

1 Carryover effects in free recall reveal how past experiences
2 influence memories of future experiences

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4 **Abstract**

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

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

18 Introduction

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

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

32 Our understanding of how we form situation models and schemas, and how they
33 interact with our subjective experiences and memories, is constrained in part by substantial
34 differences in how we study these processes. Situation models and schemas are most often
35 studied using “naturalistic” stimuli such as narratives and movies (Nastase et al., 2020;
36 Zwaan et al., 1995; Zwaan and Radvansky, 1998). In contrast, our understanding of how
37 we organize our memories has been most widely studied using more traditional paradigms
38 like free recall of random word lists (Kahana, 2012, 2020). In free recall, participants study
39 lists of items and are instructed to recall the items in any order they choose. The orders
40 in which words come to mind can provide insights into how participants have organized
41 their memories of the studied words. Because random word lists are unstructured by
42 design, it is not clear if or how non-trivial situation models might apply to these stimuli.

43 Nevertheless, there are *some* commonalities between memory for word lists and memory
44 for real-world experiences.

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

57 Over the past half-century, context-based models have enjoyed impressive success at
58 explaining many stereotyped behaviors observed during free recall and other list-learning
59 tasks (Estes, 1955; Glenberg et al., 1983; Howard and Kahana, 2002; Kimball et al., 2007;
60 Polyn and Kahana, 2008; Polyn et al., 2009; Raaijmakers and Shiffrin, 1980; Sederberg et al.,
61 2008; Shankar and Howard, 2012; Sirotin et al., 2005). These phenomena include the well-
62 known recency and primacy effects (superior recall of items from the end and, to a lesser
63 extent, from the beginning of the study list), as well as semantic and temporal clustering
64 effects (Kahana et al., 2008). The contiguity effect is an example of temporal clustering,
65 which is perhaps the dominant form of organization in free recall. This effect can be
66 seen in the tendency for people to successively recall items that occupied neighboring
67 positions in the study list (Kahana, 1996). There are also striking effects of semantic

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

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

91 We designed an experimental paradigm to explore how people organize their mem-
92 ories for simple stimuli (word lists) whose temporal properties change across different

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

112 **Materials and methods**

113 **Participants**

114 We enrolled a total of 491 Dartmouth undergraduate students across 11 experimental
115 conditions. The conditions included two primary controls (feature rich, reduced), two

116 secondary controls (reduced (early), reduced (late)), six order manipulation conditions
117 (category, size, length, first letter, color, and location), and a final adaptive condition. Each
118 of these conditions are described in the *Experimental design* subsection below.

119 Participants received course credit for enrolling in our study. We asked each partic-
120 ipant to fill out a demographic survey that included questions about their age, gender,
121 ethnicity, race, education, vision, reading impairments, medications or recent injuries,
122 coffee consumption on the day of testing, and level of alertness at the time of testing. All
123 components of the demographics survey were optional. One participant elected not to fill
124 out any part of the demographic survey, and all other participants answered some or all
125 of the survey questions.

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

137 Participants were assigned to experimental conditions based loosely on their date of
138 participation. (This aspect of our procedure helped us to more easily synchronize the
139 experiment databases across multiple testing computers.) Of the 490 participants who
140 opted to fill out the demographics survey, reported ages ranged from 17 to 31 years

141 (mean: 19.1 years; standard deviation: 1.356 years). A total of 318 participants reported
142 their gender as female, 170 as male, and two participants declined to report their gender.
143 A total of 442 participants reported their ethnicity as “not Hispanic or Latino,” 39 as
144 “Hispanic or Latino,” and nine declined to report their ethnicity. Participants reported
145 their races as White (345 participants), Asian (120 participants), Black or African American
146 (31 participants), American Indian or Alaska Native (11 participants), Native Hawaiian or
147 Other Pacific Islander (four participants), Mixed race (three participants), Middle Eastern
148 (one participant), and Arab (one participant). A total of five participants declined to report
149 their race. We note that several participants reported more than one of racial category.
150 Participants reported their highest degrees achieved as “Some college” (359 participants),
151 “High school graduate” (117 participants), “College graduate” (seven participants), “Some
152 high school” (five participants), “Doctorate” (one participant), and “Master’s degree”
153 (one participant). A total of 482 participants reported no reading impairments, and eight
154 reported having mild reading impairments. A total of 489 participants reported having
155 normal color vision and one participant reported that they were red-green color blind.
156 A total of 482 participants reported taking no prescription medications and having no
157 recent injuries; four participants reported having ADHD, one reported having dyslexia,
158 one reported having allergies, one reported a recently torn ACL/MCL, and one reported
159 a concussion from several months prior. The participants reported consuming 0 – 3 cups
160 of coffee prior to the testing session (mean: 0.32 cups; standard deviation: 0.58 cups).
161 Participants reported their current level of alertness, and we converted their responses
162 to numerical scores as follows: “very sluggish” (-2), “a little sluggish” (-1), “neutral” (0),
163 “a little alert” (1), and “very alert” (2). Across all participants, the full range of alertness
164 levels were reported (range: -2 – 2; mean: 0.35; standard deviation: 0.89).

165 We dropped from our dataset the one participant who reported having abnormal color

vision, as well as 39 participants whose data were corrupted due to technical failures while running the experiment or during the daily database merges. In total, this left usable data from 452 participants, broken down by experimental condition as follows: feature rich (67 participants), reduced (61 participants), reduced (late) (41 participants), reduced (early), (42 participants), category (30 participants), size (30 participants), length (30 participants), first letter (30 participants), color (31 participants), location (30 participants), and adaptive (60 participants). The participant who declined to fill out their demographic survey participated in the location condition, and we verified verbally that they had normal color vision and no significant reading impairments.

Experimental design

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



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 the first lists 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.

190 Stimuli

191 The stimuli in our paradigm were 256 English words selected in a previous study (Ziman
192 et al., 2018). The words all referred to concrete nouns, and were chosen from 15 unique se-
193 mantic categories: body parts, building-related, cities, clothing, countries, flowers, fruits,
194 insects, instruments, kitchen-related, mammals, (US) states, tools, trees, and vegetables.
195 We also tagged each word according to the approximate size of the object the word re-
196 ferred to. Words were labeled as “small” if the corresponding object was likely able to
197 “fit in a standard shoebox” or “large” if the object was larger than a shoebox. Semantic
198 categories varied in how many object sizes they reflected (mean number of different sizes
199 per category: 1.33; standard deviation: 0.49). The numbers of words in each semantic
200 category also varied from 12 – 28 (mean number of words per category: 17.07; standard
201 deviation number of words: 4.65). We also identified lexicographic features for each word,
202 including the words’ first letters and lengths (i.e., number of letters). Across all categories,
203 all possible first letters were represented except for ‘Q’ (average number of unique first
204 letters per category: 11; standard deviation: 2 letters). Word lengths ranged from 3 – 12
205 letters (average: 6.17 letters; standard deviation: 2.06 letters).

206 We assigned the categorized words into a total of 16 lists with several constraints.
207 First, we required that each list contained words from exactly 4 unique categories, each
208 with exactly 4 exemplars from each category. Second, we required that (across all words
209 on the list) at least one instance of both object sizes were represented. On average, each
210 category was represented in 4.27 lists (standard deviation: 1.16 lists). Aside from these
211 two constraints, we assigned each word to a unique list. After random assignment, each
212 list contained words with an average of 11.13 unique starting letters (standard deviation:
213 1.15 letters) and an average word length of 6.17 letters (standard deviation: 0.34 letters).

214 The above assignments of words to lists was performed once across all participants,

215 such that every participant studied the same set of 16 lists. In every condition we random-
216 ized the study order of these lists across participants. For participants in some conditions,
217 on some lists, we also randomly varied two additional visual features associated with each
218 word: the presentation font color, and the word’s onscreen location. These attributes were
219 assigned independently for each word (and for every participant). These visual features
220 were varied for words in all lists and conditions except for the “reduced” condition (all
221 lists), the first eight lists of the “reduced (early)” condition, and the last eight lists of the
222 “reduced (late)” condition. In these latter cases, words were all presented in black at the
223 center of the experimental computer’s display.

224 To select a random font color for each word, we drew three integers uniformly and
225 at random from the interval $[0, 255]$, corresponding to the red (r), green (g), and blue (b)
226 color channels for that word. To assign random presentation locations to each word, we
227 selected two floating point numbers uniformly at random (one for the word’s horizontal
228 x coordinate and the other for its vertical y coordinate). The bounds of these coordinates
229 were selected to cover the entire visible area of the display without cutting off any part of
230 the words. The words were shown on 27 in (diagonal) Retina 5K iMac displays (resolution:
231 5120×2880 pixels).

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

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

238 Our experimental paradigm incorporates the Google Cloud Speech API speech-to-text en-
239 gine (Halpern et al., 2016) to automatically transcribe participants’ verbal recalls into text.
240 This allows recalls to be transcribed in real time– a distinguishing feature of the experi-
241 ment; in typical verbal recall experiments the audio data must be parsed and transcribed
242 manually. In prior work, we used a similar experimental setup (equivalent to the “re-
243 duced” condition in the present study) to verify that the automatically transcribed recalls
244 were sufficiently close to human-transcribed recalls to yield reliable data (Ziman et al.,
245 2018). This real-time speech processing component of the paradigm plays an important
246 role in the “adaptive” condition of the experiment, as described below.

