

A Comparison of Adaptive and Fixed Schedules of Practice

Everett Mettler
University of California, Los Angeles

Christine M. Massey
University of Pennsylvania

Philip J. Kellman
University of California, Los Angeles

Understanding and optimizing spacing during learning is a central topic for research in learning and memory and has substantial implications for real-world learning. Spacing memory retrievals across time improves memory relative to massed practice—the well-known *spacing effect*. Most spacing research has utilized fixed (predetermined) spacing intervals. Some findings indicate advantages of expanding over equal spacing (e.g., Landauer & Bjork, 1978); however, evidence is mixed (e.g., Karpicke & Roediger, 2007), and the field has lacked an integrated explanation. Learning may instead depend on interactions of spacing with an underlying variable of learning strength that varies for learners and items, and it may be better optimized by adaptive adjustments of spacing to learners' ongoing performance. Two studies investigated an adaptive spacing algorithm, Adaptive Response-Time-based Sequencing or ARTS (Mettler, Massey & Kellman, 2011) that uses response-time and accuracy to generate spacing. Experiment 1 compared adaptive scheduling with fixed schedules having either expanding or equal spacing. Experiment 2 compared adaptive schedules to 2 fixed "yoked" schedules that were copied from adaptive participants, equating average spacing across conditions. In both experiments, adaptive scheduling outperformed fixed conditions at immediate and delayed tests of retention. No evidence was found for differences between expanding and equal spacing. Yoked conditions showed that learning gains were due to adaptation to individual items and learners. Adaptive spacing based on ongoing assessments of learning strength yields greater learning gains than fixed schedules, a finding that helps to understand the spacing effect theoretically and has direct applications for enhancing learning in many domains.

Keywords: adaptive learning, spacing effect, memory, learning

Supplemental materials: <http://dx.doi.org/10.1037/xge0000170.supp>

Among the most influential and consequential efforts in the science of learning in recent years have been studies of spacing in learning. Over a century of research on conditions of practice has determined that spacing, or distributing the study of learning material over time, improves long-term retention relative to massing or cramming the material in the short term (Dempster, 1989; Ebbinghaus, 1913; Glenberg, 1976; Rumelhart, 1967; Tsai, 1927). Spacing improves learning across a variety of materials and learning modes. Although item

memorization has been most frequently studied, spacing effects have been shown for other types of learning, such as learning of perceptual classifications (Kornell & Bjork, 2008; Mettler & Kellman, 2014; Wahlheim, Dunlosky & Jacoby, 2011). Effects of spacing are robust, affecting long-term retention at multiple timescales of practice (Cepeda, Pashler, Vul, Wixted & Rohrer, 2006; Cepeda, Vul, Rohrer, Wixted & Pashler, 2008), and they are phylogenetically broad, extending beyond human cognition (Zhang et al., 2011).

This article was published Online First April 28, 2016.

Everett Mettler, Department of Psychology, University of California, Los Angeles; Christine M. Massey, Institute for Research in Cognitive Science, University of Pennsylvania; Philip J. Kellman, Department of Psychology, University of California, Los Angeles

We gratefully acknowledge support from the U.S. National Science Foundation (NSF) Research on Education and Evaluation in Science and Engineering (REESE) Program (Award Number 1109228); the U.S. Department of Education, Institute of Education Sciences (IES), Cognition and Student Learning (CASL) Program (Award Numbers R305A120288 and R305H060070); the National Institute of Child Health and Human Development (NICHD; Award Number 5RC1HD063338); and the U.S. Department of Education, IES SBIR (Award Number ED-IES-10-C-0024 to Insight Learning Technology, Inc.). Any opinions, findings, and conclusions or recommendations expressed in this article are those of the authors and do not necessarily reflect the views of NSF, the U.S. Depart-

ment of Education, or other agencies. We thank Robert Bjork, Alan Castel, Jim Stigler, Ed Stabler, and members of the Human Perception Lab and CogFog Seminar at the University of California, Los Angeles, for helpful comments and insight. We also thank the editor, Melody Wiseheart, and reviewers, John Dunlosky and Phil Pavlik, for comments and suggestions that greatly improved the article. Systems that use learner speed and accuracy to sequence learning events are covered by U.S. Patent Number 7052277, assigned to Insight Learning Technology, Inc. For information, please contact Info@insightlt.com. Portions of this work were submitted as a dissertation by Everett Mettler. Everett Mettler is an employee of Insight Learning Technology and Philip J. Kellman is the founder and president of Insight Learning Technology.

Correspondence concerning this article should be addressed to Everett Mettler, Department of Psychology, University of California, Los Angeles, Franz Hall 2349, 405 Hilgard Avenue, Los Angeles, CA 90095-1563. E-mail: mettler@ucla.edu

Spacing has the potential to drive substantial improvements in learning for students in real educational settings (Carpenter, Cepeda, Rohrer, Kang, & Pashler, 2012; Mettler, Massey & Kellman, 2011), and it has been endorsed as a primary recommendation for organizing instruction in an Institute of Education Sciences-sponsored practice guide based on reviews of evidence by a national panel of experts (Pashler et al., 2007). However, the insights derived from both classic and recent work have largely failed to penetrate curriculum and instruction in either kindergarten to 12th grade or higher education. The most common formats for organizing curriculum, such as “layer cake” sequences (e.g., studying biology, physics and chemistry in successive grades), massed practice (e.g., studying a given math topic, completing a set of similar problems for homework and then moving on to a new topic the next day), and spiral curricula (studying fractions every year in elementary and middle school math, with long gaps in between) use learning schedules that are associated with poor outcomes in terms of long-term durability of learning (Rohrer & Taylor, 2006; Snider, 2004). Instruction in many education and training settings typically fails to make the critical distinction between performance during or immediately after instruction and long-term retention and recall (Bjork & Bjork, 2011). Further, it fails to recognize that even adult learners have little insight into their own learning processes, typically overestimating the likelihood that they will remember something in the future and not recognizing which study methods improve retention and retrieval in the long run (Bjork, 1999; Bjork, Dunlosky, & Kornell, 2013; Kornell & Bjork, 2007). While knowledgeable teachers can to some degree make up for the metacognitive weaknesses of their students, it is logistically difficult for educators to customize schedules of practice for individual students and topics.

The advent of learning technologies that can track and implement learning schedules brings an entirely new set of tools to the enterprise—tools that can off-load from both students and instructors the difficult task of optimally spacing practice during learning. However, to fully realize their benefit, it is necessary for them to incorporate scientifically sound principles to guide schedules of practice to support learning over meaningful time spans. This article explores which schedules of practice improve learning outcomes and investigates a novel hypothesis for why they might do so. The findings have important theoretical implications for understanding spacing in fixed and adaptive schedules and have direct potential application for the development of new learning resources and technologies across many domains of learning, including kindergarten to 12th grade and college education, medical and other professional education, and training in industry.

If spacing fosters greater learning, a natural question arises: Which spacing intervals are most useful? Further, if items are repeatedly presented, as is typical in real-world learning contexts, what characteristics of spacing across repeated presentations most improve learning? Answering these questions requires some understanding of the mechanisms that make spacing effective.

Why Is Spacing Effective?

A variety of explanations for spacing benefits in learning have been suggested; indeed, within and across various learning tasks, there may be a family of spacing phenomena and explanations for them (Mettler & Kellman, 2014; Glenberg, 1979).

Some proposed explanations for the spacing effect include encoding variability and deficient processing accounts. In encoding variability accounts, adding space between item presentations facilitates variability of the encoding context. That is, the conditions of practice are likely to be different at subsequent presentations of an item as more time elapses between presentations. Differences in context lead to an increase in the probability that memories are encoded in different ways, thus strengthening the memories that are formed (Glenberg, 1979). In deficient processing accounts, it is thought that learners do not process repeated instances of items when spacing intervals are too short. That is, when items are massed, or repeated rapidly in time, learners reduce the amount of attention given to subsequent presentations. Spacing, in contrast, encourages greater attention to repeated presentations of items, thus benefiting memory (Hintzman, 1974). A variant of this idea is that long term learning benefits from periodic retrievals from long term memory but not from recovering information that still resides in working memory (Baddeley, 1986).

More recent accounts have invoked other factors that influence the benefits of spacing and optimal spacing intervals, such as the role of representations of prior practice or differences among items in their ease of recognition (e.g., Benjamin & Tullis, 2010; Delaney, Verkoeijen & Spiegel, 2010; see Experiment 1 Discussion).

A strong candidate explanation for some of the major benefits of spaced practice in this context is that the value of a learning event differs depending on how well-learned an item is, that is, on an internal variable of learning strength. Learning strength will tend to decline over time, making successful retrieval more difficult as the time since the last presentation or retrieval increases, and, as suggested by many studies of spacing, it is likely influenced by a number of other variables. The optimal time to practice an item is when retrieval is difficult but can still succeed. This *retrieval effort hypothesis* follows from the desirable difficulty framework of Bjork and Bjork (1992), and has been supported by a number of studies (Johnston & Uhl, 1976; Pyc & Rawson, 2009; Thios & D’Agostino, 1976). Results show that the difficulty of retrieval can be induced in a number of ways—for example, by interleaving difficult tasks between retrieval attempts (Bjork & Allen, 1970), changing the amount of memory interference that retrieval attempts encounter (Storm, Bjork & Storm, 2010), or manipulating the number of retrieval attempts that an item receives before a test (Pyc & Rawson, 2009). Retrieval effort can also be induced by stretching the spacing intervals over which retrievals are attempted. The variety of variables shown in research to influence retrieval effort suggests that fluctuations of learning strength in a learning session arise from numerous and subtle influences that would be difficult to capture in an a priori model.

Fixed schedules incorporating expanding intervals of retrieval practice (Cull, Shaughnessy & Zechmeister, 1996; Landauer & Bjork, 1978) may improve learning because the schedule of retrievals is congruent with changes in the strength of learning items in memory. In expanding practice, initial spacing intervals are short because learning strength is initially low, but spacing intervals gradually grow, under the expectation that information can be retrieved at longer delays. Further, the greatest benefits to learning strength will be gained from difficult retrievals at the largest possible delays—temporally close to, but not past, the point of forgetting. If the intervals are felicitously chosen, expanding the

retrieval interval thus can ensure that retrievals remain difficult and widely spaced, improving long-term learning.

Despite intuitions that expanding retrieval practice is beneficial for learning, the evidence for benefits of expanding spacing relative to other spacing schedules, such as equal interval spaced presentation, is mixed (Karpicke & Roediger, 2007, 2010). Karpicke and Roediger (2007) reported that equal interval practice is actually superior to expanding practice when measured at a delayed test, and further, that there were no differences in learning outcomes between equal or expanding schedules when the size of their initial spacing interval was equated. Other studies have demonstrated similar mixed results. Karpicke and Bauernschmidt (2011) found no evidence for or against expanding interval practice in a study where learners were trained to an initial criterion of proficiency, similar to other research (Carpenter & DeLosh, 2005). Contrary to these results, Storm, Bjork and Storm (2010) reported that memory was better for an expanding schedule of practice, but only when items that intervened during spacing were highly related to spaced items, suggesting that expanding intervals are most beneficial when the potential for forgetting is high.

Spacing Intervals and Learning Strength

From the standpoint of the retrieval effort hypothesis (or any perspective that relates spacing to changing learning strength), the mixed results of research testing fixed schedules of spacing are not surprising. Fixed spacing intervals may be poorly suited to variations in the learning strength of items for a given learner. Some items may, across learners, be more difficult to learn, but learning strengths for various items in the course of learning seem likely to reflect individual interactions of learners and items. Although a preset schedule of expanding spacing intervals across trials will tend to correlate with increasing learning strength for a typical item, the match may be far from perfect. Even if learning strength increases monotonically, preset intervals may expand too much or not enough. In some cases, learning strength may actually be a nonmonotonic function of trials, depending on item difficulty and relations among items being learned. From the standpoint of the retrieval difficulty hypothesis, the use of predetermined intervals may be less effective than flexible spacing arrangements that match current learning strength to spacing intervals.

Adaptive Schedules of Practice

Ideal schedules of spacing for each item would be based on learning strength at particular times for each individual learner. How might we get ongoing measures of learning strength during a learning session? Adaptive learning methods have been proposed that determine recurrence of learning items based on accuracy (Atkinson, 1972; Mozer, Pashler, Cepeda, Lindsey & Vul, 2009; Pavlik & Anderson, 2008). However, spacing based on accuracy alone does not distinguish between easier and more difficult retrievals. Adaptive systems that estimate learning parameters for different items by carrying out a prior study with the learning materials and a similar group of learners (Atkinson, 1972; Pavlik & Anderson, 2008) may capture some of the variations in learning strength, but do so by relying on binary accuracy information alone. A more direct method of tracking learning strength might be possible using an ongoing indicator of learning strength—one that

might vary for different learners, items, and their interactions. Such a system could adjust spacing schedules in response to the ongoing behavior of each learner.

The ARTS System

Evidence indicates that response time (RT) is a useful indicator of retrieval difficulty, and thus of an item's current learning strength (Pyc & Rawson, 2009; Benjamin & Bjork, 1996; Karpicke & Bauernschmidt, 2011). This relationship offers a useful way of updating spacing to track underlying learning strength: Adaptive methods can use an individual's accuracy and RT performance data for learning items to dynamically schedule spacing intervals. Mettler, Massey, and Kellman (2011) showed that a system that determines spacing dynamically based on each learner's accuracy and speed in interactive learning trials (the adaptive response-time-based sequencing or ARTS system) produced highly efficient learning and compared favorably with a classic adaptive learning system that did not utilize RT information (Atkinson, 1972).

Unlike other adaptive systems that compute a model of memory strength for individual items (Atkinson, 1972) or a model of memory improvement per unit of practice time (Pavlik & Anderson, 2008), the ARTS algorithm does not *model* learning strength so much as attempt to read it directly through RT measures. ARTS uses a priority score system, in which the priority for an item to reappear on each learning trial is computed dynamically as a function of accuracy, RT, and trials since the last presentation. Priority scores for items can increase at different rates, and the item with the highest priority is always selected for presentation on the next trial. Therefore, priority scores represent competition for presentation rather than a direct model of learning strength.

Because all items compete for presentation on any trial through their priority scores, the system concurrently implements adaptive spacing for all learning items. As learning strength increases, as reflected in performance, delay intervals automatically expand in this system. Errors in accuracy or increases in RT can also cause the delay interval to contract. Also, in some previous implementations, the system enforces mastery criteria based on both accuracy and speed. Since it is expected that benefits to memory should be greatest when retrieval is difficult but also correct, performance during learning in terms of accuracy should stay high. Combined with the goal of improving speed of learners' responses, ARTS thus enforces efficient learning in terms of memory gain per unit time. (A similar goal is incorporated in the method of Pavlik & Anderson, 2008, where the model attempts to compute an optimal balance between correctness and difficulty for each item.)

Comparing Fixed Versus Adaptive Spacing

In the current studies, we compared an adaptive scheduling algorithm, ARTS, to fixed schedules of practice. We focused on several questions. First, do adaptive schedules of practice outperform fixed schedules (of either the equal spacing or expanding spacing types)? We tested this in Experiment 1 by directly comparing fixed and adaptive schedules. Second, if adaptive schedules are better, how are these benefits attained? In particular, can we uncover evidence indicating whether it is adaptation to individual learners versus adaptation relating to particular learning items that confers more benefit? We tested this

question in Experiment 2 using methods designed to distinguish between these influences.

