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**Ironic capture: Top-down expectations exacerbate distraction in visual search**

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RUNNING HEAD: Ironic processing in visual search

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**Abstract**

Ironic processing refers to the phenomenon where attempting to resist doing something results in a person doing that very thing. Here, we report three experiments investigating the role of ironic processing in visual search. In Experiment 1, we informed observers that they could predict the location of a salient color singleton in a visual search task and found that response times were slower in that condition than in a condition where the singleton's location was random. Experiment 2 used the same experimental design but did not inform participants of the color singleton's behavior. Experiment 3 showed that the cost in the predictable condition was not do to dual task costs or block order effects and that participants attempting to use the strategy showed a larger cost in the predictable condition than those who abandoned using that location foreknowledge. In this case, responses in the predictable color singleton condition were equivalent with the random color singleton condition. This suggests that having more knowledge about an upcoming, salient distractor ironically increases its interfering influence on performance.

A visual search for an item in an environment can conceivably be guided in two different ways. One is that search can proceed by looking for the known target features such as color, form, shape, or location. This is the most common and well-studied form of visual search with a multitude of studies demonstrating that search can be guided by known target features. Furthermore, during the search for target items, distracting items become inhibited. For example, studies investigating how non-spatial features guide attention have shown that when a stimulus is explicitly defined as the goal of the visual search, the distractor stimuli become inhibited (Cepeda, Cave, Bichot, & Kim, 1998; Chelazzi, Miller, Duncan, & Desiomne, 1993; Gaspelin, Leonard, & Luck, 2015; Lamy, Tsal, & Egeth, 2003). This distractor inhibition during search raises a second search possibility; search is guided away from known distractor features. On the one hand, generally speaking, the human cognitive system is poor at purposely not doing something, a phenomena known as ironic processing (Wegner, 2009). In contrast to this general phenomena, however, visual search studies investigating whether distractor information can be used to make search more efficient have found a range of results (Arita, Carlisle, & Woordman, 2012; Beck & Hollingworth, 2015; Becker, Hemsteger, & Peltier, 2016; Munneke, Van der Stigchel, & Theeuwes, 2008; Tsal & Makovski, 2006). In the current study, we report three experiments investigating the extent to which visual search can be made more efficient by knowledge of a salient distractor's location using a cueless design that isolates top-down influences on visual search.

*Ironic processing*

The degree to which negative information can be used to guide actions (as in, information indicating what not to do) has been studied in a variety of domains. For example, one study measured golf putting errors when participants were told to land the ball on the target compared

to being told to land the ball on the target, but be particularly careful not to overshoot the target (Wegner, Ansfield, & Pilloff, 1998). The group that was told to not overshoot ended up significantly overshooting compared to the control group (this was only the case when subjects also experienced mental load by counting down from 1000). In the same study, the investigators had participants hold a handheld pendulum under instructions to hold it steady or to not let it move in a given direction. Consistent with ironic processing, the group that was told to not let the pendulum move in a given direction was more likely to let it move in that direction.

Ironic processing also occurs in a word association task (Wegner & Erber, 1992). Words participants are being told to suppress and not report are more likely to be reported than if the participants are told the word and asked to focus on it. Ironic processing also has therapeutic implications. In a study looking at smoking behavior, subjects in the thought suppression group reported that they smoked more than those in the thought expression group (Erkine, Georgiou, & Kvavilashvili, 2010). For the most part, however, the study of ironic processing has used tasks tapping into fairly high-level processes. In the current study, we are interested in whether ironic processing effects can be found in response times (RTs) on a simple visual search task.

*Distractor inhibition*

Distractor inhibition is not a new subject for the visual search literature. One method of studying the impact of distractor inhibition on visual search has been to look at how repeating target and distractor locations change visual search performance. The repetition of target and distractor locations across trials has been found to modulate search performance such that when a target repeats the previous target's location, or when distractors repeat the previous distractor's location, search is faster (Asgeirsson, Kristjánsson, & Bundesen, 2014; Maljkovic & Nakayama, 1996; Rabbitt, Cumming, & Vyas, 1979). In contrast, when targets appear in the previous

distractor's location or when distractors repeat the previous target's location, performance suffers. Although these effects have been considered instances of the top-down control of attention (Hillstrom, 2000), they occur without the observer's explicit knowledge or intention (the current study's interest) and have been suggested to be due to a separate category of attentional control altogether (Awh, Belopolsky, & Theeuwes, 2012).

Previous studies have used a few different methods to examine whether observers can intentionally ignore known distractor locations. In one study, participants completed a flanker task (Eriksen & Eriksen, 1974) in which the task-irrelevant flankers appeared at the same location trial-by-trial (Lahav, Makovski, & Tsal, 2012; Tsal & Makovski, 2006). On a subset of trials, two dots appeared, one at a flanker's location and one at a neutral location, and participants had to report which of the two dots appeared first. Tsal and colleagues predicted that if participants were inhibiting the distractor's locations they should perceive dots appearing at those locations as appearing after those appearing at a neutral location (i.e., neutral-location cues should exhibit prior entry, Shore, Spence, & Klein, 2001). Surprisingly, however, participants reported dots appearing at the flanker's location as appearing before those appearing at neutral locations, suggesting that attention was allocated at the distractor's location. This evidence, however, is only circumstantial concerning the question at hand. This is because while inhibiting the flanker location would be a good strategy in general, on a subset of trials target probes appeared at the flanker location. Due to that, some attention would need to be allocated to the flanker location. Also, since the probes appeared randomly and at the flanker's location, it may be argued that these studies did not include true foreknowledge regarding the distractor, which is the focus of the present experiment.