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

248 We used four “control” conditions to evaluate and explore participants’ baseline behaviors.
249 We also used performance on these control conditions to help interpret performance in
250 other “manipulation” conditions. Two control conditions served as “anchor points.” In the
251 first anchor point condition, which we call the *feature rich* condition, we randomly shuffled
252 the presentation order (independently for each participant) of the words on each list. In
253 the second anchor point condition, which we call the *reduced* condition, we randomized
254 word presentations as in the feature rich condition. However, rather than assigning each
255 word a random color and location, we instead displayed all of the words in black and at
256 the center of the screen.

257 In the *reduced (early)* condition, we followed the “reduced” procedure (presenting each
258 word in black at the center of the screen) for early lists, and followed the “feature rich”
259 procedure (presenting each word in a random color and location) for late lists. Finally, in
260 the *reduced (late)* condition, we followed the feature rich procedure for early lists and the

261 reduced procedure for late lists.

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

263 Each of six *order manipulation* conditions used a different feature-based sorting procedure
264 to order words on early lists, where each sorting procedure relied on one relevant feature
265 dimension. All of the irrelevant features varied freely across words on early lists, in
266 that we did not consider irrelevant features in ordering the early lists. However, some
267 features were correlated— for example, some semantic categories of words referred to
268 objects that tended to be a particular size, which meant that category and size were not
269 fully independent. On late lists, the words were always presented in a randomized order
270 (chosen anew for each participant). In all of the order manipulation conditions, we varied
271 words’ font colors and onscreen locations, as in the feature rich condition.

272 **Defining feature-based distances.** Sorting words according to a given relevant feature
273 requires first defining a distance function for quantifying the dissimilarity between each
274 pair of features. This function varied according to the type of features. Semantic features
275 (category and size) are *categorical*. For these features, we defined a binary distance function:
276 two words were considered to “match” (i.e., have a distance of 0) if their labels are the
277 same (i.e., both from the same semantic category or both of the same size). If two words’
278 labels were different for a given feature, we defined the words to have a distance of 1
279 for that feature. Lexicographic features (length and first letter) are *discrete*. For these
280 features we defined a discrete distance function. Specifically, we defined the distance
281 between two words as either the absolute difference between their lengths, or the absolute
282 distance between their starting letters in the English alphabet, respectively. For example,
283 two words that started with the same letter would have a “first letter” distance of 0, and
284 words starting with ‘J’ and ‘A’ respectively 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" takes on multiple (positive) real values. We defined the "color" and "location" distances between two words as the Euclidean distances between their (r, g, b) color or (x, y) location vectors, respectively. Therefore the color and location distance measures always take on positive real values (upper-bounded at 441.67 for color, or 27 in for location, reflecting the distances between the corresponding maximally different vectors).

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

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

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

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

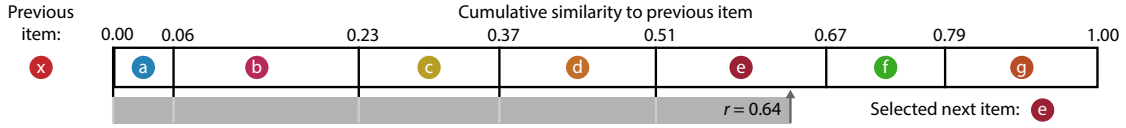


Figure 2: Generating stochastic feature-sorted lists. For a given feature dimension (e.g., color), we compute the similarity (Eqn. 1) between the feature value(s) of the previous item, x , and all yet-to-be-presented items ($a - g$). Next, we normalize these similarity scores so that they sum to one. We lay, in sequence, a set of “sticks,” one for each candidate item, whose lengths are equal to these normalized similarity scores. Note that the combined lengths of these sticks is one. 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.

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

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

318 **Adaptive condition**

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

324 All participants in the adaptive condition began the experiment by studying a set of
325 four *initialization* lists. Words and features on these lists were presented in a randomized
326 order (computed independently for each participant). These initialization lists were used
327 to estimate each participant’s “memory fingerprint,” defined below. At a high level,
328 a participant’s memory fingerprint describes how they prioritize or consider different
329 semantic, lexicographic, and/or visual features when they organize their memories.

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

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

342 **Feature clustering scores (uncorrected).** Feature clustering scores describe participants’
343 tendencies to recall similar presented items together in their recall sequences, where
344 “similarity” considers one given feature dimension (e.g., category, color, etc.). We base
345 our main approach to computing clustering scores on analogous temporal and semantic
346 clustering scores developed by Polyn et al. (2009). Computing the clustering score for
347 one feature dimension starts by considering the corresponding feature values from the
348 first word the participant recalled correctly from the just-studied list. Next, we sort all
349 not-yet-recalled words in ascending order according to their feature-based distance to the
350 just-recalled item (see *Defining feature-based distances*). We then compute the percentile rank
351 of the observed next recall. We average these percentile ranks across all of the participant’s
352 recalls for the current list to obtain a single uncorrected clustering score for the list, for the
353 given feature dimension. We repeated this process for each feature dimension in turn to
354 obtain a single uncorrected clustering score for each list, for each feature dimension.

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

365 **Permutation-corrected feature clustering scores.** Suppose that two lists contain unequal
366 numbers of items of each size. For example, suppose that list *A* contains all “large” items,
367 whereas list *B* contains an equal mix of “large” and “small” items. For a participant
368 recalling list *A*, any correctly recalled item will necessarily match the size of the previous
369 correctly recalled item. In other words, successively recalling several list *A* items of the
370 same size is essentially meaningless, since *any* correctly recalled list *A* word will be large.
371 In contrast, successively recalling several list *B* items *could* be meaningful, since (early in
372 the recall sequence) the yet-to-be-recalled items come from a mix of sizes. However, once
373 all of the small items on list *B* have been recalled, the best possible next matching recall
374 will be a large item. And all subsequent correct recalls must also be large items– so for
375 those later recalls it becomes difficult to determine whether the participant is successively
376 recalling large items because they are organizing their memories according to size, or
377 (alternatively), whether they are simply recalling the yet-to-be-recalled items in a random
378 order. In general, the precise order and blend of feature values expressed in a given list,
379 the orders and numbers of correct recalls a participant makes, the number of intervening
380 presentation positions between successive recalls, and so on, can all affect the range of
381 clustering scores that are possible to observe for a given list. An uncorrected clustering
382 score therefore conflates participants’ actual memory organization with other “nuisance”
383 factors.

384 Following our prior work (Heusser et al., 2017), we used a permutation-based cor-
385 rection procedure to help isolate the behavioral aspects of clustering that we were most
386 interested in. After computing the uncorrected clustering score (for the given list and
387 observed recall sequence), we compute a “null” distribution of n additional clustering
388 scores after randomly shuffling the order of the recalled words (we use $n = 500$ in the
389 present study). This null distribution represents an approximation of the range of cluster-

ing scores one might expect to observe by “chance,” given that a hypothetical participant was *not* truly clustering their recalls, but where the hypothetical participant still studied and recalled exactly the same items (with the same features) as the true participant. We define the *permutation-corrected clustering score* as the percentile rank of the observed uncorrected clustering score in this estimated null distribution. In this way, a corrected score of 1 indicates that the observed score was greater than any clustering score one might expect by chance; in other words, good evidence that the participant was truly clustering their recalls along the given feature dimension. We applied this correction procedure to all of the clustering scores (feature and temporal) reported in this paper.

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

Online “fingerprint” analysis. The presentation orders of some lists in the adaptive condition of our experiment (see *Adaptive condition*) were sorted according to participants’ *current* memory fingerprint, estimated using all of the lists they had studied up to that point in the experiment. Because our experiment incorporated a speech-to-text component, all of the behavioral data for each participant could be analyzed just a few seconds after the conclusion of the recall intervals for each list. We used the Quail Python package (Heusser

et al., 2017) to apply speech-to-text algorithms to the just-collected data, aggregate the data for the given participant, and estimate the participant’s memory 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 loaded as needed in future computations. This meant that our computations pertaining to updating our estimate of a participant’s memory fingerprint only needed to consider data from the most recent list. Second, each element of the null distributions of uncorrected fingerprint scores (see *Permutation-corrected feature clustering scores*) could be estimated independently from the others. This enabled us to make use of the testing computers’ multi-core CPU architectures by elements of the null distributions in batches of eight (i.e., the number of CPU cores on each testing computer). Taken together, we were able to 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

439 items. These permutations served as candidate presentation orders. We sought to select
440 the specific order that most (or least) matched f . Third, for each random permutation, we
441 computed the (permutation-corrected) “fingerprint,” treating the permutation as though
442 it were a potential “perfect” recall sequence. (We did not include temporal clustering
443 scores in these fingerprints.) This yielded a “simulated fingerprint” vector, \hat{f}_p for each
444 permutation p . We used these simulated fingerprints to select a specific permutation, i ,
445 that either maximized (for stabilize lists) or minimized (for destabilize lists) the correlation
446 between \hat{f}_i and f .

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

448 **JRM NOTE: REMINDER TO CHECK THIS PARAGRAPH AGAINST ANALYSIS**
449 **CODE FOR ACCURACY...** Following some of our prior work (Heusser et al., 2021,
450 2018), we use low-dimensional embeddings to help visualize how participants’ memory
451 fingerprints change across lists (Figs. 6A, S8A). To compute a shared embedding space
452 across participants and experimental conditions, we concatenated the full set of finger-
453 prints (across all lists, participants, and experimental conditions) to create a large matrix
454 with number-of-lists \times number-of-participants rows and seven columns (one for each
455 feature clustering score, plus an additional temporal clustering score column). We used
456 principal components analysis to project the seven-dimensional observations into a two-
457 dimensional space (using the two principal components that explained the most variance
458 in the data). For two visualizations (Figs. 6B, and S8B) we computed an additional set of
459 two-dimensional embeddings for participants’ *average* fingerprints (i.e., across lists within
460 a given group of lists— early or late). For those visualizations we averaged across the rows
461 (for each condition and group of lists) in the combined fingerprint matrix prior to pro-
462 jecting it into the shared two-dimensional space. This yielded a single two-dimensional

coordinate for each *list group*, 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.

