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Los Angeles

Encoding and Retrieval Influences on the Strategic Study of Important Information

A dissertation submitted in partial satisfaction of the requirements for the degree

Doctor of Philosophy in Psychology

by

Catherine Diane Middlebrooks

2018

ABSTRACT OF THE DISSERTATION

Encoding and Retrieval Influences on the Strategic Study of Important Information

by

Catherine Diane Middlebrooks

Doctor of Philosophy in Psychology

University of California, Los Angeles, 2018

Professor Alan Dan Castel, Chair

Convenient though it might be, remembering everything that one might hope to remember is often rather unlikely. Prior research suggests that learners are capable of strategically offsetting an inability to remember everything within a set of information that exceeds their encoding capacity by strategically attending to the most valuable or important units within the set (e.g., Castel, 2008; Castel et al., 2012). Such value-directed remembering reflects an effort to ensure that at least the most important information is retained when all cannot be. Abiding by general models of self-regulated learning (Nelson & Narens, 1990; Winne & Hadwin, 1998) and, specifically, the agenda-based regulation (ABR) model (Dunlosky et al., 2011), the research conducted in the present dissertation expands upon this robust finding of value-directed remembering to examine factors that can arise during encoding and retrieval which have been shown not only to influence the total quantity of information that a learner can encode in a given study session (and thus later remember), but have also been shown to or are

predicted to influence the extent to which a learner will study strategically in light of the limitations these factors can place on the central executive mechanism of working memory.

The results of the present research support prior value-directed remembering research and general models of self-regulated learning in that participants prioritized high-value (i.e., the most goal-relevant) information over low-value information and adapted their study as per task constraints during study and retrieval demands at test. Support for the ABR model across experiments was somewhat more nuanced, as heightened stress to the central executive mechanism from a variety of sources (e.g., inherent learner characteristics; task-related demands) did not consistently impair value-based prioritization during study or retrieval.

Ongoing research is certainly needed to identify moderating factors and to more clearly outline the extent to which working memory resources can be strained without consequence. Nevertheless, the present research establishes that the ability to recognize and strategically offset limits to one's memory during encoding by selectively prioritizing the most important/valuable information can, in some situations, withstand even rather notable stressors.

The dissertation of Catherine Diane Middlebrooks is approved.

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Alan Dan Castel, Committee Chair

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2018

DEDICATION

To my parents, Renee and William Middlebrooks:

With each passing year it becomes clearer and clearer that I have been blessed with the love,

support, and guidance of not only two devoted parents, but of two true friends.

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PUBLICATIONS

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<div align="center">**VITA**</div>

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1. **Middlebrooks, C. D.**, & Castel, A. D. (2018). Self-regulated learning of important information under sequential and simultaneous encoding conditions. *Journal of Experimental Psychology: Learning, Memory, & Cognition, 44,* 779-792. DOI: 10.1037/xlm0000480

2. **Middlebrooks, C. D.**, Kerr, T., & Castel, A. D. (2017). Selectively distracted: Memory for important information under divided attention. *Psychological Science, 28,* 1103-1115. DOI: 1.1177/0956797617702502

3. **Middlebrooks, C. D.**, Murayama, K., & Castel, A. D. (2017). Test expectancy and memory for important information. *Journal of Experimental Psychology: Learning, Memory, & Cognition, 43,* 972-985. DOI: 1.1037/xlm0000360

4. **Middlebrooks, C. D.**, Murayama, K., & Castel, A. D. (2016). The value in rushing: Memory and selectivity when short on time. *Acta Psychologica, 170,* 1-9. DOI: 1.1016/j.actpsy.2016.06.001

5. **Middlebrooks, C. D.**, McGillivray, S., Murayama, K., & Castel, A. D. (2016). Memory for allergies and health foods: How younger and older adults strategically remember critical health information. *Journals of Gerontology, Series B: Psychological Sciences and Social Sciences, 71,* 389-399. DOI: 1.1093/geronb/gbv032 [Selected as "Editor's Choice"]

BOOK CHAPTERS

1. Castel, A. D, **Middlebrooks, C. D.,** & McGillivray, S. (2016). Monitoring memory in old age: Impaired, spared, and aware. In J. Dunlosky & S. Tauber (Eds.), *The Oxford Handbook of Metamemory* (pp. 519-535). Oxford University Press.

SELECTED PRESENTATIONS

1. **Middlebrooks, C. D.** (2018, January). *Self-regulated learning of important information.* Invited talk at Radford University, Radford, VA. January 26.

2. **Middlebrooks, C. D.** (2018, January). *Self-regulated learning of important information.* Invited talk at the University of Texas at Tyler, Tyler, TX. January 22.

3. **Middlebrooks, C. D.,** Kerr, T. K., & Castel, A. D. (2016, Nov). Memory for important information: Test expectancy and variable practice. Poster presented at the *57th Annual Scientific Meeting of the Psychonomic Society*, Boston, MA. Nov 17-2.

4. **Middlebrooks, C. D.,** Kerr, T. K., & Castel, A. D. (2016, Nov). Selectively distracted: Divided attention and memory for important information. Poster presented at the *57th Annual Scientific Meeting of the Psychonomic Society*, Boston, MA. Nov 17-2.

5. **Middlebrooks, C. D.,** Kerr, T. K., & Castel, A. D. (2016, April). The value in rushing: Aging and value-directed remembering when short on time. Poster presented at the *Biennial Cognitive Aging Conference*, Atlanta, GA. April 14-17.

6. **Middlebrooks, C. D.,** Kerr, T. K., Murayama, K., & Castel, A. D. (2015, Nov). Memory and selectivity when short on time: The value in rushing. Poster presented at the *56th Annual Scientific Meeting of the Psychonomic Society*, Chicago, IL. Nov 19-22.

7. **Middlebrooks, C. D.,** Kerr, T. K., Murayama, K., & Castel, A. D. (2014, Nov). You can't take it (all) with you: How goals and subjectivity influence memory for valuable information. Poster presented at the *55th Annual Scientific Meeting of the Psychonomic Society*, Long Beach, CA. Nov 20-23.

8. **Middlebrooks, C. D.,** McGillivray, S., Worden, K., Murayama, K., & Castel, A. D. (2014, April). Aging and value-directed remembering for allergens and health foods. Poster presented at the *Biennial Cognitive Aging Conference*, Atlanta, GA. April 3-6.

CHAPTER ONE:

INTRODUCTION

Whether a student prepping for an exam or a caretaker trying to remember a charge's new allergies, one would ideally remember everything; the probability of this happening, however, is generally slim. We are regularly inundated with information, and oftentimes, some of this information is of greater import than the rest. Faced with an inability to remember everything, it behooves the rememberer to identify which elements are more important than others and prioritize the learning of high-value information over less important subsets. If forgetting *something* is inevitable, it is far better that that "something" be of lesser consequence.

Value-directed remembering research considers the intentional and strategic study of valuable or important information (Castel, 2008; Castel, McGillivray, & Friedman, 2012). When presented with a quantity of information that exceeds encoding capacity, people appear able to selectively focus their attention during study on the most valuable units of information, leading to more successful recall of high-value information than low-value (e.g., Castel, Benjamin, Craik, & Watkins, 2002; Festini, Hartley, Tauber, & Rhodes, 2013; Middlebrooks, McGillivray, Murayama, & Castel, 2016). Under the agenda-based regulation (ABR) model (Ariel, Dunlosky, & Bailey, 2009; Dunlosky & Ariel, 2011a; Dunlosky, Ariel, & Thiede, 2011), value-directed remembering can be regarded as an instance of self-regulated learning in which the learner's study decisions and strategies are primarily motivated by the value of the to-be-remembered information.

With the ABR model in mind, self-regulated learning refers to the intentional, goal-directed choices learners make when studying, whether in a broad sense (e.g., planning how to divide assigned readings throughout the semester) or on more of an item-level basis (e.g.,

spending more time reviewing Problem 2 than Problem 3; rereading Chapter 7 but not Chapter 9) (Bjork, Dunlosky, & Kornell, 2013; Dunlosky et al., 2011; Dunlosky & Ariel, 2011a; Winne & Hadwin, 1998). The ABR model proposes that learners make study decisions based not only on discrepancies between learning states and a study norm (Dunlosky & Thiede, 1998; Mazzoni, Cornoldi, & Marchitelli, 1990; Metcalfe, 2002; Metcalfe & Kornell, 2005), but also on overarching agendas that can supersede lower-level factors (e.g., the difficulty of the information).

Under the ABR model, self-regulated study decisions—like how best to distribute one's available study time across the to-be-remembered information, or whether to study (or restudy) particular units—are made with the intention of maximizing study efficiency in light of adopted agendas and performance goals (Ariel et al., 2009; Dunlosky & Ariel, 2011a). In the case of value-directed remembering, learners adopt a value-based study strategy, prioritizing high-value information over low-value when determining how best to allocate their limited cognitive and attentional resources. A selective study strategy is thus an efficient strategy, in that encoding efforts are directed specifically towards the information that, forgotten, would be the most injurious to one's goals.

The research outlined in the following dissertation is two-pronged. The first line of experiments serves as an effort to understand factors that arise during encoding that may influence a learner's ability to adopt and execute efficient study strategies when attempting to remember information that varies in its value or importance. The encoding factors chosen (viz. the format in which to-be-learned information is organized and presented; limits on study time; and the presence of distractions/divided attention while studying) were motivated by both prior self-regulated learning research and predictions made by the ABR model. The second line of

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this research is a nascent attempt to investigate the role that testing experiences can have on future study events and the probability of engaging in value-directed remembering.

Value-Directed Remembering

Value-directed remembering refers to the ability to selectively attend to, and monitor one's memory for, information based on its value when presented with a set of information large enough to exceed one's total encoding capacity (Castel, 2008; Castel et al., 2012). The primary method by which value-directed remembering has been investigated is via the selectivity paradigm or selectivity task (e.g., Castel et al., 2002; Castel et al., 2012; Castel, Farb, & Craik, 2007; Castel, Humphreys, et al., 2011; Castel, Lee, Humphreys, & Moore, 2011; Watkins & Bloom, 1999). The typical design follows as such: Participants study a sequence of items paired with a continuous range of values (e.g., 1 to 12) and are instructed to remember as many of the items as possible, with the ultimate goal being to maximize their final score, a summation of the values paired with the correctly recalled items. As such, the greater the assigned value of an item, the more important it is to remember because it is more relevant to the overarching goal of maximizing one's score at test. Items are recalled directly after each list presentation and the experimenter provides performance feedback, usually in terms of the participant's total score for that list. This study-test procedure is generally repeated across multiple lists of novel items.

Ideally, a participant would recall the *n*th most valuable items (e.g., if 5 items are recalled, perfect selectivity is achieved if they are the five-most valuable). The smaller the discrepancy between the ideal score and the achieved score relative to chance performance, the more selective the learner (Watkins & Bloom, 1999). Recalling any item correctly will increase one's score, but studying selectively—specifically prioritizing the high-value items—will *maximize* one's score. Over the course of multiple lists, participants realize this and learn to be

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more selective in their study: The total number of items remembered is largely consistent across lists, but the recalled content shifts in favor of high-value items (Castel, 2008; Castel et al., 2012; McGillivray & Castel, 2011). These results have been robustly replicated using both arbitrary item-value pairings (e.g., Ariel & Castel, 2014; Ariel, Price, & Hertzog, 2015; Castel et al., 2002; Castel et al., 2007; Castel, Balota, & McCabe, 2009) and more conceptual pairings (e.g., DeLozier & Rhodes, 2015; Festini et al., 2013; Middlebrooks, McGillivray, et al., 2016).

According to the "metacognition modifying attention" hypothesis (Castel et al., 2012), recognizing that one is incapable of remembering *all* of the information (valuable or otherwise) via monitoring mechanisms can encourage the "efficient allocation of attention toward important information at the expense of [low-value] information" (p. 246). In other words, monitoring one's insights as to the compatibility between one's possible performance, one's ideal performance, and one's necessary performance (i.e., remembering the critical information) will lead to changes in metacognitive control, expressed via changes in the learner's allocation of attention during study. In line with this hypothesis, the critical role of task experience has been repeatedly demonstrated in value-directed remembering research: Although there is typically little evidence of selectivity in recall after the first list of study, selectivity consistently improves across lists, with the composition of recalled items increasingly the more valuable of the complete studied set (Castel, 2008; Castel et al., 2012).

The adjustment of attention allocation during study as the primary mechanism of control during value-directed remembering (at least within tasks utilizing a typical selectivity paradigm) is supported by research featuring participants experiencing abnormal attention deficits. One study found that children exhibiting attention deficit/hyperactivity disorder (ADHD) were significantly less selective than healthy control children despite recalling just as many items

overall (Castel, Lee, et al., 2011). Critically, the children with ADHD remembered more high-value than low-value words, but the composition of their recall was less value-based than that of those without ADHD. The attention deficits inherent to ADHD did not seem to impair the children's metacognitive monitoring, given that they successfully adopted a value-based strategy. Rather, the children with ADHD were less able than the healthy controls to execute the strategy, signifying problems with their control mechanisms. Similar results have also been reported in older adult patients with Alzheimer's disease relative to healthy younger and older adults (Castel et al., 2009).[1]

Self-Regulated Learning and the Agenda-Based Regulation Model

Self-regulated learning research—as it pertains to individual study sessions—has largely investigated the overt choices learners make during study, with item choices during study reflected in two primary ways: via greater allocation of study time to some information at the expense of other information; and by explicitly selecting subunits to restudy, thereby neglecting the remainder of the to-be-learned information. For the most part, investigations into these choices have focused on item difficulty as a principal factor in motivating study time allocation and item selection decisions. Generally speaking, the more difficult the information is to learn, the more time participants spend studying it and the more likely they are to restudy it when given the opportunity (Cull & Zechmeister, 1994; Dunlosky & Connor, 1997; Le Ny, Denhiere, & Taillanter, 1972; Mazzoni et al., 1990; Nelson, Dunlosky, Graf, & Narens, 1994; Nelson &

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[1] Evidence suggests that the presence of value during study also activates dopaminergic circuitries in the midbrain and striatum that selectively enhance memory consolidation for rewarding or valuable information (Adcock et al., 2006; Murayama & Kitagami, 2014; Shohamy & Adcock, 2010), irrespective of intentional study (Wittmann et al., 2005). Notably, however, these dopaminergic-based effects on memory have only been reported at a delay. As value impacts *are* evident in other immediate testing situations, there must be at least another factor aside from dopaminergic reward circuitry involved, and value-directed remembering results are consistent with that factor being the strategic allocation of attention.

Leonesio, 1988; Son & Metcalfe, 2000; Thiede & Dunlosky, 1999; Thiede, Anderson, & Therriault, 2003).

This prioritization of difficult items over easy items does not, however, hold true in all circumstances. Thiede and Dunlosky reported an adaptive "shift-to-easier-materials" (STEM) effect, such that learners who are assigned a low performance goal (e.g., remember 6 of 30 noun pairs) will choose to restudy easier materials, while learners assigned a more difficult performance goal (e.g., remember 24 of 30 pairs) will continue to restudy difficult material (Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999). This STEM effect has also been reported under conditions of constrained study time (Metcalfe, 2002; Son & Metcalfe, 2000; Thiede & Dunlosky, 1999). Learners have also been shown to prioritize other item-based characteristics over item difficulty, like the probability of an item being later tested or its value (Ariel et al., 2009; Dunlosky & Ariel, 2011b). For instance, participants were more likely to choose to restudy items that had a 90% chance of appearing on a later test than those with a 30% chance, regardless of whether those items were easy or difficult to learn (Ariel et al., 2009). Thus, learners appear to base their study not only on metacognitive judgments of item-based characteristics (like difficulty), but also consider the broader circumstances of the task and its demands.

Ascertaining the impact of goals and task constraints on single-session self-regulated learning behaviors has been critical to the formation of more complete and accurate theories of self-regulated learning. Early models of (single-session) self-regulated learning predominantly considered situations in which information could be prioritized based on its level of difficulty, but not all to-be-remembered information will necessarily vary in its difficulty; moreover, difficulty may not always be the primary factor in motivating study decisions. The recently

developed agenda-based regulation (ABR) model is premised on the idea that learners construct agendas to guide their study and that, generally speaking, they strive to achieve their learning goals as efficiently as possible (Dunlosky et al., 2011; Dunlosky & Ariel, 2011a). Constructed agendas outline the various task-related factors (e.g., performance goals, time limitations, item importance) and personal factors (e.g., level of interest, motivation, self-efficacy) that will guide self-regulated learning during study, the factors on which learners should base their study choices (Dunlosky et al., 2011).

The central executive of working memory is thought to be responsible for constructing the agenda, continually monitoring the learning environment for task-relevant information, and, ideally, inhibiting task-irrelevant information (Dunlosky et al., 2011; Dunlosky & Ariel, 2011a). The central executive is also responsible for keeping the learner's agenda activated and for abiding by this agenda when choosing which information to study. At any point during study, this agenda may be modified and/or replaced entirely as per monitoring feedback regarding its efficacy and ease of execution. Importantly, the ABR model does not presume that learners always exhibit optimal self-regulation. According to Dunlosky and Ariel (2011a), learners are "expected to underachieve when they (a) fail to set goals, (b), do not remember their goals, (c) do not develop plans to achieve their goals, (d) develop ineffective plans, or (e) do not execute their effective plans properly" (p. 107). Furthermore, the critical role played by the central executive in the ABR model explains cases of sub-optimal self-regulation: In instances of limited working memory spans, multiple task constraints, or external distractions, the learner may not have adequate resources remaining to form the agenda, let alone to keep it active in working memory and to enact consistent study behaviors (Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999).

Value-Directed Remembering under the Agenda-Based Regulation Model

Under the ABR model, value-directed remembering represents a specific instance of self-regulated learning in which study is guided by an overarching value-based agenda. That self-regulated learning literature seems to exclusively discuss observable controls does not preclude the exclusive use of unobservable control mechanisms, like that of attention allocation in value-directed remembering. After all, observable mechanisms of control, like item selection and termination of study, are manifestations of attention allocation; in studying Item A over Item B, the learner has ultimately chosen to allocate more attention to A than B. Unlike other self-regulated learning tasks, however, this allocation of attention remains unobservable in typical value-directed remembering tasks, as the learner is allowed no way of otherwise visibly expressing this decision. When observable demonstrations of metacognitive control *are* allowed in value-directed remembering tasks, however, selectivity continues to be evident (Ariel et al., 2015; Castel, Murayama, Friedman, McGillivray, & Link, 2013; Middlebrooks & Castel, 2018; Middlebrooks, Murayama, et al., 2016), and, most importantly, these observable study choices are executed in a manner consistent with the hypothesized value-based differences in attention allocation.

In understanding the ways in which value-directed remembering is—or is not—consistent with previous research concerning self-regulated learning and, specifically, the ABR model, it is necessary to clarify whether factors which have either been shown to be of notable influence to self-regulated study as per a difficulty-based study agenda or are predicted by the ABR model to be of influence to value-directed remembering (i.e., self-regulated study as per a value-based study agenda). The following proposed experiments are designed to address four such factors: the format in which the information is presented during study (whether sequential or

simultaneous; Chapter 2); the presence of time constraints during study (Chapter 3); the effect of reducing or dividing attention during study (Chapter 4); and expectations regarding the nature of the upcoming memory test and its associated demands (Chapter 5). In investigating each of these overarching factors, consideration across experiments is given to the scope of the learner's overall encoding capacity (i.e., the amount of information the learner can ultimately recollect); the source of encoding capacity limitations (e.g., task-related limitations versus limitations inherent to the learner); the extent to which the learner must overtly self-regulate his/her study (e.g., experimenter-paced study versus self-paced study); and the degree to which the learner's central executive of working memory is stressed.

CHAPTER TWO:

PRESENTATION FORMAT DURING ENCODING

on VALUE-DIRECTED REMEMBERING

Prior work concerning difficulty-based self-regulated study indicates that the format in

which to-be-remembered information is presented—namely, whether all at once (i.e.,

simultaneously) or single to-be-remembered units at a time (i.e., sequentially)—can notably

impact later memory and the efficiency of study behaviors. Generally speaking, learners tend to

prioritize difficult items over easier ones, allocating more time during study to the difficult

subset and electing to restudy them more often than easier items (Dunlosky & Connor, 1997; Le

Ny et al., 1972; Mazzoni, Cornoldi, & Marchitelli, 1990; Nelson & Leonesio, 1988; Thiede &

Dunlosky, 1999). There are, however, circumstances in which learners will appropriately shift

their prioritization to easier items, such as when assigned a low performance goal (Thiede &

Dunlosky, 1999) or under conditions of limited study time (Dunlosky & Thiede, 2004; Metcalfe,

2002; Son & Metcalfe, 2000; Thiede & Dunlosky, 1999). Thus, in situations in which studying

difficult items over easy items would be an unnecessary or likely unsuccessful expenditure of

resources, learners adjust their study agenda in order to maintain study efficiency. When the to-be-remembered items are presented sequentially, however, learners display a detrimental and inefficient shift back to prioritizing more difficult items (Ariel, Dunlosky, & Bailey, 2009; Thiede & Dunlosky, 1999), even in cases in which the learner had previously demonstrated more optimal self-regulated study of similar materials (Dunlosky & Thiede, 2004).

The proposed explanation for such suboptimal regulation under sequential study is that sequential formats require learners to keep not only their study agenda in mind, but to also keep in mind previously studied items, upcoming items (e.g., How many are left?), their relevant characteristics (e.g., difficulty, importance), *and* previous study behaviors when determining how best to study the single item being presented at that moment (Ariel et al., 2009; Dunlosky & Ariel, 2011a; Thiede & Dunlosky, 1999). Simultaneous formatting, on the other hand, is thought to place less of a demand on the learner's central executive, as much of the agenda-relevant information is continually visible and so does not need to be held in working memory (or is at least held to a lesser extent). This theory is supported by findings that learners with high working memory spans are less influenced by presentation format than those with lower spans (Ariel et al., 2009; Dunlosky & Thiede, 2004).

Evidence that sequential formatting impairs one's ability to efficiently execute optimal study agendas, though, would seem to conflict with literature suggesting intact prioritization of high-value information over low-value information during study despite sequential presentation. When tasked with remembering a set of items that exceeds one's memory capacity, learners have been shown to shift their attention and encoding efforts toward the more valuable subset so as to increase the probability that at least the most important items are remembered if all cannot be (e.g., Castel, Benjamin, Craik, & Watkins, 2002; Castel, Farb, & Craik, 2007). Moreover,

learners become increasingly selective with continued task experience, recalling not only high-value items more often than low-value items, but strategically shifting their recall in favor of the highest values and away from the lowest (Castel, 2008; Castel, McGillivray, & Friedman, 2012). Such intentional prioritization on the basis of item value/importance has been termed value-directed remembering and, despite sequential formatting, has been consistently reported following both tasks in which prioritization is a consequence of unobservable shifts in attention (e.g., Ariel & Castel, 2014; Castel et al., 2002; DeLozier & Rhodes, 2015) and tasks in which learners have more direct control over study pacing and presentation (Middlebrooks, Murayama, & Castel, 2016; Robison & Unsworth, 2017).

One potential reason for this discrepancy may be that learners can learn to adopt and execute appropriate value-based study agendas under both presentation formats, but simply do so more effectively when the information is studied simultaneously. A direct comparison of sequential to simultaneous value-based study has not yet been conducted, but recent work suggests that individuals with lower working memory capacities are less likely to spontaneously adopt effective, value-based study strategies than those with greater working memory (Robison & Unsworth, 2017). Sequential formatting, relative to simultaneous formatting, may similarly stress the central executive and impair participants' ability to adopt and execute an optimally strategic agenda.

Another explanation concerns the "learn to adopt" aspect of value-directed remembering. Participants generally require task experience before demonstrating successful execution of a selective, value-based study strategy (Castel, 2008). Although participants are generally as likely to recall a low-value word after the first study block as a high-value word, the effect of value on participants' attention allocation during study and subsequent re- call markedly increases with

continued feedback across multiple study-test blocks (with each block consisting of novel items). If only the first study-test block were considered, it would certainly appear as though learners are *not* capable of prioritizing high-value information during study. It may be the same situation in other cases of agenda-based study, like that of easy and difficult information: Perhaps learners would appropriately prioritize easy items over difficult items in light of time constraints or low performance goals, in spite of sequential formatting and in a manner comparable with simultaneous formatting, with continued task experience. Research to date has predominantly considered single trials when comparing presentation format effects (e.g., Ariel et al., 2009; Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999), yet the importance of task experience and feedback is widely acknowledged as critical to the adoption, modification, and improved execution of study strategies (Ariel, 2013; Broekkamp & Van Hout-Wolters, 2007; Nelson & Narens, 1990; Winne & Hadwin, 1998).

It may also be, however, that the demands of maintaining and executing a value-based study agenda are sufficiently low that additional stressors to the central executive, like the demands of sequential formatting, are less consequential to efficient study. When studying easy and difficult items, for instance, the dominant study behavior appears to be to devote more time to the information which is further from one's norm of study (Dunlosky & Hertzog, 1998; Le Ny et al., 1972; Metcalfe & Kornell, 2005; Thiede & Dunlosky, 1999), and it is only in certain circumstances, like constrained study time, that easy items take priority. Perhaps learners must override a more habitual response behavior in these cases, which in itself should be more executively taxing (Hasher, Lustig, & Zacks, 2007; Redick, Calvo, Gay, & Engle, 2011), and the costs of doing so to self-regulated study (Ariel, Al-Harthy, Was, & Dunlosky, 2011; Ariel & Dunlosky, 2013; Dunlosky & Ariel, 2011b) may be amplified by the more executively taxing

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nature of sequential formatting. In the case of low- and high-value information, however, it would make little sense to prioritize less important information over more important information (at least in the absence of other pertinent characteristics). There is not another reasonable, competing study agenda against which learners must choose, and so selecting and maintaining a value-based agenda may be simpler and less cognitively demanding than that of other study agendas.

The following experiments serve as an effort to understand why value-based study is consistently found following sequential study, despite prior findings that suggest such efforts should markedly suffer. Experiment 1 directly contrasts sequential and simultaneous formatting using an experimenter-paced task in which learners have no control over item presentation and any value-based study can be based only on shifts in attention. Experiment 2 also contrasts sequential and simultaneous formatting, but participants in this experiment chose how to distribute their allotted study time across items and were given opportunities for restudy.

Experiment 1

The aim of Experiment 1 was to directly compare sequential to simultaneous value-based study (Castel, 2008; Castel, Murayama, Friedman, McGillivray, & Link, 2013; Robison & Unsworth, 2017; Watkins & Bloom, 1999). It may be that learners can learn to adopt and execute a value-based agenda under either format, as evidenced by prior value-directed remembering research utilizing sequential formatting. It may also be, however, that learners are ultimately more selective when the valuable information is presented for study simultaneously, consistent with research concerning the impact that presentation format can have on the adoption and execution of optimal study agendas (Ariel et al., 2009; Dunlosky & Ariel, 2011a; Thiede & Dunlosky, 1999).

Method

Participants

Participants consisted of 48 undergraduate students (37 female) from the University of California, Los Angeles, ranging in age from 17 to 27 years ($M = 19.96$, $SD = 1.60$). Participants received partial credit for a course requirement.

Materials

The study was designed and presented to participants via the Collector program (Garcia, Kerr, Blake, & Haffey, 2015). Stimuli consisted of six lists containing 20 novel words apiece. Each of the words were randomly assigned a value ranging from 1 to 10 points, with two words assigned to each value per list. The words in each list were randomly selected without replacement from a larger bank of 665 nouns, verbs, and adjectives. Within the larger bank, word length ranged from four to six letters and averaged to 7.52 (SD 1.02) on the log-transformed Hyperspace Analogue to Language (HAL) frequency scale. The 120 studied words were randomly selected from this bank for each participant so as to avoid any potential item effects (Murayama, Sakaki, Yan, & Smith, 2014).

Procedure

All participants were told that they would be studying a series of lists, each containing 20 different words that would range in value from 1 point to 10 points with two words per point value in each list. They were instructed to remember as many of the words in each list as possible while also aiming to achieve a maximal score, a sum of the points associated with each word correctly recalled at the end of the respective list's presentation. After recalling the words from a given list, participants were provided feedback about their performance in terms of the

number of points they had earned out of 110 possible points before studying the next list. This study-recall-feedback procedure was completed for each of the six word lists.

Participants were randomly assigned to either the simultaneous study condition or the sequential study condition. Participants in the sequential condition were shown each of the word-value pairings within a given list one at a time for 3 seconds each. Participants in the simultaneous condition were shown all 20 of the words and their values within a given list for a total of 60 seconds in a 5 x 4 array (see Figure 2.1). The order of presentation in the sequential condition, and the arrangement of the items in the simultaneous presentation, was randomized for each participant.

RUNG : 10	APATHY : 10	SPREE : 1	RUDDER : 5
BRIEF : 3	RATION : 8	CINDER : 4	KNOB : 3
DEGREE : 6	COPPER : 9	ENVY : 7	RUSH : 7
TRUNK : 2	DOME : 2	BOIL : 1	QUEST : 9
MITE : 4	SHORT : 5	VESSEL : 6	GUEST : 8

(a)   (b)
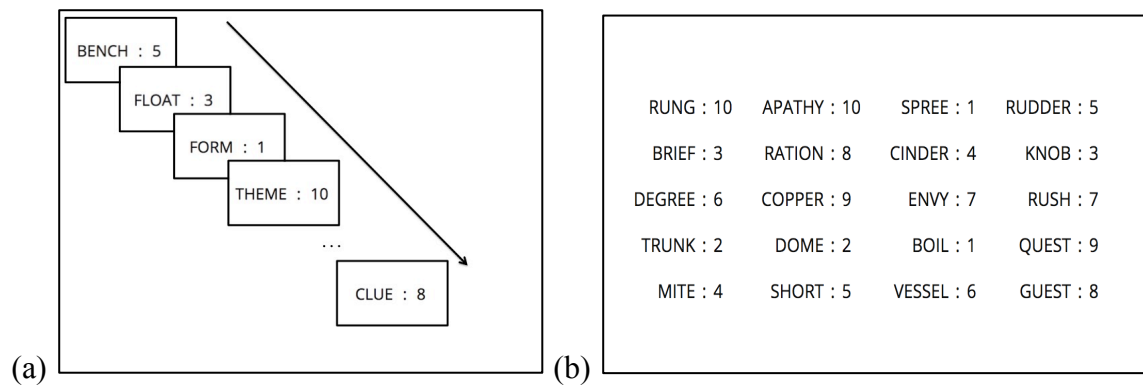
Figure 2.1. An example of the (a) sequential and (b) simultaneous presentation of the 20 items within a list during study in Experiment 1. The items within a list were presented each for 3 seconds in the sequential condition or all at once for 60 seconds in the simultaneous condition before participants progressed to the recall test, after which they were told their score (the sum of the points associated with correctly recalled items).

Results

Overall Recall Performance

The proportions of items correctly recalled across the six lists are provided in Table 2.1. Initial analyses were conducted to determine whether the presentation format affected the total number of items that participants recalled, irrespective of their item values. A 2(Condition: sequential, simultaneous) x 6(List) repeated-measures ANOVA revealed a main effect of condition, $F(1,46) = 12.72$, $MSE = 0.05$, $\eta^2_G = .12$, $p < .001$, such that participants in the

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simultaneous condition recalled more items overall ($M = .46$, $SD = .09$) than participants in the sequential condition ($M = .36$, $SD = .09$).

There was also a main effect of list, $F(5, 230) = 2.55$, $MSE = 0.01$, $\eta^2_G = .03$, $p = .03$. A post hoc trend analysis suggested a small (Bakeman, 2005) quadratic trend in the total number of items recalled across lists, $F(1, 47) = 3.93$, $MSE = 0.01$, $\eta^2_G = .08$, $p = .05$. Although speculative, these changes in overall recall may reflect some combination of general acclimation to the experimental task demands and minor interference owing to continued study of and exposure to novel items.

Value-Directed Remembering and Selectivity

Recall performance as per item value and study condition throughout the task is presented in Figure 2.2.
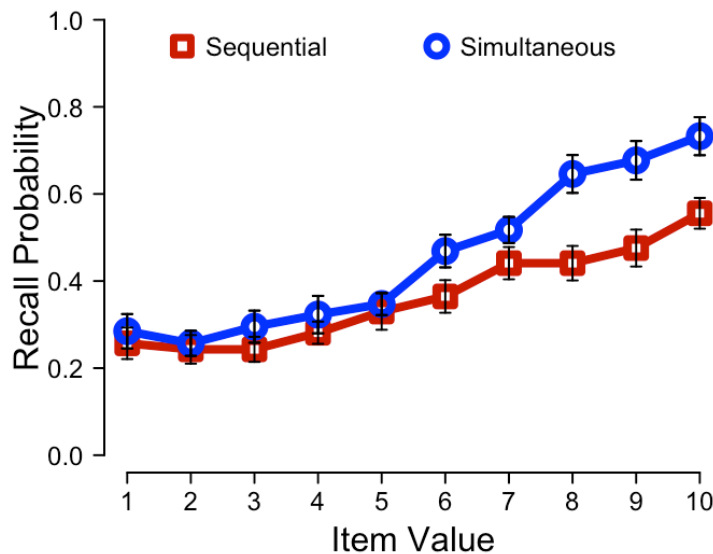
Figure 2.2. Recall probability, averaged across lists, as per item value and study condition in Experiment 1. Error bars reflect standard error.

Hierarchical linear modeling (HLM) was used to analyze recall as a function of an item's value, the list in which it was presented, and the format in which it was studied (Raudenbush & Bryk, 2002) in order to detect the use of value- directed remembering in studying the presented items

and also to determine whether there were differences in attention to value (i.e., selectivity) based on whether the to-be-remembered information was presented sequentially or simultaneously.

HLM was used for two primary reasons. First, HLM analyses maintain the continuous nature of the item values as they were studied, rather than treating each value point as a discrete entity (as would be the case in, for instance, an analysis of variance). By maintaining the continuity of the value spectrum, it is possible to identify whether there is a direct relationship between value and recall and, thus, to detect value-based study strategizing. More- over, it is possible to examine whether such a value-recall relationship changes with continued task experience, as has been previously documented (Castel, 2008; Castel et al., 2013; Middlebrooks, Kerr, et al., 2017; Middlebrooks, Murayama, et al., 2017). Second, and of particular importance, individuals likely differ in *how* they attend to value during study. A participant with high performance expectations, for example, might attend to items worth 5 or more points, while a participant with low expectations might limit attention to the few items worth 9 or 10 points. Both examples illustrate value-based study strategies devised as per metacognitive judgments of personal memory capacity and likely performance. Such between-subjects variation in strategy application would be lost in simply comparing average recall across discrete value points. HLM accounts for within- and between-subjects differences in strategy by first clustering the data within each participant and *then* considering potential study condition differences in value-directed remembering and selectivity, all while maintaining the continuous nature of the value scale as it was used in the task.

Item-level recall performance (based on a Bernoulli distribution, with 0 = *not recalled* and 1 = *recalled*; Level 1 = items; Level 2 = participants) was modeled as a function of each item's value, the list in which it was presented, and the interaction between value and list. Value

and list were entered as group-mean centered variables, such that value was anchored on the mean value point (5.5) and list on the mean list (3.5). The model further included the study conditions as a Level-2 predictor of those Level-1 effects (0 = *sequential* and 1 = *simultaneous*). Table 2.2 reports the tested model and its estimated regression coefficients. As the model is effectively a logistic regression model with a dichotomous dependent variable, the regression coefficients can be interpreted via their exponential (Raudenbush & Bryk, 2002). Specifically, exponential beta, e^B, is interpreted as the effect of the independent variable on the odds ratio of successful recall (i.e., the probability of recalling items divided by the probability of forgetting them; Murayama et al., 2014). e^B of more than 1.0 indicates a positive effect of the predictor and less than 1.0 a diminished effect.

Value positively predicted recall performance in the sequential condition ($\beta_{10} = 0.18$, $p < .001$), and there was a positive interaction between value and condition ($\beta_{11} = 0.11$, $p = .06$), indicating that the effect of value was greater when the items were presented simultaneously during study than sequentially. Thus, the odds of successfully recalling an item increased with increasing value. Participants in the sequential condition were $e^{0.18} = 1.19$ times more likely to recall an item for each one-unit increase in value than they were to forget it, while participants in the simultaneous condition were $e^{0.28} = 1.33$ times more likely.[2] In other words, the odds of participants in the sequential condition recalling a 10-point word were $e^{0.18*10} = 6.05$ times greater than the odds of their recalling a 1-point word, but the odds of participants in the simultaneous condition recalling a 10-point word were $e^{0.28*10} = 16.44$ times greater than the odds of their recalling a 1-point word.

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[2] The simple slope for the simultaneous condition can be directly calculated by adding the β_{10} and β_{11} coefficients (i.e., $0.176 + 0.108 + 0.284$). To determine whether this simple slope is statistically significant, the condition predictor in the model was recoded, such that 0 = *simultaneous* and 1 = *sequential* (Hayes, 2013). Note that this was also done to determine the significance of any reported simple slopes hereon.

There was also a small but statistically detectable List Value interaction ($\beta_{31} = 0.03$, $p = .03$), such that participants became more selective with continued task experience, increasingly prioritizing high-value items over low-value items across lists, with no detectable differences as a function of presentation format during study ($p = .72$).

Value-Based Organization During Encoding and Retrieval

Participants in Experiment 1 could control nothing about the nature of the study presentation—they could not, for instance, self-pace their study or refrain from viewing less agenda-relevant items. Participants in the simultaneous study condition appeared to more optimally allocate their attention in a value-based manner than those in the sequential condition, but how or why they were able to do so remains unclear.

One possibility is that the simultaneous formatting better lends itself to associative encoding across the items, and participants in the simultaneous condition were thus better able to associate the most important items. For instance, a participant studying simultaneously could more quickly form an image or sentence associating the 9- and 10-point items—thereby increasing the chance of later recalling those items—than a participant who studied sequentially and may have had to study (or at least view) several lower- valued items before being presented with the next high-value item. In this case, value differences might be evident not just across the particular subset of items recalled, but in the *order* in which those items were recalled and the extent to which the items were recalled in value-based clusters.

In an initial analysis, a Pearson correlation was independently calculated for each participant between the value of each recalled item and the order in which it was recalled within each of the six lists. A 2(Condition) x 6(List) repeated-measures ANOVA was then conducted on these correlations, revealing a main effect of condition, $F(1,46) = 1.99$, $MSE = 0.34$, $\eta^2_G = .06$, p

= .002, such that the correlation between value and output order was detectably stronger in the simultaneous condition (M = -.31, SD = .25) than in the sequential condition (M = -.08, SD = .22); the correlation between item value and output order was not detectably different from zero in the sequential condition (p = .08). So, of the items remembered, participants in the simultaneous condition were more likely to recall the higher-valued items first, whereas participants in the sequential condition demonstrated no such tendency.

To further explore the nature of the item value-output order relationship and potential differences that may have arisen between the two conditions, each recalled item was placed into one of three value categories. A recalled item worth 1–3 points was categorized as low-value; 4–7 points as medium-value; and 8–10 points as high-value. Once categorized, an adjusted-ratio-of-clustering (ARC) score and a modified ratio of repetition (MMR) score were calculated for each participant per list (Roenker, Thompson, & Brown, 1971; Senkova & Otani, 2012).[3] Separate 2(Condition) x 6(List) repeated-measures ANOVAs were then conducted on each the ARC scores and the MMR scores. In neither analysis were there detectable differences in scores owing to condition and/or list (ps > .08), suggesting that differences did not arise between conditions with respect to any value-based clustering as per the post hoc value categorizations.

Discussion

Participants who studied under sequential formatting in Experiment 1 not only recalled fewer items overall than those who studied simultaneously, but were also significantly less selective in their study. Though there was an effect of value in both formatting conditions—consistent with prior research (Castel, 2008; Castel et al., 2013; Hayes et al., 2013;

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[3] There is debate in the literature as to which of the multiple calculations available for determining categorical clustering in recall is appropriate in various circumstances, with each measure influenced by various "irrelevant characteristics of recall" (Roenker et al., 1971). As this was an exploratory analysis, the results of which would need to be interpreted with caution regardless of the outcome, it seemed prudent to consider multiple measures.

Middlebrooks, Murayama, et al., 2016; Robison & Unsworth, 2017)—value had less impact on the likelihood of an item being later recalled when it was studied sequentially than simultaneously. Moreover, this difference was maintained across lists, despite evidence of improved selectivity with task experience: It was not the case that sequential formatting demands relative to simultaneous study were surmounted with continued practice. Thus, reported differences in the efficiency with which information is studied owing to presentation format (Ariel et al., 2009; Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999) do not appear to be simply a consequence of limited experience with the task.

In addition to differences between conditions in the value-based composition of participants' recall, there were further differences with respect to the order in which the items were recalled: Participants in the simultaneous condition were more likely to recall higher-valued items before recalling lower-valued items, but participants in the sequential condition demonstrated no such tendency. This is consistent with the supposition that there were fundamental, format-driven differences between how participants approached and organized the to-be-remembered information. Both groups of participants prioritized high-value information to some extent, but only in the simultaneous condition did value also influence output order. Thus, the manner in which participants allocated their attention when executing their agendas—again, in the absence of any overt control of item presentation—appears to have differed as per presentation formatting.

Exploratory analyses did not definitively reveal differences between the conditions in value-based clustering of the items during recall that could have reflected particular associative or organizational encoding tendencies. In light of the continuous nature of the value spectrum, however, and the fact that the value-based categorical partitions were entirely post hoc—such

partitioning was never suggested to participants during their study—it is not possible to state conclusively that participants did or did not cluster by value in their study and recall in the current experiment.[4] Nevertheless, the correlation between item value and output order during recall in the simultaneous condition, but not the sequential condition, does suggest that further consideration should be given to the extent to which encoding behaviors differ owing to format-based limitations to executive resources and intentional, strategic approaches.

Experiment 2

The general effect of value in Experiment 1, irrespective of condition, indicates that participants recognized the wisdom of prioritizing study based on value, even if studying sequentially minimized the extent of such prioritization. When value-directed remembering can be executed via specific, overt behaviors—like choosing whether to study an item at all—rather than being fully reliant on internal attention control mechanisms, though, sequential formatting may be less detrimental. The opportunity to devise and exercise a specific strategy via self-pacing and (re)study selections might allow participants in a sequential study condition to compensate for any strain of the formatting itself.

Second, it is presently unclear what effect formatting may have on the relationships between item value, self-regulated learning choices, and subsequent recall. When given the opportunity, learners generally elect to study high-value words longer than low-value words whether studying sequentially (Middlebrooks, Murayama, et al., 2016; Robison & Unsworth, 2017) or simultaneously (Ariel, Price, & Hertzog, 2015; Castel et al., 2013). Without directly

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[4] Consider two participants who both recalled a 6-point word, followed by 10-point, 8-point, and 3-point words. If Participant A considers anything worth more than 5 points to be of high-value and worthy of attention during study, then the first three recalled items appear clustered. If Participant B limits high-value classification to only 9- or 10-point items, then there is no evidence of clustering at all. Furthermore, neither Participant A's nor Participant B's partitions would have aligned with the categorizations used in these analyses.

contrasting the two formats, however, it has yet to be determined whether self-regulated choices during sequential and simultaneous study are *similarly* driven by item value. The correlation between output order and item value in only the simultaneous condition in Experiment 1 suggests formatting-driven differences in organizational approaches to study and retrieval, but these differences may have arisen because participants had no other means of regulating their study and so were subject to inherent characteristics of the formatting (e.g., simultaneous arrangement might be more inherently conducive to associative encoding). Experiment 2 aims to disentangle the role of item importance/value from self-regulated study behaviors on learning and memory during sequential versus simultaneous study.

Method

Participants

Participants consisted of 48 undergraduate students (35 female, 1 unreported) from the University of California, Los Angeles, ranging in age from 18 to 26 years ($M = 19.98$, $SD = 1.69$). Participants received partial credit for a course requirement.

Materials & Procedure

The materials and procedure of Experiment 2 were identical to Experiment 1, except that study was self-regulated with respect to self-pacing and selecting items for both study and restudy.

In the sequential condition, participants were presented with each of the item values within a list individually. The participant could click on the presented value in order to study the associated item or elect not to study the item at all by simply pressing the spacebar to progress to the next value (see Figure 2.3a). Although participants were presented with all 20 values within a list at least once, they could advance as quickly as they liked past an item if they elected not to

study it at all. Participants were also given the opportunity to select an item for later restudy while it was on the screen (Figure 2.3a). Items were not seen again if not selected for restudy during the initial presentation. If an item was selected, it was presented again after the participant had advanced through all 20 of the items/values. Participants were allowed to restudy an item as many times as they liked within the 60-second allotment; if selected for an additional restudy, that item was presented in the next presentation cycle within that list's 60-second study period.

During the initial list presentation, for example, a participant might select *bench*, *float*, *injury*, and *theme* to restudy. These items would then be presented again after the participant had first advanced through all 20 of the list's items (regardless of whether the participant studied any of the other 16 items or simply pressed the spacebar to advance past them). If during the restudy phase the participant decided to select *bench* and *float* for a second restudy, these two items would be presented at the conclusion of the first restudy phase (i.e., after the participant had advanced past *injury* and *theme*, as well). Restudy selections in the sequential condition could only be made while the item-value pair was on the screen.

In the simultaneous condition, participants also saw only the values of the items and were to specifically click on the value in order to study the associated word. As in the sequential condition, participants were able to spend as much or as little of the 60-second allotment on the individual words. Participants only studied one word at a time, but all value points were presented in four columns of five words (see Figure 2.3b); clicking on a different value to study a new word returned the previously studied word to its original hidden state. There was no limit to the number of times a participant could study a particular item-value pairing.

In both conditions, a timer was provided on the screen to indicate how much of the 60-second study period remained.
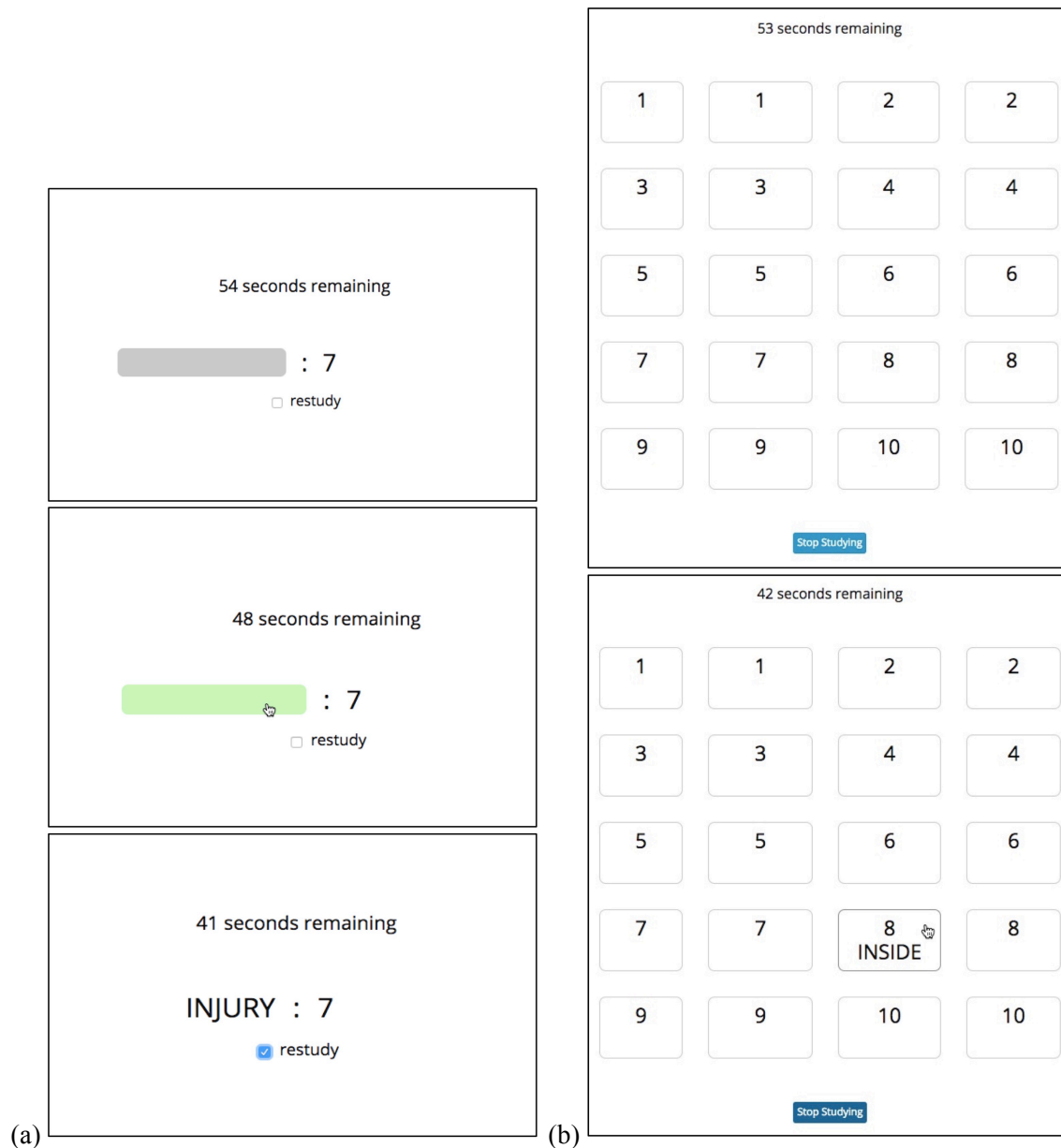
Figure 2.3. An example of the (a) sequential and (b) simultaneous displays during study in Experiment 2. Participants were to click on a given value point to see the associated word. They could study only one word at a time, but were free to study as many or as few items as they wished as often as they liked. A timer was provided at the top of the screen to indicate how many of the 60 seconds remained for studying the 20 items in the list. They could also elect to stop studying the list before the 60-second study period had finished if they were so inclined. Participants completed a recall test after studying each list, after which they were told their score (the sum of the points associated with correctly recalled items).

Results

General Study and Recall

A summary of statistics reflecting general study behaviors (e.g., self-pacing, item selections) is provided in Table 2.3 with statistically detectable differences between conditions indicated. The proportion of items correctly recalled (of all 20 items within a list, irrespective of study behaviors) and provisionally recalled (i.e., recall performance provided that the item was studied at all) across the six lists is provided in Table 2.1.

Value-Directed Remembering and Selectivity

As in Experiment 1, HLM analyses were used to analyze recall in Experiment 2 as a function of item value, the list in which the item was presented, and the format in which it was studied. Table 2.2 reports the tested model (which was the same as that used in Experiment 1) and its estimated regression coefficients. In Experiment 2, however, both recall of the full set of items and provisional recall was considered. Certainly, a participant's failure to recall an item that he or she never studied in the first place is a given and not an indication of forgetting. In analyzing provisional recall, it is possible to assess whether participants selectively prioritized the most valuable items within the subset of items that they elected to study, whether those selections differed as a consequence of presentation formatting, and how their study of this selected subset may (or may not) have deviated between conditions.

Recall of the list in its entirety, though, regardless of study behaviors, must also be considered because the goal of the task was to maximize one's recall of the complete, 20-item set of to-be-remembered material in each list. A perfectly selective participant would choose only the most valuable items to study—for instance, the six items worth 8-10 points—and subsequently recall (if not all of those studied items) only the most valuable of that most valuable

subset (e.g., the 9- and 10-point items). If a participant selected the six items worth 1-3 points to study and later recalled the four most valuable of that relatively unimportant subset, provisional recall would still suggest perfect selectivity. To an extent, selectivity would indeed have been evident in this case, but that participant ultimately chose to prioritize 3-point items over (the unstudied) 10-point items and, in light of the larger set of to-be-remembered information, was rather unselective with respect to high-value items. Analyzing recall (of the full set of items) and provisional recall separately when determining the impact of value makes it possible to distinguish between situations such as these. Figure 2.4 depicts recall performance as per item value and study condition throughout the task—regardless of study behavior—and recall performance provisional on having actually studied the item.
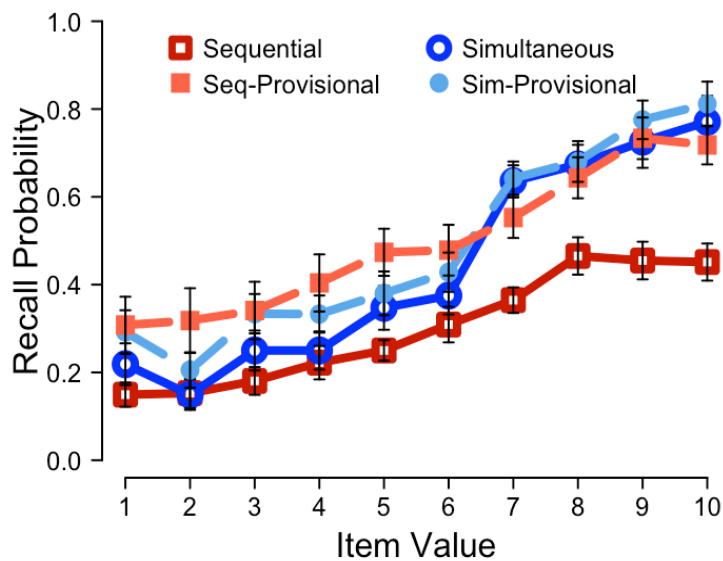
Figure 2.4. Recall probability, averaged across lists, as per item value and assigned study condition in Experiment 2. "Sequential" and "Simultaneous" refer to the probability of recalling all items (studied and not studied) within a list. "Seq-Provisional" and "Sim-Provisional" refer to the probability of recalling an item provided that the participant chose to study it in the first place. Error bars reflect standard error.

Value positively predicted provisional recall performance ($\beta_{10} = 0.31$, $p < .001$), with no statistically detectable differences between the simultaneous and sequential conditions ($\beta_{11} = 0.09$, $p = .37$). There was also a List x Value interaction ($\beta_{30} = 0.06$, $p = .002$), such that

participants became more selective with continued task experience, increasingly prioritizing high-value items over low-value items across lists, again consistent between conditions ($p = .54$).

When considering recall of the full set of items, however, the simultaneous condition was significantly more selective ($\beta = 0.50$) than the sequential condition ($\beta_{10} = 0.26$), $p = .03$, as in Experiment 1. So, although participants who studied sequentially in Experiment 2 recalled the more valuable items of those that they had chosen to study—in a manner similar to that of the simultaneous condition—they were notably less selective when considering the complete set of to-be-remembered items. There was evidence of attention to value during study in the sequential condition, but study itself was relatively suboptimal compared with the simultaneous condition.

