

Expectancy-Based Modulations of Lag-1 Sparing and Extended Sparing During the Attentional Blink

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Over 20 years of research has shown that impairments in the identification of sequential targets—known as the *attentional blink*—are often eliminated when 2 or more targets are presented in direct succession. Such instances of lag-1 sparing and, more recently, extended sparing are widely attributed to the fact that directly successive visual inputs that possess common characteristics can be processed during an extended attentional window. Critically, this explanation implies that direct temporal succession and shared characteristics are *sufficient* to result in sparing. Here, I test this proposition directly by varying the probability of successive targets appearing during an experimental session. Across 5 experiments, I show that probability manipulations significantly affect the magnitude of sparing, with less sparing when the probability of a target appearing is relatively low. This outcome cannot be explained by resource shifts between targets, errors in perception of target order, or practice effects. Instead, the results suggest that sparing is determined not only by temporal contiguity and shared target characteristics but also by endogenous control processes that directly affect the duration of the attentional window.

Keywords: attentional blink, lag-1 sparing, endogenous control, expectancy

The term *visual attention* refers to a set of mechanisms that are collectively deployed to select and prioritize processing of a portion of incoming sensory information. The purpose of these mechanisms is protective: They regulate information flow to limited-capacity cognitive resources that are needed for memory, planning, and consciousness. One consequence of this selection process is that it severely limits our ability to perform simultaneous tasks or perceive multiple objects. This limitation can be seen quite clearly during common chores such as driving an automobile, in which simultaneously talking on a phone has been associated with elevated risk for serious accidents (Young & Regan, 2007) and frequent reports of perceptual and memory failures (Strayer, Drews, & Johnston, 2003; Strayer & Johnston, 2001).

In the laboratory, perceptual failures like those just noted have been shown in the context of many paradigms, such as change blindness (Di Lollo, Enns, & Rensink, 2000), inattention blindness (Mack & Rock, 1998), repetition blindness (Chun, 1997; Kanwisher, 1987; Morris & Harris, 2004), object substitution masking (Di Lollo et al., 2000; Goodhew, Visser, Lipp, & Dux, 2011), and the attentional blink (Raymond, Shapiro, & Arnell, 1992; for a review, see Dux, Asplund, & Marois, 2009). The present article focuses on the *attentional blink* (AB), which refers to a deficit in the identification of a second of two consecutive targets (T2) that usually arises when T2 follows an initial target (T1) within about half a second.

Conventional accounts of the AB have emphasized the importance of T1 processing (Chun & Potter, 1995; Jolicoeur & Dell'Acqua, 1998), suggesting that T1 occupies limited cognitive resources required for processing T2, thus leading to decay or masking (Giesbrecht & Di Lollo, 1998). On this account, one might expect that T2 performance would be poorest when it is presented directly after T1 (i.e., a temporal position referred to as *lag 1*). On the contrary, however, numerous studies have found that T2 performance under these conditions is actually far superior to performance when the two targets are separated by one (lag 2) or two (lag 3) distractors. This phenomenon has come to be known as *lag-1 sparing* (Potter, Chun, Banks, & Muckenhoupt, 1998; Visser, Bischof, & Di Lollo, 1999).

Initial explanations for lag-1 sparing focused on the notion that early visual processing stages acted as target template (Shapiro & Raymond, 1994), or “input filter” (Visser, Bischof, & Di Lollo, 1999, 2004), that controlled access to later resources. On these accounts, potentially relevant sensory information that matched target characteristics would open an attentional window (Weichselgartner & Sperling, 1987) that allowed this information access to limited-capacity resources. This window was then thought to close slowly, thereby allowing directly trailing items that shared T1 characteristics to access central resources. This account neatly explains the limited timeframe over which sparing occurs, and allows sparing and the AB to be reconciled within a logical framework. However, it also implies that the AB and lag-1 sparing are attributable to interrelated but clearly different underlying mechanisms (Visser, Bischof, & Di Lollo, 1999).

More recently, theories have attempted to reconcile sparing and the AB under a common explanatory framework. Much of this newer work was spawned by the surprising discovery of *extended sparing*, in which up to five consecutive target items could be reported with little decrease in accuracy (Di Lollo, Kawahara, Ghorashi, & Enns, 2005; Kawahara, Kumada, & Di Lollo, 2006;

This article was published Online First January 26, 2015.

This research was supported by Australian Research Council Discovery Grant DP120102313. I thank Karen Arnell, Elkan Akyurek, and an anonymous reviewer for helpful comments on an earlier version of this article.

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Olivers, van der Stigchel, & Hulleman, 2007). In light of these demonstrations, Di Lollo et al. (2005) proposed that both extended sparing and the AB could be explained by the assumption that processing T1 leads to a disruption in cognitive control over perceptual input filtering. This loss of control renders the filter vulnerable to being exogenously reset by stimuli presented after T1. On this notion, as long as stimuli following T1 match the target category, the properties of the input filter will remain unaffected, and multiple consecutive targets may be successfully identified up to the limits of short-term memory capacity. Conversely, when a nontarget is presented, it will reset the filter configuration so that it will no longer match target characteristics. This will impair processing of any subsequent targets until endogenous control over the filter can be reestablished (i.e., when T1 processing has been completed), thereby yielding an AB.

A number of other theories have also proposed a common processing architecture to account for sparing and the AB. For example, Olivers and Meeter (2008) suggested that sensory inputs that match a target template receive a transient attentional “boost” that ensures efficient processing. This boost is afforded to incoming items as long as they continue to match the template, thus accounting for extended sparing. However, when a distractor is presented that does not match the template, an inhibitory “bounce” occurs that prevents new sensory inputs from receiving attentional resources, thus leading to an AB. Similarly, the threaded cognition framework proposed by Taatgen, Juvina, Schipper, Borst, and Martens (2009) argues that the AB arises as a result of a control rule that prioritizes memory consolidation over target detection. On this account, as long as targets follow one another directly, detection (i.e., template matching) occurs before consolidation of T1 begins, thereby allowing new targets continued access to processing resources. Conversely, when T2 is separated from T1 by at least one distractor, consolidation of T1 is given a chance to begin, thereby preventing detection of T2 and yielding an AB.

Somewhat more detailed accounts of the mechanisms underlying sparing have been provided by the episodic simultaneous type, serial token (eSTST) model proposed by Wyble, Bowman, and Nieuwenstein (2009) and the temporal integration account of Akyürek and colleagues (Akyürek et al., 2012; Akyürek, Riddell, Toffanin, & Hommel, 2007; Akyürek, Toffanin, & Hommel, 2008; Hommel & Akyürek, 2005). The eSTST model proposes that a stimulus that matches a target template triggers a “blaster” mechanism that deploys transient attentional resources to enhance the representation of the potential target item. When potential targets appear consecutively, they each benefit from the transient enhancement generated by the previous item and extend the duration of blaster activity, resulting in lag-1 or extended sparing depending on the number of consecutive items. Conversely, when consecutive targets are separated by at least one nontarget distractor, this initial period of enhancement is followed within about 200 ms by an inhibitory period that overlaps with the presentation of T2, leading to the AB. In this model, target consolidation arises from a process of type–token binding (Kanwisher, 1987), with the speed of this binding for each target, and hence the perceived order of item presentation, depending on target complexity and representation strength.

The temporal integration account of Akyürek and colleagues (Akyürek et al., 2012; Akyürek, Riddell, et al., 2007; Akyürek et al., 2008; Hommel & Akyürek, 2005) suggests that sparing arises

from the combination of consecutive targets into a single cognitive episode, whereas the AB arises when targets separated by at least one distractor are encoded into separate cognitive episodes. On this account, consecutive targets are integrated into a single episode because of their close temporal proximity (Coltheart, 1980; Di Lollo, 1980; Dixon & Di Lollo, 1994), frequently resulting in a percept of a single stimulus with the combined contours of each of the targets (Akyürek et al., 2012; Hommel & Akyürek, 2005) and an accompanying inability to correctly report order of target presentation (*order errors*; e.g., Dell’Acqua, Jolicœur, Pascali, & Pluchino, 2007; Ferlazzo, Lucido, Di Nocera, Fagioli, & Sdoia, 2007; Hommel & Akyürek, 2005; Olivers & Nieuwenhuis, 2006; Visser, Boden, & Giaschi, 2004; Visser, Davis, & Ohan, 2009; Visser & Ohan, 2007). Conversely, the AB arises because targets are separated into different cognitive episodes, with consolidation of the T1 target episode interfering with consolidation of the T2 memory episode (for a broadly similar account, see also the simultaneous type, serial token theory of Bowman & Wyble, 2007).

The proposition that lag-1 sparing and extended sparing arise from a common source was recently affirmed by Visser and Ohan (2011), who showed that extended sparing occurred under the same presentation conditions as lag-1 sparing. For example, both types of sparing failed to occur when consecutive targets appeared in different spatial locations or when targets differed along multiple dimensions, such as stimulus class and associated task. These results support theoretical assertions of a common underlying mechanism. In addition, they affirm the general notion that sparing depends on commonalities among targets along multiple attributes and the need for consecutive targets uninterrupted by the presentation of a distractor. Put differently, the results establish that it is *necessary* for consecutive incoming items to match a common template or “input filter.”

Critically, for the present purposes, however, past demonstrations that sparing and extended sparing require consecutive items to pass a common target template does not establish that these conditions are *sufficient* for sparing to occur. This sufficiency assumption is present in all of the current theories of lag-1 and extended sparing just reviewed, except for the temporal integration account of Akyürek and colleagues (Akyürek et al., 2012; Akyürek, Riddell, et al., 2007; Akyürek et al., 2008; Hommel & Akyürek, 2005). However, the available evidence investigating this assumption is scant. In fact, several studies suggest that observer expectancy plays a significant role in modulating sparing, implying that the duration of sparing is mediated by low-level template matching processes (exogenous control) and higher level attentional control mechanisms (endogenous control).

One early experiment suggesting that a level of endogenous control could be exerted over sparing was conducted by Crebolder, Jolicœur, and McIlwaine (2002), who manipulated the probability of T1 being a particular target letter and observed the effect of this on a subsequent T2 presented at varying intertarget lags. They found that T2 performance on consecutive target trials was enhanced on trials when T1 was a highly probable target letter. However, it is unclear whether these results reflected mechanisms tied to sparing per se or a more general mechanisms, because T2 performance was uniformly improved across lags extending to approximately 800 ms after T1 onset.