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. To carry out this analysis, we initialized (for each participant) a number-of-lists (16) by number-of-words-per-list (16) matrix of zeros. Then, for each list, we found the index of the word that was recalled first, and we 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).

Lag-conditional response probability curve

The lag-conditional probability (lag-CRP) curve (Kahana, 1996) reflects the probability of recalling a given item after the just-recalled item, as a function of their relative encoding positions (lag). In other words, a lag of 1 indicates that a recalled item was presented immediately after the previously recalled item, and a lag of -3 indicates that a recalled item came three items before the previously recalled item. For each recall transition (following

the first recall), we computed the lag between the just-recalled word's presentation position and the next-recalled word's presentation position. We computed the proportions of transitions (between successively recalled words) for each lag, normalizing for the total numbers of possible transitions. In carrying out this analysis, we excluded all incorrect recalls and successive repetitions (e.g., recalling the same word twice in a row). This yielded, for each list, a 1 by number-of-lags (-15 to +15; 30 lags in total, excluding lags of 0) array of conditional probabilities. We averaged these probabilities across lists to obtain a single lag-CRP for each participant.

Serial position curve

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

Identifying event boundaries

We used the distances between feature values for successively presented words (see *Defining feature-based distances*) to estimate "event boundaries" where the feature values changed more than usual (DuBrow and Davachi, 2016; Ezzyat and Davachi, 2011; Manning et al.,

2016; Radvansky and Copeland, 2006; Swallow et al., 2011, 2009). For each list, for each feature dimension, we computed the distribution of distances between the feature values for successively presented words. We defined event boundaries (e.g., Fig. 3B) as occurring between any successive pair of words whose distances along the given feature dimension were greater than one standard deviation above the mean for that list. Note that, because event boundaries are defined for each feature dimension, each individual list may contain several sets of event boundaries, each at different moments in the presentation sequence (depending on the feature dimension of interest).

Results

While holding the set of words (and the assignments of words to lists) constant, we manipulated two aspects of participants' experiences of studying each list. We sought to understand the effects of these manipulations on participants' memories for the studied words. First, we added two additional sources of visual variation to the individual word presentations: font color and onscreen location. Importantly, these visual features were independent of the meaning or semantic content of the words (e.g., word category, size of the referent, etc.) and of the lexicographic properties of the words (e.g., word length, first letter, etc.). We wondered whether this additional word-independent information might facilitate recall (e.g., by providing new potential ways of organizing or retrieving memories of the studied words) or impair recall (e.g., by distracting participants with irrelevant information). 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. We also

533 wondered whether some order manipulations might be temporally “sticky” by influencing
534 how *future* lists were remembered.

535 To obtain a clean preliminary estimate of the consequences on memory of randomly
536 varying the font colors and locations of presented words (versus holding the font color
537 fixed at black, and holding the display locations fixed at the center of the display) we
538 compared participants’ performance on the *feature rich* and *reduced* experimental condi-
539 tions (see *Random conditions*, Fig. S1). In the feature rich condition the words’ colors and
540 locations varied randomly across words, and in the reduced condition words were always
541 presented in black, at the center of the display. Aggregating across all lists for each par-
542 ticipant, we found no difference in recall accuracy for feature rich versus reduced lists
543 ($t(126) = -0.290, p = 0.772$). However, participants in the feature rich condition clustered
544 their recalls substantially more along every dimension we examined (temporal clustering:
545 $t(126) = 10.624, p < 0.001$; category clustering: $t(126) = 10.077, p < 0.001$; size clustering:
546 $t(126) = 11.829, p < 0.001$; word length clustering: $t(126) = 10.639, p < 0.001$; first let-
547 ter clustering: $t(126) = 7.775, p < 0.001$; see *Permutation-corrected feature clustering scores*
548 for more information about how we quantified each participant’s clustering tendencies.)
549 Taken together, these comparisons suggest that adding new features changes how par-
550 ticipants organize their memories of studied words, even when those new features are
551 independent of the words themselves and even when the new features vary randomly
552 across words. We found no evidence that those additional uninformative features were
553 distracting (in terms of their impact on memory performance), but they did affect partici-
554 pants’ recall dynamics (measured via their clustering scores).

555 We also wondered whether adding these irrelevant visual features to later lists (after
556 the participants had already studied impoverished lists), or removing the visual features
557 from later lists (after the participants had already studied visually diverse lists) might affect

memory performance. 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 and reduced conditions. Participants in both conditions remembered more words on early versus late lists (feature rich: $t(66) = 4.553, p < 0.001$; reduced: $t(60) = 2.434, p = 0.018$). Participants in the feature rich (but not reduced) conditions exhibited more temporal clustering on early versus late lists (feature rich: $t(66) = 2.318, p = 0.024$; reduced: $t(60) = 0.929, p = 0.357$). And participants in both conditions exhibited more semantic (category and size) clustering on early versus late lists (feature rich, category: $t(66) = 3.805, p < 0.001$; feature rich, size: $t(66) = 2.190, p = 0.032$; reduced, category: $t(60) = 2.856, p = 0.006$; reduced, size: $t(60) = 2.947, p = 0.005$). Participants in the reduced (but not feature rich) conditions exhibited more lexicographic clustering on early versus late lists (feature rich, word length: $t(66) = 0.161, p = 0.872$; feature rich, first letter: $t(66) = 0.410, p = 0.683$; reduced, word length: $t(60) = 3.528, p = 0.001$; reduced, first letter: $t(60) = 2.275, p = 0.026$). Taken together, these comparisons suggest that even when the presence or absence of irrelevant 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 irrelevant visual features constant on early lists, but allowed them to vary randomly on late lists. In a *reduced (late)* condition, we allowed the irrelevant visual features to vary randomly on early lists, but held them constant on late lists. Given our above findings that (a) participants tended to remember more

words and 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 diminished in the reduced (late) condition. However, to our surprise, participants in *neither* condition exhibited reliable early versus late differences in accuracy (reduced (early): $t(41) = 1.499, p = 0.141$; reduced (late): $t(40) = 1.462, p = 0.152$), temporal clustering (reduced (early): $t(41) = 0.998, p = 0.324$; reduced (late): $t(40) = 1.099, p = 0.278$), nor feature based clustering (reduced (early), category: $t(41) = 0.753, p = 0.456$; reduced (early), size: $t(41) = 0.721, p = 0.475$; reduced (early), length: $t(41) = 0.493, p = 0.625$; reduced (early), first letter: $t(41) = 0.780, p = 0.440$; reduced (late), category: $t(40) = -0.086, p = 0.932$; reduced (late), size: $t(40) = 0.746, p = 0.460$; reduced (late), length: $t(40) = 1.476, p = 0.148$; reduced (late), first letter: $t(40) = 0.966, p = 0.340$). We hypothesized that adding or removing the irrelevant features was acting as a sort of “event boundary” between early and late lists. 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; Manning et al., 2016).

We found that *adding* irrelevant 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 (accuracy for feature rich versus reduced (early): $t(107) = -2.230, p = 0.028$; reduced versus reduced (early): $t(101) = -2.045, p = 0.043$; 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 performance (accuracy for feature rich versus reduced (late): $t(106) = -0.638, p = 0.525$; reduced versus reduced (late): $t(100) = -0.407, p = 0.685$). These comparisons suggest that recall accuracy has a directional component (i.e., accu-

608 racy is affected differently by removing features later that had been present earlier versus
 609 adding features later that had *not* been present earlier). In contrast, we found that partic-
 610 ipants exhibited more temporal and feature-based clustering when we added irrelevant
 611 visual features to *any* lists (comparisons of clustering on feature rich and reduced lists
 612 are reported above; temporal clustering in reduced versus reduced (early) and reduced
 613 versus reduced (late) conditions: $ts \leq -9.780$, $ps < 0.001$; feature based clustering in re-
 614 duced versus reduced (early) and reduced versus reduced (late) conditions: $ts \leq -5.443$, ps
 615 < 0.001). Temporal and feature-based clustering were not reliably different in the feature
 616 rich, reduced (early), and reduced (late) conditions (temporal clustering in feature rich
 617 versus reduced (early) and feature rich versus reduced (late) conditions: $ts \geq -1.434$, ps
 618 ≥ 0.154 ; feature based clustering in feature rich versus reduced (early) and feature rich
 619 versus reduced (late) conditions: $ts \geq -1.359$, $ps > 0.177$).