Mediation Analysis of Self-Regulated Learning

As in Experiment 1, the HLM analyses for Experiment 2 indicate that participants who studied simultaneously were more selective overall than those who studied sequentially. Given that participants' study in Experiment 2 was overtly self-regulated, though, the mechanism(s) by which such selectivity was realized can be more directly investigated. Based on the fact that participants who studied sequentially *did* study selectively when considering provisional recall, but not when considering the full set of items, there must have been differences in participants' item selections. The nature of these differences, though, is unclear from the previous analyses. Did those in the simultaneous condition better prioritize high-value information only when choosing which items to study, or did differences extend to how the selected subset of information was studied, as well, in terms of restudy choices and study time allocation?

Multilevel mediation analysis was conducted to clarify the contributory roles of item value, study time allocation, and the number of times items were studied on subsequent recall across the six lists (see Castel et al., 2013 for similar analyses). The first phase of the mediation

analysis determined the total effect of value on recall; this step was conducted in the previous section (Model 1, see Table 2.2). The second and third phases of the analysis estimated the path coefficients of the model.

The second phase included two separate HLM models in which the outcome variable in Model 1 was replaced by total study time per item (in seconds) (Model 2a, Table 2.4) and the number of times the item was studied (Model 2b, Table 2.4), respectively.[5] The third phase of the analysis (Model 3) addressed the direct effect of item value on recall probability and the indirect effects of the study time allocation and study selection mediators. Model 3 was similar to Model 1, but included the total study time per item and number of study instances as group-mean centered predictors alongside value and the interactions between each predictor and list (Model 3, Table 2.5). As in Model 1, recall in Model 3 was separately considered both in terms of recall of the full set of to-be-remembered items (Figure 2.5a) and provisional recall (Figure 2.5b). This distinction resulted in similar patterns between conditions, so these coefficients are provided separately in Tables 2.4 and 2.5 and Figure 2.5 but are not further distinguished in-text for the sake of simplicity. The coefficients provided in-text reflect an analysis of the full set of items unless otherwise noted.

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[5] Study time and number of study instances are not binary outcomes, so item-level performance was not based on a Bernoulli distribution, like recall was in Model 1, but rather a continuous (normal) distribution.
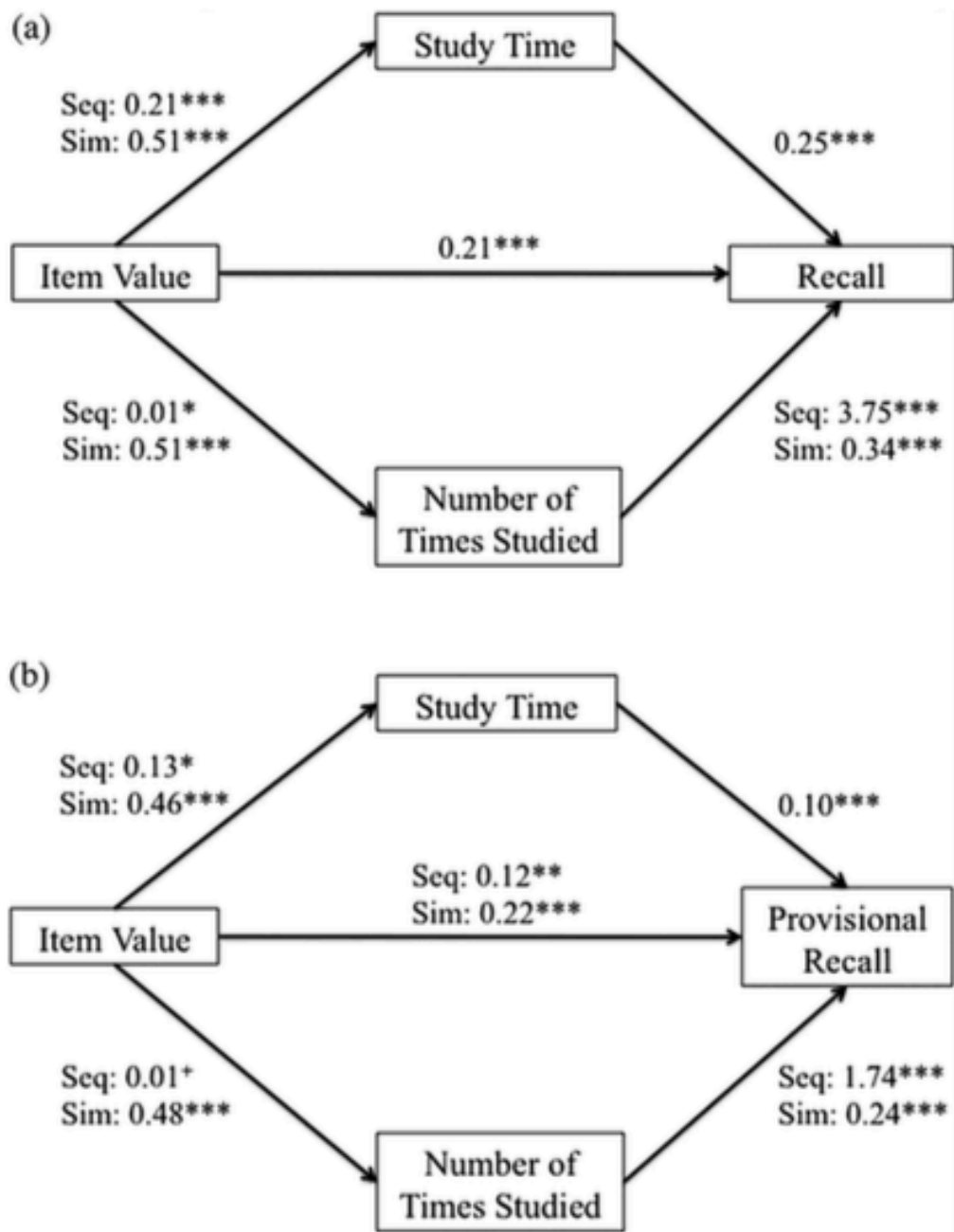
Figure 2.5. Mediation analyses using the interaction between an item's value, the total time it was studied, and the number of times it was studied to determine the likelihood of (a) recall (when considering the full set of items within each list) and (b) provisional recall (considering only those items which the participant chose to study) by participants assigned to the sequential (seq) and simultaneous (sim) study conditions. The estimated (unstandardized) path coefficients are presented separately when significant differences arose between conditions. $^{+}p < .10$ $*p < .05$ $**p < .01$, $***p < .001$

Value significantly predicted study time allocation in both study conditions (Model 2a; β_{10} = 0.21, p = .002), but it was significantly more predictive of allocation during simultaneous study than sequential (β_{11} = 0.30, p = .002), as shown in Figure 2.6.
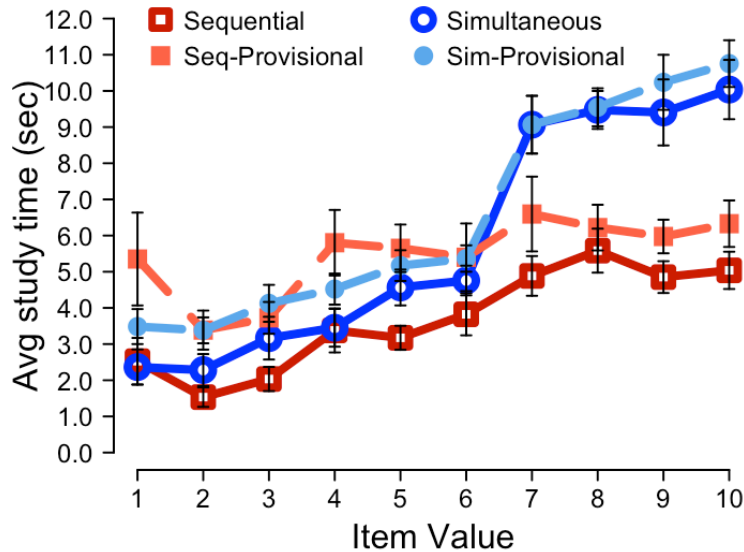
Figure 2.6. Average study time (in seconds) allocated to each item as a function of item value and study condition across lists in Experiment 2. "Sim-Provisional" and "Seq-Provisional" refer only to items that were studied at least one time in the simultaneous and sequential conditions, respectively. Error bars reflect standard error.

Value also predicted the number of times that an item was studied in the simultaneous condition (Model 2b; β = 0.51, p < .001)—with high-value items studied more frequently than low-value items—but not in the sequential condition (Model 2b; β_{10} = 0.01, p = .04). Thus, participants who studied the items simultaneously were markedly more selective when self-pacing their study and in selecting which items to study/restudy—devoting significantly more resources to high-value items relative to low-value items—than those in the sequential condition.

Naturally, the likelihood of later recalling an item increased the longer it was studied (Model 3; β_{50} = 0.25, p < .001), with no statistically detectable differences between conditions (β_{51} = -0.08, p = .21). The odds of recalling an item also increased as a function of the number of times that it was studied (Model 3; β_{30} = 3.75, p < .001), but the number of study instances had a

greater effect on recall in the sequential condition than the simultaneous condition (β_{31} = -3.40, p < .001). This may seem surprising, but it is important to keep in mind that participants in the simultaneous condition had, on average, more individual study events per item (M = 4.16) than those in the sequential condition (M = 1.03; see Table 2.3). Thus, an additional instance of study in the simultaneous condition was relatively less influential; if one has already studied an item three times, studying it one more time will logically have less of an impact on recall odds than studying an item again which has so far only been studied once, particularly when most of the other studied items were also studied only one time.

After controlling for the effects of self-pacing and the number of study instances, there remained a statistically detectable effect of value on provisional recall in the sequential condition (β_{10} = 0.12, p < .001) and, to a greater extent (p = .05), in the simultaneous condition (β = 0.22, p < .001). These condition differences in the effect of value (β_{10} = 0.21, p < .001), after controlling for differences in overt study behaviors, were not evident when considering recall of the full set of items (p = .71).

Presentation order.

A particularly notable finding in the mediation analyses was that item value did not predict study frequency in the sequential condition, but participants in the sequential condition also rarely restudied (Table 2.3). Certainly, this difference in restudy tendencies between conditions is noteworthy, but it might also minimize the role of value in making study selections in the previous analyses. As depicted in Figure 2.7a, though, value continued to be remarkably irrelevant to the sequential condition when examining simply whether an item was studied or not at all.

If participants in the sequential condition did not base their decision of whether or not to study an item (let alone restudy it) on its value, the only remaining logical factor on which they might have based it would appear to be the order in which it was presented. Part of what is thought to make sequential formatting demanding is that the order of presentation during study is more a factor with which a learner must contend rather than one which facilitates the execution of a study agenda, and it is a factor which is absent in simultaneous study as all to-be-remembered items are visible at once.

Multilevel mediation analyses were again conducted on the sequential condition alone, considering both item value and order of presentation as predictors rather than item value alone; the results of this analysis are provided in Figure 2.7b and Table 2.6.[6]

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[6] Note that provisional analyses were not considered because they would have excluded unstudied items, thereby preventing any determination of the impact of presentation order versus item value on participants' study selections.
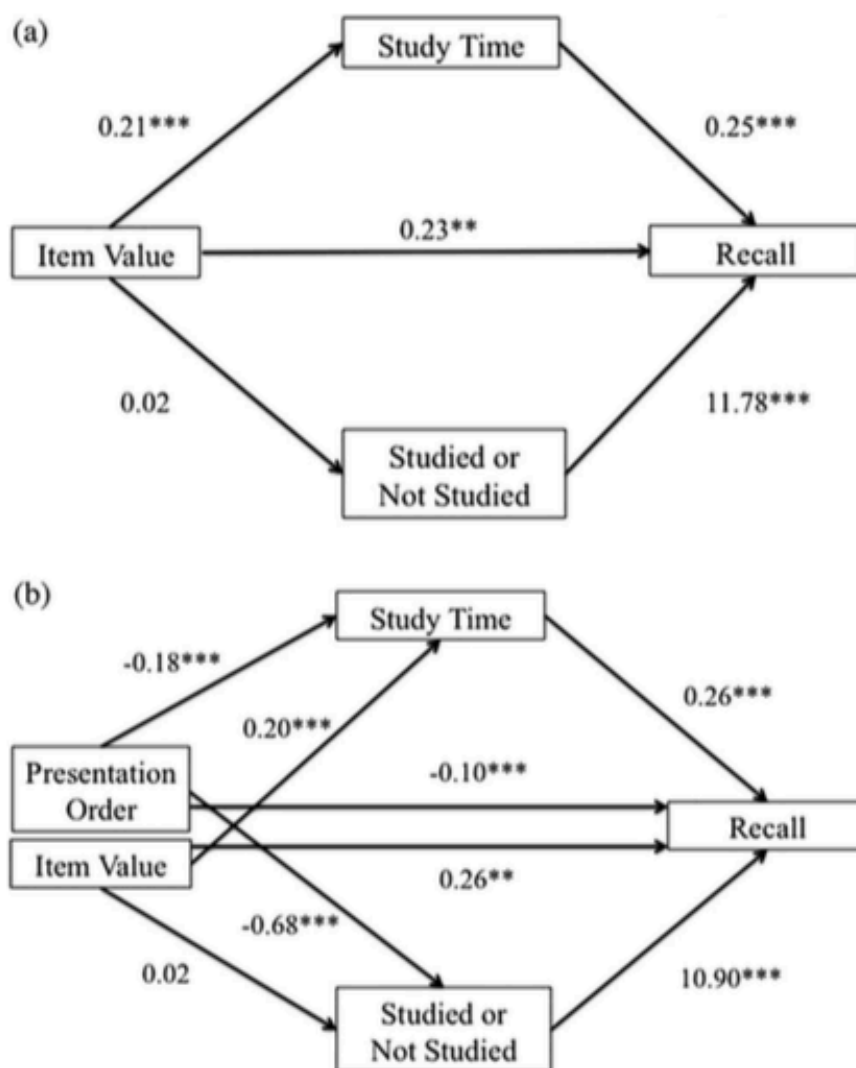
Figure 2.7. Mediation analysis using the interaction between (a) an item's value and (b) the order in which it was presented, whether it was studied (0 = *not studied*, 1 = *studied*), and the total time it was studied to determine its likelihood of being recalled by participants assigned to the sequential condition. As opposed to Figure 2.5, the separate presentation of the estimated (unstandardized) path coefficients for Value and Order does not reflect significant differences between the two predictors. $*p < .05 **p < .01, ***p < .001$

Order predicted which items participants in the sequential condition chose to study, with the

odds of studying an item decreasing the later that it was presented in the list. Order also predicted

study time, effectively to the same extent that did value: The further along the list an item was

presented, the less time it received. After controlling for study time and whether or not an item

was studied, value was more predictive of recall than was order—a 10-point item, for instance,

was more likely to be recalled than a 1-point item that was studied for the same amount of time—but the contribution of both factors to recall likelihood was statistically detectable.

Reflecting on the previous results, value had much less of an effect on self-regulated study and subsequent recall in the sequential condition than the simultaneous condition; it appears that at least some of this difference was a consequence of a complete neglect of value by the sequential condition in determining what to study in the first place. Once selected, participants in the sequential condition did appear to apply some element of a value-based strategy, demonstrating selectivity on more of a local level—considering each presented item in turn—rather than applying a global value-based strategy across the entirety of the list.[7]

Discussion

As in Experiment 1, participants in the simultaneous condition were significantly more selective in their study and consequent recall than participants in the sequential condition, even though participants in Experiment 2 could decide for how long and how often to study the items. Critically, the sequential condition relied on presentation order—not value—when determining whether or not to study an item, meaning that even similar attendance to value within the studied subset of items in the sequential condition relative to the simultaneous condition was still demonstrative of less strategic study overall. Participants in the sequential condition did not study the most valuable of the full, to-be-remembered set of items. Although both conditions demonstrated adherence to a generally value-based study agenda to varying extents, item value was thus a consistently greater determinant of self-regulated study and recall in the simultaneous

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[7] There is evidence to suggest an influence of reading habits on item selections in simultaneous presentations, wherein left-to-right reading habits (or right-to-left, as applicable) can disrupt agenda execution during study (Ariel et al., 2011; Ariel & Dunlosky, 2013; Dunlosky & Ariel, 2011b), but there were no apparent effects of column assignment on any of the self-regulated study predictors or recall in the simultaneous condition (i.e., items in the left-most columns were not prioritized over items in the right-most columns).

condition than in the sequential condition, consistent with prior research (Ariel et al., 2009; Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999).

General Discussion

According to the agenda-based regulation model of self- regulated learning, decisions during study, like electing to restudy and determining for how long to study, are generally made with the intention of studying in an efficient manner, of maximizing the return for one's cognitive expenditure (Dunlosky & Ariel, 2011a; Dunlosky, Ariel, & Thiede, 2011). Under this model, a learner's study is guided by an agenda—constructed based on not only performance goals, but also various personal- and task-related factors. This agenda is maintained and enacted during study via the central executive of working memory, excess stress to which is predicted to lead to suboptimal study (Dunlosky & Ariel, 2011a; Dunlosky et al., 2011; Dunlosky & Thiede, 2004). The way in which to-be-remembered information is presented during study— namely sequentially or simultaneously— has been identified as one such stressor to the central executive: Relative to simultaneous study, learners tasked with studying sequentially demonstrate less efficient study (e.g., allocating more of their study to difficult items than easy items despite time limits; Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999). The current experiments were designed to assess whether this same format-based impairment to study efficacy and efficiency extends to the strategic study of valuable information.

Experiment 1 directly contrasted sequential and simultaneous presentation during study to determine the effect of formatting on value-directed remembering and selectivity when the means of executing a value-based agenda were based on attention control mechanisms, while Experiment 2 examined the formatting effects on selective self-regulated study behaviors and, thus, recall. Evidence of value-directed remembering was found in both experiments regardless

of the presentation format, with participants ultimately recalling more high-value items than low-value. This is consistent with prior reports that learners can and will prioritize high-value information over low-value information during sequential study when the quantity of information exceeds encoding capacity (Ariel & Castel, 2014; Castel et al., 2012; Robison & Unsworth, 2017), even in spite of stressors like constrained study time (Middlebrooks, Murayama, et al., 2016) and divided attention (Middlebrooks, Kerr, et al., 2017). Importantly, however, participants in the simultaneous condition were *more* selective than those who studied sequentially. This difference supports prior research concerning formatting effects on the study of easy and difficult information (Ariel et al., 2009; Thiede & Dunlosky, 1999) and may help to explain instances in which sequential, value-based study has *not* been detected (DeLozier & Dunlosky, 2015). It further supports the agenda-based regulation model's position that the central executive is responsible for maintaining and executing study agendas and, as such, agenda maintenance/execution is susceptible to factors (like presentation formatting) stressful to the central executive (Dunlosky et al., 2011; Dunlosky & Ariel, 2011a).

That the agendas adopted by participants in the simultaneous and sequential conditions were value-based in Experiment 2, however, may have been the extent of their similarity. Participants in the simultaneous condition were not only more selective in their recall relative to the sequential condition, but were also more selective in their study selections, with the subset of items that they chose to study of greater value than that chosen by the sequential condition. Importantly, value did not appear to motivate item selections in the sequential condition at all. Rather, participants studied the presented items in order, regardless of their value, despite having been explicitly told that they could choose not to study items entirely if they so wished. In fact, order was just as predictive of self-pacing as was item value within the sequential condition (see

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Figure 2.7b). If considering a whole textbook, participants in the simultaneous condition were metaphorically more likely to study and remember the most important information overall; participants in the sequential condition were somewhat more likely to remember the most important than less important information, but only in the first couple of chapters. Even a perfect study strategy is relatively useless if inappropriately applied.

The present results suggest that participants who studied simultaneously were not simply better able to execute the agenda, but that they devised a more efficient value-based study agenda in the first place. Participants in the simultaneous condition not only studied the high-value items for more time overall, but they divided this longer study into shorter, more frequent study events than did participants who studied sequentially (see Figure 2.8), essentially spacing their study.[8]

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[8] Note that, although speculative, the short, but frequent study events demonstrated by participants in the simultaneous condition—in contrast with the lengthier but generally singular study instances in the sequential condition—are consistent with the idea explored in Experiment 1 that simultaneous formatting may be more conducive to (or instigate) more associative or organized encoding than sequential formatting.
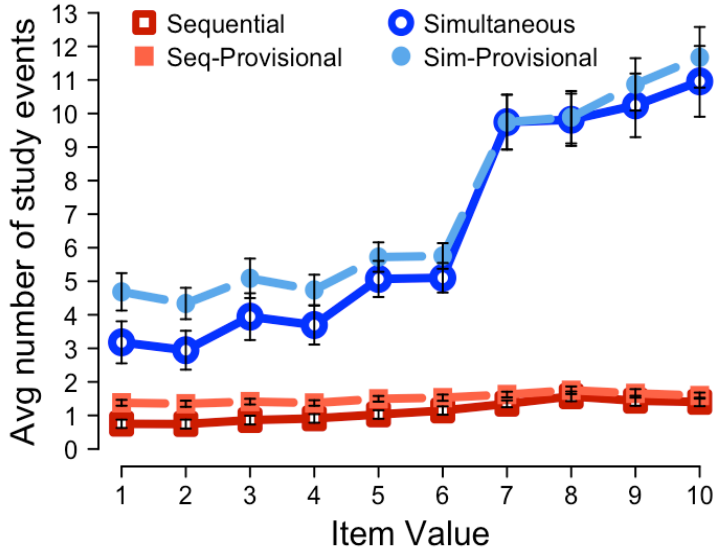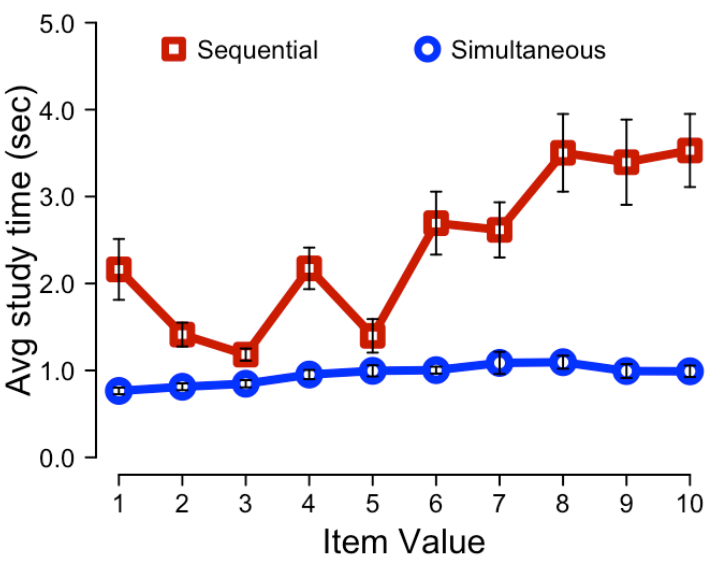
Figure 2.8. (a) Average number of times an item was studied and (b) average study time *per* study instance, in seconds, as a function of item value and assigned study condition across lists in Experiment 2. Note that Figure 2.8b cannot depict items that were not studied at all (i.e., with zero study instances). Error bars reflect standard error.

For the most part, participants in the sequential condition, however, did not take advantage of the opportunity to restudy items and instead massed their study of an item into a single instance, suggesting that what appears to be the most effective schedule to learners could be (at times inappropriately) influenced by the format in which the information is originally presented. Future research should query participants on what they consider to be the most optimal study strategy in

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light of the task goals (in the current case, to attain as many points as possible) so as to determine whether evident differences in strategic study differ between presentation formats owing to differences in beliefs about the optimal strategy, or whether the optimal strategy professed by participants in fact differs from their behaviors.

The studied materials in the current experiments were discrete items, but future research should also consider materials in which the individual units of information are conceptually related, as in text passages and textbook chapters. It may be that the benefits of simultaneous study over sequential when presented with discrete items do not extend to situations in which learners must actually discretize the information into important and less important subsets when the less important information is, nevertheless, related to the important. On the other hand, one feature of simultaneous formatting is that it should encourage relative judgments/comparisons across items or informational units (Wells, 1984; Wells, Steblay, & Dysart, 2011), potentially highlighting differences in item importance. Participants in the current experiments were not tasked with evaluating the importance of the to-be-remembered items—value was explicitly noted. The effect of formatting on more realistic, conceptual materials may depend on whether the learner must first evaluate importance before applying a value-based strategy.

Conclusion

The current experiments examined whether previously reported impairments to strategic study following sequential presentation relative to simultaneous are similarly evident in the study of and memory for valuable information. Participants generally prioritized high-value over low-value information, irrespective of the manner of presentation during study, but those presented with all of the to-be-remembered information simultaneously demonstrated greater value-based prioritization in allocating their attention (Experiment 1) and overtly self-regulating their study

(Experiment 2). These results are consistent with the theory that devising, maintaining, and executing an efficient study agenda is inherently more demanding under sequential formatting relative to simultaneous (Dunlosky & Ariel, 2011a; Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999).

Table 2.1

Presentation format

Recall probability as a function of study condition and list in Experiments 1 and 2

	Condition	List 1	List 2	List 3	List 4	List 5	List 6	Average
Experiment 1	Sequential	.35	.30	.37	.40	.40	.35	.36
		(.11)	(.11)	(.13)	(.16)	(.13)	(.14)	(.09)
	Simultaneous	.41	.46	.46	.46	.47	.46	.46
		(.16)	(.11)	(.10)	(.12)	(.13)	(.11)	(.09)
Experiment 2	Sequential	.27	.27	.30	.31	.33	.32	.44
		(.10)	(.09)	(.11)	(.08)	(.07)	(.08)	(.06)
	Simultaneous	.44	.41	.43	.44	.45	.46	.30
		(.14)	(.11)	(.12)	(.15)	(.10)	(.13)	(.09)
	Sequential-Provisional	.59	.57	.53	.45	.51	.47	.52
		(.28)	(.22)	(.24)	(.21)	(.22)	(.23)	(.19)
	Simultaneous-Provisional	.57	.52	.53	.58	.63	.66	.58
		(.18)	(.17)	(.17)	(.20)	(.23)	(.24)	(.14)

Note. Standard deviations are presented in parentheses. "Sequential-Provisional" and "Simultaneous- Provisional" in Experiment 2 reflect the probability of recalling an item provided that the participant chose to study it in the first place.

Table 2.2

Presentation format

Two-level hierarchical generalized linear model of recall performance predicted by item value, list, and study condition (Model 1)

Fixed effects	Experiment 1	Experiment 2: Unconditional recall	Experiment 2: Conditional recall
Intercept (β_{00})	-0.63***	-1.07***	-0.23
Predictors of intercept			
Condition (β_{01})	0.41**	0.59**	0.15
Value (β_{10})	0.18***	0.26***	0.31***
Predictors of value			
Condition (β_{11})	0.11$^+$	0.24*	0.09
List (β_{20})	0.03	0.02	-0.11*
Predictors of list			
Condition (β_{21})	-0.01	-0.04	0.14*
List x Value (β_{30})	0.03*	0.05**	0.06**
Predictors of list x value			
Condition (β_{31})	0.01	0.05$^+$	0.02
Random effects	**Variance**	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.14***	0.29***	0.62***
Value (r_1)	0.03**	0.13***	0.10***
List (r_2)	0.01$^+$	0.01**	0.03***
List x Value (r_3)	0.002***	0.01***	0.01***

Note. The dependent variable is recall performance coded as 0 (*not recalled*) or 1 (*recalled*). In Experiment 2, conditional recall analyses were based only on items that a participant had chosen to study; unconditional recall includes unstudied items. Logit link function was used to address the binary dependent variable. Level 1 models were of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 models were of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Condition) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Condition) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Condition) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Condition) $+ r_{3j}$. The Condition predictor was anchored on the sequential study condition (i.e., 0 = *sequential*, 1 = *simultaneous*).
$^+p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 2.3

Presentation format

Summary statistics of self-regulated study in Experiment 2

	Sequential	Simultaneous
Average number of items studied per list	10.70 (4.27)	**16.15 (3.56)**
Average value of studied items	6.31 (1.04)	6.25 (0.99)
Average number of items recalled per list (out of 20)	6.00 (1.29)	**8.79 (1.88)**
Average proportion of studied items recalled per list	.52 (.19)	.58 (.14)
Average total study time per studied item (sec)	4.14 (2.27)	3.84 (1.06)
Average number of study events per studied item	1.03 (0.09)	**4.16 (1.31)**
Average study time per study event (sec)	**5.19 (6.66)**	1.01 (0.31)

Note. Standard deviations are presented in parentheses. Bolded values indicate significant differences between conditions ($p < .05$), with the greater of the two conditions bolded.

Table 2.4

Presentation format

Two-level hierarchical generalized linear model of study time allocation (Model 2a) and (re)study selections (Model 2b) predicted by item value, list, and study condition in Experiment 2

Fixed effects	Model 2a: Total study time, unconditional	Model 2a: Total study time, conditional	Model 2b: Number of times studied, unconditional	Model 2b Number of times studied, conditional
Intercept (β_{00})	1.84***	4.58***	0.71***	1.03***
Predictors of intercept				
Condition (β_{01})	1.09***	-1.16	2.53***	2.59***
Value (β_{10})	0.21***	0.13*	0.01*	0.01^{+}
Predictors of value				
Condition (β_{11})	0.30**	0.33***	0.50***	0.48***
List (β_{20})	-0.03	-0.71^{+}	0.05***	0.01
Predictors of list				
Condition (β_{21})	0.04*	0.77^{+}	-0.05	0.04
List x Value (β_{30})	0.03*	0.04	0.001	0.004
Predictors of list x value				
Condition (β_{31})	0.004	-0.01	0.03^{+}	0.01
Random effects	**Variance**	**Variance**	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.01	8.35***	0.30***	0.29***
Value (r_1)	0.09***	0.07***	0.10***	0.09***
List (r_2)	0.0003	1.78***	0.02***	0.04***
List x Value (r_3)	0.003***	0.01***	0.003***	0.01***

Note. The dependent variable in Model 2a is the total time an item was studied (in seconds) and, in Model 2b, the number of times an item was studied. Unconditional analyses consider all 20 items within the list while the conditional analyses consider only those items that were studied at all. Level 1 of both models were of the form $Y_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 of both models were of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Condition) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Condition) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Condition) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Condition) $+ r_{3j}$. The Condition predictor was anchored on the sequential study condition (i.e., 0 = *sequential*, 1 = *simultaneous*).
$^{+}p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 2.5

Presentation format

Two-level hierarchical generalized linear model of recall performance predicted by item value, list, study time, number of times studied, and study condition (Model 3) in Experiment 2

Fixed effects	Model 3: Unconditional recall	Model 3: Conditional recall
Intercept (β_{00})	-1.62***	0.44*
Predictors of intercept		
Condition (β_{01})	1.25***	-0.22
Value (β_{10})	0.21***	0.12**
Predictors of value		
Condition (β_{11})	0.02	0.10^{+}
List x Value (β_{20})	0.05**	0.02
Predictors of list x value		
Condition (β_{21})	0.03	0.05^{+}
Number of times studied (NoS) (β_{30})	3.75***	1.74***
Predictors of NoS		
Condition (β_{31})	-3.40***	-1.50**
List x NoS (β_{40})	-0.01	0.03
Predictors of list x NoS		
Condition (β_{41})	0.14	0.001
Total time studied (ST) (β_{50})	0.25***	0.10***
Predictors of ST		
Condition (β_{51})	-0.08	0.04
List x ST (β_{60})	-0.003	-0.01
Predictors of list x ST		
Condition (β_{61})	-0.04	-0.01
Random effects	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.33***	0.59***
Value (r_1)	0.04***	0.02**
List x Value (r_2)	0.004***	0.004**
NoS (r_3)	0.12***	0.04***
List x NoS (r_4)	0.005**	0.002^{+}
ST (r_5)	0.02***	0.001**
List x ST (r_6)	0.003**	0.001**

Note. The dependent variable is recall performance coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable. Level 1 was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List x Value) $+ \pi_{3j}$ (NoS) $+ \pi_{4j}$ (List x NoS) $+ \pi_{5j}$ (ST) $+ \pi_{6j}$ (List x ST). Level 2 was of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Condition) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Condition) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Condition) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Condition) $+ r_{3j}, \pi_{4j} = \beta_{40} + \beta_{41}$ (Condition) $+ r_{4j}, \pi_{5j} = \beta_{50} + \beta_{51}$ (Condition) $+ r_{5j}, \pi_{6j} = \beta_{60} + \beta_{61}$ (Condition) $+ r_{6j}$. Condition was anchored on the sequential study condition (i.e., 0 = *sequential*, 1 = *simultaneous*). $^{+}p < .10$ $^{*}p < .05$ $^{**}p < .01$ $^{***}p < .001$

Table 2.6

Presentation format

Two-level hierarchical generalized linear mediation analysis of recall performance in the sequential condition in Experiment 2

Fixed effects	Model 1	Model 2a: Study time	Model 2b: Studied or Not studied	Model 3
Intercept (β_{00})	-1.51***	1.84***	2.13**	-11.63***
Value (β_{10})	0.32***	0.20***	0.02	0.26**
Order (β_{20})	-0.24***	-0.18***	-0.68***	-0.10***
List x Value (β_{30})	0.05*	0.04*	-0.01	0.04*
List x Order (β_{40})	0.01	0.04***	0.02	0.01
Studied or Not studied (β_{50})	---	---	---	10.90***
Total study time (β_{60})	---	---	---	0.26***
Random effects	**Variance**	**Variance**	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.42***	0.13**	11.08***	0.18
Value (r_1)	0.10***	0.03***	0.002	0.08
Order (r_2)	0.03***	0.03***	0.19***	0.01
List x Value (r_3)	0.004***	0.001	0.0003	0.003
List x Order (r_4)	0.002***	0.001**	0.03***	0.0003
Studied or Not studied (r_5)	---	---	---	0.92
Total study time (r_6)	---	---	---	0.02

Note. Recall performance was coded as 0 (*not recalled*) or 1 (*recalled*); study selections were coded as 0 (*not studied*) or 1 (*studied*). As applicable, logit link function was used to address binary dependent variables. Level 1 models in Model 1, Model 2a, and Model 2b were of the form η_{ij} (binary outcomes) or Y_{ijk} (continuous outcomes) = $\pi_{0j} + \pi_{1j}$ (Value) + π_{2j} (Order) + π_{3j} (List x Value) + π_{4j} (List x Order). Level 1 of Model 3 was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) + π_{2j} (Order) + π_{3j} (List x Value) + π_{4j} (List x Order) + π_{5j} (Studied or Not studied) + π_{6j} (Study time). Level 2 models were of the form $\pi_{0j} = \beta_{00} + r_{0j,} \pi_{1j} = \beta_{10} + r_{1j}$, et cetera. *$p < .05$ **$p < .01$ ***$p < .001$

Time limitations can negatively impact what is later remembered—what might have been remembered given more time is otherwise forgotten—the consequences of which can be wide-ranging. While limited study time is known to notably diminish the likelihood of remembering overall (Mackworth, 1962; Murdock, 1962; Posner, 1964; Roberts, 1972), it is unclear how people attempt to remember valuable information when they have limited time in which to do so. For example, how might a student approach a textbook in light of an upcoming exam? Does the student attempt to read as much of the textbook as possible, foregoing entire chapters once out of time, or does the student selectively focus on what seems important?

The impact of time constraints on the construction and execution of study agendas has been predominantly investigated with respect to the self-regulated study of information varying in difficulty. People tend to spend more time studying difficult items than easier or well-learned items (Dunlosky & Hertzog, 1998; Mazzoni et al., 1990; Nelson et al., 1994; Thiede et al., 2003). When the amount of time available to study all of the information is insufficient, though, there is a shift to prioritizing easier materials (Dunlosky & Thiede, 2004; Son & Metcalfe, 2000; Thiede & Dunlosky, 1999).

The effect of time constraints on the study of valuable information is less clear. Value-directed remembering research suggests that memory lapses suffered as a consequence of having too much information to remember may be tempered by selectively focusing on the most important information at the expense of that which is deemed less critical (e.g., Castel, 2008; Castel et al., 2002; Castel et al., 2012). As in the case of having too much to remember, having insufficient time in which to remember all of the information might similarly encourage strategizing during study, with an eye towards allocating one's resources and efforts during encoding in a manner that will maximize study productivity and later recall in spite of time limitations.

Even in the absence of time constraints, though, learners often require multiple trials or continued task experience before exhibiting value-directed remembering (Castel, 2008; Castel et al., 2012). When there is less time available to study presented information, there may also be less time to properly evaluate prior experiences and devise a corresponding course of action. Moreover, learning difficult information is intrinsically time demanding, while learning valuable information is not necessarily so. In fact, it is often the case that some to-be-remembered information is more valuable than other information despite being of similar ease/difficulty to remember (e.g., recalling the new telephone number of a close friend as opposed to that of a mere acquaintance). If the to-be-remembered information is of similar ease/difficulty to remember, as in the current study, then the successful encoding of low-value information should not inherently require more or less time than that of high-value information. Contrarily, difficult information necessarily requires more time to successfully encode than easy information. Thus, the limitations that time constraints during study present to learning may be more salient when the to-be-learned information varies in difficulty than importance.

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It may also be the case that learners continue to recognize the importance of adopting a value-based agenda when time is limited, but that they are less able to efficiently execute such an agenda in light of time constraints. The degree to which learners are selective represents the efficiency of their study: of the *n* items that one can successfully recall, are they the *n*-most important? It is possible that learners will continue to study selectively when time is limited, accommodating the decrease in allotted study time and consequential decrease in total recall by implementing more stringent criteria when determining to which subset of valuable items to attend. On the other hand, it may be that learners continue to generally prioritize high-value items over less valuable items when short on time, demonstrating value-directed remembering, but that the efficiency with which this strategy is executed diminishes. The odds of recalling a 10-point item over a 1-point item, for instance, might be lower when participants have limited study time than when time is far less constrained, indicating reduced selectivity. Learners may be less able to efficiently attend to and remember the most important information when they find themselves short on time, indicating not only quantitative costs to memory owing to time limitations, but also qualitative.

I have conducted three experiments to explore the effect that study time limitations may have on value-directed remembering, as well various factors that may mitigate, or perhaps exaggerate, the effect of time limitations. Experiment 1 (Middlebrooks, Murayama, et al., 2016) investigates whether insufficient study time alone affects the efficacy and likelihood of value-driven encoding, in addition to whether learners can adapt their study agenda to accommodate changes in the time available during study.

Experiment 2 investigates whether there are age-related differences in how learners respond to decreased study time allotments. Older adults already experience inherent limitations

to attention and working memory capacity owing to advanced age and general cognitive declines. Further constraints, like limited study time, could exacerbate attentional control deficits or, conversely, encourage more selective encoding.

Experiment 3 aims to differentiate between having insufficient study time and the *feeling* of being somewhat overwhelmed by the insufficient study time itself on the successful execution of a value-based study agenda. Anxiety can markedly impair cognitive performance (Hembree, 1988; Pan & Tang, 2005; Veenman, Kerseboom, & Imthorn, 2000) and may well commandeer the very resources necessary for strategic study (Dunlosky & Thiede, 2004; Eysenck & Calvo, 1992). Although likely absent in Experiment 1 owing to a lack of consequences, social pressures, et cetera, many real-world, time-sensitive situations that might benefit from selectivity are likely to be accompanied by feelings of anxiety, which may themselves impact value-directed remembering as a factor separate from the effects of the (insufficient) time allotment.

Experiment 1

The main goal of Experiment 1 was to examine the potential impact of time constraints on the study of valuable information: is it beneficial to study at a faster rate, in that it encourages a more selective, efficient study effort; or does memory for high-value information comparably decline with overall recall relative to a slower rate of study?

An additional goal was to investigate whether learners adjust to changes in study time and the impact such change can have on value-based study. Perhaps those participants who have studied under a constant rate are able to optimize their study by selectively allocating their attention to high-value items, but participants who experience a shift in study time are less able to recover or adapt a prior strategy in the short-term.

A further goal was to examine whether prior timing experiences transfer to situations in which study is entirely self- paced. Although shifts in study may result in an immediate decrement in selectivity, it may also be the case that learners with more varied study experiences, such as with fast and slow study, are better equipped to optimally self-regulate their study than learners who were only familiarized with a constant study rate.

Method

Participants

Participants consisted of 192 undergraduate students[9] at the University of California, Los Angeles (142 female, 1 unreported), ranging in age from 18 to 26 years ($M = 20.34$, $SD = 1.41$). Participants received partial credit for a course requirement.

Materials

The study was designed and presented to participants via the Collector program (Gikeymarcia/Collector, n. d.). Stimuli consisted of 12 lists containing 20 novel words apiece. Each of the words was randomly assigned a value ranging from 1 to 10, with two words assigned to each value. The words in each list were randomly selected without replacement from a larger word bank of 280 random nouns and verbs (e.g., twig, button, point, taste). Word length ranged from 4-7 letters and averaged to 8.81 ($SD = 1.57$) on the log-transformed Hyperspace Analogue to Language (HAL) frequency scale[10] with a range from 5.48 to 12.65 (Lund & Burgess, 1996). The 240 studied words were randomly selected from this bank for each participant in order to avoid any potential item effects (Murayama, Sakaki, et al., 2014). Thus, the words studied in List 1 for one participant might have been entirely different from another participant's List 1.

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[9] Experiment 1 was based on a pooled set of original data ($N = 96$) and replication data ($N = 96$). The results from the original collection are largely consistent with those reported from the replication and the pooled data.

[10] The Log HAL frequency measure of the words included in the English Lexical Project ranges from 0 to 17, with an average frequency of 6.16 and a standard deviation of 2.40 (Balota et al., 2007).

Furthermore, one participant might study the word "drizzle" while another might not, or might have studied "drizzle" as a 3-point word while another studied it as a 9-point word.

Procedure

Participants were shown a series of word lists, each containing 20 different words. They were told that each word would be paired with a value ranging from 1 point to 10 points and that there would be two words per point value within each list. Participants were instructed to remember as many of the words in each list as possible while also striving to achieve a maximal score, a sum of the points associated with each word correctly recalled. They would be asked to recall the words from each list at the end of its presentation, at which point they would then be told their score (out of 110 possible points). Participants were also told that the words would be presented on the screen one at a time at a rate of which they would be informed just prior to each list's commencement.

Participants were randomly assigned to one of four study time conditions, which determined the rate of presentation during their study of the first eight lists: Constant-Fast [1-1], Constant-Slow [5-5], Speed Up [5-1], or Slow Down [1-5]. Participants in the Constant conditions studied the words in Lists 1-8 at a rate of either 1 second (Constant-Fast) or 5 seconds per word (Constant-Slow). Participants in the Speed-Up condition studied at a rate of 5 seconds per word during Lists 1-4 and then 1 second per word during Lists 5-8; thus, their rate of study increased. Contrastingly, participants in the Slow-Down condition studied at a rate of 1 second per word during Lists 1-4 and then 5 seconds per word during Lists 5-8; thus, their rate of study decreased. Study was self-paced for all participants during Lists 9-12, with a cap on neither the per-item or per-list study time. This design created three different timing segments: Segment 1 consisted of Lists 1-4; Segment 2 of Lists 5-8; and Segment 3 of the self-paced Lists 9-12.

Based on prior research (e.g., Castel et al., 2007; Middlebrooks, McGillivray, et al., 2016), a rate of 5 seconds per word was chosen in order to provide sufficient time for participants to identify the word's value, determine whether or not it met any sort of strategy criterion, and/or to potentially engage in some form of elaborative rehearsal. The 1-second rate was chosen as a contrasting time; insufficient for any lengthy and elaborative rehearsal, it was still enough time for intentional encoding. Including multiple lists within each timing segment provided participants with the chance to learn from prior list performance and subsequently update their strategies (Castel, 2008; Castel et al., 2012; Metcalfe, 2002). The within-subject manipulation of study time was also intended to increase the saliency of the study time allotments. The perception of limited or insufficient study time is largely a relative judgment; participants who have only studied at a rate of 1 second per word, for instance, might not feel as short on time as participants who had previously studied at 5 seconds before dropping to 1 second. This potential difference in perception could mean that a 1-second study rate, for instance, has a divergent impact on attention allocation during study and selectivity.

Results

Overall Recall Performance

Analyses were first conducted to determine whether there was an effect of study time on overall recall performance, irrespective of item value, in order to verify that the 1-s and 5-s rates were sufficiently different in terms of encoding and recall and that shorter study time did indeed lead to a decline in recall. Table 3.1 lists the proportion of items recalled as per study time and list.

A 4(Condition: Constant-Fast [1–1], Constant-Slow [5–5], Speed Up [5–1], Slow Down [1–5]) × 3(Segment: Lists 1–4, Lists 5–8, Lists 9–12) repeated-measures ANOVA on total recall

revealed a significant Condition × Segment interaction, $F(6, 376) = 32.36$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .13$. There was a significant effect of Condition within Segment 1 and Segment 2, $ps <$ .001, but not Segment 3, $p = .76$. Within Segment 1, the Constant-Fast and Slow Down conditions recalled significantly fewer items than the Constant-Slow and Speed-Up conditions, $ps < .001$. There were no significant differences between the Constant-Fast and Slow-Down conditions, nor between the Constant-Slow and Speed-Up conditions, $ps > .62$. In other words, those conditions that studied the items at a rate of 1 s per word recalled significantly fewer words than those conditions studying at a 5-second rate. The same pattern emerged in Segment 2: participants in the Constant-Fast and Speed-Up conditions (each studying at a 1-second rate) recalled significantly fewer words than the Constant- Slow and Slow-Down conditions (5-second study rate), $ps < .001$, and there were no significant differences between conditions studying at the same rate, $ps > .24$. These results confirm that reduced study time led to reduced recall in the current experiments.

Value-Directed Remembering and Selectivity

Recall performance as per value and timing segment is presented in Figure 3.1.
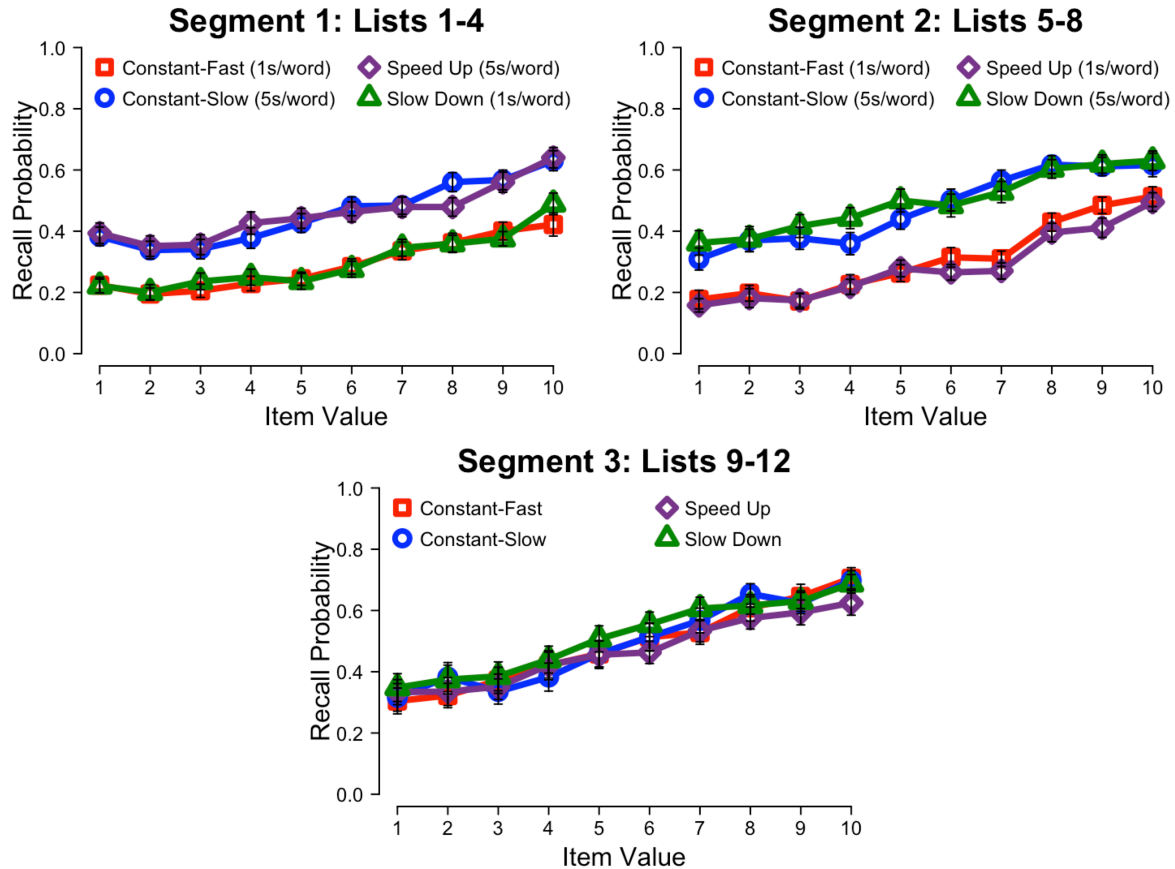
Figure 3.1. Recall probability as per item value, list, and study condition in Segments 1-3 of Experiment 1. Study during Segment 3 was self-paced. Error bars reflect standard error.

Within each timing segment, item-level recall performance (based on a Bernoulli distribution, with 0 = *not recalled* and 1 = *recalled*; level 1 = items; level 2 = participants) using HLM was modeled as a function of each item's value, the list in which it was presented, and the interaction between value and list. Value and List were entered as group-mean centered variables, with Value anchored on the mean value point (5.50) and List anchored on the mean list of the given segment. The model further included the timing conditions as level-2 predictors of those level-1 effects via three dummy-coded variables, with the Constant-Slow condition [5-5] as the reference group. Table 3.2 reports the tested model and its estimated regression coefficients per segment analysis.

Segment 1.

Value was a significantly positive predictor of recall performance in the Constant-Slow condition ($\beta_{10} = 0.15$, $p < .001$) during Segment 1, and this relationship was not significantly different in the other conditions, ps > .46. In other words, participants across all conditions were $e^{0.15} = 1.16$ times more likely to recall an item for each one-unit increase in its value. The odds of recalling a 10-point item during Segment 1, for example, were thus $e^{0.15 * 10} = 4.48$ times greater than the odds of recalling a 1-point item.

There was a significant effect of List for participants in the Constant-Slow condition ($\beta_{20} = 0.10$, $p = .005$), and there was a significant cross-level interaction between List and Condition, wherein List had an increasingly reductive effect on total recall relative to the Constant-Slow condition, irrespective of item value, for those participants in the Constant-Fast ($\beta_{21} = -0.13$, $p = .005$) and Slow-Down conditions ($\beta_{23} = -0.10$, $p = .05$) (i.e., those participants studying at a rate of 1-second per word).

There was also a marginally significant List x Value interaction in the Constant-Slow condition, such that the relationship between item value and recall probability increased with each successive list ($\beta_{30} = 0.03$, $p = .07$). Namely, participants demonstrated greater selectivity across lists, with recall increasingly conditional upon item value with each successive list. This interaction did not differ across conditions (ps > .088), indicating that participants generally increased their selectivity across Segment 1.

Segment 2: within-subject timing shift.

Value was once again a significantly positive predictor of recall performance in the Constant-Slow condition ($\beta_{10} = 0.19$, $p < .001$) during Segment 2, and there were no significant differences across the other study time conditions, ps > .27. Participants were $e^{0.19} = 1.21$ times

more likely to recall an item for each one-unit increase in its value, demonstrating not only maintained selectivity, but somewhat greater attention to value than during Segment 1 ($\beta_{10} = 0.15$ versus 0.19).

There was a significant effect of List on recall in the Constant-Slow condition ($\beta_{20} = -0.09$, $p < .001$) with the probability of recalling an item, irrespective of its value, significantly decreasing across lists. There was not a significant difference in this List effect between the Constant-Slow and Constant-Fast conditions ($\beta_{21} = 0.04$. $p = .40$), but there were marginally significant differences between the Constant-Slow condition and the Speed-Up and Slow-Down conditions ($\beta s = 0.10$, $p s = .07$), those conditions in which participants experienced a shift in their allotted study time.

Critically, there was a significant List x Value interaction in the Constant-Slow condition ($\beta_{30} = 0.03$, $p = .003$), and this did not differ across the other conditions, $p s > .25$. Thus, selectivity continued to increase across lists in Segment 2, but was impacted by neither study time differences during Segment 2 nor differences between groups regarding prior experience with Segment 1 study times.

Segment 3: self-regulated study.

To determine whether learners transfer previously adopted strategies and prior study experiences to self-regulated study situations, study during the final four lists of the task (i.e., Segment 3) was entirely self-paced: participants could study each item for as long as they desired and there was no cap on how long they could study each list in total.

As in Segments 1 and 2, Value was a significant predictor of recall performance in the Constant-Slow condition ($\beta_{10} = 0.25$, $p < .001$), with no significant differences across the other study conditions, $p s > .35$. Participants were $e^{0.25} = 1.28$ times more likely to recall an item for

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each one-unit increase in its value. Again, this effect was greater than in either of the previous study segments ($\beta_{10} = 0.15$ versus 0.19 versus 0.25), indicating increasing attention to value as the task progressed. Similar to Segment 2, there was a significantly negative relationship between list progression and overall recall in Segment 3 ($\beta_{20} = -0.15$, $p = .001$), with no condition differences ($ps > .25$). Contrary to Segments 1 and 2, there was not a significant List x Value interaction in the Constant-Slow condition ($\beta_{30} = 0.02$, $p = .22$), nor in any of the other conditions ($ps > .18$).

These results indicate that, during this period of self-paced study, participants improved upon their strategy of selective study relative to the prior, experimenter-timed segments. Differing prior experiences across the conditions with respect to the allotted study times did not, however, appear to impact this self-regulation: participants across all conditions were similarly selective and maintained this selectivity across Lists 9-12.

