Great expectations: Minor differences in initial instructions have a major impact on visual search in the absence of feedback

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Abstract

Professions such as radiology and aviation security screening that rely on visual search—the act of looking for targets among distractors—often cannot provide operators immediate feedback, which can create situations where performance may be largely driven by the searchers’ own expectations. For example, if searchers do not expect relatively hard-to-spot targets to be present in a given search, they may find easy-to-spot targets but systematically quit searching before finding more difficult ones. Without feedback, searchers can create self-fulfilling prophecies where they incorrectly reinforce initial biases (e.g., first assuming and then, perhaps wrongly, concluding hard-to-spot targets are rare). In the current study, two groups of searchers completed an identical visual search task but with just a single difference in their initial task instructions before the experiment started; those in the “high-expectation” condition were told that each trial could have one or two targets present (i.e., implying no target-absent trials) and those in the “low-expectation” condition were told that each trial would have up to two targets (i.e., implying there could be target-absent trials). Compared to the high-expectation group, the low-expectation group had a lower hit rate and quit trials more quickly, consistent with a lower quitting threshold (i.e., performing less exhaustive searches). The expectation effect was present from the start and remained across the experiment—despite exposure to the same true distribution of targets, the groups’ performance remained divergent, primarily driven by the low-expectation group’s self-fulfilling prophecy that stemmed from the simple instructions difference. In sum, initial expectations can have dramatic influences—searchers who do not expect to find a target, are less likely to find a target as they are more likely to quit searching faster.

Significance Statement

In professions such as radiology and airport security screening, success in visual search—looking for targets among distractors—can be a matter of life-or-death. Therefore, it is critical to understand the factors that influence performance. Many professional searches are carried out in situations where feedback on accuracy is, at best, delayed, but more often

never received. When searching in the absence of feedback, the searchers' initial expectations may play an outsized role in determining search performance. In this study, searchers' initial expectations about the trial distributions, namely the possibility of target-absent trials, were set by a simple manipulation of the pre-experiment instructions. In the absence of feedback, participants with low-expectations of a target being present performed less thorough searches than those with high-expectations, resulting in more missed targets. This effect was present from the beginning of the experiment such that it created a self-fulfilling prophecy whereby, despite the same true underlying distribution of targets, the low-expectation group actually experienced a lower target prevalence rate. This generated sustained differences in expectations leading to performance differences across the experiment. Beyond serving as a reminder of the importance of carefully crafting instructions for academic experiments, this study highlights the outsized effect that initial expectations can have on search performance in the absence of feedback—a scenario that describes many critical real-world searches. The current work further highlights the importance of setting accurate expectations based on the true target distribution, pointing to the need for professional search industries to fully understand the nature of their environment.

Keywords: Visual Search, Expectation, Instructions, Feedback, Self-fulfilling prophecy

Introduction

Visual search, finding targets among non-target distractors, is a nearly ubiquitous cognitive act that underlies everyday activities (e.g., finding your keys, looking for a friend in a crowd) as well as highly-important professional tasks (e.g., airport baggage screening, radiology). While efficient and effective performance is useful in everyday searches (no one wants to spend an hour locating their car keys), it is absolutely vital for many professional searches. Taking too long or making mistakes can lead to serious, even life-or-death, outcomes in professional settings such as radiology, pathology, aviation security screening, lifeguarding, border patrol efforts, and various military activities.

Given the prominent role of visual search in so many facets of life, it is not surprising that it has been widely studied (for overviews see Clark, Cain, Adamo, & Mitroff, 2012; Wolfe, 2020a). Much has been learned about the processes underlying search and the practical factors that affect performance through research in cognitive psychology (for recent reviews see Chan & Hayward, 2013; Eckstein, 2011 and Nakayama & Martini, 2011), academic radiology (e.g., Krupinski, 2010; 2015; Kundel, Nodine, & Carmody, 1978), aviation security (e.g., Mitroff, Ericson, & Sharpe, 2018; Wetter, 2013), military research (e.g., Cornes, Boardman, Ford, & Smith, 2019; Janelle & Hatfield, 2008), and more. Collectively, these efforts have served to isolate core cognitive mechanisms that drive search performance and inform how to optimize performance in a range of practical settings (Wolfe, 2020b).

Since the initial investigations of search (e.g., Koopman, 1956; Poulton, 1890), there has always been a clear and explicit acknowledgement that one of the primary goals of the research efforts is to elucidate factors that impact search performance. Through both theoretical and applied investigations there has been a focus on potential influences, including such factors as the number of non-target items present in the search (e.g., Palmer, 1994), the physical and conceptual relationship between the targets and distractors (e.g., Biggs, Adamo, Dowd, & Mitroff, 2015), external factors such as time pressure (e.g., Pieters & Warlop, 1999) and task goals (e.g., Clark, Cain, Adcock, & Mitroff, 2014), and individual differences (e.g., Biggs, Cain, Clark, Darling, & Mitroff, 2013; Boot, Becic & Kramer, 2009). Importantly, such factors do not influence performance in isolation—each can impact search on their own but they can also interact with one another in both potentially helpful or detrimental ways.