620 Taken together, our findings thus far suggest that adding item features that change
 621 over time, even when they vary randomly and independently of the items, can enhance
 622 participants' overall memory performance and can also enhance temporal and feature-
 623 based clustering. To the extent that the number of item features that vary from moment
 624 to moment approximates the "richness" of participants' experiences, our findings sug-
 625 gest that participants remember "richer" stimuli better and organize richer stimuli more
 626 reliably in their memories. Next, we turn to examine the memory effects of varying the
 627 temporal ordering of different stimulus features while holding the features themselves
 628 constant. We hypothesized that changing the order in which participants were exposed
 629 to the words on a given list might enhance (or diminish) the relative influence of different
 630 features. For example, presenting a set of words alphabetically might enhance partici-
 631 pants' attention to the studied items' first letters, whereas sorting the same list of words by
 632 semantic category might instead enhance participants' attention to the words' semantic

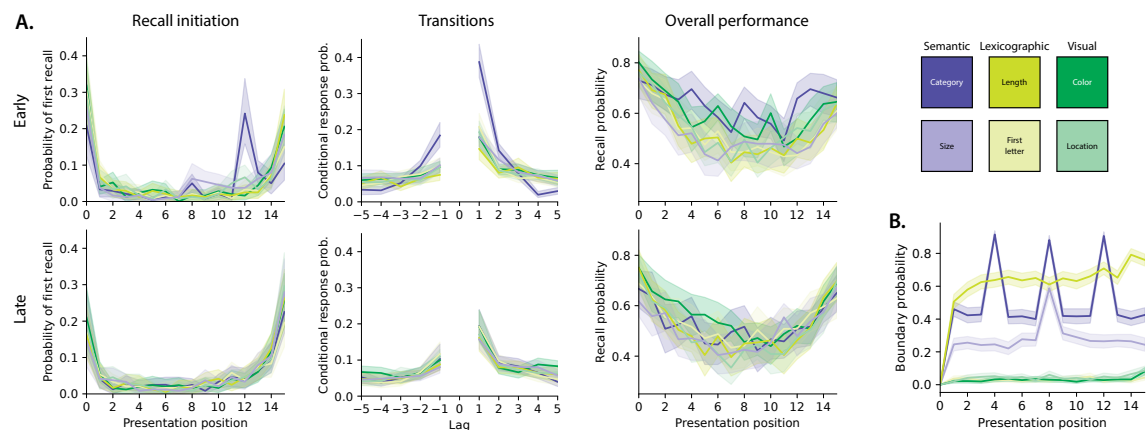


Figure 3: Recall dynamics in feature rich free recall (order manipulation conditions). **A.** Behavioral plots. **Left panels.** The probabilities of initiating recall with each word are plotted as a function of presentation position. **Middle panels.** The conditional probabilities of recalling each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

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 visual features were added or removed from a subset of the lists participants studied).

Across six order manipulation conditions, we sorted early lists by each feature dimension but randomly ordered the items on late lists (see *Order manipulation conditions*; features: category, size, length, first letter, color, and location). Participants in the category-ordered condition showed an increase in memory performance on early lists (accuracy, relative to early feature rich lists; $t(95) = 3.034, p = 0.003$). Participants in the color-ordered condition also showed a trending increase in memory performance on early lists (again, relative to early feature rich lists: $t(96) = 1.850, p = 0.067$). Participants' performance on early lists

644 in all of the other order manipulation conditions was indistinguishable from performance
 645 on the early feature rich lists ($\|t\|s < 1.013, ps > 0.314$). Participants in both of the seman-
 646 tically ordered conditions exhibited stronger temporal clustering on early lists (versus
 647 early feature rich lists; category: $t(95) = 8.508, p < 0.001$; size: $t(95) = 2.429, p = 0.017$).
 648 Participants in the length-ordered condition tended to exhibit *less* temporal clustering
 649 on early lists relative to early feature rich lists ($t(95) = -1.666, p = 0.099$), whereas par-
 650 ticipants in the first letter-ordered condition exhibited stronger temporal clustering on
 651 early lists ($t(95) = 2.587, p = 0.011$). Participants in the visually ordered conditions ex-
 652 hibited more similar performance on early lists, relative to early feature rich lists (color:
 653 $t(96) = -1.064, p = 0.290$; we found a trending enhancement for participants in the location-
 654 ordered condition: $t(95) = 1.682, p = 0.096$). We also compared feature-based clustering
 655 on early lists across the order manipulation and feature rich conditions. Since results were
 656 similar across both semantic conditions (category and size), both lexicographic conditions
 657 (length and first letter), and both visual conditions (color and location), here we aggre-
 658 gate data from conditions that manipulated each of these three feature groupings in our
 659 comparisons to simplify the presentation. On early lists, participants in the semantically
 660 ordered conditions exhibited stronger semantic clustering relative to participants in the
 661 feature rich condition (category: $t(125) = 2.524, p = 0.013$; size: $t(125) = 3.510, p = 0.001$),
 662 but showed no reliable differences in lexicographic (length: $t(125) = 0.539, p = 0.591$; first
 663 letter: $t(125) = -0.587, p = 0.558$) or visual (color: $t(125) = -0.579, p = 0.564$; location:
 664 $t(125) = -0.346, p = 0.730$) clustering. Similarly, participants in the lexicographically or-
 665 dered conditions exhibited stronger (relative to feature rich participants) lexicographic
 666 clustering (length: $t(125) = 3.426, p = 0.001$; first letter: $t(125) = 3.236, p = 0.002$) on early
 667 lists, but showed no reliable differences in semantic (category: $t(125) = -1.078, p = 0.283$;
 668 size: $t(125) = -0.310, p = 0.757$) or visual (color: $t(125) = -0.209, p = 0.835$; location:

669 $t(125) = -0.004, p = 0.997$) clustering. And participants in the visually ordered condi-
 670 tions exhibited stronger visual clustering (again, relative to feature rich participants, and
 671 on early lists; color: $t(126) = 2.099, p = 0.038$; location: $t(126) = 4.392, p = 0.000$), but
 672 showed now reliable differences in semantic (category: $t(126) = 0.204, p = 0.839$; size:
 673 $t(126) = -0.093, p = 0.926$) or lexicographic (length: $t(126) = 0.714, p = 0.476$; first letter:
 674 $t(126) = 0.820, p = 0.414$) clustering. Taken together, these order manipulation results sug-
 675 gest several broad patterns (Figs. 3A, 4). First, most of the order manipulations we carried
 676 out did *not* reliably affect overall recall performance. Second, most of the order manipula-
 677 tions increased participants' tendencies to temporally cluster their recalls. Third, all of the
 678 order manipulations enhanced participants' clustering of each condition's target feature
 679 (i.e., semantic manipulations enhanced semantic clustering, lexicographic manipulations
 680 enhanced lexicographic clustering, and visual manipulations enhanced visual clustering)
 681 while leaving clustering along other feature dimensions roughly unchanged (i.e., semantic
 682 manipulations did not affect lexicographic or color clustering, and so on).

683 When we closely examined the sequences of words participants recalled in early order
 684 manipulated lists (Fig. 3A, top panel), we noticed several differences from the dynamics of
 685 participants' recalls of randomly ordered lists (Figs. S1, S7). One striking difference is that
 686 participants in the category condition (dark purple curves, Fig. 3) most often initiated recall
 687 with the fourth-from-last item (*Recall initiation*, top left panel), whereas participants who
 688 recalled randomly ordered lists tended to initiate recall with either the first or last list items
 689 (Fig. S1, top left panel). We hypothesized that the participants might be "clumping" their
 690 recalls into groups of items that shared category labels. Indeed, when we compared the
 691 positions of feature changes in the study sequence (Fig. 3B; see *Identifying event boundaries*)
 692 with the positions of items participants recalled first, we noticed a striking correspondence
 693 in both semantic conditions. Specifically, on category-ordered lists, the category labels

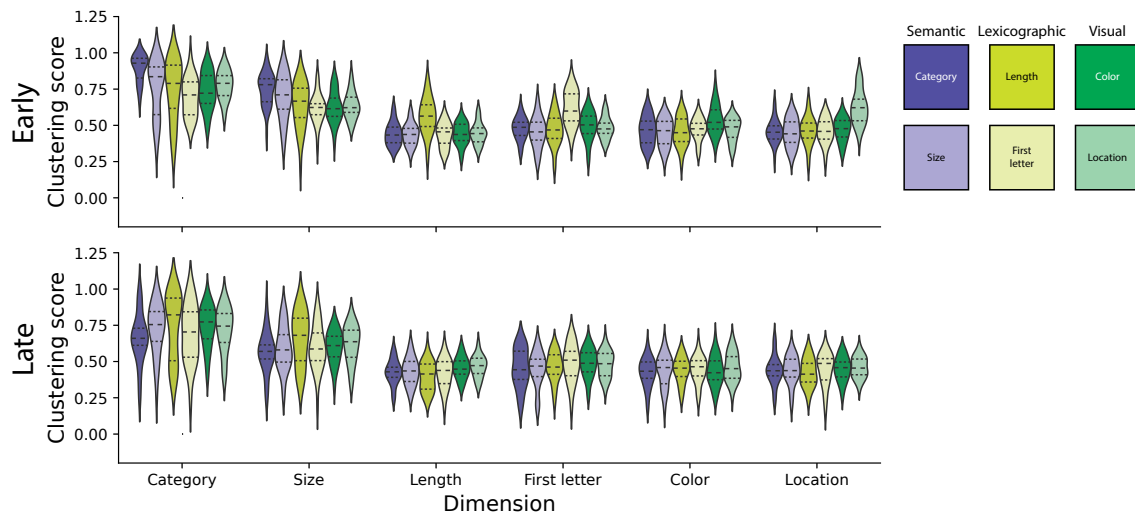


Figure 4: Memory “fingerprints” (order manipulation conditions). The across-participant distributions of clustering scores for each feature type (x -coordinate) are displayed for each experimental condition (color), separately for order manipulation (early, top) and randomly ordered (late, bottom) lists.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

694 changed every four items on average (dark purple peaks in Fig. 3B), and participants
 695 also seemed to display an increased tendency (relative to other order manipulation and
 696 random conditions) to initiate recall of category-ordered lists with items whose study
 697 positions were integer multiples of four. Similarly, for size-ordered lists, the size labels
 698 changed every eight items on average (light purple peaks in Fig. 3B), and participants
 699 also seemed to display an increased tendency to initiate recall of size-ordered lists with
 700 items whose study positions were integer multiples of eight. A second striking difference
 701 is that participants in the category condition exhibited a much steeper lag-CRP (Fig. 3A,
 702 top middle panel) than participants in other conditions. (This is another expression of
 703 participants’ increased tendencies to temporally cluster their recalls on category-ordered
 704 lists, as we reported above.) Taken together, these order-specific idiosyncrasies suggest
 705 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 irrelevant visual features. At shorter timescales, “event boundaries” can be induced across items (within a single list) by adjusting how item features change throughout the list.