Self-Regulated Study

Figure 3.2 illustrates the average proportion of total study time spent per item value during Segment 3 as well as the average study time per item value.
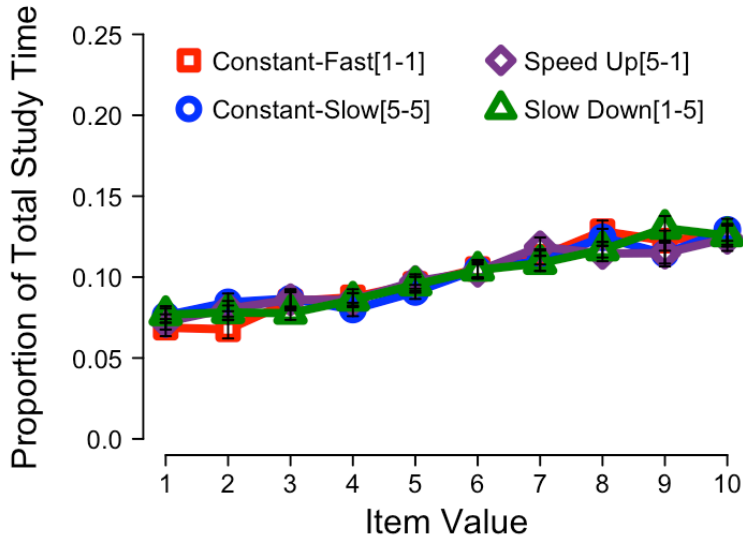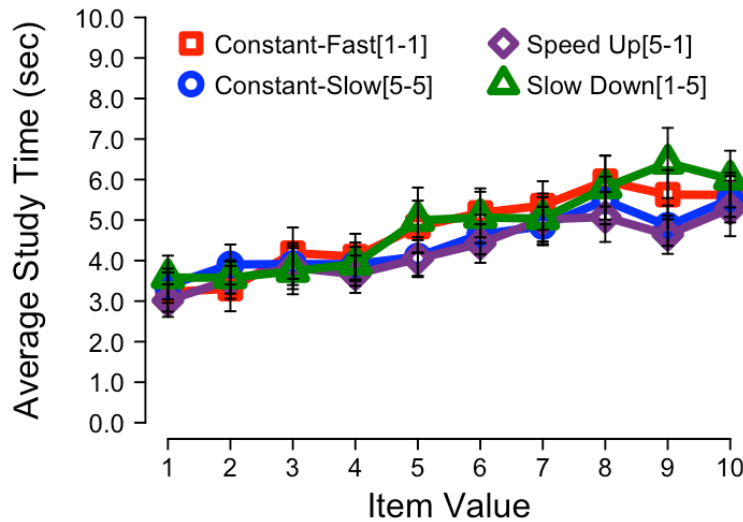
Figure 3.2. (a) The average proportion of self-paced study time and (b) the average study time, in seconds, allocated to each item value across assigned study conditions in Segment 3 of Experiment 1. The rates at which each condition studied during Segments 1 and 2, either 1 or 5 sec per word, are provided in brackets. Error bars reflect standard error.

As study was self-paced, each participant spent a different amount of time studying each of the Segment 3 lists overall while also allocating their study time across the items within each list differently. In investigating how (or if) participants considered item value in allocating their study times during Segment 3, proper consideration of individual variance is critical. Thus, HLM analyses were again implemented. Item-level study time (in seconds) was modeled as a function of each item's value, the list in which it was presented, and the interaction between value and

list, as in the previously conducted HLM analyses concerning value-based recall. The model

further included the study conditions as level-2 predictors of these level-1 effects via three

dummy-coded predictor variables with the Constant-Slow [5-5] study condition as the reference

group. Table 3.2 reports the tested model and its estimated regression coefficients.

There was a significant effect of Value on study time in the Constant-Slow condition,

with 0.22 more seconds spent studying words with each one-unit increase in assigned value (β_{10}

$= 0.22, p < .001$); this relationship did not significantly differ as a consequence of prior study

condition, ps > .21. There was also a significant effect of List on study time in the Constant-Slow

condition ($\beta_{20} = -0.58, p < .001$), such that significantly less time was spent studying each

successive list overall. This was consistent across conditions, ps > .1. There was evidence of

neither a List x Value interaction in the Constant-Slow condition ($\beta_{30} = 0.01, p = .66$), nor of a

three-way interaction between List, Value, and Condition, ps > .21.

Bayesian Analysis

The current analyses reveal a nonsignificant effect of Condition on the relationship

between item value and recall probability, indicating that there is little evidence that value-

directed remembering and one's ability to study selectively in the current task is influenced by

study time. However, as these results are based upon null hypothesis testing, it is impossible to

claim the absence of such effects (despite the large nature of the sample size, $N = 192$).

Additionally, the current analyses are based on an aggregation of the original sample and the

replication sample, and interim analyses were conducted for the original sample. Although there

was no intention to stop data collection contingent upon the obtained results, interim analyses

make the interpretation of the obtained p-values ambiguous (Murayama, Pekrun, & Fiedler,

2014). Thus, in order to confirm this null effect of Condition suggested by the HLM analyses,

and because we conducted interim analyses on this pooled data, a Bayesian analysis was also conducted. By using Bayes factors computed in Bayesian analysis, it is possible to directly compare of the probability of obtaining the present results under the null hypothesis H_0 (no Condition differences in the value effect) to the probability of the results under the alternative hypothesis H_1 (Condition differences) (Jarosz & Wiley, 2014).

As it is difficult to directly compare Bayes factors with HLM (although Bayesian information criterion [BIC] computed in HLM can provide some proxy for computing Bayes factors), a two-step approach was used to allow for simpler Bayesian analysis with hierarchical data (see Lorch & Myers, 1990; Murayama, Sakaki, et al., 2014). Specifically, item recall was regressed on item value within each list for each participant using logistic regression, and the obtained value coefficients were averaged by segment. A 4(Condition) x 3(Segment) repeated-measures Bayesian ANOVA was then conducted on these value slopes with JASP software using default priors (Love et al., 2015). Results indicated that the Bayes Factor$_{10}$ (BF$_{10}$), reflecting the probability of the data under the alternative hypothesis relative to the null, for Condition was .06. In other words, the present data are $1/.06 = 16.67$ times more likely to be consistent with the null model than with the alternative, providing strong evidence for a null effect of Condition (Jeffreys, 1961; Kass & Raftery, 1995). In sum, these results support the HLM analyses and confirm that selectivity during study was comparable across the timing conditions.

Discussion

Experiment 1 examined whether the generally negative impact of time constraints on memory might be mitigated by strategic, value-based study. It further investigated learners' ability to adjust their strategies in light of changes in study time, whether speeding up or slowing down. Additionally, Experiment 1 assessed how learners self-pace their study of valuable

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information in light of prior study experiences. While memory for the presented items was greater overall when participants were granted more time to study, there were no significant differences in participants' ability to selectively allocate their attention to the most valuable items. Participants studying at a rate of 1 s per word were just as likely to recall high-value items as participants studying at a slower (5 s) rate. Irrespective of study condition, participants showed an increase in selectivity across the lists of Segment 1. This is consistent with prior research demonstrating increases in selectivity with greater task familiarity (e.g., Castel, 2008; McGillivray & Castel, 2011; Middlebrooks, McGillivray, et al., 2016) and is indicative of strategy modification and/or more successful execution of an established value-based strategy. This selectivity continued to increase during Segment 2 in spite of mid-task shifts in study time; prior experiences with an alternate study rate did not appear to impact selectivity under novel conditions.

Although the study times were directly contrasted between Segments 1 and 2 of the task, participants might have felt that there was simply less time during study rather than insufficient time, hence the comparable selectivity across conditions. While this is certainly a possibility, self-pacing during Segment 3 would presumably have reflected a preference in study closer to the 1-second rate if participants had truly believed it to be adequate, and this was not the case (see Figure 3.2). Even the least valuable items (i.e., 1-point words) received approximately 3 s of study on average, indicating that participants generally considered a 1-second study rate to be far from sufficient. The 5-second study rate, on the other hand, was much closer to the rate at which participants chose to self-pace their study, particularly for those most valuable (10-point) items, which received approximately 6 s of study, on average. Thus, results from Segment 3 confirm not only that the 1-second study rate experienced by some of the participants was inadequate for

proper study, but also that value-based study continued to be evident when participants were able to control the pacing themselves—participants allocated greater lengths of study to increasingly valuable words and preserved their selectivity across lists.

Interestingly, though, there was a consistent decline in the total time that participants spent studying Lists 9–12, coupled with a decline in overall recall. Participants theoretically had unlimited time with which to study the items, so one might expect that study time and recall would actually increase across lists. That this was not the case may have been a result of prior task experience. The steady improvement in selectivity across Lists 1–8 suggests that participants were learning about their memory capacity ("how many items can I remember?") and learning how to study such that the limitations of their capacity were offset by the substance and quality of their recall. During these first eight lists, capacity limitations were, of course, partly based on innate ability, but also on the fact that participants had no more than either 1 or 5 s to study per item, depending upon their assigned condition. Thus, whatever participants learned about their memory capacity was partially contingent upon the limits of the task itself. Participants may have failed to recognize this when self-pacing their own study during Lists 9–12. Theoretically, participants could have studied each item for as long as it took to be fully mastered. If, however, they believed that their prior performance (again, based partly on now irrelevant task characteristics) reflected their upper limit, then there would be little sense in expending further efforts and allocating even greater time to each item. For instance, if a participant believes, based on prior performance, that he or she can recall roughly 12 items and achieve a score in the 60s, then it would be pointless to spend more time beyond what it takes to achieve that level if the participant also believes the probability of exceeding that performance level to be slim—to do so would be to "labor-in-vain"[4] (cf., Nelson & Leonesio, 1988). So,

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although participants clearly did not study the items long enough to improve, or even maintain, their overall recall during Segment 3, they continued to select a length of study that maintained their study efficiency, reducing their study time without jeopardizing their recollection of the most valuable items. This is consistent with active metacognitive judgments during study.

Experiment 1 serves as an early attempt to understand how being short on time can influence one's attempts to remember important or valuable information. Given the previously demonstrated influence of time limitations on memory and self-regulated study (e.g., Son & Metcalfe, 2000), it would not have been particularly surprising had there been a comparable impact on value-directed remembering. Participants in the current study, however, were able to plan, execute, and improve upon a value-directed and selective study strategy in spite of time limitations. They were also able to successfully adapt their acquired strategies to new study times and, when given free reign over study, continued to demonstrate comparably selective recall, with self-regulated study time allocation contingent on item value. Thus, while there are certainly memory-related costs owing to time constraints, the present findings suggest that, under certain circumstances, learners can nevertheless continue to selectively focus on, and remember, the most important information, even if they cannot remember it all.

Experiment 2

Experiment 2 examines whether the maintenance of selectivity and value-based study in Experiment 1—despite time constraints—extends to older adult populations. Aging is typically associated with pronounced declines in executive control mechanisms critical to information processing (Craik, 2002; Hasher & Zacks, 1988; Hess, 2005; Krueger & Salthouse, 2011), as well as working memory capacity and processing speed (Salthouse, 1996, 2000), associative binding (Chalfonte & Johnson, 1996; Naveh-Benjamin, 2000), and inhibitory processes (Hasher

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& Zacks, 1988; Zacks & Hasher, 2006). Despite these declines, though—or perhaps *because* of these declines—healthy older adults seem to become more selective in how they expend their resources, and they are selective in a manner that allows them to get the most out of their resource expenditure (Baltes & Baltes, 1990; Hess, 2014; Riediger & Freund, 2006). In fact, older adults experiencing normal cognitive declines seem quite able to engage in value-directed remembering, with selective performance during study comparable to that of younger adults with intact mechanisms of attention. Although older adults cannot remember as much of the studied information as younger adults, research consistently finds that they are able to comparably remember the most important subsets of information (Castel et al., 2012; Castel et al., 2013; Middlebrooks, McGillivray, et al., 2016).

The younger adults in Experiment 1 successfully adapted to time constraints during study, exhibiting no change in selectivity despite declines in overall memory during constrained study (Middlebrooks, Murayama, et al., 2016). The costs of time limitations to study in terms of remembering valuable information may be more salient, and substantial, to older adults, though, and so they may become *more* selective when the age-related encoding deficits that they already experience are further exacerbated by external factors like time constraints.

On the other hand, older adults' selective study might suffer if time limitations *arise* during the course of the task, requiring quick adjustments to one's study strategy. Prior work suggests that older adults *can* study selectively even when information is presented quickly (e.g., 1-2 seconds per to-be-remembered word) (Castel et al., 2002; Castel et al., 2009), but there may well be a difference in how older adults respond to a notably faster, 1-second presentation rate when it follows a more leisurely, 5-second rate. Additionally, prior research has not directly contrasted older adults' selectivity as a consequence of presentation rate; older adults

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may be able to engage in value-directed remembering despite time limits, but they may do so less efficiently when compared to a slower study pace. Their overarching goal may be to direct their available resources in the most optimal manner, but increased cognitive demands have been shown to undermine older adults' goal-directed behaviors (Knight, Seymour, Gaunt, Baker, Nesmith, & Mather, 2007; Mather & Knight, 2005; but see Allard & Isaacowitz, 2008). Moreover, older adults' generally more limited working memory capacity (Gazzeley, Cooney, Rissman, & D'Esposito, 2005; Hasher & Zacks, 1988) and processing speeds (Salthouse, 1996) may make quickly updating a study agenda more difficult than for younger adults. So, although older adults may make a concerted effort to selectively direct their limited resources towards the most valuable of the to-be-remembered information, factors that further limit these resources, like time constraints during study, may render these efforts at least somewhat unsuccessful.

Method

Participants

Participants consisted of 48 younger adults[11] (38 female, 8 male, 2 unreported) and 48 older adults (28 female, 19 male, 1 unreported). The younger adult participants were undergraduate students from the University of California, Los Angeles, ranging in age from 18 to 26 years ($M = 20.32$, $SD = 1.40$). The older adult participants, ranging in age from 60 to 88 years ($M = 71.48$, $SD = 7.423$, were recruited from the Los Angeles area via fliers posted throughout the community and through the UCLA Cognition and Aging Laboratory participant pool. Younger adult participants received partial credit for a course requirement in exchange for their participation; older adults received monetary compensation at a rate of $10 per hour.

Materials & Procedure

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[11] The younger adult data consists of the original sample collected in Experiment 1 (Middlebrooks, Murayama, et al., 2016).

The materials used were identical to those of Experiment 1. The procedure was also identical, except that the younger and older adults participants were randomly assigned to only one of two study conditions: Constant-Slow [5-5] or Speed Up [5-1]. Given the difficulty in older adult recruitment, it was thought that these two conditions held the most potential for a clear contrast of the effect that being rushed may have on value-directed remembering between age groups. Although the Constant-Fast [1-1] and Slow-Down [1-5] conditions would also address age-related differences in value-directed remembering under time constraints, it would have been difficult to conclude whether differences stemmed from a failure to adopt a selective strategy in the first place or an inability to execute an adopted strategy. A 5-second study rate was still expected to be insufficient for older adults to attain a high enough performance that selective study becomes unnecessary. If older adults' selectivity in the Speed-Up condition suffers when the presentation rate increases to only 1 second per item relative to 5 seconds per item, that would suggest an inability to execute the previously adopted study agenda. Improved selectivity in the Speed-Up condition during Lists 5-8 relative to the Constant-Slow condition, however, would indicate not only an intact ability to study strategically despite more limited resources, but also an ability to continue to refine an adopted strategy.

Results

Overall Recall Performance

A 2(Condition: Constant-Slow [5-5], Speed Up [5-1]) x 2(Age group: younger adults, older adults) x 3(Segment: Lists 1-4, Lists 5-8, Lists 9-12) repeated-measures ANOVA was conducted to verify that the 1-second and 5-second study rates were sufficiently different in terms of encoding and recall between and within the two age groups. The proportion of items recalled as per study time, list, and age group are listed in Table 3.3. Bonferronni adjustments

were made in all cases of multiple comparisons post hoc testing and Greenhouse-Geisser adjustments in the case of sphericity violations.

As anticipated, there was a main effect of age group, $F(1, 92) = 46.95$, $MSE = 0.05$, $p < .001$, $\eta^2_G = .28$ such that older adults recalled significantly fewer items overall ($M = .25$, $SD = .10$) than did the younger adults ($M = .43$, $SD = .16$). There was also a significant Age group x Segment interaction, $F(1.61, 148.21) = 4.12$, $MSE = 0.01$, $p = .03$, $\eta^2_G = .01$. There was a significant main effect of segment for both younger and older adults ($ps < .003$) and younger adults consistently recalled more items than older adults within each segment ($ps < .001$). The increase in overall recall from Segment 2 to 3, however, was significantly greater for younger adults than older adults, $t(80.98) = 2.44$, $p = .02$, $d = 0.51$; the decline in recall from Segment 1 to 2 was similar between age groups, $p = .14$. So, it appears that the freedom to self-pace their study in Segment 3 following Segment 2 (during which half of the sample had only 1 second to study each item) was of greater benefit to younger adults' overall recall than it was to older adults, though they did also improve relative to Segment 2.

Finally, there was a significant Condition x Segment interaction, $F(1.61, 148.21) = 20.73$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .03$. There were no significant differences in recall across segments in the Constant-Slow condition, $p = .28$, but participants in the Speed-Up condition did recall significantly fewer items during Segment 2 (i.e., when study was paced at a 1-second rate) relative to Segments 1 and 3 (i.e., when study was paced at a 5-second rate or self-paced, respectively), $ps < .001$.

Overall, these results confirm that reduced study time led to reduced recall for both younger and older adults and that both age groups devoted sufficient study time during Segment 3 to match (though not surpass) their 5-second study rate performance.

Value-Directed Remembering & Selectivity

To investigate the impact of value on strategic encoding, separate HLM analyses were conducted for each of the three timing segments. The models were largely identical to those of Experiment 1 (see Tables 6-7), except that an Age group variable (0 = *older adults*, 1 = *younger adults*) and an Age x Condition variable were also included as level-2 predictors. The Condition predictor was also entered as a single predictor anchored on the Constant-Slow condition (i.e., 0 = *Constant-Slow*, 1 = *Speed Up*). Table 3.4 reports the tested model and its estimated regression coefficients for each segment's analysis.

Segment 1.

Figure 3.3a depicts Segment 1 recall as per item value, condition, and age group.
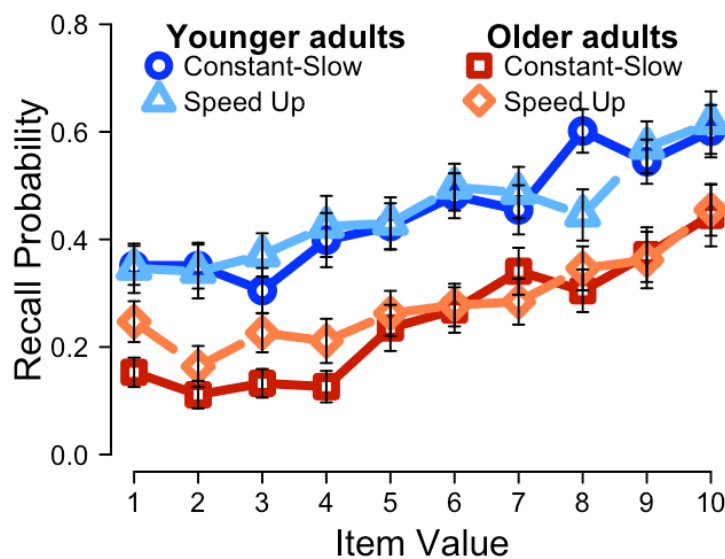
Figure 3.3a. Recall probability in Segment 1 as a function of item value, study condition, and age group in Experiment 2. Error bars reflect standard error.

Value was a significantly positive predictor of recall in the Constant-Slow condition for older adults during Segment 1 ($\beta_{10} = 0.22$, $p < .001$), with no significant differences owing to

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Condition[12] and/or Age group ($ps > .20$). Participants were $e^{0.22} = 1.24$ times more likely to recall an item for each one-unit increase in its value; the odds of recalling a 10-point item during Segment 1, for instance, were thus $e^{0.22*10} = 8.58$ times greater than the odds of recalling a 1-point item.

Older adults' recall was not significantly predicted by List in the Constant-Slow condition ($\beta_{20} = -0.05$, $p = .19$), but there was a significant Age x List interaction, $p = .03$, with younger adults' recall marginally increasing across lists relative to older adults' recall ($\beta = 0.11$, $p = .07$).[13] Note, however, that this interaction was not maintained in a post hoc HLM analysis in which the Constant-Slow and Speed-Up conditions were collapsed within each Age group to reflect the fact that there were no condition differences within the task to this point (i.e., all younger and older adult participants were studying at a constant rate of 5-seconds per item), $p = .27$.

Finally, there was a significant List x Value interaction in the Constant-Slow condition for older adults, such that the relationship between item value and recall probability increased with each successive list ($\beta_{30} = 0.08$, $p < .001$), indicating improved selectivity with continued task experience. This interaction differed by neither Condition nor Age group ($ps > .05$).[14]

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[12] No differences had yet arisen between conditions by this point in the task, so the absence of a significant difference in the value-recall relationship not only makes sense but also confirms that any subsequent differences (or lack thereof) arise from the task manipulations and not underlying group differences.

[13] The simple slope for the List effect in the younger adult age group can be directly calculated by adding the β_{20} and β_{22} coefficients (i.e., $-0.04+(0.14) = 0.10$). To determine the slope's significance, the Age group and the Age group x Condition predictors in the model were recoded (0 = *younger adults* and 1 = *older adults*) (Hayes, 2013). This was done to determine the significance of any reported simple slopes hereafter.

[14] The Age group x List x Variable interaction was marginally significant, $p = .051$. In light of there being no significant differences as a consequence of condition, and the fact that condition differences within the task had yet to arise, a post hoc analysis in which the Constant-Slow and Speed-Up conditions within each age group were pooled (yielding greater statistical power) to result in two younger and older adult conditions rather than four conditions overall indicated no significant differences in the List x Value interaction as a consequence of Age group, $p = .25$.

Segment 2.

Figure 3.3b depicts Segment 2 recall as per item value, condition, and age group.
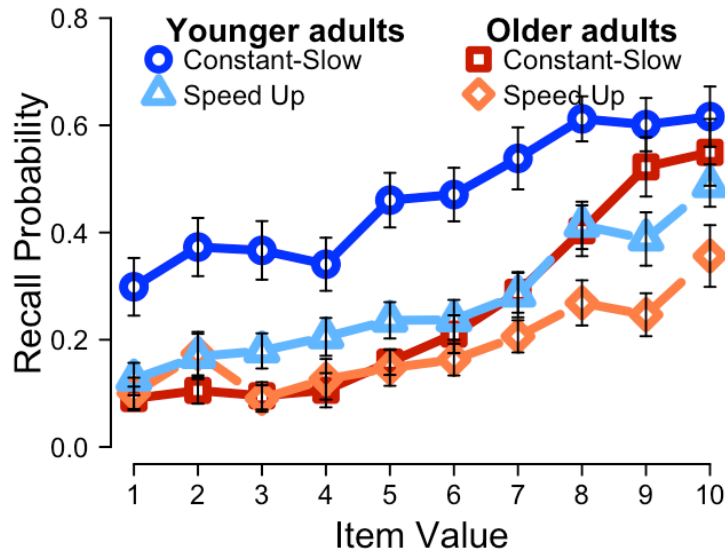
Figure 3.3b. Recall probability in Segment 2 as a function of item value, study condition, and age group in Experiment 2. Error bars reflect standard error.

As in Segment 1, Value was a significantly positive predictor of recall in the Constant-Slow condition for older adults during Segment 2 ($\beta_{10} = 0.39$, $p < .001$). There was, however, a significant Age group x Value interaction ($\beta_{12} = -0.19$, $p = .01$), such that younger adults' recall was significantly less driven by value than older adults' recall. Older adults in the Constant-Slow condition were $e^{0.39} = 1.47$ times more likely to recall an item for each one-unit increase in its value while younger adults in the Constant-Slow condition were $e^{0.19} = 1.21$; the odds of an older adult in the Constant-Slow condition recalling a 10-point item during Segment 2, for instance, was thus $e^{0.39*10} = 47.76$ times greater than the odds of recalling a 1-point item, but only 6.91 times greater for younger adults in the Constant-Slow condition.

There was also a significant Condition x Value interaction ($\beta_{11} = -0.17$, $p = .01$), wherein older adult participants in the Speed-Up condition were significantly less selective than older adults in the Constant-Slow condition. Older adults in the Constant-Slow condition were $e^{0.39} =$

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1.47 times more likely to recall an item for each one-unit increase in its value while older adults in the Speed-Up condition were $e^{0.21} = 1.23$.

Additionally, there was a significant Age x Condition x Value interaction ($\beta_{13} = 0.21$, $p = .05$). As indicated, older adult participants were significantly more selective than younger adult participants in the Constant-Slow condition, but there was no significant difference between age groups in the Speed-Up condition ($\beta = 0.02$, $p = .83$). Likewise, the superior selectivity demonstrated by older adults in the Constant-Slow condition relative to the Speed-Up condition was not emulated by younger adult participants, who did not differ in their selectivity between conditions ($\beta = 0.03$, $p = .58$)

So, consistent with Experiment 1, younger adults were just as selective under a 1-second study rate as a 5-second study rate. Older adults, however, were more selective under the slower study rate than the faster rate *and*, under the slower rate, were more selective than younger adults.

Segment 3.

Figure 3.3c depicts Segment 3 recall as per item value, condition, and age group.
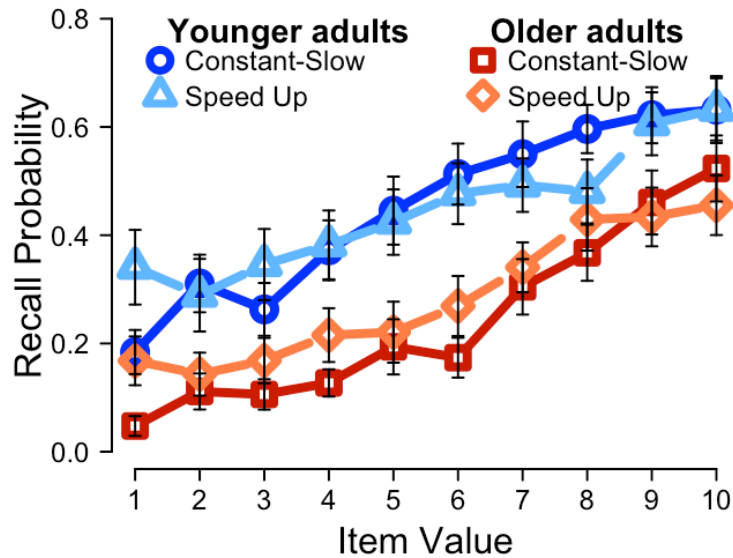
Figure 3.3c. Recall probability in Segment 3 as a function of item value, study condition, and age group in Experiment 2. Error bars reflect standard error.

Value was once again a significantly positive predictor of recall in the Constant-Slow condition for older adults during Segment 3 (during which study was self-paced) ($\beta_{10} = 0.43$, $p < .001$), with (contrary to Segment 2) no significant between older adults in the Constant-Slow and Speed-Up conditions ($p = .12$). There was also a List x Value interaction ($\beta_{30} = 0.04$, $p = .048$)— which did not differ between Age groups or Conditions, ps > .27—indicating increasing selectivity across Lists 9-12.

There was, however, a significant Age group x Value interaction ($\beta_{12} = -0.20$, $p = .04$), such that younger adults' recall in the Constant-Slow condition was once again less driven by value than older adults' recall in the Constant-Slow condition ($\beta = 0.23$, $p < .001$). This result should be interpreted cautiously, however, as Age group differences were not significant in the Speed-Up condition but the Age group x Condition x Value interaction term was not significant. In other words, the Age group x Value interaction was apparently significant or not as a consequence of the coding of the Condition reference group.

There was also an Age group x List interaction (β_{22} = -0.16, p = .03) with no qualifying Age group x List x Condition interaction (p = .23): Younger adults' recall—while still greater than older adults' overall (see the previous "Overall recall performance" analyses)—significantly decreased across Lists 9-12 (β = -0.21, p < .001) but older adults' recall remained stable (β_{22} = -0.05, p = .18).

These results indicate that older adults who had previously studied at an insufficient study rate of 1-second per item were nevertheless able to adapt their value-based study and self-pace their study when released from the time constraint such that their attention to and recall of high-value information was not impeded by their prior study experiences. Older adults may, however, have been learned more about their limitations and the ways in which they should allocate their resources than younger adults as a consequence of the task given their continued superiority in value-based recall despite the removal of time constraints (although, again, this finding should be interpreted cautiously).

Self-regulated study.

Figure 3.4 illustrates the average proportion of total study time spent per item value during Segment 3, as well as the average study time per item value, owing to age group and study condition.
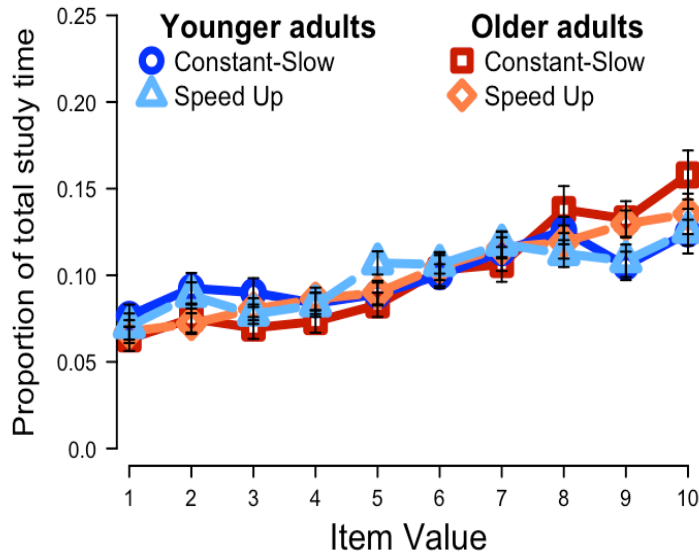
(a)
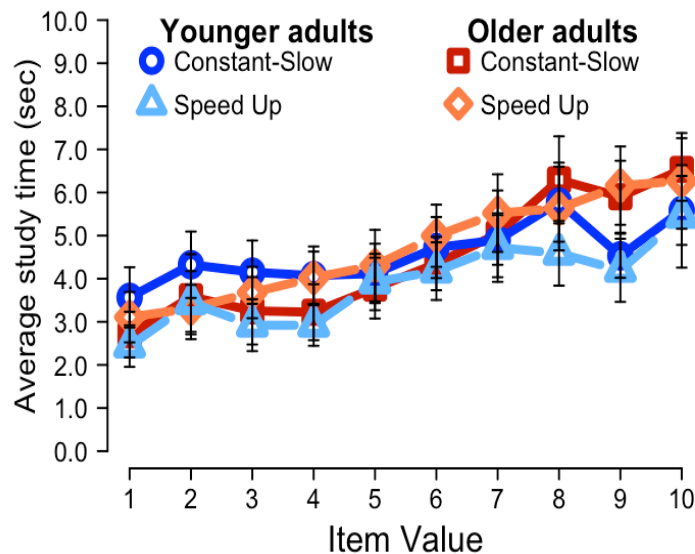
(b)

Figure 3.4. (a) The average proportion of self-paced study time and (b) the average study time (in seconds) allocated to each item value across assigned study conditions during Segment 3 in Experiment 2. Error bars reflect standard error.

As in Experiment 1, the self-paced nature of Segment 3 meant that each participant spent a different amount of time studying Lists 9-12 overall and each item within the lists. Accordingly, HLM analyses were again implemented, with item-level study time (in seconds) modeled as a function of each item's value, the list in which it was presented, and the interaction between value and list. The model further included the Age group (0 = *older adults*, 1 = *younger adults*),

Condition (0 = *Constant-Slow*, 1 = *Speed Up*), and Age x Condition variables as level-2

predictors. Table 3.4 reports the tested model and its estimated regression coefficients.

Value significantly predicted study time allocation across the items within the lists, with

0.44 additional seconds spent studying words per each one-unit increase in its value (β_{10} = 0.44,

$p < .001$). Consistent with Experiment 1, there was also a significant effect of List on study time

(β_{20} = -0.34, p = .02), such that significantly less time was spent studying each successive list

overall, with no differences owing to Age group and/or Condition. There were no other

significant differences in self-pacing owing to Age, Condition, List, or Value.

Discussion

In Experiment 1, younger adults' prioritization of high-value items during study was not

influenced by the duration of allotted study time (Middlebrooks, Murayama et al., 2016).

Moreover, younger adults were able to adapt their value-based study to accommodate changes in

study time, and they maintained their value-based strategizing when pacing was under their

control rather than that of the experimenter, with no evident influences of prior experience with

time constraints on strategy efficiency. Experiment 2 investigated whether older adults similarly

adapt to time limitations when attempting to remember high-value information despite

documented age-related declines in processing speed and executive control (e.g., Craik, 2002;

Hasher & Zacks, 1988; Hess, 2005; Salthouse, 1996, 2000).

The results of Experiment 2 indicate that older adults' selectivity did not suffer as a

consequence of shifting to the limited study time in the Speed-Up condition relative to younger

adults, but—by Segment 2—they did benefit from the longer study time in the Constant-Slow

condition, demonstrating greater selectivity than that of fellow older adults in the Speed-Up

condition *and* that of younger adults studying at the same 5-second study rate. So, while it was

not the case that time constraints were costly to older adults' value-based study if comparing across age groups, they were evidently costly if compared to what older adults can (and will) do when given more time. There was no such distinction, however, for younger adults: Younger adults were comparably selective regardless of the amount of study time at their disposal.

The shift from a 5-second study rate to the 1-second rate may, indeed—in light of limits to working memory capacity (Gazzeley, Cooney, Rissman, & D'Esposito, 2005; Hasher & Zacks, 1988) and processing speed declines (Salthouse, 1996)—have increased cognitive demands such that older adults were less able than they might otherwise have been to optimally enact a selective study strategy and compensate for their general declines in memory. Nevertheless, the fact that this lesser efficiency was relative to older adults with more study time and not relative to their younger counterparts is consistent with prior research suggesting that older adults maintain an overarching goal of selective optimization with compensation (Baltes & Baltes, 1990) in that they become more selective in how they expend their relatively limited resources in such a manner as to allow them to get the most out of their resource expenditure (Riediger & Freund, 2006).

Prior research generally indicates that older adults are just as selective as younger adults despite recalling fewer items overall (e.g., Castel et al., 2002, 2012, 2013; Middlebrooks, McGillivray, et al., 2016). The results of Experiment 2, however, suggest that older adults may, in fact, be generally *more* selective than younger adults and that task conditions (such as presentation rate) may hinder their ability to enact a selective strategy to the extent that they might otherwise have done. This raises concerns when considering the fairly unrepresentative nature of the selectivity task specifically and many experimental designs, in general. Presentation rate was a variable of interest in Experiment 2, but that is certainly not always the

case and, evidently, the chosen rate of presentation can independently have a pronounced effect on older adults' strategic engagement during study.

Although healthy aging is inarguably associated with declines in cognitive performance, the consequences of these declines as demonstrated in research settings may not generalize as cleanly to real-world situations as oft discussed. The disparity between experimental and real-world task demands—as in the present experiment—has been shown to prevent older adults from fully engaging in strategies that they might otherwise regularly employ to compensate for normal memory declines (Cavanaugh, Grady, & Perlmutter, 1983; Verhaeghen, Martin, & Sędek, 2012). If aiming to understand how people attempt to compensate for their inability to remember everything so as to remember the most important or valuable information; how these compensation attempts change across the lifespan; and how they are influenced by various factors impeding encoding and recall (viz. limited encoding time in this particular case), it is critical to ensure that the experimental design itself is not unduly influencing performance.

Given general age-related cognitive deficits, it is perfectly conceivable that older adults' superior selectivity in the case of the slower, 5-second presentation rate would not be immutable in the event of further cognitive stressors, such as distractors during study or feelings of stereotype threat (Régner, Smeding, Gimmig, Thinus-Blanc, Monteil, & Hugert, 2010; Schmader, Johns, & Forbes, 2008). Younger adults may not be as naturally selective owing to their generally being able to remember a fair amount of information with relatively minimal effort (especially as compared with older adults), but they may also be better able to withstand increased demands to their central executive. For instance, prior research indicates that younger adults are just as selective when studying under full attention as when studying while listening to background music or while completing a demanding concurrent task (Middlebrooks, Kerr, et al.,

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2017). Older adults may be generally more inclined to engage in compensatory strategies and prioritize high-value information; given that a reduction in study time prevented older adults from achieving the selectivity that they otherwise could have, though, further increases on their cognitive resources could undermine these efforts (Dunlosky & Ariel, 2011a; Knight et al., 2007; Mather & Knight, 2005) such that younger adults' selectivity is ultimately greater.

Summary

With respect to the influence of time constraints specifically during study on value-directed remembering, documented age-related differences in processing speeds and executive control evidently did not impair older adults' ability to adopt and execute a selective, value-based study strategy relative to younger adults, despite having previously experienced a slower, more comfortable 5-second study rate. They did, however, impair older adults' selectivity relative to older adults studying at a slower pace, indicating that time constraints can influence selective study (contrary to the results of Experiment 1; Middlebrooks, Murayama, et al., 2016) under certain conditions.

Experiment 3

Many real-world, time-sensitive situations that might benefit from selectivity are likely to be accompanied by feelings of anxiety. These feelings may stem from a variety of sources, including a fear of consequences associated with forgetting the information at hand (e.g., forgetting studied information before an important exam) or from social pressures (e.g., forgetting important details about an acquaintance; forgetting more information than one's peers). Anxiety can also be a direct consequence of time pressure itself, wherein the speed of the task results in a feeling of being somewhat overwhelmed. Experiment 3 aims to differentiate between the effects (or evident lack thereof) of time constraints themselves and potential effects

of any feelings of anxiety/pressure *resulting* from the time constraints on value-directed remembering during study.

Although selectivity was not impacted by study time limitations in Experiment 1, feelings of stress/anxiety have been shown to markedly impair cognitive performance (Ashcraft & Kirk, 2001; Hembree, 1988; Pan & Tang, 2005; Schmader et al., 2008; Veenman et al., 2000) and prefrontal cortical functioning (Arnsten, 2010). Pressures (e.g., social, financial) arising during study are further thought to distract learners and obstruct their ability to apply explicit study strategies owing to increased demands on executive control mechanisms (Beilock & Carr, 2005; Beilock, Kulp, Holt, & Carr, 2004; Markman, Maddox, & Worthy, 2006; Régner et al., 2010). Time limitations during study may have a more pronounced influence on value-directed remembering when their effects are internalized in a more visceral or emotional sense, thereby decreasing the cognitive resources available for such strategizing (Eysenck & Calvo, 1992; Eysenck, Derakshan, Santos, & Calvo, 2007). Contrastingly, if feeling anxious or limited encourages the adoption of compensatory strategies (Eysenck et al., 2007), then feeling rushed might lead to *enhanced* selectivity owing to a mobilization, so to speak, of remaining cognitive resources.

In an effort to induce a feeling or sense of being rushed, the rushed (i.e., 1-second) timing portions of Experiment 1 were altered such that participants were responsible for self-pacing their study, but the rushed experience was induced via penalties to their final score for extended study coupled with a flashing red timer and verbal prompting by the experimenter to study faster. The intent behind these adjustments to Experiment 1 is two-fold: Firstly, they should make the constrained nature of the study periods even more salient to participants. In Experiment 1, the pacing of the study period may have been considered more a characteristic of the task than a

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constraint per se. Self-imposed limits to study, however, should make it more apparent to participants that, in the absence of penalties, they would otherwise have chosen to study an item for a longer duration. Feelings of being rushed could also arise more strongly when the conflict between how much time one would like to devote to something relative to how much time one actually devotes is a direct consequence of self-imposed limitation, as opposed to external limitations like in Experiment 1. Secondly, placing the responsibility of pacing with participants should more closely mimic real-world situations in which learners must self-regulate their study within the boundaries of inevitable time constraints and judge the relative worth of devoting more time to certain subsets of information in spite of outside pressures.

Regulatory Fit Hypothesis

If selectivity is affected by the time constraints in Experiment 3, it is possible that such impairment would be a consequence of regulatory fit rather than exacerbated stress to the central executive. According to the regulatory fit hypothesis, one's motivation to engage in a task, and the choices one makes while engaged, are influenced by the extent to which there is a match between the task goal and one's goal orientation (Aaker & Lee, 2006; Avnet & Higgins, 2006; Barber & Mather, 2013; Förster, Higgins, & Idson, 1998; Higgins, 1997; Shah, Higgins, & Friedman, 1998). The fundamental goal in the selectivity task is to maximize one's score (i.e., to gain points), but the inclusion of a penalty system and explicitly noting the threat that extended study poses to one's final score may instead trigger a prevention focus, or a superordinate goal of avoiding a loss of points. If time pressure also invokes a loss- or prevention-focus, wherein rushed participants focus more on avoiding point deductions than on point acquisition, enhanced selectivity when rushed could be a consequence of a match between one's goal and goal orientation rather than the intentional adoption of a compensatory, value-based study strategy.

Alternatively, the method by which time pressure is induced in Experiment 3 may invoke a gains- or promotion-focus; in the event that selectivity is impaired when rushing, it could be because of costs to the central executive, but it could also be because of a mismatch between participants' goals and the pressure-induced goal orientation.

Because it is unclear whether time pressure in Experiment 3 will result in a loss or gains focus (but see Worthy, Markman, & Maddox, 2009 for evidence of induced prevention-focus owing to social pressures), the instructions in Experiment 3A adopted a loss focus and in Experiment 3B a gains focus. If time pressure invokes a loss focus, selectivity should be enhanced by rushing in Experiment 3A and impaired in Experiment 3B; if time pressure invokes a gains focus, the opposite should occur. Consistently enhanced/impaired selectivity across Experiments 3A and 3B would be inconsistent with a regulatory fit explanation and more consistent with a central executive contribution.

Experiment 3A

Method

Participants

Experiment 3A consisted of 96 undergraduate (73 female) student participants from the University of California, Los Angeles, ranging in age from 18 to 30 years ($M = 20.20$, $SD = 1.97$). Participants received partial credit for a course requirement.

Materials & Procedure

The materials in Experiment 3A were identical to those of Experiment 1. The procedure was also similar to Experiment 1 in that there were four study conditions: Constant-Fast [1-1]; Constant-Slow [5-5]; Speed Up [5-1]; and Slow Down [1-5]. When studying at a slower rate, the to-be-remembered items were presented at a rate of 5 seconds per word. During those lists in

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which participants in Experiment 1 studied at a 1-second presentation rate, though, participants in Experiment 3 self-paced their study. The rushed experience was induced via speed penalties—the loss of 1 point from the participant's final score for each additional second spent studying per item—verbal reminders to study faster from the experimenter, and the presence of a timer on the screen that rapidly flashed red if an item remained on the screen for more than 1 second.

Participants in Experiment 3A received loss-focused instructions, which are consistent with the instructions generally used in value-directed remembering tasks albeit somewhat more heavy-handed. Prior to the first list of rushed study, participants were told that they would be penalized for extended study, with one point lost for every second spent studying an item beyond 1 second. They were told to approach the task as though they are starting with 110 points (the sum value of all items presented within each list). In addition to losing points for each word that they failed to correctly recall at test, they were told that they would also lose points if they had studied a recalled item for longer than 1 second, despite having correctly recalled it. Participants were provided an example:

> "For instance, let's say that you studied "apple : 10" and you later recall apple at test. If you studied apple for 1 second, you will receive all 10 points. If you studied apple for 2 seconds, though, you will receive 9 points instead of the original 1. If you studied apple for 4 seconds, you will only receive 7 points for correctly recalling it."

The verbal reminders to study faster voiced by the experimenter in the room also incorporate a loss-focus (e.g., "You're losing points for each extra second."; "Your score is going to decline if you don't study faster."). The average number of verbal reminders provided by the experiment to participants per list and study condition is provided in Table 3.5.

Instructions in the Constant-Slow condition, during which there was no period of penalty-oriented self-pacing, was modified such that participants were only told to think of the task as

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though they are starting with 110 points and losing points for word which they fail to recall at test. The experimenter sat in the room with the participants in the Constant-Slow condition (but did not speak during the task in light of the absence of periods of rushing) so as to keep the social presence element constant across conditions.

As in Experiment 1, Lists 9-12 were entirely self-paced sans penalties (which was made clear to participants) across study conditions.

Results

Overall Recall Performance

Analyses were first conducted to determine whether there was an effect of study condition on overall recall, irrespective of item value. Table 3.6 lists the proportion of items recalled as per study condition and list.

A 4(Study time condition: Constant-Fast [1-1]; Constant-Slow [5-5]; Speed Up [5-1]; Slow Down [1-5]) x 3(Segment: Lists 1-4, Lists 5-8, Lists 9-12) repeated-measures ANOVA on total recall revealed a significant Condition x Segment interaction, $F(5.00, 153.38) = 23.11$, *MSE* $= 0.01$, $p < .001$, $\eta^2_G = .15$. There was a significant effect of Condition within Segment 1 and Segment 2, $ps < .001$, but not Segment 3, $p = .58$. Within Segment 1, the Constant-Fast and Slow-Down conditions recalled significantly fewer items than the Constant-Slow and Speed-Up conditions, $ps < .001$. There were neither significant differences between the Constant-Fast and Slow-Down conditions nor between the Constant-Slow and Speed-Up conditions, $ps > .27$. Thus, conditions in which participants self-paced their study under a 1-second penalization policy recalled significantly fewer words than those in which participants studied at a 5-second rate. The same pattern emerged in Segment 2: Participants in the Constant-Fast and Speed-Up conditions (each studying under a 1-second penalization rate) recalled significantly fewer words

than the Constant-Slow and Slow-Down conditions (5-second study rate), $ps < .001$, and there were no significant differences between conditions studying at the same rate, $ps > .37$.

These results—consistent with those of Experiment 1—confirm that participants' recall was impaired by the 1-second penalization study rate despite the fact that they could have chosen to study for a longer and potentially matched the 5-second study conditions' performance.

Rushed Self-Pacing Overall

The total time participants spent studying the 20 items in each list during the 1-second penalization study periods and the fully self-paced (i.e., penalization-free) Lists 9-12 is provided in Table 3.8. Two analyses were conducted to examine participants' overall pacing behaviors during the 1-second penalization study periods. Firstly, the total time spent studying the 20 items within each list (in seconds) was compared in a 4(Condition: Constant-Fast [Seg1][15], Constant-Fast [Seg2], Slow Down [Seg1], Speed Up [Seg2]) x 4(List) repeated-measures ANOVA to determine whether participants differed in their (penalized) self-pacing as a consequence of prior experiences in the task and continued experience with pacing-under-penalization. Did experience with a slower, 5-second study rate make it more difficult for Speed-Up participants, for instance, to abide by the 1-second study rate during Segment 2 than it was for Constant-Fast participants in Segment 2 (who had only ever experienced 1-second pacing) or for Slow-Down and Constant-Fast participants in Segment 1 (who had had no prior task experience)?

A subsequent analysis was conducted contrasting the penalization periods in each condition against a constant 1-second pace to determine whether participants, on average, spent longer than (or less than) 20 seconds per list when given the opportunity to self-pace their study

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[15] "[Seg1]" refers to Lists 1-4 and "[Seg2]" to Lists 5-8. Note that the Constant-Fast condition is included in this particular analysis twice because participants studied under the 1-second penalization instructions for the duration of Lists 1-8.

under rushed conditions. These analyses were further conducted to determine whether differences in overall recall between the 1-second and 5-second conditions were a consequence of self-imposed abidance by the 1-second rate or whether recall impairment was present in spite of prolonged (penalized) study.

In the first analysis, there was a significant Condition x List interaction on the total time spent studying (in seconds) during periods of penalized self-pacing, $F(6.08, 186.38) = 4.16$, $MSE = 13.83$, $\eta^2_G = .02$, $p = .001$. There was a significant effect of Condition in the first rushed list, $F(3, 92) = 3.30$, $\eta^2_G = .06$, $p = .02$, such that participants in the Speed Up condition (during Segment 2) ($M = 27.41$, $SD = 7.95$) spent more time studying than participants in the Constant-Fast condition during the same segment ($M = 20.92$, $SD = 6.43$), $p_{adj} = .02$. There were no other significant differences among the conditions and/or across lists. These results suggest that the Speed-Up participants took more time to study (and thus experienced greater penalization) during the first list of rushed study (List 5) following the pacing transition than Constant-Fast and Slow-Down participants had in their first list of penalized self-pacing (i.e., List 1). Nevertheless, Speed-Up participants quickly acclimated and their pacing came to resemble that of the other two groups of participants who had not previously experienced experimenter-paced study.

Because of the comparability in pacing across conditions and list (with the exception of List 5 in the Speed-Up condition), a one-sample t-test was then conducted to compare average self-pacing across lists and across conditions against a set pace of 20 seconds per list. The results of this analysis indicated that rushed participants who self-paced their study, albeit under a state of penalization, spent significantly more time studying ($M = 22.02$, $SD = 6.41$) than the 20 seconds allotted to participants in the fast conditions of Experiment 1, a pace which would have

resulted in no penalization, $t(71) = 2.67$, $p = .01$.[16] A subsequent one-sample t-test confirmed,

however, that participants who self-paced when rushed still spent significantly less time studying

than those who studied at an experimenter-paced rate of 5 seconds per item, $t(71) = -102.22$, $p <$

.001.

Value-Directed Remembering

As in Experiment 1, item-level recall performance (based on a Bernoulli distribution,

with 0 = *not recalled* and 1 = *recalled*; level 1 = items; level 2 = participants) using HLM was

modeled as a function of each item's value, the list in which it was presented, and the interaction

between value and list within each timing segment. Value and List were entered as group-mean

centered variables, with Value anchored on the mean value point (5.5) and List anchored on the

mean list of the respective segment. The model further included the study conditions as level-2

predictors of those level-1 effects via three dummy-coded variables, with the Constant-Slow

condition [5-5] as the reference group. Table 3.7 reports the tested model and its estimated

regression coefficients per segment analysis.

    Segment 1.

    Figure 3.5a depicts Segment 1 recall as per item value and assigned study condition.

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[16] These results are replicated if only considering the first four rushed lists within each of the conditions (i.e., excluding Lists 5-8 in the Constant-Fast condition).

Segment 1: Lists 1-4
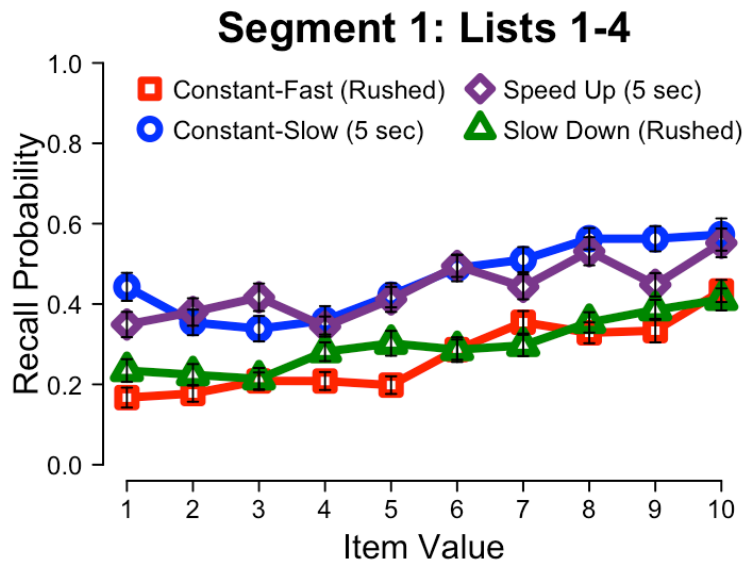
Figure 3.5a. Recall probability in Segment 1 as a function of item value and assigned study condition in Experiment 3A. Error bars reflect standard error.

Value was a significantly positive predictor of recall performance in the Constant-Slow condition ($\beta_{10} = 0.12$, $p < .001$) during Segment 1, and this relationship did not significantly differ across conditions, $ps > .39$. In other words, participants across conditions were $e^{0.12} = 1.13$ times more likely to recall an item for each one-unit increase in its value than they were to forget it. The odds of recalling a 10-point item during Segment 1, for instance, were $e^{0.12*10} = 3.27$ times greater than the odds of recalling a 1-point item.

There was a significant effect of List in the Constant-Slow condition ($\beta_{20} = 0.17$, $p = .003$), and there was a significant cross-level interaction between List and Condition, whereby List had an increasingly reductive effect on total recall relative to the Constant-Slow condition, irrespective of item value, in the Constant-Fast condition ($\beta_{21} = -0.20$, $p = .004$) and Slow-Down conditions ($\beta_{23} = -0.14$, $p = .04$) (i.e., those participants studying at a self-paced 1-second penalization study rate).

The List x Value interaction was not significant in the Constant-Slow condition, $p = .24$, and there were no evidence of List x Value x Condition interactions, $ps > .08$.

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Segment 2: within-subject timing shift.

Figure 3.5b depicts Segment 2 recall as per item value and assigned study condition.
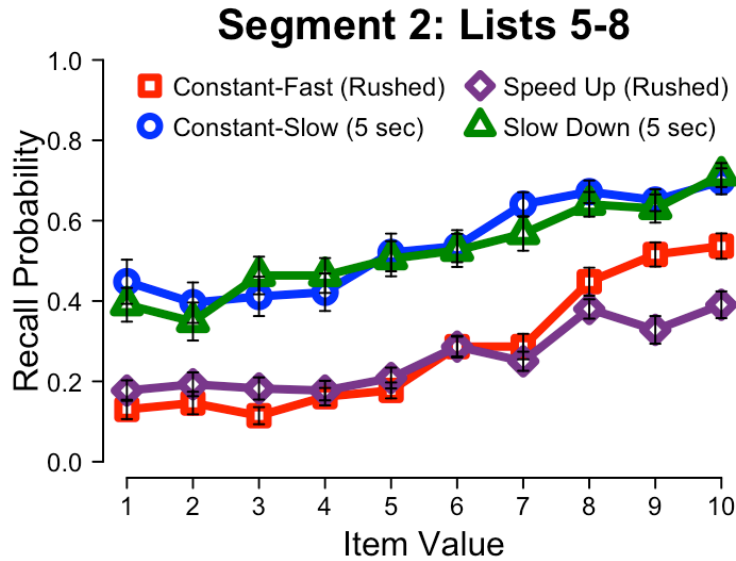
Figure 3.5b. Recall probability in Segment 2 as a function of item value and assigned study condition in Experiment 3A. Error bars reflect standard error.

As in Segment 1, Value positively predicted recall performance in the Constant-Slow condition during Segment 2 ($\beta_{10} = 0.18$, $p = .002$). There was a marginal Condition x Value interaction, such that recall in the Constant-Fast condition was driven by Value to a greater extent than in the Constant-Slow condition ($\beta_{11} = 0.14$, $p = .059$). There were no significant differences in the effect of Value between the Constant-Slow condition and either the Speed-Up or Slow-Down conditions, $ps > .86$. Participants in the Constant-Fast condition were $e^{0.33} = 1.38^{17}$ times more likely to recall an item for each one-unit increase in its value than they were to forget it; participants in the other three conditions were $e^{0.18} = 1.20$ times more likely.

