Human Memory in a Modern World:
Identifying the Cognitive Mechanism behind Poor Memory for Digitally Available Information

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A thesis submitted in partial fulfillment of the requirements for the degree of

Doctor of Philosophy

in Cognitive Psychology, Neuroscience, and Cognitive Science

at University of Colorado Boulder

May, 2017

This thesis entitled:
Human Memory in the Modern World:
Identifying the Cognitive Mechanisms Behind Poor Memory for Digitally Available Information
by Shane R. Schwikert
has been approved for the Department of Psychology and Neuroscience

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Date: 5/10/2017

The final copy of this thesis has been examined by the signatories, and we find that both the content and the form meet acceptable presentation standards of scholarly work in the above-mentioned discipline

HRC protocol # 16-0288

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Thesis directed by Professor Tim Curran

Abstract

The emergence of the Internet, and its connected devices, are fundamentally changing the way we interact with information. New technology has created affordances that both lessen the burden on human memory and facilitate access to a nearly infinite amount of knowledge. In six experiments, the current research sought to determine how increasing interactions with technology are affecting memory, and to identify the cognitive mechanisms that are driving those changes. Experiments 1a, 1b, and 2 implemented modified item-method directed forgetting paradigms to examine how future access to information affects subsequent memory. Results indicated that explicit information about future availability delivered as a post-stimulus cue did not impact memory, whereas memory was poorer for more readily available information when the inherent availability of the stimuli was manipulated. Experiment 3 delivered explicit availability as a pre-stimulus cue, and found that this change in timing resulted in poorer memory for information that was tagged as explicitly available in the future. Experiment 4 allowed availability to vary on a more natural scale, but failed to find significant memory differences that were not moderated by age. Experiment 5 implemented a list-method directed forgetting design, and corroborated findings from Experiment 3 indicating that an explicit availability pre-cue affected subsequent memory, whereas an analogous post-cue did not. Taken together, this research provides considerable evidence that availability-related memory effects are separable from directed forgetting effects, and are driven by differences that occur during the encoding phase rather than by later intentional forgetting mechanisms.

Acknowledgements

My past six years at the University of Colorado has constituted a period of intense learning and intellectual stimulation that will not soon be matched. I would first like to extend my deepest gratitude to my advisor Tim Curran. I consider myself beyond fortunate to have been able to work with a mentor who provided continuous support, invaluable feedback, and a willingness to explore new fields and ideas, which all helped make this thesis possible. I would also like to thank Alice Healy, Al Kim, Chris Loersch, and Tammy Sumner for their thoughtful intellectual contributions that helped shape this project. To my parents, Tiffany, family, friends, and the mountains – all of whom have provided endlessly wise counsel and support throughout my pursuit – you are deeply loved.

This research was funded by NSF Grant #SMA-1041755 to the Temporal Dynamics of Learning Center, an NSF Science of Learning Center

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We are in the midst of what has been coined the "Information Age" – the most recent period in human history where advancement of the digital industry has created a knowledge-based society. The Internet and personal devices such as smartphones and laptops have become an integral part of our daily lives, and spreading access to these devices has promoted an environment of constant and rapid information transfer. With ubiquitous and connected tools such as Google, we can obtain the answers to limitless questions by simply tapping a few words into a search box. Widespread accessibility to digital materials such as interactive lectures, textbooks, and video conferencing tools are inducing a seismic shift towards online learning at even the highest levels of education (Lewin, 2012).

Psychological research on this phenomenon, though in its infancy, has begun to identify different ways that this technological immersion is influencing human memory and cognition. However, the extant findings tend to appear disconnected and lack an overarching framework, as researchers across multiple domains and from different backgrounds attempt to tackle this novel question. Further, there has been no concerted effort to identify the psychological mechanisms or lower level processes that are responsible for the technology-driven changes in memory.

In the current thesis, I will first highlight the various ways in which modern technologies are creating novel affordances that enable us to interact with information in new ways. Next, I will synthesize pertinent findings concerning the effects that widespread use of technologies is having on memory, and identify future availability of information as a common theme across studies. In the final section of the Introduction, I will review literature on directed forgetting and offer a detailed account of the memory processes and cognitive mechanisms that produce this type of forgetting. Theoretical perspectives from directed forgetting will be harnessed and applied to future availability-related memory findings in order to create a fruitful research path forward. I will then introduce a rigorous series of six experiments specifically geared towards identifying and characterizing the primary psychological mechanisms underlying technology-driven memory effects. Finally, I will review the findings and discuss their implications across various domains.

Cognitive Offloading

The term "cognitive offloading" is intuitive and appealing, to the point where a definition feels unnecessary. Generally, this term fits with our intuitions and refers to our ability to utilize external resources in our environment in order to reduce internal cognitive demands. The concept has become popularized by cognitive scientists' increasing interest in broader conceptions of cognition, such as extended and embodied cognition (e.g., Clark & Chalmers, 1998; M. Wilson, 2002). Cognitive offloading provides a useful and intuitive framework for thinking about technology as an external memory resource that can be used to reduce the burden placed on internal memory (for a review, see Risko & Gilbert, 2016).

Yet, the idea that we can enhance cognition by offloading processing demands onto our environment is not new (see Clark & Chalmers, 1998; Dror & Harnad, 2008; Kirsh & Maglio, 1994; Tversky, 2011; M. Wilson, 2002; R. A. Wilson, 2004). We have been using other tools to record thoughts and memories onto external resources for many years, some of which include: paper and pen, shopping lists, calendars, books, diaries, alarm clocks, and personal photographs. Cognitive offloading even extends to more ancient phenomena such as finger-counting (Bender & Beller, 2012), where we actually offload processing demands from the mind onto other areas of the body. If this concept is not new, how might modern technologies such as the Internet and smartphones promote cognitive offloading that is notably different from that provided by conventional affordances?

Research on established offloading tools such as those noted above (i.e. paper and pen) has focused on their use as external memory stores, or a “memory aids” (Harris, 1980). The most ubiquitous and obvious memory aid in recent history is the written word. For example, research has shown that shopping lists are effective memory aids because they (externally) store the memory to purchase needed goods, which thereby frees up cognitive resources in order to simplify decision making (Block & Morwitz, 1999). Written reminders may seem trivial in today’s world, but they have played a major role in people’s ability to restructure information and organize their daily lives (Coupey, 1994; Intons-Peterson & Fournier, 1986). One characteristic of traditional memory aids (and earlier forms of external memory storage in general) is that the vast majority of offloaded information is personally relevant. Personal calendars (Payne, 1993), shopping lists (Block & Morwitz, 1999), diaries (Verbrugge, 1980), and hand written lecture notes (Mueller & Oppenheimer, 2014) can all function as memory aids, but also contain information relevant to only one specific person.

In contrast, a smartphone connected to the Internet can provide nearly infinite amounts of knowledge, far beyond what is personally relevant to an individual. Individuals can also rely on this same device to set an alarm or written reminder, communicate with friends and family, store contact information, organize multiple calendars, take written or spoken notes, and shoot pictures and video. Information can be captured across visual and auditory domains, with nearly no concern for storage limitations. And these are considered only the most basic features of contemporary smartphones. Modern devices have integrated all the most useful features of previous forms of external memory into a single, convenient hand-held device. Because these devices are so unobtrusive and vastly outperform all previous means of external memory storage, they could encourage people to offload an increasing amount of responsibility for information (Ward, 2013a). In other words, we might be leaning on these newer cognitive crutches to a much greater extent than previous external memory tools.

Transactive Memory

The concept of a transactive memory system was formally introduced by Wegner (1987) as means of externally storing information across a close-knit social network. The system functions based on the principles of accessibility, relative expertise, and negotiated responsibility (Wegner, 1995), such that different individuals are inherently responsible for remembering certain types of information. Transactive memory systems maximize cognitive efficiency by increasing the amount of information available to individuals, and distributing storage responsibilities among members of a group. Recently, several researchers have begun framing the Internet as a sort of exaggerated transactive memory partner - an external memory source that can be accessed at any time (Sparrow & Chatman, 2013; Sparrow, Liu, & Wegner, 2011; Ward, 2013a). Internet search engines such as Google have become a mnemonic tool (a transactive memory source) that can be used to instantly acquire any desired piece of information.

Prior research suggests that unclear boundaries between individuals in a transactive memory structure may cause each individual to lose track of which memories are stored internally in their own mind, and which are stored externally in a partner's mind (e.g., Hinsz, 1990; Wegner, 1995). Similarly, people who use the Internet to perform a task may misattribute the causes for their success to their own knowledge or skills rather than the Internet's knowledge. A study conducted by Ward (2013b) showed that this source confusion phenomenon could indeed translate to information obtained from the Internet. When searching the Internet using

Google, Ward found that participants often misattributed information gathered online to information contained within their own memory when trivia questions were of medium difficulty. Interestingly, when Ward conducted the same experiment but slowed the Internet speeds (and therefore Google search times), the effects disappeared. These findings imply that the blazing speed with which we can now obtain information, a novel characteristic of our modern devices, is affecting cognition in ways that previous external memory stores did not. Beyond simply using technology as a memory aid, there is evidence the Internet is having real and novel effects on our metacognitive awareness.

Making the assumption that modern devices are simply a new instantiation of conventional memory aids would be doing them a great disservice. The breadth of information transfer and management that these devices enable, combined with increasing frequency and ease of use, suggests that they could be having much different effects on memory and cognition compared to traditional external memory stores. The convenience of being able to slip these devices in our pocket and bring them with us anywhere also means that there is virtually no barrier to access – we can tap into these tools at a moment's notice. In fact, in many situations, a quick search through our smartphone could be less effortful than an exhaustive search through memory.

If our personal technologies are becoming almost as easy to use as our biological memory systems, then how do we decide what information to offload to our devices, and what to leave to our internal systems? Preliminary research has begun to address this difficult problem, and the data suggests that individuals will opt to use external storage when they value the accuracy of the information to-be-remembered (Risko & Dunn, 2015). Surprisingly, the perceived effort of maintaining memory for an item played only a small role in individuals' decisions to offload.

Yet, this research operates under an important assumption: that individuals have the means and ability to choose to offload information. Understanding how, and to what extent, individuals choose to use their devices as offloading tools is an important question with significant implications that should garner future attention. However, most of the extant research has manipulated offloading such that the participant does not have an explicit choice to offload, but rather encounters information that has been offloaded in different ways.

Future availability - A benchmark for offloading

Information in our world today exists in a type of hierarchy. There are certain types of information that most of us have reliably delegated to our devices. For example, smartphone owners (especially those of a younger generation) will be hard-pressed to recite the phone numbers of more than a handful of acquaintances from memory. Similarly, active Facebook users would likely struggle trying to recite more than a handful of their acquaintance's birthdays. Many of us keep online calendars that trigger digital reminders, and without these notifications our attendance record at work meetings would likely have a few more blemishes.

In these situations, though technically there is a choice to offload information, we are influenced by the affordances of the technologies we use. Smartphones have contact lists that make it effortless to find any stored number, and Facebook automatically reminds us of all our friends' birthdays just as our calendars reliably and punctually shuffle us towards our next meeting. The affordances of modern technologies likely guide what type of information we offload and what we choose to remember. Indeed, in modern society we are interacting with information at a historically unprecedented rate (Bawden & Robinson, 2009). Over time, as individuals experience a variety of information through different digital avenues, people may become accustomed to certain types of information being more available than others. This heightened sense of future availability could then be a useful cue when determining whether to internalize a piece of information or to offload it to a digital counterpart.

In this sense, future availability can be thought of as a type of index to offloading. Information that we anticipate being readily accessible can be offloaded, and less accessible information can be prioritized by internal memory systems. This is an appealing theory, and it is one that we will discover unifies much of the research in this domain.

Sparrow et al. (2011) performed one of the first studies to directly examine the effects of modern technology on memory by instructing participants to type trivia statements into a computer. Half the participants were told beforehand that the statements would be saved, and half were told that the statements would be erased. In addition, half the participants in each condition were explicitly told to try to remember the statements. At test, there was only a main effect of the saved/erased condition; participants who believed the statements would be erased recalled significantly more trivia statements than participants who believed the statements would be saved on a computer. Instruction to remember had no effect. The authors suggested that participants did not make the effort to remember when they thought they could later look up the trivia statements that they read, and instead relied on the external memory store (computer). The cue regarding whether information would or would not be available in the future determined memory, regardless of if they thought they would be tested on it (explicit memory instruction). These results were interpreted to support the claim that people intuitively and automatically offload responsibility for information to technology, and that this tendency is so strong that even explicit instructions to do otherwise are ineffective.

Since Sparrow et al.'s (2011) initial findings, several studies have implemented various techniques and arrived at the same conclusion: Memory for information *assumed to be available* *in the future* is strategically offloaded to external resources. These studies all demonstrate forgetting or attenuated memory in response to the external storage of information, though they can be divided into two separate groups based on the experimental procedures that were implemented.

The first group of studies manipulated different items' availabilities *prior to the presentation of the information*, by alluding beforehand that the material would be saved externally (i.e., Sparrow et al., 2011). Participants in these experiments knew at the onset whether or not the information they experienced would be available to them in the future. The second group of studies manipulated availability *after the presentation of information*, by later indicating that the study material had been saved externally. Participants in these experiments first encountered the study information, and then were told whether or not that information would be available to them in the future. Delivery of a future availability cue may come before or after the presentation of some to-be-remembered information, and this difference in timing could insinuate that different underlying mechanisms are responsible for fluctuations in subsequent memory. For this reason, we will present these two groups of experiments separately, beginning with those that presented availability information prior to or alongside of the study material (Pre-cued availability). The second group of experiments (Post-cued availability) will be presented in the next section.

Pre-cued Availability

In one of the more naturalistic experiments conducted to date, Henkel (2014) led participants on a guided tour of a museum and asked them to take digital photographs of some objects and to only observe others. Not only did participants recall fewer *details* about the objects they photographed compared to objects they simply observed, but they also recalled fewer photographed objects overall. Interestingly, participants were never explicitly told that the photographed images would be saved or available for review. Instead, prior experience with digital cameras presumably led them to assume the pictures would be saved. Henkel suggested that externally storing images of objects on a camera obviated the need to fully encode them. Participants were told to pay attention to all the objects they viewed, and the act of photographing an object actually required more time in order to angle the camera and adjust the focus of the lens. It is possible that the act of taking the picture could have directed covert attention towards the camera itself, and away from the object being photographed. However, Henkel argues that the act of photographing the object enabled people to dismiss the object from memory, thereby relying on the camera to "remember" for them, following Sparrow et al.'s (2011) argument. Similar to Sparrow et al. (2011), cues regarding future availability – in this instance, the creation of a digital photograph – appeared to override explicit instructions to attend to the objects equally. Photographed objects may not have been given full internal attention, and therefore were not fully encoded (e.g., Craik, Govoni, Naveh-Benjamin, & Anderson, 1996; Mulligan, 1998).

A study by Macias, Yung, Hemmer, and Kidd (2015) built on the findings of Henkel (2014) and Sparrow et al. (2011) to specifically test the hypothesis that individuals continually track what information they expect to be available in the future, and that they preferentially allocate memory resources to information they expect to be unavailable through other means. The authors presented participants with two different types of facts (interspersed together in a random order) that they were instructed to memorize: "lookupable" and "nonlookupable" (interspersed together in a random order). Lookupable facts were sufficiently obscure general knowledge facts that could be easily looked up online (e.g., the value of the mathematical constant *e*). Nonlookupable facts were said to be composed by an elementary school class in Rochester, New York and were similar to the lookupable facts, but pertained to people on the more local scale (e.g., a student's locker code). This gave the impression that they were not the type of facts one could easily look up online. Facts were not labeled as lookupable or nonlookupable for participants; they had to infer this information accordingly. Macias et al. found enhanced recall for nonlookupable facts compared to lookupable facts, providing evidence that human learners strategically encode information that they expect to be unavailable in the future.

Similarly to Henkel (2014) and Sparrow et al. (2011), participants in Macias et al.'s (2015) experiment were never explicitly instructed about the availability of the information, but they still appeared to implicitly sort facts based on their availability, and thus remembered them accordingly. The authors interpreted their findings as a strategy for reducing cognitive load (which is also the primary motive behind cognitive offloading), and likened their findings to Henkel's, suggesting that memory efficiently favored the encoding of information that was not readily available via another external source (i.e., a photograph).

Mueller and Oppenheimer (2014) produced another example of memory discrepancies that likely occurred as a result of offloading. In their study, participants viewed lectures and were instructed to either take notes on a laptop computer or take longhand notes on paper. Laptop note takers took significantly longer notes, and these notes contained a greater amount of verbatim overlap with the lecture material compared to longhand note takers' notes. Even when both groups were given an opportunity to study their notes, the longhand note takers consistently outperformed laptop note takers on subsequent tests of the material. Mueller and Oppenheimer attributed this difference to the fact that longhand note takers were synthesizing and summarizing content rather than transcribing lectures verbatim, which can serve as a desirable difficulty toward improved educational outcomes (e.g., Diemand-Yauman, Oppenheimer, & Vaughan, 2011; Richland et al., 2005). Their results seem to suggest that laptop note takers were focused on attempting to offload as much of the information as possible to their computers, whereas longhand note takers were focused on synthesizing and compactly summarizing the information. The process of actively offloading seemed to result in little memory benefit for the information itself, even when participants were explicitly given the opportunity to review their offloaded notes.

Post-cued Availability

Storm and Stone (2015) took a slightly different approach towards examining the effects of offloading. They tested the idea that saving information in one computer file that was expected to be available in the future would free up memory for information presented in a second computer file. The authors had participants study an initial list of words contained in a PDF file. They were then told to either save the file to the computer's hard drive, or simply close it without saving. Immediately after closing the first file, participants were presented with a second PDF file containing a new list of words to study. The authors found that participants recalled significantly more information from the second file if they had saved the first file than if they had not saved the first file. They also found that this effect only occurred if the saving process was deemed reliable. In other words, if the computer could not be trusted to store the information, participants did not exhibit any memory differences. Notably, memory for the first list could not be assessed, because participants were actually allowed to re-study the lists that they had saved, thus invalidating any pure attempt to measure memory.

Eskritt, Lee, and Donald (2001) and Eskritt and Ma (2014) conducted two similar studies to examine how participants used note-taking to aid their memory during a card game called Concentration. This game requires participants to remember both the identity and location of various pairs of cards to be successful. The authors allowed one group to study the cards before the game began, and told the other group that they could take notes on the cards to help them later on when they played the game. Before the game, the experimenters unexpectedly took away the note-takers' notes. They found that the note takers performed significantly worse at the game than the group who was allowed to observe the cards, and that their performance was actually similar to participants who never had an opportunity to see the cards before the game. An examination of the notes showed that they contained detailed information regarding both the identity and location of the various cards, indicating that the note-taking group clearly attended to and processed the information on the cards. These results suggest that the note takers were taking advantage of an opportunity to offload their memory for the cards in a similar manner as the laptop note takers in Mueller and Oppenheimer's (2014) study. The fact that the process of actively offloading, despite containing clear evidence of thorough information processing, demonstrated little benefit to memory also parallels Mueller and Oppenheimer.

These two experiments highlight findings similar to those discussed in the previous Pre-cue Availability section, as they both provide evidence of fluctuations in memory in response to offloading information to external resources. However, these procedures manipulated participants' impressions about the future availability of materials *after* their initial exposure – a procedure that is analogous to what is commonly done in directed forgetting experiments, which we will discuss next.

To this point, we have focused on higher-level theoretical explanations that characterize the relationship between internal memory and offloading to external (digital) storage, while remaining relatively silent with respect to the specific mechanism(s) that could be producing these effects. Next, we will explore the literature on directed forgetting and discuss the specific cognitive mechanisms that could be regulating memory based on digital availability.

Directed forgetting

Directed forgetting refers to the phenomenon that when given an instruction to forget some subset of information that we have studied, our memory for that information is impaired (Basden & Basden, 1998). Directed forgetting often refers to an experimental paradigm or implies a situation involving a direct instruction to forget, whereas *intentional forgetting* refers to motivated forgetting more broadly and involves the deliberate elimination or suppression of certain information that was once processed for potential future retrieval. There is a longstanding history of work on intentional forgetting, with researchers suggesting that these strategies are widely employed in everyday life to filter out or inhibit unnecessary information from long-term and short-term memory (Bjork, 1972).

Traditionally, two different types of directed forgetting paradigms have been used to study intentional forgetting (e.g., Block, 1971; MacLeod, 1975; Sahakyan & Foster, 2009). In the *item method*, participants are presented with a single stimulus and then given a cue instructing them to remember or forget that stimulus after each item is presented. In the *list method*, instructions to remember or forget are given after the presentation of an entire list of items (either sequentially or as a self-contained list). In between a first list and a second list, participants are either told to remember or forget the first list before viewing a second list. For both methods, participants are then tested on their memory for all of the stimuli they encountered. An important component these procedures all share is that study information is presented *before* any instruction to remember or forget. Thus, all items start off equally salient at presentation. Upon instruction to remember or forget, control to enhance or reduce memory can be targeted at a specific subset of stimuli depending on the given cue and the design of the study.

