is adjusted to be *minimally* similar to the current estimate of the participant’s memory fingerprint (see *Ordering “stabilize” and “destabilize” lists by an estimated fingerprint*). The orders in which participants experienced each type of list were counterbalanced across participants to help reduce the influence of potential list-order effects. Because the presentation orders on stabilize and destabilize lists are adjusted to best match each participant’s personal 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 ($t(59) = 1.740$, $p = 0.087$, $d =$

0.095, CI = [-0.187, 3.761]) and destabilize ($t(59) = 1.714$, $p = 0.092$, $d = 0.114$, CI = [-0.351, 4.108]) lists (Fig. 9A). Participants showed no reliable differences in their memory performance on destabilize versus random lists ($t(59) = -0.249$, $p = 0.804$, $d = -0.017$, CI = [-2.327, 1.578]). Participants also exhibited stronger temporal clustering on stabilize lists, relative to random ($t(59) = 3.428$, $p = 0.001$, $d = 0.306$, CI = [1.635, 5.460]) and destabilize ($t(59) = 4.174$, $p < 0.001$, $d = 0.374$, CI = [1.964, 6.968]) lists (Fig. 9B). We found no reliable differences in temporal clustering for items on random versus destabilize lists ($t(59) = -0.880$, $p = 0.382$, $d = -0.081$, CI = [-3.165, 1.127]).

As in the other experimental manipulations, participants in the adaptive condition exhibited substantial variability with respect to their overall memory performance and their clustering tendencies (Fig. 9C). We found that individual participants who exhibited strong temporal clustering scores also tended to recall more items. This held across subjects, aggregating across all list types ($r(178) = 0.701$, $p < 0.001$, CI = [0.590, 0.789]), and for each list type individually (all $rs \geq 0.651$, all $ps < 0.001$). Taken together, the results from the adaptive condition suggest that each participant comes into the experiment with their own unique memory organization tendencies, as characterized by their memory fingerprint. When participants study lists whose items come pre-sorted according to their unique preferences, they tend to remember more and show stronger temporal clustering.

We note that the multivariate aspect of the adaptive condition (i.e., sorting lists simultaneously along multiple feature dimensions) provides an important contrast with the other order manipulation conditions, where we sort lists along only a single feature dimension in each condition. We found that participants “naturally” clustered their recalls along multiple feature dimensions, even when the lists they studied were not sorted along those dimensions (as in the feature-rich condition). A caveat is that the *specific* feature dimensions participants tended to cluster along varied across participants. One way to

	<i>t</i> -value	df	Cohen's <i>d</i>	<i>p</i> -value (raw)	<i>p</i> -value (corrected)	95% CI (lower bound)	95% CI (upper bound)
Rank 1	12.751	66	0.162	< 0.001	< 0.001	8.702	20.013
Rank 2	8.196	66	0.162	< 0.001	< 0.001	4.794	12.978
Rank 3	3.243	66	0.162	0.002	0.002	1.028	7.051
Rank 4	-3.112	66	0.162	0.003	0.003	-5.282	-1.920
Rank 5	-7.154	66	0.162	< 0.001	< 0.001	-12.649	-5.568
Rank 6	-12.608	66	0.162	< 0.001	< 0.001	-22.114	-9.347
Rank 7	-18.397	66	0.162	< 0.001	< 0.001	-27.238	-14.073

Table 23: Ranked clustering scores versus “chance” for participants in the feature-rich condition. For each participant, we sorted their clustering scores in descending order (for each of the six feature dimensions, along with a seventh dimension to capture temporal clustering). The *t*-tests reported in the table (for the clustering scores at each “rank”) were carried out across-participants, and reflect data aggregated across all lists from each participant. Abbreviations used in this table are defined in Table S1.

quantify the multidimensional nature of participants’ clustering tendencies is to sort each participant’s clustering scores (for each of the six feature dimensions, along with a seventh dimension to capture temporal clustering). We can then ask whether the distribution of clustering scores at each “rank” within the sorted set of scores for each participant has a mean that is reliably different from a chance value of 0.5. We carried out these tests for each set of ranked scores, and found that participants in the feature-rich condition reliably cluster their recalls along at least three dimensions, including temporal clustering (which was often ranked highest; Tab. 23). That the clustering scores ranked in the top three dimensions were reliably greater than chance suggests that participants organize their memories along at least three feature dimensions, even when the words are studied in a random order.