The effect of Value in the Constant-Slow condition was maintained across lists ($\beta_{30} = -0.01$, $p = .67$) and this did not significantly differ in either the Speed-Up or Slow-Down

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[17] The simple slope for the Constant-Fast condition can be directly calculated by adding the β_{10} and β_{11} coefficients (i.e., 0.184+(0.142) = 0.326). To determine whether this slope is significant, the Condition predictor in the model was recoded, such that the Constant-Fast condition served as the reference group (Hayes, 2013). Note that this was also done to determine the significance of any reported simple slopes hereafter.

conditions, $ps > .19$. There was, however, a significant List x Value x Condition effect, such that participants were increasingly attentive to item value across lists in the Constant-Fast condition ($\beta_{11} = 0.07$, $p = .01$) relative to the Constant-Slow condition. There was not a significant effect of List alone on recall across conditions ($ps > .55$).

Thus, attention to value across conditions was at least maintained from Segment 1 to Segment 2, if not slightly increased ($\beta_{10} = 0.12$ versus 0.18). Participants who studied at a rushed but self-regulated pace for 8 lists, however, demonstrated greater selectivity than those who studied at either a 5-second rate exclusively or who alternated between the two pacing conditions.

Segment 3: self-regulated study.

Figure 3.5c depicts Segment 3 recall as per item value and assigned study condition.
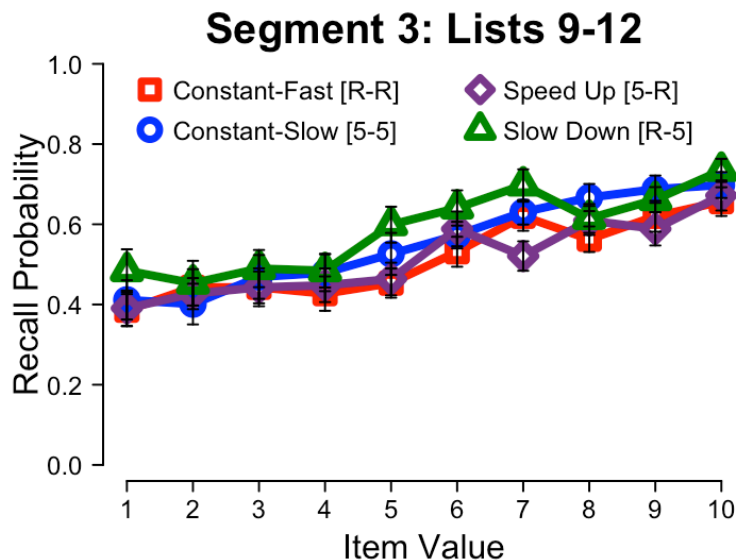
Figure 3.5c. Recall probability in Segment 3 as a function of item value and assigned study condition in Experiment 3A. Study during Segment 3 was self-paced without penalization. Error bars reflect standard error.

All participants self-paced their study of the items in Lists 9-12; participants were free to study each item for as long as they wished without penalization for studying in excess of a 1-second rate. As in Segments 1 and 2, Value significantly and positively predicted recall in the Constant-

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Slow condition ($\beta_{10} = 0.20$, $p = .001$), with no significant differences across the other study conditions, ps > .48. Participants were $e^{0.20} = 1.23$ times more likely to recall an item for each one-unit increase in its value than they were to forget it. Again, this effect was slightly greater than in either of the previous study segments ($\beta_{10} = 0.12$ versus 0.18 versus 0.20), indicating generally increasing attention to value as the task progressed; there was not, however, a significant List x Value interaction or List x Value x Condition interaction, indicating no change in the effect of Value across Lists 9-12. Ultimately, these results indicate that differing prior experiences across the conditions with respect to the allotted study times and penalizations did not impact self-regulated study with respect to item value: Participants across all conditions were similarly selective and maintained this value-based selectivity across Lists 9–12.

Note that there was also a significant List x Condition interaction ($\beta_{21} = 0.20$, $p = .01$), such that total recall increased across Lists 9-12 in the Constant-Fast condition ($\beta = -0.17$, $p = .01$), and this effect was significantly greater than in the other three study conditions, ps < .01, in which there was *not* a significant effect of List. So, those who had studied at a consistent 1-second penalization rate during Lists 1-8 recalled an increasing number of items across Lists 9-12, while those who studied under fluctuating timing conditions or at a constant 5-second rate during Lists 1-8 maintained their List 9 recall across the final lists. In light of the aforementioned nonsignificant effect of Condition on overall recall, this is consistent with participants in the Constant-Fast condition initially recalling fewer items (List 9) and ultimately recalling as many items as participants in the other study conditions by List 12 (see Table 3.6).

Self-Regulated Study

Rushed self-pacing.

Figure 3.6 illustrates the average proportion of total study time and the average total study time, respectively, spent per item value during Segment 1 and 2.

(a)

(b)
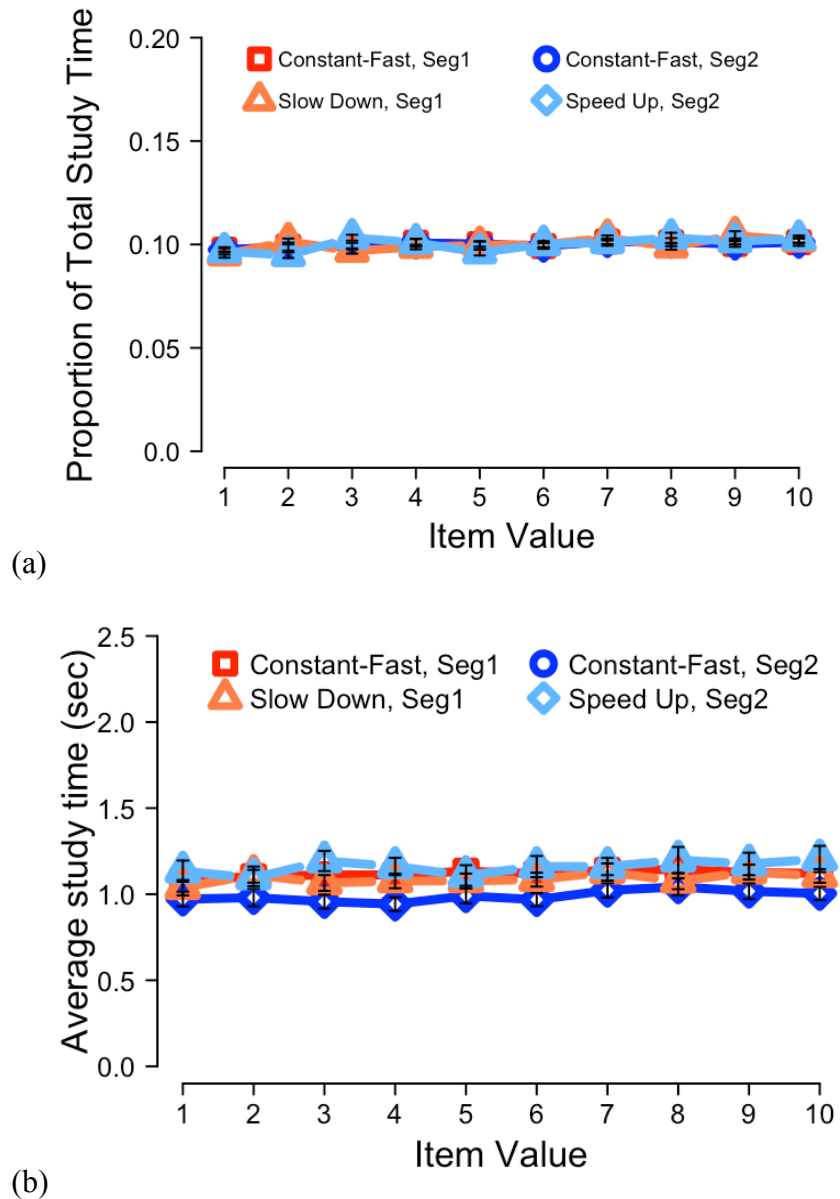
Figure 3.6. (a) The average proportion of self-paced study time and (b) the average study time (in seconds) allocated to each item value across assigned study conditions during the penalization periods of Segment 1 and Segment 2 in Experiment 3A. Error bars reflect standard error.

Although point penalties were invoked for study in excess of 1 second for any given item, participants were free to study for as little or as long as they wished. A strategic participant might consider it worth the loss of 2 points if studying a 10-point item for an extra 2 seconds means that it the item is ultimately recalled; if 2 additional seconds means that an item is recalled that would not have otherwise been, then a sum gain of 8 points (10 points for the item plus a 2-point study penalization) is certainly better than 0 points, even if that 8 points could have conceivably been 10 points.

As in Experiment 1, HLM analyses were again implemented to determine how (or if) participants considered item value in allotting their study time in light of the 1-second penalizations. Item-level study time (in seconds) during Segment 1 and Segment 2 was separately modeled per segment as a function of each item's value, the list in which it was presented within its respective segment, and the interaction between value and list. The model further included the relevant study conditions within each segment as level-2 predictors of these level-1 effects (in Segment 1: 0 = *Constant-Fast*, 1 = *Slow Down*; in Segment 2: 0 = *Constant-Fast*, 1 = *Speed Up*). Table 3.9 reports the tested model and its estimated regression coefficients.

There was a List x Value interaction in Segment 1, such that participants became significantly more selective in their study time allotment across Lists 1-4 ($\beta_{30} = 0.01$, $p = .04$). Note, however, that this effect is *very* small. Similarly, there was a significant but *very* small effect of Value on self-pacing in Segment 2 ($\beta_{10} = 0.01$, $p = .03$), such that participants spent only 0.01 seconds more studying for each one-unit increase in assigned value. So, although statistically detectable effects, there is little evidence for truly strategic rushing: participants do not appear to have accepted the cost of small penalizations in exchange for prolonged study of the highest-valued items and thus a probability of overall gain in total points at test.

Self-regulated study: Lists 9-12.

Figure 3.7 illustrates the average proportion of total study time and the average total study time, respectively, spent per item value during the fully self-paced (i.e., penalization-free) lists of Segment 3.
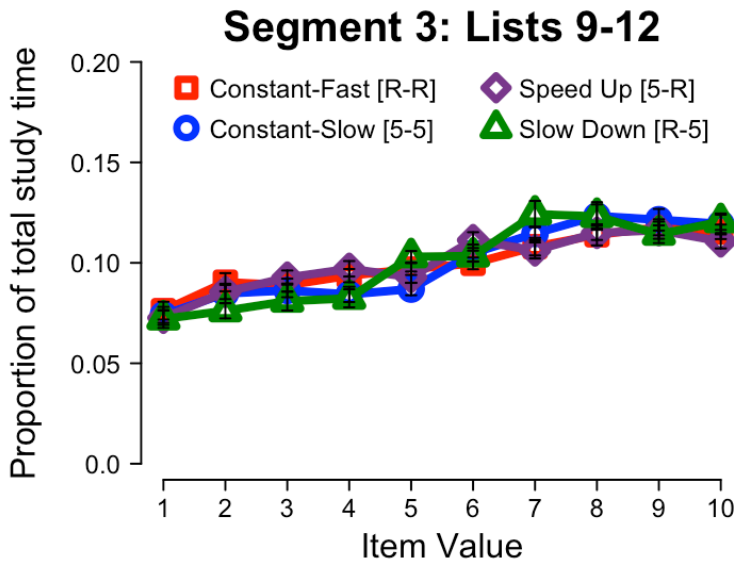
(a)
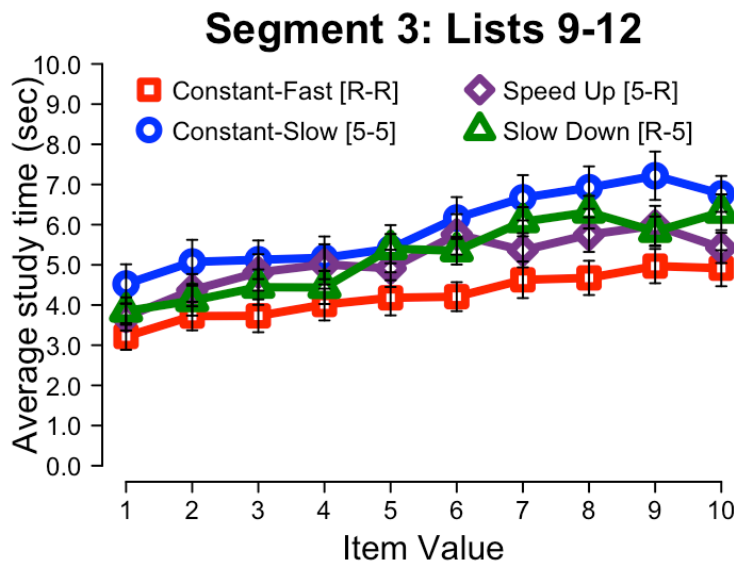
(b)

Figure 3.7. (a) The average proportion of self-paced study time and (b) the average study time (in seconds) allocated to each item value across assigned study conditions during Segment 3 in Experiment 3A. Study during Segment 3 was self-paced without penalization. Error bars reflect standard error.

The HLM model was identical to that of Experiment 1, with item-level study time (in seconds) modeled as a function of each item's value, the list in which it was presented, and the interaction between value and list. The four study conditions were included as level-2 predictors of these level-1 effects via three dummy-coded predictor variables, with the Constant-Slow [5–5] study condition as the reference group. Table 3.9 reports the tested model and its estimated regression coefficients.

As in Experiment 1, there was a significant effect of Value on study time in the Constant-Slow condition, with no significant differences across conditions, $ps > .27$; participants spent 0.30 more seconds studying words with each one-unit increase in assigned value ($\beta_{10} = 0.30$, $p = .001$). There was also a significant effect of List on study time in the Constant-Slow condition ($\beta_{10} = -0.53$, $p = .001$), such that significantly less time was spent studying each successive list overall. This pattern was also evident in Experiment 1, and was consistent in the Speed-Up and Slow-Down conditions. There was, however, a List x Condition effect, such that participants in the Constant-Fast condition did not demonstrate a List effect on their overall self-pacing ($\beta = -0.06$, $p = .70$). As illustrated in Figure 3.8, participants in all but the Constant-Fast condition demonstrated a sharp decline in total study time while those in the Constant-Fast condition maintain a steady (and notably faster) rate of study across Lists 9-12.
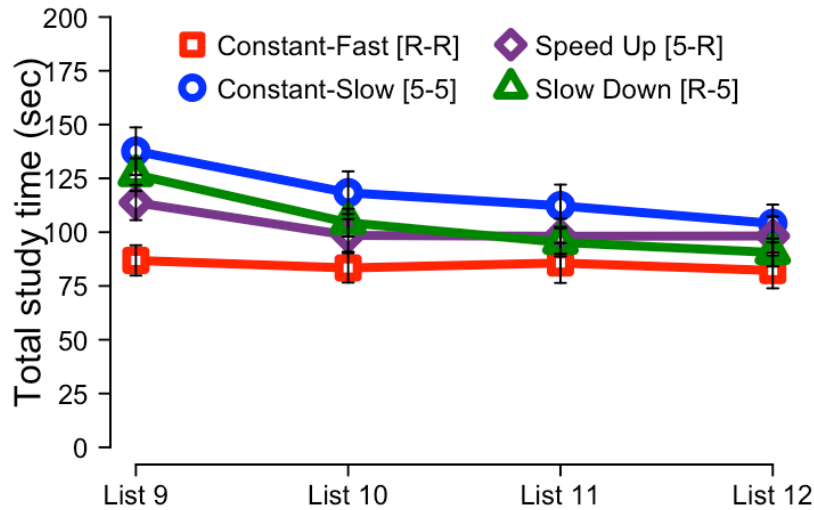
Figure 3.8. The total time spent studying each list of items in Segment 3 of Experiment 3A, during which study was self-paced without penalization. Error bars reflect standard error.

There were no other significant differences in penalization-free self-pacing during Segment 3 across conditions. So, although participants across conditions experienced different degrees of time-related stress/pressure during study and at different points throughout the task, these experiences had evidently no impact on subsequent value-based pacing in the absence of task-imposed rushing.

Discussion

Experiment 1 examined the potential impact of limited study time on attention to value during study and the activation of a value-based study strategy. Although study time constraints limited the total number of items that participants could remember, selectivity was unaffected (Middlebrooks, Murayama, et al., 2016). Experiment 3A was designed to differentiate between the effects (or evident lack thereof) of time constraints themselves in Experiment 1 and the possible consequences of any feelings of stress/anxiety *resulting* from the time constraints on value-directed remembering during study. Despite 41 of the 48 participants in the Speed-Up and

Slow-Down conditions[18] reporting that the task was indeed stressful (with some participants reporting that it even felt "overwhelming" at times), the results of Experiment 3A were largely consistent with those of Experiment 1. Participants were more likely to recall higher-valued items, but to a comparable extent across conditions.

The only exception to this was during Segment 2: Participants in the Constant-Fast condition—who had to this point already completed four lists of rushed self-pacing—were more selective than participants in the other three conditions, including those in the Slow-Down condition who had studied the first four lists under identical rushing conditions. This selectivity difference was not a consequence of a decline in selectivity in the other conditions, but rather superior selectivity within the Constant-Fast condition.

It is possible that the time pressure became less stressful after four lists of rushing, thus enabling participants in the Constant-Fast condition to better engage in a value-based strategy. (In fact, a number of participants reported that the extent to which rushing was stressful declined as the task progressed.) This explanation, however, is questionable for two primary reasons. Firstly, it is unclear as to why there would be differences in selectivity between the Constant-Fast condition and the Speed-Up condition during Segment 2 when there were no evident differences in the manner in which participants in these two conditions enacted their study. If a reduction in stress (or habituation to stressful conditions) led to enhanced selectivity in the Constant-Fast condition, then it stands to reason that there should have been some demonstrable difference in pacing relative to the less selective but similarly rushed Speed-Up condition.

That there were no differences in pacing does not in itself counter the explanation of stress habituation as the source of enhanced selectivity—after all, it could be that the same overt

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[18] Participants in the Constant-Fast condition were also asked about the stress of the rushed portions of the task, but responses were not recorded owing to computer error. In light of the general consensus in the Speed-Up and Slow-Down conditions, though, it seems probable that Constant-Fast condition participants would have concurred.

study behaviors are still more effective if time pressures and consequent affective responses exert less demand on executive control mechanisms. The point which sheds the most doubt on this explanation, however, rests in the size of the Value effect in Experiment 1 relative to Experiment 3A. Specifically, participants in Experiment 1 were $e^{0.19} = 1.21$ times more likely to recall an item than they were to forget it for each one-unit increase in its value, and this effect size (viz. 0.19) was replicated across two experiments. The corresponding effect size in Experiment 3A during Segment 2 was essentially the same at 0.18 in all but the Constant-Fast condition, which was nearly twice as large at 0.33.

If it were simply a matter of stress reduction, then the effect of value in the Constant-Fast condition should, arguably, have been more reminiscent of Experiment 1 in which there were no additional stressors beyond the relatively constrained experimenter-pacing. So, it would seem that it was continued experience with the rushed self-pacing portion of the task that resulted in enhanced selectivity and not (only) habituation to consequent feelings of pressure. In the absence of differences in pacing behavior, this finding may be explained by the regulatory fit hypothesis: There may be a match, so to speak, between the goal of avoiding point deductions in Experiment 3A and the orientation invoked by time pressures. Namely, if time pressure similarly invokes a loss- or prevention-focus, then Constant-Fast participants may have come to experience a greater match between the task goal and their goal orientation than those in the other three study conditions.

Experiment 3B

Experiment 3B was designed to test the extent to which the results of Experiment 3A—specifically, the superior selectivity demonstrated by Constant-Fast participants following prolonged rushing experience—can be explained by the regulatory fit hypothesis. If time

pressure invoked a loss focus in Experiment 3A, then there should be a mismatch between that loss focus and the gains-focused instructions in Experiment 3B, leading to comparably impaired selectivity in the Constant-Fast condition. If the results are consistent across experiments, then a regulatory fit explanation is less likely and further consideration should be given to the central executive-based explanations.

Method

Participants

Experiment 3B consisted of 96 undergraduate (76 female) student participants from the University of California, Los Angeles, ranging in age from 18 to 24 years ($M = 19.97$, $SD = 1.27$). Participants received partial credit for a course requirement.

Materials & Procedure

The materials in Experiment 3B were identical to that of Experiment 3A. The only difference in procedure between Experiments 3A and 3B was that participants in Experiment 3B received gains-focused instructions. Prior to the first list of rushed study, participants were told that the faster they study an item, the more points they would receive at test if they correctly recalled it and that they would achieve the most points possible for remembering an item if they had studied it for no longer than 1 second. They were told to think of the task as though they were starting with 0 points. For each word correctly recalled at test, they would gain points; if they recalled the word *and* had only studied it for 1 second, they would gain the full amount of points. Participants were provided an example:

> "For instance, let's say that you studied "apple : 10" and you later recall apple at test. If you studied apple for 4 seconds, you'll gain 7 points for correctly recalling it. If you only studied apple for 2 seconds, you'll gain 9 points. If you didn't spend any extra time studying (i.e., you only studied apple for 1 second or less), you'll gain all 10 points."

The verbal reminders to study faster voiced by the experimenter in the room also incorporated a gains-focus (e.g., "You can gain more points if you study faster."; "You can increase your score by studying faster."). Instructions in the Constant-Slow condition, during which there was no self-pacing in Lists 1-8 and thus no verbal reminders (although the experimenter remained in the room for the sake of consistency), were modified such that participants were only told to think of the task as though they were starting with 0 points and gaining points for each word that they correctly recalled at test. The average number of verbal reminders provided by the experimenter to participants per list and study condition is provided in Table 3.5.

As in Experiment 3A, Lists 9-12 were entirely self-paced (sans penalization) and participants completed a short survey at the end of the task to ascertain whether they experienced feelings of anxiety or of being overwhelmed during the task, whether they found the rushing techniques impacted their study, et cetera.

Results

Overall Recall Performance

Table 3.6 lists the proportion of items recalled as per study condition and list. A 4(Study time condition: Constant-Fast [1-1]; Constant-Slow [5-5]; Speed Up [5-1]; Slow Down [1-5]) x 3(Segment: Lists 1-4, Lists 5-8, Lists 9-12) repeated-measures ANOVA on total recall revealed a significant Condition x Segment interaction, $F(4.92, 15.73) = 16.60$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .15$. As in Experiment 3A, there was a significant effect of Condition within Segment 1 and Segment 2, ps $< .001$, but not Segment 3, $p = .06$[19]. Within Segment 1, the Constant-Fast and

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[19] Post hoc comparisons in light of this marginally significant effect of Condition in Segment 3 indicate greater recall by participants in the Speed-Up condition relative to the Constant-Fast condition ($p = .01$). This finding should, however, be interpreted with caution if at all in light of the nonsignificance of the initial main effect and the subsequent number of post hoc comparisons.

Slow-Down conditions recalled significantly fewer items than the Constant-Slow and Speed-Up conditions, $ps < .001$; within Segment 2, the Constant-Fast and Speed-Up conditions recalled significantly fewer items than the Constant-Slow and Slow-Down conditions, $ps < .001$. So, conditions in which participants self-paced their study under a 1-second penalization rate again recalled fewer words than those participants who studied at a set experimenter-pace of 5 seconds per item. There were no significant differences between conditions studying at the same rate, $ps > .13$.

Rushed Self-Pacing Overall

As in Experiment 3A, the total time spent studying the 20 items within each list (in seconds) was compared in a 4(Condition: Constant-Fast [Seg1][20], Constant-Fast [Seg2], Slow Down [Seg1], Speed Up [Seg2]) x 4(List) repeated-measures ANOVA to determine whether participants differed in their (penalized) self-pacing as a consequence of prior experiences in the task and continued experience with pacing-under-penalization. There was a significant Condition x List interaction, $F(7.03, 215.62) = 3.32$, $MSE = 3.33$, $\eta^2_G = .02$, $p = .002$. As in Experiment 3A, there was a significant effect of Condition in the first rushed list, $F(3, 92) = 7.25$, $MSE = 121.52$, $\eta^2_G = .12$, $p < .001$, such that participants in the Speed-Up condition (during Segment 2) ($M = 37.90$, $SD = 13.71$) spent more time studying than participants in the Constant-Fast condition during Segment 1 ($M = 28.80$, $SD = 11.48$), $p_{adj} = .03$.

Two one-sample t-tests were then conducted (as in Experiment 3A) to compare average self-pacing across lists and across conditions against a set pace of 20 seconds per list and of 100 seconds per list. The results of these analyses indicated that rushed participants spent

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[20] "[Seg1]" refers to Lists 1-4 and "[Seg2]" to Lists 5-8. Note that the Constant-Fast condition is included in this particular analysis twice because participants studied under the 1-second penalization instructions for the duration of Lists 1-8.

significantly more time studying ($M = 27.17$, $SD = 8.64$) than the 20 seconds allotted to

participants in the fast conditions of Experiment 1—a pace which would have resulted in no

penalization—$t(71) = 7.04$, $p < .001$, but also significantly less time studying than the 100

seconds allotted to those studying under the experimenter-paced rate of 5 seconds per item, $t(71)$

$= -7.52$, $p < .001$.[21]

Value-Directed Remembering

The same models as were used in Experiment 3A were used to analyze Experiment 3B.

Table 3.7 reports the tested model and its estimated regression coefficients per segment analysis.

Segment 1.

Figure 3.9a depicts Segment 1 recall as per item value and assigned study condition.
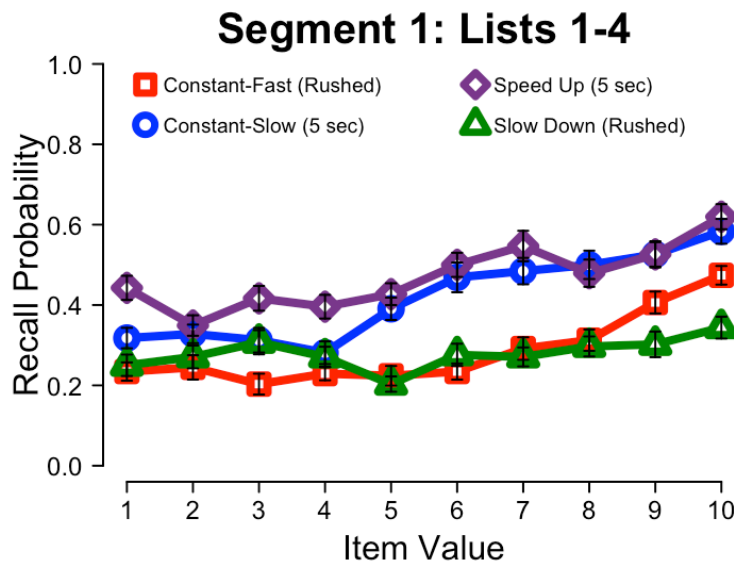
Figure 3.9a. Recall probability in Segment 1 as a function of item value and assigned study condition in Experiment 3B. Error bars reflect standard error.

Consistent with Experiment 1 and Experiment 3A, Value significantly and positively predicted

recall performance in the Constant-Slow condition during Segment 1 ($\beta_{10} = 0.15$, $p < .001$). This

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[21] These results are replicated if only considering the first four rushed lists within each of the conditions (i.e., excluding Lists 5-8 in the Constant-Fast condition).

relationship between item value and recall was consistent in the Constant-Fast and Speed-Up conditions, but there was a significant difference in the effect of Value between the Constant-Fast and Slow-Down conditions (β_{13} = -0.11, p = .01). Although participants in the Constant-Slow condition were $e^{0.15}$ = 1.16 times more likely to recall an item for each one-unit increase in its value than they were to forget it, Value did not significantly predict recall in the Slow-Down condition (β = 0.04, p = .19).

Although speculative, it is possible that this condition difference stems not from the task manipulation, but rather from individual differences amongst participants. Were the nonsignificant effect of Value a consequence of the task (including the transition to gains-focused instructions), there should have been a similar impact on selectivity in the Constant-Fast condition because task differences had not yet arisen between the Constant-Fast and Slow-Down conditions during Segment 1. Value positively predicted recall in the Constant-Fast condition, however, and Slow-Down condition participants were significantly less selective than those in the Constant-Fast condition (p = .02). In light of the consistency across the other conditions with respect to the effect of value, and the fact that there is no theoretical explanation as to why the Slow-Down and Constant-Fast conditions would differ at this stage, this finding may be more anomalous than substantive. This is not to say that it should be summarily dismissed, but simply that there is little at this stage to suggest a more meaningful explanation.

In addition to the (general) Value effect, there was also a small but significant List x Value effect in the Constant-Slow condition (β_{30} = 0.05, p = .30) that did not differ across conditions, indicating increased attention to item value across Lists 1-4.

Segment 2: within-subject timing shift.

Figure 3.9b depicts Segment 2 recall as per item value and assigned study condition.

Segment 2: Lists 5-8
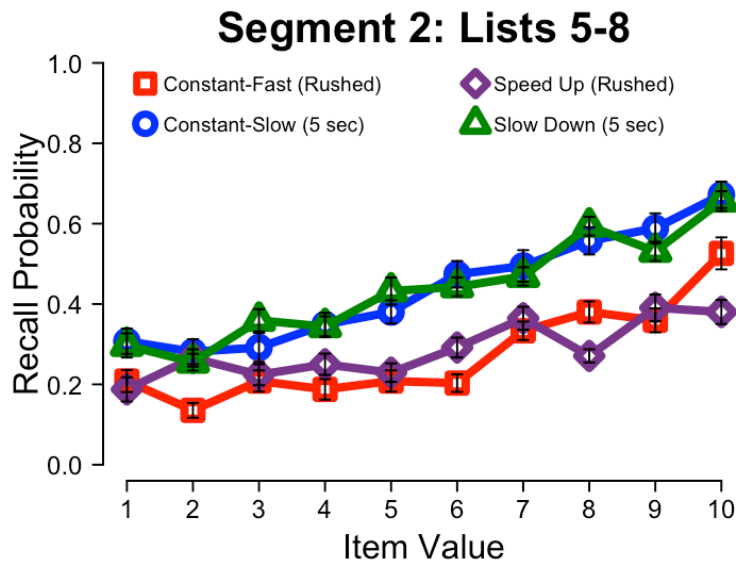
Figure 3.9b. Recall probability in Segment 2 as a function of item value and assigned study condition in Experiment 3B. Error bars reflect standard error.

As in Segment 1, Value positively predicted recall performance in the Constant-Slow condition during Segment 2 ($\beta_{10} = 0.21$, $p < .001$). There was a marginal Condition x Value interaction, such that recall in the Speed-Up condition (which had shifted from a 5-second study pace in Segment 1 to rushed self-pacing in Segment 2) was driven by Value to a *lesser* extent than in the Constant-Slow condition ($\beta_{11} = -0.10$, $p = .06$). There were no significant differences in the effect of Value between the Constant-Slow condition and either the Constant-Fast or Slow-Down conditions, $ps > .64$. Participants in the Speed-Up condition were $e^{0.11} = 1.12$ times more likely to recall an item for each one-unit increase in its value than they were to forget it; participants in the other three conditions were $e^{0.21} = 1.23$ times more likely.

There was further a List x Condition effect ($\beta_{22} = 0.15$, $p = .03$), such that there was a significant effect of List in the Speed-Up condition ($\beta = 0.12$, $p = .02$) that was not present in the other conditions ($ps > .26$). Overall recall was maintained across Lists 5-8 in all but the Speed-Up condition, in which participants increased their recall with continued experience studying

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under the new instructions of rushed/penalized self-pacing.[22] That participants increased the total number of items recalled from List 5 to List 8 supports the notion that the shift in task timing/study procedure (specifically to a faster, rushed study rate) required a degree of acclimation, the necessity of which may also explain the lower selectivity within Segment 2 relative to the other study conditions.

The effect of Value in the Constant-Slow condition was maintained across lists (β_{30} = -0.003, p = .90) and this did not significantly differ in either the Constant-Fast or Speed-Up conditions, ps > .24. There was, however, a significant List x Value x Condition effect, such that participants were more apparently more attentive to item value across lists in the Slow-Down condition (β_{33} = 0.07, p = .02) relative to the Constant-Slow condition. A simple slope analysis nevertheless suggests that any enhanced selectivity across Lists 5-8 in the Slow-Down condition was minimal (β = 0.03, p = .07) and only marginally significantly at that.

Segment 3: self-regulated study.

Figure 3.9c depicts Segment 3 recall as per item value and assigned study condition.

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[22] Note that this is a change in recall within the Speed-Up condition across lists; participants in the Constant-Slow and Slow-Down conditions still ultimately recalled more items in sum (see Overall Recall Performance analyses).

Segment 3: Lists 9-12
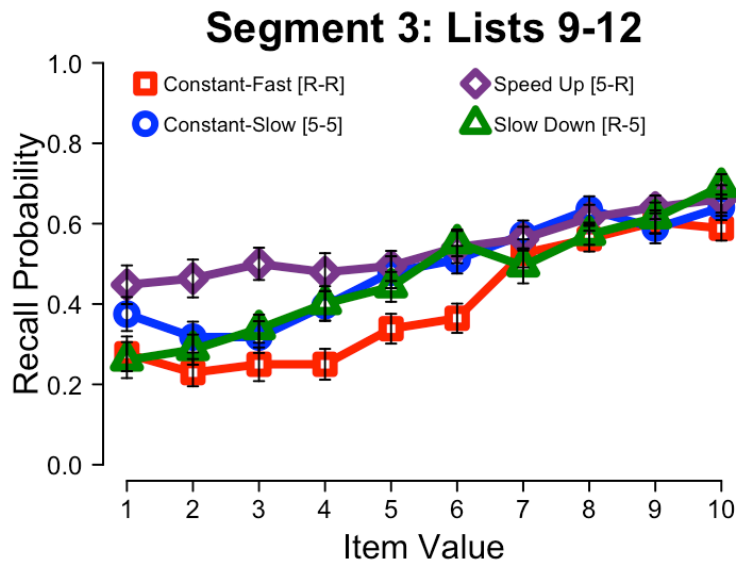
Figure 3.9c. Recall probability in Segment 3 as a function of item value and assigned study condition in Experiment 3B. Study during Segment 3 was self-paced without penalization. Error bars reflect standard error.

As in Experiment 1 and 3B, all participants self-paced their study of the items in Lists 9-12; participants were free to study each item for as long as they wished without penalization for studying in excess of a 1-second rate. As in Segments 1 and 2 (and consistent with Experiment 3A), Value significantly and positively predicted recall in the Constant-Slow condition ($\beta_{10} =$ 0.20, $p < .001$), with no significant differences across the other study conditions, $ps > .24$. Participants were $e^{0.20} = 1.23$ times more likely to recall an item for each one-unit increase in its value than they were to forget it.

As in Segment 2, there was a further List x Condition interaction ($\beta_{22} = 0.20$, $p = .01$), such that there was a significant effect of List on recall in the Speed-Up condition ($\beta = 0.08$, $p = .04$) that was not present in the other conditions ($ps > .08$). This List effect in the Speed-Up condition did not, however, correspond with any change in selectivity. There was a small but marginally significant List x Value interaction in the Constant-Slow condition ($\beta_{30} = 0.04$, $p = .056$)—with value-based recall increasing slightly across lists—but also a List x Value x

Condition interaction (β_{32} = -0.05, p = .059) owing to participants in the Speed-Up condition demonstrating no such change in selectivity across Lists 9-12 (β = -0.01, p = .48).

Self-Regulated Study

Rushed self-pacing.

Figure 3.10 illustrates the average proportion of total study time and the average total study time, respectively, spent per item value during Segment 1 and 2.
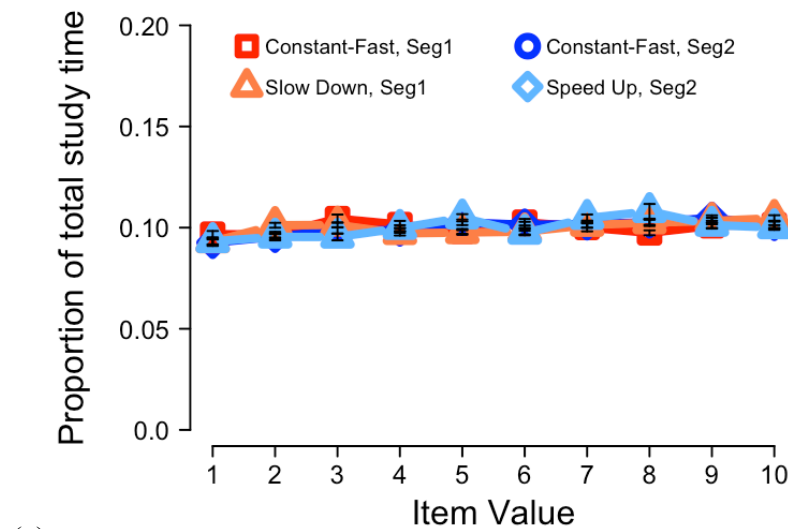
(a)
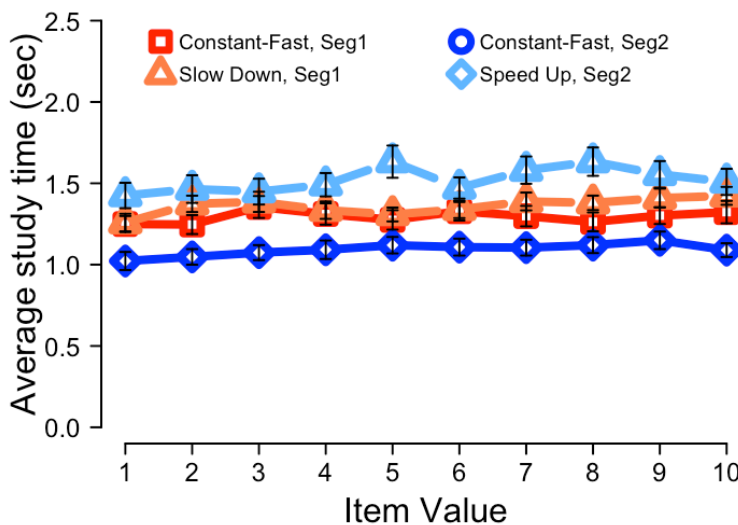
(b)

Figure 3.10. (a) The average proportion of self-paced study time and (b) the average study time (in seconds) allocated to each item value across assigned study conditions during the penalization periods of Segment 1 and Segment 2 in Experiment 3B. Error bars reflect standard error.

The same models as were used in Experiment 3A were used to analyze rushed self-pacing in

Segments 1 and 2 of Experiment 3B. Table 3.9 reports the tested model and its estimated

regression coefficients per segment analysis.

In both Segments 1 and 2, there were very small but significant List effects, with

participants spending less time studying the lists in their entirety across lists. In Segment 2, there

was a further List x Condition interaction (β_{21} = -0.13, $p < .001$): Participants in the Speed-Up

condition exhibited a much greater decline in total time spent studying across lists. As outlined

in Table 3.8, participants in the Speed-Up condition initially spent approximately 38 seconds

studying List 5 and steadily declined in their study to approximately 27 seconds for List 8.

Logically, it makes sense that Speed-Up participants studied for longer than Constant-Fast

participants as they were transitioning from a prior study rate of 5 seconds per item (i.e., 100

seconds per list), and thus would require a period to acclimate to the new task conditions (a

period during which penalizations nevertheless would have accrued).

In terms of value-based self-pacing, there was a very small List x Value x Condition

interaction in Segment 1 (β_{31} = -0.02, $p = .01$). Participants in the Constant-Fast condition did

not self-pace when rushed as per item value and this did not change across Lists 1-4; Slow-Down

participants, however, exhibited no general value-based pacing but did exhibit a *very* small,

negative effect of Value with continued study (β = -0.01, $p = .04$). (Although this alone would

not explain the significantly lesser effect of Value on recall in the Slow-Down condition during

Segment 1, it is consistent with the idea that this particular group of participants was, for

whatever reason, less attentive to item value while studying during Segment 1.) There was also a

very small, positive effect of Value on rushed self-pacing (β_{10} = 0.01, $p = .001$) during Segment

2, with no differences between the Constant-Fast and Speed-Up conditions ($p = .44$). These

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findings are consistent with that of Experiment 3A, providing little evidence for markedly strategic rushing.

Self-regulated study: Lists 9-12.

Figure 3.11 illustrates the average proportion of total study time and the average total study time, respectively, spent per item value during the fully self-paced (i.e., penalization-free) lists of Segment 3.
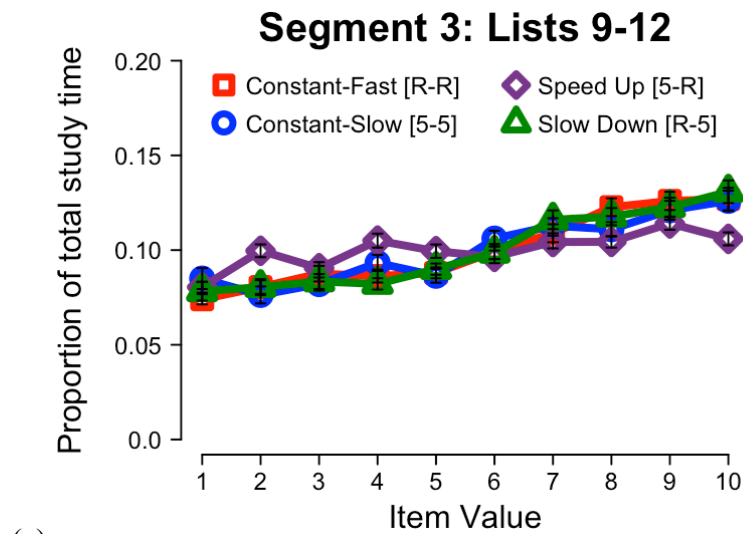
(a)
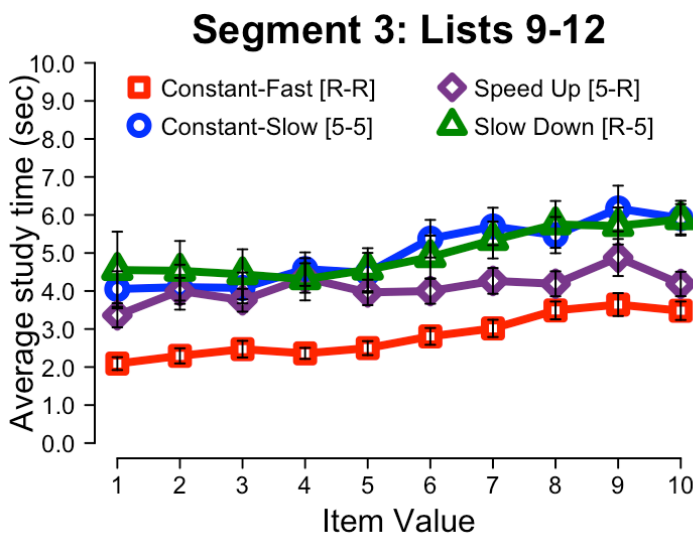
(b)

Figure 3.11. (a) The average proportion of self-paced study time and (b) the average study time (in seconds) allocated to each item value across assigned study conditions during Segment 3 in Experiment 3B. Study during Segment 3 was self-paced without penalization. Error bars reflect standard error.

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The same model used in Experiment 3A was used to analyze self-pacing in Segment 3 of Experiment 3B. Table 3.10 reports the tested model and its estimated regression coefficients per segment analysis.

There was a significant effect of Value on study time in the Constant-Slow condition, with participants spending 0.26 more seconds studying with each one-unit increase in assigned item value ($\beta_{10} = 0.26$, $p < .001$). This Value effect was consistent in the Constant-Fast and Slow-Down conditions, but differed in the Speed-Up condition: Although Value positively predicted study time in the Speed-Up condition ($\beta = 0.09$, $p = .01$), as well, it was to a significantly lesser extent than in the other conditions ($\beta_{12} = -0.16$, $p = .02$). Nevertheless, there were no similar differences in selectivity with respect to Segment 3 recall, which suggests that participants in the Speed-Up condition *may* have had a better understanding of the extent to which they needed to regulate their studying so as to maintain their recall of higher-valued items, though they also ultimately spent more time studying lower-valued items than was strictly necessary.

There was also a significant effect of List on study time in the Constant-Slow condition ($\beta_{10} = -0.77$, $p = .006$), such that significantly less time was spent studying each successive list overall. Consistent with Experiment 3A, this negative effect of List was further evident in the Speed-Up and Slow-Down conditions, but participants in the Constant-Fast condition did not demonstrate a List effect on their overall self-pacing ($\beta = -0.12$, $p = .22$). As illustrated in Figure 3.12, participants in all but the Constant-Fast condition demonstrated a sharp decline in total study time while those in the Constant-Fast condition maintain a steady (and notably faster) rate of study across Lists 9-12, just like in Experiment 3A.
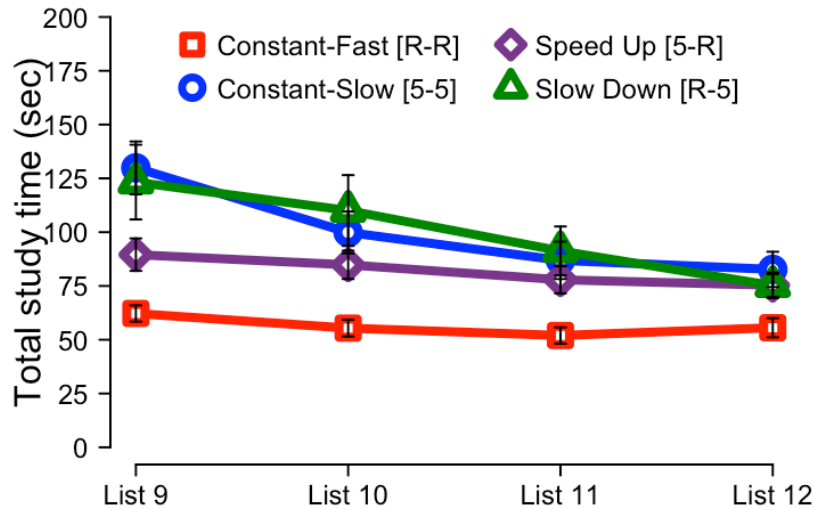
Figure 3.12. The total time spent studying each list of items in Segment 3 of Experiment 3B, during which study was self-paced without penalization. Error bars reflect standard error.

Discussion

As in Experiment 3A, the results of Experiment 3B were largely consistent with those of Experiment 1, in that participants were more likely to recall higher-valued items and, when given the opportunity to self-pace their study free of point penalizations, were more likely to study high-value items for longer than low-value. There were, however, exceptions to this general pattern. For one, participants in the Slow-Down condition were less selective in their recall during Segment 1 than participants in the other study conditions. This finding, however, should be interpreted *very* cautiously (if at all) given that no experimental differences had yet arisen between the Slow-Down and Constant-Fast conditions (both groups of participants self-paced their study under the penalization policy during Lists 1-4). Not until List 5 were task-related differences introduced, so differences in selectivity at the start of the task are likely more reflective of individual participant differences that remained despite random assignment.

Of greater import, participants in the Speed-Up condition were significantly less selective relative to the other study conditions following their shift from the experimenter-paced 5-second study rate to the rushed-penalization study pacing in Segment 2 (i.e., Lists 5-8). This contrasts

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with the *enhanced* selectivity demonstrated by those in the Constant-Fast condition during Segment 2 in Experiment 3A. These results are seemingly inconsistent with a central executive-based explanation—which would have predicted either consistent findings across experiments or, at the very least, a null effect of time pressure in Experiment 3B under gains-focused instructions in the event that a gains-focus reduced feelings of stress or pressure during study. As discussed in the following General Discussion, however, the results of Experiments 3A and 3B in conjunction are also not particularly consistent with the regulatory fit hypothesis. Rather, the results of the present experiments may instead be a consequence of the degree to which the instructional perspective (gains- or loss-focused) overtly conveyed point penalizations and study time constraints.

General Discussion

The results of Experiment 1 indicated a null influence of time constraints on strategic, value-based study and recall (Middlebrooks, Murayama, et al., 2016), but the possibility remains that time constraints themselves are of less impact to strategy adoption and execution than are the more visceral experiences that can accompany them. Experiments 3A and 3B were designed to assess the extent to which feelings of pressure and/or anxiety associated with having insufficient time with which to complete a task might affect a learner's likelihood of selectively prioritizing valuable information when all of the information cannot be retained.

Participants in Experiment 3A and 3B consistently demonstrated selective prioritization of high-value items relative to lower-valued items, but the influence of the time constraints to participants in the Constant-Fast and Speed-Up conditions differed between the experiments. Those in the Constant-Fast condition in Experiment 3A exhibited enhanced selectivity during Lists 5-8 relative to the other study conditions (and relative to those in Middlebrooks,

Murayama, et al., 2016), whereas those in the Speed-Up condition in Experiment 3B exhibited *impaired* selectivity during Lists 5-8. These results are somewhat in line with the regulatory fit hypothesis in that there may have been a match in goal orientation between the loss-focused instructions in Experiment 3A and the time constraints—resulting in enhanced selectivity—and a mismatch between the gains-focused instructions in Experiment 3B and the time constraints—resulting in impaired selectivity.[23]

Regulatory fit alone, however, cannot fully explain the obtained results as the findings between Experiments 3A and 3B somewhat conflict. Namely, enhanced selectivity was demonstrated by those in the Constant-Fast condition in Experiment 3A, but there was no corollary impairment of the Constant-Fast condition in Experiment 3B. Likewise, impaired selectivity was demonstrated by those in the Speed-Up condition in Experiment 3B, but there was no evident enhancement of the Speed-Up condition in Experiment 3A. Were the results solely a consequence of regulatory fit, there should arguably have been comparable effects between similar timing conditions (e.g., enhanced selectivity in both the Constant-Fast and Speed-Up conditions in Experiment 3A) and/or opposing effects across experiments (e.g., enhanced selectivity in the Constant-Fast condition in Experiment 3A and impaired in Experiment 3B).

Given that the enhanced selectivity demonstrated by the Constant-Fast participants in Experiment 3A was not evident until Lists 5-8, it is conceivable that the benefit of a match between the task goal and the participant's goal orientation to strategy engagement is relatively

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[23] A match between gains-focused instructions and time pressure seems implausible given that rushing led to *impaired* selectivity in the Speed-Up condition in Experiment 3B. While the enhanced selectivity in the Constant-Fast condition in Experiment 3A might still have occurred despite a mismatch between the loss-focused instructions and the time pressure (e.g., perhaps participants became more strategic so as to overcome feelings of dissonance resulting from the mismatched goal orientations), a match between goal orientations should at worst result in no influence to selectivity, not impaired consideration of and attention to item importance.

slow to build. Prior research consistently illustrates that the adoption of a selective, value-based strategy requires continued task experience (e.g., Castel, 2008; Castel et al., 2002; Middlebrooks & Castel, 2018). A match alone may not in itself automatically result in a selective study strategy; rather, it might (as in Experiment 3A) serve to support or enable its efficient execution once adopted. Likewise, the absence of enhanced selectivity in the Speed-Up condition relative to the other study conditions—particularly the Constant-Fast condition—during Segment 2 may reflect the fact that the benefit of a matched goal/goal orientation was short-lived. Had participants in the Speed-Up condition experienced an additional four lists of rushed study (for a total of eight lists like in the Constant-Fast condition), they might have exhibited comparably enhanced selectivity.

Issues arise with this explanation, though, when considering Experiment 3B because selectivity was impaired only in the Speed-Up condition. If a mismatch between the task goal and the participant's goal orientation was the primary source of impairment, then presumably those in the Constant-Fast condition would have demonstrated poorer selectivity during Segment 1 to rival that of the impairments during the initial rushed lists in the Speed-Up study schedule. Admittedly a stretch, but perhaps participants in the Constant-Fast condition were able to strategically offset the mismatch because they had only ever experienced a stable goal orientation. Those in the Speed-Up condition, however, first experienced the slower (5-second) study pacing, which may have resulted in a matched goal orientation that then became mismatched with the task goals upon switching to the rushed portion of the task.

The supposition that the regulatory fit hypothesis is of *any* relevance to the present results, however, is ultimately questionable. After all, a match in goal orientation between the loss-focused instructions in Experiment 3A and time pressure during study should be logically

complemented by a match between the gains-focused instructions in Experiment 3B and the slower, 5-second study pace. There was, however, no change in selectivity relative to the other study conditions in the Constant-Slow condition in Experiment 3B like there was in the Constant-Fast in Experiment 3A, nor in the Slow-Down condition in Experiment 3A like in the Speed-Up condition in Experiment 3B.

Rather than regulatory fit, the present results may be better explained simply by the degree to which participants perceived their study to be constrained. Participants in Experiment 3B technically experienced point penalizations for extended study during the rushed portions of the task, but the task was framed as one of point acquisition rather than the avoidance of point loss. As such, the penalizations may have been less evident and the time constraint itself may have seemed less limiting. Consistent with this interpretation, participants spent more time studying during the rushed lists (i.e., they experienced more point penalizations) in Experiment 3B than they did in Experiment 3A (see Table 3.8). In other words, participants in Experiment 3A were more active in avoiding reductions to their scores than those in Experiment 3B, with the *only* difference between the two experiments being the framing of the instructions as either loss- or gains-focused.

Although participants in Experiment 3B seemed to find the task stressful (42 of the 48 participants in the Speed-Up and Slow-Down conditions reported marked stress, with 2 participants failing to respond[24]), a perception of the task being less constrained than it truly was could also explain why participants in the Speed-Up condition exhibited impaired selectivity upon switching to the rushed portion of the task. A learner studies selectively in order to compensate for an inability to remember all of the information, to offset limitations to study and

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[24] As in Experiment 3A, participants in the Constant-Fast condition were also asked about the stress of the rushed portions of the task, but responses were not recorded owing to computer error.

retrieval; if these limitations are less apparent, the need for selective study may similarly be less apparent and thus less likely to be enacted. In contrast, the notable constraints of the task may have been sufficiently apparent in Experiment 3A under the loss-focused instructions to encourage selectivity to an even greater extent with continued task experience.