Other early evidence for endogenous influences on sparing comes from the work of Jefferies, Ghorashi, Kawahara, and Di Lollo (2007; see also Jefferies & Di Lollo, 2009), who presented observers with two peripheral rapid serial visual presentation (RSVP) streams containing targets in either the same or different streams. When observers were told which stream contained T1, lag-1 sparing occurred only when targets were presented in the same stream (Visser, Bischof, & Di Lollo, 1999). However, when the location of T1 was unknown, sparing occurred regardless of the stream that T2 was presented in. To explain this pattern, Jefferies et al. suggested that observers who did not know the location of T1 adopted an attentional window that encompassed the spatial locations of both RSVP streams, whereas observers narrowed this focus to the stream containing T1 when they were informed about its location. This implies that expectations influenced the spatial extent of the attentional window. However, it does not speak to the issue of whether expectations can modify the duration of this window, thereby affecting the likelihood of identification of consecutive targets presented in the same spatial location.

The experiments of Akyürek et al. (2008) came closer to answering this question. In them, observers viewed a single, central RSVP stream presented at a constant stimulus onset asynchrony of 100 ms but with differing combinations of stimulus presentation duration and interstimulus interval so as to yield the appearance of a “fast” or “slow” RSVP. In addition, observers’ expectancies about RSVP speed were manipulated by varying the proportion of fast and slow trials within a block. There was strong evidence that this manipulation influenced the number of order errors at lag 1, leading Akyürek et al. (2008) to propose that the attentional window was subject to endogenous control. Critically, however, there was little reliable evidence that T2 accuracy was affected by their experimental manipulation. Thus, it is unclear whether expectations directly modified the likelihood that consecutive targets could be accurately identified (i.e., the duration of the window), or affected only judgments of target presentation order (i.e., after selection had occurred).

In light of this uncertainty about whether endogenous control can directly influence identification of consecutive targets, I conducted a series of experiments in which I manipulated the proportion of trials that contained consecutive target items. In the first experiment, I compared lag-1 sparing for consecutive targets presented on either two-thirds or one-third of trials, thereby varying the probability of the final target in the sequence. I reasoned that if template matching were sufficient for sparing to occur, then it should occur equally, regardless of the probability of the final target. Conversely, reducing the proportion of consecutive target trials might reduce the likelihood of sparing if endogenous attentional control mechanisms also play a role in sparing. I repeated this procedure in the second experiment, examining trials with three consecutive targets to generalize the results to extended sparing. Subsequent experiments addressed the temporal specificity of observer expectations and determined whether expectations could influence sparing in the absence of an appropriately localized attentional window when targets were presented in different spatial locations.

In addition to examining target accuracy in each experiment, I also assessed the proportion of order errors made under the different expectancy conditions. As noted earlier, the temporal integration account of Akyürek and colleagues (Akyürek et al., 2012;

Akyürek, Riddell, et al., 2007; Akyürek et al., 2008; Hommel & Akyürek, 2005) predicts a strong association between consecutive target accuracy and proportions of order errors. Thus, consideration of both of these variables may aid significantly in adjudicating between theories of the mechanisms underlying sparing. To wit, although evidence for expectancy effects on sparing would be consistent with endogenous control over the duration of the attentional window, the absence of accompanying order effects would suggest that the root cause of order errors may be decoupled from the mechanisms underlying sparing, at least under some experimental conditions.

Experiment 1

Method

Participants. Thirty-six participants were recruited from an introductory psychology course through advertisements posted on Web-based software. Twenty participants (11 female) were in the low-probability T2 condition, whereas 16 participants (seven female) were in the high-probability T2 condition (see details presented later). The average age of participants was 21.53 years (range: 17–52). Informed consent was obtained from all participants, per standard ethical guidelines. All participants received credit toward their grade to compensate them for their time and effort, reported normal or corrected-to-normal vision, and were naïve as to the purposes of the experiment.

Apparatus and stimuli. Stimuli were presented on a 19-in. Dell (Round Rock, TX) CRT monitor (Model M992) running at a refresh rate of 100 Hz attached to a Pentium (Intel Corporation, Santa Clara, California) computer running Presentation software (Version 16.3; Neurobehavioral Systems, Austin, TX). The software was also responsible for recording responses from a computer keyboard.

Testing was conducted in a quiet, dimly lit laboratory room. All stimuli subtended a visual angle of approximately 1° at a viewing distance of 60 cm. Targets were shown in Arial font (28 point; red, green, and blue [RGB]: 70, 70, 70) and consisted of the Arabic numerals from 1 to 9. Distractors consisted of all letters of the English alphabet except I, O, P, Q, and Z, which were omitted because of their structural similarity to the digits 1, 0, 2, and 7. Distractor letters were shown in uppercase Arial font (28 point; RGB: 70, 70, 70).

Procedure. The experiment comprised 450 trials presented in a single self-paced block. In the low-probability T2 condition, 150 trials included two consecutive targets, whereas the remaining 300 trials included only a single target. Thus, in this condition, T2 followed T1 on one third of trials. In the high-probability T2 condition, these proportions were reversed, such that T2 followed T1 on two thirds of trial. Participants were not informed about the probability of two targets appearing on a trial and were simply instructed to report all of the digits that appeared at the end of each trial.

All trials began with a fixation cross presented at the center of the screen. Participants focused their gaze at fixation and pressed the space bar to initiate a trial. Following a 250-ms pause during which the display was blank, a sequence of five to eight single-letter distractors was displayed in the center of the screen. Depending on the trial type, distractors were then followed by one or

two consecutive targets. The last item on the trials was always a single-letter distractor presented at the same spatial location as the final target. This item acted as a mask for T2. Targets and distractors were presented for 20 ms and separated from the next item by an 80-ms blank display. Targets were chosen randomly without replacement. Distractors were chosen randomly with replacement, with the proviso that identical distractors were never presented in succession.

After the mask disappeared, there was a 200-ms blank display, followed by prompt(s) (e.g., *1st number?*) that signaled participants to report each of the digits that had been presented by pressing an appropriate key on the keyboard. There was one prompt on trials with a single target, and there were two prompts on trials with two targets. Participants were asked to report targets in the order they had been presented. They were also instructed to guess if they were not sure of a target's identity because a response was required to each digit. Once their last response had been made, the fixation cross reappeared, and participants began the next trial at their leisure by pressing the space bar.

Results

Single-target trials. Mean accuracy on single-target trials was 93.87% in the low-probability T2 condition and 94.08% in the high-probability T2 condition. This difference was not significant, $t(34) = 0.10$, $p = .92$, $r_{pb}^2 < .001$.

Dual-target trials. Mean T2 accuracy was calculated only for trials on which T1 was identified correctly and did not consider response order. Mean accuracy scores for both targets were submitted to a 2 (Target: T1 vs. T2) \times 2 (T2 Probability: high vs. low) mixed-design analysis of variance (ANOVA). This revealed significant main effects of target, $F(1, 34) = 16.74$, $p < .001$, $\eta_p^2 = .33$; T2 probability, $F(1, 34) = 7.15$, $p = .011$, $\eta_p^2 = .17$; and the Target \times T2 Probability interaction, $F(1, 34) = 21.14$, $p < .001$, $\eta_p^2 = .38$. As can be seen in Figure 1, this interaction reflects the fact that increasing the proportion of trials with two targets had little effect on T1 but significantly improved T2 accuracy. Consistent with this impression, T1 accuracy was statistically indistinguishable across changes in the probability of two-target trials, $t(34) = 0.90$, $p = .38$, $r_{pb}^2 = .02$. However, T2 accuracy was significantly greater in the high-probability condition than in the low-probability condition, $t(34) = 3.56$, $p = .001$, $r_{pb}^2 = .27$. This result suggests that the expectation that T2 would appear, fostered by the probability manipulation, influenced the likelihood that T2 would be spared. In turn, this implies that the extent of T2 processing when it directly follows T1 is determined not solely by whether T2 matches the template settings established on the basis of the characteristics of T1 but also depends on endogenous mechanisms that are tuned to characteristics such as the likelihood of target appearance.

Order errors. Following the approach of Akyürek et al. (2008), I calculated the mean percentage of trials on which observers reported the order of T1 and T2 incorrectly separately as a function of the probability of dual-target trials and as a percentage of either all dual-target trials or only dual-target trials on which both targets were reported correctly. Independent-groups t tests were then conducted to compare the percentages of order errors as a function of the probability of dual-target trials. These comparisons were not significant either when considering all trials (low

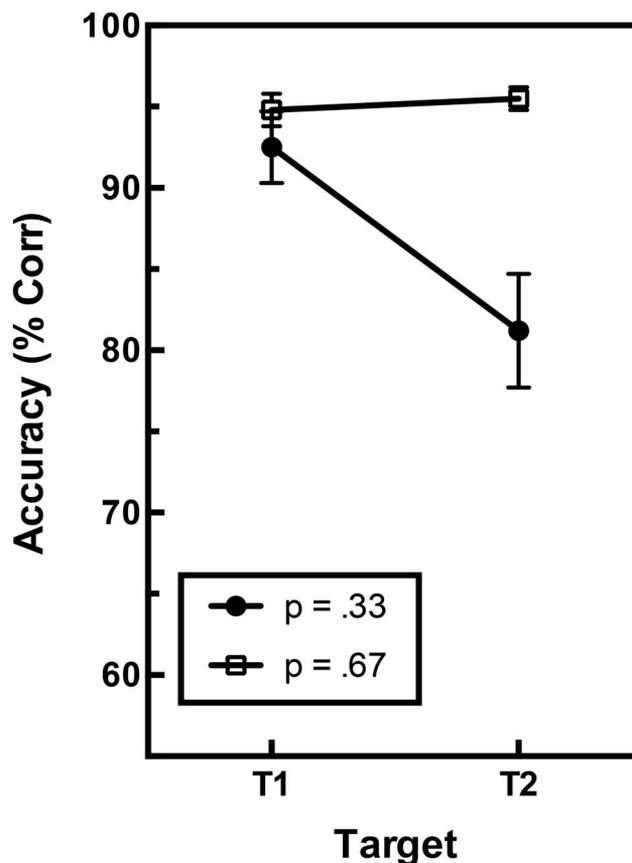


Figure 1. Target accuracy for the first (T1) and second (T2) targets on two-target trials in Experiment 1, separated by the probability of T2 appearing on a trial. Filled circles represent performance in the low-probability T2 condition, in which T2 appeared on one-third of trials. Open squares represent performance in the high-probability T2 condition in which T2 appeared on two thirds of trials. Error bars represent 1 standard error of the mean. Corr = correct.

probability: 9.43%; high probability: 10.85%), $t(34) = 0.82$, $p = .42$, $r_{pb}^2 = .02$, or only trials on which both targets were identified correctly (low probability: 13.01%; high probability: 12.09%), $t(34) = 0.42$, $p = .68$, $r_{pb}^2 = .01$. These results suggest that although top-down mechanisms may have modulated the likelihood of T2 entering the same attentional window as T1, this did not produce concomitant changes in order errors as would be predicted by the temporal integration account.