More relevant for our current purposes, another method for testing whether observers can use foreknowledge of a distractor's location to inhibit it has been to cue the distractor's location (Chao, 2010; Munneke et al., 2008). In those studies, an arrow cue indicating a distractor's location with 100% validity preceded the search display. This cue led to shorter RTs relative to no cue and decreased compatibility effects between distractors and targets (Chao replicated the benefit for cueing the distractor location, but not the compatibility benefit decrease), so long as the interval between the cue and target was sufficiently long. In a similar vein, a study by Van der Stigchel and Theeuwes (2006) had participants make eye movements towards targets appearing in locations above or below fixation, cued by an arrow with 100% validity. On 80% of trials a distractor appeared at the same time as the target. The distractor always appeared to the left of top targets and to the right of bottom targets. Their analysis indicated that the eye movements curved away from the expected distractor location, even on trials where no distractor appeared. It has also been shown that knowledge of which color to ignore on a given trial increases search RTs if a distractor is that color (Moher & Egeth, 2012). Furthermore, this effect reverses if placeholders indicating the color of items appearing there in the future are presented 800 ms before the search array. A finding consistent with the attentional selection of the to-be-ignored color which then turns to inhibition, rather than the direct, purposeful inhibition of the distractor.

What can we learn from the above precueing studies regarding whether distractor foreknowledge helps or hurts performance? The arrow precueing studies (Chao, 2010; Munneke et al., 2008) demonstrated that precueing distractor locations helped performance under some conditions, but also do not provide strong evidence for the explicit use of top-down control for distractor inhibition. This is because the sparse displays used makes it possible that, rather than

inhibiting the distractor's location, participants were attending the remaining locations (Beck &

Hollingworth, 2015; Becker et al., 2016). Using cues, along with a long cue-target onset

asynchrony and no eye movement monitoring, also leads to the possibility that the observed

inhibition results from inhibitory oculomotor processes (Jollie, Ivanoff, Webb, & Jamieson,

2016; Klein & Hilchey, 2011). In our study, we implement a design meant to address each of

these concerns to gain a clearer picture about whether or not the visual system can use advance

foreknowledge of a distractor's location to inhibit it.

*The current study*

To test whether individuals can use foreknowledge of a distractor's location to inhibit it,

we used an additional singleton paradigm in which targets, shape singletons, and distractors,

including a color singleton, are presented simultaneously and never at the same location as each

other (Theeuwes, 1992). As a measure of attentional capture, performance is compared on trials

where a color singleton is present to trials in which the color singleton is absent with difference

being the degree to which the color singleton captured attention. In order to test how additional

singleton foreknowledge affects attentional capture, we included four blocks (Experiments 1 and

2) of trials. One in which there was no additional singleton (the none condition), one in which

the additional singleton appeared at random locations, but never at the same location as the target

(random condition), one in which the additional singleton appeared at the same location on every

trial (static condition), and one in which additional singleton moved clockwise around the

display one location at a time (predictable condition). For our current purposes of investigating

whether individuals can use distractor location foreknowledge to inhibit that distractor, the

critical comparison is the predictable compared to random conditions. If individuals can use

foreknowledge of an upcoming distractor's location to inhibit that distractor (Chao, 2010;

Munneke et al., 2008), RTs will be faster in the predictable compared to the random condition. If, however, ironic processing (Wegner, 2009) occurs within the additional singleton task, then it is possible that distractor location foreknowledge will lead to an attentional shift to the distractor's location, resulting in slower RTs. In addition, if explicit knowledge can only improve performance in the static, and not the predictable, singleton condition, then successful distractor inhibition would likely reflect visual memory biasing the attentional system away from the distractor's location (Chun & Jiang, 1998).

## Experiment 1

In Experiment 1, we had participants complete an additional singleton experiment including a condition where the additional singleton moved predictably from one position to another across trials. We explicitly informed them this would be the case and that they should attempt to ignore the additional singleton. By comparing this condition to a condition where the additional singleton appeared in a random location every trial we were able to directly measure the influence of the participants' attempts to inhibit the additional singleton based on foreknowledge. Because the stimuli were identical in the random and predictable conditions, any difference between the conditions can only be attributed to the participants' attempts to use the foreknowledge of the distractor's location to inhibit the distractor.

**Method**

*Participants*

Fifteen undergraduates from the University of Toronto participated and were compensated with course credit. All participants provided informed consent and reported normal or corrected-to-normal vision. The sample size was chosen on the basis of previous studies using

the additional singleton paradigm (e.g., Theeuwes, 1992) and data was collected until we had

fulfilled this sample size before any analysis was completed.

*Stimuli and apparatus*

The experiment was conducted on a PC connected to a CRT monitor (screen resolution:

$1024 \times 768$; refresh rate: 85 Hz). Stimuli were created and presented using Matlab by Mathworks

with the Psychophysics toolbox (Kleiner, Brainard, Pelli, Ingling, Murray, & Broussard, 2007).