Discussion

We asked participants to study and freely recall word lists. The words on each list (and the total set of lists) were held constant across participants. For each word, we considered (and manipulated) two semantic features (category and size) that reflected aspects of the meanings of the words, along with two lexicographic features (word length and first letter),

997 which reflected characteristics of the words' *letters*. These semantic and lexicographic
998 features are intrinsic to each word. We also considered and manipulated two additional
999 visual features (color and location) that affected the *appearance* of each studied item, but
1000 could be varied independently of the words' identities. Across different experimental
1001 conditions, we manipulated how the visual features varied across words (within each
1002 list), along with the orders of each list's words. Although the participants' task (verbally
1003 recalling as many words as possible, in any order, within one minute) remained constant
1004 across all of these conditions, and although the set of words they studied from each list
1005 remained constant, our manipulations substantially affected participants' memories. The
1006 impact of some of the manipulations also affected how participants remembered *future*
1007 lists that were sorted randomly.

1008 **Recap: visual feature manipulations**

1009 We found that participants in our feature-rich condition (where we varied words' ap-
1010 pearances) recalled similar proportions of words to participants in a reduced condition
1011 (where appearance was held constant across words). However, varying the words' ap-
1012 pearances led participants to exhibit much more temporal and feature-based clustering.
1013 This suggests that even seemingly irrelevant elements of our experiences can affect how
1014 we remember them.

1015 When we held the within-list variability in participants' visual experiences fixed across
1016 lists (in the feature-rich and reduced conditions), they remembered more words from early
1017 lists than from late lists. For feature-rich lists, they also showed stronger clustering for
1018 early versus late lists. However, when we *varied* participants' visual experiences across lists
1019 (in the "reduced (early)" and "reduced (late)" conditions), these early versus late accuracy
1020 and clustering differences disappeared. Abruptly changing how incidental visual features

1021 varied across words seemed to act as a sort of “event boundary” that partially reset how
1022 participants processed and remembered post-boundary lists. Within-list clustering also
1023 increased in these manipulations, suggesting that the “within-event” words were being
1024 more tightly associated with each other.

1025 When we held the visual features constant during early lists, but then varied words’
1026 appearances in later lists (i.e., the reduced (early) condition), participants’ overall memory
1027 performance improved. However, this impact was directional: when we *removed* visual
1028 features from words in late lists that had been present in early lists (i.e., the reduced (late)
1029 condition), we saw no memory improvement.

1030 **Recap: order manipulations**

1031 When we (stochastically) sorted early lists along different feature dimensions, we found
1032 several impacts on participants’ memories. Sorting early lists semantically (by word cat-
1033 egory) enhanced participants’ memories for those lists, but the effects on performance of
1034 sorting along other feature dimensions were inconclusive. However, each order manipu-
1035 lation substantially affected how participants *organized* their memories of words from the
1036 ordered lists. When we sorted lists semantically, participants displayed stronger semantic
1037 clustering; when we sorted lists lexicographically, they displayed stronger lexicographic
1038 clustering; and when we sorted lists visually, they displayed stronger visual clustering.
1039 Clustering along the unmanipulated feature dimensions in each of these cases was un-
1040 changed.

1041 The order manipulations we examined also appeared to induce, in some cases, a
1042 tendency to “clump” similar words within a list. This was most apparent on semantically
1043 ordered lists, where the probability of initiating recall with a given word seemed to follow
1044 groupings defined by feature change points.

1045 We also examined the impact of early list order manipulations on memory for late
1046 lists. At the group level, we found little evidence for lingering “carryover” effects of
1047 these manipulations: participants in the order manipulation conditions showed similar
1048 memory performance and clustering on late lists to participants in the corresponding
1049 control (feature-rich) condition. At the level of individual participants, however, we
1050 found several meaningful patterns.