We know of no previous work comparing adaptive schedules to fixed schedules that have been found to be advantageous or claimed to be optimal in research on spacing. The research literature on fixed schedules of spacing and the literature on adaptive learning have been largely distinct. Substantial work has explored scheduling based on adaptive techniques (Mozer et al., 2009; Pavlik & Anderson, 2008; Woźniak & Gorzelańczyk, 1994),¹ and a separate literature addresses issues related to the scheduling of a few fixed trials of practice. Atkinson (1972) compared random schedules to adaptive schedules and Pavlik (2005) has compared adaptive schedules to fixed schedules with maximum spacing as well as fixed spacing that is yoked to prior adaptive participants; however, no prior study has attempted to assess the comparative advantages of adaptive schedules and fixed schedules where fixed schedules are “preset” to spacing delays commonly thought to be optimal in the spacing literature.

Carrying out experimental research comparing fixed spacing and adaptive schemes raises some interesting collateral issues. Studies of adaptive learning and typical studies of item memory tend to have different structures, related to different goals. Perhaps the most important difference for present purposes is whether learning sessions have fixed or variable duration. In some adaptive systems, including ARTS, learning proceeds, not for a fixed number of trials or presentations, but to criteria of mastery. An important benefit of adaptive, interactive learning when applied to real-world learning situations is that each component of learning (e.g., each item in fact learning or each category in perceptual learning; Mettler & Kellman, 2014) can be tracked in terms of an individual learner’s performance, with each learner guided to objective mastery criteria (in ARTS, both accuracy and speed of response criteria). Components that have been mastered may be dropped out (retired) from the learning set, and the course of learning ends when each component has been mastered.

In contrast, studies of predetermined equal versus expanding spacing intervals have almost all used a fixed number of item presentations, often three or four. This approach provides better experimental control for condition comparisons, although it seldom results in mastery of all of the learning material in any condition (see Rawson & Dunlosky, 2011, for criticism of the reliance on fixed amounts of practice in studies of the spacing effect).

In the experiments described here, we adopted experimental protocols that resemble prior studies of spacing intervals in memory research; specifically, each condition involved four presentations of an item in all cases. This approach provided comparability to earlier spacing work and allowed direct comparison of accuracy gains in learning across conditions, without having to factor in variable numbers of trials for individual learners to reach mastery criteria. One drawback of this approach is that adaptive learning schemes may have most value when learning to criterion is used. In fact, prior work with the ARTS system (e.g., Mettler & Kellman, 2014; Mettler et al., 2011) raises the question of whether the advantages of adaptive learning are even manifest in the first several presentations of an item. In forthcoming work, we take up the comparison of fixed and adaptive spacing schedules when learning to criterion is used (Mettler, 2014).

Experiment 1

To compare adaptive and fixed spacing schedules, we used a geography learning task. Participants learned 24 country names and locations on a map of Africa. Each item was presented four times and all items were presented in a single session. The primary experimental manipulation was the method of determining spacing intervals between the four presentations of each item. There were three different types of delay schedule: The *adaptive* group of participants received items using the ARTS adaptive algorithm (see below), which dynamically spaces item presentation intervals based on real-time performance data. Another group of participants received a fixed schedule of practice where half of their learning items were scheduled according to an equal schedule of practice (5–5–5 intervening items) and the other half of their items were scheduled according to an expanding schedule of practice (1–5–9 intervening items). These particular fixed intervals were chosen from those commonly used in the literature on spacing schedules.

In the learning session, every presentation consisted of a test trial on which a participant was shown a map of Africa with national boundaries drawn in but without names (see Figure 1). One country was highlighted and the participant was asked to pick the correct name from a list of 38 country names. Participants were given accuracy feedback and, in the case of an incorrect response, they were shown the correct answer. Participants were given a pretest before the learning session and an immediate posttest immediately after the learning session. The pre and posttests were identical to training trials except that there was no feedback given after a response. Each country was tested once in pretest and once in posttest. Finally, participants returned for a delayed posttest after 1 week. The delayed posttest was identical to the immediate posttest. The order of test items was randomized for each test. If adaptive scheduling produces better learning than fixed scheduling, we expected that participants would perform better on measures of recall at both immediate and delayed posttests.

Planned Analyses

The primary dependent measures were accuracy and RTs across items. In addition to these performance measures, the actual spacings generated by adaptive scheduling were compared to those chosen for fixed schedules. This experiment also served as a baseline for determining the individual item intervals for Experiment 2 (adaptive sequencing vs. fixed yoked schedules).

Method

Participants. Participants were 72 undergraduate psychology students who received course credit for completing the experiment.

¹ Techniques do not always agree on the goals of learning. Some techniques aim to both reduce the amount of total time spent practicing items as well as simultaneously optimize the learning of individual items (Pavlik & Anderson, 2008). Other studies fix total time but prescribe differing numbers of presentations and differing durations of practice at each repetition (Lindsey, Shroyer, Pashler, & Mozer, 2014). Few adaptive schedules attempt explicitly to maximize the duration of spacing delays to optimize learning for each item, and we know of no other techniques that rely on ongoing measures of response speed during learning.

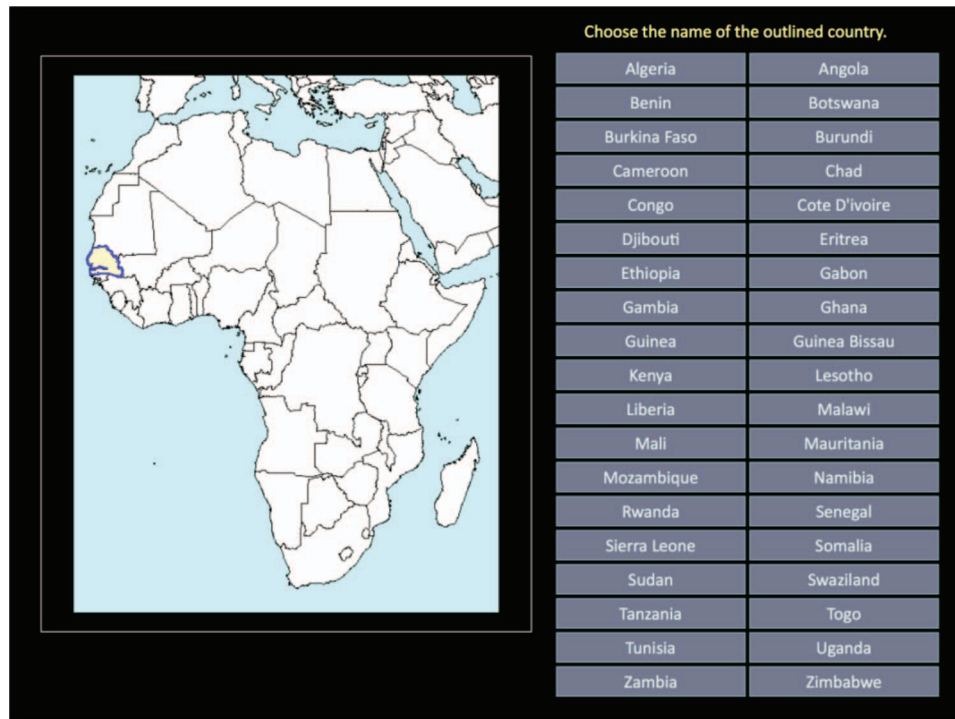


Figure 1. Example of trial format used in learning and assessment phases of the experiments. Each trial displayed a map of Africa with a target country highlighted, and a list of response choices on the right side of screen. See the online article for the color version of this figure.

We planned for 32 participants per condition but ended up with 36 per condition due to the weekly cycle of participant pool signups. Thirty-six participants was consistent with an a priori power analysis for $\alpha = .05$ and $\beta = .05$, with an expectation of large effect sizes based on prior work measuring learning effects of fixed scheduling conditions (Karpicke & Roediger, 2007; Mettler, 2014).

Materials. The learning materials consisted of 24 African countries that participants were required to identify on a map of Africa. Fourteen additional countries were used as “filler” items to space presentations appropriately, especially at the end of learning sessions (see note on filler items in the “Filler items and jitter in fixed schedules” section). All material was presented on a computer within a web-based application. Participants saw a 500-pixel \times 800-pixel map of Africa on the left side of the screen and a two-column list of African countries alphabetically organized by column then row (see Figure 1). Each list label was a software button that could be independently selected using a computer mouse.

Design. There were two between-subjects conditions, *adaptive* spacing and *fixed* spacing. There were two within-subject fixed spacing conditions, *fixed-equal* spacing and *fixed-expanding* spacing. In the fixed spacing conditions, one random half of learning items were assigned to the *fixed-equal* condition and the other half were assigned to the *fixed-expanding* spacing condition.

The ARTS algorithm calculated a priority score for each learning item, where, on any subsequent trial, priority scores were compared across items to determine which item would be presented on that trial. Details of the priority score calculation are

given in Equation 1 (and below) and parameters are given in the appendix (Table A1).²

$$P_i = a(N_i - D)[b(1 - \alpha_i) \log(RT_i/r) + \alpha_i W] \quad (1)$$

Priority P for item i was determined as a function of the number of trials since that item was last presented N_i , an enforced delay D (a constant, which was set to 1 in the experiments here), and the accuracy (α_i) and RT_i on the previous presentation of that item. Accuracy (α_i) was a binary variable determined by the correctness of the user’s response: 0 if the question was answered correctly, 1 otherwise. This binary accuracy variable acted as a switch activating either the error part of the equation (for an incorrect answer) or the RT part of the equation (for a correct answer). The rationale was that RT s for incorrect answers were not considered informative for spacing. An incorrectly answered item was given a large priority increment (W) that typically ensured re-presentation after a delay of one trial. Correctly answered items were assigned a priority score that was a log function of RT (where the logarithm was used to weight small differences among RT s more heavily for shorter RT s than for longer ones). Item presentation on a given trial was always decided by choosing the item with the largest priority score (P) in the set. In addition, the introduction of new items was controlled by the assignment of default priority scores to all items, allowing for the introduction of new items once previ-

² Parameters were borrowed from prior work and loosely tuned using pilot data to better match the format of four total presentations, which differed from prior work where learning proceeded to mastery criteria.

ously introduced items became better learned and had lower priority scores than the default. Parameters a , b , and r were weighting constants: a controlled the rapidity with which priority accumulated as a function of elapsed trials; b and r modulated the relation between RTs and spacing intervals.

Although priority score equations using RT and accuracy can take many forms, the parameters here were fixed and identical in both Experiment 1 and 2, and were also similar to those used in previously published research on item learning (Mettler, Massey & Kellman, 2011) and perceptual category learning (Mettler & Kellman, 2014). The priority scores generated by Equation 1 incorporate ongoing estimates of learning strength as indicated by accuracy and RTs. The goal of priority scores is to indicate the degree to which items need practice and to place all items on a common footing in competing to be selected for presentation on upcoming trials. This purpose incorporates learning strength inputs (accuracy and speed) but also takes account of principles of learning that interact with learning strength to suggest when the item should be presented again. For example, an incorrectly answered item would be assessed as having low learning strength, and the priority score equation would give it a hefty priority increment (W) for that reason, but the priority score equation above also includes an enforced delay for a missed item's reappearance, to make sure the answer given in feedback does not still reside in working memory.

Taken together, the elements of the priority score equation given here implement a number of principles of learning that have been derived in memory research, including rapid recurrence of missed items; enforcing at least some delay in re-presenting an item; and stretching the retention or recurrence interval as learning strength, indicated by accuracy and RT, increases.

Procedure. In all sessions of the experiment, learning items were presented singly, in the form of interactive test trials. Participants were shown a map of Africa featuring an outlined country and were asked to select, from a list of labels containing country names, the name that matched the highlighted country. Participants used the computer mouse to select from the list of names.

Participants attended two sessions, separated by 1 week. In the first session, participants initially took a pretest on all items, then completed a training phase, followed by an immediate posttest. The entire session took no more than 1 hr for each participant. Pretests contained all 38 target and filler items, presented in random order. During the pretest, participants were not given feedback. The pretest was followed by a learning phase that consisted of the same type of trial as the pretest, except that participants were given feedback after each response showing the correctness of their response as well as a label indicating the correct answer. The learning phase took up the majority of the first session of the experiment. After every 10 trials in the learning phase, participants received block feedback indicating their average response accuracy and average response speed for the previous block of 10 trials and every previous block up to 10 prior blocks. After the learning phase, an immediate posttest was administered, identical to that given in the pretest. After the posttest participants were instructed to return in 1 week and were asked not to study or reflect on the information learned. A delayed posttest, identical to the immediate posttest, was administered after 1 week. No feedback was given on either posttest.

Spacing conditions. Participants were randomly assigned to fixed or adaptive scheduling conditions, with 36 participants in

each condition. In the *adaptive* condition, all learning trials were adaptively sequenced according to the response-time-based ARTS algorithm. In the fixed condition, one random half of each participant's items were scheduled according to an equal spacing scheme, and the other random half were scheduled according to an expanding spacing scheme. Thus, in the fixed condition, every participant received two within-subject conditions that manipulated fixed scheduling in either an expanding or equal spacing scheme. This interleaving of conditions was done primarily to avoid the problem of excessive filler items in the expanding spacing condition.

In the fixed spacing group, spacing intervals between presentations were predetermined and constant. Items in the *fixed-equal* condition received spacing of five trials between items. Items in the *fixed-expanding* condition received first one, then five, then nine trials between presentations of each item. For every participant in the fixed condition, the order of presentation was preset so that every participant received the same number and order of fixed-equal or expanding trials. Items in the fixed condition did not strictly alternate between equal and expanding schedules, but every attempt was made to balance the number of equal and fixed schedules across position in the entire learning phase, so as not to confound serial position with schedule type. Although the order of presentation of items in the fixed condition was fixed, the assignment of individual items to either of the two schedule types was randomized for each participant. In addition, the order of introduction of individual items was shuffled across possible positions in the preset schedule for each participant before the learning phase began.

For every participant in the *adaptive* condition, the total schedule order was dynamically decided during the learning session, and the order of introduction of new items was chosen randomly from the remaining items in a learning set for each participant. In all conditions in Experiment 1, each learning item was presented a total of four times.

Filler items and jitter in fixed schedules. There are two problems that arise when applying the same fixed schedule of practice to every item in a learning session. First, the structure of fixed spacing intervals does not allow continuous presentation of items without conflicts in the intended interval schedule for each item. Imagine four items (labeled A through D), each presented first with a one-trial interval, then a three-trial interval. The layout of this presentation sequence would appear as follows: A, B, A, B, C, D, [A or C?]. The seventh presentation indicates a conflict between the first and third item. These conflicts appear most frequently with fixed-expanding schedules, and less frequently with fixed-equal schedules or with adaptive schedules. One solution to this type of conflict is to allow for a degree of "jitter" in any given fixed schedule. We added jitter to fixed schedules using a simple rule: Each set of three spacing intervals between the four presentations of an item in the *fixed* condition was allowed to deviate from its preset interval (e.g., 1–5–9) by one position, smaller or larger, at any interval except the first. For example, 1–6–9, 1–5–9, 1–5–10 would all be valid "jittered" versions of the 1–5–9 expanding interval. In addition to jitter, conflicts were reduced naturally as a result of mixing expanding and equal fixed schedules.