Before pursuing the hypothesis that probability modulated endogenous control over the attentional window, however, it is important to consider other potential explanations for the importance of probability. One option, in particular, is that T2 performance was superior in the high-probability condition because there were simply more trials and, hence, opportunities for practice. To check on this option, I reanalyzed the data from the high-probability T2 condition, considering only the first 150 trials that contained two targets. In this way, the same numbers of trials contributed to both low- and high-probability T2 accuracy scores. Submitting these new means to the analyses described earlier yielded an identical pattern of results. T1 accuracy scores were

statistically indistinguishable across variations in the probability of two-target trials both when only one target was presented (high: 93.92%; low: 93.87%), $t(34) = 0.02$, $p = .98$, $r_{pb}^2 < .001$, and when two targets were presented (high: 94.63%; low: 92.53%), $t(34) = 0.79$, $p = .44$, $r_{pb}^2 = .02$. Critically, however, T2 scores were still significantly lower on low-probability T2 trials than on high-probability T2 trials (high: 95.34%; low: 81.15%), $t(34) = 3.48$, $p = .001$, $r_{pb}^2 = .26$.

Another possible explanation for the results here is that they reflect a strategic shift in allocation of attentional resources between T1 and T2. On this account, observers allocated proportionally more resources to T1 when the probability of two-target trials was lower. As a result, T2 processing was disadvantaged on trials on which T2 did appear, resulting in lower identification accuracy. A finding similar to this was reported by Dux et al. (2009), who compared target identification across two groups who viewed three targets on every trial. In one group, T1 identification was required on 100% of trials (T2 and a third target [T3] were reported on 50% of trials), whereas in the other, T1 identification was required on 50% of trials (T2 was reported on 50% of trials, and T3 was reported on 100% of trials). A comparison of the groups showed higher T1 performance and lower T3 performance in the group in which T1 always required identification and the opposite pattern of performance in the group that required T1 identification on only half of trials. Dux et al. attributed this result to resource tradeoffs between T1 and T3 arising from changes in the requirement to report T1 and T3 across conditions.

Despite the surface similarities between the present results and those of Dux et al. (2009), there are also key differences that argue against a resource allocation-based interpretation of the present findings. The first key difference is that T1 was equally relevant across both conditions in this experiment. That is, a priori, there is no reason to expect differential allocation of resources to T1 on the basis of target relevance, as was the case in Dux et al. Second, and more important, there is also no evidence of a resource shift in the present data. Whereas Dux et al. found that improvements in T3 accuracy were mirrored by declines in T1 performance, T1 performance in the present results was unchanged across changes in T2 accuracy. This was true both when T1 was the only target and when T1 was followed by a subsequent target. Thus, the evidence strongly suggests that changes in T2 performance were a result of top-down modulations of mechanisms underlying sparing rather than resource allocation.

Given the strong initial evidence that probability modulated the duration of the attentional window rather than led to a resource shift between targets, it is important to verify that similar patterns of results are found with more than two consecutive targets. As noted earlier, many AB models now posit that common mechanisms can account for lag-1 sparing, extended sparing, and the AB. Thus, one would expect that the impact of target probability on extended sparing should be the same as on conventional lag-1 sparing. This expectation is bolstered by the results of Visser and Ohan (2011), who showed both types of sparing arise under identical presentation conditions.

To test this prediction, in Experiment 2, I presented observers with two or three consecutive targets to be identified. The first two targets were presented on every trial, whereas across two different groups, the probability of the third target being presented was varied in the same manner as in Experiment 1. If endogenous

control processes modulate extended sparing in the same fashion as in lag-1 sparing, then one would expect T3 performance to be better in the condition in which presentation of three-target trials is more likely. Moreover, if probability directly modulates the likelihood of T3 passing the input filter established for earlier targets rather than leading to changes in resource allocation, then one would expect T1 and T2 accuracy to be unaffected by the probability manipulation.

Experiment 2

Method

Participants. Twenty-eight participants were recruited from an introductory psychology course through advertisements posted on Web-based software. Sixteen participants (11 female) were in the low-probability three-target condition, whereas 12 participants (seven female) were in the high-probability three-target condition. The average age of participants was 17.57 years (range: 17–19). Informed consent was obtained from all participants, per standard ethical guidelines. All participants received credit toward their grade to compensate them for their time and effort, reported normal or corrected-to-normal vision, and were naïve as to the purposes of the experiment.

Apparatus and stimuli. Stimuli were presented on a 19-in. Acer (Mississauga, Ontario, Canada) CRT monitor (Model AC 716) running at a refresh rate of 100 Hz attached to a Pentium computer running Presentation software (Version 11.20). Stimuli were identical to those in Experiment 1, except that all items were presented in light gray (RGB: 170, 170, 170).

Procedure. The experiment comprised 300 trials presented in a single self-paced block. In the low-probability three-target condition, 100 trials included three consecutive targets, whereas the remaining 200 trials included two targets. These two targets were equally likely to be presented consecutively or separated by the presentation of a single distractor. In the high-probability three-target condition, 200 trials consisted of three consecutive trials, whereas the remaining 100 trials had two targets that were equally likely to be presented consecutively or separated by a single distractor. All other aspects of the procedure were identical to the procedure in Experiment 1.

Results

Dual-target trials (consecutive targets). Mean T2 accuracy was calculated only for trials on which T1 was identified correctly. Mean accuracy scores for both targets were submitted to a 2 (Target: T1 vs. T2) \times 2 (T3 Probability: high vs. low) mixed-design ANOVA. This revealed a significant main effect of target, $F(1, 26) = 10.19$, $p = .004$, $\eta_p^2 = .28$, with overall accuracy greater on T2 than T1 (87.43% vs. 82.72%). This is consistent with the presence of lag-1 sparing. No other main effects or interactions were significant ($ps > .38$, $\eta_p^2s < .03$).

Dual-target trials (targets separated by distractor). Mean T2 accuracy and identification performance was analyzed in the same fashion as on consecutive-target trials. The ANOVA revealed a significant main effect of target, $F(1, 26) = 75.44$, $p < .001$, $\eta_p^2 = .74$, with overall accuracy lower on T2 than T1 (63.84% vs. 85.15%). This is consistent with the presence of a robust AB.

No other main effects or interactions were significant ($ps > .30$, $\eta_p^2 < .05$).

Three-target trials. Mean T3 accuracy was calculated only on trials on which both T1 and T2 were identified correctly. Mean accuracy scores for all three targets were submitted to a 3 (Target: T1, T2, T3) \times 2 (T3 Probability: high vs. low) mixed-design ANOVA. This revealed a significant main effect of target, $F(2, 52) = 58.93$, $p < .001$, $\eta_p^2 = .69$; a marginal main effect of T3 probability, $F(1, 26) = 4.07$, $p = .054$, $\eta_p^2 = .14$; and a Target \times T3 Probability interaction, $F(2, 52) = 6.21$, $p = .004$, $\eta_p^2 = .19$. As can be seen in Figure 2, the interaction reflects the fact that increasing the proportion of trials with three targets had little effect on T1 or T2 but significantly improved T3 accuracy. Consistent with this impression, neither T1 accuracy, $t(26) = 0.63$, $p = .54$, $r_{pb}^2 = .02$, nor T2 accuracy, $t(26) = 1.30$, $p = .20$, $r_{pb}^2 = .06$, varied significantly across levels of T3 probability. Critically, however, T3 accuracy was significantly greater in the high-probability condition than in the low-probability condition, $t(26) = 3.17$, $p = .004$, $r_{pb}^2 = .28$. This result parallels the findings obtained in Experiment 1 and, thus, adds additional support to theories that

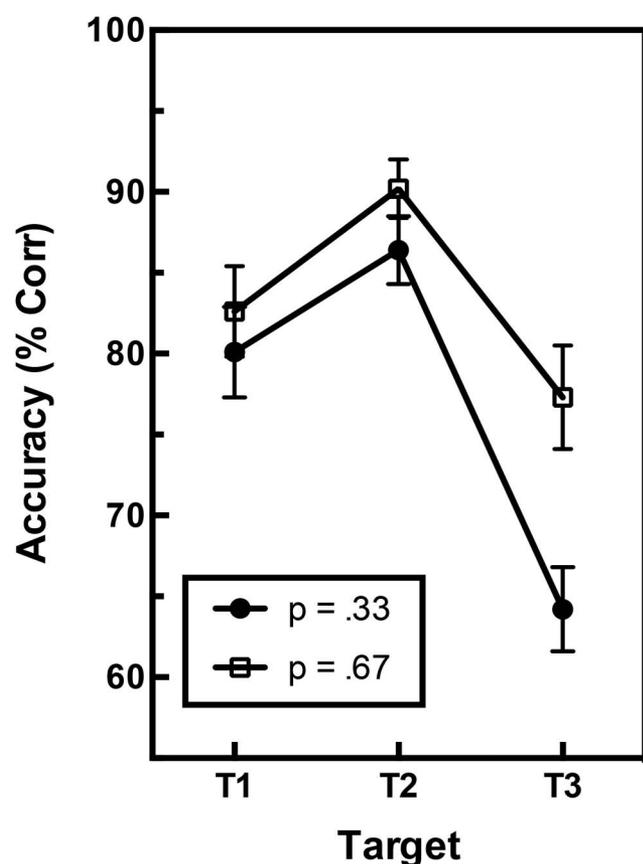


Figure 2. Target accuracy for the first (T1), second (T2), and third (T3) targets on three-target trials in Experiment 2, separated by the probability of T3 appearing on a trial. Filled circles represent performance in the low-probability T3 condition, in which T3 appeared on one-third of trials. Open squares represent performance in the high-probability T3 condition, in which T3 appeared on two-thirds of trials. Error bars represent 1 standard error of the mean. Corr = correct.

suggest lag-1 sparing and extended sparing arise from common mechanisms. More important, the results also indicate that extended sparing depends both on whether targets can pass the same input filter because of shared characteristics and whether observers anticipate the appearance of a target.

Indeed, the importance of expectations is highlighted by a comparison of T2 performance in the condition in which two targets appeared, separated by a single distractor, and T3 performance when there were three consecutive targets. Previous studies (e.g., Di Lollo et al., 2005) have shown that although the final target appears in the same temporal position in both of these conditions, T3 identification is reliably more accurate than T2 identification (extended sparing). However, I found no T3 advantage when the probability of three targets was low, $t(16) = 1.15$, $p = .268$, $r_{pb}^2 = .08$. This advantage was only evident when the probability of three targets was high, $t(12) = 5.45$, $p < .001$, $r_{pb}^2 = .73$.