1051 Participants who showed stronger feature clustering on early (order-manipulated) lists
1052 tended to better remember late (randomly ordered) lists. Participants who remembered
1053 early lists better also tended to show stronger feature clustering (along their condition’s
1054 feature dimension) on late lists (even though the words on those late lists were presented
1055 in a random order). We also observed some (weaker) carryover effects of temporal cluster-
1056 ing. Participants who showed stronger feature clustering (along their condition’s feature
1057 dimension) on early lists tended to show stronger temporal clustering on late lists. And
1058 participants who showed stronger temporal clustering on early lists also tended to show
1059 stronger feature clustering on late lists. Essentially, these order manipulations appeared to
1060 affect each participant differently. Some participants were sensitive to our manipulations,
1061 and those participants’ memory performance was impacted more strongly, both for the
1062 ordered lists and for future (random) lists. Other participants appeared relatively insen-
1063 sitive to our manipulations, and those participants showed little carryover effects on late
1064 lists.

1065 These results at the individual participant level suggested to us that either (a) some
1066 participants were more sensitive to *any* order manipulation, or (b) some participants might
1067 be more (or less) sensitive to manipulations along *particular* (e.g., preferred) feature dimen-
1068 sions. To help distinguish between these possibilities, we designed an adaptive condition
1069 whereby we attempted to manipulate whether participants studied words in an order that

1070 either matched or mismatched our estimate of how they would cluster or organize the
1071 studied words in memory (i.e., their idiosyncratic memory fingerprint). We found that
1072 when we presented words in orders that were consistent with participants' memory fin-
1073 gerprints, they remembered more words overall and showed stronger temporal clustering.
1074 This comports well with the second possibility described above. Specifically, each partici-
1075 pant seems to bring into the experiment their own idiosyncratic preferences and strategies
1076 for organizing the words in their memory. When we presented the words in an order
1077 consistent with each participant's idiosyncratic fingerprint, their memory performance
1078 improved. This might indicate that the participants were spending less cognitive effort
1079 "reorganizing" the incoming words on those lists, which freed up resources to devote to
1080 encoding processes instead.

1081 **Memory consequences of feature variability**

1082 Several prior studies have examined how varying the richness or experiences, or the ex-
1083 tensive of encoding, can affect memory. Although specific details differ (Bonin et al., 2022),
1084 in general these studies have found that richer and more deeply or extensively encoded
1085 experiences are remembered better (Hargreaves et al., 2012; Madan, 2021; Meinhardt et al.,
1086 2020). Our findings help to elucidate an additional factor that may contribute to these phe-
1087 nomenon. For example, our finding that participants better remember "feature-rich" lists
1088 (where words' appearances are varied) than "reduced" lists (where words' appearances
1089 are held constant) only when those feature-rich lists are presented *after* reduced lists sug-
1090 gests that some factors that influence the richness or depth of encoding may be relative,
1091 rather than absolute. In other words, *increases* in richness (e.g., relative to a recency-
1092 weighted baseline) may be more important than the absolute complexity or numbers of
1093 meaningful features.

1094 Some prior studies have suggested that people can “cue” their memories using dif-
1095 ferent “strategies” or “pathways” for searching for the target information. For example,
1096 modern accounts of free recall often posit that memory search typically begins by matching
1097 the current state of mental context with the contexts associated with other items in mem-
1098 ory (Kahana, 2020). Since context is the defining hallmark of episodic memory (Tulving,
1099 1983), context-based search can be described as an “episodic” pathway to recall. When
1100 episodic cueing fails to elicit a match, participants may then search for items that are simi-
1101 lar to the current mental context or mental state along other dimensions, such as semantic
1102 similarity (Davachi et al., 2003; Socher et al., 2009). These multiple pathways accounts of
1103 memory search also provide a potential explanation of why participants might have an
1104 easier time remembering richer stimuli (or experiences): richer stimuli and experiences
1105 might have more features that could be used to cue memory search. Our work suggests
1106 that there may be some additional factors at play with respect to the *dynamics* of these pro-
1107 cesses. In particular, we only observed memory benefits for “richer” stimuli when they
1108 were encountered after more “impoverished” stimuli (in the reduced (early) condition).
1109 This suggests that the pathways available to recall a given item may also depend on recent
1110 prior experiences.