Mechanisms of directed forgetting

The precise mechanisms underlying directed forgetting have been heavily debated since the field's inception in the 1960's (for a review, see Golding & MacLeod, 2013). Yet, there has been general agreement that differences in timing between the item and list method of directed forgetting could support different mechanisms of action for the two paradigms. The rationale behind this distinction is that items presented in the *list method* are already encoded into long term memory prior to the forget instruction at the end of the list, whereas more timely instruction to forget in the *item method* can prevent or influence initial encoding of that information into long term memory (Basden, 1996; Basden, Basden, & Gargano, 1993; Macleod, 1999). The underlying mechanisms thought to produce forgetting effects under these distinct circumstances have fallen in and out of favor over the years with remarkable frequency as the research has progressed. For the purposes of our review and subsequent research, we will divide these mechanisms into two groups: *encoding processes* that exert their influence on the encoding phase, and *post-encoding processes* that exert their influence on memory after encoding or at the time of retrieval.

Encoding processes

Selective Rehearsal

The most prominent and perhaps most intuitive mechanism thought to support directed forgetting is *selective rehearsal*. As its name entails, selective rehearsal refers to an individual's ability to differentially rehearse a subset of items, typically showing a preference for remember items while forget items fall out of favor (Bjork, 1972). Selective rehearsal is predominantly associated with the item method of directed forgetting. If an item has not already been committed to long term memory (as is likely the case during the item method procedure), then instruction to forget can prevent encoding by manipulating selective rehearsal to produce greater recall of to-be-remembered items than to-be-forgotten items. This process essentially produces an interference effect for to-be-forgotten information (e.g., Wilson & Kipp, 1998; Zacks, Radvansky, & Hasher, 1996), such that forgotten items fall victim to passive decay. To-be-remembered information benefits from increased rehearsal during encoding. Selective rehearsal is less commonly associated with the list method paradigm.

Attention

We begin by acknowledging that attention is decidedly *not* a mechanism typically discussed in the directed forgetting literature. Rather, typical directed forgetting procedures, which provide remember/forget cues after the viewing of certain information, actually preclude attention from systematically varying across items. This is because all stimuli are equally salient at the time of presentation in a directed forgetting task – remember/forget cues are not presented until *after* the items are viewed. There is no theoretical reason to assume attention would vary. However, we believe that attention could play a role in offloading and the digital availability-related memory effects (described in the previous section), so we will discuss it here. We will consider attention as an antecedent to selective rehearsal – an executive process that could steer cognitive resources in a certain direction prior to or during presentation of an item. Attention could play a part in determining what information makes it into working memory, but any processing or maintenance occurring after this point should be considered a selective rehearsal process.

It is well documented that reduced attention during encoding results in limited information processing, and thus poor subsequent memory (e.g., Craik et al., 1996; Jacoby, Woloshyn, & Kelley, 1989). Because information that is offloaded onto external memory stores is readily available in the future, it stands to reason that individuals pay less attention to offloaded information at the time of encoding. If one anticipates interacting with information they know is readily available elsewhere, this awareness could prompt a reduced attentional state. Alternatively, while individuals could devote a sufficient amount of overt attention to externally stored items (i.e., typing trivia statements into a computer; Sparrow et al., 2011), they may allocate different amounts of covert attention to items dependent on that item's future availability. In other words, individuals may direct less internal attention towards highly accessible information. This systematic variation in attention could lead to corresponding variation in depth of information processing at encoding, resulting in subsequent memory differences (Carrasco & McElree, 2001; Lu & Dosher, 1998).

Post-encoding processes

Retrieval inhibition (Active forgetting)

While the earliest accounts of directed forgetting were comfortable focusing solely on variations in encoding, researchers soon realized that certain directed forgetting findings could not be explained by encoding differences alone. Geiselman, Bjork, and Fishman (1983) noted that participants could influence their subsequent memories to a greater extent with a deliberate motivational set to forget than with a passive non-rehearsal or inattention strategy. *Retrieval inhibition* was introduced as the missing mechanism, suggesting that a cue to forget could initiate a process that inhibits or blocks access routes to the episodic memory traces that contain the forget items, making them less retrievable at the time of recall (Bjork, 1989; Geiselman et al., 1983). Generally, the account posits that an instruction to forget items in a directed forgetting paradigm causes them to become inhibited and less accessible, and ultimately more difficult to *retrieve* (e.g., Ullsperger, Mecklinger, & Müller, 2000), hence the "retrieval" in retrieval inhibition.

There is a certain level of ambiguity associated with this term, in part because the specific process by which it operates is not entirely understood. This ambiguity is reflected in different researchers' understandings of the term. For example, Bjork (1989) importantly noted that retrieval inhibition in the sense that he used it was meant to imply memory *suppression* – directly inhibiting or suppressing the target response in memory (as opposed to more neuroscientific interpretations of inhibition that have the consequence of strengthening or activating alternative responses). Alternatively, Bjork, Bjork, and Anderson (1998) emphasized that they used the term to refer, collectively, to the set of possible mechanisms that result in loss of retrieval *access* to inhibited items, without a commensurate loss in *availability* of those items in memory. The latter interpretation better accommodates the common finding that list-method directed forgetting is observed on free recall tests, but not recognition. The act of re-presenting an item on a recognition test is said to lift or release the inhibition for that item in the case of the list method (Geiselman et al., 1983; MacLeod, 1998).

As can be inferred from above, the list method of directed-forgetting is most frequently associated with an inhibition account. If an item has already been committed to memory, it cannot be readily expunged, so the forget instruction likely causes that item to be inhibited or suppressed prior to subsequent retrieval attempts (e.g., Basden & Basden, 1998; Bjork, 1989;

Geiselman, Bjork, & Fishman, 1983). A similar intentional inhibition process is thought to occur when a think/no-think paradigm is used, where participants must actively suppress previously encoded information (e.g., Anderson, 2003; Anderson, 2001). Importantly, this account posits that all information presented during a directed forgetting paradigm may be *encoded* equally, but forget-cued items are actively suppressed/inhibited after initial encoding.

Moving forward, we will adopt an approach similar to Fawcett, Lawrence, and Taylor (2016) and will refer to any post-encoding forgetting processes collectively as “active forgetting”. It is not our intention to make any claims regarding the specific under workings of inhibition or effortful suppression – a debate that is ongoing in the literature (e.g., Noreen & MacLeod, 2013; Wang, Cao, Zhu, Cai, & Wu, 2015). Therefore, *active forgetting* will encompass any retrieval inhibition *or* suppression-like process that acts on a memory representation after encoding to produce deficits in memory. Note that we may still use the term retrieval inhibition when referring to the literature.

Traditionally, item method directed forgetting was typically attributed to selective rehearsal, whereas list method directed forgetting was attributed to retrieval inhibition (e.g., Basden et al., 1993; Basden & Basden, 1998; Johnson, 1994; MacLeod, 1999). More recently, however, there has been a swell of support for retrieval inhibition in item method directed forgetting (Anderson, 2001; Wylie et al., 2008). For example, Ullsperger, Mecklinger, and Müller (2000) conducted an item method directed forgetting study using event-related brain potentials (ERPs) and found that forget-cued items were associated with a late right-frontal positive slow wave, suggesting that forget items were associated with a *greater* amount of post-retrieval processing, most likely indicative of retrieval inhibition. They concluded that differential encoding alone could not account for directed forgetting. Conway and Fthenaki

(2003) conducted an item method directed forgetting study using patients with various frontal and temporal lobe lesions. They found that patients with frontal lobe lesions (the area associated with inhibitory control) did not exhibit a directed forgetting effect, whereas controls as well as patients with temporal lesions did exhibit the effect, suggesting a disruption of inhibitory control of memory for those patients. These studies and others have presented significant challenges to the selective rehearsal account of item method directed forgetting.

Before moving on, we should briefly acknowledge an alternative account of list method directed forgetting that has drawn support, referred to as the contextual change hypothesis (Delaney & Sahakyan, 2007; Sahakyan & Kelley, 2002). Under this account, list method directed forgetting effects are said to be caused by a change in internal context between Lists 1 and 2 that facilitates recall for remember-cued lists. This account arguably explains the empirical data in the field as well as any other account, but we will exclude it from further discussion because: (a) it is not particularly applicable/transferable to our primary goal of identifying the mechanisms underlying availability-related memory effects, (b) it applies solely to list method directed forgetting, and (c) it is non-descript with respect to its underlying mechanisms of action.

Applications to cognitive offloading

Borrowing from the literature on directed forgetting, we will formulate a few logical predictions regarding the mechanisms that could be producing reduced memory for offloaded or digitally available information. It is important to note that the mechanisms discussed below need not assert their effects on memory in a mutually exclusive manner, especially in complex everyday situations. As discussed in the previous section, these predictions are dependent on prior knowledge regarding the future availability of information.

If an individual has prior knowledge at the time of encoding that some piece of information will be available elsewhere (i.e., on a computer, phone, notepad, the Internet), either of two encoding mechanisms may be at play. For one, awareness that information is offloaded elsewhere could result in reduced *attention* to the material, preventing full encoding before it occurs. This route might occur when individuals encounter information that they immediately know is readily available externally (e.g., lookupable online, Macias et al., 2015), such as a college lecture where the professor always provides the class slides. Alternatively, an individual might devote sufficient attention to some given material, but fail to *selectively rehearse* the externally stored information, leading to impoverished encoding of the offloaded items. This mechanism could act when individuals save information externally on an item-by-item basis (e.g., Sparrow et al., 2011), such as adding items to a grocery list on one's smartphone, or uploading pictures to a computer or cloud storage. At the extreme, because interacting with information on a device or the Internet should coincide with the assumption that it will be saved and available in the future, perhaps we trigger a reduced attentional state and/or a minimal propensity for rehearsal that weakens encoding for all information experienced through this source.

The competing hypotheses to these predictions would involve *active forgetting*. Active forgetting could work to inhibit or suppress memory for externally saved information after it is encoded. At first pass, this hypothesis may seem less appealing, but if the comparison between availability effects and directed forgetting is valid, then the recent ascension of the retrieval inhibition hypothesis would provide considerable support for this account. Further, active forgetting can adequately explain most of the examples given for the encoding processes above. Adding items to a grocery list or uploading pictures to a computer could selectively inhibit or suppress these items in memory, freeing up processing capacity for other unsaved items. Considering the college lecture example, students may take notes to serve as an external memory aid (e.g., Mueller & Oppenheimer, 2014). Once these notes are externalized, active forgetting processes could work to eliminate that information from memory, making room for new information.

Contrary to directed forgetting paradigms, these situations do not include any instruction to remember or forget (nor do the offloading experiments reviewed above). Nevertheless, studies have found evidence for intentional forgetting without explicit instruction. For example, Shapiro, Lindsey, and Krishnan (2006) used advertisements to subtly suggest that participants should forget certain product attributes. Eskritt et al. (2001) and Eskritt and Ma (2014) found a pattern of results similar to those found in intentional-forgetting studies by allowing participants to take notes while playing a memory game, despite never being instructed to forget. Future availability could act as an index, or cue, to either remember or forget.

The Current Research

The goal of the ensuing experiments is to understand the cognitive processes that cause poor memory for information that we assume will be readily available in the future through our devices (i.e., easily found online or stored on a computer). A recent wave of research investigating digital availability-related memory effects has prompted some researchers to draw comparisons between these findings and traditional directed forgetting effects (Sparrow et al., 2011; Storm & Stone, 2015). To date, this comparison has not been systematically evaluated, nor has there been any investigation targeted at identifying the cognitive mechanisms underlying these effects. Some groups have been hasty to attribute their findings to encoding differences, despite a lack of evidence to support this claim (Macias et al., 2015; Sparrow et al., 2011). These assumptions are not unreasonable, but a review of the directed forgetting literature should serve as a warning that the most intuitive processes are not always the most likely.

We will refer to a distinction in our experiments that reflects two different types of availability. *Explicit availability* will be used to describe situations where participants are given an explicit cue regarding an item's future availability – often saved vs. erased (e.g., Sparrow et al., 2011; Storm & Stone, 2015). Explicit availability in the experimental context is completely arbitrary and can be randomly assigned to various stimuli. *Perceived availability* will be used to describe situations where an item's availability is inherently set – often lookupable vs. nonlookupable (e.g. Macias et al., 2015). Perceived availability is a naturalistic trait of each stimulus and cannot be randomly assigned; it is up to the participants to discern an item's perceived availability based on their own experiences. Both types of availability will be manipulated in the following experiments, often times within the same experiment.

A core strategy of the present research program will be to leverage the wealth of literature on directed forgetting to better understand what connection, if any, exists between this body of work and recent digital availability findings. The benefits of the directed forgetting approach are two-fold: (a) The mechanisms underlying directed forgetting have been clearly identified. If we can obtain digital availability findings that are comparable to traditional directed forgetting findings, that will allow us to make inferences regarding the mechanisms underlying digital forgetting, and (b) directed forgetting can be extended to an entirely new domain, fostering new opportunities to examine the flexibility of the mechanisms underlying directed forgetting. This research promises to make contributions that will help inform theory and ongoing debate within the directing forgetting literature.

Experiment 1a: Directed forgetting extension

Experiment 1a utilized a modified item-method directed forgetting (DF) paradigm to test the effectiveness of different digital availability cues on subsequent memory. Sparrow et al. (2011) conducted an experiment where participants viewed and typed trivia statements into a computer. They were told after each statement that their typed entry had either been erased, saved, or saved into a specific folder, and found better memory for erased statements compared to saved statements, indicative of an explicit digital availability effect. Similarly, Macias et al. (2015) showed participants trivia statements that were either inherently available (lookupable online) or inherently unavailable (nonlookupable facts pertaining to a local elementary school in Rochester, NY), and they reported better memory for nonlookupable statements, indicative of a perceived digital availability effect. Sparrow et al. gave their participants explicit availability cues by telling them that statements were either saved or erased, whereas Macias et al. required their participants to determine each item's inherent perceived availability on their own. Experiment 1a will incorporate aspects of both of these studies to test how these different availability variables might affect subsequent memory for items.

Method

Participants

One-hundred participants (33 female, M_{age} = 33.6, *range* = 18-59, *SD* = 9.2) were recruited from Amazon Mechanical Turk. Sample size was chosen to correspond to the study we were attempting to replicate that used online workers (Macias et al., 2015), and remained consistent across all subsequent single-condition experiments. Participants were required to have IP addresses in the United States and were paid $2.10 for completion of the study. Eight participants were excluded from analysis: two for providing incomplete data and six for exceedingly poor performance (<10% accuracy). We chose a consistent exclusion criteria of

<10% accuracy for all experiments. Participants performing at this level correctly answered 3 or fewer out of the 40 total responses, which more than likely indicates a lack of effort on the task. Data collection stopped once the target N was reached in all experiments, and no participants were recruited to replace excluded subjects.

Materials

The current experiment and all following experiments were administered through Qualtrics as a link embedded within Amazon Mechanical Turk. The experiment consisted of a study phase and a test phase, followed by a short questionnaire (See Appendix A) to collect demographic information in addition to participants' impressions about the experiment. Stimuli were one-sentence trivia facts borrowed from Macias et al. (2015) that were composed of two distinct types: those that can be easily accessed on the Internet (*Lookupable* (L), e.g. the value of the mathematical constant *e*) and those that cannot (*Nonlookupable* (NL), e.g. a locker code; see Appendix B for the full list of stimuli). The L facts were intentionally obscure, so as to minimize the likelihood that participants would know them prior to the experiment. As an additional precaution, participants were asked after the experiment if they knew any of the facts in advance, and those trials were omitted from further analysis (0.04% of responses). There were 100 total L facts, each corresponding to an NL fact that was similar in terms of form, structure, and complexity, for a total of 200 stimuli. As a cover story, participants were told that the facts were compiled by an elementary school class in Rochester, NY. The NL facts contained similar types of information as the L facts, but pertained to people on the local scale of the elementary school, thus giving the impression that they were not the sort of facts one could find online.

Procedure

Participants were instructed to complete the task in a quiet environment and to close all other Internet tabs/browsers in order to minimize distractions. They were given 1 hour to complete the task, though most finished in less than 30 min. The experiment consisted of a 2 [lookupability: L, NL] x 2 [cue type: Saved/Erased] within-subjects design. In the study phase, participants viewed 40 facts (20 L and 20 NL, randomly interspersed) one at a time, and were told not to take any notes or utilize any other information resources. Twenty-five versions of the experiment were created that randomly sampled 40 out of the total 200 facts, so that each fact was equally represented across all versions.

Each trial began with a fixation cross on the screen for 1.5 s, followed by a fact displayed for 8 s. A randomized cue instruction stating either "*SAVED*" (S) or "*ERASED*" (E) appeared immediately after each fact and remained on the screen for 3 s before a new trial began. Cue type was counterbalanced across participants such that each fact was followed by an equal number of S and E cue types. Timing was chosen to be consistent with a traditional item-method directed forgetting experiment. Participants were told that the facts designated as SAVED would be compiled on a list, and that they would be able to review this list for as long as they like before completing a fill-in-the-blank memory test. In between the study and test phases, participants were reminded about all the instructions and were forced to wait 90 seconds before proceeding to the test phase. No participants were given an opportunity to review the S items. For the cued recall test, participants viewed each fact from the study phase in a random order, with a key word from each fact blanked out. The test phase was untimed, and participants were instructed to type the missing word into a text entry box.

Prior to the study phase, each participant completed a single practice trial with a follow-up memory question (in accordance with Macias et al., 2015) geared towards familiarizing them with the procedure. All participants saw the same NL fact, which did not appear in the experiment, followed by the “ERASED” cue - no participants practiced a “SAVED” trial. The author scored all responses blind to condition.

Results

For analysis, a mixed-effects regression model was fit to predict accuracy on the memory test from the within-subject variables fact type (L, NL), cue type (S, E), and their interaction. To ensure that any differences in group means were not influenced by differences in individual items or subjects, we included items and subjects as random effects (Baayen, Davidson, & Bates, 2008). Trials where participants indicated prior knowledge were omitted from analysis (.04% of responses).

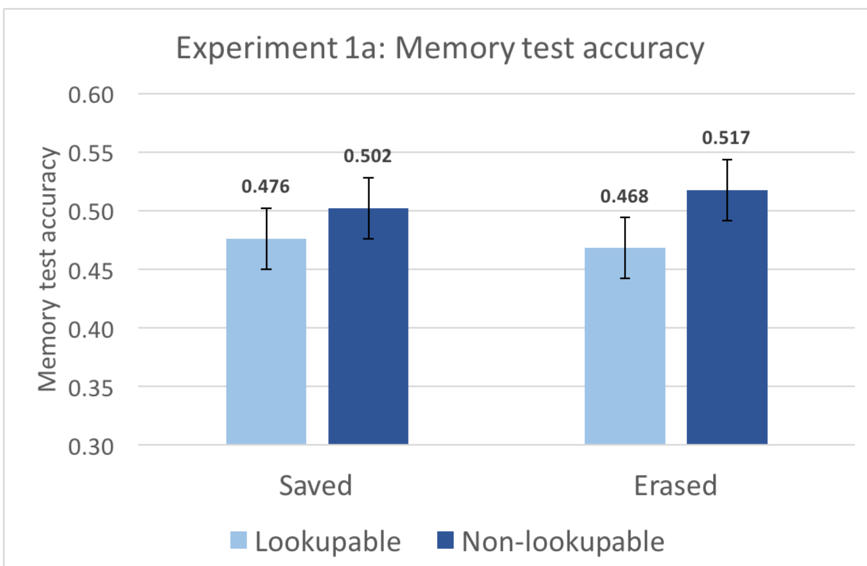

Figure 1. Experiment 1a memory test accuracy by fact type (Lookupable, Nonlookupable) and cue type (Saved, Erased), shown as proportion correct. Error bars depict standard error.

Memory test group means for all four conditions are shown in Figure 1 as proportion correct. The mixed effects regression revealed a significant main effect of lookupability (β = 0.049, $t(3568) = 2.22$, $p = .02$), such that memory was better for NL facts (M = 51.0%, SD =

.500) than L facts (*M* = 47.2%, *SD* = .499). There was no main effect of Saved vs. Erased cue type (*M*(S) = 48.9%; *M*(E) = 49.3%), as well as no interaction effect.