Pre-search expectations in the absence of feedback

The current project focused on one particular factor that can influence search performance—searchers' initial pre-search expectations. Expectations, the belief that something will happen, can be set in a variety of ways for a search task; for example,

searchers can be provided with explicit information, including pre-search instructions (e.g., Yarbush, 1967), cues presented during active search (e.g., Posner, Snyder, & Davidson, 1980), and post-search feedback (e.g., Chabukswar, Gramopadhye, Melloy, & Grimes, 2003). Likewise, expectations can develop with experience across search experience (e.g., Wolfe, Horowitz, & Kenner, 2005); for example, in “contextual cueing” experiments (Chun & Jiang, 1998), searchers become progressively more efficient at finding a target when the same search array is repeated over the course of an experimental session.

Prior research has shown that various forms of expectations can significantly impact search performance. For example, the impact of searchers’ expectations is clear from the fact that when searchers do not regularly see a target, they miss more of the targets that are actually present (e.g., Gur et al, 2004; Wolfe et al., 2005). This “low prevalence effect” has been attributed both to a criterion shift such that the searchers require more evidence to label something as a target and to a lower quitting threshold such that participants spend less time searching on trials where there is no target found (e.g., Wolfe & Van Wert, 2010). That is, when searching in a low-prevalence target environment, the argument is that searchers are quicker to decide that no target is present and they may not conduct an exhaustive search of the whole display. Research into this effect suggests that the prevalence effect gradually builds over recent experience (Wolfe & Van Wert, 2010; Ishibashi, Kita, & Wolfe, 2012).

Interestingly, it has been shown that feedback can impact the effect of low prevalence on expectations. While the vast majority of the academic work focusing on prevalence effects has provided participants with full and accurate feedback (e.g., Wolfe et al., 2005; Wolfe & Van Wert, 2010; Ishibashi et al., 2012; Ishibashi & Kita, 2014) one exception is revealing. Two experiments in Wolfe et al. (2007) presented searchers with a long sequence of low-prevalence search (1% target prevalence) with an occasional burst of high-prevalence search (50% target prevalence) inserted into the sequence. Critically, one of the experiments contained feedback on all trials while the other only included feedback during the high-prevalence bursts. There was negligible impact of the burst of high-prevalence target trials on subsequent periods of low-prevalence search performance in the presence of full feedback, but when accuracy feedback was provided only during the high-prevalence bursts, but not the low-prevalence periods, in the other experiment, there was a significant inoculation against the low-prevalence effect. That is, feedback on accuracy during the high-prevalence bursts created expectations about the underlying target frequency that carried over into the low-prevalence search periods when there was no feedback during those periods to confirm that the underlying target distribution had changed. Relatedly, another line of research has demonstrated that feedback alone can drive search behavior; when participants were presented with false performance feedback they shifted their

target present decision criterion (Schwark, Sandry, MacDonald, & Dolgov, 2012), even in the complete absence of actual targets (Schwark, MacDonald, Sandry, & Dolgov, 2013).

While much has been revealed about how various forms of expectations can affect search performance, there are open questions that can have meaningful impacts on professional search situations. Specifically, further understanding the impact on search performance of initial expectations (i.e., task instructions) in the absence of feedback is needed given that most professional searches lack timely feedback. Radiologists examining routine cases do not receive real-time feedback as to whether their evaluations are correct or incorrect. Aviation security screeners can potentially receive immediate feedback if they correctly (or incorrectly) detect a prohibited item in a passenger's bag and pull the bag for secondary search, but they will receive no, or highly delayed, feedback if they do not pull a bag for secondary search.

Arguably, without immediate feedback, professional searchers are largely left to perform "in the dark" where they do not know if their ongoing performance is appropriate or in need of adjustment. Performing a visual search task without feedback can place searchers into a situation where their initial expectations as they start to engage in the search can potentially have an unreasonably large impact on their performance; this is problematic because initial pre-search expectations may not always map correctly onto the reality of the search environment at any given moment. For example, pre-search expectations can change in professional search environments; radiologists can be provided with patient history and aviation security screeners can move between various environments (e.g., the US standard lanes vs. PreCheck lanes). Regardless of context, however, all such searches require a high level of accuracy.