The results of Experiments 3A and 3B are far from conclusive, but, to the best of my knowledge, no other experiments have examined the role of goal orientation—viz. a loss- or gains-perspective—on strategic engagement during study, particularly with respect to offsetting capacity limitations and prioritizing valuable information. It may be that participants in Experiment 1 (Middlebrooks, Murayama, et al., 2016) would have been similarly more or less selective as a consequence of such instructional manipulations. In order to truly interpret the results obtained in Experiment 3, it is critical to backtrack a bit, so to speak, and simplify: (How) do gains- and loss-focused perspectives influence value-directed remembering in the first place?

Prior work demonstrates that framing metacognitive questions as a function of likelihood of forgetting information can lead to more accurate metacognitive judgments and more optimal study behaviors than when the same questions are framed as a function of likelihood of remembering the information (Finn, 2008). Loss-focused instructions may similarly direct participants to consider the ways in which memory can fail and the improbability of remembering all of the information—namely, of the inevitably of losing points in the current task. In the same way that recognizing that the quantity of to-be-remembered information exceeds one's encoding capacity can lead to prioritization of valuable/important information (which was still evident in Experiment 3B despite the gains-focused instructions), further reminders of the imperfection of memory may serve to instill the importance of strategically offsetting likely lapses in recall so as to ultimately avoid losses (Kahneman & Tversky, 1979).

Aside from clarifying the role of loss- versus gains-based perspective on value-directed remembering, further consideration should be given to situations in which value must first be determined under situations of limited encoding time. In the case of a gains-focused perspective, learners may be more liberal with what they consider to be important information, whereas those with a loss-focus may be more stringent in deeming something worthy enough to warrant any of their limited study time. Such stringency could further mitigate potential losses if the judgments of importance are ultimately accurate, but could also exacerbate losses if one is overly conservative in his/her evaluations and thus fails to encode information that was, in fact, worthy of study.

Ultimately, the results of Experiments 3A and 3B suggest that feelings of pressure and stress in and of themselves do not prevent selective, value-based study. They also indicate, however, that there is potential for interactive influences between stress/time constraints and one's perspective or goal orientation on value-based, self-regulated study.

Conclusions

We often find ourselves short on time and suffering memory lapses as a consequence. These lapses can be particularly frustrating when the information forgotten is of higher importance than that which is ultimately remembered: imagine returning home from a shopping trip in which everything was purchased except the very item you had most intended to buy! Experiments 1-3 in this chapter examined whether the generally negative impact of time constraints on memory might be mitigated by strategic, value-based study.

Experiments 1 and 2 specifically investigate the extent to which learners are able to adapt to shifts in available study time, how prior experience with time limits affects subsequent self-pacing of study, and whether age differences (and associated differences in cognitive resources) arise in the effects of time constraints on study. Consistent with the agenda-based regulation model of self-regulated learning (Dunlosky & Ariel, 2011a), participants in Experiments 1 and 2 successfully recognized and strategically offset the increased limitations on their encoding owing to the time constraints, largely maintaining their prioritization of high-value information relative to those participants studying under sufficient timing conditions.

Experiments 3A and 3B further investigated whether stress or anxiety resulting from time constraints—rather than time constraints, per se—compromises strategic, value-based study. Although the framing of the selectivity task with either a loss-focused perspective or a gains-focused perspective may influence the extent to which learners attempt to offset limits to their encoding capacity owing both to the quantity of to-be-remembered information and the time that they have available to study it, value-directed remembering persisted in spite of the time constraints and—relative to Experiments 1 and 2— their (likely) more visceral character.

It would be inappropriate to conclude that limitations to one's study time will not notably influence one's ability to prioritize high-value or important information. In light of the stress that can be associated with time constraints and rushed study, future research should also consider populations with higher-than-average baseline levels of stress/anxiety (both generalized and specific to learning events, such as in testing situations). Given the apparent relevance of the task framing (viz. as loss- or gains-focused), additional examination of situations of higher consequence (i.e., of greater potential loss and of greater potential gain) is also warranted. The drive to avoid losses (Kahneman & Tversky, 1979) may have a more profound effect on value-directed remembering and self-regulated learning, in general, when the stakes are greater, such as when attempting to remember critical health information or endeavoring to earn a respectable grade in an important course.

Further consideration should be given to the nature of the to-be-remembered materials and, specifically, the manner by which importance is conveyed or determined. Time constraints might have a pronounced impact on selectivity during study if participants must first judge the importance of the to-be-remembered information before executing any sort of value-based study strategy. In the case of studying for an exam, for instance, a student must determine which information in the textbook is important—is it critical to a conceptual understanding; is it likely to be tested; et cetera—before being able to study selectively. When time is limited, are learners capable of identifying important information quickly enough to still execute a selective strategy? Moreover, are learners selective when making their evaluations in light of time constraints? If given 30 min to study a chapter, for example, perhaps 30% of the contents are deemed important enough to warrant attention. If given only 10 min, though, does that learner continue to ascribe equal importance to that 30%, or does he become more selective in his evaluations, thus

becoming more selective in study? A critical step in furthering the current research will be to understand the influence of time constraints on both attempts at selectivity (e.g., being more selective in evaluations) and the successful execution of selective study strategies (i.e., remembering those most important items) when evaluating importance is under the learner's purview.

The (many) questions within this domain notwithstanding, the results of Experiments 1-3 do suggest that having insufficient time during encoding, while costly to one's memory overall, does not preclude strategic allocation of one's cognitive resources (as a younger *or* older adult) to the information identified as the most important to remember.

Table 3.1

Time constraints

Recall as a function of segment, list, and study condition in Experiment 1

Condition	Segment 1					Segment 2					Segment 3				
	L1	L2	L3	L4	Average	L5	L6	L7	L8	Average	L9	L10	L11	L12	Average
Constant-Fast [1-1]	.28 (.12)	.30 (.12)	.29 (.13)	.28 (.08)	.29 (.09)	.31 (.12)	.30 (.13)	.31 (.11)	.30 (.09)	.31 (.09)	.51 (.21)	.48 (.23)	.50 (.26)	.45 (.23)	.49 (.21)
Constant-Slow [5-5]	.40 (.15)	.47 (.16)	.47 (.17)	.48 (.17)	.46 (.13)	.49 (.18)	.49 (.20)	.48 (.18)	.44 (.18)	.47 (.16)	.53 (.24)	.52 (.22)	.47 (.24)	.45 (.23)	.49 (.21)
Speed Up [5-1]	.46 (.17)	.43 (.17)	.47 (.19)	.47 (.20)	.46 (.15)	.28 (.11)	.28 (.17)	.28 (.11)	.29 (.14)	.28 (.10)	.51 (.24)	.46 (.24)	.48 (.25)	.41 (.22)	.47 (.21)
Slow Down [1-5]	.28 (.10)	.30 (.13)	.30 (.14)	.30 (.12)	.30 (.09)	.49 (.20)	.49 (.19)	.50 (.19)	.50 (.19)	.49 (.17)	.57 (.23)	.52 (.24)	.48 (.20)	.48 (.23)	.51 (.20)
Average	.35 (.16)	.38 (.17)	.38 (.18)	.38 (.18)	.37 (.14)	.39 (.18)	.39 (.20)	.39 (.18)	.38 (.18)	.39 (.16)	.53 (.23)	.50 (.23)	.48 (.24)	.45 (.23)	.49 (.21)

Note. Standard deviations are presented in parentheticals. "L1" through "L12" refer to Lists 1 through 12.

Table 3.2

Time constraints

Two-level hierarchical generalized linear model of recall performance and self-paced study (in Segment 3) predicted by item value, list, and study condition in Experiment 1

Fixed effects	Segment 1: Recall	Segment 2: Recall	Segment 3: Recall	Segment 3: Self-pacing
Intercept (β_{00})	-0.20*	-0.14	-0.10	4.46***
Predictors of intercept				
Cond1: Constant-Fast (CF) v. Constant-Slow (CS) (β_{01})	-0.80***	-0.85***	0.02	0.28
Cond2: Speed Up (SU) v. Constant-Slow (CS) (β_{02})	0.01	-0.94***	-0.09	-0.20
Cond3: Slow Down (SD) v. Constant-Slow (CS) (β_{03})	-0.76***	0.09	0.13	0.36
Value (β_{10})	0.15***	0.19***	0.25***	0.22***
Predictors of value				
Cond1: CF v. CS (β_{11})	-0.000004	0.05	0.001	0.08
Cond2: SU v. CS (β_{12})	-0.02	0.02	-0.05	0.01
Cond3: SD v. CS (β_{13})	0.01	-0.03	-0.04	0.11
List (β_{20})	0.10**	-0.09*	-0.15**	-0.58***
Predictors of list				
Cond1: CF v. CS (β_{21})	-0.13**	0.04	0.07	-0.18
Cond2: SU v. CS (β_{22})	-0.08	0.10$^+$	0.02	-0.22
Cond3: SD v. CS (β_{23})	-0.10$^+$	0.10$^+$	0.004	-0.34
List x Value (β_{30})	0.03$^+$	0.03**	0.01	0.01
Predictors of list x value				
Cond1: CF v. CS (β_{31})	0.01	0.01	-0.02	-0.03
Cond2: SU v. CS (β_{32})	0.02	-0.02	-0.02	-0.05
Cond3: SD v. CS (β_{33})	0.03$^+$	0.01	-0.02	-0.05
Random effects	**Variance**	**Variance**	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.25***	0.38***	1.23***	10.89***
Value (r_1)	0.02**	0.01**	0.05***	0.93***
List (r_2)	0.02***	0.03***	0.06***	0.14***
List x Value (r_3)	0.002*	0.001**	0.002*	0.01*

Note. Recall performance was coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable. Self-pacing was analyzed in seconds. Level 1 models for both outcomes were of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) + π_{2j} (List) + π_{3j} (List x Value). Level 2 models were of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Cond1) + β_{02} (Cond2) + β_{03} (Cond3) + r_{0j}, $\pi_{1j} = \beta_{10} + \beta_{11}$ (Cond1) + β_{12} (Cond2) + β_{13} (Cond3) + r_{1j}, $\pi_{2j} = \beta_{20} + \beta_{21}$ (Cond1) + β_{22} (Cond2) + β_{23} (Cond3) + r_{2j}, $\pi_{3j} = \beta_{30} + \beta_{31}$ (Cond1) + β_{32} (Cond2) + β_{33} (Cond3) + r_{3j}. $^+p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 3.3

Time constraints

Recall as a function of segment, list, study condition, and age group in Experiment 2

Condition	Age group	Segment 1					Segment 2					Segment 3				
		L1	L2	L3	L4	Avg	L5	L6	L7	L8	Avg	L9	L10	L11	L12	Avg
Constant-Slow [5-5]	Younger adults	.39 (.18)	.47 (.18)	.46 (.18)	.48 (.17)	.45 (.14)	.49 (.20)	.50 (.19)	.47 (.20)	.41 (.21)	.47 (.18)	.56 (.24)	.54 (.23)	.50 (.25)	.44 (.25)	.51 (.22)
	Older adults	.24 (.12)	.26 (.10)	.24 (.11)	.25 (.13)	.25 (.10)	.25 (.09)	.25 (.12)	.25 (.11)	.25 (.13)	.25 (.10)	.24 (.13)	.24 (.15)	.25 (.12)	.23 (.12)	.24 (.12)
Speed Up [5-1]	Younger adults	.46 (.18)	.43 (.19)	.48 (.19)	.44 (.21)	.45 (.16)	.26 (.09)	.26 (.15)	.26 (.12)	.30 (.16)	.27 (.11)	.47 (.25)	.47 (.25)	.46 (.27)	.41 (.22)	.46 (.23)
	Older adults	.27 (.13)	.27 (.13)	.32 (.14)	.28 (.14)	.28 (.12)	.17 (.12)	.18 (.08)	.20 (.09)	.20 (.08)	.19 (.08)	.29 (.16)	.28 (.16)	.28 (.18)	.28 (.18)	.28 (.16)
Average	Younger adults	.43 (.18)	.45 (.18)	.47 (.18)	.46 (.19)	.45 (.15)	.37 (.19)	.38 (.21)	.36 (.19)	.36 (.19)	.37 (.17)	.51 (.25)	.50 (.24)	.48 (.26)	.43 (.23)	.48 (.23)
	Older adults	.25 (.13)	.26 (.11)	.28 (.13)	.26 (.13)	.26 (.11)	.21 (.11)	.22 (.11)	.22 (.11)	.22 (.11)	.22 (.09)	.27 (.14)	.26 (.16)	.26 (.15)	.25 (.15)	.25 (.14)

Note. Standard deviations are presented in parentheticals. "L1" through "L12" refer to Lists 1 through 12.

Table 3.4

Time constraints

Two-level hierarchical generalized linear model of recall performance and self-paced study (in Segment 3) predicted by item value, list, study condition, and age group in Experiment 2

Fixed effects	Segment 1: Recall	Segment 2: Recall	Segment 3: Recall	Segment 3: Self-pacing
Intercept (β_{00})	-1.32***	-1.48***	-1.71***	4.47***
Predictors of intercept				
Condition (β_{01})	0.26	-0.28	0.46	0.23
Age group (β_{02})	1.10***	1.30***	1.74***	0.11
Age group x Condition (β_{03})	-0.27	-0.72*	-0.73	-0.93
Value (β_{10})	0.22***	0.39***	0.43***	0.44***
Predictors of value				
Condition (β_{11})	-0.08	-0.18*	-0.16	-0.05
Age group (β_{12})	-0.07	-0.19**	-0.20*	-0.25$^+$
Age group x Condition (β_{13})	0.07	0.21$^+$	0.12	0.15
List (β_{20})	-0.05	-0.05	-0.05	-0.34*
Predictors of list				
Condition (β_{21})	0.06	0.11	0.01	0.19
Age group (β_{22})	0.16*	-0.08	-0.16*	-0.25
Age group x Condition (β_{23})	-0.19$^+$	0.05	0.13	0.04
List x Value (β_{30})	0.08***	0.02	0.04*	-0.04
Predictors of list x value				
Condition (β_{31})	-0.04	-0.04	-0.003	0.07
Age group (β_{32})	-0.06$^+$	0.005	-0.03	0.04
Age group x Condition (β_{33})	0.07	0.06	-0.01	-0.08
Random effects	**Variance**	**Variance**	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.35***	0.39***	1.09***	8.81***
Value (r_1)	0.02***	0.06***	0.08***	0.23***
List (r_2)	0.02**	0.01	0.01	0.16**
List x Value (r_3)	0.04$^+$	0.001	0.0004	0.02**

Note. Recall performance was coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable. Self-pacing was analyzed in seconds. Level 1 models for both outcomes were of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 models were of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Condition) $+ \beta_{02}$ (Age group) $+ \beta_{03}$ (Age group x Condition) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Condition) $+ \beta_{12}$ (Age group) $+ \beta_{13}$ (Age group x Condition) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Condition) $+ \beta_{22}$ (Age group) $+ \beta_{23}$ (Age group x Condition) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Condition) $+ \beta_{32}$ (Age group) $+ \beta_{33}$ (Age group x Condition) $+ r_{3j}$.
$^+p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 3.5

Time constraints

The average number of verbal reminders provided by the experimenter to participants to study faster as a function of list and assigned study condition in Experiments 3A and 3B

		Segment 1					Segment 2				
	Condition	L1	L2	L3	L4	Average	L5	L6	L7	L8	Average
Experiment 3A	Constant-Fast [R-R]	0.75 (0.68)	0.50 (0.93)	0.13 (0.34)	0.21 (0.51)	0.40 (0.32)	0.29 (0.55)	0.13 (0.34)	0.25 (0.68)	0.13 (0.45)	0.20 (0.34)
	Constant-Slow [5-5]	--	--	--	--	--	--	--	--	--	--
	Speed Up [5-R]	--	--	--	--	--	2.13 (2.29)	0.17 (0.65)	0.63 (1.88)	0.74 (1.54)	1.00 (1.70)
	Slow Down [R-5]	1.26 (1.39)	0.39 (0.89)	0.00 (0.00)	0.22 (0.60)	0.51 (0.82)	--	--	--	--	--
	Average	1.00 (1.10)	0.45 (0.90)	0.07 (0.25)	0.21 (0.55)	0.45 (0.61)	1.21 (1.89)	0.15 (0.51)	0.44 (1.41)	0.43 (1.16)	0.60 (1.28)
Experiment 3B	Constant-Fast [R-R]	1.55 (1.67)	0.68 (1.07)	0.57 (1.12)	0.50 (1.19)	0.83 (1.03)	0.22 (0.44)	0.30 (0.80)	0.30 (0.72)	0.00 (0.00)	0.21 (0.40)
	Constant-Slow [5-5]	--	--	--	--	--	--	--	--	--	--
	Speed Up [5-R]	--	--	--	--	--	4.05 (3.73)	2.14 (2.99)	1.19 (2.40)	1.52 (3.46)	2.23 (2.75)
	Slow Down [R-5]	1.30 (2.29)	0.65 (1.24)	0.35 (1.16)	0.50 (0.94)	0.70 (1.12)	--	--	--	--	--
	Average	1.96 (2.06)	1.14 (2.51)	0.94 (2.56)	0.90 (2.11)	1.23 (2.37)	2.20 (3.44)	1.28 (2.42)	0.74 (1.73)	0.80 (2.47)	1.25 (2.23)

Note. Standard deviations are presented in parentheticals. "L1" through "L8" refer to Lists 1 through 8. [5] reflects a constant study rate of 5 seconds during the respective segment of the task; [R] reflects a rushed but self-paced study rate with accompanying point penalizations for study exceeding a rate of 1 second for any given item. Verbal reminders were only provided by the experimenter during periods of rushing.

Table 3.6

Time constraints

Recall as a function of segment, list, and study condition in Experiments 3A and 3B

	Condition	Segment 1					Segment 2					Segment 3				
		L1	L2	L3	L4	Avg	L5	L6	L7	L8	Avg	L9	L10	L11	L12	Avg
Experiment 3A	Constant-Fast [R-R]	.25 (.09)	.29 (.11)	.28 (.11)	.26 (.09)	.27 (.07)	.27 (.10)	.26 (.11)	.30 (.12)	.29 (.09)	.28 (.08)	.47 (.19)	.49 (.22)	.52 (.22)	.58 (.22)	.51 (.19)
	Constant-Slow [5-5]	.39 (.13)	.46 (.18)	.50 (.18)	.50 (.21)	.46 (.15)	.54 (.21)	.53 (.21)	.53 (.24)	.56 (.23)	.54 (.21)	.57 (.22)	.56 (.19)	.53 (.25)	.55 (.21)	.55 (.20)
	Speed Up [5-R]	.39 (.17)	.44 (.17)	.46 (.18)	.46 (.14)	.44 (.14)	.24 (.11)	.26 (.11)	.24 (.08)	.29 (.15)	.26 (.09)	.54 (.21)	.51 (.23)	.50 (.23)	.50 (.24)	.52 (.21)
	Slow Down [R-5]	.28 (.14)	.29 (.12)	.33 (.11)	.30 (.15)	.30 (.11)	.250 (.21)	.53 (.21)	.54 (.18)	.53 (.20)	.53 (.19)	.61 (.22)	.59 (.26)	.58 (.22)	.56 (.24)	.59 (.22)
	Average	.33 (.15)	.37 (.16)	.39 (.17)	.38 (.18)	.37 (.14)	.39 (.21)	.39 (.21)	.40 (.21)	.42 (.22)	.40 (.20)	.55 (.21)	.54 (.22)	.53 (.23)	.55 (.23)	.54 (.21)
Experiment 3B	Constant-Fast [R-R]	.26 (.09)	.31 (.11)	.30 (.12)	.28 (.09)	.29 (.07)	.25 (.08)	.27 (.12)	.29 (.11)	.29 (.09)	.28 (.07)	.39 (.17)	.43 (.19)	.39 (.21)	.39 (.20)	.40 (.17)
	Constant-Slow [5-5]	.42 (.15)	.40 (.14)	.42 (.13)	.43 (.11)	.42 (.10)	.46 (.15)	.42 (.11)	.45 (.16)	.42 (.14)	.44 (.12)	.54 (.25)	.47 (.19)	.46 (.16)	.47 (.17)	.48 (.16)
	Speed Up [5-R]	.44 (.16)	.44 (.16)	.50 (.19)	.50 (.15)	.47 (.13)	.25 (.11)	.27 (.09)	.32 (.13)	.31 (.13)	.29 (.08)	.52 (.24)	.51 (.19)	.58 (.22)	.55 (.22)	.54 (.20)
	Slow Down [R-5]	.27 (.10)	.30 (.11)	.28 (.07)	.27 (.10)	.28 (.07)	.45 (.14)	.44 (.10)	.43 (.10)	.44 (.14)	.44 (.09)	.50 (.21)	.48 (.21)	.47 (.21)	.41 (.18)	.47 (.18)
	Average	.35 (.15)	.36 (.14)	.38 (.16)	.37 (.15)	.36 (.13)	.35 (.16)	.35 (.13)	.37 (.14)	.36 (.14)	.36 (.12)	.49 (.22)	.47 (.19)	.48 (.21)	.45 (.20)	.47 (.18)

Note. Standard deviations are presented in parentheticals. "L1" through "L12" refer to Lists 1 through 12. [5] reflects a constant study rate of 5 seconds during the respective segment of the task; [R] reflects a rushed but self-paced study rate with accompanying point penalizations for study exceeding a rate of 1 second for any given item.

Table 3.7

Time constraints

*Two-level hierarchical generalized linear model of recall performance in Segments 1-3
predicted by item value, list, and study condition in Experiment 3A and 3B*

	Experiment 3A			Experiment 3B		
Fixed effects	**Seg 1**	**Seg 2**	**Seg 3**	**Seg 1**	**Seg 2**	**Seg 3**
Intercept (β_{00})	-0.17	0.15	0.22	-0.36***	-0.29**	-0.01
Predictors of intercept						
Cond1: Constant-Fast (CF) v. Constant-Slow (CS) (β_{01})	-0.93***	-1.36***	-0.15	-0.61***	-0.82***	-0.46$^+$
Cond2: Speed Up (SU) v. Constant-Slow (CS) (β_{02})	-0.10	-1.37***	-0.17	0.23	-0.69***	0.27
Cond3: Slow Down (SD) v. Constant-Slow (CS) (β_{03})	-0.75***	-0.06	0.20	-0.63***	0.01	-0.07
Value (β_{10})	0.12***	0.18**	0.20**	0.15***	0.21***	0.20***
Predictors of value						
Cond1: CF v. CS (β_{11})	0.04	0.14$^+$	-0.05	-0.02	0.0003	0.07
Cond2: SU v. CS (β_{12})	-0.03	-0.01	-0.05	-0.05	-0.10$^+$	-0.07
Cond3: SD v. CS (β_{13})	-0.01	-0.003	-0.05	-0.11*	-0.02	0.04
List (β_{20})	0.17**	0.03	-0.03	0.005	-0.04	-0.12$^+$
Predictors of list						
Cond1: CF v. CS (β_{21})	-0.20**	-0.04	0.20**	-0.01	0.08	0.07
Cond2: SU v. CS (β_{22})	-0.08	0.01	-0.02	0.10	0.15*	0.20*
Cond3: SD v. CS (β_{23})	-0.14*	0.01	-0.05	-0.03	0.0005	-0.04
List x Value (β_{30})	0.03	-0.01	-0.004	0.05*	-0.003	0.04$^+$
Predictors of list x value						
Cond1: CF v. CS (β_{31})	0.05$^+$	0.07*	0.04	-0.03	0.03	-0.01
Cond2: SU v. CS (β_{32})	0.01	0.03	0.03	-0.03	0.02	-0.05$^+$
Cond3: SD v. CS (β_{33})	0.01	0.04	0.001	-0.02	0.07*	-0.003
Random effects	**Variance**	**Variance**	**Variance**	**Variance**	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.25***	0.58***	1.00***	0.14***	0.14***	0.79***
Value (r_1)	0.02***	0.05***	0.05***	0.01***	0.03***	0.05***
List (r_2)	0.01	0.003	0.02*	0.003	0.01	0.04***
List x Value (r_3)	0.003**	0.002**	0.003**	0.004***	0.002*	0.002$^+$

Note. "Seg 1" refers to Segment 1, etc. Recall performance was coded as 0 (*not recalled*) or 1 (*recalled*).
Logit link function was used to address the binary dependent variable. Level 1 of the model in each
experiment was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 was of the form
$\pi_{0j} = \beta_{00} + \beta_{01}$ (Cond1) $+ \beta_{02}$ (Cond2) $+ \beta_{03}$ (Cond3) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Cond1) $+ \beta_{12}$ (Cond2) $+ \beta_{13}$
(Cond3) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Cond1) $+ \beta_{22}$ (Cond2) $+ \beta_{23}$ (Cond3) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Cond1) $+ \beta_{32}$
(Cond2) $+ \beta_{33}$ (Cond3) $+ r_{3j}$.
$^+p < .10$ $*p < .05$ $**p < .01$ $***p < .001$

Table 3.8

Time constraints

Total self-paced study time (sec) as a function of segment, list, and study condition in Experiments 3A and 3B

		Segment 1					Segment 2					Segment 3				
	Condition	L1	L2	L3	L4	Avg	L5	L6	L7	L8	Avg	L9	L10	L11	L12	Avg
Experiment 3A	Constant-Fast [1-1]	23.1 (5.9)	23.8 (7.0)	21.6 (6.0)	21.1 (6.3)	22.4 (5.7)	20.9 (6.4)	18.7 (5.4)	20.1 (6.4)	19.5 (5.8)	19.8 (5.6)	86.9 (48.7)	83.4 (47.1)	85.7 (64.3)	82.2 (57.2)	84.5 (49.6)
	Constant-Slow [5-5]	--	--	--	--	--	--	--	--	--	--	137.6 (76.7)	118.3 (68.8)	112.3 (68.0)	104.1 (6.6)	118.1 (65.3)
	Speed Up [5-1]	--	--	--	--	--	27.4 (8.0)	22.2 (9.7)	21.5 (8.1)	21.7 (7.6)	23.2 (7.8)	113.8 (56.7)	98.5 (52.7)	98.1 (56.5)	98.2 (62.7)	102.1 (53.4)
	Slow Down [1-5]	24.2 (8.6)	21.7 (6.1)	21.0 (5.0)	20.2 (5.1)	21.8 (5.7)	--	--	--	--	--	126.7 (52.4)	104.4 (44.8)	95.3 (45.2)	90.6 (45.0)	104.2 (43.5)
	Average											116.2 (61.7)	101.2 (54.7)	97.8 (59.0)	93.8 (56.5)	
Experiment 3B	Constant-Fast [1-1]	28.8 (11.5)	25.8 (6.8)	25.3 (9.7)	23.7 (7.0)	25.9 (7.7)	23.2 (8.2)	22.0 (7.0)	21.6 (6.7)	20.6 (5.8)	21.9 (6.5)	62.2 (26.3)	55.4 (27.0)	52.0 (26.2)	55.6 (3.9)	56.3 (24.0)
	Constant-Slow [5-5]	--	--	--	--	--	--	--	--	--	--	129.9 (85.0)	99.9 (67.2)	86.9 (6.2)	82.7 (56.8)	99.8 (59.1)
	Speed Up [5-1]	--	--	--	--	--	37.9 (13.7)	29.6 (12.9)	27.0 (8.9)	27.1 (1.9)	30.4 (1.6)	89.6 (52.3)	84.8 (45.7)	77.9 (43.7)	75.3 (4.9)	81.9 (43.7)
	Slow Down [1-5]	30.3 (9.9)	27.5 (7.7)	25.7 (6.1)	25.3 (8.0)	27.2 (6.8)	--	--	--	--	--	123.3 (12.3)	110.1 (114.1)	91.3 (78.7)	75.2 (36.7)	99.9 (8.2)
	Average	29.6 (1.6)	26.7 (7.3)	25.5 (8.0)	24.5 (7.5)	26.6 (7.2)	30.6 (13.4)	25.8 (11.0)	24.3 (8.3)	23.9 (9.2)	26.1 (9.7)	101.2 (82.6)	87.5 (73.2)	77.0 (56.9)	72.2 (43.0)	84.5 (57.7)

Note. "--" reflects a set study pace of 5 seconds per word (i.e, 100 seconds of study per list). Standard deviations are presented in parentheticals. Averages are calculated as per the conditions under which participants were self-pacing their study (e.g., the Segment 1 averages are an average of pacing times in the Constant-Fast and Slow Down conditions).

Table 3.9

Time constraints

Two-level hierarchical generalized linear model of penalized self-paced study (in Segments 1-2) predicted by item value, list, and study condition in Experiment 3

	Experiment 3A		Experiment 3B	
Fixed effects	**Segment 1**	**Segment 2**	**Segment 1**	**Segment 2**
Intercept (β_{00})	1.12***	0.99***	1.29***	1.09***
Predictors of intercept				
Speed Up (SU) v. Constant-Fast (CF) (β_{01})	___	0.17[+]	___	0.43**
Slow Down (SD) v. Constant-Fast (CF) (β_{01})	-0.03	___	0.07	___
Value (β_{10})	0.004	0.01*	0.004	0.01**
Predictors of value				
SU v. CF (β_{11})	___	0.0005	___	0.01
SD v. CF (β_{11})	0.001	___	0.01	___
List (β_{20})	-0.04*	-0.01	-0.08*	-0.04*
Predictors of list				
SU v. CF (β_{21})	___	-0.08**	___	-0.13***
SD v. CF (β_{21})	-0.02	___	-0.01	___
List x Value (β_{30})	0.01*	-0.002	0.01	-0.001
Predictors of list x value				
SU v. CF (β_{31})	___	-0.002	___	0.01[+]
SD v. CF (β_{31})	-0.01	___	-0.02*	___
Random effects	**Variance**	**Variance**	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.08***	0.12***	0.13***	0.19***
Value (r_1)	0.0001[+]	0.0005***	0.00001	0.0002*
List (r_2)	0.01***	0.01***	0.02***	0.01***
List x Value (r_3)	0.0001[+]	0.0001**	0.0002**	0.00002

Note. The Constant-Slow condition was not included in this analysis as there was no period of self-paced study during Segments 1 and 2, only a constant (experimenter-paced) study rate of 5 seconds per item. The Slow-Down condition was compared against the Constant-Fast condition for the Segment 1 recall data; the Speed-Up condition against the Constant-Fast condition for the Segment 2 recall data.

Recall performance was coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable. Level 1 of the model in each experiment was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) + π_{2j} (List) + π_{3j} (List x Value). Level 2 was of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Cond1) + β_{02} (Cond2) + r_{0j}, $\pi_{1j} = \beta_{10} + \beta_{11}$ (Cond1) + β_{12} (Cond2) + r_{1j}, $\pi_{2j} = \beta_{20} + \beta_{21}$ (Cond1) + β_{22} (Cond2) + r_{2j}, $\pi_{3j} = \beta_{30} + \beta_{31}$ (Cond1) + β_{32} (Cond2) + r_{3j}.
[+]$p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 3.10

Time constraints

Two-level hierarchical generalized linear model of penalization-free, self-paced study (in

Segment 3) predicted by item value, list, and study condition in Experiment 3

Fixed effects	Experiment 3A	Experiment 3B
Intercept (β_{00})	5.90***	4.99***
Predictors of intercept		
Cond1: Constant-Fast (CF) v. Constant-Slow (CS) (β_{01})	-1.68*	-2.18**
Cond2: Speed Up (SU) v. Constant-Slow (CS) (β_{02})	-0.80	-0.90
Cond3: Slow Down (SD) v. Constant-Slow (CS) (β_{03})	-0.69	0.01
Value (β_{10})	0.30**	0.26***
Predictors of value		
Cond1: CF v. CS (β_{11})	-0.11	-0.08
Cond2: SU v. CS (β_{12})	-0.10	-0.16*
Cond3: SD v. CS (β_{13})	-0.01	-0.07
List (β_{20})	-0.53**	-0.77**
Predictors of list		
Cond1: CF v. CS (β_{21})	0.47*	0.66*
Cond2: SU v. CS (β_{22})	0.30	0.52^{+}
Cond3: SD v. CS (β_{23})	-0.05	-0.04
List x Value (β_{30})	-0.05	0.01
Predictors of list x value		
Cond1: CF v. CS (β_{31})	0.07	0.004
Cond2: SU v. CS (β_{32})	0.06	-0.01
Cond3: SD v. CS (β_{33})	0.01	-0.01
Random effects	**Variance**	**Variance**
Intercept (person-level) (r_0)	6.99***	7.58***
Value (r_1)	0.09***	0.08***
List (r_2)	0.33***	1.36***
List x Value (r_3)	0.01*	0.002

Note. Self-pacing was analyzed in seconds. Level 1 models for both outcomes were of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) + π_{2j} (List) + π_{3j} (List x Value). Level 2 models were of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Cond1) + β_{02} (Cond2) + β_{03} (Cond3) + r_{0j}, $\pi_{1j} = \beta_{10} + \beta_{11}$ (Cond1) + β_{12} (Cond2) + β_{13} (Cond3) + r_{1j}, $\pi_{2j} = \beta_{20} + \beta_{21}$ (Cond1) + β_{22} (Cond2) + β_{23} (Cond3) + r_{2j}, $\pi_{3j} = \beta_{30} + \beta_{31}$ (Cond1) + β_{32} (Cond2) + β_{33} (Cond3) + r_{3j}.
$^{+}p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

CHAPTER FOUR:

DISTRACTIONS and DIVIDED ATTENTION DURING ENCODING

on VALUE-DIRECTED REMEMBERING

Portions of the following introductory comments, description of Experiment 1, and conclusion are taken directly from:

The threat of distraction to learning and memory fills campus libraries to capacity come exam time, with students eschewing home comforts (and showers) to maintain undivided attention whilst studying. Permanent sequestration in a hushed library is, however, plainly impossible, and even coveted study cubicles are breached by sounds of typing and whispered conversations. Moreover, there are many situations in which learners actively multitask despite the importance of later remembering presented information (Calderwood, Ackerman, & Conklin, 2014). The ubiquity of mobile devices has even led professors to dissuade or ban their use during lectures, citing the detrimental effects of multitasking—and the visibility of peers' laptop screens—on learning and comprehension (Fried, 2008; Sana, Weston, & Cepeda, 2013).

Costs of divided attention during encoding to memory are plentiful (Castel & Craik, 2003; Craik, Govoni, Naveh-Benjamin, & Anderson, 1996; Naveh-Benjamin, Craik, Perretta, & Tonev, 2000), but the effect on memory for important or valuable information, specifically, remains unclear. Is a student's exam performance at the mercy of a neighbor's radio preferences or the insatiable pull of a messaging app during studying? Or can learners mitigate divided

attention effects by selectively focusing on the most important information, even if some of the less important is lost? The cognitive demands of strategically allocating one's attention may be better met by settings conducive to devoted focus, like a quiet library. On the other hand, distractions may be less perilous if the learner is cognizant of the potential cost of distraction.

Prior work demonstrates that selective attention to, and memory for, the most critical to-be-remembered information can be maintained in spite of circumstances which otherwise result in memory impairments, like insufficient study time (Middlebrooks, Murayama, et al., 2016) and advanced age (Castel et al., 2012). Maintained prioritization of high-value information at the expense of less essential information, despite memory declines, reflects an important dissociation between memory itself and the strategizing in which learners engage during encoding. Selective study signifies an awareness of the limitations of one's study conditions (Castel et al., 2012; Dunlosky et al., 2011; Winne & Hadwin, 1998)—that remembering everything is implausible.

People seem broadly aware that memory suffers under divided attention (Barnes & Dougherty, 2007; Junco & Cotten, 2011), and even overestimate the degree to which performance will diminish (Finley, Benjamin, & McCarley, 2014), but this basic knowledge may be insufficient for motivating selective study. Despite anticipating poorer global performance when multitasking, people often fail to apply this knowledge when making item-by-item judgments of encoding quality and retrieval accuracy (Beaman, Hanczakowski, & Jones, 2014; Kelly & Sahakyan, 2003; Sacher, Taconnat, Souchay, & Isingrini, 2009). So despite acknowledging that memory will likely suffer under divided, learners tend not to account for it when evaluating their own performance, potentially decreasing the likelihood of their adopting a selective study strategy.

Relatedly, distracted learners may be less able to execute a value-based study agenda—even if recognizing the fitness of such an approach—owing to reduced cognitive resources (Dunlosky et al., 2011). Divided attention also seems to have a more pronounced impact when encoding on a deeper, semantic level (Anderson et al., 2000; Craik, 1982), which is precisely the processing in which learners are most likely to engage when studying selectively (Cohen et al., 2014). As such, the very method by which selectivity may be best achieved also seems to be the method most impacted by divided attention. Good intentions notwithstanding, divided attention may render selective study relatively unattainable.

I am proposing three experiments to investigate whether limits to one's attention during encoding affect value-directed remembering, as well as the various factors that may mitigate (or exaggerate) the impact of distractions and multi-tasking. Experiment 1 (Middlebrooks, Kerr, et al., 2017) investigates whether increased demands on attention through active multi-tasking or more passive distractions affect the efficacy and likelihood of value-driven encoding. Experiment 2 investigates whether multi-tasking/distraction affects self-regulated study (specifically, self-pacing during study) that may further impact selectivity. Experiment 3 utilizes to-be-remembered materials that place greater stress on the central executive in working memory to determine whether selectivity continues to be maintained in spite of multi-tasking/distraction.

Experiment 1

A primary goal of Experiment 1 was to examine the effect of divided attention during encoding on the study of, and memory for, valuable information. An additional goal was to investigate whether selectivity is affected by the degree to which the learner is engaged with the distractor, whether actively multitasking or more passively distracted.

Experiment 1A

In Experiment 1A, participants studied the to-be-remembered items while completing a digit detection task or while listening to background music with which they were either familiar or unfamiliar. The costs of a less involving distraction may be less pronounced relative to an attention-dividing activity and, thus, less of an impediment to strategizing. Alternatively, multitasking may be more blatantly injurious to memory, and so learners may be *more* likely to prioritize valuable information when multitasking than when merely distracted, resulting in better memory for the most important information.

Method

Participants

Participants consisted of 192[25] undergraduate students at the University of California, Los Angeles (129 female, 1 unreported), ranging in age from 18 to 30 years ($M = 20.5$, $SD = 1.75$). Participants received partial credit for a course requirement.

Materials

Stimuli.

Stimuli consisted of 6 lists, each containing 20 words. Word length ranged from 4 to 7 letters and averaged to 8.81 ($SD = 1.57$) on the log-transformed Hyperspace Analogue to Language (HAL) frequency scale, with a range from 5.48 to 12.65. The studied words in each list were randomly selected without replacement for each participant from a larger word bank of 280 random nouns and verbs (e.g., *twig*, *button*, *taste*). Each selected word was then randomly assigned a value ranging from 1 to 10 points, with two words assigned to each value per list. Accordingly, one participant might study *twig* in List 1, while another studies *twig* in List 3 or

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[25] Experiment 1A is based on a pooled set of original data ($N = 96$) and replication data ($N = 96$).

not at all. Furthermore, *twig* might be a 3-point word for one participant, but a 10-point word for another participant.

Music distractors.

An exploratory point of interest in the current study was whether or not familiarity with the background music would impact memory and selective study. It may be easier to ignore background music with which you are very familiar and, thus, perhaps somewhat habituated relative to unfamiliar music (Kang & Lakshmanan, 2017; Röer, Bell, & Buchner, 2014). On the other hand, familiar music has been shown to be more enjoyable than unfamiliar music, leading to greater activation in limbic and reward-based neural structures (Pereira et al., 2011). If familiar music heightens dopaminergic, reward-based neural activity, irrespective of the to-be-remembered item's value, then the potentially greater enjoyment resulting from listening to familiar music relative to unfamiliar music could disrupt the selective role that reward-based regions can serve with respect to remembering valuable information specifically (Cohen et al., 2014). Familiar music may also be more likely to activate related memories and thoughts (e.g., remembering other friends that like this song; remembering the last time you heard the song) (Janata, 2009) than unfamiliar music, which could also make familiar music more distracting during study than unfamiliar music.

A pilot study ($N = 48$) was conducted to select the music. Pilot participants were presented with 30-second clips of lyrical songs, along with the song's title and the artist. They rated each song on multiple dimensions, including their familiarity with and liking of the song; they could replay the song clips as desired. The chosen songs—6 familiar and 6 unfamiliar—were consistently rated as being well liked, upbeat, and mood improving. The chosen familiar

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songs had an average of 126.6 beats per minute (BPM) (ranging = 120-129) and the unfamiliar

an average BPM of 124.5 (range = 113-139).

The songs—familiar or unfamiliar as per the study condition—were randomly assigned

without replacement to the lists for each participant. A participant assigned to listen to familiar

music might study List 1 while listening to Katy Perry's "Roar," for example, but another

participant in the same condition might not hear "Roar" until List 4.

Procedure

Participants were randomly assigned to one of four different study conditions: a full

attention (FA) condition; a divided attention (DA) condition; a familiar music (FM) condition;

and an unfamiliar music (UM) condition. Participants were told that they would be shown a

series of word lists, each containing 20 different words, and that each word would be paired with

a value ranging from 1 to 10 points, with two words per point value in each list. Participants

were instructed to remember as many of the presented words as possible while also achieving a

maximal score, a sum of the points associated with each subsequently recalled word. They were

told that they would be asked to recall the words from each list at the conclusion of its

presentation, after which they would be told their score (out of 110 possible points). The words

were shown for 3 seconds each.

Participants in the DA condition were further told that a series of digits would also be

read aloud while they studied and that they were to press the spacebar every time that they heard

a sequence of three odd digits. The digits (numbers 1-9) were randomly generated with

constraints at a rate of 1 per second: unbeknownst to participants, there were exactly eight

instances of three-odd-digit sequences per list and there was never a sequence of four odd digits

in a row played, though there could be one or two odd digits in a row (for which the spacebar should *not* be pressed).

Participants in the FM and UM conditions were told that background music would be playing while they studied the to-be-remembered words. It was explained that they did not need to do anything with the music or remember it—it would simply be playing in the background— and that their task was to memorize the items while maximizing their score. Each of the songs played for the full 60-second duration of each list presentation. At the conclusion of the task, participants were also asked to indicate whether they were familiar or unfamiliar with the songs that were played: all FM participants reported being familiar with the music and all UM participants reported being unfamiliar with the music, consistent with the pilot responses initially used to select the songs.[26]

Results

Digit Detection Performance

Responses on the digit detection task by participants in the DA condition were scored as correct when made between 50-1200 milliseconds of the third odd digit in a sequence being played. (Responses made within the 50 milliseconds following the third odd digit were not recorded as correct as the initiation of any such presses would have been made prior to the third digit being played and thus presumptive.) Participants correctly identified an average of 1.87 out of 8 sequences ($SD = 0.42$) throughout the experiment. There were also an average of 1.26 incorrect detections ($SD = 0.18$), wherein participants pressed the spacebar to indicate that three

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[26] Participants in the replication experiment also completed a modified operation span task (Oswald, McAbee, Redick, & Hambrick, 2015) to determine whether the impact of the digit detection task or the background music on selectivity would differ as a function of individual differences in working memory capacity. It was thought that participants with greater working memory capacity might be better able to inhibit the distractors during study and so devote more of their attention towards the valuable information. There were, however, no evident differences in selectivity as a consequence of individual operation span scores within or between study conditions, consistent with prior research that has also failed to find differences in selectivity based on working memory capacity in healthy younger adults (Castel et al., 2009; Cohen et al., 2014; but see Hayes et al., 2013).

odd digits had been played when, in fact, they had not. There were no participants who completely neglected the digit detection and failed to press the spacebar at any point—all participants identified at least one sequence during each studied list.

Overall Recall Performance

The proportion of items recalled as a function of study condition and list are provided in Table 4.1. Initial analyses were conducted to determine whether there was an effect of divided attention via digit detection and/or music distractions on overall recall performance, irrespective of item value. Bonferronni adjustments were made in all cases of multiple comparisons post hoc testing and Greenhouse-Geisser adjustments in the case of sphericity violations. A 4(Condition: Full attention, Divided attention, Familiar music, Unfamiliar music) x 6(List) repeated-measures analysis of variance (ANOVA) revealed a significant effect of list, $F(4.56, 857.92) = 14.26$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .04$, with the total number of items recalled, on average, significantly lower in List 1 than Lists 2-6, $ps_{adj} < .001$. Critically, there was also a significant effect of condition, $F(3, 188) = 15.22$, $MSE = 0.06$, $p < .001$, $\eta^2_G = .11$, with participants in the DA condition recalling significantly fewer items overall than participants in the other conditions ($ps_{adj} < .001$). There were no other significant differences between conditions, nor was there a significant interaction between list and condition.

These results confirm that the digit detection task completed by participants in the DA condition diminished participants' ability to remember the items relative to the FA condition, consistent with prior research (Craik et al., 1996; Castel & Craik, 2003; Naveh-Benjamin et al., 2000). Background music in the FM and UM conditions did not, however, similarly impact general recall; while it is certainly possible that the music was distracting during study, it was evidently not distracting enough to actually impair recall.

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Value-Directed Remembering and Selectivity

Figure 4.1 depicts recall performance as per item value and study condition.
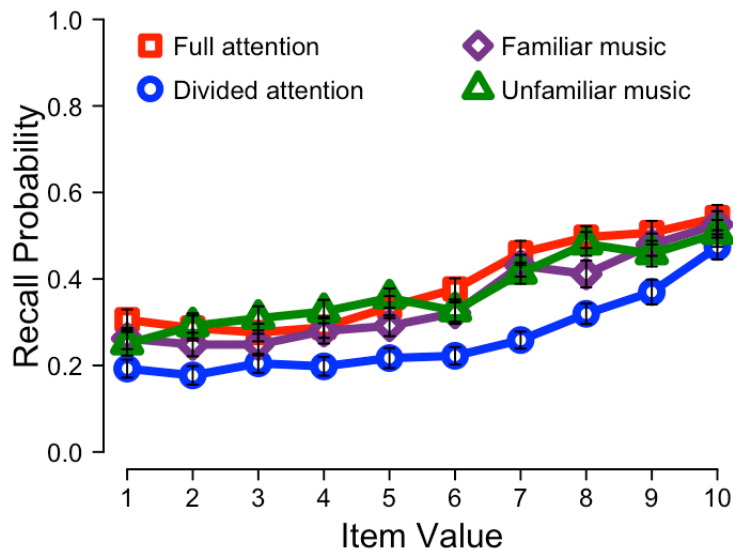
Figure 4.1. Recall probability, averaged across lists, as a function of item value and assigned study condition in Experiment 1A. Error bars reflect standard error.

HLM analysis was used to analyze recall as a function of list and item value between the four

study conditions (Castel et al., 2013; Middlebrooks, McGillivray, et al., 2016; Middlebrooks,

Murayama, et al., 2016; Raudenbush & Bryk, 2002). Item-level recall performance (based on a

Bernoulli distribution, with 0 = *not recalled* and 1 = *recalled*; level 1= items; level 2 =

participants) was modeled as a function of each item's value, the list in which it was presented,

and the interaction between value and list. Value and List were entered as group-mean centered

variables, such that Value was anchored on the mean value point (5.5) and List was anchored on

the mean list (3.5). The model further included the study conditions as level-2 predictors of those

level-1 effects via three dummy-coded variables, with the Full attention condition as the

reference group. Although the FA condition served as the control against which effects of

distraction and divided attention on recall and selectivity could be compared, the following

results are consistent regardless of the reference group. Table 4.2 reports the tested model and its estimated regression coefficients.

Value was a significantly positive predictor of recall performance in the FA condition ($\beta_{10} = 0.16$, $p < .001$), and this relationship was not significantly different across conditions, ps > .25. Thus, participants across all study conditions were $e^{0.16} = 1.17$ times more likely to recall a studied word for each one-unit increase in its value. The odds of recalling a 10-point item, for example, were $e^{0.16 * 10} = 4.88$ times greater than the odds of recalling a 1-point item, demonstrating a clear effect of item importance/value on subsequent memory. There was not a significant effect of List on recall for participants in the FA condition ($\beta_{20} = 0.04$, $p = .08$), nor was there an evident Condition x List interaction, ps > .076. (Note that the use of effect coding in the HLM model, rather than dummy coding, complements the main effect of List reflected by the previous ANOVA.)

There was, however, a significant List x Value interaction ($\beta_{30} = 0.03$, $p = .001$), such that selectivity increased with continued task experience. Participants were more likely to consider item importance whilst studying and adjust their strategies to compensate being unable to remember all of the items as the experiment progressed, regardless of the presence (or extent) of study distractions (see Figure 4.2).
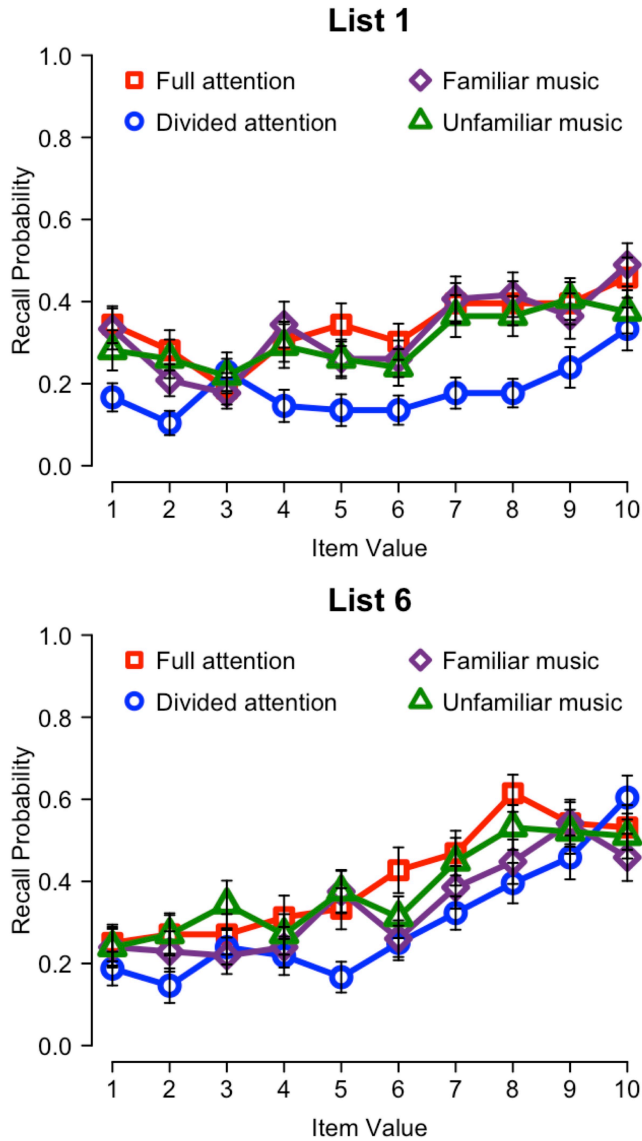
Figure 4.2. Recall probability in Experiment 1A as per item value and study condition in the first and final lists, demonstrating increased attention to value across conditions with continued task experience. Error bars reflect standard error.

Bayesian Analysis

The nonsignificant effect of study condition in the HLM analyses on the relationship between item value and recall probability suggests that selectivity and value-directed remembering in the current experiment was in no way affected by the music distractors or the digit detection task during study. As these results are based upon null hypothesis testing, though, it is truthfully impossible to claim the absence of such condition effects (despite the large sample

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size, $N = 192$). Additionally, the reported analyses are based on an aggregate of the original sample and the replication sample, on which interim analyses were conducted. There was no intention to stop data collection contingent upon the obtained results, but interim analyses can make the interpretation of obtained p-values ambiguous (Murayama, Pekrun, et al., 2014). Accordingly, a Bayesian analysis was also performed in order to surmount the potential complications of having conducted interim analyses on the pooled data set and to confirm the null effect of condition suggested by the HLM analysis (Middlebrooks, Murayama, et al., 2016).

Item recall was first regressed on item value within each list for each participant using logistic regression. A 4(Condition) x 6(List) repeated-measures Bayesian ANOVA was then conducted on these value slopes using JASP software with default priors (Love et al., 2015). The resultant Bayes Factor$_{10}$ (BF$_{10}$), which reflects the probability of the data under the alternative hypotheses relative to the null, for Condition was 0.015. The present data are thus $1/0.015 = 66.67$ times more likely to be consistent with the null model than the alternative, providing strong evidence for a null effect of study condition on the value-recall relationship (Kass & Raftery, 1995) and confirming that selectivity during study was comparable across the study conditions.

Discussion

The results of Experiment 1A indicate that participants who were either distracted by music (regardless of familiarity) or whose attention was divided by the digit detection task studied the valuable information as selectively as participants in the full attention control condition. Memory overall was not impaired by the music distractors relative to memory in the full attention condition, so the fact that selectivity remained could reflect comparable availability

of attentional resources during study. Memory *was*, however, impaired by the digit detection task and yet selectivity was maintained.

It is possible, however, that the digit detection task was simply too difficult for participants and so was largely neglected; although a common method of dividing attention, digit detection performance in Experiment 1A was notably lower than that which has been reported in other studies (e.g., Castel & Craik, 2003; Jacoby, 1991). The nature of the primary task—to not only remember presented items, but to also consider their values, contrast performance with earlier feedback, evaluate and execute strategies, etc.—may have amplified the difficulty of the digit detection task. In light of this possibility, it is unclear as to whether selectivity was maintained *in spite* of divided attention or because attention was not actually divided.

Experiment 1B

Experiment 1B was designed in part to address the concern that low digit detection performance in Experiment 1A reflected a failure to properly divide participants' study, hence their maintained selectivity. Experiment 1B also examined the extent to which participants' attending to the divided attention task may have deviated as a consequence of the studied material's value. Instead of a digit detection task, participants' attention in Experiment 1B was divided using three different tone detection tasks, across which the difficulty, and extent to which working memory may be required to complete the concurrent task, was increased to determine whether selectivity and value-directed remembering would be differentially impacted. (Tone detection was used in place of digits in an effort to reduce the potential conflict between the numbers in the divided attention task and the values of the to-be-remembered items, which may have contributed to the low digit detection performance in Experiment 1A.) Responses to these tone tasks were made during each item's presentation, enabling a more detailed analysis than

was possible in Experiment 1A of the potential costs and shifts of participants' attention between the studied material and divided attention task.

Method

Participants

Participants consisted of 96 undergraduate students at the University of California, Los Angeles (75 female, 1 unreported), ranging in age from 18 to 27 years ($M = 20.61$, $SD = 1.44$). Participants received partial credit for a course requirement.