Stimuli consisted of search items, with one circle and seven squares subtending 2.4° visual angle,

drawn in red [CIE: luminance = 17.4 cd/m², x = 0.59, y = 0.324]  or green [CIE: luminance =

17.1 cd/m², x = 0.296, y = 0.545], and white lines 2° in length and two pixels wide. A chin and

head rest was used to maintain an approximate viewing distance of 52 cm. Responses were made

with the right and left index fingers using the "z" and "/" keys on a QWERTY keyboard.

*Procedure*

The trial sequence (*Figure 1*) started with an isolated, centrally presented white fixation

cross on a black background. Following 1500 ms, a series of shapes (seven squares and one

circle) appeared arranged in a circle with a radius of 6° around the fixation cross. Appearing

simultaneously with the shapes, we presented white lines inside the shapes oriented 5° clockwise

or counter-clockwise of vertical. We asked participants to find the circle and report whether the

line inside it was oriented to the left or right as quickly as they could without sacrificing

accuracy. The trial ended and was counted as an error if no response was made within 3000 ms

of stimulus onset. In the event of an error, "MISTAKE" was printed at the center of the screen

and remained for 2000 ms. The next trial began immediately following a response or the end of

the error display. In all blocks with an additional singleton, we instructed participants to ignore it

and find the shape singleton. Critically, we verbally fully informed the participants of the

additional singleton's presence/absence and behavior; in the predictable singleton condition, we told participants that the color distractor would move clockwise around the display on each subsequent trial, and in the static singleton condition, we told participants that the color distractor would always occupy the same position on each subsequent trial. In all blocks participants were instructed to report the line in the shape singleton's orientation while ignoring the color singleton. Lastly, the first 20 trials of each block were considered practice and were not analyzed.



1500 ms                 until response
                        or 3000 ms
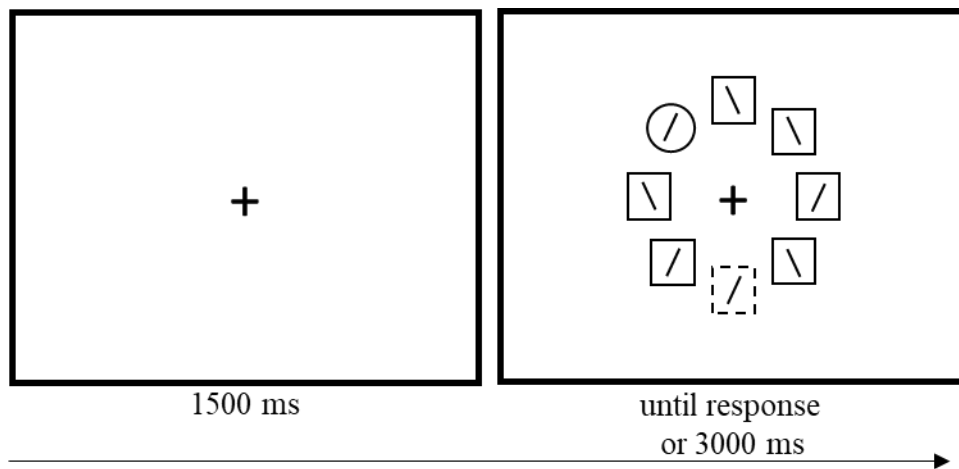
**Figure 1. Stimulus sequence and example display. The dashed square is a different color than the rest of the display. Participants searched for the circle and reported the orientation of the line in the circle. In the none condition, all of the search items were the same color. The display was shown until a response or until 3000 ms had elapsed.**

*Design*

Participants completed the four blocks of 100 test trials in a random order determined by the experimental program before each session. In the baseline condition, all the search items were drawn in the same color. In the random condition, one random square search item was drawn in a different color (red or green) than the remaining search items such that the additional singleton's color changed trial-by-trial. In the predictable condition, one search item was again drawn in a different color, and trial-by-trial it rotated clockwise around the display. In the static

condition, the additional singleton remained in the same location across all the trials. In all

blocks, the target's position was determined randomly trial-by-trial with the constraint that it

could not appear at the same location as the additional singleton, if present. Within each block,

additional singleton color and target line direction were counterbalanced and randomized (which

were collapsed across for analyses.

**Results**

Overall, participants were highly accurate with no error rates higher than 5.5% in any

condition. For the RT analyses, these error trials and any trials with RTs less than 100 ms were

removed. This represented 4.6% of the trials with no individual participants missing more than

9.75% of trials. We then conducted a one-way, repeated measures ANOVA with additional

singleton condition as the independent factor and RTs as the dependent measure. The ANOVA

revealed a main effect of additional singleton condition, $F(3,42) = 6.901$, $p = .001$, $\eta_p^2 = .330$

(*Figure 2*). Planned, paired *t*-tests (two-tailed, $\alpha = .05$) revealed that this reflected the expected

effect that RTs in the random singleton condition (675 ms) were longer than both the static

singleton condition (625 ms), $t(14) = 3.020$, $p = .009$, 95% CI [14.38,84.86] and the no singleton

condition (625 ms), $t(14) = 2.155$, $p = .049$, 95% CI [.24,100.61]. Importantly, RTs in the

predictable condition (734 ms) were longer than the no singleton condition, $t(14) = 2.566$, $p =$