The above comparisons between memory performance on early lists in the order manipulation versus feature rich conditions highlight how sorted lists are remembered differently from random lists. We also wondered how sorting lists along each feature dimension influenced memory relative to sorting lists along the other feature dimensions. Participants trended towards remembering early lists that were sorted semantically better than lexicographically sorted lists ($t(118) = 1.936, p = 0.055$). Participants also remembered visually sorted lists better than lexicographically sorted lists ($t(119) = 2.145, p = 0.034$). However, participants showed no reliable differences in recall performance on semantically versus visually sorted lists ($t(119) = 0.113, p = 0.910$). Participants temporally clustered semantically sorted lists more strongly than either lexicographically ($t(118) = 5.572, p < 0.001$) or visually ($t(119) = 6.215, p < 0.001$) sorted lists, but did not show reliable differences in temporal clustering on lexicographically versus visually sorted lists ($t(119) = 0.189, p = 0.850$). Participants also showed reliably more semantic clustering on semantically sorted lists than lexicographically (category: $t(118) = 3.492, p = 0.001$, size: $t(118) = 3.972, p < 0.001$) or visually (category: $t(119) = 2.702, p = 0.008$, size: $t(119) = 4.230, p < 0.001$) sorted lists; more lexicographic clustering on lexicographically sorted lists than semantically (length: $t(118) = 3.112, p = 0.002$; first letter: $t(118) = 3.686, p = 0.000$) or visually (length: $t(119) = 3.024, p = 0.003$; first letter: $t(119) = 2.644, p = 0.009$) sorted lists; and more visual clustering on visually sorted lists than semantically (color: $t(119) = -2.659, p = 0.009$; location: $t(119) = -4.604, p = 0.000$) or lexicographically (color: $t(119) = -2.366, p = 0.020$; location: $t(119) = -4.265, p < 0.001$) sorted lists. In summary, sorting lists by different features appeared to have slightly different effects on overall memory performance and

731 temporal clustering, and people tended to cluster their recalls along a given feature di-
732 mension more when the studied lists were (versus were not) sorted along that dimension.

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

740 At the group level, there seemed to be almost no lingering impact of sorting early
741 lists on memory for later lists. To simplify the presentation, we report these null results
742 in aggregate across the three feature groupings. Relative to memory performance on
743 late feature rich lists, participants' memory performance in all six order manipulation
744 conditions showed no reliable differences (semantic: $t(125) = 0.487, p = 0.627$; lexico-
745 graphic: $t(125) = 0.878, p = 0.382$; visual: $t(126) = 1.437, p = 0.153$). Nor did we observe
746 any reliable differences in temporal clustering on late lists (relative to late feature rich
747 lists; semantic: $t(125) = 0.146, p = 0.884$; lexicographic: $t(125) = 0.923, p = 0.358$; visual:
748 $t(126) = 0.525, p = 0.601$). Aside from a slightly increased tendency for participants to
749 cluster words by their length on late visual order manipulation lists (more than late fea-
750 ture rich lists; $t(126) = 2.199, p = 0.030$), we observed no reliable differences in any type of
751 feature clustering on late order manipulation condition lists versus late feature rich lists
752 ($|t|s \leq 1.234, ps \geq 0.220$).

753 We also looked for more subtle group-level patterns. For example, perhaps sorting
754 early lists by one feature dimension could affect how participants cluster *other* features (on
755 early and/or late lists) as well. We defined participants' *memory fingerprints* as the set of

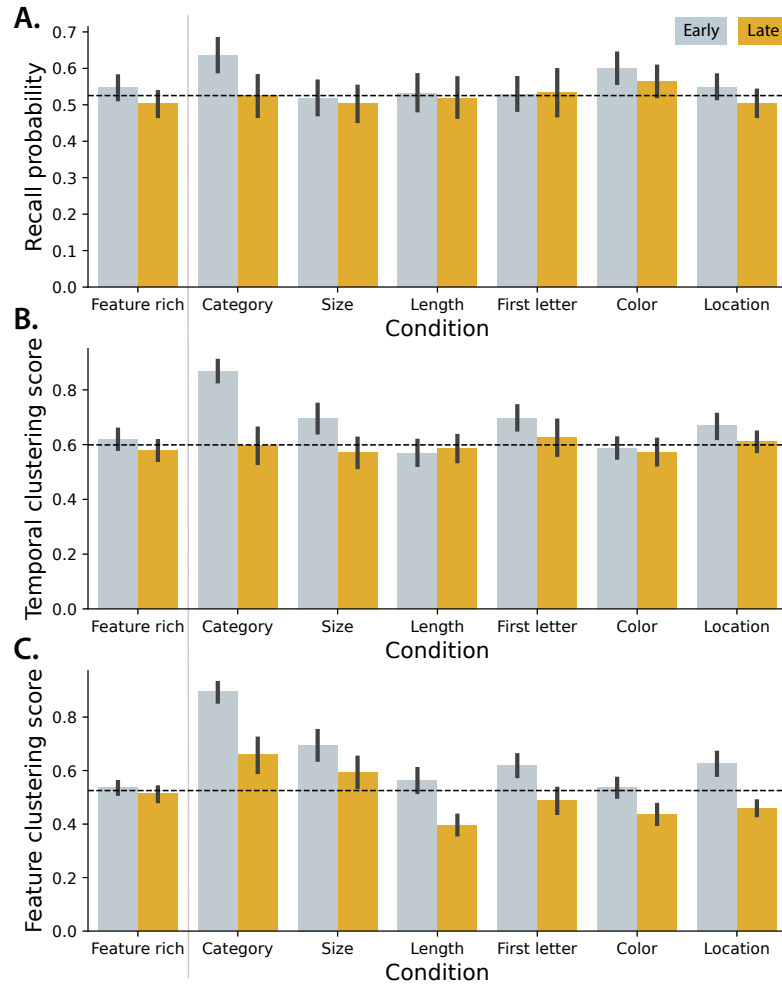


Figure 5: Recall probability and clustering scores on early and late lists. The bar heights display the average (across participants) recall probabilities (A.), temporal clustering scores (B.), and feature clustering scores (C.) for early (gray) and late (gold) lists. For the feature rich bars (left), the feature clustering scores are averaged across features. 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.

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

When we examined the data at the level of individual participants (Figs. 7 and 8), a clearer story emerged. Within each order manipulation condition, participants exhibited a range of feature clustering scores, on both early and late lists (Fig. 7A, B). Across every order manipulation condition, participants who exhibited stronger feature clustering (for



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

781 their condition's manipulated feature) recalled more words. This trend held overall across
 782 conditions and participants (early: $r(179) = 0.537, p < 0.001$; late: $r(179) = 0.492, p = 0.000$)
 783 as well as for each condition individually for early ($r_s \geq 0.386$, all $p_s \leq 0.035$) and late
 784 ($r_s \geq 0.462$, all $p_s \leq 0.010$) lists. We found no evidence of a condition-level trend; for
 785 example the conditions where participants tended to show stronger clustering scores
 786 were not correlated with the conditions where participants remembered more words
 787 (early: $r(4) = 0.526, p = 0.284$; late: $r(4) = -0.257, p = 0.623$; see insets of panels A and
 788 B). We observed carryover associations between feature clustering and recall performance
 789 (Fig. 7C, D). Participants who showed stronger feature clustering on early lists tended to
 790 recall more items on late lists (across conditions: $r(179) = 0.492, p < 0.001$; all conditions
 791 individually: $r_s \geq 0.462$, all $p_s \leq 0.010$). Participants who recalled more items on early lists
 792 also tended to show stronger feature clustering on late lists (across conditions: $r(179) =$

793 0.280, $p < 0.001$; all non-visual conditions: $r_s \geq 0.445$, all $p_s \leq 0.014$; color: $r(29) = 0.298, p =$
 794 0.103; location: $r(28) = 0.354, p = 0.055$). Neither of these effects showed condition-level
 795 trends (early feature clustering versus late recall probability: $r(4) = -0.299, p = 0.565$;
 796 early recall probability versus late feature clustering: $r(4) = 0.400, p = 0.432$). We also
 797 looked for associations between feature clustering and temporal clustering. Across every
 798 order manipulation condition, participants who exhibited stronger feature clustering also
 799 exhibited stronger temporal clustering. For early lists (Fig. 7E), this trend held overall
 800 ($r(179) = 0.924, p < 0.001$), for each condition individually (all $r_s \geq 0.822$, all $p_s < 0.001$),
 801 and across conditions ($r(4) = 0.964, p = 0.002$). For late lists (Fig. 7F), the results were more
 802 variable (overall: $r(179) = 0.348, p = 0.000$; all non-visual conditions: $r_s \geq 0.382$, all p_s
 803 ≤ 0.037 ; color: $r(29) = 0.453, p = 0.011$; location: $r(28) = 0.190, p = 0.314$; across-conditions:
 804 $r(4) = -0.036, p = 0.945$). While less robust than the carryover associations between feature
 805 clustering and recall performance, we also observed some carryover associations between
 806 feature clustering and temporal clustering (Fig. 7G, H). Participants who showed stronger
 807 feature clustering on early lists trended towards showing stronger temporal clustering
 808 on later lists (overall: $r(179) = 0.301, p < 0.001$; for individual conditions: all $r_s \geq 0.297$,
 809 all $p_s \leq 0.111$; across conditions: $r(4) = 0.107, p = 0.840$). And participants who showed
 810 stronger temporal clustering on early lists trended towards showing stronger feature
 811 clustering on later lists (overall: $r(179) = 0.579, p < 0.001$; all non-visual conditions: r_s
 812 ≥ 0.323 , all $p_s \leq 0.082$; visual conditions: $r_s \geq 0.089$, all $p_s \leq 0.632$; across conditions:
 813 $r(4) = 0.916, p = 0.010$). Taken together, the results displayed in Figure 7 show that
 814 participants who were more sensitive to the order manipulations (i.e., participants who
 815 showed stronger feature clustering for their condition's feature on early lists) remembered
 816 more words and showed stronger temporal clustering. These associations also appeared
 817 to carry over across lists, even when the items on later lists were presented in a random