A follow-up manipulation check question asked participants whether or not they actually believed they would be able to study the saved items prior to the memory test. This question resulted in 66 out of 92 participants (72%) reporting that they did indeed believe the instructions, 2 out of 92 (2%) reporting they did *not* believe them, and 24 out of 92 (26%) indicating that they were not sure whether or not they would be able to re-study the saved items. Due to a hidden data coding error in Qualtrics, we initially incorrectly believed these codes were reversed and that 72% of participants reported that they did *not* believe the instructions, while only 2% reported that they did believe the manipulation. We make note of this error because it influenced our interpretation of the current results, as well as the design of Experiment 1b.

The same mixed effects regression was run on the subset of 66 participants who claimed to have believed the saved/erased manipulation, and revealed an identical pattern of results ($M_{L,E}$ = 47.9%, $M_{L,S}$ = 50.0%, $M_{NL,E}$ = 52.1%, $M_{NL,S}$ = 53.5%). This finding indicates that our results were not skewed by “disbelievers”.

Discussion

The first experiment used a modified item-method directed forgetting paradigm and succeeded in replicating the lookupability effect obtained by Macias et al. (2015). This result reinforces their claim that people tend to assess how easily they could find a certain piece of information, and preferentially remember items that will be more difficult or impossible to access in the future. Though significant, the difference in memory test accuracy in our experiment (M_{NL} = 51.0%, M_{L} = 47.2%) was about half that reported by Macias et al. (2015; M_{NL}

= 52.8%, M_L = 44.6%). This reduction could in part be due to the addition of a saved/erased post-cue in our experiment.

Interestingly, the addition of the saved/erased post-cue had no bearing on participants' subsequent memory for the trivia facts. This manipulation introduced an explicit availability cue that we believed participants would be more likely to use to guide memory, compared to an item's inherent lookupability. Our results stand in contrast to Sparrow et al.'s (2011) Experiment 3, which reported superior memory for trivia facts that participants believed would be erased compared to those they believed would be saved. Due to the response coding error mentioned in the previous section, an early pass of the data led us to believe that the saved/erased manipulation might not have been effective. Two participants offered further comments that lent credence to this idea. One stated simply that "psychology is deceiving," while another stated "I don't believe anything [Mechanical Turk] requesters say." It could be, due to the sheer volume of studies that these workers partake in (all workers had completed at least 1000 studies), that they had developed a heightened sense of deception. Compared to the undergraduate subject pool participants who participated in Sparrow et al.'s (2011) experiment, this could be a logical conclusion. Our findings led us to doubt the believability of the saved/erased manipulation in Experiment 1a. Perhaps the experienced online workers anticipated being tricked – they were given no evidence that saved items would actually be available prior to the memory test. This circumstance may have nudged participants towards favoring memory for nonlookupable items over lookupable items, and ignoring the saved/erased instruction.

Experiment 1b: Directed forgetting extension, with practice

Experiment 1b aimed to remedy what we initially believed to be a failed saved/erased manipulation in Experiment 1a. Whereas participants only completed a single practice trial in the first experiment, Experiment 1b introduced a more thorough practice block geared towards enhancing the believability of the saved/erased manipulation. In Experiment 1b, participants were actually given an opportunity to review the saved items prior to taking a miniature memory test during the practice block, so that they might be more inclined to believe this manipulation in the actual experiment. The instructions were also strengthened in order to make the saved/erased instruction more salient.

Method

Participants

One-hundred participants (50 female, M_{age} = 36.4, *range* = 21-71, *SD* = 11.4) were recruited from Amazon Mechanical Turk. Participants were required to have IP addresses in the United States and were paid $2.10 for completion of the study. Two participants were excluded from analysis for exceedingly poor performance (<10% accuracy).

Materials and Procedure

The experimental procedure was identical to Experiment 1a, with the exception of enhanced instructions and the inclusion of a longer practice block. The 1-trial practice block in Experiment 1a was intended to simply familiarize participants with the procedure, whereas Experiment 1b included an 8-trial practice block intended to increase the experiment's believability. Participants viewed 4 erased trials and 4 saved trials, and were then given an opportunity to review only the 4 facts that were followed by the "*SAVED*" cue. The review phase was untimed. Participants then completed a fill-in-the-blank memory test for all 8 facts they viewed. Additionally, the instructions were modified to further emphasize the point that participants would have an opportunity to review the saved facts before the final test, and emphasized that the practice was identical to the real experiment with the exception of length. The author scored all responses blind to condition.

Results

Experiment 1b was analyzed just as Experiment 1a, using the same mixed effects regression model to predict accuracy on the memory test from fact type (L, NL), cue type (S, E), and their interaction. Trials where participants indicated prior knowledge were omitted from analysis (.09% of responses). There was once again a main effect of lookupability ($\beta = 0.046$, $t(3784) = 2.22$, $p = .03$), such that memory was better for NL statements ($M = 53.7\%$, $SD = .499$) than L statements ($M = 48.8\%.$, $SD = .500$). There was no main effect of Saved vs. Erased cue type ($M(S) = 51.0\%$; $M(E) = 51.5\%$), as well as no interaction effect. Memory test group mean accuracies for all four conditions are shown in Figure 2 as proportion correct.

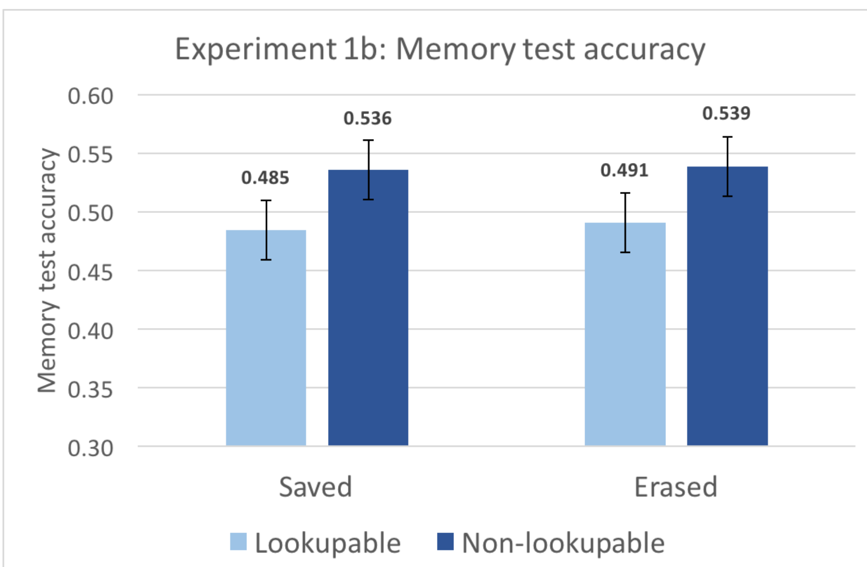

Figure 2. Experiment 1b memory test accuracy by fact type (Lookupable, Nonlookupable) and cue type (Saved, Erased), shown as proportion correct. Error bars depict standard error.

After examining the same follow-up manipulation check question included in Experiment 1a, we discovered that 85 out of 98 participants (87%) reported that they did believe the SE manipulation, whereas 0 participants did *not* believe the manipulation, and 13 (13%) were not sure. A mixed effects model run only on the subset of 85 participants who reported they believed manipulation revealed a pattern of results identical to the full analysis, indicating that the data were not skewed by “disbelievers”.

Discussion

Results from Experiment 1b were nearly identical to Experiment 1a, down to the size of each effect. Participants had better memory for nonlookupable items compared to lookupable, and while the inclusion of a robust practice block slightly increased the reported believability in the manipulation (from 72% to 87%), this change had no effect on memory for saved vs. erased items.

Based on the initially misinterpreted manipulation check questions, we were not certain if the SE manipulation failed, or if participants had simply disregarded the SE cue instead favoring an item’s inherent lookupability to guide their memory. Because only 2% of participants in Experiment 1a and 0% of participants in Experiment 1b reported that they did *not* believe the manipulation, along with confirmatory analyses run only on the subset of participants who indicated they *did* believe the manipulation, we are now convinced that this manipulation succeeded in that participants thought they would indeed be able to review the saved trivia facts before the memory test. Why might we have failed to find even the slightest effect of the SE manipulation in both Exp. 1a and 1b, despite other researchers presenting evidence for similar effects?

The SE manipulation implemented in Exp. 1a and 1b was somewhat weaker than previous manipulations performed by other researchers, by virtue of being molded after traditional item-method directed forgetting experiments. In these experiments, participants passively view stimuli and are then told to either remember or forget each item. Sparrow et al.'s (2011) second experiment manipulated the SE variable between-subjects, making it less comparable to our study where SE was manipulated within-subjects. However, they did find a significant SE effect using a within-subjects design in their third experiment (albeit a weaker effect than in Experiment 2). In both studies, Sparrow et al. had participants copy each trivia statement into a computer, and then press the spacebar to *actively* save or erase their entries, whereas our paradigm passively told participants whether statements would be saved or erased. On a subsequent recognition test, participants displayed the best memory for statements they believed had been erased (M = .93) compared to those they thought they would have access to (saved generically, M = .88; saved into a folder, M = .85). It should be noted that the reported means are close to ceiling, suggesting that Sparrow et al.'s recognition task may have been easier than our cued recall task (Experiment 1b overall M = .506). It could be that the SE manipulation is more sensitive to recognition than recall, though this idea runs contrary to the directed forgetting literature which consistently shows stronger forgetting effects indexed by recall tasks relative to recognition tasks (for a review, see MacLeod, 1989).

The act of typing out the trivia statements in Sparrow et al.'s (2011) experiments may have boosted participants' memory for the stimuli. This procedure also required the study phase of their experiment to be untimed. A close examination of the details of their procedure suggests that participants viewed whether a fact would be saved or erased *while* they typed that fact into a text box. The act of pressing the space bar appears to have solidified the saved/erased action, and moved the participant on to the next trial. This choice of methodology means that Sparrow et al. had less experimental control over how much time participants spent on each item, and they may have spent more time encoding E items relative to S items.

In summary, the major differences between Sparrow et al.'s (2011) Experiment 3 and our Experiments 1a & 1b are: (1) Sparrow et al. conducted an in-person laboratory study; our studies utilized online (mTurk) participants; (2) Sparrow et al. had participants actively type out each statement; our studies had participants passively view statements (per traditional directed forgetting paradigms); (3) Sparrow et al. showed participants both the stimulus and the cue (saved/erased) at the same time; our studies showed the stimulus prior to the cue; and (4) Sparrow et al.'s study phase was untimed; our studies used consistent timing for both stimulus and cue presentation.

It is possible that any of these differences contributed to our failure to find a SE effect. However, given our findings, we believed the most pertinent question was to determine whether the conditions that elicited null saved/erased effects in our first two experiments were sufficient to produce a traditional directed forgetting effect. It may be that experienced, online workers are warier of deception and less likely to buy into the experimental instructions. It could also be that either our stimuli (trivia facts) or the alternative cues (saved/erased) were not conducive to producing a directed forgetting effect.

Experiment 2: Traditional directed forgetting with trivia stimuli

Experiment 2 was designed to shed light on the null SE findings from Experiment 1a and 1b (henceforth referred to collectively as Experiment 1). The goal of Experiment 2 was to utilize a nearly identical procedure to test whether or not the same conditions could produce directed forgetting. First, Experiment 1 made the assumption that a directed forgetting effect could be obtained using more naturalistic trivia statements, based on both the robustness of the effect (e.g., MacLeod, 1989) as well as limited experiments in the literature that used sentence-length stimuli to obtain directed forgetting effects (Geiselman, 1974). Yet, there could be something unique about trivia statements that prevents normal directed forgetting. Second, if mTurk participants failed to believe the SE manipulation in Experiment 1, there is reason to believe that they would be similarly skeptical of a more traditional Remember/Forget (RF) manipulation. Beyond a potential heightened sense for deception, it could be that mTurk workers were simply less motivated to follow the SE instruction than in-person laboratory subjects. A literature search turned up only one published experiment that demonstrated a directed forgetting effect using mTurk workers, and the authors only reported forgetting from a select portion of their experiment (Chiu & Egner, 2015).

Failure to find a directed forgetting effect in Experiment 2 would either suggest a problem with our chosen stimuli (sentence-length trivia statements), or a difference in the behavior of online workers that precludes them from exhibiting directed forgetting. Alternatively, confirmation of a directed forgetting effect in Experiment 2 would strongly imply that the saved/erased manipulation from Experiment 1 was sufficiently believable, and that the null results were indeed valid.

Method

Participants

One-hundred participants (54 female, M_{age} = 36.5, *range* = 19-70, *SD* = 11.5) were recruited from Amazon Mechanical Turk. Participants were required to have IP addresses in the

United States and were paid $2.10 for completion of the study. Two participants were excluded from analysis for exceedingly poor performance (<10% accuracy).

Materials and Procedure

Experiment 2 was identical to Experiment 1 with the exception of different post-cues (Remember(R)/Forget(F)), as well as different instructions supporting this change. Participants were told they would view trivia facts followed by either a "*REMEMBER*" or "*FORGET*" cue, and that they would only be tested on their memory for the R facts. Participants then viewed an 8-trial practice block analogous to Experiment 1b that consisted of 4 R and 4 F study phase trials, and then completed a practice test where they were only tested on the 4 R trials. After the practice block, participants completed the study phase of the experiment with the same stimuli and timing parameters that were used in Experiment 1. When they reached the test block, they were tested on all 40 trivia statements that they viewed, just as was done in Experiment 1. The author scored all responses blind to condition.

Results

Results from the post-experiment manipulation check question closely mirrored those from Experiment 1a and 1b: only 3 out of 98 participants (3%) reported that they did *not* believe the Remember/Forget manipulation, whereas 75 out of 98 (77%) reported they did believe the manipulation, and 20 (20%) reported they were not sure. These results are in-line with Experiment 1a (72% believability) and 1b (88% believability).

Trials where participants indicated prior knowledge were omitted from analysis (.06% of responses). Unlike Experiment 1, a mixed-effects regression model predicting memory accuracy from fact type (L, NL), cue type (R, F), and their interaction did reveal a significant main effect of cue type ($\beta = 0.084$, $t(3795) = 3.97$, $p < .0001$), such that memory was better for R facts ($M =$

51.5%, *SD* = .499) than F facts (*M* = 40.6%*, SD* = .491). In other words, there was a significant directed forgetting effect. There was no main effect of L vs. NL stimuli (*M*(L) = 45.5%; *M*(NL) = 46.5%). These results were qualified by a marginally significant interaction (β = 0.051, $t(3795) = 1.70$, $p = .09$) suggesting that a marginal lookupability effect (NL > L) existed for the R stimuli ($M_{NL\text{-}L}$ = 3.6%), but not the F stimuli ($M_{NL\text{-}L}$ = -1.5%). However, post-hoc Tukey multiple comparison tests revealed no significant difference between Nonlookupable, Remember items (*M* = 53.3%) and Lookupable, Remember items (*M* = 49.7%; $p > .10$). Figure 3 displays the memory test group mean accuracies for all four conditions as proportion correct.

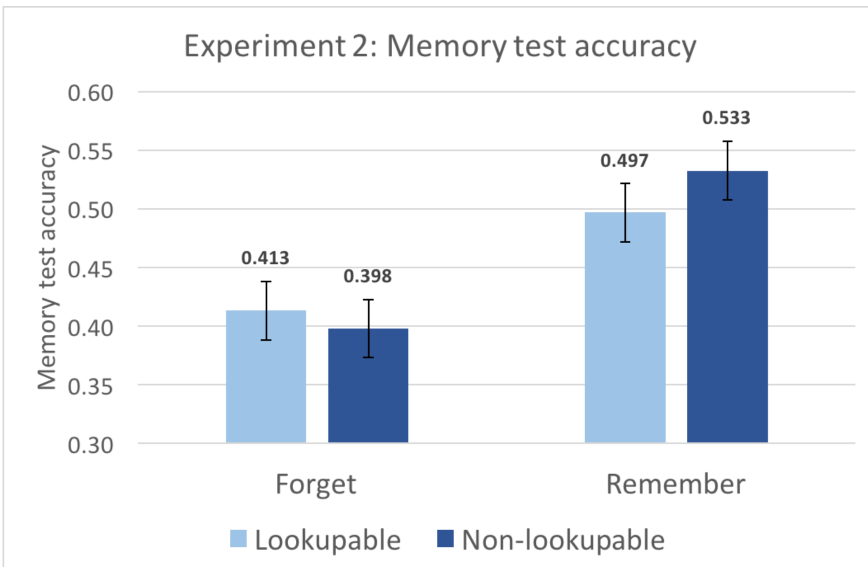

Figure 3. Experiment 2 memory test accuracy by fact type (Lookupable, Nonlookupable) and cue type (Forget, Remember), shown as proportion correct. Error bars depict standard error.

Discussion

The results from Experiment 2 convincingly demonstrate that it is possible to obtain a directed forgetting effect using online participants and the chosen stimuli. The significant main effect of the RF post-cue provides behavioral evidence (beyond the follow-up manipulation check question) that participants generally believed the instructions and manipulations for all three experiments. Further, it implies that the null saved/erased effects in Experiment 1 were valid. In other words, these results suggest that participants did not use the saved/erased information to tailor their memories in any meaningful way, despite the saved/erased manipulation being sufficiently believable.

Contrary to Experiment 1, Experiment 2 found no main effect of perceived availability (lookupability). This suggests that participants largely tailored their memory for items based on the RF post-cue information, and disregarded any differences in perceived availability. However, a marginally significant interaction between the two variables did suggest that the data trended in the direction of displaying a perceived availability effect for the R items, but not the F items. This interaction would suggest that memory is better for NL items only when one is making a conscious effort to remember items in the first place.

Although we would like to believe that the perceived availability effects in Experiment 1a and 1b (a replication of Macias et al.'s (2015) original finding) are driven by an actual perceived disparity in the two types of facts' digital availability, the possibility exists that a confound or 3^{rd} variable is actually causing elevated memory for NL facts. We have controlled for the possibility of item confounds in our analysis by including items as a random effect in our regression models (i.e., allowing random intercepts for each item), but the null perceived availability effect in Experiment 2 provides the first behavioral evidence to ameliorate these concerns. It stands to reason that if there is something simply more memorable about the NL items compared to the L items, then this effect should persist uniformly across all other conditions. The fact that the F post-cue completely erased, if not reversed the perceived availability effect, suggests there is something about that specific condition that changed participants' memory strategies. We will return to this issue in Experiment 4.

Interim Discussion

To this point, Experiments 1a, 1b, and 2 have provided modest evidence that digital availability-related memory effects operate differently from item-method directed forgetting effects. In this section, we will (a) evaluate this evidence with an eye towards identifying the underlying cognitive mechanisms that could be producing the memory effects observed so far, and (b) establish testable hypotheses for the upcoming experiments.

According to the selective rehearsal account of directed forgetting, forgetting occurs in the item-method paradigm because participants drop F items from their rehearsal set upon receiving the F cue and thus fail to fully encode them, resulting in passive decay of those items (e.g., Basden & Basden, 1998). Alternatively, item method directed forgetting may involve an active forgetting process that engages executive control processes to suppress or inhibit to-be-forgotten items (e.g., Zacks et al., 1996; Ullsperger et al., 2000). A substantial body of research has surfaced in the past 10-15 years that challenges the once-dominant selective rehearsal account of directed forgetting in the item-method paradigm, and provides significant support for an active forgetting account. For example, research has shown that intentional forgetting depends on neural structures that are distinct not only from those involved in intentional remembering, but also those involved in *incidental* or “normal” forgetting (Wylie, Foxe, & Taylor, 2008). Importantly, selective rehearsal and active forgetting can be characterized as exerting their influences at different stages of memory. Selective rehearsal acts by selectively facilitating *encoding*, such that upon subsequent retrieval attempts, reactivation of an item in memory is more probable. Active forgetting acts by selectively inhibiting or suppressing an item *after* it has been encoded in memory (i.e. *post-encoding*), such that upon subsequent retrieval attempts, its reactivation is less probable. In the context of directed forgetting, selective rehearsal can be thought of as boosting memory for R items, whereas active forgetting can be thought of as suppressing memory for F items.

Because Experiments 1a, 1b, and 2 were so similar in their experimental procedure, we can compare the post-cue main effects across all three experiments to help us better understand how the SE and RF post-cues might have influenced memory differently. Figure 4 shows the post-cue condition means compared across Experiments 1a, 1b, and 2.