Materials & Procedure

The studied items in Experiment 1B were the same as in Experiment 1A. Participants were randomly assigned to one of four study conditions: a full attention (FA) condition; a tone monitoring (TM) condition; a paired tones (PT) condition; and a 1-back condition. As in Experiment 1A, participants were told that they would be shown a series of words lists and that each word would be paired with a value ranging from 1 to 10 points, the goal of the task being to recall as many words as possible at test while also maximizing one's recall score. The words were presented for 3 seconds at a time. Participants in all but the FA condition were further told that they would hear a series of low- (400 Hz) and high-pitched (900 Hz) tones played in the background during study. These tones were played continuously throughout the study of each list, and each tone was played for 1 second with a 750-millisecond inter-tone interval, resulting in exactly two tones being played during each to-be-remembered item's presentation. The exact tone sequence was generated randomly for each participant, with the only constraints being that the same pitch could not play more than three times in a row.

Participants in the TM condition were instructed to indicate via keyboard after each pitch was played whether it was of low- or high-pitch. Participants in the PT condition were to indicate

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via keyboard whether the two tones played during a word's presentation were the same pitch (i.e., both low-pitched or both high-pitched) or of different pitch (i.e., one low-pitched and one high-pitched). Participants in the 1-back condition were to indicate via keyboard whether the current tone was the same pitch as the previous tone or different pitch. (Across conditions, sticky notes were placed on the appropriate keys to increase ease of responding.) Participants in the TM and 1-back conditions thus provided two tone-related responses for each word and participants in the PT condition provided one response after the second tone was played. A prompt to attend to the tone task was presented to participants who failed to respond (correctly or incorrectly) to more than three detections. An example of how the tone-related responses differed across conditions is provided in Figure 4.3.
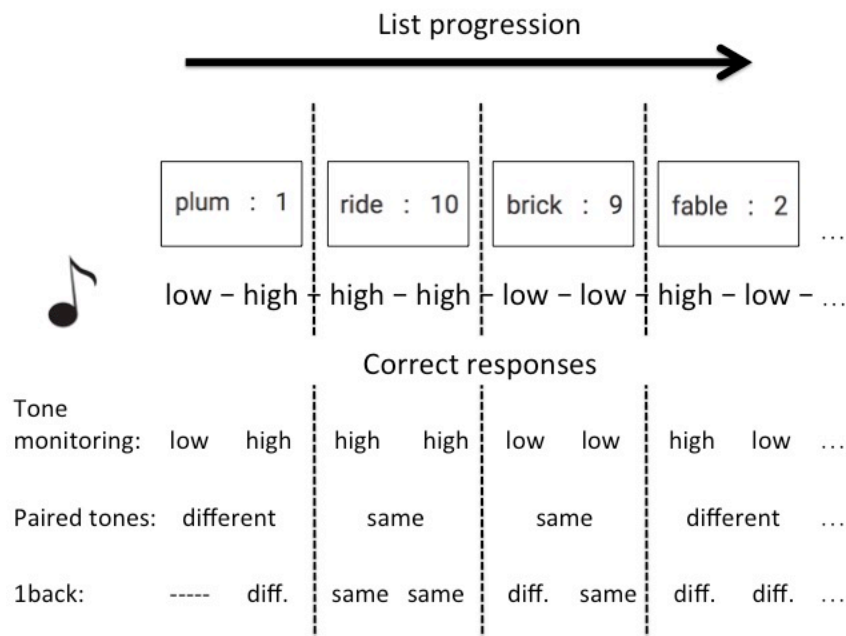
Figure 4.3. An example of a tone sequence distributed across items during study and the correct responses as per the three tone detection conditions in Experiment 1B.

In the TM condition, participants were not required to keep track of the tones playing or remember anything about them, but were only to report the pitch of the tone in the moment. Contrastingly, participants in the PT condition had to determine and remember the pitch of the

first tone played during a word's presentation and then compare it to the second tone played before providing a response, which should have required more working memory resources than in the TM condition. Working memory demand was presumed to be the most stressed in the 1-back condition as participants had to continuously monitor and compare tones across studied items, repeatedly updating the tone against which they were to compare the currently playing tone.

Results

Overall Recall Performance

The proportion of items recalled as a function of study condition and list are provided in Table 4.1. As in Experiment 1A, initial analyses were conducted to determine whether there was an effect of divided attention across the different tone conditions on overall recall performance, irrespective of item value. Bonferroni adjustments were made in all cases of multiple comparisons post hoc testing and Greenhouse-Geisser adjustments in the case of sphericity violations. A 4(Condition: Full attention, Tone monitoring, Paired tones, 1-back) x 6(List) repeated-measures ANOVA revealed a significant effect of condition, $F(3, 92) = 17.20$, $MSE = 0.05$, $p < .001$, $\eta^2_G = .25$, with participants in the FA condition recalling significantly more items overall than participants in the other conditions ($ps_{adj} < .001$). There was also a significant list x condition interaction, $F(15, 460) = 2.00$, $MSE = 0.01$, $p = .01$, $\eta^2_G = .03$. Although total recall did not change significantly across lists in the FA condition ($p > .25$), there was a significant effect of list in the other conditions ($ps < .03$), whereby the total number of items recalled increased with continued task experience. Finally, there was a significant effect of list, $F(4.45, 409.07) = 11.50$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .05$, such that total recall in the first three lists was significantly lower than in the latter three lists.

These results confirm that the tone detection task diminished participants' ability to remember the presented items relative to full attention study, consistent with prior research (Craik et al., 1996; Gardiner & Parkin, 1990). Notably, there were no significant differences in recall among the three tone detection conditions, despite differences in the demands of the tone task.

Value-Directed Remembering and Selectivity

Figure 4.4 depicts recall performance as per item value and study condition.
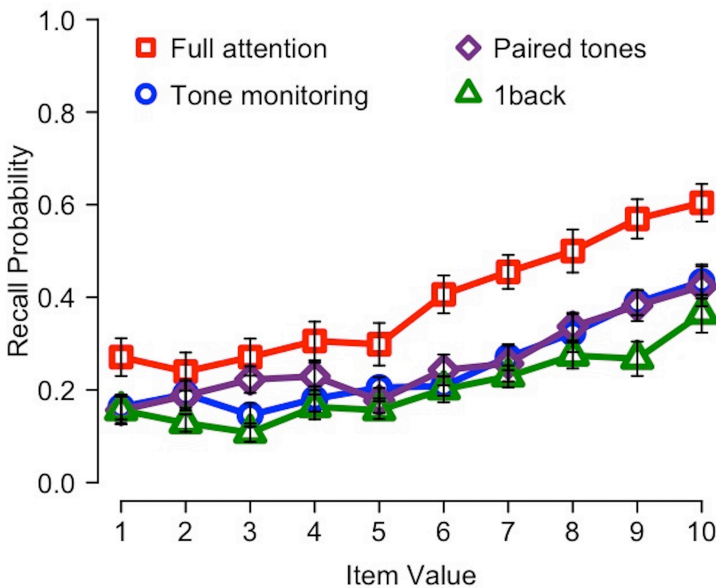
Figure 4.4. Recall probability, across lists, as per item value and study condition in Experiment 1B. Error bars reflect standard error.

As in Experiment 1A, hierarchical linear modeling (HLM) was used to analyze recall as a function of list and item value between the four study conditions. The model used was identical to that of Experiment 1 but for the differences in the actual conditions. Table 4.3 reports the tested model and its estimated regression coefficients.

Value was a significantly positive predictor of recall performance in the FA condition ($\beta_{10} = 0.21$, $p < .001$) and this relationship between item value and recall likelihood was not significantly different across conditions, $ps > .34$. There was also a significant List x Value

interaction in the FA condition ($\beta_{30} = 0.03$, $p = .01$)—which, again, did not differ across conditions, ps > .11—such that selectivity increased with continued task experience, as illustrated in Figure 4.5.
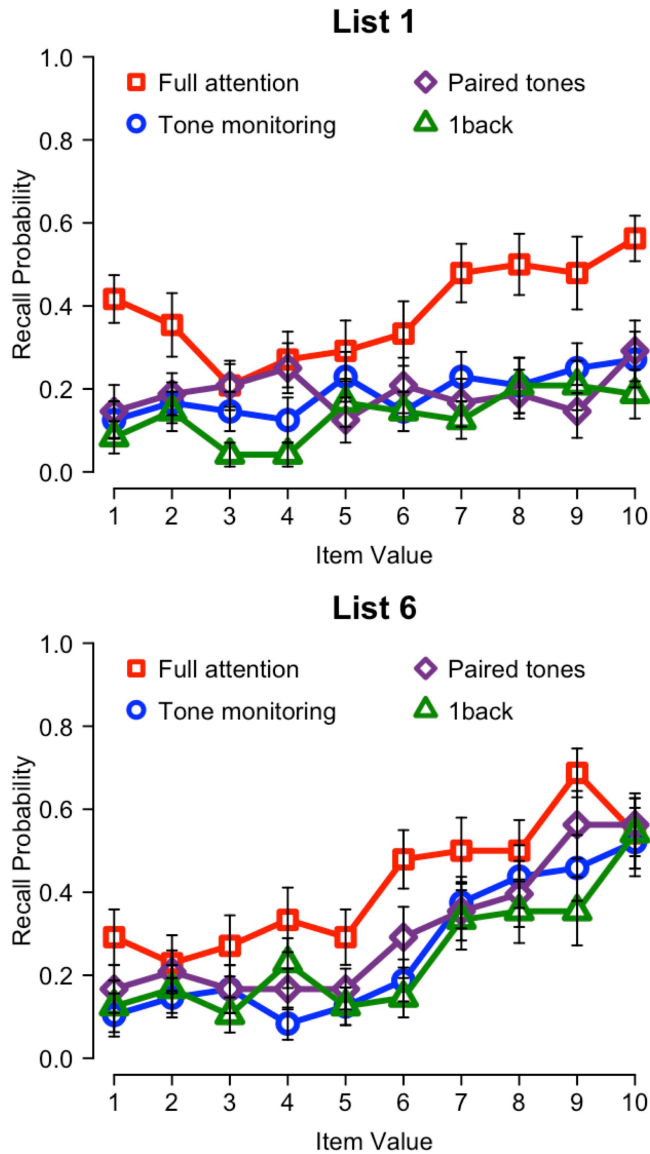
Figure 4.5. Recall probability in Experiment 1B as per item value and study condition in List 1 and List 6 (i.e., the final studied list), demonstrating increased attention to value with continued task experience. Error bars reflect standard error.

These results are consistent with Experiment 1A: Despite impairing overall recall, the tone detection tasks did not result in significant changes to selectivity relative to the full attention study condition.

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Tone Detection Performance

Responses to the tone detection task across the conditions were scored as correct when made between 50-1750 milliseconds of the respective tone's onset. Accurate tone responding within a list in the TM and 1-back conditions was out of 40 (i.e., 2 responses per word) and out of 20 (i.e., 1 response per word) in the PT condition.

A 3(Condition: Tone monitoring, Paired tones, 1-back) x 6(List) repeated-measures ANOVA was conducted in order to assess whether overall tone detection accuracy differed as a consequence of the task demands—namely, the extent to which previously heard tones had to be maintained/remembered in order to provide an accurate response. There was a significant effect of list, $F(2.87, 198.18) = 1.44$, $MSE = 0.03$, $p < .001$, $\eta^2_G = .05$, such that detection accuracy was significantly lower in List 1 than in Lists 2-6, $ps_{adj} < .02$. There was also a significant effect of condition, $F(2, 69) = 4.01$, $MSE = 0.17$, $p = .02$, $\eta^2_G = .07$. Participants in the TM condition accurately responded to a significantly greater proportion of the tone events ($M = .78$, $SD = .19$) than participants in the 1-back condition ($M = .66$, $SD = .17$), $p_{adj} = .04$. Tone performance in the PT condition ($M = .77$, $SD = .13$) was also marginally greater than in the 1-back condition, $p_{adj} = .07$, but did not significantly differ from the TM condition. So, participants were less able to successfully complete the 1-back tone detection task than the other tone tasks, consistent with the predicted difference in task difficulty owing to an increase in task demands. That performance did not differ between the TM and PT condition suggests that the difference in the two tasks' demands may not have differentially impacted their level of difficulty. Regardless, average performance indicates that participants were actively engaged in the tone tasks, assuaging the concerns in Experiment 1 as to the extent to which digit detection performance actually divided attention.

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Two HLM analyses were also conducted to determine whether tone detection accuracy and the time (in seconds) that it took participants to make their tone-related responses in the three tone conditions differed owing to item value, the list in which it appeared, and/or whether the effect of value on tone accuracy changed across lists. (Such an analysis was not possible in Experiment 1 owing to the low digit detection performance, in terms of both response rates and response accuracy.) The tested models and their estimated regression coefficients are provided in Table 4.4.

Although there were no evident effects of value or list on tone response accuracy, there was a significant effect of list on reaction time, such that participants came to make their tone responses significantly faster with continued task experience ($\beta_{20} = -0.02$, $p = .001$). There was also a small, but significant list x value interaction with respect to reaction time, such that value became slightly more predictive of reaction time across lists ($\beta_{30} = 0.003$, $p = .001$), with participants responding slightly more slowly when concurrently studying a high-value item. In general, however, value did not predict reaction time ($\beta_{10} = 0.002$, $p > .25$). The results of these analyses indicate that participants were not only engaged with the tone detection tasks, as evidenced by their overall response accuracy, but also that participants did not strategically neglect the tone task when presented with more valuable materials. Rather, participants were engaged throughout study with the concurrent tone task and consistently so across items, regardless of their values.

Discussion

Although participants in the tone conditions recalled fewer items than those who studied under full attention, recall of the most important items did not differ relative to full attention. Under divided attention, participants may have adjusted by selectively allocating their attention

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to the high-value items and refining their strategy with continued task experience, as suggested by performance in later lists (see Figure 4.5). Overall, these results provide a more detailed analysis of attention during encoding of high- and low-value items and support the main findings from Experiment 1A.

In Experiment 1A, participants studied the to-be-remembered items while completing a digit detection task or while listening to familiar/unfamiliar background music. Participants in the digit detection condition remembered fewer items overall, but there were no significant differences in memory for the higher-valued items across conditions. These results were confirmed in an exact replication of Experiment 1A and upheld in Experiment 1B using a range of tone detection tasks: despite dividing attention during study to varying degrees, selectivity was consistently maintained.

That participants were able to study selectively in spite of the concurrent tasks, and resultant memory impairments, is surprising and warrants further investigation. Divided attention appears most detrimental to elaborative, semantic processing (Anderson et al., 2000; Craik, 1982)— by which value-directed remembering is thought to be best enacted (Cohen et al., 2014)—and so should have compromised the execution of a selective strategy. Moreover, a task designed to decrease available resources should reduce one's ability to study strategically if selecting and executing an optimal strategy depends upon working memory availability (Dunlosky et al., 2011). Even if participants decided on a selective strategy in advance of study (though prior work indicates the need for task experience; Castel et al., 2012), limits to cognitive resources have nevertheless been shown to impair execution of that strategy, even if it had been previously implemented successfully (Dunlosky & Thiede, 2004). The 1-back tone condition in

Experiment 1B was specially intended to place additional demands on working memory reduce relative to the other conditions, yet selectivity was preserved.

The present results do not imply, however that selectivity will always be impervious to distraction. Participants in Experiment 1 were unable to control what they studied or when they studied it. In real-world situations, though, learners often decide when to engage with a distractor (e.g., deciding to check one's email during a pause in a lecture) and/or control the pacing of their primary task (if background music in a café is distracting, a learner could choose to re-read a passage). Pashler, Kang, and Ip (2013) reported multi-tasking effects on memory when study time was experimenter-controlled; when study was self-paced, however, participants compensated for distractions by studying for longer and memory differences were negated. Given the opportunity to self-pace in the current task, participants might believe that they can compensate for distractions by slowing their study, thereby making them less likely to study selectively and, thus, potentially *more* likely to forget the most important items.

Experiment 2

Experiment 2 examined whether participants continue to maintain their selective, value-based study in spite of distractions and multi-tasking when self-pacing their study. Theoretically, it should be easier to implement a value-based study agenda when study is self-paced, but the possibility of compensating for divided attention costs to memory (Pashler et al., 2013) might also make participants less cognizant of the necessity of selective study and thus less likely to adopt or refine a value-based strategy in the first place.

Experiment 2A

Method

Participants

Participants consisted of 96 undergraduate students at the University of California, Los Angeles (77 female), ranging in age from 19 to 26 years ($M = 20.34$, $SD = 1.69$). Participants received partial credit for a course requirement.

Materials & Procedure

The materials and procedure used in Experiment 2A were identical to those of Experiment 1A, save that participants were responsible for self-pacing their study. Rather than the to-be-remembered words being presented at a 3-second rate during encoding, participants were shown one word at a time and were instructed to spend as much or as little time studying the item as they chose before progressing to the next item in the list. In cases of participants' study of a given list in the Familiar music (FM) or Unfamiliar music (UM) condition surpassing the length of the song assigned to that list, the song was restarted and continued playing on a loop until study of the list had finished.

Results

Digit Detection Performance

Responses on the digit detection task by participants in the DA condition were scored as correct when made between 50-1200 milliseconds of the third odd digit in a sequence being played. (Responses made within the 50 milliseconds following the third odd digit were not recorded as correct as the initiation of any such presses would have been made prior to the third digit being played and thus presumptive.) Participants correctly identified an average of

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58.67%[27] of the experienced sequences throughout the experiment; 41.93% of the total

detections made by participants were incorrect. There were no participants who completely

neglected the digit detection and failed to press the spacebar at any point—all participants

(correctly or incorrectly) identified at least one sequence during each studied list.

Overall Recall Performance

Table 4.1 lists the proportion of items recalled as per study condition and list. Initial

analyses were conducted to determine whether there was an effect of divided attention via digit

detection and/or music distractions on overall recall, irrespective of item value. Bonferronni

adjustments were made in all cases of multiple comparisons post hoc testing and Greenhouse-

Geisser adjustments in the case of sphericity violations. A 4(Condition: Full attention, Divided

attention, Familiar music, Unfamiliar music) x 6(List) repeated-measures analysis of variance

(ANOVA) revealed a significant effect of List, $F(3.96, 363.94) = 1.70$, $MSE = 0.02$, $p < .001$, η^2_G

$= .03$, with the total number of items recalled, on average, significantly lower in List 1 than in

subsequent lists[28]. There were no significant differences among conditions ($p = .78$), nor was

there a List x Condition interaction ($p = .12$). The digit detection task completed by participants

in the DA condition diminished their ability to remember the presented items relative to the FA

condition in Experiment 1A, but participants in Experiment 2 were able to compensate for the

task demands when study was self-paced, consistent with Pashler et al. (2013).

Overall Self-Pacing

Analyses were also conducted too determine whether the total amount of time (in

seconds) spent studying differed across conditions. A 4(Condition) x 6(List) repeated-measures

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[27] Because study time was unconstrained, each participant experienced a different number of digit sequences during the course of each list's study period, as per the amount of time which they spent studying it. As such, digit detection performance cannot be reported in a raw score format like in Experiment 1A.

[28] With the Bonferroni correction, List 1 recall was marginally less than that of List 5, $p_{adj} = .07$.

ANOVA revealed a significant effect of List, $F(2.07, 19.26) = 9.14$, $MSE = 100752.13$, $\eta^2_G = .03$, $p < .001$, , with total study time significantly lower in Lists 5 and 6 than in earlier lists. There was not a significant effect of Condition, $p = .30$, but there was a marginally significant Condition x List interaction, $F(6.20, 19.26) = 1.95$, $MSE = 21547.53$, $\eta^2_G = .02$, $p = .07$. Participants in the DA condition spent significantly more time studying List 1 overall than did participants in the FM and UM conditions, $ps_{adj} < .02$. There were no other significant differences across or within lists in total study time as a consequence of study condition.

Value-Directed Remembering and Selectivity

Figure 4.6 depicts recall performance as per item value and study condition.
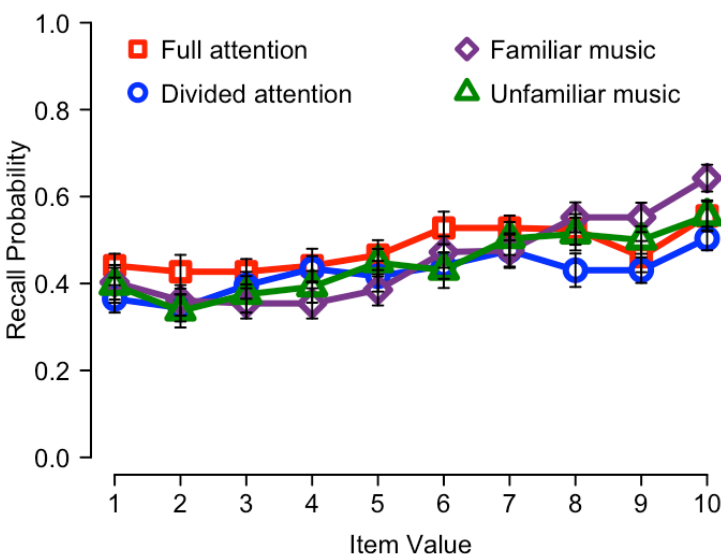
Figure 4.6. Recall probability in Experiment 2A as per item value and study condition averaged across lists. Error bars reflect standard error.

The HLM analysis used to analyze recall in Experiment 2A was identical to that of Experiment 1A. Table 4.2 reports the tested model and estimated regression coefficients.

Value was significantly and positively predictive of recall performance in the FA condition ($\beta_{10} = 0.06$, $p = .03$). Contrary to Experiment 1A, there was a marginally significant difference in this value effect between the FA and the FM conditions ($\beta_{12} = 0.10$, $p = .06$), such

that participants in the FM condition appear to have been more selective than those studying under full attention. Also, contrary to Experiment 1A (and prior research; e.g., Middlebrooks, Murayama, et al., 2016), there was neither a significant List x Value interaction nor a Condition x List x Value interaction, such that the relationship between an item's value and the likelihood of being later recalled did not change with continued task experience—participants did not become more selective.

Self-Regulated Study

Figure 4.7 illustrates the average proportion of total study time spent per item value across lists, as well as the average study time per item value.
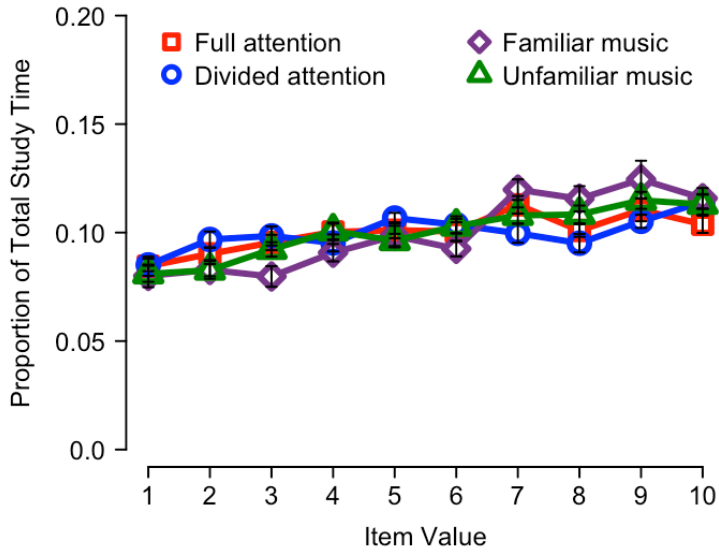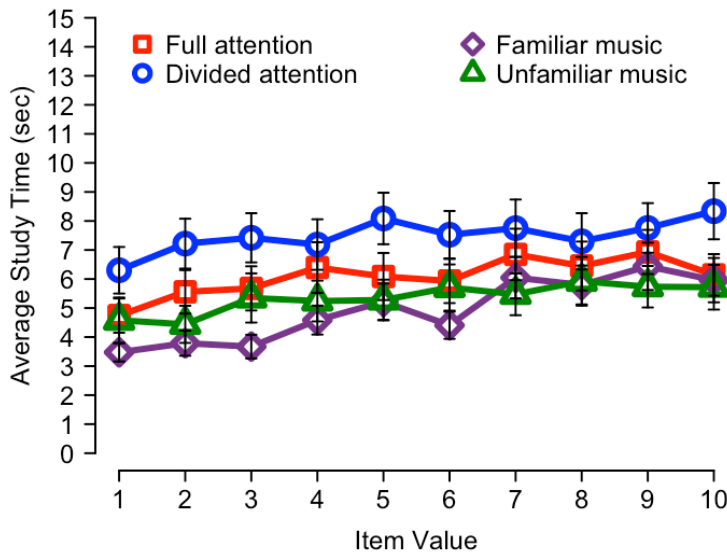
Figure 4.7. (a) The average proportion of self-paced study time and (b) the average study time, in seconds, allocated to each item value across assigned study conditions in Experiment 2A. Error bars reflect standard error.

As study was self-paced, each participant spent a different amount of time studying the items in

each list individually and overall; HLM analyses were again implemented to account for these

individual differences and how they may have interacted with item characteristics and across

conditions. The model used to investigate self-pacing differences was identical to the model used

to investigate value-directed remembering patterns except that the outcome variable was study

time (in seconds) rather than recall. Table 4.2 reports the tested model and its estimated regression coefficients.

There was a significant effect of Value on study time in the FA condition, with 0.17 more seconds spent studying words with each one-unit increased in assigned value ($\beta_{10} = 0.17$, $p = .01$). This relationship between item value and study time did not significantly differ across conditions, $ps > .19$, and there were no other significant differences in study time owing to list and/or condition. These results indicate that participants devoted more study time to high-value items, consistent with prior research (Ariel et al. 2015; Castel et al., 2013; Middlebrooks, Murayama, et al., 2016), but they were not influenced by the presence of distractions or multi-tasking.

Discussion

Participants' study and recall in Experiment 2A was value-driven in that they were more likely to recall high-value information than low-value information, and they spent more time studying high-value items. There was further no apparent effect of divided attention, such that completing an ongoing digit detection task during study hindered neither overall memory nor selectivity. This finding is consistent with Pashler et al. (2013) in that participants were able to compensate for divided attention costs to encoding.

Interestingly, the participants in the FM condition were marginally more selective than those in the FA condition. It is not immediately clear as to why familiar music, but not unfamiliar music, would prove beneficial to study relative to no music. It may be that listening to popular music made the task more enjoyable, leading to greater task engagement, although pilot ratings in Experiment 1A indicated comparable ratings of liking between the unfamiliar and familiar music. Because there were no evident music effects when study was experimenter-paced

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(Experiment 1A) and the familiar music did not result in self-pacing or study time allocation differences, the finding of enhanced selectivity should be interpreted with caution. At the very least, listening to familiar background music did not impair selectivity; were this finding of exclusively enhanced selectivity to be replicated in other self-regulated learning research, understanding the mechanism would be of particular importance as students are probably more likely to listen to music with which they are predominantly familiar than unfamiliar during study.

Although a direct comparison cannot be made statistically, the effect of value on recall in Experiment 2A was visibly lower than was demonstrated in either Experiment 1A or 1B. Participants *did* recall more high-value items than low-value items, consistent with prior research indicating value-directed remembering despite self-pacing (Ariel et al., 2015; Castel et al., 2013), but the ability to self-pace may still have discouraged selective study, irrespective of divided attention tasks or distractors. Participants might have mistakenly believed (given that recall was not at ceiling) that they could better compensate for the number of to-be-remembered items per list exceeding their encoding capacity when length of study was under their purview. As such, they may have been less stringent in determining which value points were worthy of attention and which, relatively speaking, were not. A direct comparison of value-directed remembering under experimenter-paced study and participant-paced study has yet to be conducted.

As opposed to Experiment 1A, digit detection performance in the current experiment does not suggest that participants were unengaged with the task. That recall performance was not impacted by the task in this case, however, suggests that participants may have traded off between studying an item and listening to the digit being played at that moment to determine whether or not a three-odd-digit sequence was likely. Because the sequences were randomly generated, it was not the case that such trade-offs would have been consistent across items, and

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across items there was not an equal demand for responding to the digit detection task. Task-switching may be less imperative for low-value items that participants were not intending to study anyway; similarly, response rates to a divided attention task might decline when presented with high priority items. The extent to which participants may have task-switched thus remains unclear, as do the consequences of such task-switching as a function of item value.

Experiment 2B

A comparison of Experiments 1A and 1B to Experiment 2A suggests that, even under full attention, recall was less driven by value when participants could determine for how long to study each item than when the items were presented at a constant, 3-second rate. Experiment 2B directly contrasted the effects of self-pacing versus experimenter-pacing on selectivity—considering also differences in the effects of self-pacing when total study time was either limited or unlimited—to determine whether selectivity is influenced by the degree to which pacing during study is regulated by the learner.

In Experiment 2A, participants could spend as much time as they wanted studying not only the items individually, but the list as a whole. If participants are generally less selective when self-pacing their study, as suggested by Experiments 1A/1B and 2A, irrespective of distraction, it may be that selectivity is maintained (relative to an experimenter-paced task) when self-pacing is more a function of determining how best to distribute a predetermined allotment of time. In executing a selective, value-based study strategy, a learner makes relative judgments across items, determining which to prioritize at the expense of others in the event that all cannot be remembered. If study time is unlimited, learners may be less likely to make such judgments. In the case of technically unlimited study, participants may ask themselves only "for how long

should I study this 6-point word," not "for how long should I study 6-point words versus 10-point words".

Secondly, Experiment 2B investigated the extent to which participants may compensate for concurrent demands on attention during study by switching between the tasks. Instead of the digit detection task used in Experiment 2A, participants' attention in Experiment 2B was divided using the 1-back tone task from Experiment 1B. Responses to this tone task were made during each item's presentation, allowing for a more detailed analysis of potential shifts in participants' attention between the to-be-remembered material and divided attention task. Though there were no evident shifts in attention from the 1-back tone task in Experiment 1B when study was experimenter-paced, participants may be more inclined to switch between tasks when study pacing is under their control, spending more time studying relative to a full attention but self-paced condition than those whose study is experimenter-paced.

Differences may also arise in task-switching propensity between those who self-pace their study while bound by a total study time constraint and those who self-pace without any limitation. A constraint to total study time should prevent participants from being able to compensate for divided attention effects on total recall (as found in Experiment 2A in support of Pashler et al., 2013), but they may be able to compensate for divided attention effects on specific items by neglecting the tone task (e.g., poorer performance on the tone task when studying high-value items than low-value items). They may also compensate by allocating a greater proportion of their limited time to high-value items than those who are similarly constrained but studying under full attention (i.e., they may be more selective in their study time allocation).

Participants

Participants consisted of 144 undergraduate students (87 female) at the University of California, Los Angeles, ranging in age from 17 to 27 years ($M = 20.16$, $SD = 1.39$). Participants received partial credit for a course requirement.

Materials & Procedure

The studied materials used in Experiment 2B were the same as those used in Experiments 1A, 1B, and 2A. Participants were randomly assigned to one of three pacing conditions. Participants in the experimenter-paced (EP) conditions studied the to-be-remembered items at a constant rate of 2.5 seconds per word (Middlebrooks, Kerr, et al., 2017). Participants in the constrained-pacing (CP) conditions self-paced their study of the 20 items within a list, with the constraint that they could spend no more than 60 seconds studying each list in total (the same total amount of time allotted per list to those in the EP conditions).[29] Finally, participants in the fully self-paced (SP) conditions were, as in Experiment 2A, allowed to spend as much time as they liked studying each item with no list-based limit. Within each of the pacing conditions, half of the participants studied under full attention (FA-EP, FA-CP, and FA-SP) and the other half under divided attention, during which they studied the items according to their assigned pacing rate while concurrently completing the 1-back tone task used in Experiment 1B (see Figure 4.3) (DA-EP, DA-CP, and DA-SP). The 1-back tones were played at 1.75-second intervals (as in Experiment 1B), resulting in 1-2 tones played per item in the DA-EP condition.

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[29] To keep the total possible study time consistent between the EP and CP conditions, items were presented for 2.5 seconds in the EP condition with a 0.5 second interstimulus interval (rather than the 3-second pacing used in Experiments 1A, 1B, and 2A), resulting in a total study duration of 60 seconds per list.

Results

Overall Recall Performance

Table 4.5 lists the proportion of items recalled as per study condition and list. For the purposes of clarity, overall recall is also depicted in Figure 4.8.
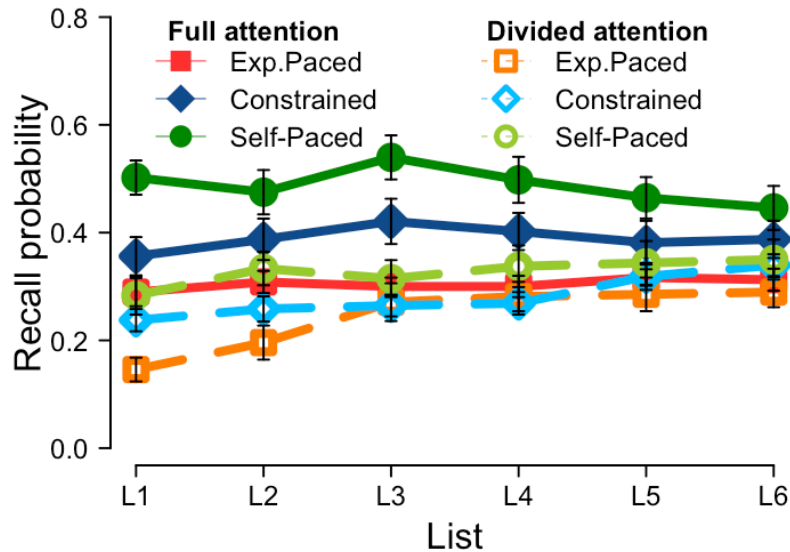
Figure 4.8. Average recall probability across lists as a function of study condition in Experiment 2B. Errors bars reflect standard error.

Initial analyses were conducted to determine whether the extent to which participants were responsible for allocating their study time impacted their overall recall, as well as the effect of the tone monitoring task relative to studying under full attention, irrespective of item value. Bonferroni adjustments were made in all cases of multiple comparisons post hoc testing and Greenhouse-Geisser adjustments in the case of sphericity violations.

A 2(Attention: full attention, divided attention) x 3(Pacing: experimenter-paced, constrained-pacing, self-paced) x 6(List) repeated-measures analysis of variance (ANOVA) revealed a significant main effect of Attention, $F(1, 138) = 25.84$, $MSE = 0.10$, $\eta^2_G = .29$, $p < .001$, with participants studying under full attention recalling, on average, more items overall ($M = .39$, $SD = .15$) than participants studying under divided attention ($M = .28$, $SD = .13$),

confirming the costs of the 1-back tone task to encoding (Middlebrooks, Kerr, et al., 2017). There was further a significant main effect of Pacing, $F(2, 138) = 12.70$, $MSE = 0.10$, $\eta^2_G = .29$, $p < .001$. As expected, participants in the SP conditions who could fully self-pace their study and study each item for as long as they wished recalled more items overall ($M = .41$, $SD = .17$) than those who were limited to 60-seconds of study per list in the CP conditions ($M = .34$, $SD = .14$) or 3-seconds of study per item in the EP conditions ($M = .28$, $SD = .10$), $ps_{adj} < .03$. There was no significant difference in total recall between the EP and CP conditions, $p_{adj} = .07$. In addition to main effects of Attention and Pacing, there was also a main effect of List, $F(5, 690) = 6.80$, $MSE = 0.01$, $\eta^2_G = .05$, $p < .001$, with List 1 recall, on average, significantly lower than that of Lists 3-6, $ps_{adj} < .004$.

There was further a marginally significant three-way Attention x Pacing x List interaction, $F(10, 690) = 1.77$, $MSE = 0.01$, $\eta^2_G = .03$, $p = .06$. Within the Full Attention conditions, there was no effect of List on recall, $p = .22$, but there was a significant effect of Pacing consistent with the described main effect above. Within the Divided Attention conditions, there was a significant List x Pacing interaction, $p = .02$. There was no effect of List on recall in the DA-SP condition; within the DA-CP condition, List 1 recall was significantly lower than that of List 6, with no other significant differences; and within the DA-EP condition, recall in Lists 1-2 was significantly lower than that of Lists 4-6. There were no significant differences in recall under divided attention across the three pacing conditions in Lists 3-6, indicating that participants in the DA-CP and DA-EP conditions were able to adapt to the timing constraints to improve their recall, but that participants in the DA-SP condition did not, perhaps, take advantage of their ability to prolong their study and thus compensate for the costs of concurrently completing the 1-back tone task.

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Overall Self-Pacing

The total time spent studying each list is illustrated in Figure 4.9.
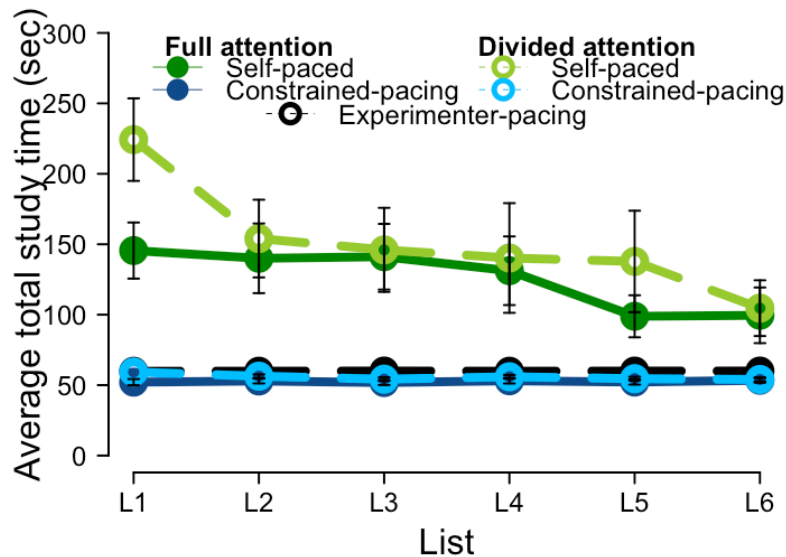
Figure 4.9. The total time spent studying each list of items in Experiment 2B. The 60-seconds per list allotted to participants in the FA-EP and DA-EP conditions is provided for point of reference. Participants in the FA-CP and DA-CP conditions could (but were not obligated to) spend a maximum of 60 seconds studying each list. Error bars reflect standard error.

Further analyses were conducted to determine whether the total amount of time (in seconds) spent studying in the CP and SP conditions, respectively, differed between the full and divided attention conditions. Separate 2(Attention: full attention, divided attention) x 6(List) repeated-measures ANOVAs were conducted for each the CP and SP conditions.[30] Within the SP conditions, there was a significant effect of List on overall study time, $F(2.75, 126.29) = 8.35$, $MSE = 8352.55$, $\eta^2_G = .04$, $p < .001$, with participants spending more time the items in Lists 1-3 than in List 6, $ps_{adj} < .05$. Two one-sample t-tests also confirmed that participants in the FA-SP and DA-SP conditions spent significantly more time studying than the 60 seconds that were

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[30] A Pacing predictor was not included in a single ANOVA because CP participants were limited to a maximum 60-seconds of study, so differences in the total amount of time spent studying per list between the two conditions would reflect the task constraints rather than participants' self-regulated study decisions.

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allocated to participants in the CP and EP conditions, ps < .004 (d = 0.70 and 0.69 under full and divided attention, respectively).

Within the CP conditions, there was a marginal effect of Attention, $F(1, 45) = 3.79$, MSE = 180.07, η^2_G = .04, p = .058; participants in the FA-CP, on average, spent less time studying each list (M = 52.60, SD = 6.23) than participants in the DA-CP condition (M = 55.71, SD = 4.41). Participants in each the FA-CP and DA-CP conditions also spent significantly *less* time studying than the 60 seconds that they had been allotted, ps < .001 (d = -1.18 and -0.97 under full and divided attention, respectively). This difference in study time, however, did not, as reported in the previous section, result in differences in overall recall between the CP and EP conditions, suggesting that CP participants did not cheat themselves despite reaching the end of the study list a bit faster than did those in the EP conditions.

Value-Directed Remembering and Selectivity

Figure 4.10 depicts recall performance (averaged across lists) as per item value and study condition.
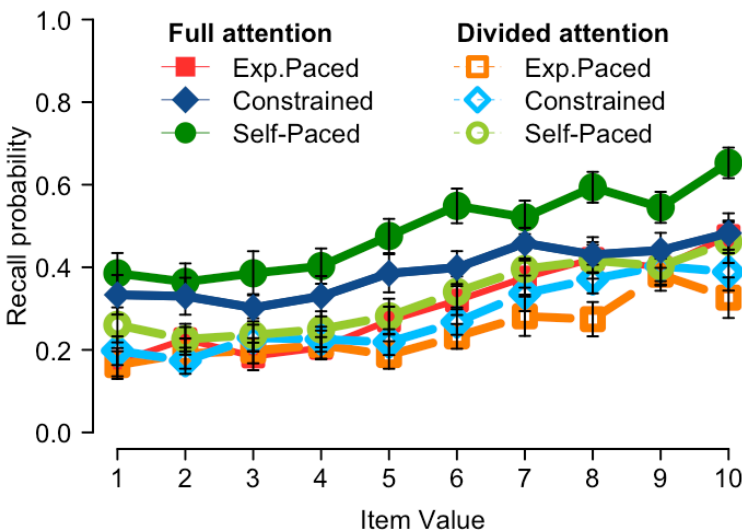
Figure 4.10. Recall probability in Experiment 2B as per item value, state of attention during study, and pacing constraints, averaged across lists. Error bars reflect standard error.

HLM was again used to analyze the probability of recalling an item in Experiment 2B. Item-level recall performance (based on a Bernoulli distribution; 0 = *not recalled*, 1 = *recalled*; level 1 = items, level 2 = participants) was modeled as a function of each item's value, the list in which it was studied, and the interaction between Value and List. Value and List were entered as group-mean-centered variables, such that Value was anchored on the mean value point (5.5) and List was anchored on the mean list (3.5). The model also included the study conditions as Level 2 predictors of those Level 1 effects. Level 2 included an Attention predictor (0 = *full attention*, 1 = *divided attention*), two dummy-coded Pacing predictors—with the experimenter-pacing conditions serving as the reference group), and interaction terms between each of the Pacing predictors and the Attention predictor. Note that the following results are consistent regardless of the chosen Attention and/or Pacing reference groups. Table 4.6 reports the tested model and estimated regression coefficients.

Value significantly and positively predicted recall performance in the FA-EP condition ($\beta_{10} = 0.19$, $p < .001$), and this value-recall relationship did not differ across Attention and/or Pacing conditions, $ps > .09$. Thus, participants across all study conditions were $e^{0.19} = 1.21$ times more likely to recall a studied word for each 1-unit increase in its value than they were to forget it. The odds of recalling a 10-point item, for example, were $e^{0.19*10} = 6.83$ greater than the odds of recalling a 1-point item, demonstrating a distinct effect of item value on subsequent memory.

There was not a significant effect of List on recall in the FA-EP condition ($\beta_{20} = 0.02$, $p < .27$), but there were significant Attention x List ($\beta_{21} = 0.12$, $p < .001$) and Attention x Pacing interactions ($\beta_{23} = -0.07$, $p = .04$), consistent with the patterns demonstrated in the ANOVA

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conducted on overall recall. The effect of list was not qualified by a value interaction across conditions, ps > .18.[31]

Self-Regulated Study

Figure 4.11 illustrates the average proportion of total study time spent per item value across lists in the CP and SP pacing conditions under full and divided attention.
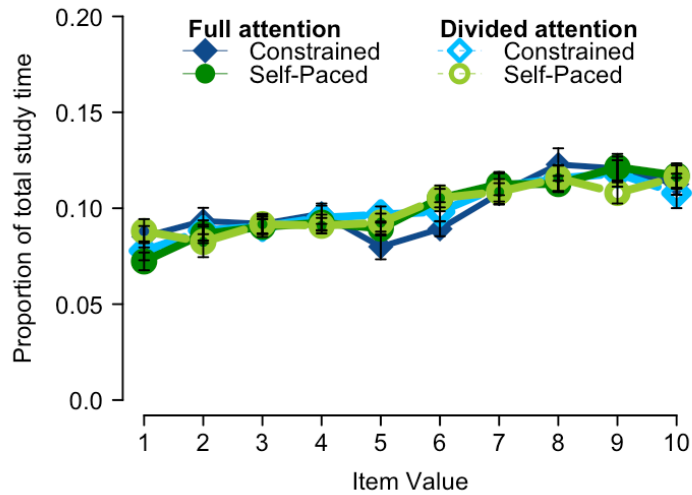
Figure 4.11. The average proportion of total study time (60 seconds in sum for the Constrained-pacing conditions or unlimited in the fully Self-paced conditions) allocated to each item value across assigned conditions in Experiment 2B. Note that the Experimenter-paced conditions are excluded from this graph because the amount of time spent studying each item/item value was a constant and predetermined by the experimenter. Error bars reflect standard error.

As study was self-paced, each participant in the CP and SP conditions spent a different amount of time studying the items in each list individually and overall. HLM analyses were again implemented to account for individual differences in study time (and how these differences may have interacted with item characteristics across pacing and attention conditions). The amount of time one could study, however, was not completely under CP participants' purview like it was in the SP condition; thus, differences between the CP and SP conditions in the effect of value on

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[31] Note that this interaction term was significant when the reference group in Level 2 was either the DA-EP or the DA-CP condition, but in neither case were there further interactions. So, the conclusion that there was or was not a significant List x Value interaction is ultimately determined by the reference group. Erring on the side of caution in an effort to avoid Type I errors, it would seem appropriate to conclude minimal to no evidence of changing selectivity across lists in Experiment 2B.

study time might not only reflect participants' self-regulated study choices, but also the constraints of the task itself. The intention of this analysis was to determine the influence of value on how participants elected to distribute their time available across items, not factors that were outside of participants' control. As a consequence, the outcome variable of the model was the proportion of total study time within a list devoted to each item rather than raw study time in seconds so as to overcome the restriction on potential variance when considering raw study time.

The model used to analyze the effects of value and study condition on self-pacing was similar to that used to analyze recall, with the only differences being the outcome variable—item-level study time (as a proportion of total study time) rather than recall performance—and the Level-2 predictors. Within Level 2, only one pacing predictor ($0 = constrained\text{-}pacing$, $1 = self\text{-}paced$) and one Attention x Pacing interaction predictor were included alongside the Attention predictor ($0 = full\ attention$, $1 = divided\ attention$). Table 4.7 reports the tested model and its estimated regression coefficients. Consistent with Experiment 2A and prior research (e.g., Castel et al., 2014; Middlebrooks, Murayama, et al., 2016), there was a significant effect of Value on study time in the FA-CP condition ($\beta_{10} = 0.002$, $p < .001$), with no significant differences between the divided attention and/or self-paced conditions, $ps > .50$.[32]

For clarity of interpretation, two subsequent HLM analyses were also conducted on the CP and SP conditions separately, with the outcome variable now raw study time (in seconds). Level 1 of these models was the same as in the previous analyses (item-level study time modeled as a function of item value, list, and a list x value interaction term), but Level-2 included only an Attention predictor ($0 = full\ attention$, $1 = divided\ attention$). Table 4.8 reports the tested models and their estimated regression coefficients. In neither the CP nor SP conditions were the

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[32] There was also a marginally significant List x Value interaction in the FA-CP condition ($\beta_{30} = 0.0005$, $p = .053$) with no significant differences across conditions; this interaction was not, however, significant when the predictor terms were recoded with any other reference groups and so should be considered with caution.

differences owing to studying under divided attention relative to full attention, and Value significantly predicted study time. Each one-unit increase resulted in an increase of 0.29 seconds of study in the SP conditions and 0.13 seconds of study in the CP condition.

Tone-Detection Performance

As in Experiment 1B, responses to the 1-back tone-detection task in the divided-attention conditions were scored as correct when made between 50 and 1,750ms of the respective tone's onset. Accuracy for responses to tones within a list was a proportion of accurate tone responses relative to the possible number of responses; in the DA-EP condition, this was fixed at 33 possible responses, but fluctuated as per study duration in the DA-CP and DA-SP conditions.

A 3(Pacing: experimenter-paced, constrained-pacing, self-paced) x 6(List) repeated-measures ANOVA was conducted in order to assess whether overall tone-detection accuracy differed as a consequence of the extent to which participants had to self-regulate their study while completing the concurrent tone task. There was a significant effect of Pacing, $F(2, 69) = 7.72$, $MSE = 0.18$, $\eta^2_G = .14$, $p = .001$; participants in the DA-EP condition were significantly more accurate in their tone responses ($M = .62$, $SD = .16$) than those in either the DA-CP ($M = .43$, $SD = .15$) or DA-SP ($M = .49$, $SD = .20$) conditions (between which there was not a significant difference, $p_{adj} = .72$), $ps_{adj} < .04$. There was neither a significant List effect nor a List x Condition interaction on tone-detection accuracy, $ps > .19$. Interpretation of the accuracy difference between the DA-EP and DA-SP conditions should be made cautiously in light of the fact that the different study durations resulted in a substantially different number of possible tone responses (for point of reference, the maximum number of possible accurate responses across participants for a given list was as high as 562 and an average of 88.10 possible responses across participants and lists). The DA-CP condition, however, could have no more possible responses

than the DA-EP condition and, on average, had significantly fewer owing to the significantly

lower total study time, $t(23.36) = -3.83, p = .003, d = 0.98$. As such, the significantly poorer tone

detection response accuracy in the DA-CP condition relative to DA-EP suggests that the

additional responsibility of having to determine how best to distribute their limited study time

allotment made the 1-back tone task more difficult to complete than it was in the DA-EP

condition.

Because the total number of possible tone responses per item (and, thus, across item

value points) differed across pacing conditions, three separate HLM analyses were conducted to

determine whether tone-detection accuracy within each pacing condition differed owing to item

value or the list in which it appeared, or whether the effect of value on changed across lists.

Level 1 of each model was the same as in prior HLM analyses (including Value, List, and List x

Value predictors); level 2 (i.e., the participant level) included no predictor terms. Table 4.9

reported the tested models and estimated regression coefficients. Across conditions, there were

no significant differences in tone detection accuracy owing to characteristics of the concurrently

studied item, providing no evidence for strategic neglect of the 1-back tone task when studying

high-value items, consistent with Experiment 1B (Middlebrooks, Kerr, et al., 2017).

Discussion

As in Experiment 2A, participants' recall and, as applicable, self-paced study in

Experiment 2B was value-driven. Moreover, the influence of item value on recall did not

significantly differ as a consequence of either the (manipulated) state of participants' attention

during study or the scope of their control over each to-be-learned item's presentation rate.

Participants were just as selective in their prioritization of valuable information when studying

under full attention as when simultaneously completing a 1-back tone detection task, and this

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comparable selectivity was not impacted by the extent to which participants could control their study pacing.

The results of Experiment 2A suggested that learners may be less selective in their study when they can study the to-be-learned material for as long as they wish, but this finding of demonstrably lower selectivity relative to prior research (e.g., Castel et al., 2013; Middlebrooks, McGillivray, et al., 2016; Middlebrooks, Murayama, et al., 2016, 2017; Middlebrooks, Kerr, et al., 2017) was not replicated. The FA-SP condition in Experiment 2B was identical to that of the Full attention condition in Experiment 2A, but the magnitude of the value effect (which did not differ across conditions) was much greater than in Experiment 2A and, again, consistent with magnitudes reported in prior value-directed remembering research. As such, interpretations of Experiment 2A's results are likely best limited to the confirmed (albeit smaller) effect of value on recall and self-pacing reported in prior work (e.g., Castel et al., 2013; Middlebrooks, Murayama, et al., 2016) and the absence of condition differences as similarly reported in prior work (Middlebrooks, Kerr, et al., 2017) and confirmed in Experiment 2B.[33]

Contrary to Experiment 2A and Pashler et al. (2013), participants in Experiment 2B did *not* fully compensate for the costs of divided attention to their recall when given the option to self-pace their study. Participants in the self-paced condition (DA-SP) in Experiment 2B could have conceivably counteracted the demands of the 1-back task to compensate for costs to their memory by studying longer, yet they actually chose to *decrease* their study time with continued

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[33] In fact, the marginally greater value effect in the Familiar music condition relative to the Full attention condition exhibited in Experiment 2A might reflect this unusually low value effect rather than potentially reflecting task difficulty eased by enhanced enjoyment—differences in selectivity arose not as a consequence of the music manipulation, but rather because of individual differences across participants. When the reference group of the condition predictors in level 2 of the HLM analysis of recall probability in Experiment 2A is recoded, the magnitude of the Value beta coefficient ranges from 0.06 in the Full and Divided attention conditions to 0.12 in the Unfamiliar music condition and 0.16 in the Familiar music condition. Although these differences remain nonsignificant (or marginally significant, as reported), the Full attention condition in Experiment 2A may simply—as a function of chance—have included an unusually greater number of less selective participants than did the other conditions.

task experience. The demands of the 1-back tone detection task were, however, demonstrably greater than that of the digit detection task in Experiment 2A. Relative to the digit detection task, far more responses are demanded by the 1-back tone task: Participants need only respond after three odd digits in a row have been played during the digit detection task, but 1-2 responses per studied item (and generally more than that in the event of self-pacing) are demanded by the 1-back task. For point of reference, a participant with 100% accuracy in the digit detection task would respond 8 times over the course of a 60-second study period, but 33 times (in Experiment 2B; 40 times in Experiment 1B) in the 1-back tone task. Even in the case of less-than-perfect responding, though, the 1-back task was far more continuous in nature than the digit detection task. At no point does the 1-back provide a natural pause in which the participant can ignore a tone. In the digit detection task, however, a participant could momentarily cease to attend to the task upon hearing an even digit; for example, a sequence of "1-4-1" would mean that a participant could temporarily (albeit briefly) ignore the digit task upon hearing "4," as at least three more (odd) digits would need to play before a response from the participant is required.

Notably, though, the number of items participants recalled in the DA-SP condition remained stable across lists, despite this decline in total study time. It is possible that the reduced study time reflects a judgment of the benefits of lengthier study against the potential costs of delaying the recall test. Longer study could mean accurate recollection of a few items that might otherwise not have been remembered, but delaying the test might also mean the loss of items that might have been remembered (Ebbinghaus, 1885/1964) (or at least a belief that such a loss might occur) (Cohen, Yan, Halamish, & Bjork, 2013), particularly given the demands of the ongoing tone task, which would have continued as study continued.