Order errors. Using the procedure outlined in Experiment 1, I again examined whether the present manipulation of the probability of three-target trials influenced order errors on either two-target or three-target trials. On dual-target trials when targets were separated by a distractor, comparisons of order errors across levels of three-target trial probability were not significant either when considering all trials (low probability: 6.41%; high probability: 5.18%), $t(26) = 0.67$, $p = .51$, $r_{pb}^2 = .03$, or only trials on which both targets were identified correctly (low probability: 14.63%; high probability: 10.11%), $t(26) = 1.07$, $p = .30$, $r_{pb}^2 = .01$. On dual-target trials with consecutive targets, comparisons of order errors across levels of three-target trial probability were again nonsignificant, either when considering all trials (low probability: 25.78%; high probability: 26.89%), $t(26) = 0.26$, $p = .80$, $r_{pb}^2 < .01$, or only trials on which both targets were identified correctly (low probability: 37.86%; high probability: 34.88%), $t(26) = 0.48$, $p = .63$, $r_{pb}^2 = .01$.

On three-target trials, I calculated mean order errors separately for T1–T2 order errors, T1–T3 order errors, T2–T3 order errors, and order errors in which all three targets were reported in the wrong temporal positions (all-target order errors). These means can be seen in Table 1 and were entered into a 4 (Order Error: T1–T2, T–T3, T2–T3, all) \times 2 (Trials: all vs. correct only) \times 2 (T3 Probability: high vs. low) mixed-design ANOVA to determine whether probability affected the percentage of any of these types of order errors. The analysis revealed main effects of order error, $F(3, 78) = 18.64$, $p < .001$, $\eta_p^2 = .42$, indicating there were fewer T1–T3 order errors than the other types, and trials, $F(1, 26) = 102.72$, $p < .001$, $\eta_p^2 = .80$. There was also an Order Error \times Trials interaction, $F(3, 78) = 8.58$, $p < .001$, $\eta_p^2 = .25$, indicating that the difference between calculating error means for all three-target trials compared with only correct three-target trials was smaller for T1–T3 order errors than for the other three types. Importantly, however, there were no other main effects or interactions ($F_s < 3.10$, $ps > .09$, $\eta_p^2 < .11$). In sum, as in Experiment 1, there was no indication that changes in the probability of three-target trials influenced the number of order errors in any condition.

As in the first experiment, it is possible that the accuracy results here were attributable to more extensive practice on three-target trials, which made up a considerably larger proportion of trials in the high-probability condition. To check on this option, I reana-

Table 1
Percentages of Different Types of Order Errors on Three-Target Trials in Experiment 2

Type of order error	T3 probability			
	Low		High	
	All trials	Corr. trials only	All trials	Corr. trials only
T1-T2	6.4 (0.9)	16.0 (1.8)	9.4 (1.0)	16.9 (2.1)
T1-T3	2.3 (0.4)	7.4 (1.9)	2.6 (0.5)	5.0 (2.2)
T2-T3	8.2 (1.1)	19.6 (1.7)	11.0 (1.3)	18.7 (1.9)
All	5.9 (0.9)	14.3 (1.8)	8.7 (1.0)	15.5 (2.1)

Note. Numbers in parentheses represent 1 standard error of the mean. T = target; Corr. = correct.

lyzed data from the high-probability T3 condition using only the first 100 trials to equate the overall number of three-target trials across probability levels. This new analysis showed an identical pattern of results on two-target trials, with significant lag-1 sparing when two targets were presented consecutively, $F(1, 26) = 6.48$, $p = .017$, $\eta_p^2 = .20$, and a significant AB when the two targets were separated by a single distractor, $F(1, 26) = 58.85$, $p < .001$, $\eta_p^2 = .69$. Importantly, results were also identical on three-target trials, with a significant Target \times T3 Probability interaction, $F(2, 52) = 7.55$, $p = .001$, $\eta_p^2 = .23$. Again, neither T1, $t(26) = 0.35$, $p = .73$, $r_{pb}^2 < .01$, nor T2, $t(26) = 0.52$, $p = .61$, $r_{pb}^2 = .01$, accuracy changed across T3 probability levels. However, T3 accuracy was significantly higher in the high-probability T3 condition, $t(26) = 2.78$, $p = .01$, $r_{pb}^2 = .23$.

It should also be noted that there was no indication in this experiment that changes in T3 accuracy could be attributed to a shift of resources to T1. As in Experiment 1, T1 report was required on all trials across both conditions. Moreover, T1 accuracy was unchanged across variations in the probability of three-target trials, both when presented only along with T2 and when presented with T2 and T3. This result cannot be attributed to a performance ceiling, as might have been the case in Experiment 1, because overall T1 accuracy was about 10% lower than in Experiment 1 and well below ceiling level.

In the first two experiments, the probability of the final item in a multiple-target sequence (T2 in Experiment 1, T3 in Experiment 2) was always manipulated. However, if the results of this manipulation reflect endogenous control exerted over the duration of the attentional window, then one would predict that changes in the probability of the second item in a three-target sequence could also influence sparing for T2. The evidence thus far for this prediction is limited. In Experiment 2, for example, the increase in the probability of three-target trials across conditions also increased the probability of trials with at least two targets (from .67 to .83). However, this did not improve T2 accuracy either on dual-target trials with consecutive targets or as part of a three-target sequence. That said, the change in the probability of trials with at least two consecutive targets was relatively small compared with the change for three-target trials and, thus, might not have been sufficient to influence performance. To examine this question more closely, in Experiment 3 I presented two types of trials: dual-target trials with two targets separated by a single distractor and three-target trials. For one group of observers, dual-target trials occurred on two-

thirds of trials, and three-target trials occurred on the rest; for the other group of observers, these proportions were reversed. As a result of this setup, the probability of being presented with at least two consecutive targets or three consecutive targets varied identically (.33 vs. .67) across both conditions.

Experiment 3

Method

Participants. Thirty-nine participants were recruited from an introductory psychology course through advertisements posted on Web-based software. Eighteen participants (13 female) were in the low-probability three-target condition, whereas 21 participants (15 female) were in the high-probability three-target condition. The average age of participants was 19.79 years (range: 16–43). Informed consent was obtained from all participants, per standard ethical guidelines. All participants received credit toward their grade to compensate them for their time and effort, reported normal or corrected-to-normal vision, and were naive as to the purposes of the experiment.

Apparatus and stimuli. Stimuli were presented on a 24-in. BenQ (Costa Mesa, CA) LED monitor (Model XL2420TE) running at a refresh rate of 100 Hz attached to a Pentium computer running Presentation software (Version 17). Stimuli were identical to those in Experiment 2.

Procedure. The experiment comprised 450 trials presented in a single self-paced block. In the low-probability three-target condition, 150 trials included three consecutive targets, whereas the remaining 300 trials included two targets separated by the presentation of a single distractor. In the high-probability three-target condition, these proportions were reversed. All other aspects of the procedure were identical to those in Experiment 2.

Results

Dual-target trials (targets separated by distractor). Mean T2 accuracy was calculated only for trials on which T1 was identified correctly. Mean accuracy scores for both targets were submitted to a 2 (Target: T1 vs. T2) \times 2 (T3 Probability: high vs. low) mixed-design ANOVA. The ANOVA revealed a significant main effect of target, $F(1, 37) = 90.32$, $p < .001$, $\eta_p^2 = .71$, with overall accuracy lower on T2 than T1 (76.29% vs. 95.15%). This is consistent with the presence of a robust AB. No other main effects or interactions were significant ($ps > .22$, $\eta_p^2 < .04$).

Three-target trials. Mean T3 accuracy was calculated only for trials on which both T1 and T2 were identified correctly. Mean accuracy scores for all three targets were submitted to a 3 (Target: T1, T2, T3) \times 2 (T3 Probability: high vs. low) mixed-design ANOVA. This revealed a significant main effect of target, $F(2, 74) = 87.30$, $p < .001$, $\eta_p^2 = .70$; a main effect of T3 probability, $F(1, 37) = 6.25$, $p = .017$, $\eta_p^2 = .14$; and a Target \times T3 Probability interaction, $F(2, 74) = 4.77$, $p = .011$, $\eta_p^2 = .11$. As can be seen in Figure 3, the interaction reflects the fact that increasing the proportion of trials with three targets had little effect on T1 but benefitted both T2 and T3 accuracy. Consistent with this impression, T1 accuracy did not vary with the proportion of three-target trials, $t(37) = 0.63$, $p > .16$, $r_{pb}^2 = .01$. However, T2 accuracy, $t(37) = 2.16$, $p = .037$, $r_{pb}^2 = .11$, and T3 accuracy, $t(37) = 2.65$,

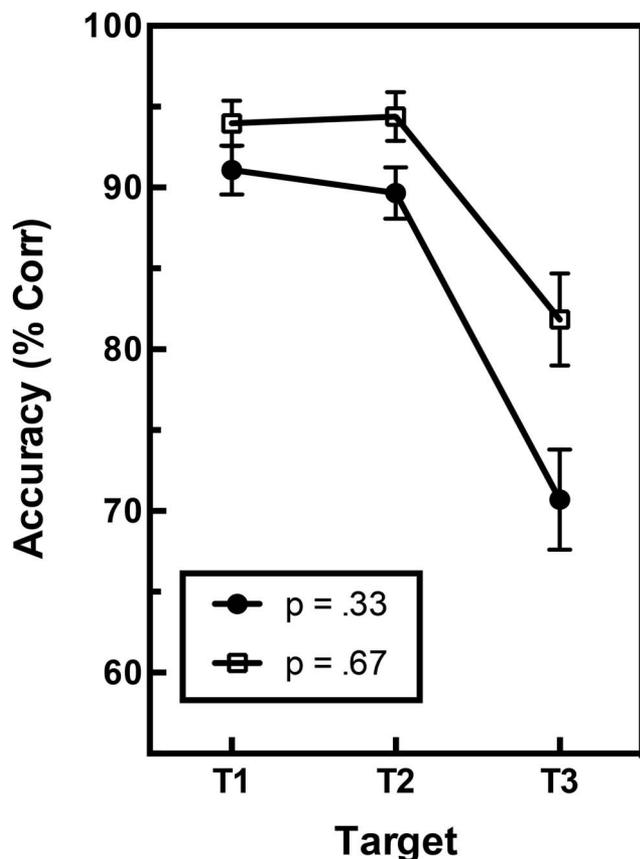


Figure 3. Target accuracy for the first (T1), second (T2), and third (T3) targets on three-target trials in Experiment 3, separated by the probability of three-target trials. Filled circles represent performance in the low-probability three-target condition, in which three consecutive targets appeared on one-third of trials. Open squares represent performance in the high-probability three-target condition, in which T3 appeared on two-thirds of trials. Error bars represent 1 standard error of the mean. Corr = correct.