.022, 95% CI [17.95,200.95], the static condition, $t(14) = 3.209$, $p = .006$, 95% CI

[36.03,181.26], and, more surprisingly, the random condition, $t(14) = 2.621$, $p = .020$, 95% CI

[10.71,107.33]. Lastly, the static condition did not differ from the no singleton condition, $t(14) <$

1, $p = .968$, 95% CI [-42.93,41.32].
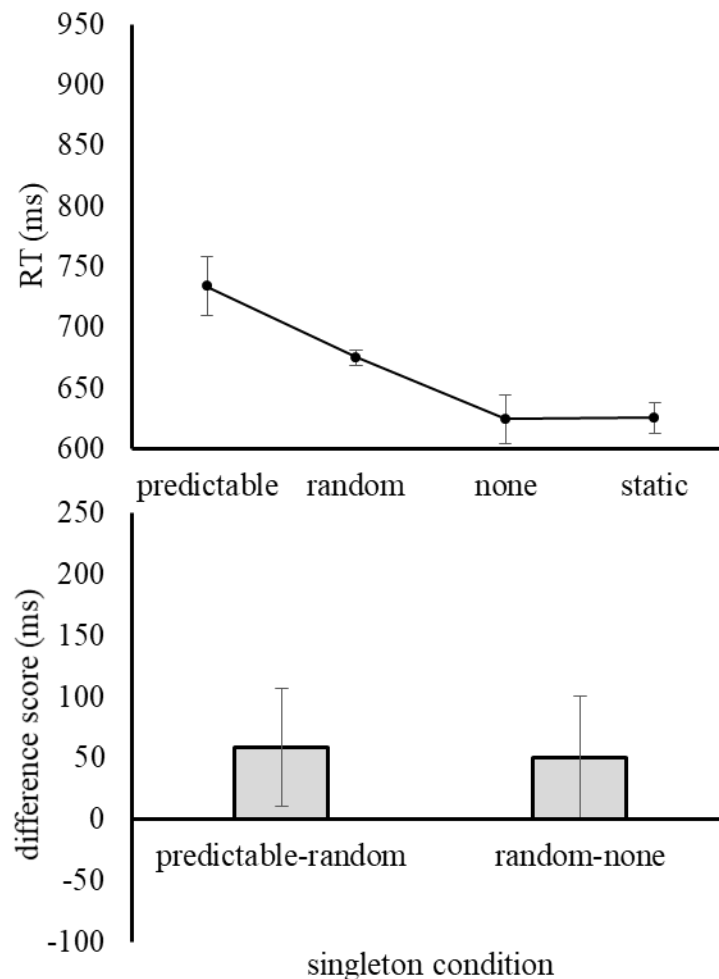
**Figure 2. Experiment 1 data. Top panel: Response times by singleton condition. Error bars represent the within subjects *SE*. Bottom panel: The difference scores for the relevant conditions. Error bars in the upper panel represent within-subject SEs (Cousineau, 2005). Error bars in the lower panel represent the 95% confidence interval for the mean difference (Pfister & Janczyk, 2013).**

To control for possible confounds we also ran a number of additional analyses.

Controlling for between trial position priming (Geyer, Zehetleitner, & Müller, 2010; Gokce,

Müller, & Geyer, 2015; Maljkovic & Nakayama, 1996), we compared the predictable and

random conditions after removing trials from the random condition in which the additional

singleton on trial *n* repeated the additional singleton's location from trial *n*-1, and still found a

significant difference, $t(14) = 2.564$, $p < .022$, 95% CI [9.43,105.80]. We also coded trials for

whether the current trial's additional singleton was the same or different color than the trial

before it (Becker, 2007) and ran a 2 (color repeat/switch) × 4 (additional singleton condition)[1],

repeated measures ANOVA. The color repeat/switch factor did not interact with our additional

singleton condition main effect, $F(3,42) = 1.536$, $p = .219$, $\eta_p^2 = .099$.[2] We also tested whether

the predictable-random difference interacted with whether participants completed the random or

predictable condition first and found a marginally significant interaction in a two-way, mixed

ANOVA with block order as a between subjects factor, $F(1,13) = 4.150$, $p = .063$, $\eta_p^2 = .242$.

This indicated that participants that completed the predictable condition before the random

condition were slower in the predictable condition, $t(8) = 2.815$, $p = .023$, 95% CI

[16.79,168.95], but those that completed the random condition first did not, $t(5) < 1$, $p = .398$,

95% CI [-14.69,31.19]. Next, to test the spatial specificity of the random and predictable

singleton effects, we analyzed RTs within each condition as a function of distance from the

additional singleton. We found no main effect of distance, $F(1,13) < 1$, $p = .517$, $\eta_p^2 = .060$, or an

interaction between condition and distance, $F(1,13) < 1$, $p = .643$, $\eta_p^2 = .045$. Lastly, to control

for practice effects, we also compared RTs for the last 60 trials of the predictable and random

blocks and still found slower RTs in the predictable condition, $t(14) = 2.348$, $p = .034$, 95% CI

[6.23,137.95].

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[1] In the no additional singleton case, a switch is when all the display items changed color from the previous trial.