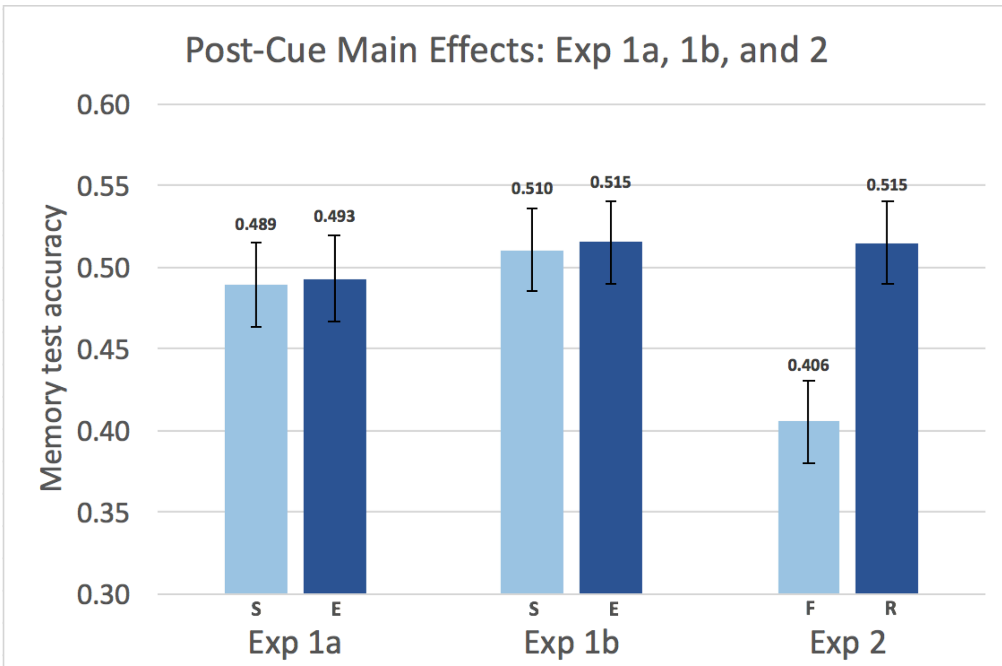

*Figure 4***.** A comparison of post-cue group means from Exp. 1a, 1b, & 2. Light blue bars indicate Saved trials for Exp. 1a & 1b, and the analogous Forget trials for Exp. 2. Dark blue bars indicate Erased trials for Exp. 1a & 1b, and the analogous Remember trials for Exp. 2. Error bars depict standard error.

Examining these means, we observe a relatively consistent average recall for all cue types across all experiments, with one exception: the F condition in Experiment 2. On average, the F cue was particularly effective in reducing recall - approximately 10% poorer (M = 40.6%) than any of the other S, E, or R post-cues (Avg. M = 50.4%). To verify these visual differences, an ANOVA including experiment number as a between-subjects variable and fact type and cue type as within-subjects variables revealed a significant experiment number by cue type interaction ($F(1, 7973) = 20.1$, $p < .0001$), indicating that the post-cues differentially affected memory accuracy across the three different experiments.

The substantial memory reduction induced by the F post-cue in Experiment 2 relative to all other cues can be interpreted in two ways. Participants in Experiment 2 did not believe they would be tested on the F items, so the first possibility is that they selectively rehearsed the R items and immediately dropped the F items from their rehearsal set. This interpretation follows the selective rehearsal account, where the F-related memory reduction is caused by poor encoding relative to all other conditions. The second possibility is that participants initially encoded all items equally well, but upon viewing the F cue engaged an active forgetting process that inhibited/suppressed their memory for these items that they believed would not be tested. This interpretation follows an active forgetting account, where the F post-cue memory reduction is caused by post-encoding inhibition. Because the F post-cue condition elicited a large *decrease* in memory relative to all Experiment 1 conditions, we hypothesize that post-encoding, active forgetting processes are primarily responsible for the directed forgetting effect seen in Experiment 2. If selective rehearsal was responsible for the directed forgetting effect, it is likely that we would see at least a marginal *increase* in memory for R items relative to all Experiment 1 conditions.

Additionally, the contrasting results from Experiment 1 compared to Experiment 2 lead us hypothesize that selective rehearsal is primarily responsible for the perceived availability (i.e., lookupability) effects seen in Experiment 1. In other words, we hypothesize that perceived availability effects (and digital availability effects more broadly) are caused by encoding differences, and that item-method directed forgetting effects, at least in this context, are caused by post-encoding differences.

This is a good point to emphasize the morphing goals of this project and mention limitations of the research design. Ideally, we would like to identify the separable mechanisms that could contribute to memory differences that are presumably driven by assumptions regarding the future availability of information. Considering the diversity of these situations in real life, and the flexibility of our cognitive machinery to utilize different specific mechanisms in different cases (e.g., a strategy for temporarily remembering a phone number is likely different from a strategy for studying lecture slides), perhaps a more practical and generalizable goal would be to understand whether observed memory differences are due to processes intervening at encoding or after encoding (post-encoding). The distinction between encoding variations and post-encoding forgetting is likely more generalizable to real life situations than the action of specific mechanisms. Further, our experimental approach does not permit us to make direct and specific conclusions about underlying mechanisms – we cannot be certain if participants actually engaged in selective rehearsal – instead we must rely on indirect inferences provided by our manipulations. This reality is the reason that these mechanisms are the subject of such lively debate in the directed forgetting literature in the first place – they are notoriously difficult to pin down.

With that in mind, we should still define some of the key mechanisms in question in an attempt to clarify some of their inherent ambiguities. We will operationalize attention as an executive process that acts to decide what information in the environment enters into working memory. After a piece of information enters working memory, any processing or maintenance of that information will be considered a rehearsal process. These definitions are intended to minimize the gray area between attention and rehearsal, though a clean separation between these mechanisms is not crucial because we hypothesize that they will operate in the same direction to produce memory differences.

Figure 5 incorporates the proposed mechanisms that could produce digital availability-related memory effects. This model, which is intended as a conceptual aid and should not be interpreted as a formal model, is largely informed by the results of the first three experiments.

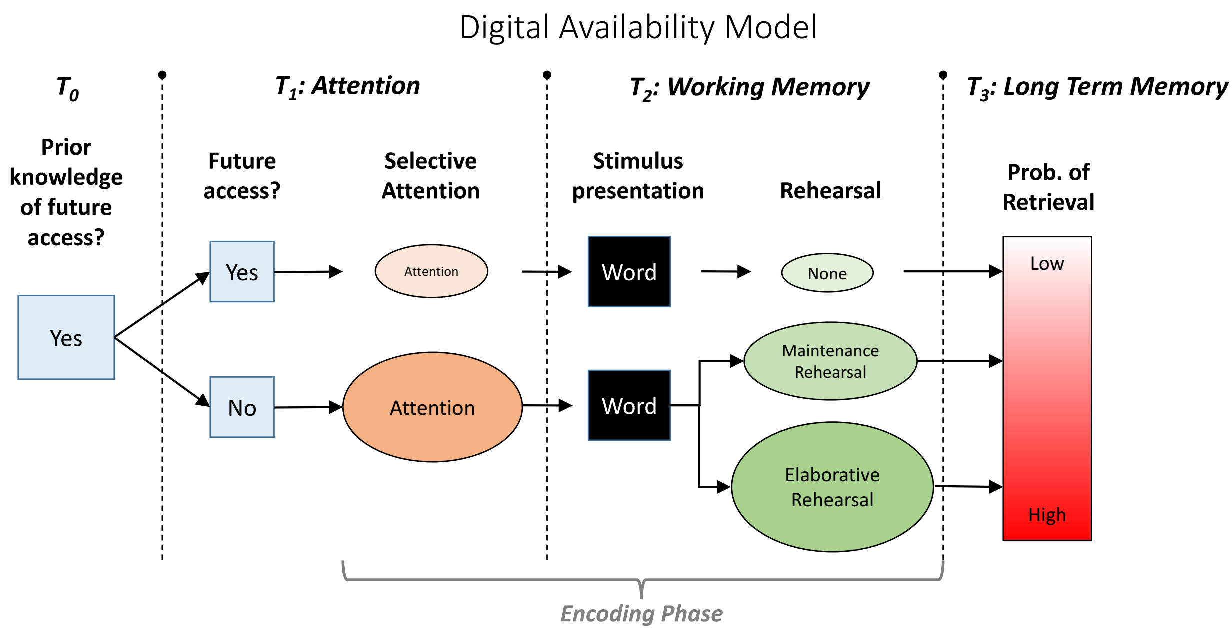

Figure 5. Information processing schematic illustrating how attention and rehearsal influence encoding of information dependent on future availability. Oval shapes represent cognitive processing mechanisms, with larger sized ovals indicating greater levels of processing. Square boxes represent externally acquired information. Note that this model requires that individuals have prior knowledge regarding the future availability of the information they encounter.

The schematic represents a natural flow of information processing, displaying the proposed underlying mechanisms and specific time points where we hypothesize that they might affect memory. We will briefly highlight how some important features of this model reflect our current understanding of availability-related memory effects.

First, this model requires that individuals are keenly aware of whether or not a certain piece of information is available to them in the future (e.g., they know they can look up a fact online; they know lecture slides will not be posted after class). Although the model reads as a serial time-course progression of information processing, it is not intended to rule out the possibility of parallel processing. For example, ascertaining a piece of information's availability may coincide with the processing of that information (e.g., Macias et al.'s [2015] lookupable stimuli). Second, the model includes an attention regulation phase (T_1), which is not a mechanism promoted by the directed forgetting literature. We believe that individuals may modulate the amount of attention they allocate to information *based on* their preexisting knowledge regarding future availability, which is a concept that does not map to directed forgetting. Third, Figure 5 does not include any post-encoding forgetting processes. Experiments 1 and 2 did not clearly indicate any contribution of post-encoding forgetting to saved/erased availability effects. This makes sense considering that, in the natural world, individuals will often know the importance or availability of information at the time they process it. Assumptions about future availability would only be violated in rare situations (e.g., losing a phone or computer, a website being taken down). This is in contrast to a traditional directed forgetting paradigm, where individuals do not know the importance of the information until after they view it. Similar amounts of attention should be allocated to each stimulus at onset in a directed forgetting paradigm.

In the real world, Figure 5 suggests that information deemed unimportant or readily available in the future could be allocated fewer attentional resources upon presentation and/or sparsely rehearsed, resulting in poor encoding and reduced memory. Because these items would be poorly encoded in the first place, it would be unnecessary to further recruit effortful active forgetting processes, so we have excluded post-encoding processes from the current model.

The remaining experiments were designed to test the hypotheses visualized in Figure 5; that availability-related memory effects are caused by differences in encoding, whereas directed forgetting effects (in this context) are caused by differences in post-encoding processes (i.e. active forgetting).

Experiment 3: Pre-cuing

Directed forgetting paradigms, by definition, require that remember or forget cues are always administered after a given stimulus. At the time of stimulus presentation, participants are unaware whether they will be told to remember or forget the current stimulus. This design is suitable for testing how memory for information can be augmented *after* it is initially processed (i.e. read), but it does not allow the participant to anticipate or strategically adjust their initial processing of that information. However, often times in the real world we know the importance, salience, or external availability of information before we even receive it, and it stands to reason that this prior knowledge could influence how we go about processing information. For example, students in a college course might know that information contained in the lecture slides will be posted online before they even read the slides. Logically, any prior knowledge that changes the way we initially process (or encode) information could have implications for subsequent memory.

The first three experiments in this thesis utilized an item-method directed forgetting paradigm where each trial consisted of a trivia fact displayed for 8 s, followed by a post-cue for 3 s, and finally a fixation cross for 1.5 s. One could argue that the task timing in these experiments limited the flexibility of encoding, and that these tasks were better suited to elicit differences in post-encoding processes. This is because the post-cue did not appear until more than half way through the encoding window of each trial (the 9th second of each 12.5 s trial). The window for selective rehearsal of each item was therefore only 4.5 s (3 s of cue duration + 1.5 s fixation), as participants necessarily spend the first 8 s reading and presumably equally encoding each item, given they have no salient prior knowledge. Further, the fast-paced nature of these tasks coupled with the relative complexity of the stimuli would make it difficult to engage in deeper types of processing or elaborative rehearsal in a brief 4.5-second span that would produce large subsequent memory benefits.

Experiment 3 seeks to enable a more flexible encoding process relative to previous experiments while keeping other parameters relatively consistent. Instead of presenting a post-cue after each item, Experiment 3 presents this same information as a pre-cue that occurs just prior to viewing the target item. Utilization of a pre-cue will create a greater window and opportunity for differential selective rehearsal for each item, in addition to allowing attentional processes to modulate encoding. Importantly, this task design allows us to consider attention as a relevant acting mechanism for the first time in this series of experiments.

Experiment 3 will also test an alternative hypothesis for the Experiment 1 and 2 findings. A simple explanation for why we obtained a directed forgetting effect in Experiment 2, but no saved/erased effect, is that participants in Experiment 2 thought they would never be tested on the F items. Conversely, participants in Experiment 1 knew they would eventually be tested on all items, regardless if they were saved or erased. Therefore, participants in Experiment 1 might have made a conscious effort to remember all items equally in order to save themselves some time reviewing the items later. If this were the case, then providing the SE cue *before* each item rather than after should also have no effect on subsequent memory for items.

If digital availability effects are driven by differences in encoding, then providing a SE pre-cue will offer an increased opportunity for attention and selective rehearsal processes to influence subsequent memory, relative to Experiments 1 and 2. Experiment 3 also includes an RF pre-cue as a separate between-subjects condition. Inclusion of this condition will allow us to compare the size of the effects as a within-experiment comparison, and provide further insight regarding the potential mechanisms underlying each cue type. The pre-cue procedure will also increase the ecological validity of the current research because more often than not, people know how available certain information will be prior to their exposure to that information (e.g., we know that anything we read online will likely be accessible at that same location in the future).

Method

Participants

One-hundred and sixty participants (76 female, M_{age} = 35.6, *range* = 19-68, *SD* = 10.5) were recruited from Amazon Mechanical Turk. Participants were required to have IP addresses in the United States and were paid $2.10 for completion of the study. Twelve participants were excluded from analysis: three for containing incomplete data, and nine for exceedingly poor performance (<10% accuracy).

Materials and Procedure

Experiment 3 incorporated the same design as Experiments 1-2 but also introduced cue condition [(SE), (RF)] as a between-subjects variable. Participants were randomly placed into either the SE group (N = 76) or the RF group (N = 72), and then all participants completed the same 2 [sentence type: L, NL] x 2 [cue type: (S,E) or (R,F)] design. Participants completed the 8-trial practice block from either Experiment 1b (SE group) or 2 (RF group).

Stimuli and procedure were identical to Experiments 1-2, except that the 3 s cues now appeared *before* the trivia fact was displayed rather than after. Participants in the SE group were given the same instructions as those in Experiment 1, and participants in the RF group were given the same instructions as those in Experiment 2. All participants were tested on all 40 trivia statements using the same cued recall task. The author scored all responses blind to condition.

Results

Trials where participants indicated prior knowledge of the trivia facts were omitted from analysis (.07% of responses). To test if the effect of cue type (S vs. E and R vs. F) was different between the two cue groups (SE vs. RF), the data were first analyzed using a mixed-effects regression model predicting memory accuracy from the between-subjects variable cue condition (SE, RF), the within-subject variables fact type (NL, L) and cue type (S, E *or* R, F), and all interaction terms. Item and subject were entered as random effects. This model revealed a significant main effect of cue type ($\beta = -0.304$, $t(5707) = -12.79$, $p < .0001$) as well as a significant cue condition by cue type interaction effect ($\beta = 0.246$, $t(5699) = 7.41$, $p < .0001$). This interaction suggests that the S vs. E pre-cues in the SE condition had a different effect on memory accuracy than the R vs. F pre-cues in the RF condition. No other main effects or interactions reached significance. Figure 6 displays the group means for both the SE and RF conditions, with the significant main effects denoted by asterisks.

After confirming that the effect of cue type differed between the SE and RF conditions, mixed effects regression models were run separately for the SE cue condition and the RF cue condition, with item and subject as random effects. Even though the full model did not indicate a main effect of lookupability (L vs. NL), we included this variable as a predictor in addition to cue type to see if there were any marginally significant interactions.

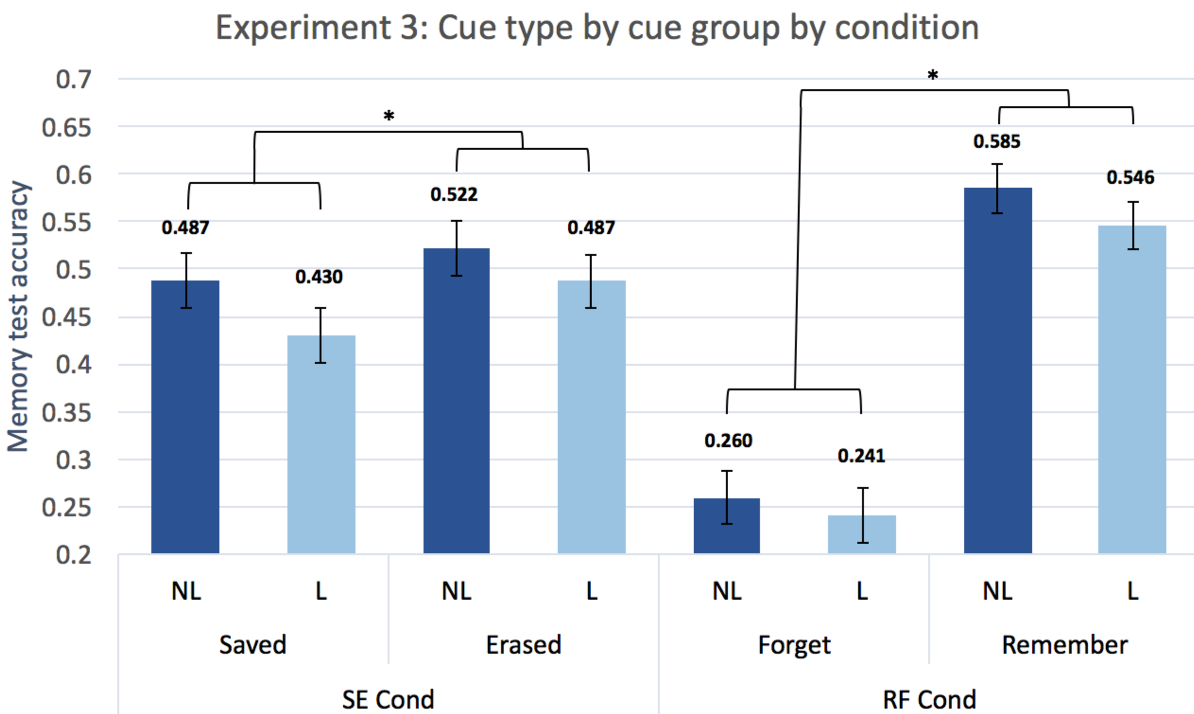

Figure 6. Memory test accuracy for each cue type (NL, L), shown separately by the cue group (S, E, R, F) for each condition (SE, RF). Error bars depict standard error. Asterisks denote significant main effects.

For the SE condition, there was a significant main effect of cue type (β = -0.056, $t(2915) = -2.37$, $p = .02$), such that memory was better for Erased facts ($M = 50.5\%$, $SD = .500$) than Saved facts ($M = 43.4\%$, $SD = .461$). There was no significant main effect of lookupability ($M_L = 45.8\%$, $M_{NL} = 48.1\%$), and no significant interaction.

For the RF condition, there was a significant main effect of cue type (β = -0.306, $t(2777) = -13.13$, $p < .0001$), such that memory was better for Remember facts ($M = 56.6\%$, $SD = .496$) than Forget facts ($M = 25.1\%$, $SD = .433$). There was no significant main effect of lookupability ($M_L = 39.4\%$, $M_{NL} = 42.2\%$), and no significant interaction.

Tukey multiple comparisons for all condition by cue type contrasts (i.e., collapsing across lookupability) were run to test for specific differences. Tests revealed that all condition by cue type cell means significantly differed from one another (See Figure 6 for cell means), with the lone exception of the SE(Erased) vs. RF(Remember) comparison ($\beta = -0.06$, $p = .21$), indicating that memory for saved items in the SE condition was no different from remember items in the RF condition.

Discussion

The results from Experiment 3 establish the first proof of concept of the saved/erased manipulation, demonstrating superior memory for erased items compared to saved items. By moving the SE cue to precede the stimulus, we have replicated Sparrow et al.'s (2011) explicit availability effect. All perceived availability effects (lookupability) that were significant in Experiment 1 were no longer significant. A condition by cue type interaction suggested that the directed forgetting effect in the RF condition was larger than the explicit availability effect in the SE condition.

Experiment 3 provides support for our hypothesis that encoding differences are responsible for the explicit availability (SE) memory effect. By providing prior knowledge about an item's future availability, we enabled more flexible encoding of each item so that participants could tailor their memory towards less available items. In Experiment 1, the perceived availability (lookupability) of each item was ascertainable as soon as each item was read by participants - *before* they received the explicit SE availability cue. Thus, it seems logical that participants may have latched onto the earlier availability cue as a means of guiding their selective rehearsal towards NL stimuli, and largely ignoring the later explicit cue. However, in Experiment 3, the explicit availability cue was presented before the perceived availability could be determined. Participants appear to have used this cue to guide encoding, and mostly disregarded the item's perceived availability.