1111 We did *not* find any evidence that changing words’ appearances *harmed* memory per-
1112 formance, e.g., by distracting them with irrelevant information (Lange, 2005; Marsh et al.,
1113 2012, 2015; Reinitz et al., 1992). Nor did we find any evidence that *changes* in the presence
1114 of potentially “distracting” features adversely affected memory. For example, when we
1115 increased or decreased the variability in words’ appearances on late versus early lists (as in
1116 the reduced (early) and reduced (late) conditions), we found no evidence that this harmed
1117 participants’ memories. One potential interpretation under the “multiple pathways to
1118 recall” framework is that the availability of multiple pathways to recall do not appear to

1119 specifically interfere with each other.

1120 **Context effects on memory performance and organization**

1121 In real-world experiences, each moment's unique blend of contextual features (where we
1122 are, who we are with, what else we are thinking of at the time, what else we experience
1123 nearby in time, etc.) plays an important role in how we interpret, experience, and re-
1124 member that moment, and how we relate it to our other experiences (e.g., for review see
1125 Manning, 2020). What are the analogues of real-world contexts in laboratory tasks like
1126 the free recall paradigm employed in our study? In general, modern formal accounts of
1127 free recall (Kahana, 2020) describe context as comprising a mix of (a) features pertaining
1128 to or associated with each item and (b) other items and thoughts experienced nearby in
1129 time, e.g., that might still be "lingering" in the participant's thoughts at the time they
1130 study the item. Item features can include semantic properties (i.e., features related to the
1131 item's meaning), lexicographic properties (i.e., features related to the item's letters), sen-
1132 sory properties (i.e., feature related to the item's appearance, sound, smell, etc.), emotional
1133 properties (i.e., features related to how meaningful the item is, whether the item evokes
1134 positive or negative feelings, etc.), utility-related properties (e.g., features that describe
1135 how an item might be used or incorporated into a particular task or situation), and more.
1136 Essentially any aspect of the participant's experience that can be characterized, measured,
1137 or otherwise described can be considered to influence the participant's mental context at
1138 the moment they experience that item. Temporally proximal features include aspects of
1139 the participant's internal or external experience that are *not* specifically occurring at the
1140 moment they encounter an item, but that nonetheless influence how they process the item.
1141 Thoughts related to percepts, goals, expectations, other experiences, and so on that might
1142 have been cued (directly or indirectly) by the participant's recent experiences prior to the

1143 current moment all fall into this category. Internally driven mental states, such as thinking
1144 about an experience unrelated to the experiment, also fall into this category.

1145 Contextual features need not be intentionally or consciously perceived by the partic-
1146 ipant to affect memory, nor do they need to be relevant to the task instructions or the
1147 participant’s goals. Incidental factors such as font color (Jones and Pyc, 2014), back-
1148 ground color (Isarida and Isarida, 2007), inter-stimulus images (Chiu et al., 2021; Ger-
1149 shman et al., 2013; Manning et al., 2016), background sounds (Beaman and Jones, 1998;
1150 Sahakyan and Smith, 2014), secondary tasks (Masicampto and Sahakyan, 2014; Oberauer
1151 and Lewandowsky, 2008; Polyn et al., 2009), and more can all impact how participants
1152 remember, and organize in memory, lists of studied items.

1153 Consistent with this prior work, we found that participants were sensitive to task-
1154 irrelevant visual features. We also found that changing the dynamics of those task-
1155 irrelevant visual features (in the reduced (early) and reduced (late) conditions) *also* affected
1156 participants’ memories. This suggests that it is not only the contextual features themselves
1157 that affect memory, but also the *dynamics* of context—i.e., how the contextual features
1158 associated with each item change over time.