[2] There was a marginal main effect of color repeat/switch, $F(3,42) = 3.316$, $p = .090$, $\eta_p^2 = .191$, with mean RTs being 7 ms slower on switch compared to repeat trials, consistent with previous work (Becker, 2007).

To check for speed-accuracy trade-offs, the same ANOVA as above was conducted with errors as the dependent variable and no effect was found, $F(3,42) = 1.122$, $p = .351$, $\eta_p^2 = .074$, indicating no speed-accuracy trade-off.

**Discussion**

To summarize, in the predictable condition, when observers attempted to ignore a distractor based on foreknowledge of its location, RTs increased relative to the random condition, even though both conditions used the same stimuli. This difference could not be accounted for by selection history, additional singleton switching/repeating color, or practice effects. Notably, however, only participants that completed the predictable condition first showed the predictable-random cost. It is possible that by completing the random condition before the predictable condition participants acquired a baseline for how long the task should take and, upon realizing the active inhibition strategy was not working in the predictable condition (i.e., was slowing them down, relative to previous blocks), abandoned it.[3] Some indirect evidence for this hypothesis comes from looking at the predictable-random difference across the trial block in each group. For the predictable first group, the difference became larger across each block quartile (77, 75, 108, and 111 ms). For the random first condition, however, the difference was largest in the first quartile of the block (34, -12, 17, and -17 ms). Overall, these data indicate that it is possible for completely spatially-predictable distractors to more effectively capture attention than completely spatially-unpredictable distractors.

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[3] Of course, this hypothesis also assumes that the effect is phenomenologically valid. In our experience of testing the experimental program, we found that this was case, though we have much more experience with these tasks than our participants do. Anecdotal conversations during participant debriefing, however, suggested that participants experienced the same costs.

Why is it the case that the static additional singleton did not slow down RTs? This finding is consistent with visual search studies which have found that when distractors repeat location visual search is more efficient (Chun & Jiang, 1998; Jiang & Wagner, 2003). Those studies have participants complete a visual search task in which they experience search displays with the same distractor arrangement many times across the experiment. They find that search efficiency improves across the experiment as participants gain more experience with the search displays even though they do not notice that the displays repeat. This suggests a visual memory mechanism that interacts with the attentional system to allow it to deploy attention more efficiently. Similarly in the current study, participants repeatedly experienced the color singleton distractor in the same location such that the same visual memory mechanism may have caused attention to be deployed elsewhere, leading to no cost of there being a color singleton present.

Before discussing the attentional capture possibility further, however, another possible alternative explanation needs to be addressed. It is possible that the random-predictable cost reflects a statistical learning effect (Zhao, Al-Aidroos, & Turk-Browne, 2013). In particular, it may be the case that, rather than the effect being due to participants attempting to intentionally inhibit the predictable singleton, the predictability of the singleton in the predictable condition lead to greater attentional capture, generating a cost in the that condition. Experiment 2 tests this possibility.

## Experiment 2

When sequences of stimuli consistently follow a pattern, those stimuli capture attention relatively to randomly presented stimuli, all else being equal (Zhao, Al-Aidroos, & Turk-Browne, 2013). In Zhao et al.'s study, statistically predictable sequences of stimuli were shown to capture attention without the participants' awareness of the sequence. Thus, it is possible that

the predictable singletons in Experiment 1 captured attention because of the predictable location

sequence and not because participants were actively attempting – and ironically failing at –

inhibiting them. To test for this, Experiment 2 replicated Experiment 1, except that we did not

inform participants about the additional singleton's behavior. If implicit statistical learning

accounts for the slowed responses in Experiment 1's predicable condition Experiment 2 will

replicate that effect. However, if the slowed responses were due to explicit attempts at top-down

inhibition RTs in the predictable and random conditions will be equivalent in Experiment 2,

since no foreknowledge about the singleton's behavior was given to participants.

*Participants*

Fifteen undergraduate psychology students from the University of Toronto participated in

exchange of course credit or $10 cash. None of the participants had participated in Experiment 1

and all were naïve to the purpose of the study. All participants provided informed consent and

reported normal or corrected-to-normal vision.

*Stimuli, apparatus, procedure, and design*

All aspects of Experiment 2 were the same as Experiment 1 with the exception of how we

instructed participants. Whereas in Experiment 1 we fully informed participants of the additional

singleton's behavior, in Experiment 2 we did not include this information. We did, however,

continue to tell participants to ignore the additional singleton if it was present.

**Results**

Participants were once again highly accurate with a mean error rate of 3.7%. Data were

trimmed using the same method as Experiment 1. This resulted in 4.1% of trials being removed

for analysis with a minimum of 71 trials remaining in any individual cell. We conducted a one-

way, repeated measures ANOVA with additional singleton condition as the independent factor

and RT as the dependent variable (*Figure 3*). Once again, there was a reliable main effect,

$F(3,42) = 3.838$, $p = .016$, $\eta_p^2 = .215$. The pattern of results was similar to Experiment 1; RTs

were longer in the random condition (719 ms) than in the no singleton condition (666 ms), $t(14)$

$= 3.459$, $p = .004$, 95% CI [20.29,86.54]. RTs were similar in the random and static conditions

(689 ms), $t(14) = 1.805$, $p = .093$, 95% CI [-5.7,66.67]. RTs were also longer in the predictable