A second problem with interleaving fixed schedules is that there are inevitably gaps, or trials where no item is scheduled to be presented. Filler items can be used to support the interleaving of

items with fixed schedules while preventing gaps, and also to maintain appropriate spacing intervals at the end of a learning session, when no target learning items remain in the set. Filler items in the current study consisted of presentations of 14 additional countries, randomly selected whenever filler items were needed. Filler items were necessary in the *fixed* conditions and the *adaptive* presentation conditions; in both cases, the final few presentations of items occur at larger and larger spacing intervals, requiring filler items when no new target items are available.

By combining expanding and equal schedule presentations into the same session, and by applying jitter as well as adding filler items, we were able to design a single fixed session that used limited filler items. Thus, filler items were utilized primarily to fill expanding schedules at the end of training and their use was equated across both *adaptive* and *fixed* conditions. Filler items were removed for the purposes of statistical analyses of assessment and training and are not included in any graph.

Sequencing parameters. The default adaptive sequencing parameters are described in the Appendix, Table A1. In this study, the default parameters were used for the adaptive algorithm, with some modifications. It was found in pilot testing that our default parameters were less effective when applied to a learning session limited by a total number of presentations per item, rather than a learning session where learners continue until meeting a learning criterion. The following parameters were changed to better support the current type of study: “RT weight,” $r = 3.0$; “enforced delay,” $D = 1$.

Results

The primary results of Experiment 1 are shown in Figure 2, which shows mean accuracy across phase. The *adaptive* condition

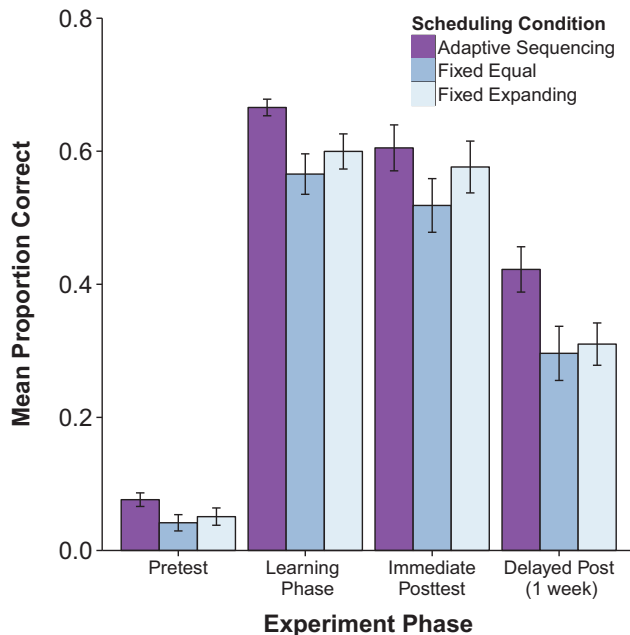


Figure 2. Mean proportion correct by experiment phase across three scheduling conditions in Experiment 1. Error bars show ± 1 SEM. See the online article for the color version of this figure.

showed higher accuracy than both *fixed* spacing conditions in the learning phase and also at delayed posttest, where *fixed-equal* and *fixed-expanding* conditions showed similar performance. In the immediate posttest, the *adaptive* condition produced higher performance than *fixed-equal*, with *fixed-expanding* scores intermediate between the other two conditions.

These observations were confirmed by the analyses. At pretest, *adaptive* accuracies were highest ($M = 0.076$, $SD = 0.27$), followed by *fixed-expanding* ($M = 0.051$, $SD = 0.22$) and *fixed-equal* ($M = 0.042$, $SD = 0.20$). Comparisons between conditions showed a significant difference for *adaptive* versus *fixed-equal*, $t(70) = 2.19$, $p = .032$, but not for *adaptive* versus *fixed-expanding*, $t(70) = 1.54$, $p = .13$, or *fixed-equal* versus *fixed-expanding*, $t(35) = 0.73$, $p = .47$. These differences indicate some pretest differences in performance across groups, despite random assignment of participants to conditions. Overall mean pretest scores ($M = 0.056$, $SD = 0.072$) were significantly different from chance responding (one sample t test): $t(107) = 4.13$, $p < .01$, suggesting that some participants possessed some prior knowledge of some countries. (Chance responding would have been one correct item out of 38, or .026.) Because it is not clear whether pretest scores reflected random variation or modest systematic differences between conditions, we considered in the analyses below both posttest accuracies as well as change scores between pretest and posttests.

Accuracy. Performance results were not analyzed using a standard one-way analysis of variance (ANOVA) due to the special combination of between- and within-subjects factors. (Only the *adaptive* vs. *fixed* comparisons were between subjects.) Three ANOVAs were used, one for each comparison of pairs of conditions, with test-phase as a within subjects factor.

A 2×2 mixed-factor ANOVA with factors of *adaptive* versus *fixed-equal* conditions and posttest phase (immediate vs. delayed) found a significant main effect of condition, $F(1, 70) = 4.63$, $p = .035$, $\eta_p^2 = .062$, a main effect of test phase, $F(1, 70) = 110.56$, $p < .001$, $\eta_p^2 = .612$, and no Condition \times Test Phase interaction, $F(1, 70) = 1.06$, $p = .31$, $\eta_p^2 = .015$. These results indicate significantly higher accuracies in the posttests for the *adaptive* condition versus the *fixed-equal* condition. For *adaptive* versus *fixed-expanding* conditions, a 2×2 mixed-factor ANOVA on condition and test phase found no significant main effect of condition, $F(1, 70) = 2.37$, $p = .13$, $\eta_p^2 = .033$, a significant main effect of test phase, $F(1, 70) = 147.0$, $p < .001$, $\eta_p^2 = .677$, and a significant Condition \times Test Phase interaction, $F(1, 70) = 5.1$, $p = .027$, $\eta_p^2 = .068$. For the two *fixed* conditions, a 2×2 condition by posttest phase repeated measures ANOVA found a marginal main effect of condition, $F(1, 70) = 3.13$, $p = .081$, $\eta_p^2 = .043$, a main effect of test phase, $F(1, 35) = 126$, $p < .001$, $\eta_p^2 = .783$, and no Condition \times Test Phase interaction, $F(1, 70) = 1.18$, $p = .28$, $\eta_p^2 = .017$. A Bartlett's test confirmed homogeneity of variance for accuracies at both posttests (immediate: $p = .64$, delayed: $p = .31$).

At immediate posttest, average accuracies were highest for the *adaptive* condition ($M = 0.61$, $SD = 0.21$), lower for the *fixed-expanding* condition ($M = 0.58$, $SD = 0.23$), and lowest for the *fixed-equal* condition ($M = 0.52$, $SD = 0.24$). Individual comparisons showed that accuracies did not differ significantly at immediate posttest between the *adaptive* and *fixed* conditions, *adaptive* versus *fixed-equal*: $t(70) = 1.63$, $p = .11$; *adaptive* versus *fixed-*

expanding: $t(70) = 0.55, p = .58$. A paired t test showed that the two within-subject *fixed* conditions differed significantly, $t(35) = 2.15, p = .039$, Cohen's $d = 0.24$.

At delayed posttest, accuracies were highest in the *adaptive* condition ($M = 0.42, SD = 0.20$), and lower for the two *fixed* conditions: *fixed-expanding* ($M = 0.31, SD = 0.19$) and *fixed-equal* ($M = 0.30, SD = 0.24$). Individual comparisons showed average accuracies for the *adaptive* condition were significantly greater than both of the *fixed* spacing conditions, *adaptive* versus *fixed-expanding*: $t(70) = 2.41, p = .019$, Cohen's $d = 0.56$; *adaptive* versus *fixed-equal*: $t(70) = 2.38, p = .02$, Cohen's $d = 0.57$. A paired t test showed that the *fixed-expanding* and *fixed-equal* spacing means were not significantly different from each other, $t(35) = 0.45, p = .65$.

Change and gain scores. Because there were detectable differences between conditions at pretest, we examined posttest results in terms of scores that looked at posttest accuracy in relation to pretest scores. We computed two types of change score for each participant, which we labeled *change* scores and *gain* scores. Change scores were computed by subtracting average pretest accuracies from average posttest accuracies.

Immediate posttest change scores were computed by subtracting a participant's average pretest accuracy from their average posttest accuracy, and delayed posttest change scores were computed by subtracting average pretest accuracy from average delayed posttest accuracy. Posttest and delayed posttest change scores are shown in Figure 3.

The ANOVAs conducted on change scores were parallel to those carried out for accuracy scores above. For *adaptive* versus *fixed-expanding*, a 2×2 mixed-factor ANOVA on condition and

test phase found no main effect of condition, $F(1, 70) = 1.21, p = .28, \eta_p^2 = .017$, a main effect of test phase, $F(1, 70) = 147.13, p < .001, \eta_p^2 = .678$, and a significant Condition \times Test Phase interaction, $F(1, 70) = 5.03, p = .028, \eta_p^2 = .067$. For *adaptive* versus *fixed-equal* spacing, there was no significant main effect of condition, $F(1, 70) = 2.37, p = .13, \eta_p^2 = .032$, a main effect of test phase, $F(1, 70) = 110.68, p < .001, \eta_p^2 = .613$, and no significant Condition \times Test Phase interaction, $F(1, 70) = 1.03, p = .31, \eta_p^2 = .015$. For *fixed-expanding* versus *fixed-equal* spacing, there was no main effect of condition, $F(1, 70) = 1.29, p = .26, \eta_p^2 = .018$, a main effect of test phase, $F(1, 35) = 126, p < .001, \eta_p^2 = .783$, and no significant Condition \times Test Phase interaction, $F(1, 70) = 0.88, p = .35, \eta_p^2 = .012$.

Individual comparisons at immediate test showed that change scores were similar for all schedules and did not differ significantly, *adaptive* versus *fixed-equal*: $t(70) = 1.03, p = .30$, Cohen's $d = 0.02$; *adaptive* versus *fixed-expanding*: $t(70) = 0.076, p = .94$, Cohen's $d = 0.25$; paired t test between *fixed-equal* and *fixed-expanding* conditions: $t(35) = 1.58, p = .12$, Cohen's $d = 0.22$. At delayed-test, change scores appeared to be higher in the *adaptive* condition ($M = 0.35, SD = 0.18$) than in either the *fixed-equal* condition ($M = 0.25, SD = 0.24$) or the *fixed-expanding* condition ($M = 0.26, SD = 0.17$). There was a significant difference between the *adaptive* and *fixed-expanding* condition, $t(70) = 2.11, p = .04$, Cohen's $d = 0.50$ and a marginally significant difference between the *adaptive* and *fixed-equal* condition, $t(70) = 1.81, p = .07$, Cohen's $d = 0.43$. A paired t test between the two *fixed* conditions showed no significant difference, *fixed-equal* versus *fixed-expanding*: $t(35) = 0.13, p = .90$, Cohen's $d = 0.02$.

In addition to change scores, we computed *gain* scores by subtracting pretest scores from posttest scores, but did not include items that were accurate at pretest but inaccurate at posttest. Gain scores were computed to address the possibility that differences in pretest scores were primarily due to chance. Gain scores showed similar results to change scores, with the following differences: an ANOVA found a marginally significant main effect of condition between *adaptive* and *fixed-equal* conditions ($p = .08$) and t tests at delayed posttest showed significant differences between *adaptive* and both *fixed* conditions (*adaptive* vs. *fixed-equal*, $p = .03$; *adaptive* vs. *fixed-expanding*, $p = .02$).

Forgetting score. In addition to change scores, we computed a forgetting score, subtracting delayed posttest accuracy from immediate posttest accuracy, where we included only those items that were answered correctly at the immediate posttest.³ Forgetting scores are shown in Figure 4. Mean forgetting scores were lowest for the *adaptive* condition ($M = 0.44, SD = 0.19$), next lowest for the *fixed-expanding* condition ($M = 0.57, SD = 0.26$) and highest for the *fixed-equal* condition ($M = 0.60, SD = 0.30$). Paired comparisons showed that the difference between *adaptive* and both *fixed* conditions was significant, *adaptive* versus *fixed-equal*: $t(70) = 2.6, p = .01$, Cohen's $d = 0.63$; *adaptive* versus *fixed-expanding*: $t(70) = 2.33, p = .02$, Cohen's $d = 0.55$, but the difference between the two *fixed* conditions was not significant, $t(35) = 0.57, p = .57$, Cohen's $d = 0.11$.

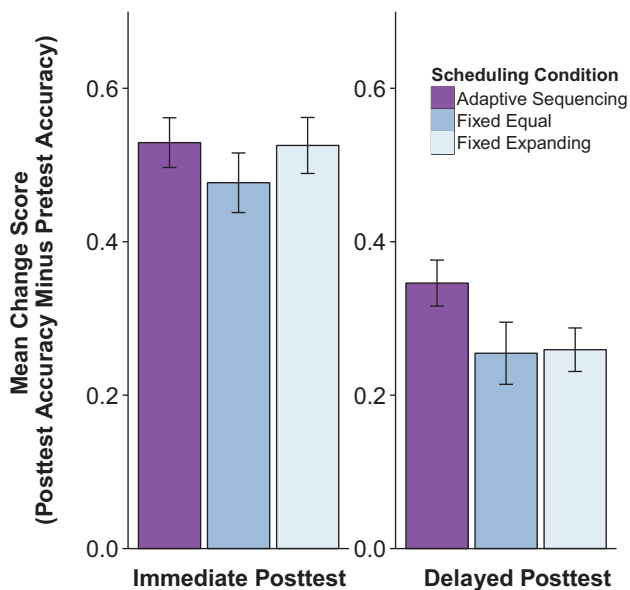


Figure 3. Mean change in accuracy from pretest to posttests across three scheduling conditions in Experiment 1. Left panel shows difference between proportion correct in the immediate posttest and pretest. Right panel shows difference between proportion correct in the delayed posttest and pretest. Error bars show ± 1 SEM. See the online article for the color version of this figure.

³ We thank the editor, Melody Wiseheart, for suggesting the forgetting score analysis.

Response times. Average RTs are shown in Figure 5 for each condition and for three experimental phases: the learning phase, immediate posttest and delayed posttest. RT data only include RTs from trials that were answered correctly. Pretests were ignored owing to the few items that were answered correctly in that phase. Of most interest were RTs at training and at immediate and delayed posttests.