In light of the maintained recall levels across lists, DA-SP participants may also have gauged how long it would take to remember a certain number of items (presumably what they considered to be a reasonable number as per capacity limitations) and adjusted their study times to maintain this recall performance without "laboring-in-vain" (Nelson & Leonesio, 1988). The reduction in study time coupled with maintained recall demonstrated by participants in Experiments 2A and 2B (and in prior value-directed remembering research; Middlebrooks, Murayama, et al., 2016) is consistent with their aiming for a personally-defined performance goal with only so much effort as was necessary and sufficient for its achievement, as predicted by the agenda-based regulation model (Dunlosky & Ariel, 2011a). It is perfectly conceivable that DA-SP participants underestimated their potential recall capabilities—with prolonged study, they may well have been able to recall additional items despite the 1-back task—but their pacing behavior suggests that participants self-regulated their study as per a judgment of probable performance and adjusted their effort expenditure accordingly.

Experiments 1A, 1B, 2A, and 2B all confirm that the presence of distractors or the act of multi-tasking does not inherently prevent or impair selective attendance to and prioritization of valuable information during study relative to study under full attention. Experiment 2B (and, to an extent, Experiment 2A), further demonstrates that increased self-regulatory activities during study (viz. having to decide during study how to optimally allocate one's time, limited or not)— and the extra stress this presumably places on the central executive component of working memory relative to study in which presentation rate has been predetermined (i.e., experimenter-paced) (Dunlosky et al., 2011)—also do not inherently prevent/impair selectivity.

Conclusions

Distractions are often unavoidable and, despite a global awareness of consequent impairments (Barnes & Dougherty, 2007; Finley et al., 2014), learners frequently partake in distracting activities, leading to poorer comprehension of and memory for to-be-learned information (Fried, 2008; Sana et al., 2013). Experiments 1A/1B and 2A/2B examined whether distractions and divided attention during encoding similarly diminish selective attendance to valuable information when remembering everything is unachievable and whether the extent to which learners engage with the distraction during encoding and are responsible for self-regulating their study impacts selectivity.

In Experiment 1A, participants studied the to-be-remembered items while completing a digit-detection task or while listening to familiar/unfamiliar background music. Participants in the digit-detection condition remembered fewer items overall, but there were no significant differences in their prioritization of higher-valued items across conditions. These results were confirmed in an exact replication of Experiment 1A and upheld in Experiment 1B using a series of tone-detection tasks that varied in the demand placed on the central executive: Selectivity was consistently maintained regardless of the division of attention during study (Middlebrooks, Kerr, et al., 2017). Experiments 2A and 2B extended these findings, investigating the extent to which the learner's control over study pacing might mitigate the costs of divided attention to overall memory and, specifically, memory for the most valuable/important information. The results of these experiments suggest that learners are still just as selective when distracted/multi-tasking as those who study under full attention when able to self-regulate their study.

The agenda-based regulation model of self-regulated learning, however, is clear in its predictions that attentional loads and stressors to working memory—including internal or

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environmental distractors—can and will "undermine effective agenda use" (Dunlosky et al., 2011). Moreover, there is supportive evidence that lower working memory spans (whether inherent at an individual level or as a function of the task) are associated with less optimal or strategic self-regulated learning (Ariel et al. 2009; Dunlosky & Thiede, 2004; Middlebrooks & Castel, 2018; Robison & Unsworth, 2017; Thiede & Dunlosky, 1999). Nevertheless, participants across the current experiments adopted, maintained, and comparably executed their value-based agendas despite variations in stressors to their attention and central executive.

The magnitude of distractions/multi-tasking that learners can withstand without suffering a cost to their self-regulated learning thus remains to be defined. In understanding the boundary conditions of attentional divisions during self-regulated learning (in general and of important/valuable information, specifically), it will also be necessary to examine potentially moderating factors on attentional influences, such as the difficulty of the to-be-learned material; the learner's interest in the material as compared to their interest in the distraction or concurrent activity; and whether the onus has been placed on the learner to gauge the importance/value of the to-be-learned material.

If a learner underestimates the importance of information, or fails to identify it as such as a consequence of being distracted, then any self-regulated learning efforts would be largely doomed from the start. Stress to the central executive (whether owing to the to-be-remembered information or external factors) may exact its most consequential effects in the initial stages of evaluation and strategy conception rather than strategy execution. It is additionally possible that the consequences of stress to attention and the central executive owing to distractions or multi-tasking might depend on the nature of the to-be-learned information: namely, whether the information that a learner is studying is already taxing to working memory resources. In cases

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where demands on the central executive are already high owing to the nature of the to-be-remembered information itself, learners may be less likely/able to recognize the necessity of selective study when attention has been further commandeered by external distractions or task demands (Dunlosky et al., 2011; Dunlosky & Ariel, 2011a; Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999).

Interestingly, participants in Experiments 1B and 2B did not neglect the divided attention tasks as a function of the value of the item they were studying at the moment. While it confirms continued engagement and that maintained selectivity was not a consequence of having been less distracted when studying high-value information, it also suggests that participants did not take advantage of the opportunity to strategically task-switch by neglecting the secondary task in favor of studying valuable items. Participants may not have considered doing this (or may have considered doing so but not ultimately elected to) simply because they were aiming to abide by the task as it was assigned to them rather than taking liberties with the instructions. On the other hand, it might not have occurred to them as a strategy because of the demands of the task. Robison and Unsworth (2017) demonstrated that learners with lower working memory capacities were less likely to spontaneously adopt optimal encoding strategies. Perhaps the combination of study and distraction/multi-tasking was not detrimental to selectivity, but it was sufficiently taxing to working memory so as to prevent participants from recognizing ways in which their value-based agenda could be further optimized.

Relatedly—and of particular relevance when considering ways in which the current experiments extend to real-world situations of distraction and multi-tasking—participants may not have been sufficiently motivated to *look* for ways in which to further optimize their strategy. Although participants robustly engage in the selectivity task (Ariel et al., 2015; Ariel & Castel,

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2014; Castel, 2008; Castel et al., 2013; DeLozier & Rhodes, 2015; Hayes, Kelly, & Smith, 2013; Middlebrooks & Castel, 2018; Middlebrooks, McGillivray, et al., 2016; Middlebrooks, Murayama, et al., 2016, 2017; Siegel & Castel, 2018), there is ultimately no consequence to their not being perfectly selective in their study and recall. Yes, participants are more likely to recall high-value information than low-value, but they also forget some high-value items and remember low-value items. Participants would likely have improved their odds of recalling high-value information if they had neglected the concurrent tone tasks in Experiments 1B and 2B, but they did not. How representative of real-world situations is this? Would a student who is checking Facebook during a lecture not return to their lecture notes upon hearing an instructor announce that the upcoming information would almost certainly be on the test?

In other words, when encountering information that is clearly of greater importance than other information, are learners just as likely as in the current experiments to continue their engagement with concurrent distractors, or are they likely to pause the distractor and return their attention (if only momentarily) to the primary task? Certainly, there is performance-based evidence to suggest that they are *not* likely to pause the task (consider drivers who text despite ever-growing evidence that this impairs attention and driving competence), but how well does this carry over to situations of learning and memory, and is it more or less probable owing to the learner's interest in the distractor? These are questions that presently remain unanswered, but must be addressed in order to more comprehensively understand the consequences of divided attention to self-regulated learning.

At least within the current experiments, however, neither active multitasking nor relatively passive distractions prevented participants from strategically self-regulating their learning and prioritizing high-value items during study. Participants compensated for limitations

owing to divided attention by devoting their remaining resources to the most important items, providing further evidence that factors that worsen memory do not necessarily similarly affect study strategizing, despite (contrary to predictions made by the agenda-based regulation model; Dunlosky et al., 2011; Dunlosky & Ariel, 2011a) increasing demands to attention and working memory resources.

Table 4.1

Divided attention

Recall probability as a function of study condition and list in Experiments 1A, 1B, and 2A

	Condition	List 1	List 2	List 3	List 4	List 5	List 6	Average
Experiment 1A	Full attention	.34 (.14)	.38 (.13)	.40 (.15)	.40 (.13)	.41 (.14)	.40 (.13)	.39 (.10)
	Divided attention	.18 (.10)	.24 (.09)	.27 (.12)	.29 (.10)	.29 (.11)	.30 (.10)	.26 (.08)
	Familiar music	.33 (.14)	.35 (.10)	.36 (.17)	.38 (.18)	.38 (.18)	.34 (.16)	.35 (.11)
	Unfamiliar music	.31 (.14)	.37 (.17)	.38 (.13)	.37 (.15)	.37 (.15)	.38 (.18)	.37 (.11)
Experiment 1B	Full attention	.39 (.16)	.37 (.18)	.38 (.18)	.39 (.14)	.41 (.16)	.41 (.15)	.39 (.14)
	Tone monitoring	.19 (.10)	.24 (.10)	.30 (.16)	.27 (.12)	.25 (.10)	.26 (.09)	.25 (.07)
	Paired tones	.19 (.12)	.26 (.14)	.25 (.11)	.26 (.11)	.30 (.13)	.30 (.12)	.26 (.10)
	1-back	.14 (.06)	.17 (.07)	.28 (.15)	.24 (.09)	.23 (.08)	.25 (.09)	.21 (.05)
Experiment 2A	Full attention	.40 (.23)	.49 (.22)	.59 (.27)	.46 (.21)	.45 (.23)	.49 (.24)	.48 (.19)
	Divided attention	.31 (.20)	.48 (.20)	.45 (.23)	.47 (.20)	.40 (.18)	.43 (.20)	.42 (.17)
	Familiar music	.40 (.18)	.47 (.18)	.49 (.23)	.49 (.22)	.44 (.25)	.44 (.24)	.46 (.18)
	Unfamiliar music	.39 (.23)	.44 (.25)	.50 (.23)	.44 (.23)	.49 (.22)	.42 (.24)	.45 (.20)

Note. Standard deviations are presented in parentheses.

Table 4.2

Divided attention

Two-level hierarchical generalized linear model of recall in Experiments 1A and 2A and of self-pacing in Experiment 2A predicted by item value, list, and study condition

Fixed effects	Experiment 1A: Recall	Experiment 2A: Recall	Experiment 2A: Study time (sec)
Intercept (β_{00})	-0.52***	-0.05	6.08***
Predictors of intercept			
Cond1: Divided attention v. Full attention (β_{01})	-0.62***	-0.29	1.41
Cond2: Familiar music v. Full attention (β_{02})	-0.20$^+$	-0.17	-1.14
Cond3: Unfamiliar music v. Full attention (β_{03})	-0.07	-0.19	0.73
Value (β_{10})	0.16***	0.06*	0.17**
Predictors of value			
Cond1: DA v. FA (β_{11})	0.01	0.002	-0.03
Cond2: FM v. FA (β_{12})	0.02	0.10$^+$	0.17
Cond3: UM v. FA (β_{13})	-0.02	0.06	-0.02
List (β_{20})	0.04$^+$	0.04	-0.66
Predictors of list			
Cond1: DA v. FA (β_{21})	0.05$^+$	.002	-0.40
Cond2: FM v. FA (β_{22})	-0.03	-0.04	0.39
Cond3: UM v. FA (β_{23})	0.01	-0.02	0.45
List x Value (β_{30})	0.03**	0.01	0.003
Predictors of list x value			
Cond1: DA v. FA (β_{31})	0.01	0.01	0.02
Cond2: FM v. FA (β_{32})	-0.01	0.01	0.01
Cond3: UM v. FA (β_{33})	-0.01	0.004	-0.02
Random effects	Variance		
Intercept (person-level) (r_0)	0.21***	0.81***	23.83***
Value (r_1)	0.01***	0.03***	0.10***
List (r_2)	0.03***	0.02***	1.79***
List x Value (r_3)	0.001***	0.001***	0.004

Note. Recall performance was coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable when analyzing recall. Level 1 of the model in each experiment was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 was of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Cond1) $+ \beta_{02}$ (Cond2) $+ \beta_{03}$ (Cond3) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Cond1) $+ \beta_{12}$ (Cond2) $+ \beta_{13}$ (Cond3) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Cond1) $+ \beta_{22}$ (Cond2) $+ \beta_{23}$ (Cond3) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Cond1) $+ \beta_{32}$ (Cond2) $+ \beta_{33}$ (Cond3) $+ r_{3j}$.
$^+p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 4.3

Divided attention

Two-level hierarchical generalized linear model of recall performance predicted by item value, list, and study condition in Experiment 1B

Fixed effects	Coefficient
Intercept (β_{00})	-0.52***
Predictors of intercept	
Cond1: Tone monitoring v. Full attention (β_{01})	-0.72***
Cond2: Paired tones v. Full attention (β_{02})	-0.67**
Cond3: 1-back v. Full attention (β_{03})	-0.98***
Value (β_{10})	0.21***
Predictors of value	
Cond1: TM v. FA (β_{11})	-0.02
Cond2: PT v. FA (β_{12})	-0.05
Cond3: 1-back v. FA (β_{13})	-0.05
List (β_{20})	0.01
Predictors of list	
Cond1: TM v. FA (β_{21})	-0.01
Cond2: PT v. FA (β_{22})	0.06
Cond3: 1-back v. FA (β_{23})	0.09**
List x Value (β_{30})	0.03**
Predictors of list x value	
Cond1: TM v. FA (β_{31})	0.02
Cond2: PT v. FA (β_{32})	0.03
Cond3: 1-back v. FA (β_{33})	0.0003
Random effects	**Variance**
Intercept (person-level) (r_0)	0.22***
Value (r_1)	0.03***
List (r_2)	0.002
List x Value (r_3)	0.003***

Note. Recall performance was coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable. Level 1 of the model in each experiment was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) + π_{2j} (List) + π_{3j} (List x Value). Level 2 was of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Cond1) + β_{02} (Cond2) + β_{03} (Cond3) + $r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Cond1) + β_{12} (Cond2) + β_{13} (Cond3) + $r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Cond1) + β_{22} (Cond2) + β_{23} (Cond3) + $r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Cond1) + β_{32} (Cond2) + β_{33} (Cond3) + r_{3j}.
$^+p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 4.4

Divided attention

Two-level hierarchical generalized linear model of tone detection accuracy and reaction time as predicted by item value, list, and study condition in Experiment 1B

Fixed effects	Outcome: Tone detection accuracy	Outcome: Tone detection reaction time (sec)
Intercept (β_{00})	0.78***	0.80***
Predictors of intercept		
Cond1: Tone monitoring v. Paired tones (β_{01})	-0.01	0.21***
Cond2: Tone monitoring v. 1back (β_{02})	-0.10*	0.11**
Value (β_{10})	-0.004	0.002
Predictors of value		
Cond1: TM v. PT (β_{11})	0.002	-0.001
Cond2: TM v. 1back (β_{12})	-0.0005	-0.004
List (β_{20})	0.003	-0.02**
Predictors of list		
Cond1: TM v. PT (β_{21})	0.03	0.01
Cond2: TM v. 1back (β_{22})	0.02	-0.01
List x Value (β_{30})	-0.001	0.003**
Predictors of list x value		
Cond1: TM v. PT (β_{31})	0.001	< 0.0001
Cond2: TM v. 1back (β_{32})	-0.0003	-0.002
Random effects	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.03***	0.01***
Value (r_1)	< 0.0001	< 0.0001
List (r_2)	0.002***	0.0004***
List x Value (r_3)	< 0.0001	< 0.0001

Note. Level 1 models were of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 models were of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Cond1) $+ \beta_{02}$ (Cond2) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Cond1) $+ \beta_{12}$ (Cond2) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Cond1) $+ \beta_{22}$ (Cond2) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Cond1) $+ \beta_{32}$ (Cond2) $+ r_{3j}$.
"TM" refers to the Tone monitoring condition and "PT" to Paired tones.
*$p < .05$ **$p < .01$ ***$p < .001$

Table 4.5

Divided attention

Recall probability as a function of study condition and list in Experiment 2B

Attention	Pacing	List 1	List 2	List 3	List 4	List 5	List 6	Average
Full attention	Experimenter-paced	.29 (.13)	.31 (.10)	.30 (.08)	.30 (.10)	.32 (.07)	.31 (.10)	.30 (.06)
	Constrained-pacing	.36 (.17)	.39 (.19)	.42 (.21)	.40 (.17)	.38 (.20)	.39 (.17)	.39 (.16)
	Self-paced	.50 (.16)	.48 (.20)	.54 (.20)	.50 (.21)	.46 (.19)	.45 (.20)	.49 (.16)
	Average	.38 (.18)	.39 (.18)	.42 (.20)	.40 (.18)	.39 (.17)	.38 (.17)	.39 (.15)
Divided attention	Experimenter-paced	.15 (.11)	.20 (.15)	.27 (.17)	.28 (.13)	.29 (.15)	.29 (.14)	.24 (.13)
	Constrained-pacing	.24 (.10)	.26 (.11)	.26 (.10)	.27 (.10)	.32 (.12)	.34 (.10)	.28 (.07)
	Self-paced	.28 (.17)	.33 (.15)	.31 (.17)	.34 (.19)	.34 (.20)	.35 (.18)	.33 (.16)
	Average	.22 (.14)	.26 (.15)	.28 (.15)	.30 (.15)	.32 (.16)	.33 (.14)	.28 (.13)

Note. Standard deviations are presented in parentheses.

Table 4.6

Divided attention

Two-level hierarchical generalized linear model of recall performance predicted by item value, list, and study condition in Experiment 2B

Fixed effects	Coefficient
Intercept (β_{00})	-0.95***
Predictors of intercept	
Attention: Full attention (FA) v. Divided attention (DA) (β_{01})	-0.45***
Pacing1: Experimenter-paced (EP) v. Constrained-pacing (CP) (β_{02})	0.54**
Pacing2: Experimenter-paced (EP) v. Self-paced (SP) (β_{03})	0.89***
Attention x Pacing 1 (β_{04})	-0.17
Attention x Pacing 2 (β_{05})	-0.27
Value (β_{10})	0.19***
Predictors of value	
Attention: FA v. DA (β_{11})	-0.07
Pacing1: EP v. CP (β_{12})	-0.08^{+}
Pacing2: EP v. SP (β_{13})	-0.05
Attention x Pacing 1 (β_{14})	0.11
Attention x Pacing 2 (β_{15})	0.05
List (β_{20})	0.02
Predictors of list	
Attention: FA v. DA (β_{21})	0.12***
Pacing1: EP v. CP (β_{22})	-0.01
Pacing2: EP v. SP (β_{23})	-0.07*
Attention x Pacing 1 (β_{24})	-0.05
Attention x Pacing 2 (β_{25})	-0.04
List x Value (β_{30})	0.01
Predictors of list x value	
Attention: FA v. DA (β_{31})	0.02
Pacing1: EP v. CP (β_{32})	0.01
Pacing2: EP v. SP (β_{33})	-0.004
Attention x Pacing 1 (β_{34})	-0.02
Attention x Pacing 2 (β_{35})	-0.02
Random effects	**Variance**
Intercept (person-level) (r_0)	0.32***
Value (r_1)	0.003^{+}
List (r_2)	0.03***
List x Value (r_3)	0.001***

Note. Recall performance was coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable. Level 1 of the model in each experiment was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) + π_{2j} (List) + π_{3j} (List x Value). Level 2 was of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Attention) + β_{02} (Pacing1) + β_{03} (Pacing2) + β_{04} (Attention x Pacing1) + β_{05} (Attention x Pacing2) + r_{0j}, $\pi_{1j} = \beta_{10} + \beta_{11}$ (Attention) + β_{12} (Pacing1) + β_{13} (Pacing2) + β_{14} (Attention x Pacing1) + β_{15} (Attention x Pacing2) + r_{1j}, $\pi_{2j} = \beta_{20} + \beta_{21}$ (Attention) + β_{22} (Pacing1) + β_{23} (Pacing2) + β_{24} (Attention x Pacing1) + β_{25} (Attention x Pacing2) + r_{2j}, $\pi_{3j} = \beta_{30} + \beta_{31}$ (Attention) + β_{32} (Pacing1) + β_{33} (Pacing2) + β_{34} (Attention x Pacing1) + β_{35} (Attention x Pacing2) + r_{3j}.
$^{+}p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 4.7

Divided attention

Two-level hierarchical generalized linear model of self-paced study time (as a proportion of total study time) predicted by item value, list, and study condition in Experiment 2B

Fixed effects	Coefficient
Intercept (β_{00})	0.05***
Predictors of intercept	
Attention: Full attention (FA) v. Divided attention (DA) (β_{01})	0.0001
Pacing: Constrained-pacing (CP) v. Self-paced (SP) (β_{02})	-0.000003
Attention x Pacing (β_{03})	-0.0001
Value (β_{10})	0.002***
Predictors of value	
Attention: FA v. DA (β_{11})	-0.0004
Pacing: CP v. SP (β_{12})	0.0001
Attention x Pacing (β_{13})	-0.001
List (β_{20})	-0.000002
Predictors of list	
Attention: FA v. DA (β_{21})	-0.0001
Pacing: CP v. SP (β_{22})	0.000002
Attention x Pacing (β_{23})	0.0001
List x Value (β_{30})	0.0005+
Predictors of list x value	
Attention: FA v. DA (β_{31})	-0.0003
Pacing: CP v. SP (β_{32})	-0.0003
Attention x Pacing (β_{33})	0.0003
Random effects	**Variance**
Intercept (person-level) (r_0)	1.44×10^{-8}
Value (r_1)	1.00×10^{-8}
List (r_2)	3.65×10^{-6}***
List x Value (r_3)	5.78×10^{-7}***

Note. The study time outcome was the proportion of the total time spent studying a respective list on an individual item within that list for each individual participant. Level 1 of the model in each experiment was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 was of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Attention) $+ \beta_{02}$ (Pacing) $+ \beta_{03}$ (Attention x Pacing) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Attention) $+ \beta_{12}$ (Pacing) $+ \beta_{13}$ (Attention x Pacing) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Attention) $+ \beta_{22}$ (Pacing) $+ \beta_{23}$ (Attention x Pacing) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Attention) $+ \beta_{32}$ (Pacing) $+ \beta_{33}$ (Attention x Pacing) $+ r_{3j}$.
$^{+}p < .10$ $^{*}p < .05$ $^{**}p < .01$ $^{***}p < .001$

Table 4.8

Divided attention

Two-level hierarchical generalized linear model of self-paced study time (in seconds) predicted by item value, list, and study condition in Experiment 2B

Fixed effects	Constrained-Pacing	Self-Paced
Intercept (β_{00})	2.61***	6.30***
Predictors of intercept		
Attention: Full attention (FA) v. Divided attention (DA) (β_{01})	0.16$^+$	1.52
Value (β_{10})	0.13***	0.29***
Predictors of value		
Attention: FA v. DA (β_{11})	-0.02	-0.17
List (β_{20})	0.01	-0.52*
Predictors of list		
Attention: FA v. DA (β_{21})	-0.04	-0.41
List x Value (β_{30})	0.03$^+$	0.01
Predictors of list x value		
Attention: FA v. DA (β_{31})	-0.02	0.07
Random effects	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.01	31.26***
Value (r_1)	0.0001	1.05***
List (r_2)	0.01***	0.15***
List x Value (r_3)	0.002**	0.06***

Note. The study time outcome was the total time (in seconds) spent studying. Level 1 of the model in each experiment was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 was of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Attention) $+ r_{0j}, \pi_{1j} = \beta_{10} + \beta_{11}$ (Attention) $+ r_{1j}, \pi_{2j} = \beta_{20} + \beta_{21}$ (Attention) $+ r_{2j}, \pi_{3j} = \beta_{30} + \beta_{31}$ (Attention) $+ r_{3j}$.
$^+p < .10$ $*p < .05$ $**p < .01$ $***p < .001$

Table 4.9

Divided attention

Two-level hierarchical generalized linear model of 1-back tone detection accuracy predicted by item value and list in Experiment 2B

Fixed effects	Experimenter-Paced	Constrained-Pacing	Self-Paced
Intercept (β_{00})	0.62***	0.42***	0.47***
Value (β_{10})	-0.003	0.004	-0.002
List (β_{20})	0.01	-0.002	0.01$^+$
List x Value (β_{30})	-0.001	0.001	-0.002
Random effects	**Variance**	**Variance**	**Variance**
Intercept (person-level) (r_0)	0.02***	0.03***	0.05***
Value (r_1)	0.002***	0.001**	0.001***
List (r_2)	0.00004	0.00004	0.00004
List x Value (r_3)	4.16 x 10^{-6}	0.00002	0.00002*

Note. The study time outcome was the total time (in seconds) spent studying. Level 1 of the model in each experiment was of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 was of the form $\pi_{0j} = \beta_{00} + r_{0j}, \pi_{1j} = \beta_{10} + r_{1j}, \pi_{2j} = \beta_{20} + r_{2j}, \pi_{3j} = \beta_{30} + r_{3j}$.
$^+p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

CHAPTER FIVE:

RETRIEVAL and TESTING INFLUENCES on VALUE-DIRECTED REMEMBERING

      Whether information is successfully remembered at a later time is impacted by

innumerable factors, but one critical component is the method by which such retrieval is tested.

There are multiple ways in which to test one's memory—whether implicitly or explicitly—to

determine the extent to which information has been encoded. Within the domain of explicit

memory, testing is generally categorized as either recognition-based or recall-based. Although

there is debate as to the extent to which recall and recognition processes utilize similar

mechanisms (e.g., Anderson & Bower, 1972; Carey & Lockhart, 1973; Mandler, 1980; Rotello,

Macmillan, & Van Tassel, 2000; Wixted, 2007; Yonelinas & Parks, 2007), the act of recognizing

information seems to be qualitatively different than that of recalling it (Cabeza et al., 1997).

These differences can have a notable impact on which information is later retrieved. Further-

more, the differences between recall and recognition, and the beliefs people have about these

methods of retrieval, can also impact the encoding that takes place prior to any actual testing.

A number of studies indicate that participants who study with the expectation of an upcoming recall test outperform participants who study with the expectation of a recognition test on both recall and recognition tests (Balota & Neely, 1980; Hall, Grossman, & Elwood, 1976; Meyer, 1934, 1936; Neely & Balota, 1981; Schmidt, 1983; Thiede, 1996). In other words, studying with the expectation of an upcoming recall test can lead to better memory for the information and greater performance on the test, regardless of its actual format, than studying with the expectation of an upcoming recognition test.[34] These performance differences may partly stem from learners' beliefs about the demands of recognition-based test formats relative to recall-based formats. Indeed, learners generally expect recall tests to be more difficult than recognition tests (d'Ydewalle, Swerts, & De Corte, 1983; Hall et al., 1976; Murayama, 2005; Thiede, 1996) and expect to perform better on tests of recognition than recall (Speer & Flavell, 1979; Thiede, 1996). These expectations seem to be based not on experience with the specific tests in question, but rather with general experiences of recalling and recognizing information, as expectations of higher performance on recognition tests remain even when prior test performance suggests otherwise: Thiede (1996) reported that participants consistently provided higher judgments of learning (JOLs) when anticipating a more difficult recognition test than a less difficult recall test, despite having had prior experience with the tests and, thus, exposure to the difficulty.

These beliefs that learners hold about the differences in testing demands may encourage them to engage in entirely different encoding strategies as per the anticipated testing format. For

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[34] Not all studies of test expectancy have consistently demonstrated this "recall superiority." Classroom-based studies and studies using more realistic materials (e.g., a multiple-choice exam based on text passages vs. an old/new recognition test based on a list of unrelated words) have reported that expectancy-congruency between the anticipated test format and the received test is more important than test format itself (cf. Lundeberg & Fox, 1991). In laboratory contexts, however, recall superiority is the oft-reported finding. Additionally, both results (whether recall superiority or expectancy-congruent) are consistent with the notion that encoding itself is affected by expectations about the upcoming method of retrieval.

instance, there is some evidence to suggest that anticipation of a recall test leads learners to engage in more associative encoding than anticipation of a recognition test (Anderson & Bower, 1972, 1974; Balota & Neely, 1980; Staresina & Davachi, 2006; but see Neely & Balota, 1981). This is consistent with educational reports: when anticipating a test in which they must simply recognize the correct answer, as in a multiple-choice exam, students emphasize detail-based memorization and a more unit-based focus; when anticipating a test in which they must produce the answer (e.g., an essay or short-answer exam), however, they endorse more holistic, associative strategies (e.g., drawing conceptual connections across multiple chapters; Terry, 1933; but see Hakstian, 1971). Critically, such test-based differences in encoding seem to be purposeful and strategic. For example, participants who studied a series of cue-target word pairs in anticipation of a free recall test (during which they need only recall the target words) reported intentionally ignoring the cue words, whereas those anticipating a cued-recall test intentionally engaged in cue-target association strategies (Finley & Benjamin, 2012).

Altering one's encoding strategy to be more consistent with expectations of an upcoming test's demands should certainly not be considered ill advised or inappropriate. On the contrary, "learning to the test," so to speak, suggests active metacognitive judgments and thoughtful self-regulation of one's study. The mistake on the part of the learner, however, would be in assuming that one's evaluations of the test demands are accurate or, perhaps worse, that high test performance owing to format-based strategizing during encoding is necessarily indicative of strong learning of the material. Implementing strategies during encoding specifically designed to match the anticipated demands of a given testing format may lead to high performance, consistent with a transfer- appropriate processing view (e.g., Morris, Bransford, & Franks, 1977), but high test performance does not necessarily mean that the learner has actually learned the

material (Bjork, 1999; Funk & Dickson, 2011). The risk in assuming that performance reflects learning, or that one has accurately judged the demands of future retrieval situations, heightens when considering that to-be- remembered information in real-life situations often varies in terms of how important it is to remember. Oftentimes, it is not necessarily *how much* we remember, but *what* we remember that matters most, and any faulty expectations regarding testing demands, or misevaluations of true learning, could have more notable consequences if particularly important information ends up being forgotten.

When presented with enough to-be-remembered information that successfully recalling all of it is unlikely, research indicates that people can learn to be selective on the basis of value, attending specifically during study to the most important information at the expense of less important information (Castel, 2007; Castel et al., 2002; Castel et al., 2012; Middlebrooks, McGillivray, et al., 2016; Middlebrooks, Murayama, et al., 2016). If the task demands are such that participants can (or at least *expect* to) remember most/all of the information, as when anticipating a recognition test (Shepard, 1967; Standing, Conezio, & Haber, 1970), selectively focusing on the subset of most important information and neglecting the less important information during study would seem not only unnecessary, but also rather counterproductive— why attempt to remember only a fraction of the information if you expect to remember it all?

The (anticipated) demands of recall tests, relative to recognition, may lead learners who are expecting to later receive a test of recall to adopt a value-directed, selective study strategy, whereas those expecting to receive a recognition-based test may forego such selectivity— learners studying for a recognition test should be markedly less likely to remember the most important information when given a surprise recall test than learners who studied with the expectation of having to later recall the information. Experiment 1 was designed to examine

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whether the aforementioned patterns of "recall superiority" in the test expectancy literature (e.g., Balota & Neely, 1980; Thiede, 1996) also extend to the study of and memory for valuable information specifically.

Experiment 1

Experiment 1 investigates the possible impact of test expectancy on encoding strategies that learners use when confronted with information that varies in value and the effects these strategies have on subsequent test performance. Research investigating the methods and mechanisms by which people study and remember valuable information has used tests of recall and recognition (e.g., Castel et al., 2002; Hennessee, Castel, & Knowlton, 2017; Middlebrooks, McGillivray, et al., 2016), but no work to date has investigated the impact of testing expectations themselves on value-based learning.

An additional goal was to examine whether prior experience with one test format influences future value-based encoding and memory performance, even when the learner anticipates an alternative test format. Participants in Experiment 1 were explicitly told to expect either a recall test or a recognition test after each studied word list. After having studied four lists and receiving tests congruent with expectations, those participants who had been told to expect a recognition test instead received a surprise recall test following the fifth list. For subsequent lists, *all* participants were told to expect (and received) recall tests. Prior research indicates that test structure and content can guide strategy selection and use during future encoding (deWinstanley & Bjork, 2004; Garcia-Marques, Nunes, Marques, Carneiro, & Weinstein, 2015; Storm, Hickman, & Bjork, 2016), such that one learns what to study based on previous testing experience. In a similar vein, it may be the case that prior experience with a testing format that does not require a selective study strategy for optimum performance impacts subsequent strategy

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selection, despite changes in test format. It is presently unclear whether learners maintain prior format-based strategy use or whether they appropriately and successfully adapt to format changes.

Method

Participants

Participants consisted of 48 undergraduate students at the University of California, Los Angeles (38 female) ranging in age from 18 to 26 years ($M = 20.40$, $SD = 2.10$). Participants received partial credit for a course requirement.

Materials

Stimuli consisted of 8 lists containing 20 novel words apiece. Each of the words was randomly assigned a value ranging from 1 point to 10 points, with two words per list assigned to each value. The words in each list were randomly selected without replacement for each participant from a larger word bank of 280 random nouns (e.g., twig, button, point, brush) in order to avoid potential item effects (Murayama, Sakaki, et al., 2014). Words ranged from 4 to 7 letters and averaged to 8.81 ($SD = 1.57$, range = 5.48-12.65) on the log-transformed HAL frequency scale (Lund & Burgess, 1996).

Procedure

Participants were told that they would be shown a series of word lists, each containing 20 different words. They were further told that each word would be paired with a value ranging from 1 to 10 points, that there would be two words per point value within each list, and that the words would be presented on the screen one at a time for 3 seconds apiece. Participants were instructed to remember as many of the words in each list as they could while also endeavoring to

earn as many points as possible on a later test, with one's score a sum of the points associated with each correctly remembered word.

Participants were randomly assigned to one of two testing conditions: an All Recall (All Rc) control group and a Recognition-then-Recall (RgRc) group. Participants in the All Rc group were told that they would be asked to recall the words from each list at the end of its presentation, at which point they would then be told their score (out of 110 possible points) and the number of words that they had successfully recalled. Thus, each of the 8 lists was followed by a recall test for participants in the All Rc group. Participants in the RgRc group were explicitly told to expect a recognition test at the conclusion of each list. They were told that each of the 20 studied words would be presented on the screen simultaneously along with 20 new words[35] (the words were arranged randomly on the screen) and that they would need to select the 20 items that they remembered having studied in the just-presented list. Importantly, participants *had* to select 20 items as having been studied before they could progress to the next list. In so doing, the proportion of items correctly recognized can be directly compared to the proportion of items correctly recalled (of the 20 items selected as "old," how many were actually studied?) and false alarm rates are equivalent to miss rates. No words were reused across recognition tests (i.e., a word could not serve as a new word in multiple recognition tests and no studied words from prior lists were ever presented in later tests as new words). After selecting 20 items, participants were told their score (out of 110 possible points) and the number of words that they had successfully recognized.

Critically, participants in the RgRc group received recognition tests for Lists 1–4, but recall tests for Lists 5–8. The recall test following List 5 was unexpected—participants studied

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[35] These "new" words were selected from the same 280-word word bank as the studied words. For any given participant in the RgRc group, words from the word bank had as much chance of being used as a studied word as of being used as a new word during a recognition test.

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List 5 with the expectation of receiving a recognition test based on prior instruction and experience. After this surprise recall test, participants in the RgRc group were explicitly told to expect recall tests for the remainder of the task.

<div align="center">

Results

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Overall Memory Performance

Table 5.1 lists the proportion of items correctly recalled or recognized, as applicable, across the 8 lists. A 2(Condition: All Recall or Recognition-then-Recall) x 4(List) repeated-measures ANOVA on total recall in Lists 5-8 revealed a marginally significant List x Condition interaction, $F(3, 138) = 2.50$, $MSE = 0.01$, $\eta^2_G = .02$, $p = .06$. RgRc participants recalled significantly fewer items ($M = .30$, $SD = .19$) in List 5 than All Rc participants ($M = .42$, $SD = .16$), $t(46) = 2.38$, $d = 0.70$, $p = .02$. There were no significant condition differences in recall in Lists 6-8, $ps > .38$.

Although participants' recall when expecting a recall test was superior to those expecting a recognition test (i.e., List 5), recall in the RgRc condition did not significantly differ from that of the All Rc condition in subsequent lists (i.e., Lists 6-8) (when all participants expected to receive a recall test), despite having had prior experience with a different testing format and having been exposed to additional items serving as "new" words during the recognition test. Consistent with prior research (Balota & Neely, 1980; Thiede, 1996), however, participants recalled more items when they studied in expectation of a recall test than a recognition test, despite recognition performance in Lists 1-4 having approached ceiling (see Table 5.1).

Value-Directed Remembering and Selectivity

List 5: Unexpected recall test.

When studying List 5, participants in the RgRc condition expected to receive a recognition test as per pre-task instructions and prior experience in Lists 1–4. By presenting participants instead with a surprise recall test, it was possible to ascertain how (whether) participants attended to item value during study with the expectation of a recognition test format. Overall recognition performance to this point had been quite high, which could be interpreted as participants having had strong knowledge of the material. The question was whether participants had truly learned the high-value items or whether high recognition performance was actually masking low memory for the most important items. Participants may not have needed to be selective to perform well on the recognition tests (there is no cause to study selectively when one can perform well without having done so), but did they consider value at all when studying? To compare value-based recollection between the two conditions, item-level recall performance in List 5 (based on a Bernoulli distribution, with 0 *not recalled* and 1 *recalled*; level 1 items; level 2 participants) was first modeled as a function of each item's value. Value was entered into the model as a group-mean centered variable, such that Value was anchored on the mean value point (5.50). The model further included the two study conditions as a level-2 predictor of those level-1 effects, with the Condition variable anchored on the All Rc control group (i.e., 0 = *All Recall*, 1 = *Recognition-then-Recall*). Figure 5.1 depicts recall performance in List 5 as per item value and condition. Table 5.2 reports the tested model and its regression coefficients.

Figure 5.1. Recall probability in Experiment 1 as per condition and item value in List 5. Error bars represent standard error.

There was a significant, positive effect of Value in the All Rc condition (β_{10} = 0.31, $p <$ .001). In other words, participants in the All Rc condition were $e^{.31}$ = 1.37 times more likely to recall an item for one unit increases in its value. There was also a significant, negative interaction between Value and Condition (β_{11} = -0.35, $p <$ .001), indicating that the effect of value was significantly weaker in the RgRc condition than the All Rc condition. Simple slope analysis revealed that, contrary to the All Rc condition, Value did not significantly predict recall probability in the RgRc condition (β = -0.04, p = .35)[36]. Thus, despite achieving high performance on the recognition tests, participants studying with the anticipation of a recognition-based test format did not learn the high-value information to the extent that might have been expected from their overall performance and seemed to have studied the items without attending to their general importance. While this inattention to value may have been a sufficient strategy

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[36] The simple slope for the RgRc condition can be directly calculated by adding the β_{10} and β_{11} coefficients (i.e., 0.31+(-0.35) = -0.04). To determine whether this slope is significant, the Condition predictor in the model was recoded, such that 0 = *Recognition-then-Recall* and 1 = *All Recall* (Hayes, 2013). Note that this was also done to determine the significance of any reported simple slopes hereafter.

for recognition, it was insufficient for true learning of the most important information, as evidence by the differences between conditions.

Lists 6-8: Expected recall tests for everyone.

The model used to investigate value-directed remembering in Lists 6–8, during which all participants studied with the expectation of a recall test, was similar to that of the model for List 5, the only differences being the inclusion of a List predictor (anchored on List 7) and a Value x List interaction predictor to level-1 of the model. Figure 5.2 depicts recall performance averaged across Lists 6–8 as per item value and condition. Table 5.3 reports the tested model and its estimated regression coefficients.
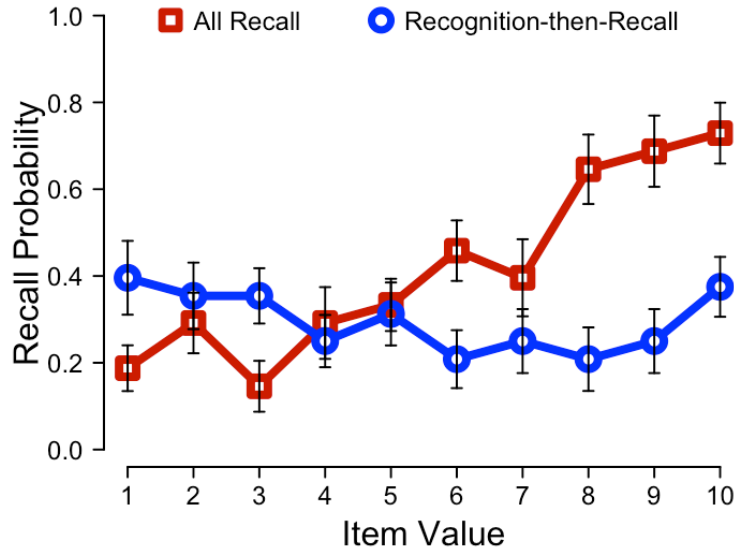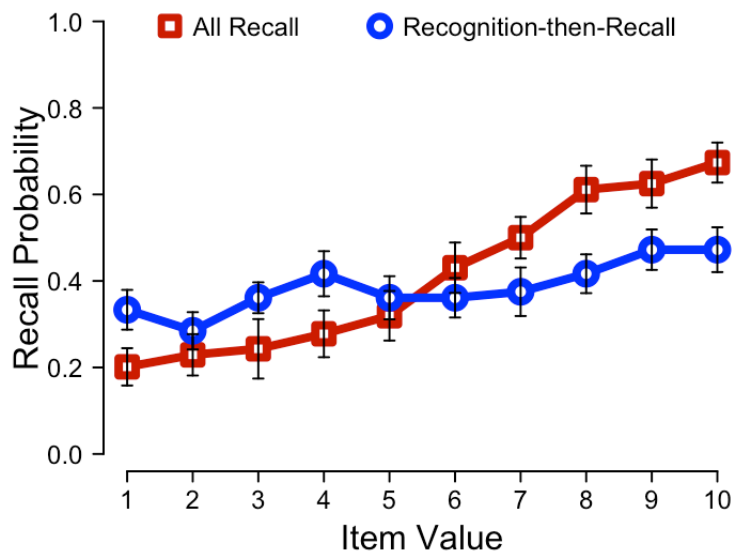

Figure 5.2. Recall probability in Experiment 1 as per condition and item value in Lists 6-8. Error bars represent standard error.

Value was a significantly positive predictor of recall performance in the All Rc condition ($\beta_{10} = 0.32$, $p < .001$). There was also a significant, negative interaction between Value and Condition ($\beta_{11} = -0.25$, $p < .001$), with the effect of value on recall probability being significantly weaker in the RgRc condition than in the All Rc condition. Simple slope analysis revealed that the effect of value in the RgRc condition, while significant and positive ($\beta = 0.08$, $p = .002$), was

lower than in the All Rc condition. Thus, participants in the All Rc condition were $e^{0.32} = 1.38$ times more likely to recall an item for each one-unit increase in its value while participants in the RgRc condition were only $e^{0.08} = 1.08$ times more likely. The odds of All Rc participants recalling a 10-point item, for instance, were thus $e^{0.32 \cdot 10} = 24.53$ times greater than the odds of recalling a 1-point item, but only 2.23 times greater for RgRc participants.

There was not significant a difference in recall owing to List ($p = .21$), nor was there a List x Condition interaction ($p = .91$), consistent with the previously conducted repeated-measures ANOVA. There was also not a List x Value interaction between in either the All Rc condition ($p = .11$) or the RgRc condition ($p = .19$); selectivity was constant across the final lists and condition differences in selectivity were maintained.

Initial recall experience.

Comparing value-directed remembering between conditions during Lists 6-8 reveals the differential effects that prior task experience had on participants' study strategies as a consequence of the manner in which their memory was to be tested. Namely, participants were less selective in their recall when their previous testing experience consisted of recognition tests than of recall tests. This would suggest that participants learned from testing and adjusted their strategies accordingly (Garcia-Marques et al., 2015; Jensen, McDaniel, Woodard, & Kummer, 2014; Storm et al., 2016). That RgRc participants were not as selective as the All Rc participants indicates that they did not learn to prioritize high-value items simply as a consequence of study design—such prioritization was unnecessary when their test performance was at ceiling—and thus did not have the same amount of practice executing a value-based strategy as those in the All Rc condition, for whom such a strategy was always important.

To determine whether the differences in selectivity between the two conditions was a consequence of the amount of practice in studying for a recall test (and thus the amount of practice utilizing a value-based study strategy), recall performance in Lists 6-8 by the RgRc condition was compared with recall performance in Lists 1–3 by the All Rc condition, depicted in Figure 5.3.
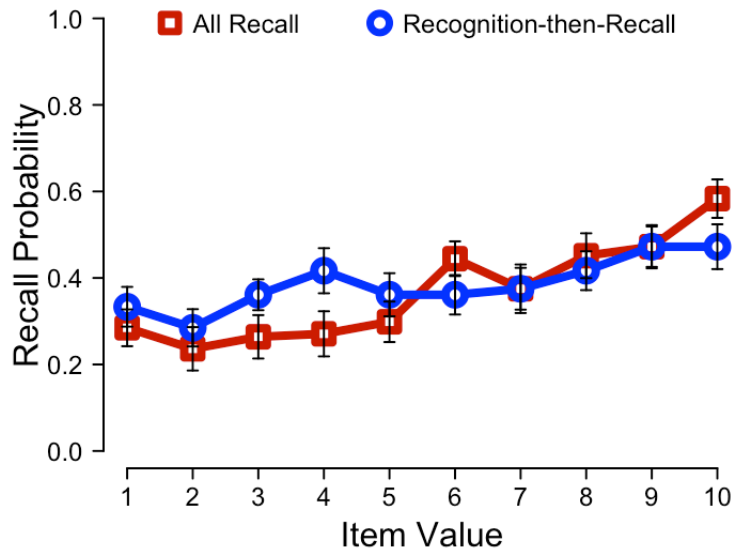
Figure 5.3. Recall probability in Experiment 1 as per condition and item value in the first three expected recall tests, averaged across lists. Error bars represent standard error.

In so doing, performance in the first three lists of *expected* recall tests could be directly assessed. The same HLM model was used as when testing value-directed remembering in Lists 6-8, save that the list on which the List predictor was anchored was the second list for each condition (i.e., List 2 for the All Rc condition and List 7 for the RgRc condition). Table 5.3 reports the tested model and its estimated regression coefficients.

Value was a significantly positive predictor of recall performance in the All Rc condition ($\beta_{10} = 0.17$, $p < .001$) during Lists 1-3. Once again, there was a significant cross-level interaction between Value and Condition ($\beta_{11} = -0.09$, $p = .04$), such that the effect of value in the RgRc condition in Lists 6-8, while positive ($\beta = 0.08$, $p = .02$), was also significantly less than that of

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the All Rc control in Lists 1-3. There were no other significant effects ($ps > .34$). Participants who had previously studied for recognition tests were thus less attentive to important items than participants with no prior testing experience, recall or otherwise.

Discussion

The results of Experiment 1 are threefold. First, participants who studied in anticipation of a recognition test were far less attentive to item importance during study than those who studied in anticipation of a recall test. With average performance at ceiling, this lack of attention to value was not apparent from the recognition test performance but became quite evident in the surprise recall test following List 5. So, although it may have appeared that RgRc participants had learned those most important items during study, it was actually the case that participants who anticipated a recall test better learned the most important items than those anticipating a recognition test despite recalling fewer items overall than their counterparts could recognize.

Second, participants in the RgRc condition failed to recover from the shift in test formats. After it was made clear to RgRc participants that, while List 5 was surprising, Lists 6-8 would also now be followed by recall tests instead of recognition tests, they continued to be significantly less attentive to item importance than those in the All Rc condition. Notably, there was no evidence of change in RgRc participants' attention to value across these three lists; their failure to attain selectivity comparable to that of All Rc participants was owing not only to a lack of practice—after all, the All Rc participants had studied for and experienced multiple recall tests by this point in the task— but a failure to adapt to the demands of the recall format relative to the recognition format.

Third, participants in the RgRc condition were *still* less selective than those in the All Rc condition after controlling for experience with recall testing. It would seem that having had

earlier experience with recognition tests actually served to impair their selectivity during study, that prior experience with a test not requiring prioritization of high-value information impaired their ability to do so when later required. Based on the results of Experiment 1, learning to be selective and strategic in the study of valuable information would seem to require not only experience with the material and general task (i.e., study 20 items varying in value and remember as many as possible while also maximizing one's score), but also experience with the testing format itself.

Experiment 2

In Experiment 1, prioritization of high-value information during study was evidently unnecessary for achieving high recognition test performance as participants were capable of correctly recognizing nearly all of the items without any special attendance to the most valuable. A selective study strategy, even in anticipation of a recognition test, however, may become a more sensible study choice were such high performance not so easily attained. Some research suggests that students modify their study based on the anticipated test demands (e.g., Dunlosky & Ariel, 2011a; Entwistle & Entwistle, 2003; Garcia-Marques et al., 2015; Winne & Hadwin, 1998) rather than the testing format, per se. For instance, when anticipating a test which will necessitate deep processing, learners adopt deeper processing study strategies; likewise, anticipation of a test necessitating shallower processing leads to shallow processing during study (Ross, Green, Salisbury-Glennon, & Tollefson, 2006). Thus, test-expectancy effects on memory may be driven less by the test format itself than by judgments of the task demands associated with particular formats. The effect of anticipating one type of recognition test on study and memory might be quite different than another recognition test with different task demands, even when the to-be-remembered material itself is identical.

The recognition tests in Experiment 2 were made more difficult in order to investigate whether the evident effect of testing expectations on value-based learning demonstrated in Experiment 1 was driven by participants' general beliefs about testing formats or beliefs about the upcoming test's specific demands and difficulty, irrespective of format. Should expectations regarding the general format of the recognition test be more critical to encoding behaviors/choices than the demands of the *specific* recognition test with which they have been tasked, then value should continue to be a largely irrelevant factor to participants' study, and differences in List 5 selectivity/value-based recall should remain. On the other hand, participants in Experiment 2 should be more likely to recall high-value items than low-value items on the surprise recall test if value becomes a more relevant factor during study in light of a demanding recognition test.

In an attempt to create a more demanding recognition test condition, similarity[37] was increased between the studied items and the unstudied lure items presented at test. Test items were also presented sequentially rather than simultaneously; simultaneous presentation can lead to more accurate recognition than sequential presentation because it is easier to make comparisons among items when all choices are visible at the same time (Finley, Roediger, Hughes, Wahlheim, & Jacoby, 2015; Steblay, Dysart, & Wells, 2011).

Forced-choice recognition formats in which participants can directly compare target and lure items, as in Experiment 1, may also rely more on mechanisms of familiarity than recollection (Holdstock et al., 2002; Kroll, Yonelinas, Dobbins, & Frederick, 2002), further increasing the retrieval demands at test. Correct recognition of previously studied valuable information, when driven by recollection, seems to be enhanced by its value (Cohen, Rissman,

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[37] Similarity was addressed in multiple ways, including semantic similarity (e.g., Elias & Perfetti, 1973; Underwood, 1965), pronunciation/acoustic similarity (e.g., Conrad, 1964; Kintsch & Buschke, 1969), and orthography (e.g., Logie, Del Sala, Wynn, & Baddeley, 2000).

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Castel, Hovhannisyan, & Knowlton, 2017). Item importance has no apparent effect on familiarity-based recognition when the high-value information was not intentionally prioritized during study. In other words, there are no demonstrable differences in the recognition of low- and high-value information when learners can rely on feelings of familiarity; when based on recollection, however, correctly recognized items tend to be the more valuable (Cohen et al., 2017). Participants may attend less to value during study when test demands are such that correct recognition can be easily achieved via familiarity, but more likely to adopt a value-based strategies in anticipation of recall testing, *despite* previous experience with a recognition-based test format, when recognition depended on explicit recollections.

Method

Participants

Forty-eight undergraduate students at the University of California, Los Angeles (28 female) ($M = 20.4$, $SD = 1.8$, range = 18-26 years) participated for partial credit for a course requirement.

Materials

The materials used in Experiment 2 were very similar to those used in Experiment 1: there were 8 lists of 20 novel words with each word randomly assigned a value of 1–10 points and two words per list assigned to each value. The words in each list were randomly selected without replacement from a larger word bank of 200 random nouns. Word length ranged from 4–7 letters and averaged to 8.64 ($SD = 1.58$) on the log- transformed HAL frequency scale, with a range from 5.53 to 12.53.

Each word was also paired with two lure words that were presented along with the corollary studied word during the recognition tests, as applicable. For instance, participants who

studied the word "shovel" also saw the words "shove" and "hovel" during the recognition test; participants who studied the word "rain" saw the words "reign" and "train" at test. Lures were created in a number of different ways: some were homophones of the studied target item; others were homonyms, semantically similar words, words with similar spellings, and so forth. Lures were not created with the intention of systematically investigating whether error patterns differed depending on the alteration (e.g., are participants more likely to incorrectly select a target's homonym than homophone?), but were simply intended to increase test difficulty, such that anything but careful attendance during study would leave a participant vulnerable to incorrectly selecting an item similar in sound, meaning, or appearance.

Procedure

The procedure used in Experiment 2 was identical to Experiment 1 save for the recognition tests. During the recognition tests, each of the 20 studied words was presented on the screen sequentially, rather than simultaneously, along with the 40 corollary new words (2 lures per studied word); participants were to select the 20 items that they remembered as having been just-presented during study. As in Experiment 1, participants were required to select 20 items as having been studied during the recognition test. Participants who had yet to select 20 items as having been studied before the test was finished were forced to select the n remaining items as "old." For example, if a participant had selected only 15 items as "old," and there were only 5 more items to be presented in the test, the option to indicate that an item was new was removed and participants were forced to select "old" for the 5 remaining items. Likewise, the option to indicate that items were "old" was removed in the event that a participant designated 20 items as "old" prior to the conclusion of the recognition test; participants were forced to select "new" for the remaining items. In addition to keeping hit rates comparable to recall accuracy, and false

alarm rates equivalent to miss rates, requiring participants to select "old" or "new" in this manner ensured that participants who completed the recognition tests were consistently exposed to 60 items, keeping potential interference from new items presented during testing constant across RgRc participants.

Completion of the recognition tests was self-paced, but participants could not change their old/new response to an item once they had progressed to the next item. At the conclusion of the test, participants were told their score (out of 110 possible points) and the number of words that they had correctly recognized.

Results

Overall Recall Performance

The proportion of items correctly recalled or recognized, as applicable, across the 8 lists are provided in Table 5.1. As in Experiment 1, a 2(Condition: All Recall or Recognition-then-Recall) x 4(List) repeated-measures ANOVA on total recall in Lists 5-8 revealed a significant Condition x List interaction, $F(3, 138) = 3.01$, $MSE = 0.01$, $\eta^2_G = .02$, $p = .03$. While there were no significant changes in recall across lists for those in the All Rc condition ($p = .66$), there was a significant List effect in the RgRc condition, $F(3, 69) = 3.64$, $MSE = 0.01$, $\eta^2_G = .14$, $p = .02$, such that recall significantly increased from List 5 to List 8, $ps < .033$. Moreover, participants in the All Rc condition recalled significantly more items than the RgRc condition in Lists 5 and 6 ($ds = 0.75$ and 0.73, respectively; $ps < .018$), but there were no significant condition differences in recall for Lists 7 and 8 ($ds = 0.09$ and 0.25, respectively; $ps > .39$).