The significant SE effect in Experiment 3 also provides important evidence to counter the argument that participants in Experiment 1 might have made a conscious effort to remember all items equally, knowing that they would later be tested on all items. Participants in Experiment 3 were equipped with this same knowledge, and yet still prioritized memory for erased items compared to saved items. This suggests that the procedural conditions in Experiment 1 may not have been sufficient to allow flexible encoding for different items.

Within-experiment multiple comparisons of the RF and SE conditions (See Figure 6 for cell means, but note that only main effect significances are labeled) demonstrated that participants' memory for F items in the RF condition was significantly worse than participants' memory for S items in the SE condition. Beyond suggesting that the F cue is a stronger driver of memory than the S cue, we can infer some potential mechanistic conclusions from this comparison. We hypothesize that encoding processes are primarily responsible for digital availability effects, whereas active forgetting processes are primarily responsible for directed forgetting effects. The finding of significantly worse memory for F items than S items could mean one of two things according to this hypothesis. For one, this finding could demonstrate that active forgetting processes have a much stronger influence on memory than encoding processes in this situation. Alternatively, this finding could suggest that both encoding *and* post-encoding processes were working to suppress memory for F items, whereas only encoding processes were responsible for suppressed memory for S items. However, we should acknowledge that the design of this experiment does not allow us to make strong conclusions about the potential action of post-encoding processes. It could be that participants are simply ignoring F items at presentation, resulting in significantly poorer encoding for F items relative to S items (with no influence from active forgetting). Finally, the fact that memory for R items was not significantly better than memory for E items means there is no statistical evidence for enhanced encoding of R items relative to E items, which, if significant, would have suggested that differential encoding contributed to directed forgetting in this experiment.

The explicit availability finding from Experiment 3 makes an important contribution to the literature. Superior subsequent memory for pre-cued E items compared to S items in Experiment 3 is the only replication to-date of Sparrow et al.'s (2011) third experiment, which showed that *explicitly manipulating availability on an item-by-item basis* can result in subsequent memory differences. All other studies to date have either reported memory differences based on perceived availability (e.g., Henkel, 2014; Macias et al., 2015) or manipulated explicit availability as a between-subjects or block design (e.g., Sparrow et al., 2011 [Exp. 2]; Storm & Stone, 2015). Experiment 3 also provides significant evidence for the importance of the encoding phase to availability-related memory effects. Compared with the null post-cue availability effects from Experiment 1, the recall advantage produced by pre-cued availability in Experiment 3 provides the strongest extant evidence that these type of memory differences are due to differential attention and/or rehearsal processes at encoding, rather than post-encoding processes.

Experiment 4: Feeling-of-Findability

The perceived availability (lookupability) effect that was found in Experiment 1a and 1b mostly vanished in Experiment 3. One explanation for this disappearance is that explicit availability was made more salient in Experiment 3 by delivering this information as a pre-cue, before participants could assess perceived availability. It could be that participants latch on to the first availability cue they encounter, which then allows them the maximum amount of time to flexibly adjust their encoding strategy before the end of the trial. Experiments 1 and 3 always included two sources of availability for each stimulus: perceived availability (inherently nonlookupable vs. lookupable) and explicit availability (saved vs. erased cue). Relative to explicit availability, perceived availability has always been at a disadvantage as an encoding cue because it was never made salient in Experiments 1-3. Participants were never informed of the difference between the two types of stimuli they view (L vs. NL). Perceived availability has also had to compete with explicit availability as an encoding cue in all previous experiments. Experiments 4 will probe the effects of perceived availability in isolation to more closely examine its potential impacts on subsequent memory.

Macias et al. (2015) presented evidence that, presumably due to increasing familiarity with the Internet and digital tools, people have the ability to automatically track the future availability of information, and are then able to preferentially remember information that they anticipate limited access to. Borrowing their stimuli, we replicated their main effect of lookupability, finding superior recall for NL facts compared to L facts in both Experiment 1a and 1b. However, we have been wary of potential item effect confounds in this stimulus set. It could be that NL items are more memorable for reasons other than their perceived availability. These stimuli concerns were alleviated by the finding of null lookupability effects in Experiments 2 and 3, and were also combatted by including items as a random effect in our analyses. Nevertheless, a solid replication of the perceived availability effect with alternative stimuli would go a long way towards not only establishing the effect as real, but also gaining an understanding of the underlying mechanism(s) that produce the effect. Experiment 4 attempts to replicate the lookupability effect found in Experiment 1a and 1b using a new set of stimuli, and will leverage a new procedure aimed towards making each item's perceived availability more salient.

Risko, Ferguson, and McLean (2016) recently reported a phenomenon similar to Macias et al. (2015): that people have developed a sense of the relative availability of information stored on the Internet – an idea they refer to as the feeling-of-findability (FOF). Specifically, Risko et al. used a normed set of general knowledge questions to show that when individuals do not know the answer to a question, their reported FOF accurately predicts the amount of time it takes them to actually locate the answer on the Internet. They also found that participants' FOF ratings were unrelated to the difficulty of the question itself. Rather, FOF ratings appeared to be related to how difficult it was to generate a successful search query term for each item, as well as how frequently participants believed that type of information was being sought. The question we are most interested in, which Risko et al. did not measure or test, is whether an item's FOF rating had any bearing on subsequent memory for that item.

Exp. 4 will implement a modified version of Risko et al.'s (2016) FOF paradigm that will include a memory test to see if an item's perceived findability predicts subsequent memory for that item. The stimuli used in their experiments were general knowledge questions taken from Tauber, Dunlosky, Rawson, Rhodes, and Sitzman (2013). These normed stimuli are particularly useful because they are well validated, with each item containing descriptive information regarding probability of recall (i.e., likelihood of someone knowing the answer) and average response latency, among several other measures[1]. Insofar as the FOF represents a gradient of "lookupability", we can use these validated stimuli in an attempt to replicate the perceived availability effect from Experiment 1.

Method

[1] Evan Risko generously provided average FOF ratings he has obtained for a subset of 90 of the 299 stimuli. Unfortunately, for reasons detailed in the methods section, we were unable to use most of these select stimuli in our experiment.

Participants

One-hundred and three participants (47 female, M_{age} = 37.4, *range* = 22-74, *SD* = 10.9) were recruited from Amazon Mechanical Turk. The experiment was piloted on three initial subjects to test for functionality and experiment duration, before 100 additional participants were recruited. Pilot subjects were included in the analysis because none of the experiment parameters were modified after their participation. Participants were required to have IP addresses in the United States and were paid $2.10 for completion of the study. Six participants were excluded from analysis: five for exceedingly poor performance (<10% accuracy), and one for indicating too much previous knowledge of the stimuli (>75% of stimuli known).

Materials

Experiment 4 used general knowledge norms made available by Tauber et al. (2013), who updated and expanded on the list originally developed by Nelson and Narens (1980). Each item is phrased in the form of a question, has a one-word answer, and can be found on the Internet. The entire list contains 299 stimuli, along with descriptive information for each stimulus, from which we selected a subset of 40 items in accordance with our previous experiments (See Appendix C for the full list of stimuli). This selection process is detailed below.

The most important property of this dataset for our purposes is the probability of recall for each item, which indexes the likelihood of someone knowing the answer. Because participants will be tested on their memory for the general knowledge questions, it is important that they only encounter questions to which they are unlikely to know the answer (similar to Macias et al.'s [2015] lookupable stimuli). Of the 299 general knowledge questions from Tauber et al. (2013), 85 were suitable for our needs in that they have a normed probability of recall of

<1%, meaning that there is less than a 1% chance that an individual knows the answer to the question. However, 55 of these 85 questions are of a similar style - they all ask specifically for the last name of a certain person or fictional character. This is a disproportionately large amount of last-name questions compared to the full sample of 299, which contains a greater spread of question types across many knowledge domains. In order to maximize external validity, we wished to obtain as diverse a subset of general knowledge questions as possible. For this reason, we chose the remaining subset of 30 non-last-name questions having a normed probability of recall of <1% and greater variety of question diversity. The final 10 questions needed were randomly drawn from the pool of 55 remaining last-name questions. Due to the limiting criteria necessary for the manipulation, every participant viewed the same set of 40 stimuli.

Procedure

Participants were instructed to complete the task in a quiet environment and to close all other Internet tabs/browsers in order to minimize distractions. They were given one hour to complete the task, though most finished in less than 30 min. The experiment consisted of a study phase and a test phase and closely resembled Risko et al.'s (2016) FOF paradigm, which was an adaptation of a standard paradigm used for investigating feeling-of-knowing (Koriat & Goldsmith, 1996; Nelson & Narens, 1980). Prior to beginning the experiment, participants completed a brief 3-trial practice block that included a study and test phase, in order to familiarize themselves with the procedure.

Each trial began with a general knowledge question (e.g., "What is the name of the instrument used to measure wind speed?") displayed on the screen for 8 s. Participants were then asked whether or not they knew the answer to that question, by responding with "I know" or "I do not know". Following this response, participants made a feeling-of-findability (FOF) judgment on a 9-point Likert scale that asked them to estimate how quickly they believed they could retrieve the answer to the question using the Internet (1 = I would find it almost instantly, 9 = I would find it in a few minutes). Note that the direction of this scale is counterintuitive, with lower numbers indicating *more* findable items. After the FOF rating, if they had indicated in the previous frame that they knew the answer, they immediately entered their answer in a textbox on the screen. If they had indicated that they did not know the answer, they were immediately shown the answer for a duration of 4 s. Analyses were limited to questions participants reported they did not know the answer to, regardless of the accuracy of their response. The study phase only differed from Risko et al.'s (2016) procedure in that the question display was timed, and that participants who selected "Do not know" were shown the answer for 4 s. This marked the end of each trial, after which a new question appeared on the screen. The instructions encouraged participants to be honest when making their "Know/Do not know" ratings, and noted that the questions they would see were intentionally obscure. Participants were told not to look anything up or use any other resources during the entirety of the study.

After the study phase, participants were reminded of all instructions and forced to wait 90 s before proceeding to the test phase. During the test phase, participants saw each question from the study phase in a random order. The full question appeared on the top of the screen, and participants were instructed to enter the answer in the text box below it. The test phase was untimed, though participants were not encouraged to spend more than 30 s on each question. The author scored all responses blind to condition. Novel questions about participants' online and Internet search habits were added to the post-experiment questionnaire (See Appendix D for full questionnaire), so that we could test whether participants with more frequent online search habits had varying memory for information. Notable questions included how many minutes per day participants spent online on average, as well as how many minutes per day participants spent using the Internet to look up information.

Results

Despite all stimuli used in Experiment 4 having a normed probability of recall below 1%, respondents in our dataset indicated a much greater knowledge of the stimuli. On average, participants responded "Know" to 4.9 (SD = 6.5) out of the 40 stimuli and subsequently entered the correct answer on 70% of these trials. A closer examination of the Tauber et al. (2013) paper revealed that these stimuli were normed on undergraduate students ($M_{\text{age}} = 20.1$), whereas our Mechanical Turk sample is comprised of a significantly older crowd ($M_{\text{age}} \sim 35$). Many items in the stimulus set contained references to dated cultural phenomena, which could help explain the disparity in prior knowledge responses. These trials where participants indicated previous knowledge (12.3% of total responses) were excluded from analysis.

To test an initial hypothesis, whether participants who rated information easier to find on average had poorer memory overall, we ran a standard linear model. FOF ratings and memory accuracy were averaged across each participant, and are plotted below in Figure 7. The model revealed no significant effect of average FOF rating on memory accuracy across participants ($t(95) = .17$, $p > .50$), indicating that individuals' perceived ability to find information online was not associated with their overall memory performance. A larger linear model was run that included the following variables from the post-experiment questionnaire as predictors of memory accuracy, in addition to average FOF rating: total minutes spent online per day on average, minutes spent online per day looking up information, and age. This model revealed no significant main effects other than age ($\beta = 0.01$, $t(92) = 4.35$, $p < .0001$), which interestingly indicated that, on average, older participants had better memory for information presented in the experiment (M_{age} = 37.4, *range* = 22-74, *SD* = 10.9).

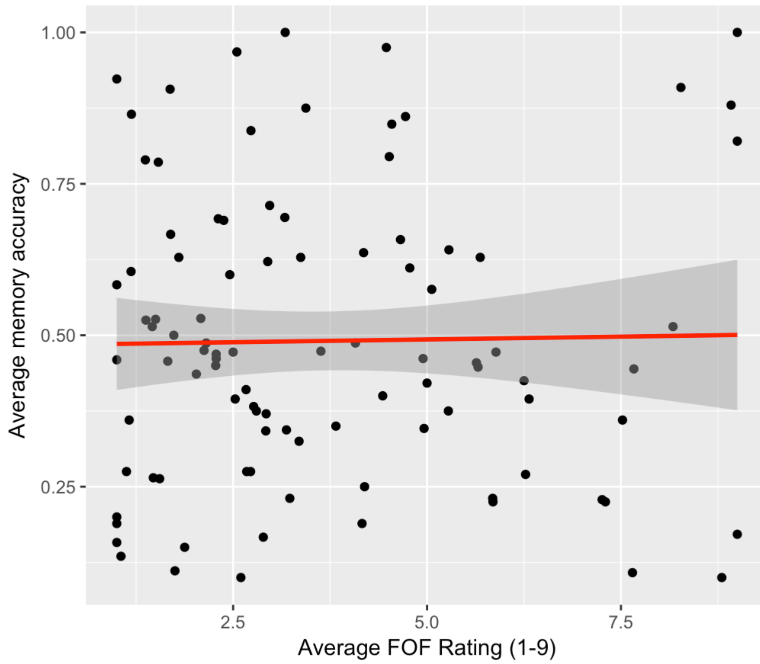

Figure 7. Scatterplot showing the average Feeling-of-Findability (FOF) ratings on a scale from 1 (easy to find) to 9 (hard to find) for each participant, plotted against their overall memory accuracy. Lower FOF scores indicate easier findability ratings, and higher FOF scores indicate harder findability ratings.

To test our primary hypothesis, whether items rated as more findable would be associated with worse memory, we fit a mixed effects regression model predicting memory accuracy from FOF ratings for all trials. Items and subjects were included as random effects to account for the variation in FOF ratings between subjects, so that each item and subject's intercepts were allowed to vary. The mixed effects model revealed a significant main effect of FOF rating on memory accuracy (β = -0.017, $t(3305) = -3.77$, $p < .001$). However, this effect was in the opposite direction of our hypothesis, such that for every 1-level decrease in FOF rating (one level *more* findable), memory accuracy improved on average by 1.7% (β = -0.017). Figure 8 displays this regression and the line of best fit.

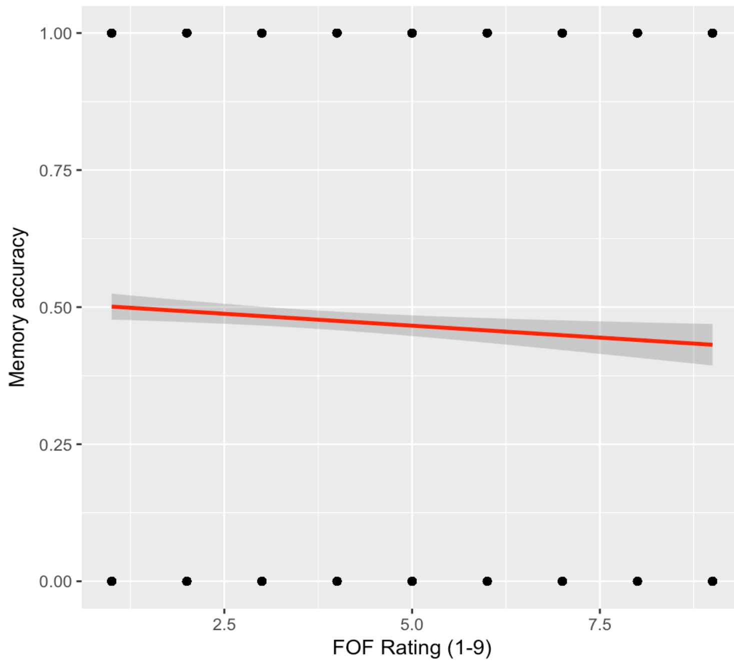

Figure 8. Linear model showing Feeling-of-Findability (FOF) ratings on a scale from 1 (easy to find) to 9 (hard to find) regressed onto memory test accuracy. Lower FOF scores indicate easier findability ratings, and higher FOF scores indicate harder findability ratings. All trials are overlaid, represented within the black dots. Shaded gray areas indicate linear fit based on the regression model, with wider regions indicating poorer fit.

Because our initial linear model indicated that age was a significant predictor of memory accuracy, we decided to test another mixed effects model that included age as a covariate, in addition to FOF rating and their interaction term. This model revealed a significant main effect of age on memory accuracy ($\beta = 0.01$, $t(95) = 3.99$, $p < .001$), again indicating that older participants had greater memory accuracies. However, the addition of age as a covariate in the model resulted in FOF ratings no longer being a significant predictor of memory accuracy ($\beta = -0.006$, $t(3304) = -0.36$, $p > .50$). There was no significant interaction effect between FOF ratings and age ($\beta = -0.003$, $p > .10$). A final mixed effects model was run to test the direct relationship between age and FOF ratings, and this model indicated that age was not a significant predictor of FOF rating ($\beta = 0.03$, $p > .10$).

Discussion

Ultimately, Experiment 4 failed in its primary goal of reproducing a lookupability effect similar to that found in Experiment 1. Rather, results from Experiment 4 initially appeared to indicate the opposite pattern of results, with participants demonstrating better memory for more *easily* found information. However, further analyses suggested that this relationship was moderated by age, with older participants demonstrating greater memory performance than younger participants. After controlling for age, the FOF ratings had no impact on subsequent memory, despite age having no direct relation to FOF.

By asking participants to rate the findability of each statement, we were asking them to think more deeply about the content of each item, and by doing so we hoped to encourage deeper and more elaborative processing of the materials relative to Experiment 1. We had hypothesized that participants would utilize findability as an encoding cue, and more deeply encode the items that would require greater effort to retrieve in the future. However, this was not the case, as the results suggest that, if anything, participants more deeply encoded the more easily findable items. Additionally, individuals who tended to rate information as less findable did not have better memory on average (nor did those who rated information as more findable), and the amount of time that individuals reported to spend looking up information every had no impact on their ability to remember information.

A question on the post-experiment questionnaire asked participants what strategy they used to remember the different items. The following are the response options, followed by the percentage of participants that selected each option in parentheses:

I prioritized memorizing the answers I felt were the HARDEST to find online (11%)
I prioritized memorizing the answers I felt were the EASIEST to find online (2%)
I tried to memorize all the answers equally (82%)
Other (please briefly describe your strategy – 5%)

While these data do not indicate support for our hypothesis (only 11% responded accordingly), neither do they indicate support for the contradictory hypothesis of superior memory for more easily found information (2% of responses). Despite the fact that memory was better for more *easily* findable items (when age was not controlled for), participants do not appear to have made a conscious effort to prioritize these items in their memory. Perhaps the most likely explanation for these pattern of results lies within our choice of stimuli. While Tauber et al. (2013) updated and recalibrated Nelson and Narens' (1980) general knowledge statements to reflect modern day norms, the statements themselves were still heavily influenced by cultural themes more popular around 1980. This would explain why older participants recorded better memory for these items: because they were alive when the content of Nelson and Narens' statements were culturally relevant, and thus the topics covered were more familiar to them. Older participants likely experienced many more "tip-of-the-tongue" type of moments when rating the statements as known or unknown, and likely selected several statements as unknown due to a lack of confidence in their knowledge, giving them a mere exposure advantage on the cued recall test.

This experiment was admittedly high risk, when compared to Macias et al.'s (2015) approach. While Macias et al. used stimuli that could be categorically placed into one of two extreme ends of the findability spectrum (easily lookupable, or impossible to look up), the stimuli used in Experiment 4 represented a much narrower range of findability that participants may not have been sensitive to. Nonetheless, it represented a necessary testing of the extent to which we may tailor our memory for information based on its perceived availability, rather than its assigned (explicit) availability.

Experiment 5: Encoding vs. Post-encoding processes

Our working hypothesis, based on our findings as well as the relevant literature, posits that digital availability-related memory effects are brought about by different levels of encoding for various items, whereas directed forgetting effects are predominantly caused by an active forgetting process that occurs after items are encoded, thus suppressing the retrieval of select items. Experiments 1-3 were molded after traditional item-method directed forgetting paradigms, wherein each item is immediately associated with a cue. For quite some time, the consensus was that selective rehearsal of R items was the mechanism underlying this type of forgetting (e.g., (Basden & Basden, 1998), though a flurry of studies over the past two decades has amounted considerable challenges to this theory, proposing that post-encoding inhibition processes are the hidden actor behind item-method forgetting (e.g., Wylie et al., 2008).