(737 ms) than in the no singleton condition $t(14) = 2.402$, $p = .032$, 95% CI [7.57,133.69], but

not the static condition, $t(14) = 1.593$, $p = .133$, 95% CI [-16.52,111.89]. Critically, RTs were

similar in the predictable compared to the random condition, $t(14) < 1$, $p = .421$, 95% CI [-27.32,61.75].
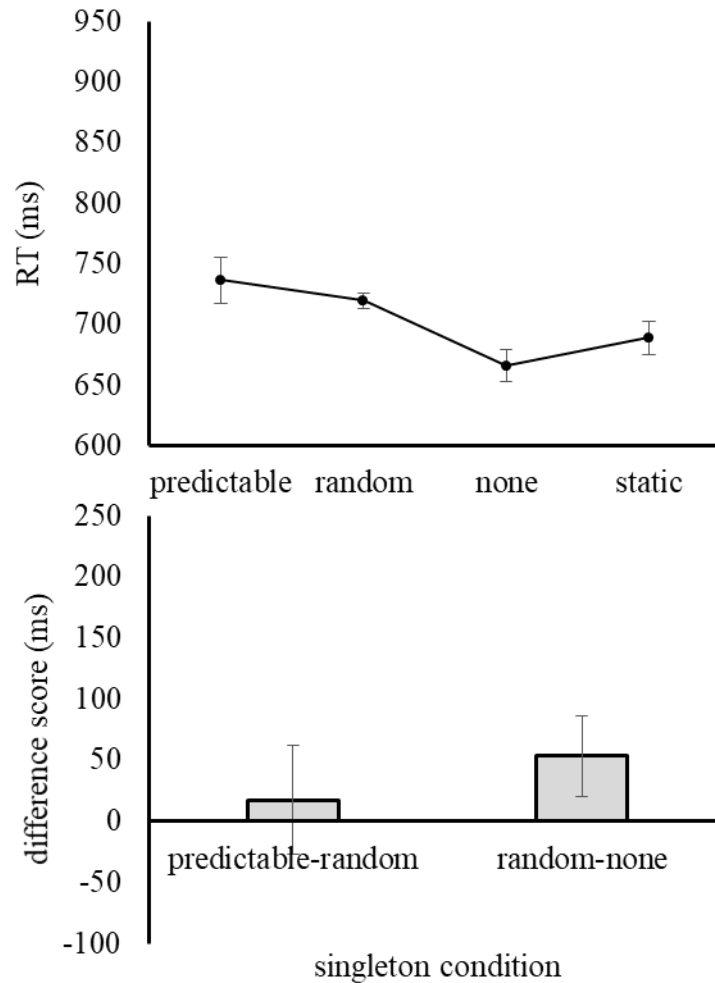
**Figure 3. Experiment 2 data. Top panel: Response times by singleton condition. Error bars represent the within-subjects *SE*. Bottom panel: The difference scores for the relevant conditions. Error bars in the upper panel represent within-subject SEs (Cousineau, 2005). Error bars in the lower panel represent the 95% confidence interval for the mean difference (Pfister & Janczyk, 2013).**

As with Experiment 1, the random-predictable comparison was unchanged by removing

the position repeat trials, $t(14) < 1$, $p = .643$, 95% CI [-51.37,80.47], by only considering the last

60 trials in each block, $t(14) < 1$, $p = .612$, 95% CI [-48.75,79.92], and whether the additional

singleton repeated or switched colors across trials did not interact with our additional singleton

condition main effect, $F(3,42) < 1$, $p = .838$, $\eta_p^2 = .020$.[4] Critically, however, block order no

longer interacted with condition, $F(1,13) = 2.502$, $p = .138$, $\eta_p^2 = .161$ and there was no

difference in block order between the two experiments, $\chi^2 = .536$, $p = .715$.

To check for speed-accuracy trade-offs, the same ANOVA as above was conducted with

errors as the dependent variable and no effect was found, $F(3,42) = 1.154$, $p = .339$, $\eta_p^2 = .076$,

indicating no speed-accuracy trade-off.

## Discussion

In Experiment 2, we observed two important findings. When we did not inform

participants about the predictable singleton, there was no longer a cost in that condition relative

to the random condition. Furthermore, whereas in Experiment 1 we found evidence of block

order affecting the predictable-random cost, raising the possibility it was a practice effect, there

was no such interaction in Experiment 2 despite the block orders being equally distributed.

Together, these indicate that Experiment 1's effects were caused by participants actively

attempting to use their knowledge of the predictable singleton's location to resist capture.

It may be surprising that we did not observe a statistical learning effect in the current

experiment. One difference between this experiment and those showing that statistical

regularities capture attention is that our regularity was spatial whereas studies showing that

regularities capture attention have mostly used non-spatial features. Of course, there is a large

sequence learning literature in which the locations of targets follow specific sequences (Cohen,

Ivry, & Keele, 1990; Destrebecqz & Cleeremans, 2001; Grafton, Hazeltine, & Ivry, 1995). Those

show that participants successfully learn the sequences as indicated by faster RTs with more

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[4] Color repeat/switch main effect, $F(3,42) = 2.599$, $p = .129$, $\eta_p^2 = .157$.

exposure to the sequence (up down a certain point where performance reaches ceiling). It does not follow from this, however, that a distractor appearing in predictable locations necessarily will capture attention. Indeed, Experiment 2 suggests that distractor location regularities may not capture attention, something that may be worthwhile examining in and of itself in future research.