Self-fulfilling prophecy of search without feedback

Searching without feedback can create a self-fulfilling prophecy where the searchers' initial expectations going into the search can dramatically shape their actual performance during the search, which in turn serves to bolster their initial expectations. In general, if searchers do not know if targets are present and/or how many possible targets might be present in any given search, then they must adopt a quitting threshold that will determine when they stop looking and move on to the next search (Chun & Wolfe, 1996; Cain, Vul, Clark, & Mitroff, 2012). In some situations where accuracy is vital, searchers will ideally adopt a fully exhaustive search strategy where they do not quit the specific search until they have inspected and considered every possible item. However, many factors can lead to non-exhaustive search strategies, including time pressure (Pieters & Warlop, 1999) and expectations (e.g., in low prevalence search; Wolfe & Van Wert, 2010). The choice of a quitting threshold can be driven by feedback; when searchers are provided with trial-by-trial, real time feedback on search accuracy, they can adapt their quitting threshold to

appropriately map the structure of their search environment. For example, when receiving feedback in low prevalence search environments that an exceedingly few trials have a target present, searchers will generally speed up and adopt a lower quitting threshold (e.g., Wolfe et al., 2007).

Consider a search environment that can contain an equal number of both relatively easy and relatively hard to detect targets (e.g., water bottle vs. bullets in aviation security screenings; high-salience black T vs. low-salience light gray T in Figure 1A). In such a search environment, the relatively easy-to-spot targets, on average, will be found quicker—but that does not make them any more prevalent; they are just quicker to be found since they stand out more. If the searchers receive real-time accuracy feedback on target detection, then they can learn that easy and hard targets are equally likely in this particular environment, and they may adjust their quitting threshold to search longer when they do not quickly spot an easy target in anticipation that a hard target might be present.

Now, take the above search scenario in the absence of feedback. The searchers, on average, will find the easy targets quicker and more easily, and they will be less likely to detect the hard targets. Without trial-by-trial accuracy feedback, the searchers might incorrectly conclude that easy targets are more prevalent than hard targets, and might adopt a lower quitting threshold such that they will quit the search sooner since their own experiences during the search suggest that targets are likely to be detected quickly.

When search environments do not provide immediate feedback, the searchers' initial expectations about the nature of the search can create a self-fulfilling prophecy that is divorced from the actual nature of the true search parameters. Continuing with our hypothetical search scenario where there are an equal number of easy- and hard-to-spot targets, imagine there are two groups of participants—the first group *a priori* expects few, if any, trials to contain a target while the second group expects every trial to contain at least one target. The first group might adopt a lower quitting threshold going into the search and prematurely end their search before fully inspecting every item, while the second group might adopt a higher quitting threshold and will conduct more exhaustive searches (Figure 1B). The first group will mostly find the easy targets but miss the hard targets (Figure 1C) while the second group will likely find both easy and hard targets (Figure 1D). Without feedback, there is nothing to adjust these behaviors and the two groups' actual experiences will reinforce their respective initial quitting threshold settings.

Current study

The current project explored how initial pre-search expectations could have a lasting impact on visual search when no feedback is provided. Two groups of searchers completed the same search task, with one sole difference between groups—one sentence of the initial

instructions was altered between the groups. One group (“low-expectation” group) was given a sentence in the instructions that implied that search trials could have 0, 1, or 2 targets and the other group (the “high-expectation” group) was given a sentence in the instructions that implied that search trials could have 1 or 2 targets. There were no other differences between the two conditions and the critical question was whether this small difference in instructional wording at the start of the experiment could meaningfully impact search performance.

Figure 1. Example stimuli and model of the predicted results. A) Example dual-target display. The easy target (dark gray rotated T) is located in the upper left quadrant and the hard target (light gray T) is located in the lower left. B) Depiction of underlying difficulty distributions from which the “Hard” (light gray) and “Easy” (dark gray) targets for each trial were drawn from. More difficult-to-find targets fall farther to the right on the distribution and will take longer to find. The prediction is that expectations about the underlying trial structure differentially set the quitting threshold (low-expectation in orange, high-expectation in blue). C) Predicted distribution of found targets for the low-expectation quitting threshold in panel B. D) Predicted distribution of found targets for the high-expectation quitting threshold in panel B. Comparing panels C and D, illustrates how the higher quitting threshold leads to higher hit rates by allowing time for the searcher to find the more difficult targets from the far right of the distribution in panel B. In turn, the mean response time for finding a target increases with the higher quitting threshold, particularly for the hard targets.