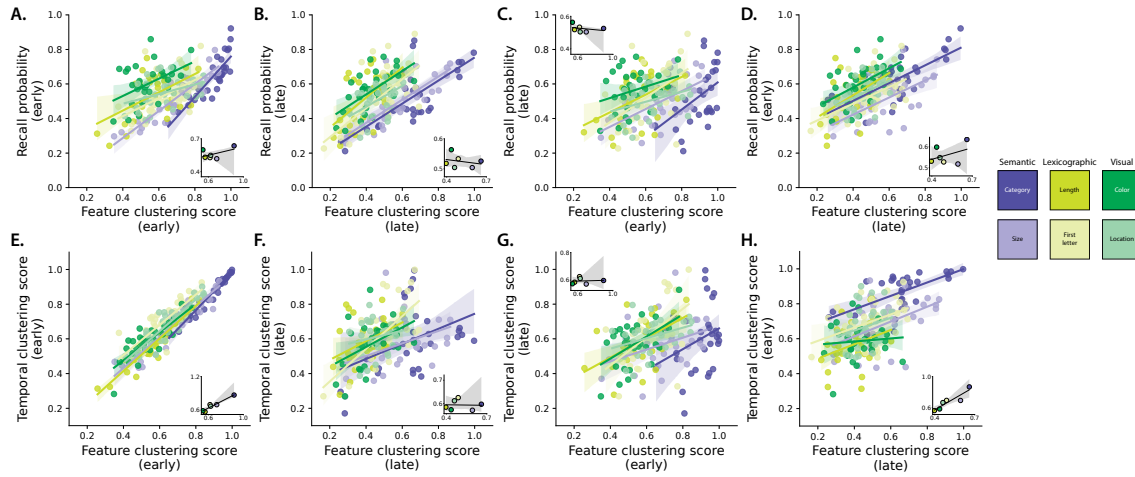


Figure 7: Interactions between feature clustering, recall probability, and contiguity. **A.** Recall probability versus feature clustering scores for order manipulation (early) lists. **B.** Recall probability versus feature clustering for randomly ordered (late) lists. **C.** Recall probability on late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

818 order.

819 If participants show different sensitivities to order manipulations, how do their be-
820 haviors carry over to later lists? We found that participants who showed strong feature
821 clustering on early lists often tended to show strong feature clustering on late lists (Fig. 8A;
822 overall across participants and conditions: $r(179) = 0.592, p < 0.001$; non-visual feature
823 conditions: all $r_s \geq 0.350$, all $p_s \leq 0.058$; color: $r(29) = -0.071, p = 0.704$; location:
824 $r(28) = 0.032, p = 0.868$; across conditions: $r(4) = 0.934, p = 0.006$). Although participants
825 tended to show weaker feature clustering on late lists (Fig. 6) on *average*, the associations
826 between early and late lists for individual participants suggests that some influence of

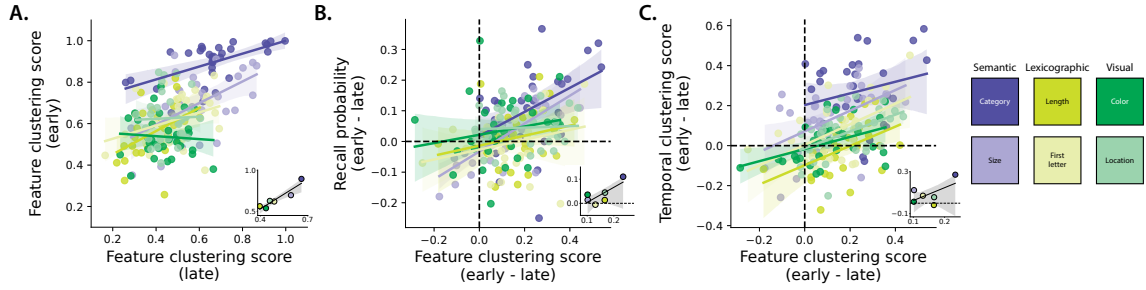


Figure 8: Feature clustering carryover effects. **A.** Feature clustering scores for order manipulation (early) versus randomly ordered (late) lists. **B.** Accuracy differences (on early versus late lists) versus feature clustering “carryover” (defined as the differences between the average clustering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

early order manipulations may linger on late lists. We found that participants who exhibited larger carryover in feature clustering (i.e., continued to show strong feature clustering on late lists) for the semantic order manipulations (but not other manipulations) also tended to show a larger improvement in recall (Fig. 8B; overall: $r(179) = 0.378, p < 0.001$; category: $r(28) = 0.419, p = 0.021$; size: $r(28) = 0.737, p < 0.001$; non-semantic conditions: all $r_s \leq 0.252$, all $p_s \geq 0.179$; across conditions: $r(4) = 0.773, p = 0.072$) on late lists, relative to early lists. Participants who exhibited larger carryover in feature clustering also tended to show stronger temporal clustering on late lists (relative to early lists) for all but the category condition (Fig. 8C; overall: $r(179) = 0.434, p < 0.001$; category: $r(28) = 0.229, p = 0.223$; all non-category conditions: all $r_s \geq 0.448$, all $p_s \leq 0.012$; across conditions: $r(4) = 0.598, p = 0.210$).

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

sion, they will simply adopt that feature as a dominant dimension for organizing those items and subsequent (randomly ordered) items. This flexibility in memory organization might afford such participants a memory advantage, explaining their strong recall performance. An alternative interpretation is that each participant comes into our study with a “preferred” way of organizing incoming information. If they happen to be assigned to an order manipulation condition that matches their preferences, then they will appear to be “sensitive” to the order manipulation and also exhibit a high degree of carryover in feature clustering from early to late lists. These participants might demonstrate strong recall performance not because of their inherently superior memory abilities, but rather because the specific condition they were assigned to happened to be especially easy for them, given their pre-experimental tendencies. To help distinguish between these interpretations, we designed an *adaptive* experimental condition (see *Adaptive condition*). The primary manipulation in the adaptive condition is that participants each experience three key types of lists. On *random* lists, words are ordered randomly (as in the feature rich 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 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 (potentially unique) memory fingerprint, the adaptive condition removes uncertainty about whether participants’ assigned conditions might just “happen” to match their preferred ways of organizing their memories.

Participants’ fingerprints on stabilize and random lists tended to become (numerically)

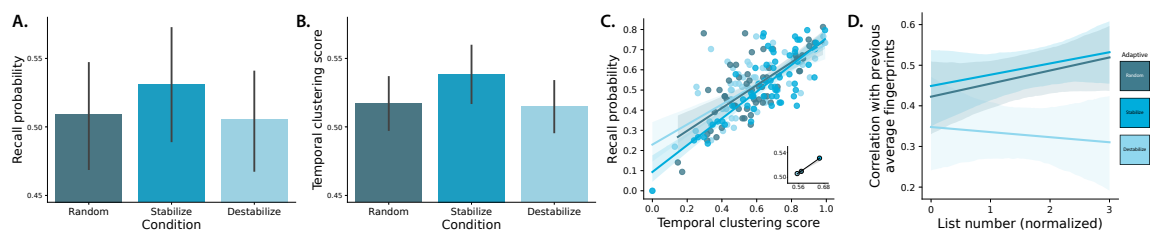


Figure 9: Adaptive free recall. **A.** Average probability of recall (taken across words, lists, and participants) for lists from each adaptive condition. **B.** Average temporal clustering scores for lists from each adaptive condition. **C.** Recall probability versus temporal clustering scores by participant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

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 random ($t(59) = 1.740, p = 0.087$) or destabilize ($t(59) = 1.714, p = 0.092$) lists (Fig. 9A). Participants showed no reliable differences in their memory performance on destabilize versus random lists ($t(59) = -0.249, p = 0.804$). Participants also exhibited stronger temporal clustering on stabilize lists, relative to random ($t(59) = 3.554, p = 0.001$) and destabilize ($t(59) = 4.045, p < 0.001$) lists (Fig. 9B). We found no reliable differences in temporal clustering for items on random versus destabilize lists ($t(59) = -0.781, p = 0.438$).

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 ($r(178) = 0.721, p < 0.001$), and for each list type

881 individually (all $r_s \geq 0.683$, all $p_s \leq 0.001$). Taken together, the results from the adaptive
882 condition suggest that each participant comes into the experiment with their own unique
883 memory organization tendencies, as characterized by their memory fingerprint. When
884 participants study lists whose items come pre-sorted according to their unique preferences,
885 they tend to remember more and show stronger temporal clustering.