## Experiment 3

Experiment 3 replicated the first experiment while controlling for several possible alternative explanations for why RTs were slower in the predictable condition. First, we counterbalanced block order to control for block order effects. This was done because block order effects can be strong in similar tasks. For example, completing a feature search task (searching for a specific shape among a heterogeneous shape distractors) before a singleton search task (searching for the different shape among homogenous shape distractors) can eliminate the additional singleton effect (Leber & Egeth, 2006). By counterbalancing the block order we can eliminate the possibility that the predictable-random cost is due to block order effects. Second, we had participants self-start trials by pressing the spacebar in order to give them as much time as necessary to track and try to inhibit the upcoming distractor location to reduce cognitive load effects. Third, we recorded the tilt of the line in the additional singleton so that we could check for compatibility effects between it and the target line. The presence or absence of such a compatibility effect allows us to examine the spatial specificity of the observed effects (Theeuwes & Burger, 1998; but see Gibson & Bryant, 2008). Finally, at the end of the predictable singleton block, we showed participants a gray search array and asked them to click on the location where the additional singleton would be on the next trial. This allowed us to

check to see whether a given participant continued to use the distractor foreknowledge throughout the block.

*Participants*

Twenty four undergraduate psychology students from the University of Toronto participated in exchange of course credit. None of the participants had participated in Experiments 1 or 2 and all were naïve to the purpose of the study. All participants provided informed consent and reported normal or corrected-to-normal vision.

*Stimuli, apparatus, procedure, and design*

Experiment 3 replicated Experiment 1 with the following changes. The block order was counterbalanced across participants rather than randomized, we no longer included the static additional singleton condition, each block was shortened to 64 trials, and participants self-started the trials by pressing the spacebar. In addition, at the end of the predictable singleton block, we asked participants to click on where the additional singleton would have been on the next trial to verify that they performed the active-ignoring strategy.

**Results**

Participants were highly accurate with an error rate of 6.7%. Data were trimmed using the same method as the previous experiments resulting in 7.7% of trials being removed for analysis. We conducted a one-way, repeated measures ANOVA with additional singleton condition as the independent factor and RT as the dependent variable (*Figure 4*). Once again, there was a reliable main effect, $F(2,46) = 19.494$, $p < .001$, $\eta_p^2 = .459$. The results replicated those from Experiment 1; RTs were longer in the random condition (777 ms) than in the no singleton condition (678 ms), $t(23) = 4.370$, $p < .001$, 95% CI [52,146]. Critically RTs were longer in the predictable

singleton (906 ms) condition than in the random singleton condition, $t(23) = 3.141$, $p = .005$, 95% CI [44,214].
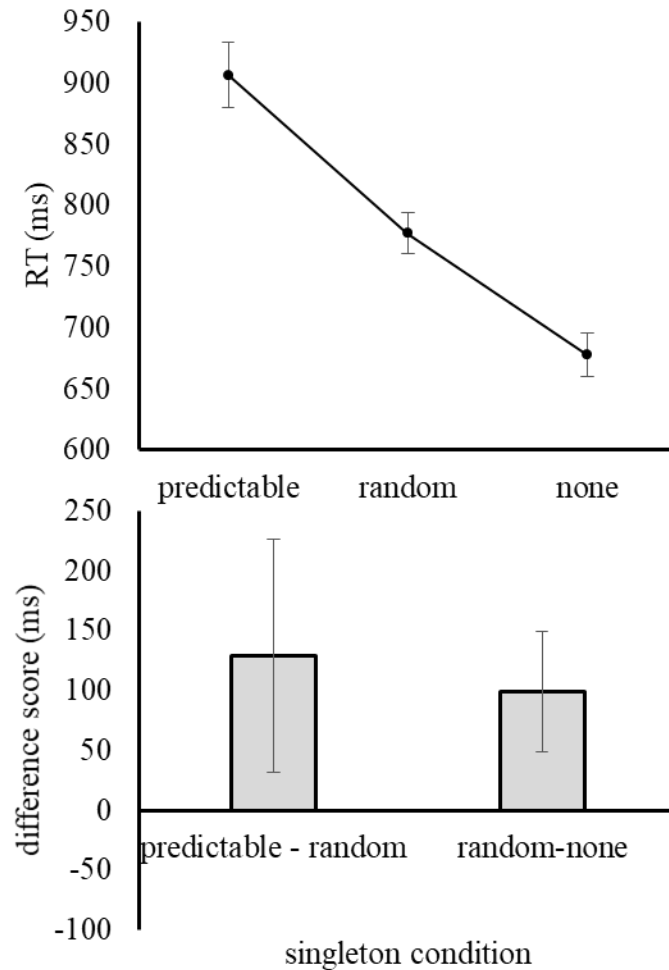


**Figure 4. Experiment 3 data. Top panel: Response times by singleton condition. Error bars represent the within-subjects *SE*. Bottom panel: The difference scores for the relevant conditions. Error bars in the upper panel represent within-subject SEs (Cousineau, 2005). Error bars in the lower panel represent the 95% confidence interval for the mean difference (Pfister & Janczyk, 2013).**