The prediction was that the simple, one sentence instruction manipulation would set different expectations about the presence of target-absent trials leading the “low-expectation” group to conduct less thorough searches than the “high-expectation” group (Figure 1B) due to a lower quitting threshold (e.g., Lau & Huang, 2010). Further, the current project explored whether this potential effect would manifest as an initial, short-lived difference or whether it would remain as a lasting effect on search performance across the full experimental session. The prediction was that despite exposure to the same true distribution of trials, the instruction manipulation would lead to different expectations, and, without feedback on search accuracy, the groups would have different subjective experiences of the same underlying target distribution (Figure 1). The different subjective experiences would serve as reinforcement of the initial difference in expectation, functioning as a self-fulfilling prophecy whereby the initial difference in expectation is matched in each groups’ experience despite the absence of an actual difference in the true target distribution across groups. This could cause the difference between the groups to remain, or even amplify, across the experiment. As shown in the model in Figure 1, the difference between the two groups was expected to be more extreme on relatively harder trials which will be more affected by variation in quitting thresholds.

The prediction was that the instruction manipulation employed here would alter the expectations of the low- and high-expectation groups, manifesting in differences in their quitting thresholds (Figure 1B). This difference in quitting threshold, in turn, should directly result in the low-expectation group having fewer timeouts, lower response times on miss trials, and, consequently, lower accuracy (Figure 1C&D).

Methods

Two independent participant groups completed the same experimental protocol with the only difference being their initial task instructions. The experimental methods are described in detail below, and they were the same design employed in Adamo, Cox, Kravitz, & Mitroff(2019). The data for the low-expectation group were previously published as part of a different project with different goals (Porfido, Cox, Adamo, & Mitroff., 2020; start of semester Spring 2019 cohort).

Participants

Participants were recruited from The George Washington University’s Psychology department’s subject pool and received course credit. There were two independent participant cohorts: the low-expectation group (N=39, 9 male/ 30 female, mean age 19.67 years, SD 1.47) and the high-expectation group (N=45, 5 male/ 40 female, mean age 19.42 years, SD 1.16). Four additional participants from each group were determined to have contributed outlier data, and their data were removed from all analyses. Outliers were defined as participants whose data were more than two standard deviations from the mean

for their own group for easy single-target hit rate (1 low- and 2 high-expectation), false alarm rate (2 low- and 1 high-expectation), or timeout rate (1 low- and 1 high-expectation).

Stimuli

The stimuli are identical to previously published work (Adamo, Cox et al., 2019; different salience condition). Each search display contained 25 items (each $1.3^\circ \times 1.3^\circ$ assuming a 60 cm viewing distance) presented in an invisible 8×7 grid (jittered 0–4 pixels from the center of each position within the grid). All items were pairs of perpendicular bars with a small gap between them; targets were perfectly aligned to create a 'T' shape and distractors had the crossbars slightly offset by 1–5 pixels from center to create an 'L' shape. Each item was presented in one of four rotations (0° , 90° , 180° , and 270°) on a white background. Items were either easy-to-spot high-salience (a grey of 57–65% black; 50% of targets; 5% of distractors) or hard-to-spot low-salience (a grey of 22–45% black; 50% of targets; 95% of distractors). Each display contained one or two targets with the remaining items being distractors. Displays were generated as matched triplets where each dual-target trial (i.e., a trial with two targets present) was made up of an easy target, a hard target, and 23 distractors. Then the matching easy single-target trial was made by converting the hard target into a low-salience distractor and the hard single-target trial was created by converting the easy target into a high-salience distractor. This matched display process was implemented in line with suggestions for properly examining the impact of multiple targets on visual search (Adamo, Cox et al., 2019).

Procedure

Participants were seated approximately 60 cm from a 19-in LCD monitor without head restraint. The stimuli were presented with MATLAB Psychtoolbox (Kleiner, Brainard, & Pelli, 2007). Participants used a computer mouse to click on found targets and pressed the spacebar when finished with the trial. To mimic the time pressures of real world searches, participants had a 15-second time limit per trial. Failure to complete the search in the time allotted resulted in a timeout warning message, and targets found up to that point were counted as hits. Participants started with 12 practice trials, which consisted of a randomized presentation of an equal number of each of the three types of trial (dual-target, easy single-target, and hard single-target) with feedback on hits, misses, and false alarms. Participants then completed 9 experimental blocks that each contained 33 trials with no feedback on accuracy (only the timeout warning). Each block contained an equal number of each trial type (dual-target, easy single-target, hard single-target) amounting to 99 total trials of each type across the entire experiment. Displays from the matched triplets were distributed across the first, middle and last third of the blocks in a counterbalanced fashion (See Adamo, Cox et al., 2019 for additional details). The dual-target trials played a key role in the procedural manipulation to generate different expectations across groups, but they

were secondary for the planned analyses; the primary focus was on first target search performance.

Critical procedural manipulation of interest

The manipulation of interest in the current study was the pre-experiment instructions provided to the participants. The participants in the *low-expectation group* received the following instructions: “On each trial there will be up to 2 targets, meaning you will find 0, 1, or 2 targets.” The participants in the *high-expectation condition* received the following instructions: “On each trial there will be 1 or 2 targets”. There were no other methodological differences between the two participant cohorts.