ANOVAs were not carried out on RTs, due to missing data for four participants who answered no items correctly at either immediate or delayed posttest.⁴ During training, *adaptive* RTs were lowest ($M = 4.04$ s, $SD = 0.99$) followed by *fixed-equal* ($M = 4.59$, $SD = 1.34$), then *fixed-expanding* ($M = 4.61$, $SD = 1.4$). Individual comparisons showed that the difference between the *adaptive* and the two *fixed* conditions was marginally significant, *adaptive* versus *fixed-expanding*: $t(70) = 1.97$, $p = .052$, Cohen's $d = 0.47$; *adaptive* versus *fixed-equal*: $t(70) = 1.97$, $p = .053$, Cohen's $d = 0.47$, but a paired t test between the two *fixed* conditions showed no significant difference, $t(35) = 0.11$, $p = .9$, Cohen's $d = 0.012$. At immediate posttest, t tests between conditions showed no significant difference between *adaptive* and the two *fixed* conditions, *adaptive* versus *fixed-expanding*: $t(69) = 0.07$, $p = .94$, Cohen's $d = 0.02$; *adaptive* versus *fixed-equal*: $t(70) = 1.46$, $p = .15$, Cohen's $d = 0.35$, and a paired t test between the two *fixed* conditions showed no significant difference, $t(34) = 1.39$, $p = .17$, Cohen's $d = 0.43$. At the delayed posttest there was a significant difference between the *adaptive* and *fixed-expanding* conditions, $t(69) = 2.3$, $p = .02$, Cohen's $d = 0.64$, but other RTs were not significantly different from one another, *adaptive* versus *fixed-equal*: $t(66) = 1.31$, $p = .19$, Cohen's $d = 0.36$; *fixed-equal* versus *fixed-expanding*: $t(30) = 1.48$, $p = .15$, Co-

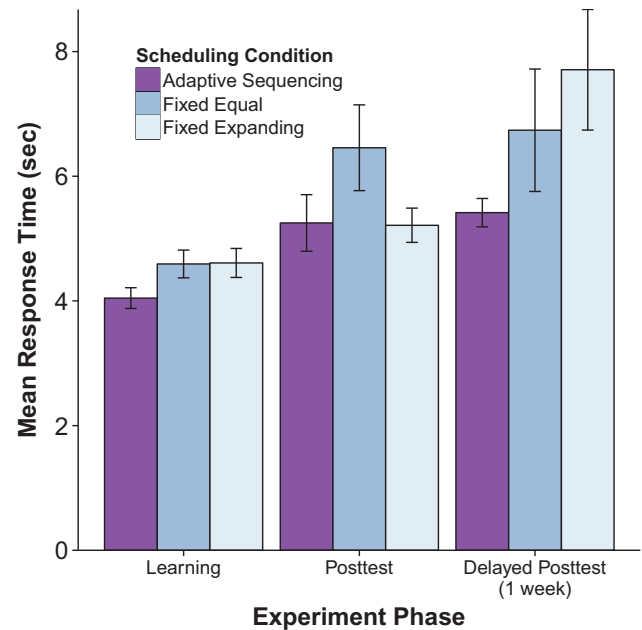


Figure 5. Mean response times (in seconds) at each test phase across three scheduling conditions in Experiment 1. Response times are from correctly answered trials only. Error bars show ± 1 SEM. See the online article for the color version of this figure.

hen's $d = 0.166$. Comparing RTs across posttest phases, only the difference between the *fixed-expanding* condition at posttest versus delayed posttest was significant, $t(33) = 2.8$, $p = .008$; all other $ps > .70$.

We also examined the RTs at each presentation in learning across the three schedules. RTs during the learning phase are shown in Figure 6 by scheduling condition and presentation number.

Examination of RTs revealed that conditions did not differ in RTs at the third or fourth presentation. The two *fixed* conditions differed at the first presentation, *fixed-equal* versus *fixed-expanding*: $t(13) = 3$, $p = .01$; all other t test $ps > .05$. Other conditions showed differences at the second presentation. There were significant differences between the *adaptive* and *fixed* conditions, *adaptive* versus *fixed-expanding*: $t(70) = 2.59$, $p = .01$; *adaptive* versus *fixed-equal*: $t(69) = 3.64$, $p < .001$, but not between the two *fixed* conditions, $t(34) = 1.06$, $p = .29$, paired t test.

Analyses of average spacing intervals. We define the *spacing interval* (or shorthand, *interval*) as the number of trials intervening between two presentations of the same learning item. *Adaptive* and *fixed* conditions differed in the size of spacing intervals for individual items during learning sessions. The mean spacing interval per learner was calculated by averaging the mean presentation interval for each item and averaging over all items. In the *fixed-equal* and *fixed-expanding* conditions, the intervals chosen in the experiment (1–5–9 and 5–5–5) ensured that mean intervals were always five trials in length. Average adaptive sched-

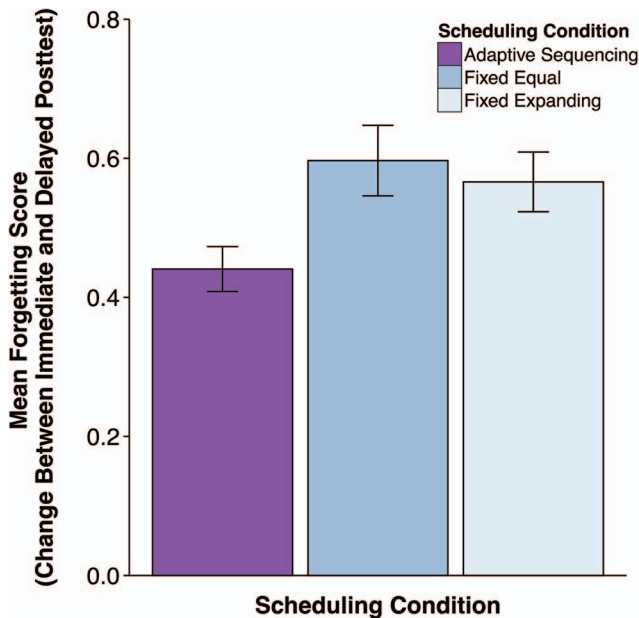


Figure 4. Mean forgetting score—reduction in the proportion of items answered correctly from immediate to delayed posttest—for the three scheduling conditions in Experiment 1. Forgetting scores are from items answered correctly on the immediate posttest only. Error bars show ± 1 SEM. See the online article for the color version of this figure.

⁴ We considered these data missing not at random (see Howell (2007)); however, we conducted t tests in an effort to further explore the data.

ule intervals were close in length but with some variance ($M = 6.7$, $SD = 2.033$).⁵ We also looked at the size of intervals conditional on whether the presentation before the interval was responded to correctly or not. The mean interval sizes by scheduling condition and conditional on response accuracy are shown in Figure 7. Although the mean adaptive interval size was similar to the mean interval size for fixed schedules, adaptive spacing intervals after incorrect responses were short ($M = 1.01$, $SD = 0.09$) owing to the enforced delay mechanism, and they were longer following correct responses ($M = 10.88$, $SD = 6.50$).

Finally, we examined the average spacing intervals at each presentation number for the *adaptive* condition. Because each item was presented four times, there were three spacing intervals. The mean sizes of the three intervals in the *adaptive* condition are shown in Figure 8. The mean initial interval was the smallest ($M = 1.62$, $SD = 0.71$), the second interval largest ($M = 10.95$, $SD = 4.46$), and the third interval smaller than the second interval ($M = 7.52$, $SD = 2.20$).

While it appears that the pattern of retrievals was not expanding, but expanding-then-contracting, in fact, a line of best fit to these points still yields a positive slope. There were also four adaptive participants who showed strictly expanding profiles, positively increasing interval sizes at each presentation.

Discussion

As demonstrated by a variety of measures, an adaptive sequencing algorithm outperformed predetermined schedules of practice. These patterns were clear in posttest accuracy as well as two derived measures of accuracy that discounted prior knowledge from measures of learning. *Change scores* were computed by

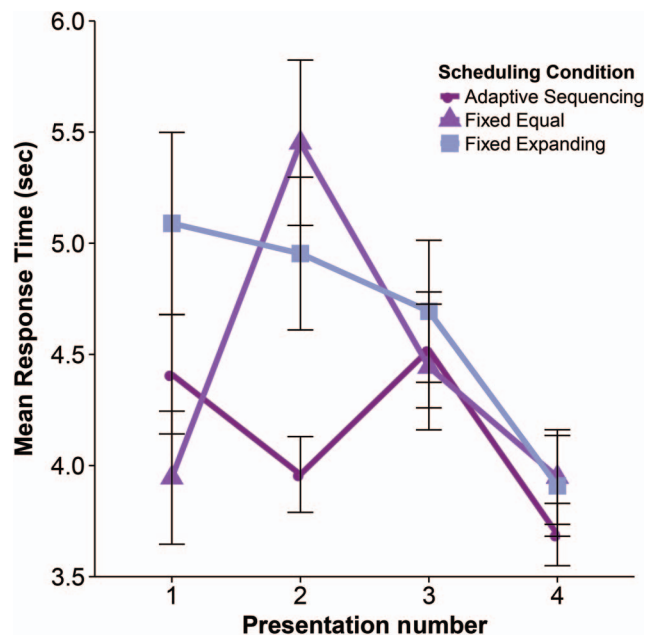


Figure 6. Mean response times (in seconds) at each presentation (one to four) during learning, across the three scheduling conditions in Experiment 1. Response times are from correctly answered trials only. Error bars show ± 1 SEM. See the online article for the color version of this figure.

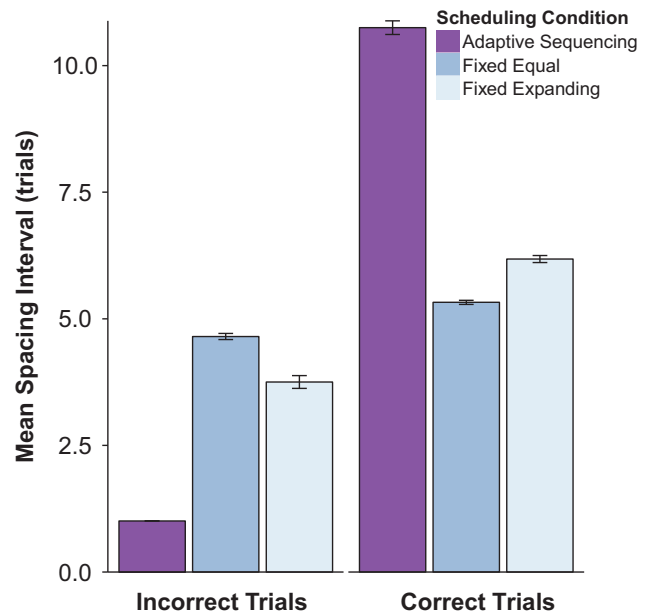


Figure 7. Mean spacing interval (in trials) across three scheduling conditions in Experiment 1 conditional on whether the trial preceding the interval was answered correctly or not. Error bars show ± 1 SEM. See the online article for the color version of this figure.

subtracting pretest accuracy from posttest accuracy for each participant, and *gain scores* were computed by subtracting from posttest accuracy only those items that were known at both pretest and posttest. Both measures showed significant differences in learning across scheduling conditions. Change scores showed that learners experienced significantly greater learning in the *adaptive* condition than in the *fixed-equal* condition. Gain scores showed significantly stronger gains in the *adaptive* condition than in either of the *fixed* scheduling conditions. In addition, these improvements were present with medium to large effect sizes, and gains were retained across a considerable delay (1 week), suggesting that adaptive scheduling techniques produce greater and more durable learning. In addition, when measuring the amount of forgetting between immediate and delayed posttests for each condition, adaptive scheduling had significantly less forgetting than both fixed conditions.

In addition to learning gains measured by accuracy, there was a trend toward greater fluency (faster responding) for participants who learned using an adaptive scheduling algorithm than for participants who learned using fixed-expanding schedules of practice. Our expectations for RT differences between conditions were consistent with these trends. If the adaptive features of ARTS are effective in building learning strength as learning progresses, it was hoped that responses would become faster, and thus spacing delays longer, over time. It appears that adaptive scheduling may produce better fluency, an important learning goal that relates not only to the durability of learning but the ability to use learned

⁵ Across items, in the *fixed* conditions there was also minor variation in spacing interval size due to the use of "jitter" as described in the Method section.

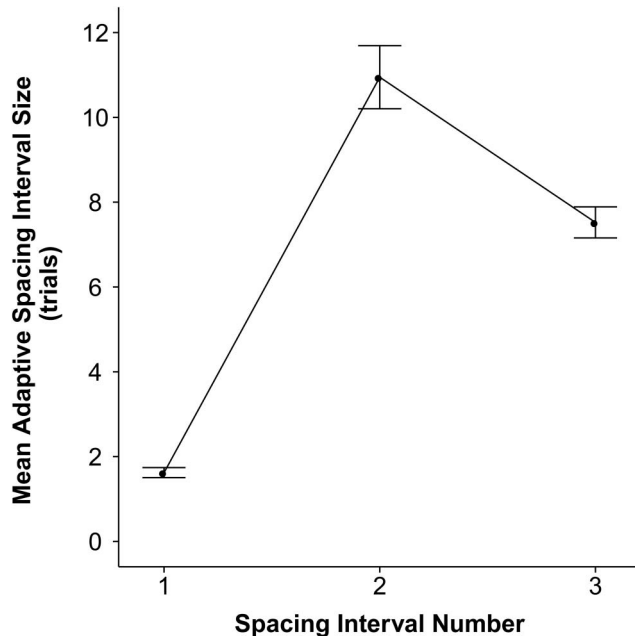


Figure 8. Mean spacing interval size (in trials) across three spacing intervals in the adaptive scheduling condition in Experiment 1. Error bars show ± 1 SEM.

information or skills in complex tasks (Kellman & Garrigan, 2009; Pavlik & Anderson, 2008).

These results are consistent with mechanisms that have been proposed to explain the benefits of spacing in learning. Retrieval difficulty, a major driver of spacing effects, may depend on underlying learning strength, an internal variable that is likely to vary from trial to trial, between individuals, and among learning items. Study-phase retrieval accounts of the spacing effect posit that retrieval difficulty depends on access to representations of prior practice, the memorability of initial presentations, or the ease of recognition of items (see Benjamin & Tullis, 2010; Delaney, Verkoeijen, & Spiegel, 2010; Johnston & Uhl, 1976). Given that the determinants of retrieval difficulty may be numerous, predetermined intervals of spacing, even when based on elaborate cognitive models, are likely unable to consistently match the requirements of optimal practice. Fluctuations in learning strength, in the ability to access previous memory traces, or in the difficulty of item recognition or recall may preclude consistent, predictable levels of retrieval difficulty.

The ARTS system used here adds RT to accuracy as an ongoing measure of learning strength for each learning item, specific to each learner, at each point in learning at which the next spacing interval needs to be determined. Though RTs may have other determinants—such as lapses in attention—RTs are at least a reasonable proxy for learning strength, a connection supported by other research (Pyc & Rawson, 2009). Relative differences in RTs can reflect changes that occur in learning strength of individual items as learning progresses and can be used to optimize spacing for each item. The advantages of adaptive spacing in the current study further confirms that RTs can serve as useful measures of learning strength (Benjamin & Bjork, 1996; Karpicke & Bauernschmidt, 2011; Pyc & Rawson, 2009) and potentially of fluctuations in the characteristics of item retrieval dur-

ing the study-phase, such as item recognition, or changes in context (Mozer et al., 2009; Raaijmakers, 2003). It is also possible that ARTS is sensitive to the combined effect of learning strength and study-phase retrieval characteristics. More of these distinctions could be explored in future research.