Although scholars continue to debate the mechanisms underlying item-method directed forgetting, there is less controversy regarding the mechanisms that produce list-method directed forgetting. In a traditional list-method experiment, participants view a sequential list of items that they believe they will be tested on. At the end of the first list, the experimenter delivers a "forget" instruction, typically informing participants that the first list was a practice and that they should forget those items and will not be tested on them later. Participants then view a second list of equal length, and are then surprisingly tested on all items. Compared to a control group that is told to remember both lists, participants in the F condition show worse memory for List 1.

Because considerable time has passed, the items on List 1 are presumably already encoded by the time participants receive the delayed mid-list F cue. In other words, list-method directed forgetting precludes the experimenter from attributing encoding differences to any forgetting that is witnessed (e.g., Basden, 1996). Rather, any differences in subsequent memory between the F group and R group for List 1 are attributable to post-encoding or retrieval differences, because all items should have been encoded under the same pretenses.

Because the mechanisms underlying the list-method of directed forgetting are less controversial, and theoretically allow us to rule out memory differences attributable to encoding differences, a list-method experiment is an excellent candidate to help us further understand the processes underlying directed forgetting vs. digital availability-related memory effects.

Our previous experiments have manipulated cue condition (directed forgetting [RF] vs. explicit availability [SE]), cue type ([S vs. E], [R vs. F]), and cue timing (pre, post). Because all of these variables remain theoretically interesting, allow us to make unique predictions for each cell, and have all played a significant role in at least one study, we will incorporate all three variables in Experiment 5 to maximize its potential impact.

Experiment 5 follows a traditional list-method design as all participants will view two separate lists sequentially. Traditionally, experimenters compare the R group (List 1 = Remember, List 2 = Remember) to the F group (List 1 = Forget, List 2 = Remember), where the RF instruction is delivered as a mid-list instruction. The focus of analysis is typically memory for List 1 items. If we consider memory for List 1 in isolation, participants in the F group are in a sense receiving a F post-cue, because they do not receive the forget instruction until they have viewed the entire first list. For Experiment 5, we created four between-subjects conditions. The first two conditions were the R (R-R) and F (F-R) groups described above, which comprise a typical list method directed forgetting experiment, where the R group serves as the control. The third and fourth conditions used saved/erased cues, with List 1 being saved and List 2 being erased for both groups (S-E). Participants in the third condition were notified of the saved/erased manipulation prior to beginning the experiment (S-E Pre), whereas participants in the fourth condition were not notified of the saved/erased manipulation until after they had viewed the first list (S-E Post). The R group will serve as the control for the other three conditions. This design will allow us to compare the effects of list-method directed forgetting against the effects of explicit availability when encoding can influence memory (S-E Pre) and when encoding cannot influence memory (S-E Post).

We will briefly consider how our working hypotheses should inform our predictions for each of the key variables listed above. Our previous studies have demonstrated that explicit availability only impacts subsequent memory when it is administered as a pre-cue (Experiment 3). This leads us to predict superior memory for E items compared to S items in the pre-cue condition (S-E Pre) in Experiment 5, because participants will be able to flexibly adjust their encoding. Any evidence of superior memory for E items compared to S items in the post-cue condition (S-E Post) would contradict our hypothesis, and imply the use of post-encoding processes in availability-related memory effects. Experiments 2 and 3 demonstrated that directed forgetting can be observed when RF cues are administered as either a pre-cue or post-cue. The list-method paradigm provides a more controlled test for post-encoding processes, because encoding differences at study can be ruled out (presuming the F instruction is given *after* list 1 presentation). Accordingly, if directed forgetting memory differences are attributable to post-encoding processes in the context of this experiment, then we should also obtain a directed forgetting effect in the F condition of Experiment 5.

The theoretical implications that a list method design carries should not be understated. The list-method represents a strong test for active forgetting processes because it precludes theoretical interpretations based on experimentally-induced encoding differences (in situations where cues are not delivered until after List 1 presentation). If any forgetting or memory reductions are witnessed in the F or S-E Post conditions relative to the control, we can confidently project that those differences are attributable to the action of post-encoding processes.

Method

Participants

One-hundred and seventy-six participants (77 female, $M_{age} = 36.6$, *range* = 19-77, $SD = 11.5$) were recruited from Amazon Mechanical Turk. We targeted a minimum of 40 participants per cell, and pre-emptively recruited an additional 4 per cell in anticipation of having to exclude multiple poor performers. Participants were required to have IP addresses in the United States and were paid $2.10 for completion of the study, with an opportunity to earn a $0.20 bonus for successfully entering a response to all previous knowledge questions. Eleven participants were excluded from analysis: two for containing incomplete data, seven for exceedingly poor performance (<10% accuracy), and two for indicating excessive previous knowledge of the stimuli (>75% of stimuli known).

Materials and Procedure

Participants were randomly placed into one of four conditions corresponding to the instructions they would receive: Remember-Remember (R-R), Forget-Remember (F-R), Saved-Erased pre-cue (S-E Pre), or Saved-Erased post-cue (S-E Post). Participants were instructed to complete the task in a quiet environment and to close all other Internet tabs/browsers in order to minimize distractions, and they were given one hour to complete the task though most finished in less than 30 minutes. Participants were also instructed not to take any notes or consult any other resources while completing the experiment.

Experiment 5 used the same 40 general knowledge question stimuli from Experiment 4 (See Appendix C). All participants completed a modified list-method paradigm that consisted of a study phase and a test phase. During the study phase, all participants viewed two separate lists containing 20 questions each, with a mid-list instruction between lists. Two lists of 20 items were randomly generated from the pool of 40 questions. Each participant saw both lists in the same order in order to facilitate comparisons across conditions (we are most interested in List 1 comparisons across conditions, rather than within-group comparisons). Questions were presented sequentially in a random order for both lists.

Each trial began with a general knowledge question displayed for 10 s. Below each question, participants were asked to indicate whether they "Know" or "Do not know" the answer to that question by choosing a response. In order to ensure consistent study phase timing, we did not force participants to respond to this question, and the experiment automatically advanced after 10 s. A one-time $0.20 bonus was offered to participants who successfully responded to all 40 questions within the 10 s window as a means to incentivize their responses. After 10 s, participants were shown the answer to the previous question for 4 s, regardless of whether they responded "Know" or "Do not know." This question was included to ensure that analyses could be limited to trials where participants had no prior knowledge. Participants were encouraged to be honest when evaluating whether or not they know the answer. After viewing the answer, a new trial began. Because of the nature of the List-method design, most previous researchers choose to forego a practice block when conducting these studies to avoid contaminating their experimental manipulations. Because the inclusion of a robust practice phase in Experiment 1b, but not Experiment 1a, did not change the results or believability of the manipulation, practice was omitted from the current experiment.

All participants were told prior to the beginning of the experiment that their memory would be tested for the items they viewed in both lists. Additionally, participants in the S-E Pre-cue condition were told that the items on the first list they viewed would be saved and stored online for their reference. They were told they would be given an opportunity to review these items *after* viewing a second list, but before taking the memory test.

After completing List 1, participants in the control condition (R-R) were told to continue remembering the items on List 1, as well as the items they would view on List 2. Participants in the F-R condition were told that they had just completed the *practice* list, and that they would not be tested on their memory for the list they just viewed. They were told that List 2 consisted of the actual experiment, and that they should do their best to remember the upcoming list. After viewing List 1, participants in the S-E Post-cue condition were given the same instructions that participants in the S-E Pre-cue condition received prior to List 1: that the items they had just viewed in List 1 had been saved for them, and that they would have an opportunity to review these items for as long as they like after the second list but prior to the memory test. At this point, participants in both S-E conditions were told that List 2 would be erased and not available for study prior to the memory test.

Upon completion of the second list, participants performed a brief distractor task (3 arithmetic questions) before beginning the memory test. Participants first viewed all 20 questions from List 1 sequentially in a random order, and entered their answers in a text box, followed by the same procedure for List 2 items. The consistent ordering of the List 1 and List 2 controls for possible output interference effects in the different groups' List 1 performance, and thus allows for a more direct test across conditions (Sahakyan & Kelley, 2002; Zellner & Bäuml, 2006). Participants finally completed a post-experiment questionnaire similar to Experiment 4 (See

Appendix D), which included questions about the amount of time they spent online and looking up information every day. The author scored all responses blind to condition.

Results

Similar to Experiment 4, we noted that participants in our experiment had a significantly higher prior knowledge of the stimuli than the normed probability of recall from Tauber et al. (2013) suggests. Participants indicated prior knowledge for 13.4% of the items, and additionally failed to log responses for 1.0% of the items. These 14.4% of trials were omitted from analysis.

We first created two mixed-effects regression models including items and subjects as random effects. The first predicted memory accuracy from condition, list number, and their interaction. The second model was identical to the first, but included FOF ratings (obtained in Experiment 4) as a covariate. FOF ratings did not account for a significant amount of variance in the second model ($F(1, 36.5) = .01$, $p > .90$), so analyses proceeded with the model that did not include FOF ratings as a covariate.

This model revealed a marginally significant main effect of condition on memory accuracy ($F(3, 5600) = 2.4$, $p = .06$) as well as a significant condition by list number interaction ($F(3, 5600) = 6.3$, $p < .001$). There was no main effect of list number. Figure 9 shows average memory test accuracies broken down by list number and condition.

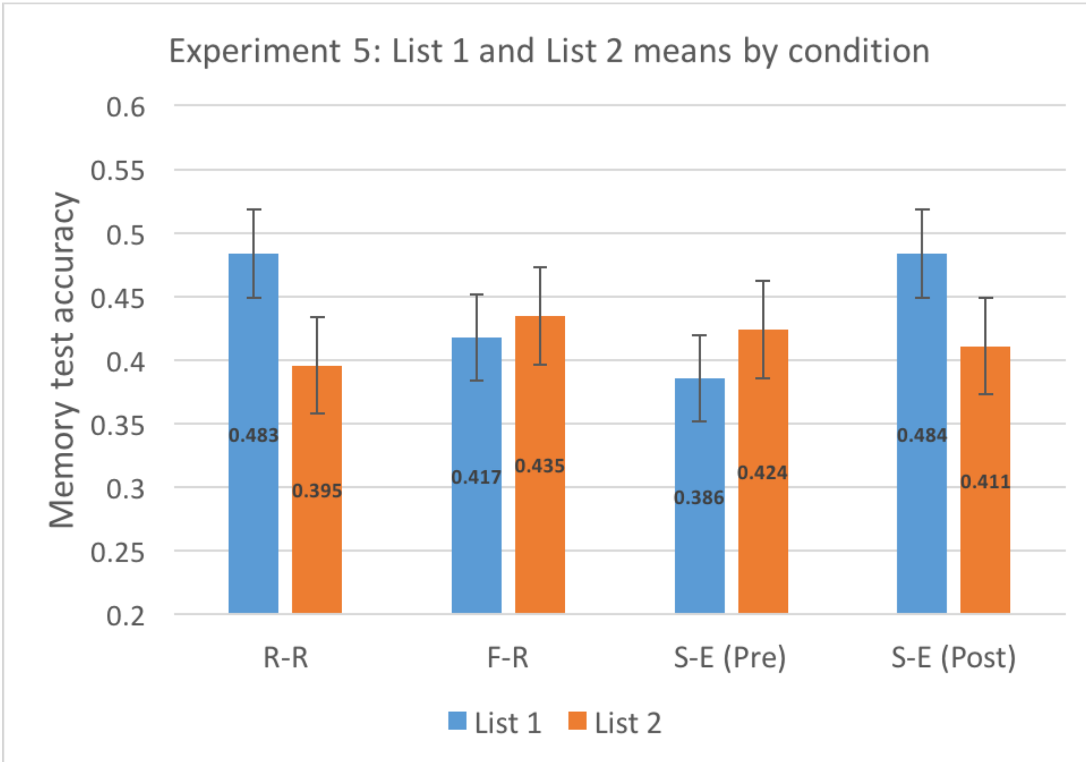

Figure 9. Memory test accuracy for List 1 (blue) and List 2 (orange) shown separately for the R-R (Remember-Remember) condition, F-R (Forget-Remember) condition, S-E Pre (Saved-Erased Pre-cue) condition, and S-E Post (Saved-Erased Post-cue) condition. Error bars depict standard error.

Our main comparison of interest was List 1 memory accuracy across groups, as this is where the manipulations were primarily targeted. After confirming that the effect of list differed across conditions, we ran separate mixed-effects models for List 1 and List 2, both predicting memory accuracy from condition. For List 1, there was a significant main effect of condition ($F(3, 2864.3) = 7.99$, $p < .0001$), indicating that List 1 memory accuracies varied across the four conditions. For List 2, there was no main effect of condition ($p > .40$), indicating that participants in all conditions performed equally well on the memory test for List 2 items.

Tukey multiple comparisons were performed to further examine the memory accuracy differences among conditions in List 1. These tests revealed that List 1 memory was significantly better for those in the R-R (control) condition (M = 48.3%, SD = .499) compared to those in the

S-E Pre-cue condition (M = 38.5%, SD = .487; β = -0.09, $p < .001$), suggesting that the "saved" pre-cue significantly reduced memory relative to the control condition. List 1 memory for those in the S-E Post-cue condition (M = 48.4%, SD = .500) was also significantly better than those in the S-E Pre-cue condition (M = 38.5%; β = 0.10, $p < .001$), as well as those in the F-R condition (M = 41.7%, SD = .493; β = 0.065, p = .04). These findings suggest that (1) the "saved" pre-cue was effective in reducing participants' memory, whereas the "saved" post-cue was not, and (2) that a mid-list instruction to "forget" List 1 significantly reduced memory for those items compared to a mid-list "saved" instruction. Finally, the post-hoc tests revealed that those in the R-R control condition (M = 48.3%) had marginally better memory for List 1 items than those in the F-R condition (M = 41.7%), indicating a marginal directed forgetting effect (β = -0.063, p = .06). No other paired comparisons reached significance.

Experiment 4 enabled us to obtain feeling-of-findability (FOF) judgments for each of the 40 items that were used in Experiment 5. These ratings did not account for a significant amount of variance when included in the full model above, but we wanted to test for any further evidence of a replication of Experiment 4 findings. We fit a mixed effects model with the item FOF ratings obtained in Experiment 4 as the sole predictor of overall memory accuracy, but the effect did not reach significance ($p > .90$). Other models were fit using total minutes spent online per day and minutes spent looking up information online per day, but none reached significance. FOF ratings were also included as a covariate in the first model reported, but they did not explain a significant amount of variance ($p > .80$). There is a strong possibility that FOF ratings did not predict accuracy because they lacked sufficient range and variability (see Figure 8) to explain the variance in memory accuracy. Additionally, the procedure in Experiment 5 did not require participants to rate findability, so this feature of the stimuli was never made salient.

Discussion

The primary goal of the fifth and final experiment was to specifically determine whether encoding processes (attention/selective rehearsal) or post-encoding processes (active forgetting) are responsible for digital availability-related memory effects. Separating the impact of encoding processes vs. post-encoding/retrieval processes on subsequent memory is a notoriously difficult task to accomplish using behavioral experiments, because anything that influences encoding will be reflected at the retrieval stage as well. However, it *is* possible to manipulate processes that occur after encoding without influencing encoding itself, and that is what Experiment 5 set to accomplish through its list-method design and cue timing manipulation. There is considerable consensus in the literature that post-encoding processes (e.g., inhibition) are the driving force behind directed forgetting effects observed using the list-method paradigm (e.g., Basden & Basden, 1998; Geiselman et al., 1983). Therefore, the results from Experiment 5 represent a stronger test for the contribution of post-encoding processes to digital availability effects relative to previous experiments in the current research. We will briefly discuss each finding highlighted above, and consider the implications with reference to the mechanisms put forth in our digital availability memory effects model from Figure 5 (re-copied below for reference).

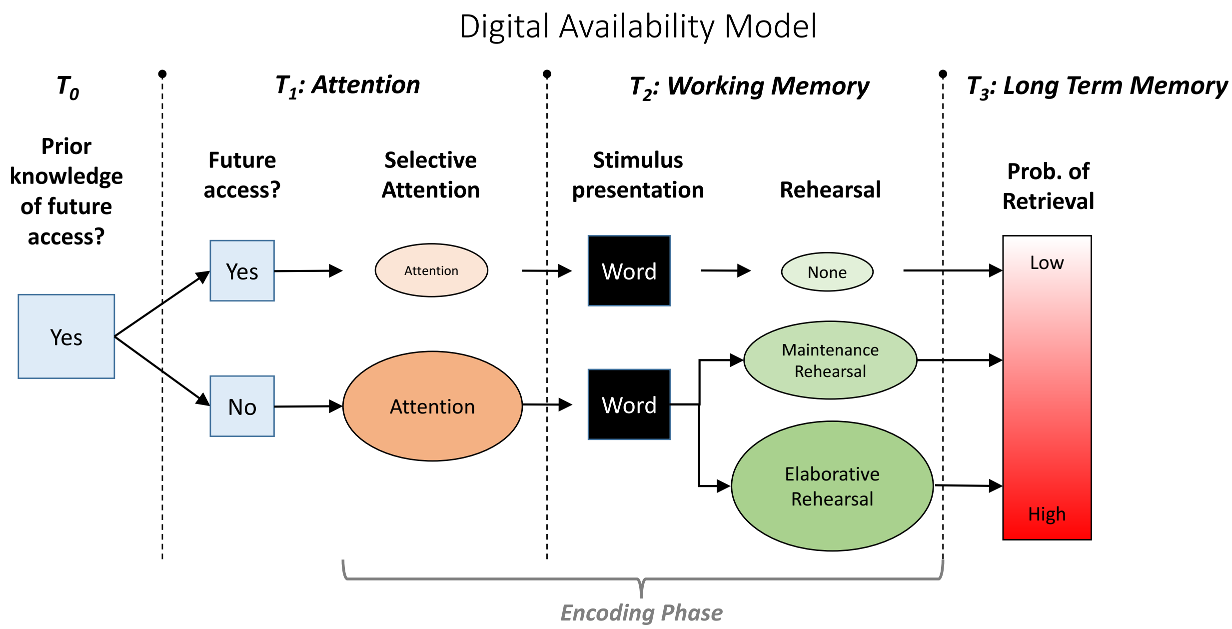

Figure 5. Digital Availability memory schematic. Re-copied from pg. 46 for reference.

The R-R condition and F-R condition were included in Experiment 5, among other reasons, to serve as a replication of a traditional list-method directed forgetting effect using our chosen stimuli and experimental parameters. Specifically, the directed forgetting effect refers to a decrease in List 1 memory for participants who received the mid-list "forget" instruction relative to participants who were told to continue remembering. A visual examination of the means in Figure 9 shows a marked decrease in memory in the expected direction, though this effect just missed significance (p = .06). One concern we had prior to beginning Experiment 5 was whether our memory test would be sufficient to produce a list-method directed forgetting effect. Researchers often report obtaining directed forgetting effects for the list-method when memory is tested via free recall, but they do not obtain the effect when memory is tested via recognition (e.g., Basden & Basden, 1998). Our experiment utilized a cued recall test where participants were given a question and only needed to recall the answer. This is a marginally weaker test of memory than free recall, though certain studies have measured directed forgetting using cued recall (Barnier et al., 2007; Paller, 1990). Design of the memory test may have contributed to the marginal significance of the directed forgetting effect. However, for the purposes of our interpretations, we will assume that this was a real effect. Further, because participants had already encoded List 1 prior to viewing the "forget" instruction, this necessarily implies that these participants engaged in some type of active forgetting process that effortfully suppressed memory post-encoding. This finding is paramount to our assertion that directed forgetting effects influence post-encoding processes.

Experiment 5 indicated that participants who were told that List 1 would be saved prior to viewing it (S-E Pre-cue condition) had significantly worse memory for that list than participants in the control condition (R-R). This result is analogous to the Experiment 3 findings, which also demonstrated decreased memory for items that were pre-cued as saved. This pattern fits perfectly with our hypothesis that poor encoding causes reduced memory for digitally available information, because it is the only condition in Experiment 5 where participants were given information about future availability prior to the encoding of List 1. Participants in this condition were free to flexibly adjust (i.e., downregulate) their encoding of List 1 information in anticipation of being able to view it again later.