1159 **Priming effects on memory performance and organization**

1160 When our ongoing experiences are ambiguous, we can draw on our past experiences,
1161 expectations, and other real, perceived, or inferred cues to help resolve these ambiguities.
1162 We may also be overtly or covertly “primed” to influence how we are likely to resolve
1163 ambiguities. For example, before listening to a story with several equally plausible inter-
1164 pretations, providing participants with “background” information beforehand can lead
1165 them towards one interpretation versus another (Yeshurun et al., 2017). More broadly, our
1166 conscious and unconscious biases and preferences can influence not only how we interpret

1167 high-level ambiguities, but even how we process low-level sensory information (Katabi
1168 et al., 2023).

1169 In more simplified scenarios, like list-learning paradigms, the stimuli and tasks partic-
1170 ipants encounter before studying a given list can influence what and how they remember.
1171 For example, when participants are directed to suppress, disregard, or ignore “distracting”
1172 stimuli early on in an experiment, participants often tend to remember those stimuli less
1173 well when they are re-used as to-be-remembered targets later on in the experiment (Tip-
1174 per, 1985). In general, participants’ memories can be influenced by exposing them to
1175 a wide range of positive and negative priming factors before they encounter the to-be-
1176 remembered information (Balota et al., 1992; Clayton and Chattin, 1989; Donnelly, 1988;
1177 Flexser and Tulving, 1982; Gotts et al., 2012; Huang et al., 2004; Huber, 2008; Huber et al.,
1178 2001; McNamara, 1994; Neely, 1977; Rabinowitz, 1986; Tulving and Schacter, 1991; Watkins
1179 et al., 1992; Wiggs and Martin, 1998).

1180 The order manipulation conditions in our experiment show that participants can also be
1181 primed to pick up on more subtle statistical structure in their experiences, like the dynamics
1182 of how the presentation orders of stimuli vary along particular feature dimensions. These
1183 order manipulations affected not only how participants remembered the manipulated
1184 lists, but also how they remembered *future* lists with different (randomized) temporal
1185 properties.

1186 **Free recall of blocked versus random categorized word lists**

1187 A large number of prior studies have compared participants’ memories for categorized
1188 word lists that are presented in blocked versus random orders. In “blocked” lists, all
1189 of the words from a given semantic category (e.g., animals) are presented consecutively,
1190 whereas in “random” lists, the words from different categories are intermixed. Most of

1191 these studies report that participants tend to better remember blocked (versus random)
1192 lists (Bower et al., 1969; Cofer et al., 1966; D’Agostino, 1969; Dallett, 1964; Kintsch, 1970;
1193 Luek et al., 1971; Puff, 1974; Shapiro, 1970; Shapiro and Palermo, 1970). Other studies
1194 suggest that these order effects may also be modulated by factors like list length and the
1195 numbers of exemplars in each category (e.g., Borges and Mangler, 1972).

1196 Although we did not directly manipulate “blocking” in our order manipulation condi-
1197 tions, our sorting procedures in those conditions (see *Constructing feature-sorted lists*) have
1198 *indirect* effects on the lists’ blockiness. For example, lists that are stochastically sorted by
1199 semantic category will tend to contain runs of several same-category words in succession.
1200 Consistent with the above work on blocked versus random categorized lists, we found
1201 that participants tended to better remember lists that were sorted semantically (Fig. 5B).
1202 However, this memory improvement did not appear to extend to the other order ma-
1203 nipulation conditions we considered (e.g., to lexicographically or visually sorted lists).
1204 One possibility is that the memory benefits of blocked versus random lists are specific to
1205 semantic categories, and do not generalize to other feature dimensions. Another possi-
1206 bility is that the memory benefits are due to the presence of infrequent “jumps” between
1207 successive items (e.g., from different categories). Because the features we manipulated in
1208 the lexicographic and visual conditions were less categorical than the semantic features,
1209 feature values across words in those conditions tended to vary more gradually. Relatively
1210 stable features that are punctuated by infrequent large changes (e.g., as words transition
1211 from a same-category sequence to a new category) may also relate to perceived “event
1212 boundaries,” which can have important consequences for memory (DuBrow and Davachi,
1213 2013, 2016; DuBrow et al., 2017; Radvansky and Zacks, 2017).