These results indicate that studying with the expectation of a recall test once again led to better memory for the to-be-remembered items than studying with the expectation of a recognition test, as demonstrated by the condition differences in List 5 recall. That the

differences remained for List 6 recall may reflect effects of interference from the lengthier

recognition tests relative to Experiment 1. Regardless, condition differences in overall recall

attenuated by List 7 (i.e., the RgRc group's third recall test), suggesting that any potential effect

of interference was minimal or, at least, surmountable. RgRc participants' recall did not

significantly differ from that of participants in the All Rc condition when expecting to receive a

recall test in the final lists.

Value-Directed Remembering & Selectivity

HLM analyses of Experiment 2 data are identical to those of Experiment 1.

List 5: unexpected recall test.

Figure 5.4 depicts recall performance in List 5 as per item value and condition. Table 5.2

reports the tested model and its estimated regression coefficients.
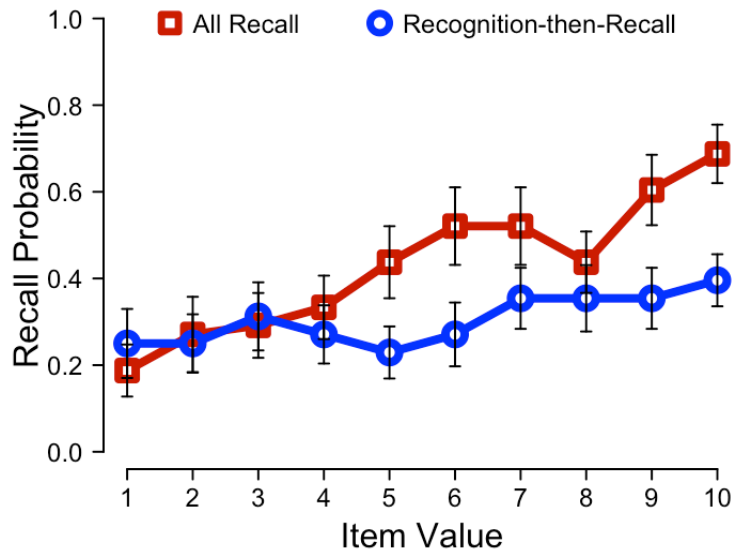
Figure 5.4. Recall probability in Experiment 2 as a function of condition and item value in List 5. Error bars
represent standard error.

As in Experiment 1, there was a significant, positive effect of Value in the All Rc condition (β_{10}

$= 0.23, p < .001$) and a significant cross-level interaction between Value and Condition ($\beta_{11} = -$

$0.14, p = .03$)—participants were, once again, less selective in their study when anticipating a

test of recognition than participants expecting a recall test. Unlike in Experiment 1, however, the effect of value in the RgRc condition was significantly positive ($\beta = 0.09$, $p = .02$). These results indicate that RgRc participants expecting to receive a recognition-based test in Experiment 2 *did* consider item importance during study, albeit notably less so than participants expecting a recall test in the first place.

Lists 6-8: expected recall tests for everyone.

Figure 5.5 depicts recall performance averaged across Lists 6-8 as per item value and condition. Table 5.3 reports the tested model and its estimated regression coefficients.
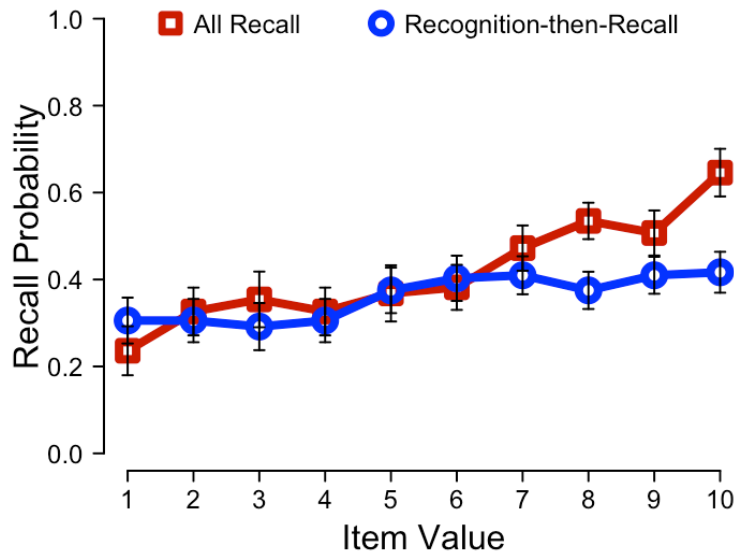
Figure 5.5. Recall probability in Experiment 2 as per condition and item value in Lists 6-8. Error bars represent standard error.

As in Experiment 1, Value was a significantly positive predictor of recall probability in the All Rc condition ($\beta_{10} = 0.19$, $p < .001$). There was also a significant cross-level interaction between Value and Condition, ($\beta_{11} = -0.11$, $p = .049$), such that the positive effect of value in the RgRc condition ($\beta = 0.08$, $p = .008$) was significantly less than in the All Rc condition. So, while both groups were attentive to value in the final lists of the task, participants in the All Rc condition were more selective than those in the RgRc.

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There were no significant differences in recall owing to List in the All Rc condition (p = .49), nor was there a List x Condition interaction (p = .16) for Lists 6-8. As in Experiment 1, there was also not a significant List x Value interaction in the All Rc condition (p = .37), nor a three-way interaction between List, Value, and Condition (p = .31), indicating that condition differences were maintained across these final lists.

Initial recall experience.

As in Experiment 1, performance in the first three lists of *expected* recall was directly compared between conditions (i.e., Lists 1-3 in the All Rc group versus Lists 6-8 in the RgRc group). Figure 5.6 depicts recall performance averaged across the first three anticipated recall tests as per item value and condition.
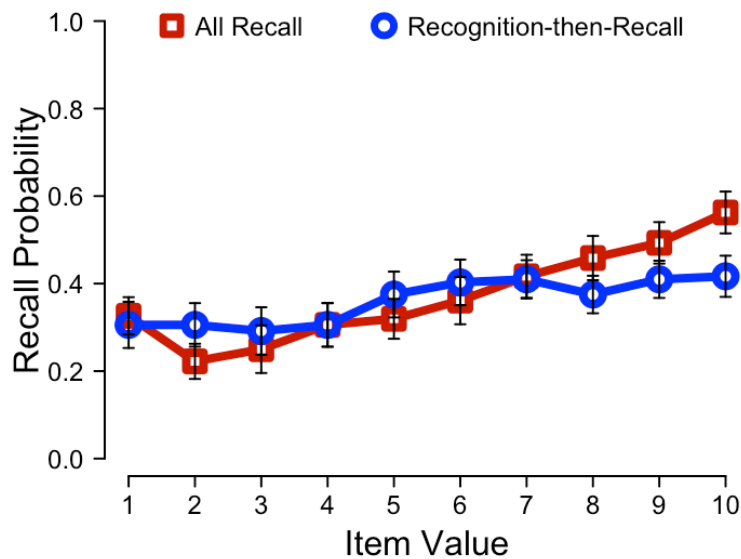
Figure 5.6. Recall probability in Experiment 2 as per condition and item value in the first three expected recall tests, averaged across lists. Error bars represent standard error.

Table 5.3 reports the tested model and its estimated regression coefficients. As in Experiment 1, Value positively predicted recall performance in the All Rc condition (β_{10} = 0.14, p < .001) during Lists 1-3. Contrary to Experiment 1, however, there was *not* a significant difference in the effect of value on recall between conditions (p = .14), indicating comparable selectivity during

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the first three lists of anticipated recall testing. There was a significant List x Value interaction in the All Rc condition ($\beta_{30} = 0.06$, $p = .045$), such that selectivity improved across these first lists, with no significant differences between conditions ($p = .35$).

Discussion

Recognition testing demands were increased in Experiment 2 relative to Experiment 1— instead of selecting the 20 studied items from a list of 40 items presented on the screen simultaneously, RgRc participants in Experiment 2 were to select the 20 studied items from a sequentially presented list of 60 items. Additionally, each studied item had two corollary lures in the recognition test that were designed to be confusable with the studied item owing to similar pronunciations (e.g., racquet vs. racket), spelling (e.g., stump vs. stomp), meaning (e.g., bandage vs. bandaid), and so forth.

Consistent with Experiment 1, participants expecting to receive a recognition test but who, in fact, received a recall test (List 5) were significantly less attentive to value during study than All Rc participants expecting to receive the recall test in the first place, despite having previously demonstrated strong recognition of the studied items, in general. Notably, however, item value had a positive effect on List 5 recall probability for RgRc participants in Experiment 2, whereas there was no such value effect on the surprise test for Experiment 1 RgRc participants. So, it appears that the changes made to the recognition test in Experiment 2 were such that item value was now considered, or at least salient, during study, although to a lesser extent than in anticipation of a recall test.

Participants in the All Rc condition continued to study more selectively than those in the RgRc condition in Lists 6-8, during which both groups were told to expect (and received) free recall tests. These condition differences were negated, however, when experience with the recall

test format itself was taken into account, directly contrasting the results of Experiment 1. So, contrary with Experiment 1 results, experience with the general task and study materials encouraged attendance to item value, though the extent of such selectivity depended upon the anticipated test format. Moreover, prior recognition testing did not hinder the appropriate adoption of a value-based study strategy in Experiment 2 as it did in Experiment 1.

That RgRc participants in Experiment 2 adapted to the change in test format from recognition to recall, and came to adopt value- based study strategies in anticipation of recall testing, suggests that the adjustments made to the recognition test in Experiment 2 relative to that which was administered during Experiment 1 encouraged changes in study and retrieval that enabled participants to more easily adapt to new formatting demands.

General Discussion

When attempting to remember information, one of many contributing factors to successful memory at a later time is the method by which one expects the memories to be tested (e.g., Balota & Neely, 1980; Finley & Benjamin, 2012; Lundeberg & Fox, 1991; Meyer, 1934, 1936; Murayama, 2006). Knowledge of the upcoming test format can affect both evaluations of encoding success (has this information been sufficiently learned?; Thiede, 1996) and the particular behaviors/strategies in which learners engage during study (*how* should this information be learned?) (Finley & Benjamin, 2012; Garcia-Marques et al., 2015; Terry, 1933, 1934). The current experiments examined how expectations regarding the upcoming test format can affect one's study of information varying in value or importance—are learners similarly likely to learn and remember important information when anticipating a recall test as when anticipating a test of recognition? In light of differences in value-based study between recall and recognition test formats in Experiment 1, Experiment 2 aimed to clarify how encoding during

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study may differ owing to the test's specific demands, rather than the general format, and the extent to which the likelihood of adopting and successfully executing strategies appropriate for one test format is influenced by prior experience with an alternate format.

In both experiments, participants who studied in anticipation of a recognition test were far less likely to remember the most important items in a surprise recall test than those participants who studied with the expectation of a recall test, consistent with re- search indicating better memory overall when studying with the expectation of a recall-based than recognition-based test (e.g., Balota & Neely, 1980; Thiede, 1996). Notably, this pattern of "recall superiority" was evident despite the fact that prior recognition test performance was quite high and would have otherwise suggested sufficient knowledge of the important information. Thus, it would seem that high recognition performance in the current experiments largely masked poorer learning of the valuable material relative to participants expecting to receive a recall test. Had only recognition tests been administered, it would have appeared as though the most important information had been effectively learned when, in fact, memory was significantly inferior to that of participants studying for a recall test (Funk & Dickson, 2011).

Additionally, participants in both experiments who had previously received recognition tests, but were told to now expect recall tests for the remainder of the task, were significantly less selective in their study of the valuable information than participants who had only ever studied for and received recall tests, indicating the importance of experience with not only the material and general task structure, but also the method of testing itself. The way in which participants chose to study was not purely dependent upon the materials themselves, but rather on how they would later be asked to retrieve the information—participants learned what and how to learn

based on the test format (cf., Finley & Benjamin, 2012; Garcia-Marques et al., 2015; Jensen et al., 2014).

It is possible that RgRc participants failed to adjust their selectivity in light of the recall tests because they doubted the veracity of the experimenter's instruction to expect recall tests for the remainder of the task, given that prior expectations of recognition testing were a consequence of similarly explicit instruction provided at the start of the task that had been violated in List 5. Participants may have been hesitant to accommodate this change in instruction, believing it possible that the format would unexpectedly switch back to recognition again (or to an entirely novel format). There is, however, reason to suspect that a lack of trust in experimenter instruction does not explain the present results. First, participants could clearly recognize more items than they could recall. Had participants seriously entertained the possibility that a recognition test might, at some point, be administered during Lists 6-8, despite instructions to the contrary, they still should have prepared for recall—being able to recognize an item does not guarantee accurate recall (which would have become evident during the List 5 recall test), but one can surely recognize an item which can also be recalled.

A perhaps more convincing argument against distrust motivating selectivity differences, though, is based on the differences seen between Experiment 1 and 2 with respect to RgRc participants' adaptation to the recall tests. Experiment 1 participants with prior recognition testing experience were significantly less selective than participants with experience only of recall testing; despite having studied six lists of item-value pairings, RgRc participants were still less attentive to important information and less able to remember it than All Rc participants with entirely no task experience. This was not the case in Experiment 2. Although less selective in Lists 6-8 than All Rc participants, Experiment 2 participants were not less selective once

accounting for prior experience with recall testing specifically. If a lack of trust motivated the persistent differences in selectivity between the RgRc and All Rc participants in Experiment 1, it should have similarly done so in Experiment 2. There is no reason to believe that Experiment 1 participants were so much less trusting than participants in Experiment 2, to the point that they failed to adapt to the recall test format.

The differential impact of the recognition test format relative to the recall test format on selectivity between Experiments 1 and 2 is, however, consistent with the notion that testing expectation effects on strategy adoption and encoding behavior are less a consequence of the expectations learners have about what recognition- and recall-based formats *generally* entail, but their expectations regarding the inherent demands of the specific test which they are to receive. Learners seem to hold broad beliefs about the demands and relative difficulty levels of recognition- based and recall-based tests (Terry, 1933), but the results of the current experiments suggest that they can modify their study based on continued experience with the specific demands of the test.

In the absence of any other indicators regarding the demands of an upcoming recognition test, learners may generally believe that a feeling of familiarity at test will be an efficacious determinant of correct recognition—along the lines of "I'll know it when I see it" (Terry, 1933). Widely endorsed dual-process models, however, clearly outline both recollection and familiarity components of recognition memory (cf. Yonelinas, 2002). Whereas familiarity alone might have been sufficient for correctly recognizing "plane" and rejecting "drizzle" in Experiment 1, it was likely insufficient for the correct recognition of "plane" and correct rejection of "plain" in Experiment 2 (Gallo, 2004; Holdstock et al., 2002; Schmid, Herholz, Brandt, & Buchner, 2010). This is not to say that recognition in Experiment 1 would never have been based on recollection,

or that feelings of familiarity never contributed to recognition in Experiment 2. After all, had RgRc participants in Experiment 2 relied purely on recall mechanisms while completing the recognition tests, their overall recall performance on the unexpected List 5 recall test would likely not have been significantly lower than that of All Rc participants. The demands of the recognition test in Experiment 2 were, nonetheless, such that correct recognition very likely depended more heavily on explicit recollection than in Experiment 1 (Holdstock et al., 2002; Kroll et al., 2002).

The demonstrated attention to value on the (surprise) List 5 recall test by RgRc participants in Experiment 2, but not Experiment 1, is further consistent with recent work indicating that recognition driven by explicit recollection is more likely to be value-based than recognition driven by feelings familiarity, even in the absence of intentional strategizing (Cohen, Rissman, Hovhannisyan, et al., 2017). Extending this finding, the absence of a value effect in Experiment 1 by RgRc participants indicates not only a lack of value-based study strategizing when anticipating recognition testing, but also implies that prior recognition performance in Lists 1– 4 was not (primarily) driven by recollection—recognition based on explicit recollections of the studied items would have been enhanced by the value of the items themselves and, thus, resulted in some degree of a value effect on the surprise recall. The List 5 value effect exhibited by RgRc participants in Experiment 2, however, is consistent with the supposition that recognition was more greatly aided by recollection than in Experiment 1. Importantly, RgRc participants were only twice as likely to recall a 10-point word as a 1-point word, whereas All Rc participants were approximately 25 times as likely. So although this effect of value on recall was significant for RgRc participants, the small magnitude suggests more automatic value effects on

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recollection-based recognition than value- based strategizing during encoding (Cohen et al., 2017).

The differences between RgRc performance relative to All Rc performance in Experiments 1 and 2 cannot, however, be solely explained by the possible differences in the dominant mechanism (whether familiarity or recollection) underlying their recognition performance. This may account for the differences in List 5 recall, but it does not completely elucidate why differences between the RgRc and All Rc conditions perpetuated, even after accounting for recall experience, in Experiment 1 but not Experiment 2. It may be that the overall design of the recognition test in Experiment 2 made participants more keenly aware of ways in which they could potentially misremember items or confuse the studied and unstudied items at test. This knowledge may have led RgRc participants in Experiment 2 to engage in deeper or more elaborative encoding strategies (Craik & Lockhart, 1972; Craik & Tulving, 1975) to emphasize defining characteristics of the items and later aid in differentiating the studied from unstudied (which may also have made the items more distinctive as a consequence; Gallo, Meadow, Johnson, & Foster, 2008).[38]

Recent work suggests that the study of high-value information relative to low-value information, in anticipation of a recall test, is associated with greater activity in regions of semantic processing (Cohen et al., 2014; Cohen, Rissman, Hovhannisyan, et al., 2017). If RgRc participants' recognition in Experiment 2 was based more on recollection than familiarity judgments, this could have made the transition to a purely recall-based test format less jarring, in

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[38] There have been numerous studies to suggest that such deep encoding can actually encourage false recall and recognition of critical lures (e.g., Dodd & MacLeod, 2004; Rhodes & Anastasi, 2000; Thapar & McDermott, 2001; Toglia, Neuschatz, & Goodwin, 1999). Importantly, these studies have predominantly relied on the DRM paradigm, in which there is a single critical lure (e.g., "sleep") associated with a given set of studied items ("bed," "rest", "dream," etc.) (Deese, 1959; Roediger & McDermott, 1995), which differs from the current set of experiments. In Experiment 1, participants were required to distinguish studied items from unrelated items; in Experiment 2, studied items were associated with two lures during test, but the lures were related only to a single studied item and were not lures as a consequence of the studied list in its entirety, or even a substantial subset of the studied list.

that participants could have adapted encoding strategies already being used in anticipation of recognition testing for the recall tests. Although RgRc participants in Experiment 2 may not have intentionally encoded high-value items specifically to a deeper extent than low-value items when expecting recognition tests (Cohen, Rissman, Hovhannisyan, et al., 2017), it would have been conceivably easier to incorporate additional aspects of the material, like value, in their study if they were already utilizing deeper encoding strategies/processing. RgRc participants in Experiment 1 may not have studied in a manner that could be optimally adapted to recall-based testing, hence the struggle to study with the goal of being able to produce the items at test *and* remember the most valuable ones.

There is good reason to suspect that the adjustments made to the recognition test in Experiment 2 resulted in differences with respect to how recognition was realized, whether via recollection or familiarity (Holdstock et al., 2002; Kroll et al., 2002), but it cannot be confirmed based on the design of the current experiments. Future research could test this notion using more direct tests of recollection and familiarity (e.g., remember/know judgments; Tulving, 1985). Robust selectivity in spite of prior recognition testing experience, as in Experiment 2, would indicate that recognition tests necessitating recollection or explicit retrieval could be less damaging to the future strategizing when full retrieval, as with a free recall test, is necessary.

Future research should also investigate the extent to which general test difficulty contributed to condition differences. Although the recognition test in Experiment 2 was designed to be more demanding than that of Experiment 1, the recall test format was arguably the most demanding. Rather than the selectivity differences being driven by the extent to which explicit recollection was required at test, they may instead have been driven by the more general differences in task demand. Including a modified All Rc condition in which Lists 1-4 are

followed by an easy recall test relative to Lists 5-8 might help to qualify the impact that the retrieval mechanisms and broader cognitive demands of the task had on selectivity and value-based study. A failure to adapt to more challenging recall tests after experiencing easy recall tests would suggest that the differences in the current experiments arose from general demands, rather than format-based demands or characteristics. Alternatively, swift adaptation from an easy to difficult recall tests would highlight not only the importance of common retrieval mechanisms in adapting to changing test demands, but also the importance of anticipating recollection-based testing formats when studying valuable information.

The decision to engage in a selective, value-based strategy, to prioritize high-value items over less valuable items when all cannot be remembered, reflects an active monitoring of one's limitations and the effectiveness of alternative study attempts or strategies. As such, it may be that asking learners who are anticipating tests which are less likely to stimulate selective study, such as the recognition tests in the current experiments, to make predictions regarding how well they will remember the important information, or to provide JOLs during study, would help to overcome any detrimental effects that testing expectations might have on the consideration of item importance. Doing so could make the effectiveness (or lack thereof) of any non-value-based study strategies more salient to learners, particularly with continued task experience (Hertzog, Price, & Dunlosky, 2008), thus encouraging value-based study in spite of test format.

Given the current findings, future research should also investigate whether expectations of either recognition-based or recall-based testing alter learners' attention to pedagogical importance of more realistic study materials (e.g., text passages) and testing formats, such as multiple-choice or essay exams (Lundeberg & Fox, 1991; McDaniel, Blischak, & Challis, 1994; Murayama, 2003). Students may be less likely to consider material importance when preparing

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for tests of recognition, as suggested by the current experiments, but more likely to do so when preparing for open-response, recall-based exams (Rickards & Friedman, 1978). In addition, consideration should also be given to whether testing expectations influence how learners allocate study time as a function of item importance (cf., Ariel et al., 2009; Middlebrooks, Murayama, et al., 2016). Although it cannot be said that the influence recognition tests had in the current experiments will be the same for multiple-choice tests or other recognition-based formats, the results do suggest that knowledge of the upcoming format and its design, as well as prior experience with alternate formats, can impact the encoding of important information.

Table 5.1

Test expectancy

Recall and recognition probability as a function of list and study condition

	Condition	Expectancy-Congruent Testing					Recall Testing				
		L1	L2	L3	L4	Average	L5	L6	L7	L8	Average
Experiment 1	All Recall (All Rc)	.38 (.12)	.36 (.14)	.36 (.11)	.39 (.15)	.37 (.10)	.42 (.16)	.43 (.15)	.40 (.13)	.41 (.20)	.42 (.14)
	Recognition-then-Recall (RgRc)	.86 (.12)	.86 (.09)	.85 (.12)	.86 (.11)	.86 (.08)	.30 (.19)	.39 (.14)	.40 (.16)	.36 (.20)	.36 (.13)
	Average						.36 (.18)	.41 (.14)	.40 (.15)	.39 (.20)	.39 (.13)
Experiment 2	All Recall (All Rc)	.34 (.15)	.40 (.20)	.36 (.14)	.41 (.15)	.38 (.11)	.43 (.15)	.44 (.17)	.40 (.19)	.41 (.17)	.42 (.14)
	Recognition-then-Recall (RgRc)	.70 (.10)	.76 (.12)	.80 (.12)	.78 (.15)	.76 (.11)	.30 (.19)	.33 (.14)	.38 (.20)	.37 (.17)	.35 (.15)
	Average						.37 (.18)	.38 (.16)	.39 (.19)	.39 (.17)	.38 (.15)

Note. "L1" through "L8" refers to Lists 1 through 8. Values for Lists 1-4 reflect recall performance for participants in the All Recall group and recognition performance for participants in the Recognition-then-Recall group. Values for Lists 5-8 reflect recall performance for all participants. Standard deviations are presented in parentheses.

Table 5.2

Test expectancy

Two-level hierarchical generalized linear model of recall performance in List 5 predicted by item value and study condition

Fixed effects	Experiment 1	Experiment 2
Intercept (β_{00})	-0.43*	-0.34*
Predictors of intercept		
Condition (β_{01})	-0.52*	-0.57*
Value (β_{10})	0.31***	0.23***
Predictors of value		
Condition (β_{11})	-0.35***	-0.14*
Random effects		
Intercept (r_0)	0.55***	0.45***
Value (r_1)	0.01^{+}	0.02**

Note. The dependent variable is recall performance coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable. Level 1 models were of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value). Level 2 models were of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Condition) $+ r_{0j}$, $\pi_{1j} = \beta_{10} + \beta_{11}$ (Condition) $+ r_{1j}$. Condition was coded as 0 = *All Recall* and 1 = *Recognition-then-Recall*.
$^{+}p < .10$ $^*p < .05$ $^{**}p < .01$ $^{***}p < .001$

Table 5.3

Test expectancy

Two-level hierarchical generalized linear model of recall performance predicted by item value, list, and study condition

Fixed effects	Experiment 1: Lists 6-8	Experiment 1: Expected Recall Tests 1-3	Experiment 2: Lists 6-8	Experiment 2: Expected Recall Tests 1-3
Intercept (β_{00})	-0.53**	-0.60***	-0.43*	-0.61***
Predictors of intercept				
Condition (β_{01})	-0.02	0.10	-0.22	-0.03
Value (β_{10})	0.32***	0.17***	0.19***	0.14***
Predictors of value				
Condition (β_{11})	-0.25***	-0.09*	-0.11*	-0.07
List (β_{20})	-0.10	-0.07	-0.05	0.01
Predictors of list				
Condition (β_{21})	0.01	-0.01	0.14	0.06
List x Value (β_{30})	0.05	0.02	-0.02	0.06*
Predictors of list x value				
Condition (β_{31})	-0.05	-0.02	0.03	-0.04
Random effects	**Variance**	**Variance**	**Variance**	**Variance**
Intercept (r_0)	0.41***	0.18***	0.48***	0.33***
Value (r_1)	0.10**	0.08**	0.01	0.01
List (r_2)	0.03***	0.01***	0.03***	0.01***
List x Value (r_3)	0.01^{+}	0.004^{+}	0.002	0.004

Note. The dependent variable is recall performance coded as 0 (*not recalled*) or 1 (*recalled*). Logit link function was used to address the binary dependent variable. Level 1 models were of the form $\eta_{ij} = \pi_{0j} + \pi_{1j}$ (Value) $+ \pi_{2j}$ (List) $+ \pi_{3j}$ (List x Value). Level 2 models were of the form $\pi_{0j} = \beta_{00} + \beta_{01}$ (Condition) $+ r_{0j}$, $\pi_{1j} = \beta_{10} + \beta_{11}$ (Condition) $+ r_{1j}$, $\pi_{2j} = \beta_{20} + \beta_{21}$ (Condition) $+ r_{2j}$, $\pi_{3j} = \beta_{30} + \beta_{31}$ (Condition) $+ r_{3j}$. Condition was coded as 0 = *All Recall* and 1 = *Recognition-then-Recall.* Note that "Expected Recall Tests 1-3" refers to the first three lists following which participants were told to expect recall tests: Lists 1-3 for participants in the All Recall condition and Lists 6-8 for participants in the Recognition-Recall condition.
$^{+}p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

CHAPTER SIX:

GENERAL CONCLUSIONS and FUTURE DIRECTIONS

Despite one's best efforts, forgetting is as much a part of memory as is remembering. Undoubtedly, remembering *everything* one encounters would be, at best, tedious—and there are certainly events one would sooner forget than not—but the costs of forgetting can also at times be quite dear. Forgetting sometimes result from having failed to devote sufficient attention to the information during encoding, but not everything *can* be realistically and sufficiently attended to when the amount of information ideally remembered exceeds one's encoding capacity. Given that remembering everything one might wish to remember is simply not feasible, strategic prioritization of the most important information during encoding, even at the expense of forgetting the less important, is critical to avoiding consequential outcomes.

Prior research indicates that learners can compensate for an inability to remember the entirety of a to-be-learned set of information (Castel, 2008; Castel et al., 2012), whether as a consequence of an excessive quantity of information or personal limitations to the learner's encoding, such as those which are associated with advanced age (Castel et al., 2012, 2013; Castel, Humphreys, et al., 2011) or—to an extent—inherent attentional deficits (Castel, Lee, et al., 2011). The agenda-based regulation (ABR) model of self-regulated learning posits that learners decide which information to study; for how long to study it; whether to restudy it; et cetera based on their learning and performance goals and an overarching agenda which they have constructed so as to attain these goals as efficiently at possible (Ariel et al., 2009; Dunlosky et al., 2011; Dunlosky & Ariel, 2011a). In the case of value-directed remembering, the importance or value of the to-be-learned information is the primary factor upon which learners are basing their study decisions. In other words, the learner has adopted and executed a value-based study

agenda so as to strategically offset an inability to remember the full set of to-be-learned information. Under the ABR model, the central executive of working memory is responsible for constructing, maintaining, modifying, and executing this agenda during study. Thus, the extent to which a learner *can* adopt and execute an efficient, compensatory value-based study strategy should depend upon the state of the learner's central executive and working memory, consistent with reductions in selective performance accompanying attentional deficits.

The research conducted in the preceding four chapters was designed to clarify the impact that various factors during encoding and retrieval—including those factors that generally impair memory in a global sense and thus further limit the learner's encoding capacity—can have on a learner's self-regulated study and strategic prioritization of important information. The examined factors were selected because they have either have been shown to influence the decisions that learners make during study (and the efficacy of these decisions) or are expected to be of influence as per models of self-regulated learning (Nelson & Narens, 1990; Thiede & Dunlosky, 1999; Winne & Hadwin, 1998) and, specifically, the ABR model (Dunlosky & Ariel, 2011a). In investigating each of these factors, consideration across experiments was given to the scope of the learner's overall encoding capacity (i.e., the amount of information the learner can ultimately recollect) and the source of limitations to this capacity; the extent to which the learner was tasked with overtly self-regulating his/her study (e.g., whether the learner chose what/when to study information or was automatically directed by the experimenter); and the degree to which the learner's central executive of working memory was stressed.

Across all examined factors, there was a consistently greater probability of participants attending to and recalling high-value items over low-value items (at least when participants' expectations with respect to the upcoming testing circumstances whilst studying were congruent

with reality; see Chapter 5; Middlebrooks, Murayama, et al., 2017), consistent with models of self-regulated learning in that participants considered relevant task constraints and item features (viz. value/importance) and appropriately modified their study behaviors (overt or otherwise, as per the task design) (Dunlosky et al., 2011; Nelson & Narens, 1990; Winne & Hadwin, 1998). That said, there were some situations in which stress to the central executive did, as predicted, appear to impede value-based study/recall (Chapter 2) and others in which—contrary to the ABR model's predictions—value-based study and recall was maintained despite additional stress to the central executive mechanism of working memory (see Chapters 3 and 4).

Overview of Findings

Chapter 2: Presentation format.

Two experiments were conducted in Chapter 2 to determine whether documented impairments to self-regulated learning when studying information sequentially, as opposed to simultaneously (Ariel et al., 2009; Dunlosky & Thiede, 2004; Thiede & Dunlosky, 1999), extend to the learning of and memory for valuable information (Middlebrooks & Castel, 2018). Participants studied at a preset presentation rate of 3 seconds per item (Experiment 1) or self-paced their study (for up to 60 seconds of study per list) and could choose to restudy items or skip items entirely as it suited (Experiment 2). Participants across experiments prioritized high-value over low-value information, irrespective of presentation format, but those who studied with all of the to-be-learned information accessible on the screen simultaneously demonstrated superior value-based prioritization with respect to recall, study selections, and self-pacing. These results support prior findings that devising, maintaining, and executing an efficient study agenda is inherently different under sequential formatting than simultaneous. As sequential formatting is thought to be of greater demand to working memory resources than simultaneous

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formatting (Ariel et al., 2009; Thiede & Dunlosky, 1999), evidence of impairment to value-directed remembering under sequential study conditions relative to simultaneous presentation suggests that value-directed remembering is not immune to central executive stressors (Middlebrooks & Castel, 2018), consistent with the ABR model.

Chapter 3: Time constraints.

Four experiments were conducted in Chapter 3 to assess the impact of time limits during study in isolation (Experiment 1; Middlebrooks, Murayama, et al., 2016) and in conjunction with potential diminishment of the central executive owing to age-related changes in cognition (Experiment 2) and feelings of time-related stress/pressure (Experiments 3A and 3B) to value-directed remembering. In Experiment 1, half of the participants studied at a constant presentation rate of either 1 second or 5 seconds per item for the first eight lists of the task and the other half of participants studied under both rates (a 1-second rate during Lists 1-4 and a 5-second rate during Lists 5-8, or vice versa); all participants then self-paced their study during the final four lists (i.e., Lists 9-12), without a limit to the total time they could study. Regardless of assigned study time or prior timing experiences, there were no significant differences in selectivity or (during Lists 9-12) value-based self-paced study.

Using the same paradigm, Experiment 2 extended this finding to determine whether older adults similarly accommodate limits to the time with which they have to study information. Despite general age-related declines in working memory capacity and executive control mechanisms (e.g., Salthouse, 1996, 2000), older adults were just as selective as younger adults under conditions of fast-paced study.[39] The results of Experiments 1 and 2 were thus consistent with models of self-regulated learning (including the ABR model): Participants evidently

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[39] Although older adults were actually *more* selective under a slower study rate than they were at the faster rate, they were nevertheless able to adapt their value-based study to accommodate the insufficient study time so as to perform comparably to younger adults, if not to their own potential.

considered the constraints of the task and adapted their studying accordingly (Dunlosky et al., 2011; Winne & Hadwin, 1998).

Experiments 3A and 3B considered potential stress to the central executive caused by anxiety (Beilock & Carr, 2005; Markman et al., 2006; Régner et al., 2010) induced via feelings of pressure resulting from limited study time. The paradigm used in Experiment 1 was modified such that the faster-paced portions of the task were self-paced rather than experimenter-paced, but speeded study equivalent to the 1-second experimenter rate was encouraged via point penalizations (viz. loss of a point from the participant's final score for each additional second of study; a timer on the screen indicating the time spent studying a given item that began flashing red after 1 second of study; and verbal prompts from the experimenter in the testing room to study faster). Experiments 3A and 3B differed only in whether the task instructions were loss-focused (i.e., avoid losing points because of prolonged study) or gains-focused (i.e., gain as many points as possible by studying faster), respectively.

The results of these Experiments 3A and 3B were, admittedly, less straightforward—participants who were continuously rushed during the task became more selective in Experiment 3A than participants in the other conditions, but participants who were rushed after prior experience with a slower study rate became *less* selective than other participants in Experiment 3B. Participants reported notable feelings of anxiety owing to the rushed portions of the task in both experiments (and so it is reasonable to suspect there was at least some degree of additional stress to the central executive relative to non-rushed participants), but there was not a consistent effect—positive *or* negative—of rushing on selectivity either within or between experiments. It is thus impossible to determine the extent to which these differences in selectivity arose owing to feelings of anxiety or task perspective in isolation or interactively from Experiments 3A and 3B

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alone. Continued research into both of these factors is clearly necessary as their independent influences are presently unclear, yet at least one factor was plainly influential.

Nevertheless, there is evidence across the experiments within Chapter 3 to indicate that learners can and will (at least in certain situations) accommodate constraints to study time—consistent with models of self-regulated learning (Dunlosky et al., 2011; Dunlosky & Ariel, 2011a; Nelson & Narens, 1990; Winne & Hadwin, 1998)—continuing to prioritize valuable information in light of their (even further) limited encoding capacity. Moreover, costs to working memory that arise independent of or as a consequence of such time constraints will not (necessarily) impair a learner's self-regulated study of valuable information, which is ultimately more inconsistent than not with the predictions of the ABR model (Dunlosky et al., 2011) and with prior demonstrations of impaired self-regulated learning under cognitively stressful study circumstances (e.g., Ariel et al., 2011; Ariel & Dunlosky, 2013; Dunlosky & Ariel, 2011b; Middlebrooks & Castel, 2018; Thiede & Dunlosky, 1999).

Chapter 4: Distractions and multi-tasking.

Four experiments were conducted in Chapter 4 to examine the impact of restrictions to a learner's attention owing to distractions or multi-tasking on the self-regulated study of valuable information. Across experiments, neither listening to background music—with which the participant was familiar or not—nor completing concurrent digit- or tone-detection tasks that ranged in the extent to which they demanded working memory resources influenced selectivity, let alone impaired it (Middlebrooks, Kerr, et al., 2017). Furthermore, distractions and multi-tasking continued to be inconsequential to value-based study regardless of participants' responsibility for self-regulating the pacing of their study.

These results are fundamentally inconsistent with the predictions of the ABR model (Dunlosky et al., 2011). The immunity of participants' value-based study to background music is less surprising—ignoring background music at least to the point of preventing it from impacting one's study and recall should place some demand on cognitive resources, but perhaps not so much as to overwhelm the central executive and impede agenda-based study—but the consistently null effect of multi-tasking to self-regulated study and prioritization of valuable information across experiments is particularly noteworthy. Not only is multi-tasking reliably detrimental to memory and cognition (e.g., Barnes & Dougherty, 2007; Gaspelin et al., 2013; Naveh-Benjamin et al., 2000), but the act of multi-tasking is inherently a central executive task and so should affect a learner's ability to adopt, maintain, and execute an optimal study agenda.

Given that there is certainly evidence in favor of the ABR model (e.g., Ariel et al., 2009, 2015; Ariel & Dunlosky, 2013; Middlebrooks & Castel, 2018; Thiede & Dunlosky, 1999), the null effects reported in Chapter 4 should not be viewed as strong evidence *against* the model. Rather, the present findings highlight the continued necessity of research investigating not only the consequences of multi-tasking to self-regulated learning—specifically of valuable information and more broadly—but also of the scope of demands to the central executive that can nevertheless be managed whilst still maintaining and executing efficient study agendas.

Chapter 5: Testing influences.

Experiments 1 and 2 in Chapter 5 examined the potential influence of a learner's beliefs and expectations about the circumstances under which the studied information will later need to be retrieved (Winne & Hadwin, 1998) on their study of and memory for valuable/important information (Middlebrooks, Murayama, et al., 2017). Half of the participants in each experiment were told to expect a recognition test after each list of study; the other half was told to expect a

recall test. After several lists of receiving tests congruent with expectations, participants studying for a recognition test instead received an unexpected recall test. In Experiment 1, participants who had studied for a recognition test recalled less of the valuable information than participants anticipating the recall format. These participants continued to attend less to item value on future (expected) recall tests than participants who had only ever experienced recall testing, even after controlling for general experience with the recall test format.   When the recognition tests were made more demanding in Experiment 2, value-based recall improved relative to Experiment 1. Although memory for the valuable information remained superior when participants studied with the expectation of having to recall the information, there were no longer significant differences after accounting for recall testing experience. Thus, recall-based testing encouraged strategic, value-based encoding and enhanced retrieval of important information, whereas recognition testing in some cases *discouraged* value-based study and memory.

Again, nothing about the study periods themselves in Experiments 1 and 2 differed across participants, only the beliefs that participants entertained as to what the subsequent retrieval context would require from them, what they would be expected to produce from the just-studied list. As opposed to instances of poorer self-regulated, value-based study under demanding study conditions (e.g., Ariel et al., 2009; Middlebrooks & Castel, 2018), participants who anticipated recognition tests in the present research were not less selective as a consequence of increased difficulty executing (or even an inability to execute) a strategic study agenda, but rather owing to an *intentional* decision on their part to be *less* attentive to item importance during study. This finding is consistent with models of self-regulated learning and the ABR model in that participants considered the retrieval circumstances as a guiding factor upon which to base study

decisions. It is even demonstrative of strategic study in a sense—why direct your attention in an effortful fashion when doing so is unnecessary for the particular requirements of the retrieval circumstances? This particular strategy—again, adopted based on beliefs/expectations about the testing demands—however, was ultimately detrimental to participants' learning of the most important information and made it difficult to adapt to ultimately demanding retrieval circumstances.

Future Directions

Stress to the central executive during encoding.

As noted, the results of the experiments within this dissertation are all indicative of participants having self-regulated their study of the presented, to-be-learned materials as per a value-based study agenda. The inconsistencies with respect to the effects of heightened demands on attention and working memory during study relative to the predictions made by the agenda-based regulation (ABR) model of self-regulated learning, however, make it clear that further research is needed to elucidate the extent to which the central executive of working memory can be stressed before affecting the efficiency with which learners study and selectively prioritize valuable information.

One potential explanation for the discrepancies in the influence of central executive stress to value-directed remembering in the present research is the difference between the ability to adopt/execute a value-based study agenda and the ability (or even inclination) to do so spontaneously. Robison and Unsworth (2017) reported that individuals with lower working memory capacity were less likely to spontaneously adopt optimal study strategies but could largely abide by instructions as to how best to allocate their study time when provided, thereby minimizing the disadvantage of their comparatively lower working memory resources.

Similarly, the differences in selectivity demonstrated between participants who studied under sequential formatting and those who studied all of the to-be-learned information simultaneously may not have arisen because sequential formatting stressed the central executive to the point that participants were unable to execute a similarly selective strategy, but rather that they were less likely/able to *spontaneously* generate one (see Chapter 2).

Had participants in the sequential study conditions been provided with explicit guidance as to a better study strategy (e.g., to skip low-value information entirely; see Chapter 2, Experiment 2 and Figure 2.3)—or even been asked to explicitly describe the strategy by which they were presently abiding and to consider ways upon which it might be improved—differences in selectivity owing to presentation format might well have been negated. Of course, even if it is the case that moderate stress to the central executive is of greater direct influence to spontaneous strategy adoption than to strategy execution, impediments to adoption are nonetheless costly to efficient study, but they could perhaps be more easily circumvented (e.g., via explicit strategy recommendations) than impediments to execution.

If it is the case that strategy adoption is more greatly influenced by stressors to the central executive than is strategy execution, the question still remains as to why demands on attention and working memory (like feelings of stress, listening to background music, or multi-tasking) during study did not similarly impact selectivity (see Chapters 3 and 4). One possibility is that these sources of stress to working memory resources were more salient to participants than the stress of sequential formatting. As such, participants may have been more aware of a need to overcome the constraints posed by such factors and thus more likely to actively (and spontaneously) devise a compensatory strategy. Future research should aim to clarify not only whether stress to the central executive differentially influences agenda adoption relative to

agenda execution, but also the consequences of subtle versus blatant stressors during study to spontaneous agenda adoption and self-regulated learning, in general. The ABR model proposes that diminished central executive resources will prevent learners from studying efficiently (Dunlosky et al., 2011; Dunlosky & Ariel, 2011a). This may be true in the extreme, but it is also possible that minor-to-moderate stress, especially when not particularly salient, simply reduces the likelihood of spontaneous agenda adoption.

Source of importance.

The research conducted within this dissertation has focused on situations in which learners are attempting to encode information that differs in importance or value. Critically, these values were always explicitly provided during the encoding stage. While it was the participant's responsibility to determine how best to incorporate this value information during study (if at all; see Chapter 5), at no point in the present research did the participant need to first judge the importance of the to-be-remembered information before adopting or executing their encoding strategies. In real-world situations that might warrant selective, value-based encoding, however, the burden is typically on the individual to first judge the importance of the information.

There are countless reasons for why a learner might consider information important or not, and certainly situations in which a learner's judgment of importance is arguably inaccurate. The same unit of information, for example, might be deemed unimportant by an uninterested learner but important by a (perhaps even similarly uninterested) learner who is motivated by different performance goals. Similarly, a novice might be less able to distinguish the differences in importance between two units of information in attempting to master a concept, whereas an expert might readily recognize the differences and so be in a better position to encode extensive

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information strategically. In a general sense, efforts to understand how learners self-regulate their study as per the importance of the information set before them must be accompanied by efforts to understand how learners ascertain importance in the first place and how their judgments align with relatively objective criteria.

In terms of learning and the study behaviors in which learners will engage, whether information is important or not is at least partially dependent upon whether that information will likely be needed in future. If there is no chance of a given set of information appearing on an upcoming test, is it not efficient for a learner to therefore bypass that information during study in favor of information which likely be tested? Prior research—including that of Chapter 5 (Middlebrooks, Murayama, et al., 2017)—demonstrates that learners adapt their study to perceived testing demands (d'Ydewalle et al., 1983; Finley & Benjamin, 2012; Garcia-Marques et al., 2015; Jensen et al., 2014; Lundeberg & Fox, 1991; McDaniel et al., 1994; Ross et al., 2006; Storm et al., 2016; Thiede, 1996; Thiede, Wiley, & Griffin, 2011). When anticipating free-recall tests, for example, learners will intentionally ignore cue words in to-be-remembered cue-target pairings, while those anticipating cued-recall tests will intentionally focus on the cue-target association (Finley & Benjamin, 2012); when expecting a more difficult test, they will study for longer (Thiede, 1996). Evidence also indicates that learners will use previous testing experiences as a guide for metacognitive judgments of comprehension/learning about novel materials (Kelemen, Winningham, & Weaver, 2007; Thiede et al. 2011).

When the probability of a studied item appearing on the upcoming test is explicitly noted (e.g., indicating whether a presented item has a 10% or 90% chance of appearing on the upcoming test), learners spend more time studying/restudying—and better remember—the high-probability items relative to the low-probability items (Ariel et al., 2009; Dunlosky & Thiede,

1998). Testing probability is generally *not* explicitly noted, though, at the time of study. One question is whether instances of prior testing/retrieval influence perceptions of what constitutes importance and whether such perceptions (if held) influence subsequent study events. For instance, if an instructor tests his students on bolded vocabulary terms from the textbook on the midterm exam but never asks about the diagrams, this may suggest to students that they should prioritize vocabulary terms when studying for the upcoming final exam (Broekkamp & Van Hout-Wolters, 2007)—at least as it pertains to this instructor, knowing the course jargon would seem to be important if attempting to improve one's exam score.

Early research suggests that strategy adoption and modification as per prior testing experiences may be less apparent than when importance or testing probability is explicitly noted (Ariel, 2013), but the fundamental role of retrieval and performance feedback in self-regulated learning models deserves continued study in this domain. Moreover, if it *is* the case that learners are not gauging importance as per prior testing experiences, this is itself a point of interest and even a sphere in which instructional interventions might be warranted. Even in the event that learners fare more poorly when the onus is placed on them to identify importance prior to executing a value-based study strategy, proactive measures could perhaps be taken to provide more explicit guidelines as to importance when attempting to convey to-be-learned information (e.g., bolding/underlining key points in textbooks or pamphlets).

Aside from judgments of testing probability, consideration should also be given to situations in which there is conflict between the learner's subjective judgment of importance and other relevant (but still largely subjective) criteria, such as an instructor's judgment of importance. Considering that instructors themselves have been shown to disagree in their identifications of the most important elements within course materials (Broekkamp et al., 2002;

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Jetton & Alexander, 1997; Schellings & Van Hout-Wolters, 1995), catering one's study to the instructor (or, more generally, party responsible for the retrieval context) rather than the material, so to speak, is arguably more strategic than aligning one's study with independent judgments of importance. The extent to which learners regulate their study as per instructional importance—what does the *instructor* think is the most important and on what is the instructor therefore most likely to test? (Alexander et al., 1994; Broekkamp & Van Hout-Wolters, 2007; Entwistle & Entwistle, 2003; Jetton & Alexander, 1997; Schellings & Van Hout-Wolters, 1996)—instead of structural importance (the extent to which something contributes to one's understanding of a concept) is presently unclear.

Stress to the central executive during retrieval.

The ABR model of self-regulated learning emphasizes the importance of the central executive mechanism of working memory to self-regulated learning during study (Dunlosky et al., 2011; Dunlosky & Ariel, 2011a). Presumably, though, the central executive is also important to agenda-based study and self-regulated learning during retrieval. The focus in the present dissertation (and in prior research; cf. Castel, 2008 and Castel et al., 2012) has been on how learners allocate their available resources during study across information that varies in importance when all of it cannot be feasibly remembered, but might stress to the central executive during *retrieval* disrupt agenda-consistent recall?

For instance, would a distracted test-taker be less likely to recall the most important information—or less likely to consistently recall it before recalling the less important information—when pressed for time? This might be less likely in the event that the information had been studied extremely selectively in the first place; if far more important information was originally encoded than was less important information, it would be somewhat of a challenge to

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recall the less important in the first place simply because there would be a relatively smaller proportion of it. In the event that an adopted value-based agenda during study did not summarily exclude everything but the most important information, however, strategic value-based retrieval might be desirable if time is of the essence (as it often is), and stress to the central executive during this time might render prioritization of high-value retrieval over low-value retrieval less probable.

Aside from consequences to the retrieval/test event itself, stress to the central executive during retrieval could negatively impact future instances of self-regulated study of related information. A pivotal point in self-regulated learning models is the contribution of retrieval or testing experiences to future encoding behaviors (Nelson & Narens, 1990; Winne & Hadwin, 1998). Learners are guided by set goals during self-regulated study.[40] Ideally, the learner regularly contrasts his/her current state of knowledge with this goal state and, based on metacognitive monitoring (e.g., judgments of learning, feelings-of-knowing; Bjork et al., 2013; Nelson & Narens, 1990), adjusts study behaviors, agendas, et cetera to reduce the discrepancy between the current state and the goal state. External sources of feedback (e.g., an exam score) can provide the learner with pertinent information as to the accuracy of his/her metacognitive judgments (e.g., were expectations as to prior retrieval performance met? had prior study actually been as constructive as anticipated?) and clarify the distance between the current and goal learning/performance states that remains to be traversed. Ultimately, the success of one's self-regulated study will largely depend on the accuracy of one's monitoring, including the incorporation of external feedback/information (Thiede et al., 2003).

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[40] Note that the goal may not be optimal, realistic, or even particularly specific, but certainly *some* goal is being entertained if one is intentionally attempting to encode a set of information. Self-regulated learning models do not depend upon the acumen of the learner, but merely outline a trajectory of behaviors and relevant factors for consideration during the act of self-regulating one's study.

In order for the learner to optimally acquire agenda-relevant information about the retrieval context so as to subsequently incorporate this information during future study sessions (e.g., "I studied vocabulary, but there are no questions about vocabulary on this Midterm…won't do that for the Final!"; "I spent hours rereading those chapters and now I can't remember any of it!"), it may be necessary to have sufficient working memory resources to recall the prior study session(s) and contrast previously held expectations about the retrieval circumstances with the realities of the retrieval event in real-time. This is not to say that compromised working memory at the time of test would make it impossible to later retrieve relevant features during a future study event, but it could make strategic study in the future more effortful if the learner must actively retrieve prior retrieval instances and appropriately modify his/her study agenda at the time of later encoding than if the agenda could have been so modified during or immediately following the retrieval event.

Whether stress to the central executive during retrieval could have such consequence on future study efficacy might partly depend on whether the retrieval event is itself a part of the broader study effort. Consider the act of testing one's self during a study session. Testing has been shown to be one of the most beneficial methods of learning (Dunlosky, Rawson, Marsh, Nathan, & Willingham, 2013; see Roediger & Karpicke, 2006 for a review). Although much of the benefit is a direct consequence of the act of retrieval serving to strengthen the memory itself, an indirect benefit of testing is also that it highlights which of the to-be-learned information has yet to be adequately mastered. If a learner incorporates testing into his/her study sessions (e.g., by completing a "practice test" provided by an instructor or textbook company, or simply by attempting to retrieve the information unaided), the adopted study agenda in the bout of study immediately following a test phase may be more notably modified by what was ascertained from

the test (e.g., "I should restudy vocabulary," "I forgot the most important event during Abraham Lincoln's presidency!") than in the case of a prolonged delay between a testing event and subsequent study (e.g., the weeks between taking a midterm and studying for a final exam). If the learner completed the practice test under conditions that are stressful to the central executive, s/he may fail to sufficiently recognize and incorporate agenda-relevant information from the test during subsequent study—for instance, the learner might neglect to restudy important information that was previously forgotten on the test, or to study even more selectively in the event of lower-than-anticipated test performance.

One final point of consideration (for the purposes of this dissertation) with respect to the potential consequences of stress to the central executive during retrieval is the possibility that it might impede evaluations of importance when importance is based on probability of future need (e.g., the probability of appearing on a future test), thereby decreasing the learner's ability to subsequently execute a value-based study strategy of relevant information. If the learner fails to ascertain what type of information is or is not likely to be important in the future based on prior experience, then they may not have sufficient information by which to guide their future study and any value-based prioritization efforts. Although it is presently unclear as to whether learners acquire such value-based information during retrieval even in the absence of central executive stressors (Ariel, 2013), there is certainly evidence that people *can* learn to improve their study techniques and to refine their strategies from prior retrieval experiences (e.g., Finley & Benjamin, 2012; Garcia-Marques et al., 2015; Middlebrooks, Murayama, et al., 2017). If the learner's working memory capacity during retrieval is relatively diminished, s/he may be less able to judge importance/testing probability whilst also completing the test and then later utilize those judgments at the time of future study. In this case, stress to the central executive during

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retrieval—irrespective of working memory resources during study—could prove to impair future value-directed remembering of related material with similar retrieval need/testing probability simply because, having not been acquired during the prior test, such information would be unavailable during study.

Conclusion

Critical future directions notwithstanding, the results of the experiments outlined in the present dissertation are all consistent with general models of self-regulated learning. Regardless of the particular factor being investigated—the manner in which information was organized during study; the presence of time constraints during study; ongoing distractors or concurrent tasks during study; and the anticipated format and demands of the testing circumstances— participants considered and adapted to constraints during encoding and retrieval, continuing to selectively prioritize high-value information during study in an effort to offset an inability to recall the full set of to-be-learned information with which they were tasked.

This continued prioritization of high-value information during study was not, however, consistently aligned with predictions made by the agenda-based regulation model of self-regulated learning, which posits that a learner's ability to adopt and abide by a particular agenda during study is dependent upon the central executive mechanism of working memory, stress to which should impair agenda-based study (Ariel et al., 2009; Dunlosky et al., 2011). Participants in the present experiments continued to study as per a value-based agenda despite a range in stress to the central executive during encoding; only in the case of stress owing to suboptimal organization of the to-be-learned information was there any indication of (relatively) suboptimal value-based study.

Continued research is warranted to identify relevant boundary conditions and further moderating factors, but the present experiments demonstrate that a learner's ability to recognize and strategically offset encoding capacity limitations during study by selectively prioritizing the most important or valuable information is not so fragile as to immediately succumb to even fairly pronounced stressors.

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