In isolation, this finding provides strong support for our hypothesis, but it carries substantially more weight when considering that participants who received the "saved" instruction *after* encoding List 1 (S-E Pre-cue condition) had significantly better memory for List 1 than the pre-cued participants. Participants in these two conditions received the same "saved" manipulation - the only difference being that one group viewed it prior to List 1 and the other group viewed it after List 1. Crucially, this finding suggests that participants in the S-E Post-cue condition did *not* engage in any type of active forgetting or post-encoding inhibition. Participants in this condition performed just as well as participants in the R-R control condition, down to 0.1% accuracy. Further corroborating this account, participants in the S-E Post-cue condition performed significantly better than those in the F-R condition. Individuals in both of these groups received mid-list instructions: one to “forget” the previous list and the other that the previous list was “saved.” From this result, we can infer that participants in the S-E Post-cue condition did *not* engage in the same post-encoding, active forgetting processes that participants in the directed forgetting condition displayed. This is a positive result for our hypotheses, but we also note that participants knew they would be tested on all the stimuli, so it may have been counterproductive for them to engage in active forgetting. We will further elaborate on this point in the General Discussion.

Taken together, the current results provide considerable support for the digital availability memory model seen in Figure 5. Experiment 5 simultaneously provided (a) evidence in support of the encoding account of digital availability effects (S-E Pre-cue condition), (b) evidence against an active forgetting account of digital availability effects (S-E Post-cue condition), and (c) evidence in support of an active forgetting account of directed forgetting (F-R condition).

Lastly, Experiment 5 applied the FOF ratings obtained from Experiment 4 to test whether or not an item’s normed findability would affect subsequent memory for that item when its findability is not made salient (participants in Experiment 5 did not rate each item’s findability). Results indicated that these ratings had no meaningful impact on participants’ subsequent memory in Experiment 5. The implications of this finding to Experiment 1 will be discussed in the following section.

General Discussion

The structure of the world's knowledge is rapidly evolving. As information is transferred from print to digital formats, the floodgates are opened for this information to leach from device to device and person to person. Existing research has begun to document important ways in which interactions with technology are affecting the functioning of human memory. Yet, there has been no systematic investigation aimed towards understanding how these changes to memory are occurring. The current research sought to determine what specific components of increasing interactions with technology are affecting memory, as well as the underlying cognitive mechanisms that are driving those changes. The six experiments reported here all highlight the future availability of information as an important cue, and confirm that human memory can indeed be influenced by the anticipated accessibility of the information it is processing. We identified situations where both inherent, perceived availability can influence memory (Experiment 1a, 1b), as well as those where explicit, directed availability influences memory (Experiment 3, 5). Further, the current line of research suggests that information presumed to be more readily available will be less thoroughly *encoded* than comparable information that is less readily available. Different levels of encoding are likely achieved through some combination of attentional control and selective rehearsal that is dependent upon future availability of the content. Additionally, the currents experiments provide complementary evidence that implicates an active forgetting account of directed forgetting, indexed by marked reductions in memory for to-be-forgotten information.

Experiments 1a and 1b pitted the influence of perceived availability effects against those of explicit availability effects in order to determine which type of digital availability, if any, was more likely to influence peoples' memory for general information. Common sense dictated that giving people a direct cue regarding whether or not they would have access to tested information would be a reasonable index for encoding (Sparrow et al., 2011), but this cue had absolutely no bearing on subsequent memory for either experiment. On the contrary, Experiments 1a and 1b successfully replicated Macias et al.'s (2015) finding, showing a memory advantage for information that could *not* be found online, relative to readily searchable information.

Experiment 2 provided some necessary perspective to help interpret the null explicit availability effects from Experiment 1a/1b. Swapping out the saved/erased cues for remember/forget cues resulted in a reversal of the effects for Experiment 2: Perceived availability no longer seemed to influence memory, and the explicit cue now elicited better memory for remember items than forget items. There was an interesting marginally significant interaction in Experiment 2 that indicated that perceived availability may have influenced memory only when participants were making a conscious effort to remember. Importantly, Experiment 2 also demonstrated that it was possible to achieve a directed forgetting effect using the same procedure from Experiment 1a and 1b. Together, these findings suggested that directed forgetting and digital availability-related memory effects were not operated by the same mechanisms. Specifically, Experiments 1-2 suggested that "saving" information was not sufficient to produce active forgetting for that information, which *did* appear to occur following a "forget" instruction.

Experiment 3 provided evidence in support of the hypothesis that encoding variations produce digital availability effects. By shifting the explicit availability cue to appear before the stimulus, participants were given more control over their encoding processes, which resulted in a memory benefit for erased information relative to saved information, completing an important replication of Sparrow et al. (2011). Transferring the explicit availability cue to a pre-cue also coincided with the disappearance of the lookupability effects that were present in Experiment 1a and 1b. Memory appeared to be latching on to the first available cue to guide encoding, regardless whether that was perceived or explicit availability.

Experiment 4 stepped outside the realm of directed forgetting with a design that attempted to generalize Macias et al.'s (2015) perceived availability effect (as well as the perceived availability effect from Experiment 1a and 1b) to an alternative stimulus set and different procedure. Risko et al.'s (2016) demonstration that people have accurate metacognitive awareness of how easily they can find certain information online provided a convenient avenue to test if this metacognitive awareness could also guide encoding. Experiment 4 found that general knowledge questions which were rated as more easily findable were associated with better subsequent memory, contrary to our hypothesis. However, after controlling for age, this effect disappeared. Given the dated stimuli used in this experiment, older participants were likely more familiar with the content they were tested on, which may have played a role in artificially inflating memory for more findable stimuli.

Experiment 5 represented a more controlled test for the presence of post-encoding processes, separating saved/erased availability cues and directed forgetting cues while implementing a new list method design of directed forgetting. This experiment combined nearly all of the elements and conditions from Experiments 1-3, and incorporated them into a single list-method experiment. The experiment succeeded in eliciting a directed forgetting effect (although the effect was marginally significant). The analogous condition within the saved/erased group did *not* demonstrate the same effect. After viewing a list of items, an instruction to forget weakened memory for those items, whereas the saved instruction did not reduce memory.

A bird's eye view of the research conducted here reveals a pattern of findings compatible with an encoding account of digital availability-related memory effects. We succeeded in obtaining traditional directed forgetting effects for both item-method (Experiment 2) and list-method (Experiment 5) designs. At the same time, we importantly demonstrated that introducing explicit availability as a pre-cue (Experiment 3 & 5) successfully moderated memory, whereas the same cues introduced as post-cues did not significantly affect memory (Experiment 1a & 1b). Taken together, these findings strongly suggest an encoding account for digital availability-related memory effects.

Explicit availability

Although couching digital availability-related memory effects in the directed forgetting literature provides a useful (and otherwise absent) overarching framework for understanding these effects and their underlying mechanisms (Sparrow et al., 2011; Storm & Stone, 2015), it also presents some obvious challenges. Perhaps one of the biggest challenges for comparing these effects side by side is the fact that explicit digital availability manipulations (i.e. saved/erased cues) always carry the implication of a potential future memory test for both saved and erased items, whereas directed forgetting instructions explicitly rule out the possibility of a future memory test for forget items. It is not difficult to imagine that particularly motivated participants would disregard any saved/erased instruction entirely if it had no bearing on what they would ultimately be tested on. In fact, perhaps the most efficient strategy to complete many of these experiments in the shortest amount of time would be to remember all items uniformly.

This necessary difference between manipulation instructions undoubtedly plays a part in explaining why digital availability effects are generally smaller than analogous directed forgetting effects (Experiment 3, Experiment 5). It could also preclude participants from engaging in active forgetting processes –making a concerted effort to inhibit memory for an item (e.g. saved items) would seem illogical if one knows they will be tested on it in the near future.

We will take a moment to discuss retrieval inhibition, in light of this question: Would participants in the saved/erased condition go so far as to engage active forgetting processes, even when they know they will be tested on the items they are inhibiting later?

As mentioned in the introduction, some researchers believe that retrieval inhibition operates by simply inhibiting *access* or reactivation of a memory trace at the time of retrieval (e.g., Bjork & Bjork, 2003; Geiselman et al., 1983), whereas others imagine retrieval inhibition as a more permanent suppression of memory (e.g., Bjork, 1989). The former implies a more flexible account of inhibition, which nicely accommodates the finding that recognition tests consistently fail to record list-method directed forgetting effects (MacLeod, 1999). The idea is that the act of re-presenting a stimulus triggers "release" from inhibition, and grants full access to a previously encoded memory trace. Here, retrieval inhibition is viewed as serving a more adaptive purpose, reducing *proactive interference*. Previously encountered items can be inhibited in memory, in order to free up processing resources for future items, thereby reducing the competition or interference in memory to hold multiple items. The function of retrieval inhibition in this context actually warrants an analogy to cognitive offloading: the goal of both processes is to dismiss less relevant information in order to maximize processing capacity for more relevant information.

If we adopt this flexible version of retrieval inhibition, which is among the more favored interpretations in the directed forgetting literature (Basden et al., 1993; Geiselman et al., 1983; Sahakyan & Delaney, 2003), it may not seem as inefficient for participants in a digital availability manipulation to engage in active and effortful forgetting processes. Perhaps in a task that required one to memorize a rather expansive amount of information, with some items saved and some items erased, it would indeed benefit performance if an individual could inhibit saved items in order to increase memory capacity for a larger load of erased items, hence reducing proactive interference and increasing memory efficiency. However, this entire consideration, if true, would directly contradict our evidence that digital availability effects are brought about by differential encoding of items. The failure to find evidence for active forgetting in the S-E Post-cue conditions in Experiments 1 and 5 suggests that active forgetting processes such as retrieval inhibition are not engaged by saved instructions.

A careful look at the means across groups in the current series of experiments leaves some room for interpretation of multiple mechanisms to exert their effects. For example, in Experiment 3, participants in the R condition were about 6% more accurate than participants in the analogous E condition. Though this difference was not significant, we would not rule out selective rehearsal of R items helping to slightly boost memory for these items, even though we posit that active forgetting processes are predominantly responsible for directed forgetting effects. One way to consider the action of all these mechanisms is as an “adaptive toolbox” approach, taken from the heuristics and biases literature (Bröder, 2003). This framework suggests that individuals have any number of heuristics they could use at any time, and their application of these heuristics is flexible. Similarly, participants could apply the memory mechanism (i.e. selective rehearsal, attention, active forgetting) that would result in the best performance in the most appropriate situation. Notably, other factors such as environmental influence can also affect how one uses the toolbox. Therefore, it would not be surprising if subtleties within the instructions or across conditions resulted in selection of a different mechanism. With that said, our results do indicate a fair amount of consistency, in that they suggest participants are selecting similar mechanisms in similar situations.

There is also evidence in the literature that could implicate active forgetting processes in digital availability effects. Storm and Stone (2015) conducted a study similar to Experiment 5 that utilized a different type of list-method procedure (this study is described above in the introduction). Participants viewed a list of words on a PDF file, either saved or erased that file, viewed a second list of words, and then took a memory test. They only looked at memory for the second list (because participants were allowed to re-study the first list), but found subsequent memory benefits for this list after the first list was saved. This is a common finding in the list-method directed forgetting literature that is referred to as the benefit of forgetting. Memory gains are said to be a result of reducing proactive interference (Sahakyan & Foster, 2009). Although Storm and Stone could not report any memory deficits for List 1 following a mid-list save instruction, it is unlikely that a benefit for List 2 would emerge without a corresponding cost to List 1.

Reduced memory for List 1 saved items in Storm and Stone's (2015) task, which appears may have been likely, would contradict our Experiment 5 findings in that it would implicate that active forgetting processes were engaged by a saving process. Their procedure was considerably more active than ours, as participants in their experiment viewed a PDF file and then either saved it to the computer's desktop themselves, or closed the file without saving it. It is possible that being given the agency to actively save or erase information (as was also done by Sparrow et al., 2011) could recruit active forgetting processes. Perhaps the process of actively saving or erasing information has consequences for memory in the same way that simply generating information improves memory for that information (Slamecka & Graf, 1978).

Future research in this area should be done to determine if explicit availability-related memory effects operate differently if one is simply given an accessibility cue (i.e. Sparrow et al., 2011; the current research) versus being given the agency to save, erase or offload information themselves.

Perceived availability

Our experiments returned mixed results with respect to perceived availability, making it difficult to conclude any consistent impact on memory. For one, results from Experiments 1 and Experiment 4 resulted in slightly contradictory results. Experiments 1a and 1b found a memory benefit for less available (i.e. nonlookupable) information, whereas Experiment 4 found a memory benefit for more available (i.e. more findable/searchable) information. However, this effect disappeared when controlling for age, and was also not replicated in Experiment 5. Unfortunately, there are not many other data points for comparison beyond Macias et al.'s (2015) study showing superior memory for nonlookupable statements compared to lookupable statements.

Anecdotally, perceived availability seems like a cue that would only influence memory in situations where it had obvious ramifications. Computer passwords is one natural example that comes to mind. Many websites and portals require passwords that contain a dizzying amount of variety, and being locked out of those sites because one cannot recall their password induces an equivalent frustration. Depending on an individual's organizational habits, many of these passwords are not recorded anywhere, and are not something we can search for online – they are inherently unavailable. There are also consequences to forgetting this information, of which I am sure we are all well aware. This is a situation where a perceived availability memory effect (increased memory for inherently less available information) would be quite understandable.

In Macias et al.'s (2015) experiment, there is no immediately obvious benefit for prioritizing memory for nonlookupable versus lookupable information; neither is there in our

Experiment 1a and 1b, which is why these findings are surprising. Participants never believe they will have to look up information. The only explanation put forth for these effects is that people are becoming so used to interacting with information through the Internet and their devices, that they automatically track their expectations about what information they anticipate future access to, and tune their memories accordingly (Macias et al., 2015). Another explanation for these findings (as well as our own Experiment 1 findings) is an experimental confound. Specifically, nonlookupable items might be more memorable because they are all connected by an overarching context: the elementary school. Whereas the lookupable facts are all independent pieces of information, the nonlookupable facts involve similar teachers and a unifying context that could aid in subsequent retrieval. However, the fact that these differences disappeared when participants were instructed to forget (and thus had lower overall subsequent memories; Experiment 2 and 3) seems to argue against this point, as the effect of the confound should remain consistent.

Experiment 4 was designed to extend the perceived availability effects obtained in Experiment 1, and test for a continuous rather than discrete effect while allowing the participants to discern their own perceived availability for each item (Risko et al., 2016). However, the results were somewhat inconclusive due to the perceived availability effect being moderated by age. It is hard to explain how, when not controlling for age, more findable items were associated with better subsequent memory. One possibility could be that items rated as more findable were also more familiar to participants. This result would appear to contradict Macias et al. (2015) as well as our own Experiment 1 findings. Experiment 4's failure to generalize the perceived availability effects from Experiment 1a and 1b lends further credence to the idea that these early effects may have been caused by an experimental confound.

One concern raised prior to conducting Experiment 4 was that we did not know what the range or spread of FOF ratings would be – too narrow of a spread could hinder any extant effects from emerging. This did not appear to be an issue in Experiment 4, but may have hindered their potential effect when we failed to replicate the finding in Experiment 5. Participants in Experiment 5 did not provide their own findability ratings. Our incorporation of a memory test required us to select more obscure knowledge statements from the larger set of 299. Similar to Macias et al.'s (2015) stimuli, there may be a third variable associated with these items (such as sense of familiarity, or how interesting they are) that increased memory for more findable items. Experiment 5's failure to replicate left us with two perceived availability experiments suggesting contradictory findings and one null effect.

Future research on perceived availability effects would greatly benefit from a rigorous collection of reliable stimuli that maximally vary in their availability, as limited stimuli issues have had a hand in preventing us from drawing any concrete conclusions from the data.

Practical Implications & Future Directions

The current series of experiments provides evidence that when individuals have prior knowledge about the future availability of information, they can flexibly adjust their encoding strategies to reflect this knowledge (Experiments 3 and 5). We cannot confidently say if these effects were due to different amounts of selective rehearsal or differential attention. This could be a particularly fruitful avenue for future research. For example, if we are systematically downregulating our attentional states when we process information that we know is accessible, that could have more global implications for our information processing tendencies. There is preliminary evidence in support of a similar idea, which shows that merely interacting with information on a digital device predisposes someone to think on a more concrete level, whereas interacting with information on more traditional medium (i.e. books, magazines) predisposes one to think on a more abstract level (Kaufman & Flanagan, 2016).

Alternatively, if selective rehearsal is significantly contributing to digital availability effects, that implicates a more conscientious effort to act on the digital availability cue. In other words, one must choose to selectively rehearse some piece of information, whereas attentional modulation could vary more passively. A systematic investigation of the perceptual processes underlying digital availability-related memory effects could be particularly interesting as well, and could speak to variations in attention.

Our findings support the idea that individuals offload responsibility for information that is easily accessible in the future in favor of more focused processing of less accessible information. There has been a swell of articles and books in popular culture that have pointed to many of these findings and anticipated the downward spiral of human cognition (e.g., Carr, 2008). Alternatively, I propose that this offloading is an adaptive response. Through frequent and consistent interactions with information on our devices, our minds have learned to trust these information sources as reliable and useful. The affordances that these devices provide most likely free up cognitive resources that we can direct elsewhere to facilitate problem solving and increase creativity. This behavior is only problematic if we lose access to these information sources, or perhaps it could hinder our efforts if we are trying to gain expertise in a certain area.

With respect to our experiments in particular, we can rest easy knowing that our minds are not racing around and actively attempting to suppress or inhibit information that we read on our phones or in digital textbooks. On a mechanistic level, it is highly doubtful our memories are getting “worse.” Rather, we are carefully sorting information and prioritizing that which will be most useful in the future. We should also acknowledge that the sizes of the digital availability effects we present are quite modest. Recent interesting research from Risko and Dunn (2015) suggests that people are primarily motivated to offload information in cases where it will result in increased performance on a future task. This is the best-case scenario: We utilize technology when it can be to our advantage in the future, and rely on our internal systems when technology will not suffice.

Conclusion

Humans are well aware of their cognitive limitations, and these limitations are well defined (Nisbett & Ross, 1980). However, our ability to develop tools and harness resources in the world is beginning to outpace those limitations. New technologies are providing new affordances, and evidence is beginning to accumulate showing that our cognitive systems are adaptively capitalizing on these affordances.

In this thesis, we replicated previous research suggesting that memory is sensitive to the future availability of information that it encounters. Whereas previous researchers have suggested that availability-related memory reductions are comparable to traditional directed forgetting effects (Sparrow et al., 2011; Storm & Stone, 2015), our experiments revealed distinct memory patterns between these two processes that lead us to reject this comparison. Specifically, our data supports an encoding account of digital availability-related memory effects, showing that availability pre-cues, but not post-cues, were influential for subsequent memory. At the same time, our data seem to support an active forgetting account of directed forgetting, suggesting that the forgetting produced in this context was caused by effortful inhibition or suppression that occurred *after* encoding. Finally, we presented the field's first systematic evidence to suggest that digital availability-related findings are caused by differences in encoding; specifically, attention and selective rehearsal mechanisms. The current research makes an important contribution to our understanding of how cognition and memory are adapting to an increasingly dense information landscape, and has hopefully provided a number of jumping off points that will serve to accelerate research in the field and allow us to better understand how our memory system operates. A greater understanding of these mechanisms can empower us to design optimal learning curriculums, interfaces, and systems that would help society maximize the benefits and circumvent the deleterious effects of learning with technology.

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Appendix A: Experiment 1a & 1b post-questionnaire

What type of device did you use to complete this study?
- ❍ Desktop computer
- ❍ Laptop computer
- ❍ Tablet or iPad
- ❍ Smartphone
- ❍ Other ____________________

Did you encounter any trivia statements in the experiment that you knew in advance of the experiment?
- ❍ Yes
- ❍ No

Which trivia topic(s) did you know of in advance of the experiment?

You may have noticed that you were not given an opportunity to review the facts that you were told would be SAVED. While you were taking the experiment, did you believe that you would be able to review these facts before taking the memory test, as the instructions stated?
- ❍ I wasn't sure
- ❍ Yes
- ❍ No (If no, why didn't you believe the instructions?) ____________________

Did you notice any differences in the types of trivia facts that were presented?
- ❍ DURING the experiment, I noticed that some facts pertained to the elementary school and others were general knowledge
- ❍ AFTER the experiment (just now), I realized that some facts pertained to the elementary school and others were general knowledge
- ❍ I did not notice any differences among the trivia statements

(AttnCheck) In this study, we were interested in assessing how people implicitly utilize their memory for different types of information based on the future availability of that information. For example, perhaps people automatically remember information better when they don't believe they'll have access to it in the future. Select the second answer choice for this question. Also, people may remember information that is more interesting (i.e. general trivia facts) better than information that is less interesting (i.e. elementary school records). Which of the following choices do you feel best represented your personal memory strategy while you were completing this experiment?