886 Discussion

887 We asked participants to study and freely recall word lists. The words on each list (and
888 the total set of lists) were held constant across participants. For each word, we considered
889 (and manipulated) two semantic features (category and size) that reflected aspects of the
890 *meanings* of the words, along with two lexicographic features (word length and first letter),
891 which reflected aspects of the words' *letters*. These semantic and lexicographic features
892 are intrinsic to each word. We also considered and manipulated two additional visual
893 features (color and location) that affected the *appearance* of each studied item, but could be
894 varied independently of the words' identities. Across different experimental conditions,
895 we manipulated how the visual features varied across words (within each list), along with
896 the orders of each list's words. Although participants' task (verbally recalling as many
897 words as possible, in any order, within one minute) remained constant across all of these
898 conditions, and although the set of words they studied on each list remained constant,
899 our manipulations substantially affected participants' memories. The impact of some of
900 the manipulations also affected how participants remembered *future* lists that were sorted
901 randomly.

902 **Recap: visual feature manipulations**

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

909 When we held the within-list variability in participants' visual experiences fixed across
910 lists (in the feature rich and reduced conditions), they remembered more words on early
911 versus late lists. On feature rich lists, they also showed stronger clustering on early versus
912 late lists. However, when we *varied* participants' visual experiences across lists (in the
913 "reduced (early)" and "reduced (late)" conditions), these early versus late accuracy and
914 clustering differences disappeared. Abruptly changing how irrelevant visual features
915 change across words seems to act as a sort of "event boundary" that partially resets
916 how participants process and remember post-boundary lists. Within-list clustering also
917 increases in these manipulations, suggesting that the "within-event" words are being more
918 tightly associated with each other.

919 When we held the visual features constant on early lists, but then varied words'
920 appearances on later lists (i.e., the reduced (early) condition), this improved participants'
921 overall memory performance. However, this impact was directional: when we *removed*
922 visual features on late lists that had been present on early lists (i.e., the reduced (late)
923 condition), we saw no memory improvement.

924 **Recap: order manipulations**

925 When we (stochastically) sorted early lists along different feature dimensions, we found
926 several impacts on participants' memories. Sorting early lists semantically (by word cat-
927 egory) enhanced participants' memories for those lists, but the effects on performance of
928 sorting along other feature dimensions were inconclusive. However, each order manipu-
929 lation substantially affected how participants *organized* their memories of words from the
930 ordered lists. When we sorted lists semantically participants displayed stronger semantic
931 clustering; when we sorted lists lexicographically they displayed stronger lexicographic
932 clustering; and when we sorted lists visually they displayed stronger visual clustering.
933 Clustering along the unmanipulated feature dimensions in each of these cases was un-
934 changed.

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

939 We also examined the impact of early list order manipulations on memory for late
940 lists. At the group level, we found little evidence for lingering "carryover" effects of
941 these manipulations; participants in the order manipulation conditions showed similar
942 memory performance and clustering on late lists to participants in the corresponding
943 control (feature rich) condition. At the level of individual participants, however, we
944 found several meaningful patterns.

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

949 in a random order). We also observed some (weaker) carryover effects of temporal cluster-
950 ing. Participants who showed stronger feature clustering (along their condition's feature
951 dimension) on early lists tended to show stronger temporal clustering on late lists. And
952 participants who showed stronger temporal clustering on early lists also tended to show
953 stronger feature clustering on late lists. Essentially, these order manipulations appeared
954 to affect each participant differently. Some participants were sensitive to our manipula-
955 tions, and those participants showed stronger impacts on their memory performance for
956 the ordered lists as well as future (random) lists. Other participants appeared relatively
957 insensitive to our manipulations, and those participants showed little carryover effects on
958 late lists.

959 These results at the individual participant level suggested to us that either (a) some
960 participants were more sensitive to *any* order manipulation, or (b) some participants
961 might be more (or less) sensitive to manipulations along *particular* (e.g., preferred) feature
962 dimensions. To help distinguish between these possibilities, we designed an adaptive
963 manipulation whereby we attempted to manipulate whether participants studied words
964 in an order that matched (or mismatched) our estimate of how they would cluster or orga-
965 nize the studied words in memory (i.e., their idiosyncratic memory fingerprint). We found
966 that when we presented words in orders that were consistent with participants' memory
967 fingerprints, they remembered more words overall and showed stronger temporal clus-
968 tering. This comports well with the second possibility described above. Specifically, each
969 participant seems to bring into the experiment their own idiosyncratic preferences and
970 strategies for organizing the words in their memories. When we presented the words in
971 an order consistent with each participant's idiosyncratic strategies, their memory perfor-
972 mance improved. This might indicate that the participants were spending less cognitive
973 effort "reorganizing" the incoming words on those lists, which freed up resources to devote

974 to encoding processes instead.

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

976 In real-world experience, each moment's unique blend of contextual features (where we
977 are, who we are with, what else we are thinking of at the time, what else we experience
978 nearby in time, etc.) plays an important role in how we interpret, experience, and re-
979 member that moment, and how we relate it to our other experiences (e.g., for review see
980 Manning, 2020). What are the analogues of real-world contexts in laboratory tasks like
981 the free recall paradigm employed in our study? In general, modern formal accounts of
982 free recall (Kahana, 2020) describe context as comprising a mix of (a) features pertaining
983 to or associated with each item and (b) other items and thoughts experienced nearby in
984 time, e.g., that might still be "lingering" in the participant's thoughts at the time they
985 study the item. Item features can include semantic properties (i.e., features related to the
986 item's meaning), lexicographic properties (i.e., features related to the item's letters), sen-
987 sory properties (i.e., feature related to the item's appearance, sound, smell, etc.), emotional
988 properties (i.e., features related to how meaningful the item is, whether the item evokes
989 positive or negative feelings, etc.), utility-related properties (e.g., features that describe
990 how an item might be used or incorporated into a particular task or situation), and more.
991 Essentially any aspect of the participant's experience that can be characterized, measured,
992 or otherwise described can be considered to influence the participant's mental context at
993 the moment they experience that item. Temporally proximal features include aspects of
994 the participant's internal or external experience that are *not* specifically occurring at the
995 moment they encounter an item, but that nonetheless influence how they process the item.
996 Thoughts related to percepts, goals, expectations, other experiences, and so on that might
997 have been cued (directly or indirectly) by the participant's recent experiences prior to the

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

1000 Contextual features need not be intentionally or consciously perceived by the partic-
1001 ipant to affect memory, nor do they need to be relevant to the task instructions or the
1002 participant’s goals. Incidental factors such as font color (Jones and Pyc, 2014), background
1003 color (Isarida and Isarida, 2007), inter-stimulus images (Chiu et al., 2021; Gershman et al.,
1004 2013; Manning et al., 2016), background sounds (Beaman and Jones, 1998; Sahakyan and
1005 Smith, 2014), secondary tasks (Masicampo and Sahakyan, 2014; Polyn et al., 2009), and
1006 more can all impact how the participant remembers, and organizes in memory, lists of
1007 studied items.

1008 Consistent with this prior work, we found that participants are sensitive to task-
1009 irrelevant visual features. We also found that changing the dynamics of those task-
1010 irrelevant visual features (in the reduced (early) and reduced (late) conditions) *also* affects
1011 participants’ memories. This suggests that it is not only the contextual features themselves
1012 that affect memory, but also the *dynamics* of context– i.e., how the contextual features
1013 associated with each item change over time.

1014 **Priming effects on memory performance and organization**

1015 When our ongoing experiences are ambiguous, we can draw on our past experiences,
1016 expectations, and other real, perceived, or inferred cues to help resolve the ambiguities.
1017 We may also be overtly or covertly “primed” to influence how we are likely to resolve
1018 ambiguities. For example, before listening to a story with several equally plausible inter-
1019 pretations, providing participants with “background” information beforehand can lead
1020 them towards one interpretation versus another (Yeshurun et al., 2017). More broadly, our
1021 conscious and unconscious biases and preferences can influence not only how we interpret

1022 high-level ambiguities, but even how we process low-level sensory information (Katabi
1023 et al., 2023).

1024 In more simplified scenarios, like list learning paradigms, the stimuli and tasks partic-
1025 ipants encounter before studying a given list can influence what and how they remember.
1026 For example, when participants are directed to suppress, disregard, or ignore “distracting”
1027 stimuli early on in an experiment, participants often tend to remember those stimuli less
1028 well when they are re-used as to-be-remembered targets later on in the experiment (Tipper,
1029 1985). In general, participants’ memories can be influenced by a wide range of positive
1030 and negative factors (Balota et al., 1992; Clayton and Chattin, 1989; Donnelly, 1988; Flexser
1031 and Tulving, 1982; Gotts et al., 2012; Huang et al., 2004; Huber, 2008; Huber et al., 2001;
1032 McNamara, 1994; Neely, 1977; Rabinowitz, 1986; Tulving and Schacter, 1991; Watkins et al.,
1033 1992; Wiggs and Martin, 1998).

1034 The order manipulation conditions in our experiment show that participants can also be
1035 primed to pick up on more subtle statistical structure in their experiences, like the dynamics
1036 of how the presentation orders of stimuli vary along particular feature dimensions. These
1037 order manipulations affected not only how participants remembered the manipulated
1038 lists, but also how they remembered *future* lists with different (randomized) temporal
1039 properties.