1214 **Expectation, event boundaries, and situation models**

1215 Our findings that participants' current and future memory behaviors are sensitive to
1216 manipulations in which features change over time, and how features change across items
1217 and lists, suggest parallels with studies on how we form expectations and predictions,
1218 segment our continuous experiences into discrete events, and make sense of different
1219 scenarios and situations. Each of these real-world cognitive phenomena entail identifying
1220 statistical regularities in our experiences, and exploiting those regularities to gain insight,
1221 form inferences, organize or interpret memories, and so on. Our past experiences enable
1222 us to predict what is likely to happen in the future, given what happened "next" in our
1223 previous experiences that were similar to now (Barron et al., 2020; Brigard, 2012; Chow
1224 et al., 2016; Eichenbaum and Fortin, 2009; Gluck et al., 2002; Goldstein et al., 2021; Griffiths
1225 and Steyvers, 2003; Jones and Pashler, 2007; Kim et al., 2014; Manning, 2020; Tamir and
1226 Thornton, 2018; Xu et al., 2023).

1227 When our expectations are violated, such as when our observations disagree with our
1228 predictions, we may perceive the "rules" or "situation" to have changed. *Event boundaries*
1229 denote abrupt changes in the state of our experience, for example, when we transition
1230 from one situation to another (Radvansky and Zacks, 2017; Zwaan and Radvansky, 1998).
1231 Crossing an event boundary can impair our memory for pre-boundary information and en-
1232 hance our memory for post-boundary information (DuBrow and Davachi, 2013; Manning
1233 et al., 2016; Radvansky and Copeland, 2006; Sahakyan and Kelley, 2002). Event bound-
1234 aries are also tightly associated with the notion of *situation models* and *schemas*—mental
1235 frameworks for organizing our understanding about the rules of how we and others are
1236 likely to behave, how events are likely to unfold over time, how different elements are
1237 likely to interact, and so on. For example, a situation model pertaining to a particular
1238 restaurant might set our expectations about what we are likely to experience when we

1239 visit that restaurant (e.g., what the building will look like, how it will smell when we enter,
1240 how crowded the restaurant is likely to be, the sounds we are likely to hear, etc.). Similarly,
1241 as mentioned in the *Introduction*, we might learn a schema describing how events are likely
1242 to unfold *across* any sit-down restaurant—e.g., open the door, wait to be seated, receive a
1243 menu, decide what to order, place the order, and so on. Situation models and schemas can
1244 help us to generalize across our experiences, and to generate expectations about how new
1245 experiences are likely to unfold. When those expectations are violated, we can perceive
1246 ourselves to have crossed into a new situation.

1247 In our study, we found that abruptly changing the “rules” about how the visual
1248 appearances of words are determined, or about the orders in which words are presented,
1249 can lead participants to behave similarly to what one might expect upon crossing an event
1250 boundary. Adding variability in font color and presentation location for words on late
1251 lists, after those visual features had been held constant on early lists, led participants to
1252 remember more words on those later lists. One potential explanation is that participants
1253 perceive an “event boundary” to have occurred when they encounter the first “late” list.
1254 According to contextual change accounts of memory across event boundaries (e.g., Flores
1255 et al., 2017; Gold et al., 2017; Pettijohn et al., 2016; Sahakyan and Kelley, 2002), this could
1256 help to explain why participants in the reduced (early) condition exhibited better overall
1257 memory performance. Specifically, their memory for late list items could benefit from less
1258 interference from early list items, and the contextual features associated with late list items
1259 (after the “event boundary”) might serve as more specific recall cues for those late items
1260 (relative to if the boundary had not occurred).