These results are among the first to show that adaptive learning systems, specifically the ARTS system, are capable of producing learning gains under circumstances similar to that of standard memory studies. Learning with a limited, preset number of presentations (three or four in many memory studies) is not comparable to most real-world learning situations. It provides learners with few chances to retain knowledge, and usually results in learning gains that are far from what would be sufficient for real learners to achieve mastery in a real learning domain. We chose to study adaptive spacing under these conditions for reasons that have motivated their use by many researchers: experimental control and comparability to other studies. Doing so allows the present data to help bridge two research literatures—studies of spacing schedules on one hand and adaptive learning on the other. At the same time, it is possible that an adaptive system designed to individualize learning and bring each learner to objective mastery criteria for each item (e.g., Mettler & Kellman, 2014; Mettler, Massey, & Kellman, 2011) would not show its full effects under these conditions. Despite the limitation of four total presentations per item, differences between adaptive and fixed conditions were evident in a learning session. In addition to demonstrable learning gains, differences persisted across a delay, speaking to the effectiveness of adaptive strategies to promote durable learning even without learning to criterion.

Connections With Prior Research

Initial spacing intervals. Unlike some prior research, we found evidence that initial intervals in a spacing schedule are not powerful enough to dictate long-term learning outcomes for any particular schedule. In our experiment, items in the *adaptive* condition received an “enforced delay” of one trial whenever items were answered incorrectly (cf. Pavlik & Anderson, 2008, who used an enforced delay of two trials). Thus, the vast majority of initial intervals in the *adaptive* condition possessed a one-trial spacing interval (due to most items being responded to incorrectly on the initial trial). This one-trial delay was equivalent to the one-trial delay in the *fixed-expanding* condition (where the delay applied to all items regardless of response). Nevertheless, performance at a delayed test was still greater in the adaptive condition than in the *fixed-expanding* condition. This differential degree of learning despite a rough equivalence of initial intervals suggests that short initial intervals do not convey as much power to learning as other features of spacing—specifically, the pattern of spacing intervals after the initial interval. While this result is contrary to the claims of some research (Karpicke & Roediger, 2007), we do not think it necessarily diminishes those prior researchers’ conclusions as applied to *fixed* schedules of practice. However, it is not clear whether those conclusions also apply to *adaptive* schedules of practice. Initial intervals of practice remain an important and potentially potent locus of scheduling consideration in many different scheduling schemes, including *adaptive* ones.

Expanding versus equal spacing. The fixed spacing intervals tested here did not show consistent learning advantages for expanding spaced practice over equal spaced practice. This finding is

similar to some results in the spaced practice literature and different from others. However, the advantages of adaptive spacing shown here are consistent with the hypothesis that there is no simple, general answer to the question of whether fixed or expanding spacing is superior. Optimal spacing intervals may vary with learning items, overall difficulty of learning material, and learners. They may fluctuate differently for different learners for each specific item during the course of learning. Optimal spacing would seem to require adaptive systems that can assess learning strength in a specific and ongoing manner.

In this experiment, fixed schedules appeared to be somewhat equally suboptimal in their ability to respond to fluctuations in learning strength. Despite the similarity in learning outcomes across fixed schedules, we found that the patterns of spacing intervals generated in adaptive schedules tended to increase in size during learning (although spacing sometimes flattened out or contracted across later presentations). As we found a general trend toward expanding patterns in the adaptive condition, and as *fixed-expanding* schedules resulted in greater raw accuracy at immediate posttest, our results do not contradict the idea that expanding retrieval practice is often an effective arrangement for learning. We investigate this issue further in Experiment 2.

Limitations of this experiment. There are limitations in interpreting the current results. Spacing intervals were slightly longer on average in the adaptive case; perhaps greater spacing alone led to greater learning benefits in the adaptive condition. While possible, this theoretical concern points out the practical limitations of fixed spacing intervals and the advantages of adaptive schedules. Without prior knowledge of the ideal spacing, it is impossible to choose optimal intervals before the start of learning. Investigating the issue of whether average spacing intervals were responsible for the effects seen in Experiment 1 was one of the goals of Experiment 2, in which spacing interval sizes were equated across fixed and adaptive conditions.

Experiment 2: Adaptive Sequencing Versus Yoked-Adaptive Fixed Spacing

Experiment 1 provided clear evidence that adaptive sequencing produces better learning than some common fixed spacing schedules that have been shown to benefit learning in prior research. In Experiment 2, we attempted to determine the *locus* of learning effects in adaptive schedules. To do this, we compared adaptive schedules with new, specially devised fixed schedules that were matched to have patterns of spacing intervals similar to those generated by adaptive schedules.

What drives the benefits of adaptive scheduling? We have suggested that the power of adaptive intervals rests on adaptation to ongoing learning strength. If so, there are at least two possible sources of this advantage—adaptation to individual items and adaptation to individual learners. In order to assess each of these influences, we compared adaptive learning to two new kinds of fixed schedules. These fixed “yoked” schedules had spacing intervals that were identical to those participants had generated using adaptive schedules in Experiment 1.

Yoking fixed schedules to adaptive schedules was accomplished in the following way: A participant in a “yoked” condition received the same schedule of spacing intervals that a prior participant in an adaptive condition had received. Yoked schedules were

predetermined (fixed) and had no relation to participants’ ongoing pattern of performance during learning. One of the yoked conditions (the *yoked-item* condition) was designed to preserve spacing intervals that were found for individual items. In this condition, a learner received the same schedule of intervals that a prior adaptive participant had received. Each item was presented in the same order, and the pattern of intervals given to each item was retained. To give a concrete example, if a prior adaptive learner had received the country “Angola” with a 1–5–15 series of intervals, with the first appearance of Angola occurring on Trial 12, a *yoked-item* user would get Angola at the same point in their learning session with the same spacing intervals.

In the other yoked condition (the *yoked-random* condition), a learner received the same schedule of spacing intervals that a prior participant received, but items were shuffled across the prespecified schedule of spacing intervals. If a prior adaptive learner had received Angola as described above, a *yoked-random* learner would receive the same series of spacing intervals (1–5–15), beginning with the same trial number for initial appearance, but for a different item (e.g., “Botswana”).⁶

The yoking manipulations served multiple purposes. First, if the advantages of adaptive learning found in Experiment 1 resulted merely from the distributions of spacing intervals that occurred in the adaptive condition, or interleaving or variability of retrieval contexts that occurred from adaptive spacing, we would expect that those advantages would be fully preserved in both yoked conditions. If, on the other hand, adaptive scheduling is responsive to learning strength for particular learners and items, simply duplicating the kinds of intervals produced by adaptive spacing for other users should not produce learning results at the level given by individualized adaptive learning. The *yoked-random* condition tests this possibility, as it uses spacing intervals characteristic of performance with the adaptive algorithm but not based on the current participant’s responses or most suited to individual items.

The yoked-item condition tests the possibility that beneficial spacing can be predicted to some degree, across learners, by variations in individual learning items. It is possible that the advantages evident with adaptive sequencing in Experiment 1 occurred because some items are in general more difficult than others, and the adaptive algorithm detected this from learners’ performance. If so, the “magic” of adaptive sequencing might reside in adjusting spacing to fit item difficulty. This possibility would have potential practical consequences: adaptive systems that track individual responses and adjust spacing dynamically might not be needed if item difficulties are similar across learners and can be somehow determined in advance. If, on the other hand, learning strengths differ as a function of interactions of individual learners and items, then we would expect that replicating the item spacings from previous participants would not be as effective as adaptive scheduling.

⁶ Another variation would involve averaging across all item delays for participants in an adaptive condition and yoking new participants’ delays to these averages. Of many possible variations along these lines, we chose direct yoking because of the power it gave us in generating subconditions that examine aspects of item versus learner differences in learning.

Method

Participants. The participants were 48 undergraduate psychology students who received course credit for completing the experiment. Data collection was planned for 16 participants in each condition, allowing for more if in a week cycle of participant pool signups there were more than 16. Sixteen participants was consistent with an a priori power analysis for $\alpha = .05$ and $\beta = .05$ with an expectation of an effect size of 1.2. While effect sizes were not as high as 1.2, we did find large effect sizes in the study, along with other statistical analyses suggesting reliable and substantial effects.

Materials. The learning materials were identical to Experiment 1—that is, 24 African countries as well as filler items.

Design. Experiment 2 retained the pretest, posttest, delayed posttest design of Experiment 1. There were three between-subjects conditions (16 participants per condition): learning items were presented to participants in either an adaptive schedule (identical to Experiment 1), or in one of two yoked fixed schedules. Each participant in the fixed conditions was assigned a single adaptive yoked “target” participant, usually the participant who had last run in the adaptive condition. An adaptive participant was run first, followed by two fixed participants. Participants were thus effectively randomly assigned to condition. In every condition during the learning phase, each learning item was presented a total of four times.

Yoking conditions. For participants in the *adaptive* condition, scheduling of item presentation was dynamically determined by the ARTS system, as in Experiment 1. For participants in both yoked conditions, items were presented on a fixed, preset schedule. Each participant’s schedule was based on a prior adaptive participant’s trial record. In the *yoked-item* condition, the trial record was simply copied, so that a new yoked participant received a duplicate version of the trial record of the prior adaptive participant including the order of introduction of items, the size of the spacing intervals delivered to items, and the number and schedule of filler items. In the *yoked-random* condition, the trial record of the previous adaptive participant was retained but the mapping of items to sets of spacing intervals was shuffled, so that new yoked participants received for each item the same sequence of spacing intervals that an earlier participant had generated adaptively, but the specific item was different. For example, if a prior participant in the adaptive scheduling condition received three spacing intervals of 2–4–10 for the item “Angola,” a participant in the *yoked-item* condition would get the same item, at the same point in the learning session with the same intervals; whereas a participant in the *yoked-random* condition would receive the same series of intervals but for a different item (e.g., “Botswana”). For both yoked conditions, then, every series of spacing intervals (e.g., 2–4–10) occupied the same serial position in the learning session as had been generated by a prior adaptive learner, but for the *yoked-random* group, items were shuffled across the preset series of intervals. The yoking design, as noted above, aimed to have each participant in a yoked condition copy a unique adaptive participant’s trial schedule. However, due to instances of errors with participant login to the system, four participants in the *yoked-random* condition shared two yoked schedules, and two participants in the *yoked-item* condition shared the same yoked schedule.

Procedure. The order of the pretest, learning phase, posttest and delayed posttests were identical to Experiment 1. Trial presentations were identical to Experiment 1.

Results

The primary results of Experiment 2 are shown in Figure 9, which depicts mean accuracy by condition in all phases of the experiment. The *adaptive* condition showed higher accuracy than both yoked conditions in the learning phase, immediate posttest, and delayed posttest. There appears to be a trend for the *yoked-item* condition to outperform the *yoked-random* condition in the learning phase and in both posttests. These observations were confirmed by the analyses.

Pretest scores. Mean accuracies did not differ significantly at pretest as shown by a one-way ANOVA with condition as the between-subjects factor, $F(2, 45) = 1.23, p = .30, \eta_p^2 = .052$. Mean pretest scores ($M = 0.053, SD = 0.061$) did differ significantly from chance responding, one sample t test: $t(47) = 2.88, p < .01$, suggesting that some participants possessed prior knowledge of some countries. As a result, we computed change and gain scores between pretest and posttest in addition to comparing average accuracies.

Posttest accuracy. Accuracy data were analyzed by a 3×2 mixed-factor ANOVA with condition (*adaptive* vs. *yoked-random* vs. *yoked-item*) as a between-subjects factor and test phase (immediate vs. delayed posttest) as a within-subjects factor. There was a significant main effect of condition, $F(2, 45) = 3.3, p = .046, < \eta_p^2 = .128$, a significant main effect of test phase, $F(1, 45) = 77.09, p < .001, \eta_p^2 = .631$, and no Condition \times Test Phase interaction, $F(2, 45) = 0.36, p = .7, \eta_p^2 = .016$. A Bartlett’s test confirmed homogeneity of variance for accuracies at both posttests

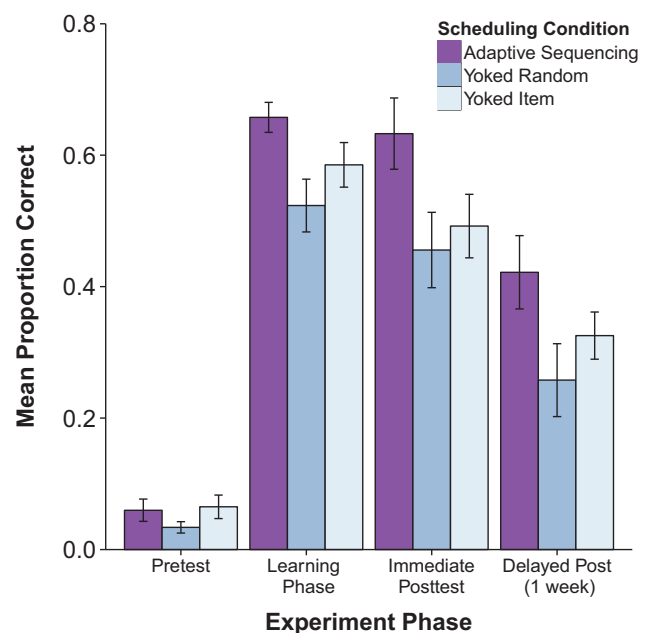


Figure 9. Mean proportion correct by phase across the three scheduling conditions in Experiment 2. Error bars show ± 1 SEM. See the online article for the color version of this figure.

(immediate: $p = .80$, delayed: $p = .19$). At the immediate posttest, average accuracies were highest for the *adaptive* condition ($M = 0.63$, $SD = 0.22$), lower for the *yoked-item* condition ($M = 0.49$, $SD = 0.19$), and lowest for the *yoked-random* condition ($M = 0.46$, $SD = 0.23$). Comparing means at the immediate posttest, t tests showed average accuracies for the *adaptive* condition were significantly greater than the *yoked-random* condition, $t(30) = 2.24$, $p = .032$, Cohen's $d = 0.80$ and *adaptive* spacing marginally exceeded the *yoked-item* condition, $t(30) = 1.94$, $p = .062$, Cohen's $d = 0.69$. The two *yoked* conditions did not differ significantly from one another, $t(30) = 0.49$, $p = .63$, Cohen's $d = 0.17$. Accuracies at the delayed posttest were highest in the *adaptive* condition ($M = 0.42$, $SD = 0.22$), lower for the *yoked-item* condition ($M = 0.326$, $SD = 0.144$) and lowest for the *yoked-random* condition ($M = 0.26$, $SD = 0.22$). Similar to the immediate posttest, at the delayed posttest, average accuracies for the *adaptive* spacing condition were significantly greater than the *yoked-random* condition, $t(30) = 2.09$, $p = .045$, Cohen's $d = 0.74$, but did not significantly exceed the *yoked-item* condition, $t(30) = 1.45$, $p = .16$, Cohen's $d = 0.53$. The two *yoked* conditions did not differ, $t(30) = 1.03$, $p = .31$, Cohen's $d = 0.37$.

Change and gain scores. Since there was measurable prior knowledge, we examined posttest results in terms of change scores computed between pretest and posttests. We computed the same two types of change scores as in Experiment 1: change scores and gain scores. Change scores were computed by subtracting average pretest accuracies from average posttest accuracies. Gain scores were computed by subtracting pretest scores from posttest scores, ignoring items that were accurate at pretest and inaccurate at posttest. Immediate and delayed posttest change scores are shown in Figure 10.