1040 **Expectation, event boundaries, and situation models**

1041 Our findings that participants’ current and future memory behaviors are sensitive to
1042 manipulations in which features change over time, and how features change across items
1043 and lists, suggest parallels with studies on how we form expectations and predictions,
1044 segment our continuous experiences into discrete events, and make sense of different
1045 scenarios and situations. Each of these real-world cognitive phenomena entail identifying

1046 statistical regularities in our experiences, and exploiting those regularities to gain insight,
1047 form inferences, organize or interpret memories, and so on. Our past experiences enable
1048 us to predict what is likely to happen in the future, given what happened “next” in our
1049 previous experiences that were similar to now (Barron et al., 2020; Brigard, 2012; Chow
1050 et al., 2016; Eichenbaum and Fortin, 2009; Gluck et al., 2002; Goldstein et al., 2021; Griffiths
1051 and Steyvers, 2003; Jones and Pashler, 2007; Kim et al., 2014; Manning, 2020; Tamir and
1052 Thornton, 2018; Xu et al., 2023).

1053 When our expectations are violated, such as when our observations disagree with our
1054 predictions, we may perceive the “rules” or “situation” to have changed. *Event boundaries*
1055 denote abrupts change in the state of our experience, for example when we transition
1056 from one situation to another (Radvansky and Zacks, 2017; Zwaan and Radvansky, 1998).
1057 Crossing an event boundary can impair our memory for pre-boundary information and
1058 enhance our memory for post-boundary information (Manning et al., 2016; Radvansky and
1059 Copeland, 2006; Sahakyan and Kelley, 2002). Event boundaries are also tightly associated
1060 with the notion of *situation models* and *schemas*– mental frameworks for organizing our
1061 understanding about the rules of how we and others are likely to behave, how events are
1062 likely to unfold over time, how different elements are likely to interact, and so on. For
1063 example, a situation model pertaining to a particular restaurant might set our expectations
1064 about what we are likely to experience when we visit that restaurant (e.g., what the building
1065 will look like, how it will smell when we enter, how crowded the restaurant is likely to
1066 be, the sounds we are likely to hear, etc.). Similarly, we might learn a schema describing
1067 how events are likely to unfold *across* any sit-down restaurant– e.g., open the door, wait
1068 to be seated, receive a menu, decide what to order, place the order, and so on. Situation
1069 models and schemas can help us to generalize across our experiences, and to generate
1070 expectations about how new experiences are likely to unfold. When those expectations

1071 are violated, we can perceive ourselves to have crossed into a new situation.

1072 In our study, we found that abruptly changing the “rules” about how the visual
1073 appearances of words are determined, or about the orders in which words are studied,
1074 can lead participants to behave similarly to what one might expect upon crossing an event
1075 boundary. Adding in variability in font color and presentation locations for words on
1076 late lists, after those visual features had been held constant on early lists, led participants
1077 to remember more words on those later lists. (We found the same pattern when we
1078 varied words’ colors and locations on early lists but held them constant on late lists.) One
1079 potential explanation is that participants perceive an “event boundary” to have occurred
1080 when they encounter the first “late” list. According to contextual change accounts of
1081 memory across event boundaries (e.g., Sahakyan and Kelley, 2002), this could help to
1082 explain why participants in the reduced (early) and reduced (late) conditions exhibited
1083 better overall memory performance. Specifically, their memory for late list items could
1084 benefit from less interference from early list items, and the contextual features associated
1085 with late list items (after the “event boundary”) might serve as more specific recall cues
1086 for those late items (relative to if the boundary had not occurred).

1087 **Theoretical implications**

1088 Although most modern formal theories of episodic memory have been developed and
1089 tested to explain memory for list learning tasks (Kahana, 2020), a number of recent studies
1090 suggest some substantial differences between memory for lists versus naturalistic stimuli
1091 (e.g., real-world experiences, narratives, films, etc.) (Heusser et al., 2021; Lee et al., 2020;
1092 Manning, 2021; Nastase et al., 2020). One reason is that naturalistic stimuli are often much
1093 more engaging than the highly simplified list learning tasks typically employed in the
1094 psychological laboratory, perhaps leading participants to pay more attention, exert more

1095 effort, and stay more consistently motivated to perform well (Nastase et al., 2020). Another
1096 reason is that the temporal unfoldings of events and occurrences in naturalistic stimuli
1097 tend to be much more meaningful than the temporal unfoldings of items on typical lists
1098 used in laboratory memory tasks. Real-world events exhibit important associations at a
1099 broad range of timescales. For example, an early detail in a detective story may prove to
1100 be a clue to solving the mystery later on. Further, what happens in one moment typically
1101 carries some predictive information about what came before or after (Xu et al., 2023). In
1102 contrast, the lists used in laboratory memory tasks are most often ordered randomly, by
1103 design, to *remove* meaningful temporal structure in the stimulus (Kahana, 2012).

1104 On one hand, naturalistic stimuli provide a potential means of understanding how our
1105 memory systems function in the circumstances we most often encounter in our everyday
1106 lives. This implies that, to understand how memory works in the “real world,” we
1107 should study memory for stimuli that reflect the relevant statistical structure of real-
1108 world experiences. On the other hand, naturalistic stimuli can be difficult to precisely
1109 characterize or model, making it difficult to distinguish whether specific behavioral trends
1110 follow from fundamental workings of our memory systems, from some aspect of the
1111 stimulus, or from idiosyncratic interactions or interference between our memory systems
1112 and the stimulus. This challenge implies that, to understand the fundamental nature of
1113 memory in its “pure” form, we should study memory for highly simplified stimuli that
1114 can provide relatively unbiased (compared with real-world experiences) measures of the
1115 relevant patterns and tendencies.

1116 The experiment we report in this paper was designed to help bridge some of the gap
1117 between naturalistic tasks and more traditional list learning tasks. We had people study
1118 word lists similar to those used in classic memory studies, but we also systematically var-
1119 ied the lists’ “richness” (by adding or removing visual features) and temporal structure

1120 (through order manipulations that varied over time and across experimental conditions).
1121 We found that participants' memory behaviors were sensitive to these manipulations.
1122 Some of the manipulations led to changes that were common across people (e.g., more
1123 temporal clustering when words' appearances were varied; enhanced memory for lists
1124 following an "event boundary;" more feature clustering on order-manipulated lists; etc.).
1125 Other manipulations led to changes that were idiosyncratic (especially carryover effects
1126 from order manipulations; e.g., participants who remembered more words on early order-
1127 manipulated lists tended to show stronger feature clustering for their condition's feature
1128 dimension on late randomly ordered lists, etc.). We also found that participants remem-
1129 bered more words from lists that were sorted to align with their idiosyncratic clustering
1130 preferences. Taken together, our results suggest that our memories are susceptible to ex-
1131 ternal influences (i.e., to the statistical structure of ongoing experiences), but the effects of
1132 past experiences on future memory are largely idiosyncratic across people.

1133 **Potential applications**

1134 Every participant in our study encountered exactly the same words, split into exactly the
1135 same lists. But participants' memory performance, the orders in which they recalled the
1136 words, and the effects of early list manipulations on later lists, varied according to how
1137 we presented the to-be-remembered words.

1138 Our findings raise a number of exciting questions. For example, how far might these
1139 manipulations be extended? In other words, might there be more sophisticated or clever
1140 feature or order manipulations that one might implement to have stronger impacts on
1141 memory? Are there limits to how much impact (on memory performance and/or or-
1142 ganization) these sorts of manipulations can have? Are those limits universal across
1143 people, or are there individual differences (based on prior experiences, natural strate-

1144 gies, neuroanatomy, etc.) that impose person-specific limits on the potential impact of
1145 presentation-level manipulations on memory?

1146 Our findings indicate that the way word lists are presented affects how people re-
1147 member them. To the extent that word list memory reflects memory processes that are
1148 relevant to real-world experiences, one could imagine potential real-world applications of
1149 our findings. For example, we found that participants remembered more words when the
1150 presentation order agreed with their memory fingerprints. If analogous fingerprints could
1151 be estimated for classroom content, perhaps they could be utilized manually by teachers,
1152 or even by automated content presentation systems, to optimize how and what students
1153 remember.

1154 **Concluding remarks**

1155 Our work raises deep questions about the fundamental nature of human learning. What
1156 are the limits of our memory systems? How much does what we remember (and how we
1157 remember) depend on how we learn or experience the to-be-remembered content? We
1158 know that our expectations, strategies, situation models learned through prior experiences,
1159 and more, collectively shape how our experiences are remembered. But those aspects of
1160 our memory are not fixed: when we are exposed to the same experience in a new way, it
1161 can change how we remember that experience, and also how we remember, process, or
1162 perceive *future* experiences.

1163 **Author contributions**

1164 Conceptualization: JRM and ACH. Methodology: JRM and ACH. Software: JRM, KZ,
1165 PCF, CEF, and ACH. Analysis: JRM, PCF, and ACH. Data collection: KZ, EW, PCF, MRL,
1166 AMF, BJB, and CEF. Data curation and management: KZ, EW, PCF, MRL, ACH. Writing

1167 (original draft): JRM. Writing (review and editing): KZ, EW, PCF, MRL, AMF, BJB, CEF,
1168 and ACH. Supervision: JRM and ACH. Project administration: KZ, EW, and PCF. Funding
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1170 **Data and code availability**

1171 All of the data analyzed in this manuscript, along with all of the code for carrying out the
1172 analyses may be found at <https://github.com/ContextLab/FRFR-analyses>. Code for run-
1173 ning the non-adaptive experimental conditions may be found at <https://github.com/Context->
1174 Lab/efficient-learning-code. Code for running the adaptive experimental condition may be
1175 found at <https://github.com/ContextLab/adaptiveFR>. We have also released an associated
1176 Python toolbox for analyzing free recall data, which may be found at [https://cdl-quail.read-](https://cdl-quail.read-the-docs.io/en/latest/)
1177 thedocs.io/en/latest/.

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