$p = .012$, $r_{pb}^2 = .16$, were higher when three-target trials were more frequent, supporting the notion that probability led to changes in the duration of the attentional window, thereby mediating sparing.

Order errors. On dual-target trials, comparisons of order errors across levels of three-target trial probability were nonsignificant either when considering all trials (low probability: 2.50%; high probability: 2.34%), $t(37) = 0.24$, $p = .81$, $r_{pb}^2 < .01$, or only trials on which both targets were identified correctly (low probability: 4.78%; high probability: 3.72%), $t(37) = 0.61$, $p = .55$, $r_{pb}^2 = .01$.

On three-target trials, I calculated mean order errors separately for T1–T2, T1–T3, T2–T3, and all-target order errors. These means can be seen in Table 2 and were entered into a 4 (Order Error) \times 2 (Trials: all vs. correct only) \times 2 (T3 Probability: high vs. low) mixed-design ANOVA to determine whether probability of three-target trials affected the percentage of any of these types of order errors. The analysis revealed significant main effects of order error, $F(3, 111) = 152.62$, $p < .001$, $\eta_p^2 = .81$, and trials, $F(1, 37) = 58.33$, $p < .001$, $\eta_p^2 = .61$. There were also significant interactions: Order Error \times Trials, $F(3, 111) = 56.99$, $p < .001$,

$\eta_p^2 = .61$; Order Error \times T3 Probability, $F(3, 111) = 4.01$, $p = .009$, $\eta_p^2 = .10$; Trials \times T3 Probability, $F(1, 37) = 5.02$, $p = .031$, $\eta_p^2 = .12$; and Order Error \times Trials \times T3 Probability, $F(3, 111) = 10.44$, $p < .001$, $\eta_p^2 = .22$. The main effect of T3 probability was not significant, $F(1, 37) = 0.91$, $p > .34$, $\eta_p^2 = .02$. Inspection of Table 2 suggests that increases in the probability of three-target trials tended to increase T1–T2 and T1–T3 order errors but decrease T1–T3 (particularly when conditional on correct identification of all targets) and all-target order errors. Follow-up analyses comparing each type of order error across the different probabilities of three-target trials indicated that T2–T3 order errors on trials in which all targets were correctly identified decreased significantly with increases in three-target trial probability, $t(37) = 2.77$, $p = .009$, $r_{pb}^2 = .17$. However, this difference was not significant if alpha levels were Bonferroni corrected to control for familywise error across the eight relevant comparisons.

In sum, the results of Experiment 3 provide additional evidence for modulations in consecutive-target sparing via endogenous control over the duration of the attentional window. In particular, the results of this experiment show that with sufficient changes in probability across conditions, modulations may affect multiple targets, thereby addressing the confound of probability and target position that was present in Experiments 1 and 2. Interestingly, there was also no evidence that the increased proportion of trials with a target immediately trailing T1 influenced distractor processing on the dual-target trials. In fact, there was a nonsignificant trend in the opposite direction (73.74% vs. 78.85%), $t(37) = 1.07$, $p = .29$, $r_{pb}^2 = .03$. This is somewhat surprising given that it might be expected that participants who anticipate having another target presented immediately after the first would be more likely to process a distractor as well. On this basis, it would be informative to explore the possible impact of target probability on nontarget processing in greater detail in future work. Finally, there was again little evidence that changes in sparing were accompanied by changes in order errors, with the possible exception that T2–T3 confusions may have declined in frequency with an increase in sparing. This is inconsistent with the suggestion of Akyürek and colleagues (Akyürek et al., 2012; Akyürek, Riddell, et al., 2007; Akyürek et al., 2008; Hommel & Akyürek, 2005) that sparing is mediated by an increase in temporal integration across spared targets.

In Experiment 4, I wanted to further probe whether the changes in sparing seen in Experiments 1–3 were the result of changes in

Table 2
Percentages of Different Types of Order Errors on Three-Target Trials in Experiment 3

Type of order error	T3 probability			
	Low		High	
	All trials	Corr. trials only	All trials	Corr. trials only
T1–T2	4.3 (0.7)	8.1 (1.2)	5.8 (0.6)	8.8 (1.1)
T1–T3	0.6 (0.2)	1.2 (0.4)	0.8 (0.2)	1.4 (0.4)
T2–T3	12.3 (1.1)	23.0 (1.7)	11.6 (1.0)	16.5 (1.6)
All	3.3 (0.5)	6.8 (1.1)	3.2 (0.5)	5.0 (1.0)

Note. Numbers in parentheses represent one standard error of the mean. T = target; Corr. = correct.

the duration of the attentional window. To this end, I repeated Experiment 2 but introduced a change in the spatial position of the final target. Several studies have shown that this modification is sufficient to eliminate lag-1 sparing (Visser, Bischof, & Di Lollo, 1999; Visser, Zuvic, Bischof, & Di Lollo, 1999) and extended sparing (Visser & Ohan, 2011) because the spatially shifted target falls outside the focus of the attentional window (as long as attention is tightly focused on a single RSVP stream containing T1; Jefferies & Di Lollo, 2009; Jefferies et al., 2007). Thus, I reasoned that if the present results were attributable to variations in the duration of the attentional window, then probability-based changes in sparing should not be seen when the final target was presented at a spatial location outside that window.

Experiment 4

Method

Participants. Forty participants were recruited from an introductory psychology course through advertisements posted on Web-based software. Twenty participants (15 female) were in the low-probability T3 condition, whereas 20 participants (18 female) were in the high-probability T3 condition. The average age of participants was 18.48 years (range: 17–33). Informed consent was obtained from all participants, per standard ethical guidelines. All participants received credit toward their grade to compensate them for their time and effort, reported normal or corrected-to-normal vision, and were naïve as to the purposes of the experiment.

Apparatus and stimuli. Apparatus and stimuli were identical to those in Experiment 2.

Procedure. The experiment was identical to Experiment 2 except that the final target on each trial (either T2 or T3) was always presented in a location above, below, left, or right of the central RSVP stream. This peripheral location was chosen randomly on each trial. The center-to-center separation between the central RSVP stream and the peripheral target was approximately 3° of visual angle. On trials on which the peripheral target appeared directly after a central target, it was accompanied by a distractor presented at the location of the central RSVP stream, which acted as a mask for the central target. A peripheral distractor presented in the same eccentric spatial location always followed the final target, acting as a mask.

Results

Dual-target trials (consecutive targets). Mean T2 accuracy was calculated only for trials on which T1 was identified correctly. Mean accuracy scores for both targets were submitted to a 2 (Target: T1 vs. T2) \times 2 (T3 Probability: high vs. low) mixed-design ANOVA. This revealed a significant main effect of target, $F(1, 38) = 14.08, p = .001, \eta_p^2 = .27$, and a Target \times T3 Probability interaction, $F(1, 38) = 6.95, p = .012, \eta_p^2 = .16$. The main effect of T3 probability was not significant ($p = .32, \eta_p^2 = .03$). Examination of mean performance suggested that T1 and T2 accuracy levels were similar (87.62% vs. 85.78%) when T3 probability was low, $t(19) = 0.85, p = .404, r_{pb}^2 = .04$, but that T2 accuracy was lower than T1 accuracy (88.39% vs. 77.88%) when T3 probability was high, $t(19) = 4.22, p < .001, r_{pb}^2 = .48$, indicating the presence of an AB deficit.

Dual-target trials (targets separated by distractor). Performance was analyzed in the same fashion as on consecutive target trials. The ANOVA revealed a significant main effect of target, $F(1, 38) = 32.79, p < .001, \eta_p^2 = .46$, and a Target \times T3 Probability interaction, $F(1, 38) = 8.33, p = .006, \eta_p^2 = .18$. The main effect of T3 probability was not significant ($p = .34, \eta_p^2 = .02$). Examination of mean performance indicated that T2 accuracy was reliably lower than T1 accuracy both when T3 probability was low, (82.71 vs. 88.57%), $t(19) = 3.04, p = .007, r_{pb}^2 = .33$, and high (73.34 vs. 91.10%), $t(19) = 4.87, p < .001, r_{pb}^2 = .56$, indicating a robust AB. However, the AB was clearly larger when T3 probability was high.

Three-target trials. Mean T3 accuracy was calculated only for trials where both T1 and T2 were identified correctly. Mean accuracy scores for all three targets were submitted to a 3 (target: T1, T2, T3) \times 2 (T3 probability: high vs. low) mixed-design ANOVA. This revealed a significant main effect of target, $F(2, 76) = 57.80, p < .001, \eta_p^2 = .60$, but no other main effects or interactions ($ps > .46, \eta_p^2 < .02$). As can be seen in Figure 4, T2 accuracy was reliably higher than T1 accuracy, $t(39) = 5.35, p <$

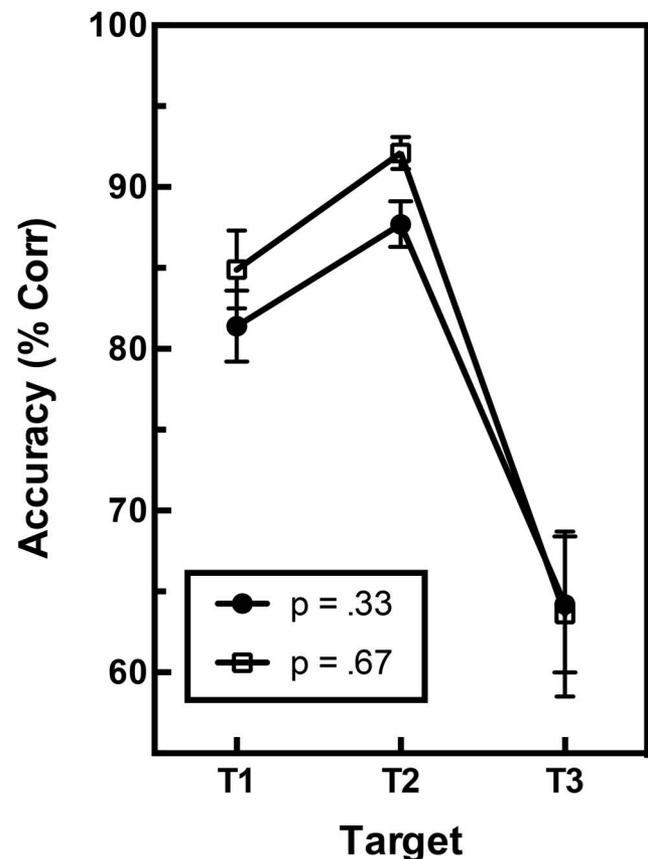


Figure 4. Target accuracy for the first (T1), second (T2), and third (T3) targets on three-target trials in Experiment 4, separated by the probability of three-target trials. Filled circles represent performance in the low-probability three-target condition, in which three consecutive targets appeared on one-third of trials. Open squares represent performance in the high-probability three-target condition, in which T3 appeared on two-thirds of trials. Error bars represent 1 standard error of the mean. Corr = correct.