A 3×2 mixed-factor ANOVA on condition and test phase revealed a significant main effect of condition, $F(2, 45) = 3.64$,

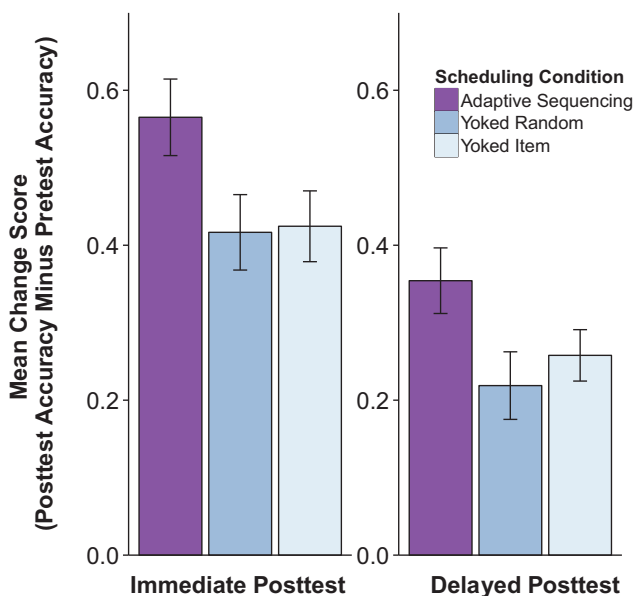


Figure 10. Mean change score at immediate and delayed posttests in Experiment 2. Error bars show ± 1 SEM. See the online article for the color version of this figure.

$p = .034$, $\eta_p^2 = .139$, a significant main effect of test phase, $F(1, 45) = 77.09$, $p < .001$, $\eta_p^2 = .631$, and no Condition \times Test Phase interaction, $F(2, 45) = 0.36$, $p = .7$, $\eta_p^2 = .016$. Change scores at immediate posttest were highest in the *adaptive* condition ($M = 0.57$, $SD = 0.20$), lowest in the *yoked-random* condition ($M = 0.42$, $SD = 0.20$) and nearly as low in the *yoked-item* condition ($M = 0.42$, $SD = 0.18$). Comparing means, t tests were significantly different between the *adaptive* and both of the two *yoked* conditions, *adaptive* versus *yoked-item*: $t(30) = 2.09$, $p = .045$, Cohen's $d = 0.74$; *adaptive* versus *yoked-random*: $t(30) = 2.14$, $p = .04$, Cohen's $d = 0.76$, but the two *yoked* conditions did not differ significantly, $t(30) = 0.12$, $p = .91$, Cohen's $d = 0.04$. Delayed posttest change scores were lower but similar: average scores were highest in the *adaptive* condition ($M = 0.35$, $SD = 0.17$), lowest in the *yoked-random* condition ($M = 0.22$, $SD = 0.17$) and nearly as low in the *yoked-item* condition ($M = 0.26$, $SD = 0.13$). Comparing means, t tests showed significant differences between the *adaptive* and the *yoked-random* conditions, $t(30) = 2.23$, $p = .033$, Cohen's $d = 0.78$, and a marginally significant difference between *adaptive* and *yoked-item*, $t(30) = 1.79$, $p = .08$, Cohen's $d = 0.63$. The difference between the two *yoked* conditions was not significant, $t(30) = 0.71$, $p = .48$, Cohen's $d = 0.25$.

In addition to change scores, we also computed gain scores. Pretest scores were subtracted from posttest scores, excluding items that were accurate at both posttest and pretest. Gain score results were different from change score results in the following ways: the paired comparisons between *adaptive* and the two *yoked* conditions at immediate posttest were only marginally significant (*adaptive* vs. *yoked-random*: $p = .057$; *adaptive* vs. *yoked-item*: $p = .058$), and the paired comparison at delayed posttest between *adaptive* and *yoked-item* conditions was not significant ($p = .12$).

Forgetting score. In addition to change scores, we computed a forgetting score, subtracting delayed posttest accuracy from immediate posttest accuracy, where we included only those items that were answered correctly at the immediate posttest. Forgetting scores are shown in Figure 11. Mean forgetting scores were lowest for the *adaptive* condition ($M = 0.41$, $SD = 0.22$), next lowest for the *yoked-item* condition ($M = 0.50$, $SD = 0.21$) and highest for the *yoked-random* condition ($M = 0.56$, $SD = 0.24$). Paired comparisons showed that the difference between the *adaptive* and *yoked-random* conditions was marginally significant, $t(30) = 1.85$, $p = .07$, Cohen's $d = 0.66$; the difference between the *adaptive* and the *yoked-item* condition was not significant, $t(30) = 1.24$, $p = .23$, Cohen's $d = 0.44$; and the difference between the two *yoked* conditions was not significant, $t(30) = 0.72$, $p = .48$, Cohen's $d = 0.25$.

Response times. Mean RTs are shown in Figure 12 for each condition and each experimental phase except pretests. (Pretest RTs were ignored owing to the few items that were answered correctly in that phase.) RT data include only RTs from trials on which correct answers were given.

In the learning phase, a one-way ANOVA showed no significant differences between conditions ($p > .18$). A 3×2 ANOVA with scheduling condition and posttest phases as factors found no significant effect of condition, $F(2, 45) = 0.26$, $p = .77$, $\eta_p^2 = .011$, no effect of test phase, $F(1, 45) = 1.12$, $p = .29$, $\eta_p^2 = .024$, and no scheduling condition \times posttest phase interaction, $F(2, 45) =$

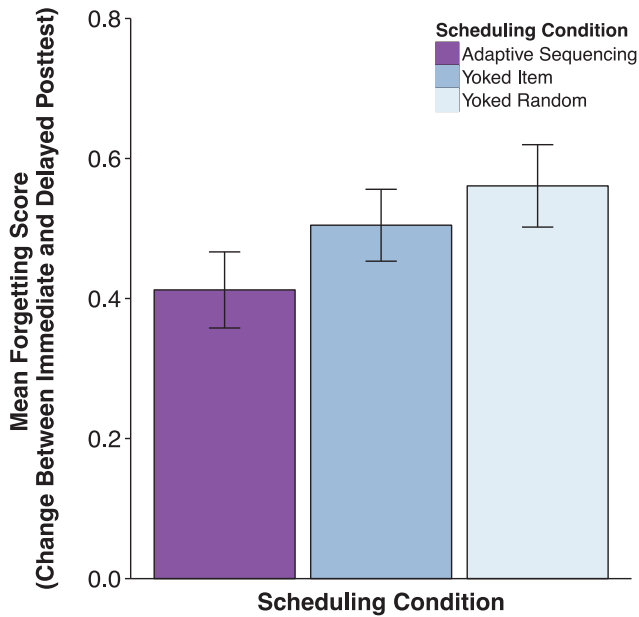


Figure 11. Mean forgetting score—reduction in the proportion of items answered correctly from immediate to delayed posttest—for the three scheduling conditions in Experiment 2. Forgetting scores are from items answered correctly on the immediate posttest only. Error bars show ± 1 SEM. See the online article for the color version of this figure.

0.03, $p = .97$, $\eta_p^2 = .001$. Individual comparisons showed that RTs at each phase were not significantly different from one another (all $ps > .05$). Comparing RTs across posttest phases, no conditions showed significantly different RTs across the two posttests (all $ps > .05$).

Analyses of spacing intervals. *Adaptive* and *yoked* conditions differed only slightly in the size of spacing intervals delivered to individual items during the learning session. The mean spacing interval per learner was calculated by averaging the mean spacing intervals for every item for that learner and averaging over items. Mean adaptive spacing intervals were close in length to the *adaptive* condition in Experiment 1 (*adaptive*: $M = 6.77$, $SD = 1.46$, *yoked-random*: $M = 6.81$, $SD = 1.40$, *yoked-item*: $M = 6.89$, $SD = 1.43$). We also looked at the size of spacing intervals conditional on whether the presentation before the spacing interval was responded to correctly to distinguish the sizes of adaptive versus enforced delay spacing intervals. Mean spacing intervals by presentation are shown in Figure 13. As in Experiment 1, the conditional values revealed that adaptive spacing interval sizes were bimodal: larger for correct responses ($M = 10.92$, $SD = 5.04$) and smaller for incorrect ones ($M = 1.0$, $SD = 0.0$).

Finally we examined the average spacing interval at each presentation number for the *adaptive* condition to see if interval sizes corresponded to those generated in Experiment 1. Figure 14 shows mean sizes of spacing intervals in the *adaptive* condition as similar to Expt. 1: mean initial interval was the smallest ($M = 1.67$, $SD = 0.98$), the second interval largest ($M = 10.42$, $SD = 2.12$), and the third interval smaller than the second interval ($M = 8.22$, $SD = 1.82$).

Discussion

In Experiment 2 adaptive scheduling led to larger learning improvements than fixed schedules at both an immediate and delayed posttest. Adaptive schedules performed better despite the fact that the fixed schedules in this study possessed highly similar spacing intervals to adaptive schedules. These fixed schedules were “yoked” to mimic the spacing interval characteristics of schedules generated by an adaptive algorithm. In the *yoked-item* condition intervals were tuned to individual items: Participants received the exact schedule that a prior adaptive participant received, such that spacing intervals associated with each item were exactly duplicated. In the *yoked-random* condition, intervals were not attached to individual items: participants received a prior adaptive schedule but items were introduced in a random order so that each item received the schedule of intervals appropriate for some different item. This schedule tested for effects of the distribution of spacing intervals overall, apart from specific effects of particular items.

Adaptive scheduling showed significantly greater learning as measured by change scores between pretest and posttest than both *yoked* conditions at an immediate posttest. Adaptive scheduling also significantly outperformed the *yoked-random* condition, both in terms of greater learning accuracy at both posttests, and in terms of change-scores and gain scores at a delayed test. There were several marginal effects of *adaptive* over both *yoked-item* and *yoked-random* conditions: gain scores were marginally better at immediate posttest, accuracies were marginally better at delayed posttest for the *adaptive* condition than the *yoked-item* condition, and in all cases, *yoked-item* performance trended numerically lower than performance in the *adaptive* condition. In addition,

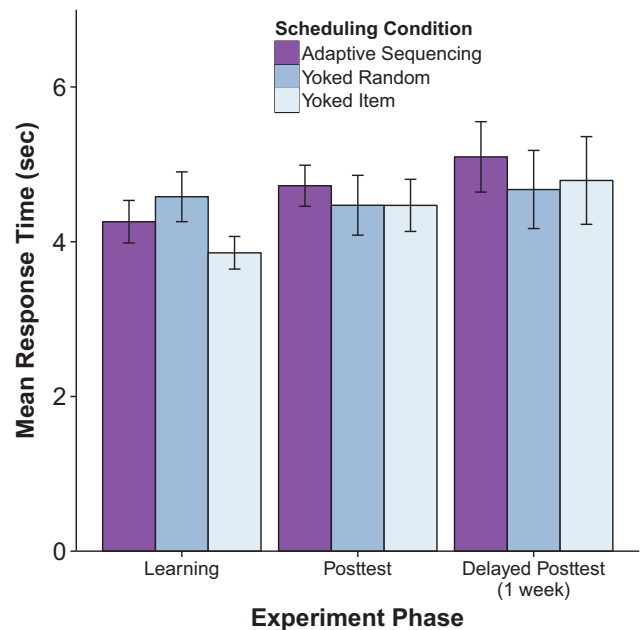


Figure 12. Mean response time (in seconds) at learning and test phases across three scheduling conditions in Experiment 2. Response times are from correctly answered trials only. Error bars show ± 1 SEM. See the online article for the color version of this figure.

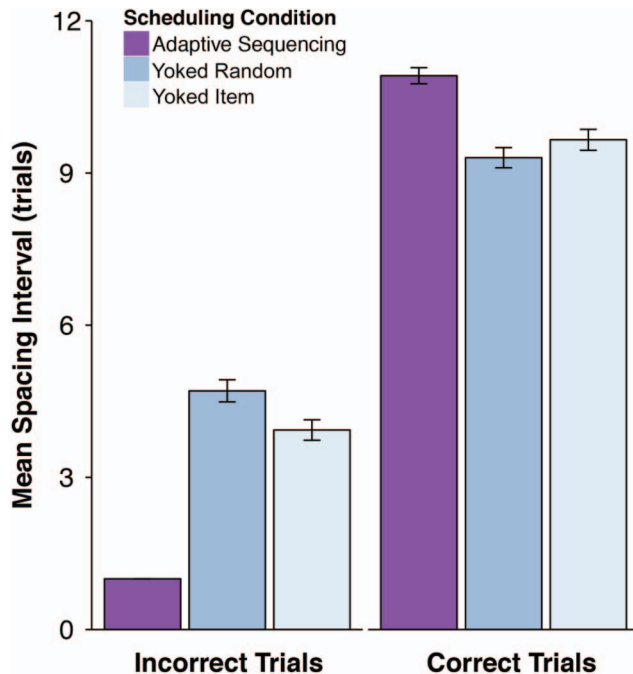


Figure 13. Mean spacing interval size (in trials) across three scheduling conditions in Experiment 2 conditional on whether the trial preceding the interval was answered correctly or not. Error bars show ± 1 SEM. See the online article for the color version of this figure.

when measuring the amount of forgetting between immediate and delayed posttests for each condition, the *adaptive* condition had significantly less forgetting than the *yoked-random* condition. Even the weakest statistical (marginally significant) comparisons between *adaptive* and the *fixed* conditions showed effect sizes from .57 to .70; these are considered medium to large effect sizes.

In no case, neither at immediate nor delayed posttest, nor for any measure of performance, did the two yoked conditions differ significantly from each other. The numerical advantage in the *yoked-item* condition over the *yoked-random* condition may suggest that tuning intervals to the spacing requirements of individual items could be of some value in generating a predetermined schedule. However, the present results suggest that such schedules perform more poorly than adaptive schedules, indicating that knowledge of item difficulty is not the primary driver of gains in adaptive scheduling.

These results echo and extend the results of Experiment 1. Learning is better when spacing intervals are a function of ongoing learner performance. The results support the hypothesis that optimizing spacing requires attunement to learning strength, which varies for learners and items in a dynamic way. Since there is no way to predict the pattern of learning strength changes for items for a new learner, adaptive spacing offers the only avenue toward optimizing spacing intervals for sets of items across a learning session.

General Discussion

The spacing effect is a powerful driver of human learning. It is also a major focus of research, with 7,880 entries appearing on

Google Scholar and 4,540 entries between 2005 and 2016. A significant portion of work on this effect has been aimed at determining what spacing schedules promote the best learning. Most of that work, and most explicit implementations of spacing in learning applications, have utilized fixed arrangements of spacing intervals.

The present work provides evidence that fixed intervals of spacing, in general, cannot be optimal. Experiment 1 showed that adaptive spacing based on ongoing assessment of learning strength for individual items and learners outperforms typical fixed spacing schedules. Experiment 2 probed more deeply the reasons for the advantages of adaptive spacing. Even when overall properties of spacing distributions were matched across *adaptive* and *yoked* fixed conditions, the *adaptive* condition produced better learning outcomes. This experiment also revealed that the advantages of adaptive spacing cannot be captured in a fixed, predetermined schedule based on data about the differential difficulty of various learning items: The *yoked-item* condition of Experiment 2 preserved spacing characteristics for individual items that adaptive learners had produced. These did indeed vary somewhat across items, but replicating those differences with new learners did not produce learning outcomes comparable to those obtained with an adaptive system that used RTs to track learning strength for particular learners and items.