Results

The primary variables of interest were accuracy (hit rate, false alarm rate) and response time (time to find first target, search termination time on miss trials), as well as timeout rate. The analyses focused on whether performance differed between the participant groups (low-expectation and high-expectation groups). To anticipate the results, the simple and seemingly innocuous one-sentence difference in the initial pre-experiment instructions led to multiple significant differences that suggest the two groups differed in their quitting threshold during search.

Figure 2. Analysis of search termination times and timeout rates for the high-expectation (blue) and low-expectation (orange) groups. Reported p-values are from planned pairwise comparisons. All error bars depict the S.E.M. A) Total search times on hard and easy single-target trials where the participants failed to find the target. B) Timeout rates for hard and easy single-target trials. C) The proportion of participants in each group whose misses were entirely due to timeouts for hard and easy single-target trials.

Search termination times and timeout rates

Evidence for a lower quitting threshold in the low-expectation group compared to the high-expectation group can be seen via analyses of the total trial time and trial timeouts. Participants were to terminate each trial by pressing the spacebar when they had either

clicked on each target they found or decided to move on to the next trial. Participants were encouraged not to reach the 15-second timeout limit.

Total search time was compared between the two participant groups. For this analysis trials that reached the 15-second timeout limit were included in the analyses with a value of 15 seconds since removing the timeout trials would have skewed the data with some participants losing excessive numbers of trials (see analysis of timeouts below). Due to the boundary at 15 seconds the total search time data is not normally distributed, therefore the nonparametric Mann-Whitney U test was used to compare groups.

Total search time for the low-expectation group was significantly shorter on trials without successfully finding a target on both hard single-target trials (low-expectation: mean = 9.43s, SD = 2.12s, high-expectation: mean = 13.74s, SD = 2.40s; $Z=6.34$; $p<0.001$) and easy single-target trials (low-expectation: mean = 9.60s, SD = 2.15s, high-expectation: mean = 13.35s, SD = 2.70s; $Z=5.41$; $p<0.001$; N.B., performance on easy single targets was very high—two participants from the high-expectation and one from the low-expectation group never missed an easy target), see Figure 2A. In contrast, when a target was found on single-target trials, there was no significant difference in the total search time between the two groups for either the hard single-target trials (low-expectation: mean = 9.56s, SD = 1.61s, high-expectation: mean = 9.03s, SD = 1.58s; $Z=-1.27$; $p=0.20$) or the easy single-target trials (low-expectation: mean = 8.88s, SD = 1.54s, high-expectation: mean = 8.43s, SD = 1.70s; $Z=-1.12$; $p=0.26$).

Additional support for an effect of the instructions manipulation on the quitting threshold comes from the timeout rates. Again, the nonparametric Mann-Whitney U test was used to compare groups because the timeout rate data were not normally distributed. Consistent with a lower threshold for quitting, the low-expectation group had a significantly lower timeout rate than the high-expectation group for hard single-target trials (low-expectation: mean = 4.72%, SD = 6.76%, high-expectation: mean = 11.99%, SD = 6.74%; $Z=4.95$; $p<0.001$) and easy single-target trials (low-expectation: mean = 2.46%, SD = 2.86%, high-expectation: mean = 4.35%, SD = 3.82%; $Z=2.73$; $p<0.01$), see Figure 2B, but there was no difference in timeout rates on dual-target trials (low-expectation: mean = 1.35%, SD = 2.72%, high-expectation: mean = 1.17%, SD = 1.43%; $Z=1.23$; $p=0.22$). No significant difference in time-to-quit and the timeout rates between groups once a target was detected shows that instruction manipulation successfully set different expectations for 0 vs 1 target, but not 1 versus 2 targets.

Arguably the most striking support for an effect of the instruction manipulation on quitting thresholds came from comparing the proportion of participants who only missed targets due to timeouts. Specifically, this analysis looked at the rate of participants who never

terminated a trial prior to the 15-second limit without finding a target—their only missed targets came from trials in which they searched fruitlessly for 15 seconds, see Figure 2C. On the hard single-target trials, 1/39 (2.56%) low-expectation participants only missed targets due to timeouts compared to a much larger proportion of 31/45 (68.89%) for the high-expectation group ($\chi^2=38.97$; $p<0.001$). The same pattern held for the easy single-target trials, 2/39 (5.13%) low-expectation participants only missed targets due to timeouts compared to 26/45 (57.78%) for the high-expectation group ($\chi^2=26.06$; $p<0.001$).