.001, $r_{pb}^2 = .42$, indicating lag-1 sparing, whereas T3 accuracy was reliably lower than T1 accuracy, $t(39) = 7.02, p < .001, r_{pb}^2 = .63$, indicating an AB. This is to be expected given that T1 and T2 were presented in the same location on three-target trials, whereas T1 and T3 were not (Visser & Ohan, 2011). More important for the present purposes, there were no significant differences between T3 accuracy, $t(38) = 0.10, p = .92, r_{pb}^2 < .01$, or T1 accuracy, $t(38) = 1.06, p = .293, r_{pb}^2 = .03$, across high- and low-probability T3 conditions.

Order errors. On dual-target trials when targets were separated by a distractor, comparisons of order errors across levels of three-target trial probability were again nonsignificant either when considering all trials (low probability: 5.40%; high probability: 4.43%), $t(38) = 0.72, p = .47, r_{pb}^2 < .02$, or only trials on which both targets were identified correctly (low probability: 7.85%; high probability: 7.47%), $t(38) = 0.16, p = .87, r_{pb}^2 < .01$. On dual-target trials with consecutive targets, comparisons of order errors across levels of three-target trial probability were not significant either when considering all trials (low probability: 7.35%; high probability: 7.07%), $t(38) = 0.20, p = .84, r_{pb}^2 < .01$, or only trials on which both targets were identified correctly (low probability: 10.35%; high probability: 12.04%), $t(38) = 0.70, p = .49, r_{pb}^2 < .02$.

On three-target trials, I again calculated mean order errors separately for T1–T2 order errors, T1–T3 order errors, T2–T3 order errors, and all-target order errors. These means can be seen in Table 3 and were entered into a 4 (Order Error) \times 2 (Trials: all vs. correct only) \times 2 (T3 Probability: high vs. low) mixed-design ANOVA to determine whether probability affected the percentage of any of these types of order errors. The analysis revealed main effects of order error, $F(3, 114) = 56.58, p < .001, \eta_p^2 = .60$, indicating that there were fewer T1–T3 order errors than the other types, and trials, $F(1, 38) = 99.70, p < .001, \eta_p^2 = .72$. There was also a Order Error \times Trials interaction, $F(3, 114) = 35.06, p < .001, \eta_p^2 = .48$, indicating that the difference between calculating error means for all three-target trials compared with only correct three-target trials was smaller for T1–T3 order errors than the other three types. Importantly, however, there were no other main effects or interactions ($F_s < 0.82, p_s > .37, \eta_p^2_s < .03$). Once again, there was little evidence for significant changes in the proportion of order errors with changes in third-target probability despite concomitant changes in target accuracy.

Table 3
Percentages of Different Types of Order Errors on Three-Target Trials in Experiment 4

Type of order error	T3 probability			
	Low		High	
	All trials	Corr. trials only	All trials	Corr. trials only
T1–T2	7.5 (0.8)	21.7 (2.1)	8.1 (0.8)	20.2 (2.1)
T1–T3	0.7 (0.2)	1.9 (0.5)	0.5 (0.2)	1.6 (0.5)
T2–T3	5.6 (1.1)	12.6 (1.5)	5.8 (1.1)	11.4 (1.5)
All	3.7 (0.5)	11.1 (1.7)	3.8 (0.5)	10.0 (1.7)

Note. Numbers in parentheses represent 1 standard error of the mean. T = target; Corr. = correct.

The present experiment was designed to test whether the effects of probability demonstrated in earlier experiments were a result of changes in the duration of the attentional window. To do this, I presented T3 at a different spatial location—a manipulation that conventionally eliminates sparing (Visser, Bischof, & Di Lollo, 1999; Visser & Ohan, 2011). This change eliminated the robust effect of probability on T3 accuracy found in Experiment 2. This outcome provides strong support for the notion that the probability effects demonstrated earlier were uniquely tied to the mechanisms underlying sparing. In turn, this implies that sparing depends not only on a target matching a template consistent with earlier items but also on an expectancy that that target is likely to be presented.

Although I did not find an effect of target probability on T3 accuracy in this experiment, this is not to say that probability did not affect other aspects of performance. On both types of dual-target trials, there was a robust interaction between probability and target type, with T2 accuracy falling significantly when T3 probability increased. The most likely explanation for this change is that it is because of the relationship between T3 probability and the likelihood that T2 would be presented in a different spatial location than T1. When T3 probability was low, T2 was presented in a different spatial location than T1 on 67% of trials; however, this was the case on only 33% of trials when T3 probability was high. As a result, T2 performance was worse when T3 probability was high, because observers were not expecting to need to shift spatial attention to process T2, thus leading to more errors. This explanation is also consistent with the finding that T2 accuracy increased on three-target trials when T3 probability was high compared with when it was low, $t(38) = 2.51, p = .017, r_{pb}^2 = .14$. In this case, T2 accuracy benefitted from the greater probability that it would occur in the same location as T1 in the high-probability T3 condition.

Experiment 4 provided strong evidence that manipulating the probability of three-target trials influenced the duration of the attentional window. However, it did not rule out the possibility that varying the probability of consecutive-target trials might also lead to a more generalized enhancement of target accuracy at longer lags as well (i.e., as a result of mechanisms other than direct variations in the attentional window). For example, a block of trials with a large proportion of lag-1 trials might also lead to beneficial effects for T2 when presented at lag 3. Consistent with this possibility, several studies that have directly or indirectly provided cues about the expected onset of T2 during the AB (e.g., Choi, Chang, Shibata, Sasaki & Watanabe, 2012; Tang, Badcock, & Visser, 2014; Visser, Tang, Badcock, & Enns, 2014) have found that such cues modulate target accuracy over a time period of around 100 ms surrounding the actual time of target presentation. Moreover, in the experiments of Crebolder et al. (2002), variations in the probability of a given T1 identity significantly influenced T2 accuracy at both lag 1 and lags extending up to approximately 800 ms from T1 onset.

To check on the possible role of general enhancement in performance induced by my manipulation of target probability, in Experiment 5, I presented AB trials with intertarget lags of 1, 3, or 7. The proportion of trials was varied across participants such that 80% of trials were at lag 1 or lag 7 (with 10% of trials at each of the other two lags). Of chief interest here was performance on lag-3 trials, which comprised 10% of trials in both conditions. If a preponderance of lag-1 trials leads to benefits for T2 at later lags as well, then one would expect T2 accuracy on lag-3 trials to be greater in this condition compared with when a preponderance of trials are presented at lag 7.

Conversely, if the benefits of a preponderance of lag-1 trials are limited to these trials alone, then T2 accuracy at lag 3 should be no different between the two conditions.

Experiment 5

Method

Participants. Twenty-five participants were recruited from an introductory psychology course through advertisements posted on Web-based software. Thirteen participants (11 female) were in the high-probability lag-1 condition, whereas 12 participants (eight female) were in the low-probability lag-1 condition. The average age of participants was 19.16 years (range: 17–48). Informed consent was obtained from all participants, per standard ethical guidelines. All participants received credit toward their grade to compensate them for their time and effort, reported normal or corrected-to-normal vision, and were naïve as to the purposes of the experiment.

Apparatus and stimuli. Apparatus and stimuli were identical to those in Experiment 3.

Procedure. The design of the experiment was similar to that of the dual-target trials in previous experiments. Each condition comprised 500 trials presented in a single self-paced block. In the high-probability lag-1 condition, there were 400 trials with consecutive targets (lag 1), 50 trials in which targets were separated by two distractors (lag 3), and 50 trials in which targets were separated by six distractors (lag 7). In the low-probability lag-1 condition, 50 trials were presented at lag 1, 50 trials were presented at lag 3, and 400 trials were presented at lag 7.

Results

T1. Mean T1 accuracy was calculated separately as a function of intertarget lag and lag-1 probability. These means, which ranged from 91.7% to 94.9%, were then submitted to a 3 (Lag: 1, 3, 7) \times 2 (Lag-1 Probability: high vs. low) mixed-design ANOVA. This analysis revealed no significant main effects or interactions ($p_s > .27$, $\eta_p^2 < .06$).

T2. Mean T2 accuracy was calculated only for trials on which T1 was identified correctly and can be seen in Figure 5. As with T1, these scores were submitted to a 3 (Lag: 1, 3, 7) \times 2 (Lag-1 Probability: high vs. low) mixed-design ANOVA. This analysis revealed a main effect of lag, $F(2, 46) = 11.17$, $p < .001$, $\eta_p^2 = .33$, as well as a Lag \times Probability interaction, $F(2, 46) = 3.24$, $p < .05$, $\eta_p^2 = .12$. The main effect of probability, however, was not significant, $F(1, 23) = 0.36$, $p > .55$, $\eta_p^2 = .02$. Examination of Figure 5 suggests that lag-1 probability influenced T2 accuracy at lags 1 and 7 but had little effect at lag 3. To confirm this impression, between-subjects t tests were conducted separately at each lag, comparing performance across levels of lag-1 probability. These analyses revealed that T2 performance was significantly better in the high-probability lag-1 condition at lag 1, $t(23) = 2.13$, $p < .05$, $r_{pb}^2 = .16$, and marginally better in the low-probability lag-1 condition at lag 7 (where the preponderance of trials were at lag 7), $t(23) = 1.51$, $p = .072$ (one-tailed), $r_{pb}^2 = .09$. Importantly, however, there was no difference in T2 performance between the high- and low-probability lag-1 conditions at lag 3, $t(23) = 0.34$, $p = .74$, $r_{pb}^2 = .02$. This result suggests that the benefits of