These results cohere with an emerging account of spacing effects. Although spacing likely benefits learning for multiple reasons, the explanation that may be most relevant for determining the optimal recurrence interval for a learning item (or category; see Mettler & Kellman, 2014) involves the importance of retrieval difficulty and its relation to learning strength. A new learning trial confers optimal benefit for learning a given item when that item can be retrieved with greatest difficulty but has not yet been

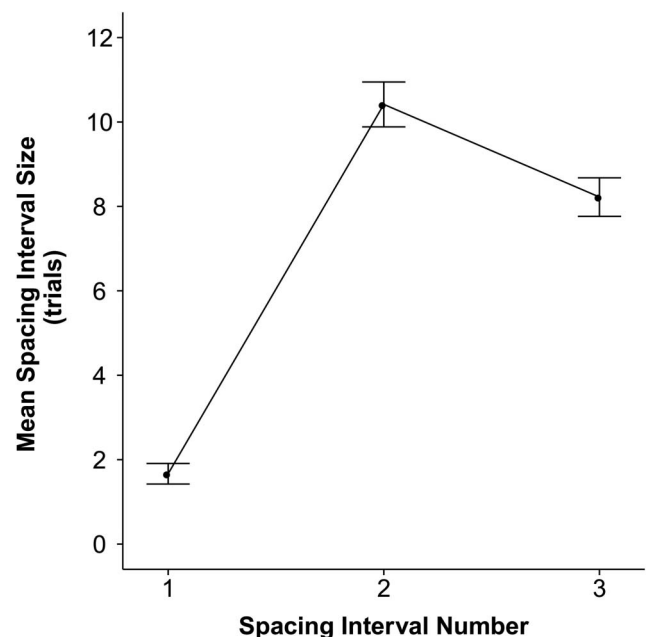


Figure 14. Mean spacing interval size (in trials) across three intervals in the adaptive scheduling condition of Experiment 2. Error bars show ± 1 SEM.

forgotten (Pyc & Rawson, 2009; Bjork & Bjork, 1992). The difficulty of retrieving an item will generally increase with trials or time elapsed since its last presentation, due to decay in learning strength, interference from intervening learning trials, or presentation of confusable items. The specific relations between these variables and difficulty are likely mediated by learning strength. Not all effects of intervening trials and items reduce learning strength; some influences that may increase learning strength are reminders or cues that may be provided by other items, or presentation of other items or feedback that help differentiate items. Also intervening items that are unrelated to learning items probably have less effect on learning strength than items closely related to learning items (Storm, Bjork & Storm, 2010). Fluctuations in effort or arousal could influence learning strength in either direction. All of these influences reduce the effectiveness of predetermined schedules relative to adaptive systems that gauge learning strength in an ongoing manner.

The present data indicate that use of RTs together with accuracy in adaptive learning, as in the ARTS system used here, allows dynamic assessment of ongoing learning strength. ARTS outperformed typical fixed spacing schemes often employed in the literature on the spacing effect. Although the assessment and mastery features of ARTS are designed to guide learners to mastery criteria, implying that the time to mastery will vary for different learners, here we omitted the mastery features and tested ARTS under conditions typically used in spacing experiments. In both experiments, items were presented a limited number of times. It was possible that the benefits of ARTS would not be apparent with only four presentations; continuing adaptive learning to mastery would have likely led to differing numbers of presentations for each item, higher levels of performance, and may have been particularly valuable for retention after a delay. However, despite imposing on the adaptive system the limitations of a small, equal set of presentations for each item, adaptively scheduling the spacing proved more beneficial than presenting items with fixed schedules. Also, because the amount of forgetting differed between conditions, it is likely that the effects of adaptive scheduling changed the strength of learning items in memory, rather than simply conveying benefit to the speed of learning during the learning phase. The benefit of adaptive scheduling over fixed schedules was substantial, with medium to large effect sizes that persisted across a 1-week delay. In no condition and at no test were fixed schedules found to perform better than adaptive schedules.

Theoretical Implications

Results from studies of adaptive scheduling offer a window onto theoretical debates about the optimal schedule of practice in learning and memory, specifically, debates about equal or expanding spacing (Karpicke & Roediger, 2007; Landauer & Bjork, 1978; Storm, Bjork, & Storm, 2010; Carpenter & Delosh, 2005) and research investigating the locus of learning effects in spaced practice (Karpicke & Bauernschmidt, 2011; Pashler, Zarow, & Triplett, 2003). We comment on each in turn.

Is expanding practice optimal for retention? A major controversy in the spacing literature has been whether fixed schedules of equal intervals or schedules of expanding spacing intervals produce better learning. Expanding retrieval practice is sometimes thought to be the most effective distributed scheduling technique

(Landauer & Bjork, 1978; Pimsleur, 1967; Storm, Bjork, & Storm, 2010); however other evidence indicates there is no difference between expanding and equal schedules of practice (Karpicke & Bauernschmidt, 2011) or even that equal interval practice is superior to expanding practice (Karpicke & Roediger, 2007; Logan & Balota, 2008) or superior at a delay (Cull, 2000).

Our results have several implications for this issue. First, as we suggested above, there will not be a single, general answer to the question of the best fixed schedule. Variations in published results using varied material, conditions of learning, and learners can be explained by effects of these variables on retrieval difficulty as mediated by learning strength (cf. Storm, Bjork & Storm, 2010). That said, our results with adaptive learning do offer some support for expanding schedules in the learning domain we studied. Our retrospective analyses of the patterns of spacing intervals generated in adaptive conditions showed that these tended to be expanding. We also found some evidence that long-term performance correlates with expanding trial spacing rather than equal or contracting spacing. When spacing intervals expanded for an item, as measured by successively increasing interval sizes across presentations, delayed posttest scores for those items were greater. (See Supplemental Materials, Figure 3). Pavlik and Anderson (2008) also found that the spacing generated by their adaptive algorithm was expanding. While the spacing results of an adaptive scheduling system are only indirect evidence for the benefits of expanding spacing, they are nonetheless strongly indicative of advantages. It should be noted that in our study, although expanding spacing was often the actual spacing outcome of adaptive scheduling, not all participants or items experienced expanding spacing intervals. In fact, while the trend across presentations in the adaptive condition was *on the whole* expanding, only a few participants experienced strictly expanding spacing for all learning items across all presentations. These findings further confirm the operation of influences on learning strength that are not predictable in advance by predetermined schedules having either equal or expanding spacing intervals.

Further debate between choices of optimal fixed spacing schedules is likely to remain equivocal. When spacing is decided in advance of dynamic assessment of learner performance, retrievals may fail due to exceedingly long delays, or initial retrievals may be too easy and fail to add much to learning strength. Karpicke and Roediger (2007) commented:

Considering the widespread belief in the utility of expanding retrieval, it is surprising that there is not a larger base of research with consistent evidence showing expanding retrieval practice to be the superior spaced practice technique for improving long-term retention. (p. 705)

We would argue instead that the lack of consistent evidence for any fixed spacing scheme is unsurprising, given that fixed schedules lack the flexibility to match spacing parameters to specific materials, items and learners across a variety of situations.

What makes spacing beneficial? Our experiments also reflect on hypothesized drivers of spacing advantages—for example, characteristics of spacing interval size such as absolute delay length (Karpicke & Bauernschmidt, 2011). If generic characteristics of absolute spacing intervals were crucial, we would have expected equivalent performance in two conditions that received the same pattern and size of delays. In fact, a different outcome

occurred: Even when schedule characteristics were equated, learning suffered in comparison to a condition where spacing intervals did adapt to individual learners' interactions with items. The primary reason to alter spacing intervals during practice is to match the characteristics of ongoing learning strength, not to meet particular delay characteristics or criteria of spacing schedules in the abstract. Because ARTS can measure learning strength as learning progresses, it can optimize learning events to a degree that fixed spacing schedules cannot match, no matter the specific delay characteristics of the fixed intervals.

This point applies to considerations regarding initial and later intervals of practice. Evidence suggests that optimizing initial retrievals when learning strength is low for poorer learners or for difficult material can improve learning (Cull, Shaughnessy & Zechmeister, 1996). It has also been suggested that after appropriate initial intervals, later intervals have very little effect on learning (Karpicke & Roediger, 2010). In our results, differences in learning emerged from manipulations of spacing intervals even when schedules were matched on their initial intervals (Experiment 2). Specifically, when spacing intervals were adaptive, learning benefits can accrue despite matches with fixed spacing conditions in the size of the initial spacing interval. The results suggest that appropriately adjusting spacing throughout learning—not just at the beginning—is an important and effective way to generate learning gains.

Comparison with other adaptive systems. The present results suggest that these benefits of adaptively arranged spacing might be relatively easy to realize in real-world learning settings and improve upon techniques used in other adaptive systems. The ARTS system was able to extract useful assessments of ongoing learning strength while in use by learners. Extraction of RT data along with accuracy is relatively simple and unobtrusive. Adaptive systems have commonly required prior studies with particular learning content and similar participants to obtain model parameters (Atkinson, 1972; Pavlik & Anderson, 2008), or they attempt to find optimal scheduling without relying on prior studies, but do not adapt to ongoing changes in learning strength (Khajah, Lindsey, & Mozer, 2014). In real-world learning settings, it would often be impractical to run a prior experiment with similar learners and the same learning material. There are advantages to an adaptive learning system that does not require such prerequisites. Moreover, some results of the current studies indicate there are limits to the efficiencies attainable using data obtained from other learners; optimal spacing may require personalized, ongoing attunement to each learner's performance during learning.

In addition to efficiencies in implementation, ARTS's use of RT measures in addition to accuracy provides a potentially more accurate assessment of learning strength than other systems. Prior adaptive systems have relied primarily on accuracy alone, in some cases informed by theoretical models about how learning strength might grow or decay. Accuracy is a binary variable and provides limited information; adding RTs can provide more nuanced, up-to-date information about learning strength. Although ARTS was not directly compared with adaptive systems in the current research, some evidence indicates that use of ongoing RT data provides a better measure of learning strength and thus translates to greater learning performance and delayed retention than other systems (Mettler, Massey & Kellman, 2011).

Bridging studies of fixed and adaptive spacing. The present work directly compared an adaptive learning system with the fixed schedules of spacing typically studied in the memory literature. To our knowledge, this has not been done in any previous work. In bridging two research literatures that have been largely separate, the present work, and future work of this kind, has substantial potential to clarify major issues in understanding learning in general and spacing in particular. First and foremost, as described above, comparing fixed and adaptive schedules offers a window into the mechanisms of spacing. The present results help illuminate prior findings and disagreements in the fixed spacing literature, as well as the advantages of adaptive spacing. They converge on an understanding of much of the value of spacing in terms of three ideas: the retrieval difficulty hypothesis, the connection between retrieval difficulty and learning strength, and the value of up-to-the-moment assessment of learning strength from accuracy combined with RTs. This emerging understanding may clarify a number of issues in the field, such as theories that attempt to explain which fixed schedules are effective. For example, some models of learning predict schedules that tend to be contracting rather than expanding (see Lindsey, Mozer, Cepeda, & Pashler, 2009) while other adaptive schemes show smoothly expanding practice (Pavlik & Anderson, 2008) and are in agreement with theories of expanding spacing (Cull et al., 1996; Landauer & Bjork, 1978; Rea & Modigliani, 1985). Second, comparisons between fixed and adaptive spacing would appear to be important threshold tests for adaptive systems. An adaptive schedule should be more effective than fixed schedules, else the theoretical assumptions and the practical implications of that adaptive model are suspect. In addition, connecting these lines of research may be relevant to other features of learning systems. Adaptive schedules often use learning to mastery criteria, an important element in many real-world settings. In the studies here, we used a fixed number of presentations for items, but further comparisons of adaptive and fixed presentations might be useful where the number of presentations is not set in advance and mastery criteria are employed. In general, unifying these research areas may connect each with the theoretical tools and insights of the other. In particular, the current research suggests an important conclusion: that predetermined (fixed) schedules cannot be optimal, as they do not adjust to ongoing fluctuations in learning strength—involving individual items, learners, and times in a learning session—and thus cannot determine the best spacing in terms of retrieval effort and successful retrieval.

Practical applications. The techniques discussed here have important implications and relevance in many domains including theories of optimal educational practice, the cognitive science of learning, and the psychological understanding of learning and memory processes. The techniques developed here have already been applied to real world learning problems such as mathematics learning (Mettler, Massey & Kellman, 2011) and extend to perceptual or category learning (Mettler & Kellman, 2014), such as the training of expertise in perceptual learning in domains like aviation and a number of medical learning domains, such as echocardiography, radiology, dermatology and pathology (Krasne, Hillman, Kellman, & Drake, 2013; Rimoim, Altieri, Craft, Krasne, & Kellman, 2015; Thai, Krasne & Kellman, 2015). It is important to note that there will likely be some differences when laboratory studies such as those in this article are generalized to large scale,

real-world educational domains. However, the techniques described in this article have already been successfully deployed in large-scale studies, with long-term consequences for learning. In work applying the adaptive learning system described here to perceptual category learning in medical domains, for example, learning gains in a Histopathology perceptual adaptive learning module (PALM) were substantially preserved in delayed posttests given 6–7 weeks later (Krasne et al., 2013); in a dermatology PALM, advantages for students who completed the module over those who did not were clearly evident in delayed tests given a year later (Rimoin et al., 2015), and in an echocardiography PALM, third-year medical students who invested about 45 min per day for 2 days to complete the module outperformed second-year emergency medicine residents, for whom ECG interpretation is a centrally important skill, with the learning gains for the PALM group being substantially preserved in a delayed posttest given a year later (Krasne, Stevens, Kellman, & Niemann, submitted). Adaptive systems based on ongoing assessment of learning strength can likely enhance learning in any domain where spacing and scheduling are important moderators of long-term learning strength. As such, they are likely to be valuable tools in many future applications of learning technology.