Table 1

Experimental Measure	Low-Expectation Group	High-Expectation Group	Statistical Comparison
<u>Total Search Time</u>			
Hard single-target misses	9.43s (2.12s)	13.74s (2.40s)	$Z=6.34$; $p<0.001$
Easy single-target misses	9.60s (2.15s)	13.35s (2.70s)	$Z=5.41$; $p<0.001$
Hard single-target hits	9.56s (1.61s)	9.03s (1.58s)	$Z=-1.27$; $p=0.20$
Easy single-target hits	8.88s (1.54s)	8.43s (1.70s)	$Z=-1.12$; $p=0.26$
<u>Timeout Rate</u>			
Hard single-target trials	4.72% (6.76%)	11.99% (6.74%)	$Z=4.95$; $p<0.001$
Easy single-target trials	2.46% (2.86%)	4.35% (3.82%)	$Z=2.73$; $p<0.01$
Dual-target trials	1.35% (2.72%)	1.17% (1.43%)	$Z=1.23$; $p=0.22$
<u>Hit Rate</u>			
Hard single-target trials	70.33% (11.67%)	82.56% (9.87%)	$t(82)=5.20$; $p<0.001$
Easy single-target trials	90.97% (7.02%)	94.46% (4.56%)	$t(82)=2.73$; $p<0.01$
Hard (dual-target trials)	64.89% (12.95%)	64.85% (15.38%)	$t(82)=-0.10$; $p=0.92$
Easy (dual-target trials)	88.05% (7.16%)	86.80% (7.01%)	$t(82)=-0.81$; $p=0.42$
Dual-target: Both Hit	56.16% (14.23%)	53.09% (18.76%)	$t(82)=-0.84$; $p=0.41$
<u>Hit Response Time</u>			
Hard single-target trials	5.40s (0.84s)	5.93s (0.88s)	$t(82)=2.80$; $p<0.01$
Easy single-target trials	3.28s (1.02s)	3.43s (1.15s)	$t(82)=0.64$; $p=0.52$
<u>False Alarm Rate</u>			
Hard single-target trials	3.16% (6.46%)	5.84% (7.04%)	$Z=2.78$; $p<0.01$
Easy single-target trials	2.88% (6.65%)	3.30% (4.34%)	$Z=1.79$; $p=0.07$
Dual-target trials	2.31% (4.49%)	2.31% (3.04%)	$Z=0.39$; $p=0.70$

Hit rate and response time

Table 1 provides accuracy and response time results and statistical tests. For single-target trials, a 2x2 ANOVA with factors of group (low- and high-expectation) and target difficulty (easy, hard) produced significant main effects of both factors and a significant interaction

(main effect of group $F(1,82)=23.86$, $p<0.001$, $\eta_p^2=0.225$; target difficulty $F(1,82)=268.09$, $p<0.001$, $\eta_p^2=0.766$; group x difficulty interaction $F(1,82)=19.37$, $p<0.001$, $\eta_p^2=0.191$). As predicted by a difference in quitting thresholds between groups (Figure 1 & 2), the low-expectation group had a lower hit rate than the high-expectation group for both hard single-target trials (low-expectation: mean = 70.33%, SD = 11.67%, high-expectation: mean = 82.56%, SD = 9.87%; $t(82)=5.20$; $p<0.001$) and easy single-target trials (low-expectation: mean = 90.97%, SD = 7.02%, high-expectation: mean = 94.46%, SD = 4.56%; $t(82)=2.73$; $p<0.01$). Furthermore, as predicted the effect of the instruction manipulation was greater for the hard targets, see Figure 3A.

Figure 3. A) Hit rate and B) response time data and analyses for easy and hard single-target trials for the high-expectation (blue) and low-expectation (orange) groups. P-values are from planned comparisons across groups using two-sample t-tests. The error bars depict the S.E.M.

Based on the prediction that the instruction manipulation would set different expectations about the first to-be-found target and the lack of a difference in search termination time after one target was found, an effect of group was not predicted for accuracy in the dual-target trials. The 2x2 ANOVA on hit rate in the dual-target trials bore this prediction out, with no significant main effect of group, no significant interaction, and only a main effect of target difficulty (main effect of group $F(1,82)=0.15$, $p=0.70$, $\eta_p^2=0.002$; target difficulty $F(1,82)=261.64$, $p<0.001$, $\eta_p^2=0.761$; group x difficulty interaction $F(1,82)=0.11$, $p=0.74$, $\eta_p^2=0.001$).