- I remembered the "ERASED" facts best.
- I remembered the "SAVED" facts best.
- I remembered the elementary school-related facts best.
- I remembered the general knowledge trivia facts best.

What is your age?

Which gender do you identify with?

- Male
- Female
- Other

Ethnic category (Please select one of the two choices below)

- Hispanic or Latino. A person of Cuban, Mexican, Puerto Rican, South or Central American, or other Spanish culture or origin, regardless of race.
- Not Hispanic or Latino

Racial category (Please select one of the six choices below)

- American Indian or Alaskan native. (A person having origins in any of the original peoples of North America, and who maintains a cultural identification through tribal affiliation and community recognition)
- Asian. (A person having origins in any of the original peoples of the Far East, Southeast Asia, or the Indian subcontinent. This area includes, for example, Cambodia, China, India, Japan, Korea, Malaysia, Pakistan, the Philippine Islands, Thailand, and Vietnam)
- Native Hawaiian or Other Pacific Islander. (A person having origins in any of the original peoples of Hawaii, Guam, Samoa, or other Pacific Islands)
- Black or African American. (A person having origins in any of the black racial groups of Africa)
- White. (A person having origins in any of the original peoples of Europe, the Middle East, or North Africa).
- More than one race. (A person having origins that are a combination of 2 or more of the above categories).
- Check here if you do not wish to provide some or all of the above information.

Appendix B: Stimuli for Experiment 1a, 1b, 2, & 3

Borrowed with gratitude from Macias, Yung, Hemmer, and Kidd (2015)

Number	Lookupable Fact
1	The mathematical constant e is equal to 2.71828.
2	The shortest man in the world is from Nepal.
3	A fear of darkness is called achluophobia.
4	A fear of clowns is called coulrophobia.
5	A fear of sunlight is called heliophobia.
6	Emetophobia is a fear of vomiting.
7	Pogonophobia is a fear of beards.
8	Tokophobia is a fear of childbirth.
9	The longest time between two twins being born is 87 days.
10	The shortest gap between two siblings is 6 1/2 months.
11	The Ford motor company was founded in 1903.
12	Casu marzu is a cheese that contains live maggots.
13	Casu marzu is a specialty of Sardinia.
14	The first film containing an on-screen toilet flush was "Psycho".
15	The film "Psycho" was directed by Alfred Hitchcock.
16	The giant huntsman spider is the world's largest by leg-span.
17	Philippine tarsier has the highest eye-to-body size ratio of any mammal.
18	A Russian peasant set the record for most children birthed by a single woman.
19	The world record for most children birthed by a single woman is 69.
20	The world record for most children birthed by a single woman was set in 1765.
21	The top selling album of all times is "Thriller" by Michael Jackson.
22	The top-selling album of all time sold 42.4 million copies, according to music recording sales certification.
23	The Ford Focus is the most popular car in the world today.
24	Josephine Baker became the first African-American woman to star in a major motion picture.
25	The first major motion picture to star an African-American woman was ZouZou.
26	The first major motion picture to star an African-American woman came out in 1934.
27	The first major motion picture to star an African-American woman was directed by Marc Allégret.
28	The first major motion picture to star an African-American woman was French.
29	Apple was founded in 1976.
30	The smallest motobike ever produced weighed 1.1 kg.
31	The world record for the most number of children a father has ever had is 867.
32	The world's smallest fish is the paedocypris.
33	The world's smallest fish is from Indonesia.
34	The world's largest commercially available hamburger weighs 777 pounds.
35	Kakapo are endangered flightless birds that are native to New Zealand.
36	The most expensive art work ever sold at auction is "Three Studies of Lucien Freud".
37	September 16 is the most common birthday in the United States.
38	The world's shortest woman has a growth anomaly called achodroplasia.
39	The world's shortest woman is named Jyoti Amge.
40	Chandra Bahadur Dangi is the world's shortest man.
41	The world's shortest woman and the world's shortest man met for the first time in 2012.

42	Princesse Tam Tam was a popular black-and-white film that came out in 1935.
43	Shanghai, China is the most populous city in the world.
44	The top-selling album of 2013 was Justin Timberlake's "20/20 Experience".
45	Mumbai is the most populous city in India.
46	The most common Hindu last name is Das.
47	The world record for the longest hair was set by Xie Qiuping.
48	The person with the longest hair in the world is from China.
49	The longest hair in the world is 5.627 meters long.
50	The woman with the longest hair in the world started growing her hair in 1973.
51	Spider Man 3 is the most expensive movie ever made.
52	The most expensive movie ever made cost $258 million.
53	The woman with the longest dreadlocks in the world lives in Atlanta, Georgia.
54	Asha Mandela holds the record for the world's longest dreadlocks.
55	The longest dreadlocks in the world weight 25 pounds when wet.
56	The longest known case of hiccups in the world lasted for 68 years.
57	Charles Osborne holds the record for the longest recorded case of hiccups.
58	The advice column "Dear Abby" was first published in 1956.
59	The world's loudest ever recorded burp was 109.9 db.
60	The world's loudest ever recorded burp was achieved by Paul Hunn.
61	The world's loudest ever recorded burp was achieved on August 23, 2009.
62	The biggest baby ever born weighed 23 pounds, 12 ounces.
63	Kitti's hog-nosed bat is the world's smallest mammal according to skull size.
64	The Kitti hog-nosed bat weighs only 1.5 to 2 grams.
65	The Etruscan shrew is the smallest mammal by mass.
66	The Etruscan shrew weighs only 1.8 grams on average.
67	The Dumbo Octopus lives at more extreme depths than any other octopi.
68	The most popular ice cream flavor in the United States in vanilla.
69	The Honda Accord is the most commonly stolen car in the United States.
70	Royal Dandies are the smallest breed of pot-bellied pigs in the United States.
71	The pygmy marmoset is the world's smallest monkey species.
72	The world's smallest species of monkey are 5 inches long on average.
73	Royal Dandy pot-bellied pigs weigh 37.5 pounds on average.
74	The world record for the most magicians in a magic show is 106.
75	The world record for the most magicians in a magic show was achieved at the Landestheater Tübingen, Germany.
76	The world record for the most magicians in a magic show was achieved on March 4, 2012.
77	Tuber magnum pico (the white truffle) is the world's most expensive edible fungus.
78	The world's most expensive edible fungus is valued at $3,000 per kilo.
79	Charlie D'Angelo holds the record for the longest ice-cream man career.
80	The man who holds the record for the longest ice-cream man career is from Clifton, New Jersey.
81	Ann Atkin holds the record for the largest collection of gnomes and pixies in the world.
82	The world's largest collection of gnomes and pixies has 2,042 gnomes and pixies in it.
83	The world's largest collection of gnomes and pixies resides on a 4-acre "Gnome Reserve".
84	Charlotte Lee holds the record for the largest collection of rubber ducks in the world.
85	The world's largest collection of rubber ducks includes 5,631 different ones.
86	The world's largest collection of rubber ducks resides in Huntington Beach, California, in the United States.

87	The smallest living horse in the world is named Thumbelina.
88	The smallest living horse in the world is a miniature sorrell brown mare.
89	The smallest living horse in the world is 17.5 inches tall.
90	The smallest living horse in the world lives on Goose Creek Farm in St. Louis, Missouri.
91	The best-selling video game of all time was Tetris (1984)
92	Quvenzhané Wallis was the youngest ever Best Actress Oscar nominee.
93	The best-selling trading card game in the world is Yu-Gi-Oh!
94	The card game Yu-Gi-Oh! has sold more than 25 billion cards globally.
95	The most people riding unicycles simultaneously is 1,142.
96	The most people riding unicycles simultaneously happened on June 12, 2005.
97	The largest litter of puppies ever on record is 24.
98	The record for the largest litter of puppies was set by a Neapolitan mastiff.
99	The full name of the dog who set the record for the largest litter of puppies was Abellatino Arabella.
100	The first cartoon character to ever have been made into a balloon for a parade was Felix the Cat.
Number	**Nonlookupable Fact**
101	The code for the playground equipment locker is 6 - 0 - 4 - 6 - 8 - 4.
102	The smallest kid in 3rd grade is 3 feet tall.
103	The elementary-school art teacher is from Crane Beach.
104	The elementary-school music teacher is from Barbados.
105	The 3rd grader with the highest math grades is from Nigeria.
106	Half of the 3rd graders are afraid of snakes.
107	The elementary school team name is named the Blue Frogs.
108	The 3rd-grade class pet gerbil is named Fuzzbomb.
109	The elementary school choir is called the Sunshine Singers.
110	The school mascot is named Hoppy Harry.
111	Only two kids in 3rd grade are afraid of ghosts.
112	There are no twins in kindergarden at the elementary school.
113	One-third of the 3rd graders at the elementary school have at least one sibling.
114	The elementary school was founded in 1934.
115	The 3rd graders' favorite art material is clay.
116	The 3rd graders' favorite song to sing together is "Sweet Home Alabama".
117	The first international student who joined the 3rd grade class is from Croatia.
118	The favorite food of the only Croatian kid in class is macaroni and cheese.
119	The 3rd-grade class lizard is a green anoloe lizard.
120	Kyle Romero has the most hats of anyone in the 3rd grade class.
121	The 3rd-grade art class is learning about the surrealist art movement.
122	The elementary school record for long jump was set in 1972.
123	The most successful bake sale in school history was held to raise money for a new school gymnasium.
124	The marching band sold 167 cookies at their bake sale last month.
125	Grilled cheese is the most popular cafeteria lunch at the school.
126	Jennifer Tucker was the first student from our elementary school to get a starring role in a community theater play.
127	The first community theater play to star a student from the elementary school was "Matilda".
128	The first community theater play to star a student from the elementary school opened in May of 2012.

129	The first community theater play to star a student from the elementary school was directed by Alfonse Denis.
130	The first community theater play to star a student from the elementary school was a musical.
131	The parent-teacher association at the elementary school was founded in 1945.
132	The most snow days the elementary school ever had in one year was 8.
133	The total number of kids enrolled in the elementary school right now is 590.
134	The tallest 3rd grader this year is 5 feet and 0 inches tall .
135	The 3rd-grade spelling bee winner is named Sam Xu.
136	The 3rd-grade spelling bee winner is originally from Beijing.
137	The most popular piece of equipment on the playground is the see-saw.
138	The largest pizza party fundraiser at the elementary school this year was attended by 114 kids.
139	The most popular book in the elementary school library is "Harry Potter and the Goblet of Fire".
140	November 8 is "Daisy Day" at the elementary school.
141	Sharona Elvins is the shortest girl in the elementary school.
142	Isaiah Haber is the tallest kindergardener.
143	The two tallest boys in the 3rd grade have been friends with each other since pre-school.
144	The first production of "Oliver" at the elementary school was in 1973.
145	Cheese sticks are the most popular snack at our elementary school.
146	The best school assembly in the elementary school this year featured a group of Tamil Nadu folk dancers.
147	There are two 3rd graders at the elementary school named Emily.
148	There are 15 boys in the elementary school named Matt.
149	The largest 3rd grade class in the elementary school is taught by Mr. Lawson.
150	The most common last name in the elementary school is Brown.
151	The teacher who talks the fastest in the elementary school is Ms. Barbour-Adams.
152	The teacher who talks the fastest in the elementary school has lived in four different states.
153	The 3rd grader in the elementary school with the longest hair hasn't cut her hair since she was 4 years old.
154	Lasagna is the most expensive meal option in the school cafeteria.
155	The most expensive meal option in the school cafeteria cost $4.27.
156	The most abundant vegetable in the 3rd graders' garden is Swiss chard.
157	Brussels sprout is the least popular vegetable grown in the 3rd graders' garden.
158	The total weight of all the vegetables harvested from the 3rd graders' garden this year is 77 pounds.
159	The funniest case of hiccups in the 3rd grade happened during a field trip to the ballet.
160	The last 3rd grader to get hiccups at school was Jon Cabot.
161	The PTA newsletter has been distributed to parents during carpool since 1989.
162	The longest handstand held by a student at the elementary school was done by Lizzy Samiljian.
163	The longest handstand held by a student at the elementary school took place in the school atrium.
164	The longest handstand held by a student at the elementary school took place on April 14th, 2013.
165	The tallest teacher in the elementary school l is 7 feet
166	The cafeteria at the elementary school has a maximum capacity of 328 students.
167	The cafeteria at the elementary school has 41 tables.
168	The playground at the elementary school is covered with red mulch.
169	The longer slide on the elementary school playground is 17.5 feet.
170	Mr. Kissinger's class has read more books than any other class in the elementary school.
171	The most popular soda at the elementary school is Coca-Cola.
172	Soccer is the most popular team sport among 3rd graders at the elementary school.

173	The cheapest item at the elementary school bookstore is $0.15.
174	Ms. Darmon's class has the best attendance rate of all 3rd grade classes at the elementary school.
175	3rd grader Alex Everett has not missed a day of class this year.
176	The 3rd grade boys at the elementary school averaged 4 minutes, 49 seconds for the half-mile run.
177	Last year, 42 students performed in the elementary school talent show.
178	Last year, the winner of the elementary school talent show was Mackie Samuels.
179	The elementary school talent show took place in the Loggins Theatre.
180	The elementary school gymnasium got new wood flooring installed on November 14, 2013.
181	The most expensive piece of equipment in the 3rd-grade classroom is the Smart Board.
182	The most expensive piece of equipment in the 3rd-grade classroom cost $4,000.
183	The most expensive piece of equipment in the the 3rd-grade classroom was purchased from the EduYou Warehouse.
184	Sally Antonio won the ice-cream eating contest at the elementary school's Spring Carnival fundraiser.
185	The winner of the ice-cream eating contest at the elementary school's Spring Carnival was in 5th grade.
186	Bo Summers has the most Yu-Gi-Oh! cards of anyone in the 3rd grade at the elementary school.
187	The 3rd grader at the elementary school who has the biggest Yu-Gi-Oh! card collection has 1,516 cards.
188	The 3rd grader at the elementary school who owns the most Yu-Gi-Oh! cards keeps them in a blue and yellow Trapper Keeper.
189	Tamara Greene has the largest sticker collection of anyone in the elementary school.
190	The girl with the largest sticker collection in the elementary school has 756 pages of stickers in her collection.
191	The 3rd grader with the most shoes is Taylor Wolf.
192	The 3rd grader with the the most shoes owns 18 pairs.
193	The first gluten-free snack ever served in the elementary school was apples.
194	The most popular board game during recess at the elementary school is "Operation".
195	Molly Cogan was the first elementary school spelling bee champion who was in the 3rd grade.
196	The post popular playground equipment at the elementary school is the soccer ball.
197	The most kids anyone in 3rd grade has ever seen on the jungle gym at one time was 15.
198	The most kids anyone in 3rd grade has ever seen on a jungle gym at one time happened in March 2013.
199	The largest elementary school choir group has 34 kids in it.
200	There was a clown at the elementary school fair last year who made elephant-shaped balloon animals for the 3rd graders

Appendix C: Stimuli for Experiment 4 & 5

Borrowed with gratitude from Tauber, Dunlosky, Rawson, Rhodes, & Sitzman (2013)

Note: Right column indicates normed probability of recall as measured by Tauber et al. (2013)

N	***Answer***	$M_{ProbRec}$
What is the name of the avenue that immediately follows Atlantic Avenue in the game of Monopoly?	Ventnor	0.01
Which game uses a doubling cube?	Backgammon	0.01
The general named Hannibal was from what city?	Carthage	0.007
What is the name of the Indian college in Pennsylvania for which Jim Thorpe played football?	Carlisle	0.007
What is the name of the play in which Elwood P. Dowd is a character?	Harvey	0.007
What famous knot did Alexander the Great undo?	Gordian	0.007
What was the name of the unsuccessful auto that was manufactured by the Ford motor company from 1957–1959?	Edsel	0.007
In what profession was Emmett Kelly?	Clown	0.006
What is the name of the river that runs through Rome?	Tiber	0.006
Who was the first ruler of the Holy Roman Empire?	Charlemagne	0.005
What is the name of Roy Rogers' horse?	Trigger	0.005
What was the name of the nuclear submarine that sunk in the Atlantic in 1963?	Thresher	0.005
What is the name of the man who removed the thorn from the lion's paw in the story from Aesop's fables?	Androcles	0.005
What is the name of the brightest star in the sky excluding the sun?	Sirius	0.005
What is the name of the land of the giants in "Gulliver's Travels"?	Brobdingnag	0.005
What is the name of the largest desert on earth?	Antarctica	0
What is the name of Germany's largest battleship that was sunk in World War II?	Bismarck	0
What is John Kenneth Galbraith's profession?	Economist	0
What was the name of the union ironclad ship that fought the confederate ironclad Merrimack?	Monitor	0
What is the name of the mountain range that separates Asia from Europe?	Ural	0
What is the name of the instrument used to measure wind speed?	Anemometer	0
What is the name of the rubber roller on a typewriter?	Platen	0
What was the name of the largest confederate military prison during the civil war?	Andersonville	0
What is the name of the first movie to receive the Academy Award for best picture?	Wings	0
From what musical is the song "Baubles, Bangles and Beads"?	Kismet	0
What is the name of a number two wood in golf?	Brassie	0
What was the name of Gene Autry's horse?	Champion	0
What is the name of the town through which Lady Godiva supposedly made her famous ride?	Coventry	0
Who was the racehorse of the year for many successive years in the 1960s?	Kelso	0
What is the highest mountain in South America?	Aconcagua	0
What is the last name of the actor who portrayed Sergeant Friday on "Dragnet"?	Webb	0

What is the last name of the actor who portrayed the sheriff in the movie “High Noon”?	Cooper	0
What is the last name of the man who created the comic strip “Li’l Abner”?	Capp	0
What is the last name of Flash’s girlfriend in the comic strip “Flash Gordon”?	Arden	0
What is the last name of the man who was most responsible for photographing the U.S. civil war?	Brady	0
What is the last name of the man who was the voice of Mr. Magoo?	Backus	0
What was the last name of the ventriloquist who provided the voice for Charlie McCarthy?	Bergen	0
What is the last name of the pilot of the U-2 spy plane shot down over Russia in 1960?	Powers	0
What is the name of the author who received a Pulitzer prize for his writings about Abraham Lincoln?	Sandburg	0
What was the last name of Billy the Kid?	Bonney	0

Appendix D: Experiment 4 post-questionnaire

What type of device did you use to complete this study?
- ❍ Desktop computer
- ❍ Laptop computer
- ❍ Tablet or iPad
- ❍ Smartphone
- ❍ Other ____________________

Consider a typical day in your life. You may use the Internet for many reasons--reading/writing e-mails, checking social media, searching for information, looking up the weather, and so on. Approximately how many minutes per day do you spend using the Internet, total?

Approximately how many minutes per day do you spend using the Internet to look up information?

Please rate how strongly you agree with the following statements:

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
Information located on the Internet will be available forever	❍	❍	❍	❍	❍
Information located in printed materials will be available forever	❍	❍	❍	❍	❍

(AttnCheck) In this study, we were interested in assessing how people utilize their memory for different types of information, based on the future availability of that information. For example, perhaps people automatically remember information better when they don't believe they'll have easy access to it in the future. Which of the following choices do you feel best represented your personal memory strategy while you were completing this experiment?
- ❍ I prioritized memorizing the answers I felt were the HARDEST to find online
- ❍ I prioritized memorizing the answers I felt were the EASIEST to find online
- ❍ I tried to memorize all the answers equally
- ❍ Other (please briefly describe your strategy) ____________________

What is your age?

Which gender do you identify with?

- ❍ Male
- ❍ Female
- ❍ Other
- ❍ Decline to respond

Ethnic category (please select one of the two choices below):

- ❍ Hispanic or Latino. A person of Cuban, Mexican, Puerto Rican, South or Central American, or other Spanish culture or origin, regardless of race.
- ❍ Not Hispanic or Latino

Racial category (please select one of the six options below):

- ❍ American Indian or Alaskan native. (A person having origins in any of the original peoples of North America, and who maintains a cultural identification through tribal affiliation and community recognition)
- ❍ Asian. (A person having origins in any of the original peoples of the Far East, Southeast Asia, or the Indian subcontinent. This area includes, for example, Cambodia, China, India, Japan, Korea, Malaysia, Pakistan, the Philippine Islands, Thailand, and Vietnam)
- ❍ Native Hawaiian or Other Pacific Islander. (A person having origins in any of the original peoples of Hawaii, Guam, Samoa, or other Pacific Islands)
- ❍ Black or African American. (A person having origins in any of the black racial groups of Africa)
- ❍ White. (A person having origins in any of the original peoples of Europe, the Middle East, or North Africa).
- ❍ More than one race. (A person having origins that are a combination of 2 or more of the above categories).
- ❍ Check here if you do not wish to provide the above information.