1261 **How do different types of clustering relate to each other, and to memory perfor-**
1262 **mance?**

1263 When the words on a studied list are presented in a random order, different types of
1264 clustering in participants' recalls often tend to be negatively correlated. For example,
1265 words that occur nearby on the list will not (on average) tend to be semantically related, and
1266 vice versa. Therefore a participant who shows a strong tendency to temporally cluster their
1267 recalls will tend to show weaker semantic clustering, and so on (Healey and Uitvlugt, 2019;
1268 Howard and Kahana, 2002b; Sederberg et al., 2010). Further, there is some evidence that
1269 temporal clustering is positively correlated with memory performance, whereas semantic
1270 clustering is negatively correlated with memory performance (Sederberg et al., 2010).

1271 The notion of “multiple pathways to recall” discussed above (see *Memory consequences*
1272 *of feature variability*) suggests one potential explanation for these patterns. For exam-
1273 ple, temporal clustering has been proposed to reflect reliance on contextual cues in an
1274 “episodic” pathway to search memory, whereas semantic clustering reflects a relies on
1275 specific item features. These two pathways may “compete” with each other during re-
1276 call (Socher et al., 2009). Meanwhile, extra-list intrusion errors (i.e., false “recalls” of items
1277 that were never encountered on the list) often tend to share semantic features with recently
1278 recalled items (Zaromb et al., 2006) and also often lead the participant to stop recalling
1279 additional items (Miller et al., 2012). Speculatively, over-reliance on semantic cues may
1280 lead to more intrusion errors, which in turn may lead to fewer recalls overall.

1281 Our findings extend these prior results to consider lists that are *not* ordered randomly.
1282 Because ordering the words on a list along a particular feature dimension removes the
1283 “conflict” between temporal and feature clustering, the order manipulation conditions in
1284 our study represent an “edge case” whereby different pathways to recall are not neces-
1285 sarily in conflict with each other. For example, the same participants who exhibit strong

1286 feature clustering *also* show strong temporal clustering on ordered lists (Fig. 7E). This
1287 is presumably at least partly due to an inability to separate temporal and feature clus-
1288 tering on ordered lists (also see *Factoring out the effects of temporal clustering*). However,
1289 features that change gradually with time (i.e., presentation position) could also serve to
1290 strengthen the episodic (contextual) cues associated with each item. In other words, par-
1291 ticipants might essentially combine multiple noisy measures of change to form a more
1292 stable internal representation of temporal context.

1293 **Theoretical implications**

1294 Although most modern formal theories of episodic memory have been developed and
1295 tested to explain memory for list-learning tasks (Kahana, 2020), a number of recent studies
1296 suggest some substantial differences between memory for lists versus naturalistic stim-
1297 uli (e.g., real-world experiences, narratives, films, etc.; Heusser et al., 2021; Lee et al., 2020;
1298 Manning, 2021; Nastase et al., 2020). One reason is that naturalistic stimuli are often much
1299 more engaging than the highly simplified list-learning tasks typically employed in the
1300 psychological laboratory, perhaps leading participants to pay more attention, exert more
1301 effort, and stay more consistently motivated to perform well (Nastase et al., 2020). Another
1302 reason is that the temporal unfoldings of events and occurrences in naturalistic stimuli
1303 tend to be much more meaningful than the temporal unfoldings of items on typical lists
1304 used in laboratory memory tasks. Real-world events exhibit important associations at a
1305 broad range of timescales. For example, an early detail in a detective story may prove to
1306 be a clue to solving the mystery later on. Further, what happens in one moment typically
1307 carries some predictive information about what came before or after (Xu et al., 2023). In
1308 contrast, the lists used in laboratory memory tasks are most often ordered randomly, by
1309 design, to *remove* meaningful temporal structure in the stimulus (Kahana, 2012).

1310 On one hand, naturalistic stimuli provide a potential means of understanding how our
1311 memory systems function in the circumstances we most often encounter in our everyday
1312 lives. This implies that, to understand how memory works in the “real world,” we should
1313 study memory for stimuli that reflect the relevant statistical structure of real-world expe-
1314 riences. On the other hand, naturalistic stimuli can be difficult to precisely characterize or
1315 model, making it difficult to distinguish whether specific behavioral trends follow from
1316 fundamental workings of our memory systems, from some aspect of the stimulus, or from
1317 idiosyncratic interactions or interference between participants’ memory systems and the
1318 stimulus. This challenge implies that, to understand the fundamental nature of memory
1319 in its “pure” form, we should study memory for highly simplified stimuli that can pro-
1320 vide relatively unbiased (compared with real-world experiences) measures of the relevant
1321 patterns and tendencies.