References

- Atkinson, R. C. (1972). Optimizing the learning of a second-language vocabulary. *Journal of Experimental Psychology*, 96, 124–129. <http://dx.doi.org/10.1037/h0033475>
- Baddeley, A. D. (1986). *Working memory*. Oxford, United Kingdom: Clarendon Press.
- Benjamin, A. S., & Bjork, R. A. (1996). Retrieval fluency as a metacognitive index. In L. Reder (Ed.), *Implicit memory and metacognition* (pp. 309–338). Hillsdale, NJ: Erlbaum.
- Benjamin, A. S., & Tullis, J. (2010). What makes distributed practice effective? *Cognitive Psychology*, 61, 228–247. <http://dx.doi.org/10.1016/j.cogpsych.2010.05.004>
- Bjork, R. A. (1999). Assessing our own competence: Heuristics and illusions. In D. Gopher & A. Koriati (Eds.), *Attention and performance XVII. Cognitive regulation of performance: Interaction of theory and application* (pp. 435–459). Cambridge, MA: MIT Press.
- Bjork, E. L., & Bjork, R. A. (2011). Making things hard on yourself, but in a good way: Creating desirable difficulties to enhance learning. In M. A. Gernsbacher, R. W. Pew, L. M. Hough, & J. R. Pomerantz (Eds.), *Psychology and the real world: Essays illustrating fundamental contributions to society* (pp. 56–64). New York, NY: Worth.
- Bjork, R. A., & Allen, T. W. (1970). The spacing effect: Consolidation or differential encoding? *Journal of Verbal Learning and Verbal Behavior*, 9, 567–572. [http://dx.doi.org/10.1016/S0022-5371\(70\)80103-7](http://dx.doi.org/10.1016/S0022-5371(70)80103-7)
- Bjork, R. A., & Bjork, E. L. (1992). A new theory of disuse and an old theory of stimulus fluctuation. In A. Healy, S. Kosslyn, & R. Shiffrin (Eds.), *From learning processes to cognitive processes: Essays in honor of William K. Estes* (Vol. 2, pp. 35–67). Hillsdale, NJ: Erlbaum.
- Bjork, R. A., Dunlosky, J., & Kornell, N. (2013). Self-regulated learning: Beliefs, techniques, and illusions. *Annual Review of Psychology*, 64, 417–444. <http://dx.doi.org/10.1146/annurev-psych-113011-143823>
- Carpenter, S. K., Cepeda, N. J., Rohrer, D., Kang, S. H., & Pashler, H. (2012). Using spacing to enhance diverse forms of learning: Review of recent research and implications for instruction. *Educational Psychology Review*, 24, 369–378. <http://dx.doi.org/10.1007/s10648-012-9205-z>
- Carpenter, S. K., & DeLosh, E. L. (2005). Application of the testing and spacing effects to name learning. *Applied Cognitive Psychology*, 19, 619–636. <http://dx.doi.org/10.1002/acp.1101>
- Cepeda, N. J., Pashler, H., Vul, E., Wixted, J. T., & Rohrer, D. (2006). Distributed practice in verbal recall tasks: A review and quantitative synthesis. *Psychological Bulletin*, 132, 354–380.
- Cepeda, N. J., Vul, E., Rohrer, D., Wixted, J. T., & Pashler, H. (2008). Spacing effects in learning: A temporal ridge of optimal retention. *Psychological Science*, 19, 1095–1102. <http://dx.doi.org/10.1111/j.1467-9280.2008.02209.x>
- Cull, W. L. (2000). Untangling the benefits of multiple study opportunities and repeated testing for cued recall. *Applied Cognitive Psychology*, 14, 215–235. [http://dx.doi.org/10.1002/\(SICI\)1099-0720\(200005/06\)14:3<215::AID-ACP640>3.0.CO;2-1](http://dx.doi.org/10.1002/(SICI)1099-0720(200005/06)14:3<215::AID-ACP640>3.0.CO;2-1)
- Cull, W. L., Shaughnessy, J. J., & Zechmeister, E. B. (1996). Expanding understanding of the expanding-pattern-of-retrieval mnemonic: Toward confidence in applicability. *Journal of Experimental Psychology: Applied*, 2, 365–378. <http://dx.doi.org/10.1037/1076-898X.2.4.365>
- Delaney, P. F., Verkoeijen, P. P., & Spiegel, A. (2010). Spacing and testing effects: A deeply critical, lengthy, and at times discursive review of the literature. *Psychology of Learning and Motivation*, 53, 63–147. [http://dx.doi.org/10.1016/S0079-7421\(10\)53003-2](http://dx.doi.org/10.1016/S0079-7421(10)53003-2)
- Dempster, F. N. (1989). Spacing effects and their implications for theory and practice. *Educational Psychology Review*, 1, 309–330. <http://dx.doi.org/10.1007/BF01320097>
- Ebbinghaus, H. (1913). *Memory: A contribution to experimental psychology*. New York: Teachers College, Columbia University. <http://dx.doi.org/10.1037/10011-000>
- Glenberg, A. M. (1976). Monotonic and nonmonotonic lag effects in paired-associate and recognition memory paradigms. *Journal of Verbal Learning and Verbal Behavior*, 15, 1–16. [http://dx.doi.org/10.1016/S0022-5371\(76\)90002-5](http://dx.doi.org/10.1016/S0022-5371(76)90002-5)
- Glenberg, A. M. (1979). Component-levels theory of the effects of spacing of repetitions on recall and recognition. *Memory & Cognition*, 7, 95–112. <http://dx.doi.org/10.3758/BF03197590>
- Hintzman, D. L. (1974). Theoretical implications of the spacing effect. In R. L. Solso (Ed.), *Theories in cognitive psychology: The Loyola Symposium* (pp. 77–99). Potomac, MD: Erlbaum.
- Howell, D. C. (2007). The analysis of missing data. In W. Outhwaite & S. Turner (Eds.), *Handbook of Social Science Methodology*. London, United Kingdom: Sage. <http://dx.doi.org/10.4135/9781848607958.n11>
- Johnston, W. A., & Uhl, C. N. (1976). The contributions of encoding effort and variability to the spacing effect on free recall. *Journal of Experimental Psychology: Human Learning and Memory*, 2, 153–160. <http://dx.doi.org/10.1037/0278-7393.2.2.153>
- Karpicke, J. D., & Bauernschmidt, A. (2011). Spaced retrieval: Absolute spacing enhances learning regardless of relative spacing. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 37, 1250–1257. <http://dx.doi.org/10.1037/a0023436>
- Karpicke, J. D., & Roediger, H. L., III. (2007). Expanding retrieval practice promotes short-term retention, but equally spaced retrieval enhances long-term retention. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 33, 704–719. <http://dx.doi.org/10.1037/0278-7393.33.4.704>
- Karpicke, J. D., & Roediger, H. L., III. (2010). Is expanding retrieval a superior method for learning text materials? *Memory & Cognition*, 38, 116–124. <http://dx.doi.org/10.3758/MC.38.1.116>
- Kellman, P. J., & Garrigan, P. (2009). Perceptual learning and human expertise. *Physics of Life Reviews*, 6, 53–84. <http://dx.doi.org/10.1016/j.plrev.2008.12.001>
- Khajah, M. M., Lindsey, R. V., & Mozer, M. C. (2014). Maximizing students' retention via spaced review: Practical guidance from computational models of memory. *Topics in Cognitive Science*, 6, 157–169. <http://dx.doi.org/10.1111/tops.12077>
- Kornell, N., & Bjork, R. A. (2007). The promise and perils of self-regulated study. *Psychonomic Bulletin & Review*, 14, 219–224. <http://dx.doi.org/10.3758/BF03194055>
- Kornell, N., & Bjork, R. A. (2008). Learning concepts and categories: Is spacing the “enemy of induction”? *Psychological Science*, 19, 585–592. <http://dx.doi.org/10.1111/j.1467-9280.2008.02127.x>

- Krasne, S., Hillman, J. D., Kellman, P. J., & Drake, T. A. (2013). Applying perceptual and adaptive learning techniques for teaching introductory histopathology. *Journal of Pathology Informatics*, 4, 34–41. <http://dx.doi.org/10.4103/2153-3539.123991>
- Krasne, S., Stevens, C. D., Kellman, P. J., & Niemann, J. T. (submitted). Mastering ECG interpretation skills through a perceptual and adaptive learning module. *Advances in Health Science Education*. Manuscript under review.
- Landauer, T. K., & Bjork, R. A. (1978). Optimum rehearsal patterns and name learning. In M. Gruneberg, P. Morris, & R. Sykes (Eds.), *Practical aspects of memory* (pp. 625–632). London, United Kingdom: Academic Press.
- Lindsey, R., Mozer, M., Cepeda, N. J., & Pashler, H. (2009). Optimizing memory retention with cognitive models. In A. Howes, D. Peebles, & R. Cooper (Eds.), *9th International Conference on Cognitive Modeling—ICCM2009*, Manchester, United Kingdom.
- Lindsey, R. V., Shroyer, J. D., Pashler, H., & Mozer, M. C. (2014). Improving students' long-term knowledge retention through personalized review. *Psychological Science*, 25, 639–647. <http://dx.doi.org/10.1177/0956797613504302>
- Logan, J. M., & Balota, D. A. (2008). Expanded vs equal spaced retrieval practice in healthy young and older adults. *Aging, Cognition, and Neuropsychology*, 15, 257–280.
- Mettler, E. (2014). *Studies of adaptive and fixed schedules in factual and perceptual learning*. Retrieved from Proquest. #3605425.
- Mettler, E., & Kellman, P. J. (2014). Adaptive response-time-based category sequencing in perceptual learning. *Vision Research*, 99, 111–123. <http://dx.doi.org/10.1016/j.visres.2013.12.009>
- Mettler, E., Massey, C. M., & Kellman, P. J. (2011). Improving adaptive learning technology through the use of response-times. In *Proceedings of the 33rd Annual Meeting of the Cognitive Science Society* (pp. 2532–2537). Boston, MA: Cognitive Science Society.
- Mozer, M. C., Pashler, H., Cepeda, N., Lindsey, R. V., & Vul, E. (2009). *Advances in Neural Information Processing Systems: Vol. 22. Predicting the optimal spacing of study: A multiscale context model of memory* (pp. 1321–1329).
- Pashler, H., Rohrer, D., Cepeda, N. J., & Carpenter, S. K. (2007). Enhancing learning and retarding forgetting: Choices and consequences. *Psychonomic Bulletin & Review*, 14(2), 187–193.
- Pashler, H., Zarrow, G., & Triplett, B. (2003). Is temporal spacing of test helpful even when it inflates error rates? *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 29, 1051–1057.
- Pavlik, P. I., & Anderson, J. R. (2005). Practice and forgetting effects on vocabulary memory: An activation-based model of the spacing effect. *Cognitive Science*, 29(4), 559–586.
- Pavlik, P. I., & Anderson, J. R. (2008). Using a model to compute the optimal schedule of practice. *Journal of Experimental Psychology: Applied*, 14, 101–117. <http://dx.doi.org/10.1037/1076-898X.14.2.101>
- Pimsleur, P. (1967). A memory schedule. *Modern Language Journal*, 51, 73–75. <http://dx.doi.org/10.1111/j.1540-4781.1967.tb06700.x>
- Pyc, M. A., & Rawson, K. A. (2009). Testing the retrieval effort hypothesis: Does greater difficulty correctly recalling information lead to higher levels of memory? *Journal of Memory and Language*, 60, 437–447. <http://dx.doi.org/10.1016/j.jml.2009.01.004>
- Raaijmakers, J. G. W. (2003). Spacing and repetition effects in human memory: Application of the SAM model. *Cognitive Science*, 27, 431–452. http://dx.doi.org/10.1207/s15516709cog2703_5
- Rawson, K. A., & Dunlosky, J. (2011). Optimizing schedules of retrieval practice for durable and efficient learning: How much is enough? *Journal of Experimental Psychology: General*, 140, 283–302. <http://dx.doi.org/10.1037/a0023956>
- Rea, C. P., & Modigliani, V. (1985). The effect of expanded versus massed practice on the retention of multiplication facts and spelling lists. *Human Learning: Journal of Practical Research & Applications*, 4, 11–18.
- Rimoin, L., Altieri, L., Craft, N., Krasne, S., & Kellman, P. J. (2015). Training pattern recognition of skin lesion morphology, configuration, and distribution. *Journal of the American Academy of Dermatology*, 72, 489–495. <http://dx.doi.org/10.1016/j.jaad.2014.11.016>
- Rohrer, D., & Taylor, K. (2006). The effects of overlearning and distributed practice on the retention of mathematics knowledge. *Applied Cognitive Psychology*, 20, 1209–1224. <http://dx.doi.org/10.1002/acp.1266>
- Rumelhart, D. E. (1967). *The effects of interpresentation interval on performance in a continuous paired-associate task*. Tech. Rep. No. 116, Institute for Mathematical Studies in Social Sciences, Stanford University.
- Snider, V. E. (2004). A comparison of spiral versus strand curriculum. *Journal of Direct Instruction*, 4, 29–39.
- Storm, B. C., Bjork, R. A., & Storm, J. C. (2010). Optimizing retrieval as a learning event: When and why expanding retrieval practice enhances long-term retention. *Memory & Cognition*, 38, 244–253. <http://dx.doi.org/10.3758/MC.38.2.244>
- Thai, K. P., Krasne, S., & Kellman, P. J. (2015). Adaptive perceptual learning in electrocardiography: The synergy of passive and active classification. In D. C. Noelle, R. Dale, A. S. Warlaumont, J. Yoshimi, T. Matlock, C. D. Jennings, & P. P. Maglio (Eds.), *Proceedings of the 37th Annual Meeting of the Cognitive Science Society* (pp. 2350–2355). Austin, TX: Cognitive Science Society.
- Thios, S. J., & D'Agostino, P. R. (1976). Effects of repetition as a function of study-phase retrieval. *Journal of Verbal Learning and Verbal Behavior*, 15, 529–536. [http://dx.doi.org/10.1016/0022-5371\(76\)90047-5](http://dx.doi.org/10.1016/0022-5371(76)90047-5)
- Tsai, L. (1927). The relation of retention to the distribution of relearning. *Journal of Experimental Psychology*, 10, 30–39. <http://dx.doi.org/10.1037/h0071614>
- Wahlheim, C. N., Dunlosky, J., & Jacoby, L. L. (2011). Spacing enhances the learning of natural concepts: An investigation of mechanisms, meta-cognition, and aging. *Memory & Cognition*, 39, 750–763. <http://dx.doi.org/10.3758/s13421-010-0063-y>
- Woźniak, P. A., & Gorzelańczyk, E. J. (1994). Optimization of repetition spacing in the practice of learning. *Acta Neurobiologiae Experimentalis*, 54, 59–62.
- Zhang, Y., Liu, R.-Y., Heberton, G. A., Smolen, P., Baxter, D. A., Cleary, L. J., & Byrne, J. H. (2011). Computational design of enhanced learning protocols. *Nature Neuroscience*, 15, 294–297. <http://dx.doi.org/10.1038/nn.2990>

Appendix

Adaptive Sequencing Parameters

Table A1

Parameters for the Adaptive Sequencing Algorithm in Experiments 1 and 2

Parameter	Value
a – counter weight	.1
b – default weight	1.1
r – response time weight	3.0
W – incorrect priority increment	20
D – delay constant	1

Received June 19, 2015

Revision received March 9, 2016

Accepted March 14, 2016 ■

ORDER FORM

Start my 2016 subscription to the *Journal of Experimental Psychology: General*® ISSN: 0096-3445

☐ \$160.00 **APA MEMBER/AFFILIATE**
☐ \$395.00 **INDIVIDUAL NONMEMBER**
☐ \$1,789.00 **INSTITUTION**

Sales Tax: 5.75% in DC and 6% in MD

TOTAL AMOUNT DUE \$ _____

Subscription orders must be prepaid. Subscriptions are on a calendar year basis only. Allow 4-6 weeks for delivery of the first issue. Call for international subscription rates.



AMERICAN
PSYCHOLOGICAL
ASSOCIATION

SEND THIS ORDER FORM TO

American Psychological Association
Subscriptions
750 First Street, NE
Washington, DC 20002-4242

Call **800-374-2721** or 202-336-5600
Fax **202-336-5568** :TDD/TTY **202-336-6123**
For subscription information,
e-mail: **subscriptions@apa.org**

☐ **Check enclosed** (make payable to APA)

Charge my: ☐ Visa ☐ MasterCard ☐ American Express

Cardholder Name _____

Card No. _____ Exp. Date _____

Signature (Required for Charge)

Billing Address

Street _____

City _____ State _____ Zip _____

Daytime Phone _____

E-mail _____

Mail To

Name _____

Address _____

City _____ State _____ Zip _____

APA Member # _____

XGEA16