For response time on single-target trials (time to click on the target), the 2x2 ANOVA produced significant main effects of group and target difficulty, but not a significant interaction (main effect of group $F(1,82)=4.31, p=0.041, \eta_p^2=0.050$; target difficulty $F(1,82)=276.541, p<0.001, \eta_p^2=0.771$; group x difficulty interaction $F(1,82)=1.82, p=0.18, \eta_p^2=0.191$). Follow-up t-tests revealed that the participant groups significantly differed for the hard single targets (low-expectation: mean = 5.40s, SD = 0.84s, high-expectation: mean = 5.93s, SD = 0.88s; $t(82)=2.80; p<0.01$), but did not significantly differ in response time on easy single targets (low-expectation: mean = 3.28s, SD = 1.02s, high-expectation: mean = 3.43s, SD = 1.15s; $t(82)=0.64; p=0.52$), see Figure 3B.

Stability of initial expectation effect

Repeated measures ANOVAs were conducted to examine the evolution of the significant instruction manipulation effects reported above. The instruction manipulation groups served as a 2-level between-subject factor (low-expectation, high-expectation) and the experimental blocks served as a 9-level within-subject factor (blocks 1-9). The primary interest in these analyses were the interaction terms, as significant interactions would suggest that the two groups were differentially affected by experience across the full experimental session while non-significant interactions would be consistent with an initial difference between the groups being maintained across the full experiment. The ANOVA for the hard single-target hit rate showed a main effect of group ($F(1,82)=26.58; p<0.001; \eta_p^2=0.245$), but no main effect of block ($F(8,656)=1.49; p=0.16; \eta_p^2=0.018$) nor a significant interaction ($F(8,656)=1.46; p=0.17; \eta_p^2=0.018$), see Figure 4A. The ANOVA for the easy single-target hit rate revealed main effects of group ($F(1,82)=7.36; p<0.01; \eta_p^2=0.082$) and block (Greenhouse-Geisser corrected; $F(6.19,507.46)=5.15; p<0.001; \eta_p^2=0.059$) but no significant interaction (Greenhouse-Geisser corrected; $F(6.19,507.46)=1.53; p=0.17; \eta_p^2=0.018$), see Figure 4B. The ANOVA for the hard single-target response time showed a main effect of group ($F(1,82)=10.52; p<0.005; \eta_p^2=0.114$) and block ($F(8,656)=2.05; p<0.05; \eta_p^2=0.024$), but no significant interaction ($F(8,656)=0.86; p=0.55; \eta_p^2=0.010$), Figure 4C. Given there was no effect of the instruction manipulation on the easy single-target response time data, the effect was not examined by block.

The block x group analyses suggest that the instruction manipulation effects on single-target hit rate and response time were present from the beginning of the experiment and did not significantly decrease. That is, despite the fact that participant groups were run on the same underlying distribution of targets, their pre-experiment expectations appear to have set the groups onto different trajectories that were not impacted by the true trial structure. Specifically, this is consistent with the hypothesis that the initial instructions set the participants' expectations about target prevalence, with the low-expectation group setting a lower quitting threshold. Without feedback, this expectation was able to shape the

experienced target distribution creating a self-fulfilling prophecy wherein the low-expectation group experienced fewer targets, and fewer hard targets specifically.

Figure 4. Analysis of the significant effects of high-expectation (blue) versus low-expectation (orange) on hit rate and response time as a function of experimental block to examine the evolution of the effect over time. The difference between groups was present at the beginning of the experiment and remained present over the course of the experiment for A) easy single-target hit rate, B) hard single-target hit rate, and C) hard single-target response time. The group x block interaction effect was not significant for all three measures. Error bars depict the S.E.M.

Item-level false alarm rates

The above analyses establish an effect of the simple instruction manipulation on search performance, but questions remain about the cause(s) of the effect. The prediction was that the instruction manipulation would alter participants' quitting threshold, but it was possible that the participants' decision criterion could have also been affected when evaluating each individual stimulus within a display (Wolfe & Van Wert, 2010). The current experiment did not include target absent trials, which does not allow for assessing false alarms at the trial level and signal detection theory analysis. However, the response mechanism did allow for an item-level analysis of false alarms; on each trial participants used the computer mouse to click on any item they felt was a target, which provided an assessment of false alarms as the rate at which they incorrectly identified distractors as targets.

Item-level false alarms were relatively infrequent (Table 1) and false alarm rates were not normally distributed across participants, necessitating the use of nonparametric statistical comparisons. The Mann-Whitney U test was used to compute statistically significant differences in false alarm rate between the low- and high-expectation groups across trials. There were significant differences between the low- and high-expectation groups in the percentage of trials that contained item-level false alarms for the hard single-target trials (low-expectation mean = 3.16%, SD = 6.46% vs. high-expectation mean = 5.84%, SD = 7.04%, $Z=2.78$, $p<0.01$) and a trend for easy single-target trials (low-expectation mean =

2.88%, SD = 6.65% vs. high-expectation mean = 3.30%, SD = 4.34, $Z=1.79$, $p=0.073$). There was no significant difference for dual-target trials (low-expectation mean = 2.31%, SD = 3.04% vs. high-expectation mean = 3.01%, SD = 4.49%, $Z=0.39$, $p=0.70$). These results potentially suggest that in addition to a change in quitting threshold, the instruction manipulation might have also led to a shift in the decision criterion.