1322 The experiment we report in this paper was designed to help bridge some of this gap
1323 between naturalistic tasks and more traditional list-learning tasks. We had people study
1324 word lists similar to those used in classic memory studies, but we also systematically var-
1325 ied the lists’ “richness” (by adding or removing visual features) and temporal structure
1326 (through order manipulations that varied over time and across experimental conditions).
1327 We found that participants’ memory behaviors were sensitive to these manipulations.
1328 Some of the manipulations led to changes that were common across people (e.g., more
1329 temporal clustering when words’ appearances were varied, enhanced memory for lists
1330 following an “event boundary,” more feature clustering on order-manipulated lists, etc.).
1331 Other manipulations led to changes that were idiosyncratic (especially carryover effects
1332 from order manipulations; e.g., participants who remembered more words on early order-
1333 manipulated lists tended to show stronger feature clustering for their condition’s feature
1334 dimension on late randomly ordered lists, etc.). We also found that participants remem-

bered more words from lists that were sorted to align with their idiosyncratic clustering preferences. Taken together, our results suggest that our memories are susceptible to external influences (i.e., to the statistical structure of ongoing experiences), but the effects of past experiences on future memory are largely idiosyncratic across people.

Potential applications

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

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

Our findings indicate that the ways word lists are presented affects how people remember them. To the extent that word list memory reflects memory processes that are relevant to real-world experiences, one could imagine potential real-world applications of our findings. For example, we found that participants remembered more words when the presentation order agreed with their memory fingerprints. If analogous fingerprints could be estimated for classroom content, perhaps they could be utilized manually by teachers, or even by automated content-presentation systems, to optimize how and what students

1359 remember.

1360 **Concluding remarks**

1361 Our work raises deep questions about the fundamental nature of human learning. What
1362 are the limits of our memory systems? How much does what we remember (and how we
1363 remember) depend on how we learn or experience the to-be-remembered content? We
1364 know that our expectations, strategies, situation models learned through prior experiences,
1365 and more collectively shape how our experiences are remembered. But those aspects of
1366 our memory are not fixed: when we are exposed to the same experience in a new way, it
1367 can change how we remember that experience, and also how we remember, process, or
1368 perceive *future* experiences.

1369 **Author contributions**

1370 Conceptualization: JRM and ACH. Data curation: JRM, PCF, ACH. Formal analysis:
1371 JRM, PCF, and ACH. Funding acquisition: JRM. Investigation: ECW, PCF, MRL, AMF,
1372 BJB, DR, CEF, and ACH. Methodology: JRM and ACH. Project administration: ECW
1373 and PCF. Resources: JRM. Software: JRM, PCF, CEF, and ACH. Supervision: JRM and
1374 ACH. Validation: JRM, PCF, and ACH. Writing (original draft): JRM. Writing (review and
1375 editing): ECW, PCF, MRL, AMF, BJB, DR, CEF, and ACH.

1376 **Author note**

1377 All of the data analyzed in this manuscript, along with all of the code for carrying out the
1378 analyses may be found at <https://github.com/ContextLab/FRFR-analyses>. Code for run-
1379 ning the non-adaptive experimental conditions may be found at [https://github.com/Con-](https://github.com/ContextLab/efficient-learning-code)
1380 [textLab/efficient-learning-code](https://github.com/ContextLab/efficient-learning-code). Code for running the adaptive experimental condition

1381 may be found at <https://github.com/ContextLab/adaptiveFR>. We have also released an as-
1382 sociated Python toolbox for analyzing free recall data, which may be found at [https://cdl-
1383 quail.readthedocs.io/en/latest](https://cdl-quail.readthedocs.io/en/latest). Note that this study was not preregistered. Some of the
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