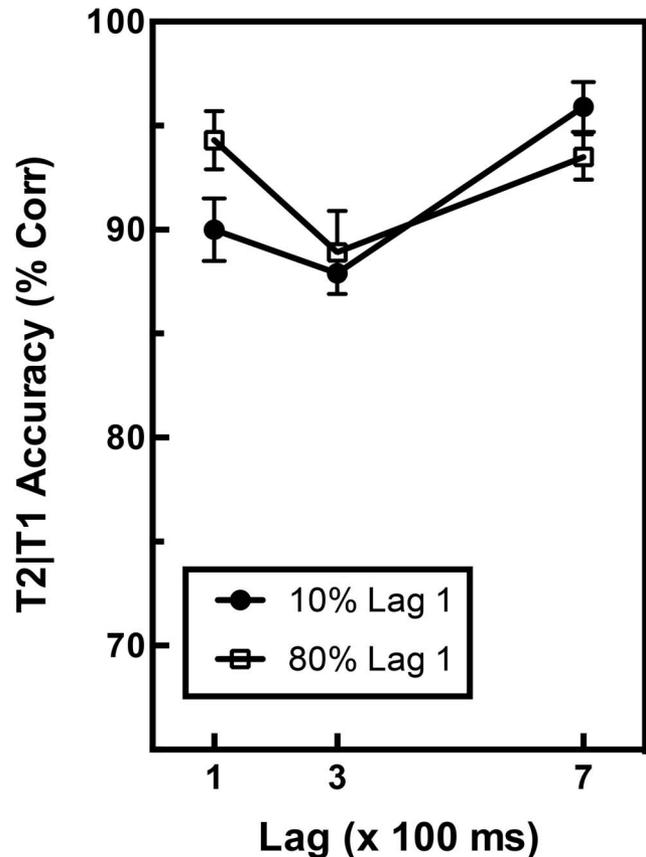


Figure 5. Second target accuracy (given correct identification of the first target) in Experiment 5, separated by the probability of trials at lag 1. Filled circles represent performance in the low-probability lag-1 condition, in which T2 appeared at lag 1 on 10% of trials. Open squares represent performance in the high-probability lag-1 condition, in which T2 appeared at lag 1 on 80% of trials. Error bars represent 1 standard error of the mean. Corr = correct.

manipulating the probability of consecutive-target trials is closely tied to the duration of the attentional window rather than a more general mechanism extending over longer time periods. This point is revisited in the General Discussion.

Order errors. As in earlier experiments, I examined the impact of manipulating lag-1 probability on the proportion of order errors. Considering only lag-1 trials, comparisons of order errors across levels of lag-1 probability were nonsignificant either when considering all trials (low probability: 15.03%; high probability: 15.57%), $t(23) = 0.19$, $p = .85$, $r_{pb}^2 < .01$, or only trials on which both targets were identified correctly (low probability: 17.76%; high probability: 18.74%), $t(23) = 0.27$, $p = .79$, $r_{pb}^2 < .01$.

The present experiment replicated the chief finding of Experiment 1: The probability of lag-1 trials influenced the magnitude of lag-1 sparing. Moreover, the results here are consistent with the notion that probability specifically altered the duration of the attentional window rather than leading to a more general enhancement of T2 processing across a broader time frame. Of course, it is possible that if T2 performance were assessed a shorter lag, then some benefits in accuracy might also be seen. However, given the

evidence that the benefits of temporal cues do seem to be somewhat temporally imprecise (Choi et al., 2012; Tang et al., 2014; Visser et al., 2014), this outcome would still be consistent with the idea that the duration of the attentional window changes. It would simply imply that the precision of this duration change is relatively low. What is clear from the present results, however, is that the alterations to T2 performance here were temporally confined rather than extending for several hundred milliseconds as in previous experiments (Crebolder et al., 2002).

General Discussion

Over 20 years of research has shown that observers are severely limited in their ability to identify target objects presented in rapid succession—a deficit known as the AB. Curiously, however, although this limitation reliably occurs when targets are separated by distractors, it is often absent when targets are presented in direct succession. Although early theories suggested that the AB arose from somewhat different mechanisms than the sparing seen with consecutive items (e.g., Potter et al., 1998; Visser, Bischof, & Di Lollo, 1999), more recent theories have presented a unified account of both phenomena (e.g., Di Lollo et al., 2005; Olivers & Meeter, 2008; Taatgen et al., 2009; Wyble et al., 2009). Common to all accounts, though, is the notion that sparing occurs when targets share critical characteristics that allow them to pass a target template, or “input filter,” and thus access high-level resources.

Although there is ample evidence that shared characteristics are a crucial aspect of both lag-1 sparing with two consecutive targets (Visser, Bischof, & Di Lollo, 1999; Visser, Zuvic, et al., 1999) and extended sparing across multiple targets (Visser & Ohan, 2011), the present research addressed whether shared characteristics are both necessary and sufficient for sparing to occur. In this regard, there has been considerable theoretical debate. The majority of theories suggest that shared characteristics are both necessary and sufficient (e.g., Di Lollo et al., 2005; Olivers & Meeter, 2008; Taatgen et al., 2009; Wyble et al., 2009) for sparing to occur. In contrast, Akyürek and colleagues (Akyürek, Riddell, et al., 2007; Akyürek et al., 2008; Akyürek et al., 2012; Hommel & Akyürek, 2005) have posited that shared characteristics are necessary for sparing but that the magnitude of sparing can be modulated by factors such as observer expectancy (Akyürek et al., 2008). That is, sparing is under a considerable degree of endogenous control rather than being exogenously driven by stimulus characteristics alone.

To examine the role of endogenous control in sparing in more detail, I conducted several experiments that compared levels of lag-1 sparing and extended sparing across groups of individuals who viewed RSVP streams with consecutive targets on a minority (33%) or a majority (67%) of trials. Variations in probability were expected to induce endogenously driven changes in the duration of the attentional window that would, in turn, influence sparing. The results were clear. In Experiment 1, T2 performance was consistently superior when it followed T1 on a high proportion of trials compared with when consecutive targets occurred infrequently. In Experiment 2, I replicated this result for T3 performance when the probability of three-target trials was high compared with when this type of trial was infrequent.

In Experiment 3, I showed that probability-based manipulations of expectancies were not limited to the final target in the sequence.

When two- and three-target trials were mixed such that the probability of three consecutive targets covaried with the probability of two consecutive targets occurring, both T2 and T3 performance improved. Finally, in Experiments 4 and 5, I focused on whether expectancy influenced the duration of the attentional window. Experiment 4 showed that changes in T3 performance with expectancy were isolated to cases in which all targets were presented in the same spatial location. When T3 was presented in a different spatial location—a manipulation known to eliminate sparing with single RSVP streams (Visser, Zuvic, et al., 1999)—probability no longer affected T3 accuracy. In Experiment 5, manipulations of the probability of lag-1 trials in the context of a more conventional AB task with multiple lags altered the magnitude of lag-1 sparing but did not influence performance at lag 3. This outcome indicates that the probability manipulation influenced the attentional window rather than a more general process extending across temporal lags. In sum, the results here point to a significant degree of endogenous control over sparing mediated by changes in the duration of the attentional window thought to underlie successful processing of consecutive targets. Thus, although shared characteristics are necessary for sparing, they are not sufficient. At the least, observers must also expect consecutive targets to be presented for sparing to occur.

Consideration of Alternative Explanations

Although I suggest that target probability in the present experiments altered the duration of the attentional window, thereby varying the number of consecutive items processed, there are a number of possible alternative explanations. Two of these are suggested by previous studies that have looked at factors modulating lag-1 sparing. First, Jefferies and colleagues (Jefferies & Di Lollo, 2009; Jefferies et al., 2007) showed that altering expectations about the location of T1 in a multistream RSVP modulated the spatial area across which consecutive targets were processed. When T1 appeared in an unknown stream, sparing occurred regardless of whether T1 and T2 were presented in the same stream; when the location of the T1 stream was known, sparing only occurred when T2 appeared in the same stream as T1.

Although these results, like the present ones, indicate that the magnitude of lag-1 sparing can depend on task demands, a comparison of respective methodologies highlights significant underlying differences. In the work of Jefferies and colleagues (Jefferies & Di Lollo, 2009; Jefferies et al., 2007), the manipulation of T1 location information apparently expanded the spatial area across which items could enter the attentional window, leading to sparing across multiple RSVP streams. However, such space-based changes cannot have been at work in the present experiments because variations in sparing occurred in experiments using a single, centrally presented RSVP stream.

Another possible account for the present effects is suggested by the experiments of Akyürek and colleagues (Akyürek et al., 2012; Akyürek, Riddell, et al., 2007; Akyürek et al., 2008; Hommel & Akyürek, 2005), who found that observer expectations altered order errors in the report of consecutive targets. Such results imply that the mechanisms underlying order errors might also have been at work in the present experiments. Contrary to this suggestion, however, a closer examination of Akyürek et al.'s results reveals that changes in the proportion of order errors were not accompa-

nied by variations in lag-1 sparing. Similarly, in the present work, there was no evidence that variations in the magnitude of sparing were accompanied by reliable changes in order errors. Thus, I suggest that the present findings also cannot be explained by mechanisms underlying variations in order errors observed in earlier studies.

In addition to these possible explanations, the present results also rule out several other accounts for the effects of probability. One option is that changes in final target accuracy were mediated by resource shifts among targets (Olivers, Hilkenmeier, & Scharlau, 2011) similar to those shown by Dux et al. (2009), who demonstrated tradeoffs in target accuracy depending on report requirements. This option may be refuted on the grounds that probability-linked changes in final target accuracy occurred in the absence of corresponding deficits for earlier targets (see Experiments 1 and 2). Another possibility is that the present results are attributable to practice effects, given that more consecutive target trials occurred when their probability was higher. Arguing against this possibility, however, is the fact that identical results were obtained when the same numbers of consecutive target trials were compared across high- and low-probability conditions.

Given that one can reasonably discount all of the explanations just discussed for the variations in sparing obtained here, the option that probability variations resulted in endogenously mediated changes in the duration of the attentional window is the most likely remaining alternative. This possibility is supported by the results of Experiment 4, which showed that probability did not influence sparing when T1 and T2 were presented in different spatial locations—an experimental manipulation reliably shown to eliminate lag-1 and extended sparing (Visser, Bischof, & Di Lollo, 1999; Visser & Ohan, 2011). This option is also consistent with the results of Experiment 5, in which the influence of target probability was confined to lag 1 rather than extending to later lags. This specificity implies that probability did not influence a more temporally diffuse or general attention mechanism, which would have led to broad-based improvements in target accuracy across lags.

Relationship to Temporal Cuing

Manipulations of target probability, such as those used here, have been widely used to examine the impact of temporal cues on perception. *Temporal cues* broadly refers to any type of information that signals the upcoming time at which a stimulus of interest may appear (Correa, Lupiáñez, & Tudela, 2005; Coull & Nobre, 1998; Nobre, Correa, & Coull, 2007). Variations in target probability can provide temporal information in at least two ways (Visser, Ohan, & Enns, 2015). One is via the so-called foreperiod effect (Niemi & Näätänen, 1980). Here, given an equal distribution of targets across a range of possible time points, the likelihood of target appearance increases with the passage of time. A second type of temporal information occurs when the probability of targets appearing at different time points varies. In this case, it may be possible to orient attention to the most likely time of target onset.