Discussion

In this study, two participant groups completed the same simple visual search task, but with one difference in their procedures—the “low-expectation” group was given initial instructions that suggested that there could be no-target trials and the “high-expectation” group that was given initial instructions that suggested that all trials contained at least one target. This singular difference in the initial experimental instructions placed the participant groups on different trajectories from the start based on their pre-search expectations about the nature of the search environment (i.e., whether it would be typical or atypical to fail to find a target on a given trial).

The initial expectations in the absence of feedback created completely different search experiences between the groups. Specifically, a significant change in quitting threshold, made it so that the low-expectation group experienced a search environment in which targets were not always expected, hard-to-spot targets seemed to be more rare, and it was more sensible to quit a trial without having found any target. Without trial-by-trial accuracy feedback, the difference in each group’s initial search expectations led to performance differences that remained across the experiment—regardless of the fact that the groups were doing the identical task.

While the current study demonstrated significant differences in performance based on initial expectations, not every search setting will be equally susceptible. There are a wide variety of professional, and non-professional, settings that involve visual search, and each has its own set of particular circumstances. For example, if a search is easy enough that, despite any initial expectation differences, target accuracy is near perfect, then initial differences in expectations would not lead to a difference in experience of the underlying target distribution, and any initial differences in search times would likely dissipate over time. However, many critical professional searches are quite difficult with imperfect accuracy (e.g., Pinto & Brunese, 2010), suggesting this would not be a mitigating factor in many cases.

Overall, the current results are rather straightforward—in the absence of corrective feedback, searchers will experience a self-fulfilling prophecy wherein their own expectations actually mold their experiences. More broadly though, this project is a stark reminder of the importance of subtle differences of instruction. A single sentence

difference in the initial instructions led to different expectations, which led to different quitting thresholds which, in turn, led to lower accuracy. This has clear implications for research efforts, emphasizing the need to carefully and intentionally determine the instructions for each experiment as a seemingly subtle change could produce wildly different outcomes. Importantly, such meaningful differences from a minor wording change in instructions also highlights the complex nature of replications from one lab to another (Open Science Collaboration, 2015).

Beyond the lab, the current results have implications for professional search scenarios that cannot provide immediate case-by-case accuracy feedback. In such environments, it is important to consider professional searchers' incoming expectations when they are not provided with real-time feedback, as their expectations can guide their self-chosen quitting threshold. Moreover, care must be taken when requiring a professional searcher to move from one search scenario to another, because it may not be trivial to instantaneously update internal expectations and to set appropriate search parameters. For example, radiologists may be provided with patient history, which can create different expectations that result in changes in detection accuracy (e.g., Carney et al., 2012). In routine mammography exams, only 0.5% of the cases will have cancer present (Breast Cancer Surveillance Consortium, 2009), however if the patient has the BRCA1 or BRCA2 genetic mutation (colloquially referred to as the "breast cancer gene"), there is a 82% chance of developing cancer during their lifetime (King, Marks, & Mandell, 2003). These are dramatically different expected prevalence situations, which can impact the radiologist's expectations going into the read.

Another implication of the current work for professional searches stems from the fact that while the two participant groups differed in their expectation based on the initial instructions, one was actually accurate as there was in fact a target on every trial. This highlights the point that with appropriate information about the nature of the environment, searchers' expectations can potentially have a beneficial effect. That is, if searchers have accurately set their expectations, they can perform well in their particular environment even in the absence of case-by-case feedback. However, to provide the searchers with accurate information, each professional search industry needs to have "ground truth" information about their particular setting. The more that is known about the rate and nature of potential targets, the better that training efforts can be designed to empower the workforce to be efficient and effective. Industries may be well-served to do a self-study to determine the ground truth for their particular environment and then use this knowledge to make the best attempt at setting appropriate expectations.

Declarations

- **Ethics approval and consent to participate**

The research was approved by the Institutional Review Board at The George Washington University, and all participants provided written consent at the start of the experiment.

- **Consent for publication**

Not applicable.

- **Availability of data and material**

The data and materials are available upon reasonable request to the authors.

- **Competing interests**

The authors declare that they have no competing interests.

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- **Authors' contributions**

All authors made substantial contributions to the design and theoretical construction of the project. PHC, DJK, and SRM all obtained funding to support their efforts during this project. PHC led the analysis and writing efforts. All authors contributed to the writing and to the submission process.

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