In the context of the AB, the use of probability as a means of conferring temporal cue information has been examined on multiple occasions (Martens, Elmallah, London, & Johnson, 2006; Martens & Johnson, 2005; Shen & Alain, 2012; Tang et al., 2014; Visser et al., 2014, 2015). Notably, these studies have shown

somewhat mixed results. For example, Martens and Johnson compared performance between trials on which an initial fixation marker gave no information about the upcoming target lag and trials on which the fixation marker indicated upcoming trial lag with perfect accuracy. This comparison showed reliable evidence for a reduction in the AB on trials with temporal cues versus those without. However, a similar experiment comparing trials with random, unpredictable lags and blocks of trials with a consistent lag (i.e., 100% probability of a given lag) showed no benefits of predictability.

Visser et al. (2014) examined the impact of target predictability in greater detail, hypothesizing that observers could make use of predictability to ameliorate the AB, but only when this relationship was highlighted. Consistent with this conjecture, when they repeated the procedure of Martens and Johnson (2005) with an explicit instruction to use the consistent lags as a way to improve performance, observers showed a smaller AB compared with a group of control observers who viewed trials with unpredictable intertarget lags. Visser et al. (2015) also looked at the effects of varying the probability of targets presented at varying lags using a hybrid AB paradigm requiring a speeded response to T2 (Zuvic, Visser, & Di Lollo, 2000). This more sensitive measure also revealed benefits linked to target probability—however, these benefits were relatively small and only occurred when probability varied by approximately .25 across conditions.

The present work replicates the findings of Visser et al. (2015) using T2 accuracy as a dependent measure. This provides additional evidence that probability can serve as strong temporal cue as long as differences in probability are sufficiently salient. Why was probability particularly salient here? I suggest that the present use of a within-subject manipulation of probability highlighted variations in this dimension by providing a strong context for participants. This context did not occur in previous experiments because probability was always varied across different groups of participants. Notably, this hypothesis can also account for earlier results obtained by Shen and Alain (2012), who also found robust effects of T1 probability on performance, in the absence of specific mention of this relationship, in a within-subject design.

It is also important to consider how the theoretical explanation proposed for the present results fits in with similar accounts advanced in earlier temporal cuing studies. Martens and Johnson (2005) suggested that temporal cues lend a unique attribute to targets presented at the cued interval. This, in turn, aids them in a postperceptual interitem competition for perceptual resources. Choi et al. (2012) suggested that their training effects (which have been attributed to temporal expectancy; Tang et al., 2014; Willems, Damsma, Wierda, Taatgen, & Martens, 2014) arose from changes in attentional control mechanisms governing target selection and distractor inhibition. Consistent with this idea, Visser et al. (2014) showed that temporal expectancy led to greater improvements in T2 accuracy when target–distractor similarity was increased. They suggested that this arose from strategic deployment of the attentional window around the expected time of target arrival, thereby increasing the likelihood that distractors would avoid selection.

The present results accord well with the findings of Choi et al. (2012), Tang et al. (2014) and Visser et al. (2014) in implicating a role for cognitive control and strategic deployment of the attentional window. Here, I suggest that in addition being able to exert

control over the time at which the attentional window opens to accept incoming stimuli, it is also possible to exert control over when the window begins to close. The alternative option, suggested by the account of [Martens and Johnson \(2005\)](#), that probability influenced the balance of interitem competition does not seem tenable here because there was no evidence for performance tradeoffs between targets or variations in order errors as might be expected on this proposal.

Broader Implications of the Present Findings

The chief aim of the present work was to test whether temporal contiguity was necessary and sufficient to induce lag-1 and extended sparing. As noted in the introduction, nearly all theories of the AB (e.g., [Chun & Potter, 1995](#); [Di Lollo et al., 2005](#); [Olivers & Meeter, 2008](#); [Wyble et al., 2009](#)) postulate that this is the case as long as targets also possess sufficient commonalities along the dimensions of stimulus category, associated task, modality, and location ([Visser, Bischof, & Di Lollo, 1999](#); [Visser & Ohan, 2011](#)). This viewpoint implies that sparing is essentially stimulus driven and that endogenous control is likely to be limited at best.

In contrast, the temporal integration account of [Akyürek](#) and colleagues ([Akyürek et al., 2012](#); [Akyürek, Riddell, et al., 2007](#); [Akyürek et al., 2008](#); [Hommel & Akyürek, 2005](#)) argues that sparing arises as the result of combining items into a single cognitive episode (i.e., attentional window), whereas the AB occurs when targets are separated by at least one distractor, leading to separate cognitive episodes. On the basis of this distinction, the temporal integration account predicts that sparing should be linked with order errors, because the presentation time of combined items would be necessarily difficult to distinguish when combined into a single attentional episode. This conjecture overlaps well with reliable drops in T1 accuracy often observed at lag 1, increases in order errors seen at lag 1 relative to later lags ([Hommel & Akyürek, 2005](#)), and increased instances of temporal integration at lag 1 ([Akyürek et al., 2012](#)). Moreover, the model also predicts that the duration of the cognitive episode is endogenously modifiable. This is consistent with the previously reviewed findings of [Hommel and Akyürek](#), who showed modulations of order errors on the basis of observer expectations and perceived RSVP speed.

On the whole, the present findings accord well with the prediction of the integration accounts that the duration over which targets can be processed together is under endogenous control. However, an analysis of order errors across experiments suggests that the posited association between such errors and sparing does not hold for the present data. Although I found robust variations in sparing magnitude, these changes were not accompanied by variations in order errors. This was true across experiments and whether order errors were measured as a function of all trials or only those in which both targets were correctly identified. In fact, taken together with the findings of [Hommel and Akyürek \(2005\)](#), the present data appear to show a double dissociation whereby expectations of RSVP speed influence order errors but not target accuracy, whereas variations in target probability affect target accuracy but not order errors.

The present work agrees broadly with several other studies that have shown that factors that would not be expected to influence temporal integration nonetheless have significant impact on order errors. For example, [Olivers et al. \(2011\)](#) found that cuing the

onset of a colored T1 by presenting an immediately preceding distractor in the same color reduced order errors. They attributed this to a shift in processing resources from T2 to T1. [Spalek, Lagroix, Yanko, & Di Lollo \(2012\)](#) also examined the influence of lag between T1 and directly consecutive T2 and T3 items as well as the presence of distractors on T3 sparing following T2. They found that T3 sparing varied substantially with temporal lag and distractor presence. Again, this would be unexpected if entry into a common attentional window were the sole determinant of order errors, as suggested by the integration account. Instead, both [Olivers et al. \(2011\)](#) and [Spalek et al.](#) argued that order errors reflect the strength of stimulus representations of consecutive items, which can dynamically vary with a variety of factors.

The upshot of all of this work seems to be that order errors occur when consecutive targets are presented and that this may be linked to temporal integration of these items ([Akyürek et al., 2008](#)). However, order errors are not tightly tied to the ability of stimuli to enter a common attentional window ([Olivers et al., 2011](#); [Spalek et al., 2012](#)), and order errors can be dissociated from changes in actual target identification accuracy, as seen in the present study. Thus, although temporal integration may play a role in order errors and might also be expected to lead to identification errors in some cases, other factors are also important in determining performance when targets are presented in direct succession.

Returning to the present evidence for the role of endogenous factors in sparing, it is important to note that these results align broadly with those obtained in other paradigms. For example, [Spalek \(2007\)](#) showed that visual cuing influences observers' predictions about where an upcoming target will appear, suggesting that such expectancies may partly explain the inhibition of return effect ([Posner & Cohen, 1984](#)). In visual search, [Müller, Reimann, and Krummenacher \(2003\)](#) showed that cuing the dimension along which an upcoming pop-out target would likely differ (e.g., color orientation) from distractors led to improved search efficiency. Moreover, many models of visual attention and search explicitly incorporate top-down influences (e.g., [Bundesen, 1990](#); [Corbetta & Shulman, 2002](#); [Wolfe, Cave, & Franzel, 1989](#)) on performance. In other fields, expectancy about whether an upcoming test will involve recall or recognition influences encoding ([Balota & Neely, 1980](#)), recall is reliably better for unexpected than expected information in scenes ([Brewer & Treyns, 1981](#); [Pezdek, Whetstone, Reynolds, Askari, & Dougherty, 1989](#)), and contextually expected information leads to memory intrusions ([Roediger & McDermott, 1995](#)).

In this tradition, the present results argue for a reconceptualization of sparing as a phenomenon that also arises from the interplay of top-down expectations and bottom-up template matching. Current theories suggest that sparing begins when an initial item that matches a target template or input filter triggers the opening of an attentional window to high-level processing. This window both closes slowly and can be reopened by the appearance of subsequent targets, with both of these properties ensuring that several consecutive targets can gain access to attentional resources. Conversely, the appearance of a distractor following a target creates competition in visual short-term memory ([Shapiro, Raymond, & Arnell, 1994](#)), initiates inhibition ([Olivers & Meeter, 2008](#)), reconfigures the input filter ([Di Lollo et al., 2005](#)), changes control rule priorities ([Taatgen et al., 2009](#)), or directly impairs transient attention ([Wyble et al., 2009](#)). These actions, in turn, deny subse-

quent targets access to necessary processing resources, thereby eliminating sparing and initiating an AB.

Given these parameters, I suggest that a simple change to accommodate endogenous factors in sparing would be to assume that the period that the window remains open following the presentation of an initial target is determined by expectations about whether another target will be presented. This proposal seems sensible on the grounds that if another object that matches the current template is not expected to appear, there is little advantage to keeping the window open. Indeed, such preservation would seem costly to maintain in terms of cognitive resources required to maintain the target template, which are shared with target identification processes (Di Lollo et al., 2005) as well as working memory (Akyürek, Hommel, & Jolicoeur, 2007; Visser, 2010). In addition, maintaining an input filter set to characteristics that are unlikely to be relevant to future perception would also carry a cost to the speed and accuracy of future target identification (Giesbrecht & Di Lollo, 1998; Zuvic et al., 2000).

In sum, the present findings show that lag-1 sparing and extended sparing are not sufficiently explained by current theories suggesting that temporally contiguous targets that share sufficient attributes will always pass through an attentional window triggered by the first target in the sequence. Rather, it is clear that sparing also depends on top-down influences, such as the probability of multiple targets appearing. This finding necessitates changes to all current models. The nature of this change may be relatively simple, but its exact parameters will require significant further testing to be established. Such work will help us understand object perception in more detail, both in cases in which identification is spared and those in which it is impaired.

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Received July 15, 2013

Revision received December 16, 2014

Accepted December 17, 2014 ■

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