In addition to the above analysis, we performed a number of follow-up analyses. The predictable-random difference remained after excluding trials from the random condition in which the distractor location repeated across trials, $t(23) = 3.165$, $p = .004$, 95% CI [45.45, 217.01]. We tested for a compatibility effect between the line in the additional singleton and the

target line and whether this compatibility effect interacted with condition. There was no main effect of target-distractor compatibility, $F(2,46) < 1$, $p = .896$, $\eta_p^2 = .001$, nor was there an interaction, $F(2,46) < 1$, $p = .601$, $\eta_p^2 = .012$. We also tested whether the distance between the target and additional singleton affected RTs. There was no main effect of target-distractor distance, $F(2,69) = 2.059$, $p = .086$, $\eta_p^2 = .082$, and the interaction was not significant, $F(2,46) = 1.115$, $p = .349$, $\eta_p^2 = .046$. The color repeat/switch analysis revealed a main effect of color repeat/switch, $F(1,23) = 12.972$, $p = .002$, $\eta_p^2 = .361$. There was also an interaction between color repeat/switch and singleton condition, $F(2,44) = 8.293$, $p = .001$, $\eta_p^2 = .265$. This interaction indicated faster RTs in the color repeat condition than the switch condition in the predictable and random, but not in the none conditions. Finally, we split the data by whether or not the participant successfully selected the location that the predictable singleton would have appeared in on the next trial (10 people successfully selected the singleton's future location). We then tested whether these two groups of participants differed in performance across the conditions. This analysis revealed that the two groups did differ in performance across conditions, $F(2,44) = 9.317$, $p < .001$, $\eta_p^2 = .298$. In particular, the random-predictable difference was significant in the group that successfully selected where the singleton would be, $t(9) = 3.983$, $p = .003$, 95% CI [124,450], but this difference was not significant in the group that did not successfully predict the singleton's location, $t(13) < 1$, $p = .869$, 95% CI [-61,144].

To check for speed-accuracy trade-offs, the same ANOVA as above was conducted with errors as the dependent variable and no effect was found, $F(2,46) < 1$, $p = .810$, $\eta_p^2 = .009$, indicating no speed-accuracy trade-off.

**Discussion**

While controlling for block order effects and allowing participants to self-start trials to reduce any dual-task costs, we replicated Experiment 1. In particular, RTs in the random condition were significantly slower than the none condition and RTs in the predictable condition were significantly slower than the random condition. There were various other observations as well. We did not find a compatibility effect between the line in the additional singleton and the target line in the predictable or random singleton condition. This is consistent with previous work indicating that compatibility effects are not found when the additional singleton paradigm is implemented with target/distractor color changes (Becker, 2007). Furthermore, once again there was no target-distractor distance effect. Not finding the distance effect is consistent with Becker's findings leading to the possibility the costs in the predictable and/or random additional singleton conditions result from non-spatial filtering. More important for our purposes, however, that the predictable-random difference was once again significant. Whether this difference is related to spatially specific capture or non-spatial filtering will require more research. In any case, it is clear that performance is worse in the predictable singleton condition, consistent with attempting to ignore a particular area of space ironically causing attention to shift to that location, leading to a higher likelihood of attentional capture.

In Experiment 3 we attempted to reduce the possibility that the observed predictable-random cost was due to dual-task costs by having participants self-start trials. The underlying idea being that this would allow them as much time as they needed to inhibit the distractor's location such that there was only the main task of finding the target once the stimuli appeared. It was certainly possible that allowing participants as long as they needed would have led to an inhibition effect as previous studies have shown cueing a distractor benefits search, but only at

longer cue-target intervals (Chao, 2010; Moher & Egeth, 2012). It remains possible, however, that there is a different dual-task cost taking place that may account for the predictable-random difference. In particular, it is possible that on each trial individuals must encode and process the singleton location so that they can continue tracking its location. Since this is only possible in the predictable singleton condition, the RT increase in that condition may reflect that additional processing.[5]

## General Discussion

In the current study, we demonstrate that top-down knowledge of an upcoming distractor can ironically lead to increased interference by that distractor. When participants were given information about the additional singleton's location, RTs were longer than when additional singletons appeared in random locations, even after controlling for selection history (Belopolsky et al., 2010), practice effects, additional singleton color switch/repeat (Becker, 2007), and statistical regularity learning (Zhao et al., 2013). Therefore, it appears that participants attempted to apply their knowledge of the additional singleton's location which made attentional capture by the additional singleton more likely. This phenomenon demonstrates that top-down attempts to suppress visual distractors can, ironically, lead to increased distraction has implications for researchers interested in visual search as well as ironic processing.

There has been dispute over how data from the additional singleton paradigm should be interpreted. On the one hand, the paradigm was designed as a way of measuring attentional capture. That is to say, slower RTs in the presence of additional singletons should be interpreted as the additional singleton capturing spatial attention. Spatial attention must then disengage from the additional singleton before orienting towards the target. This has been tested using eye

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[5] Thank you to an anonymous reviewer for this suggestion.

movement as a proxy for spatial attention (Theeuwes, de Vries, Godijn, 2003). Those studies

find that eye movements are erroneously generated towards the additional singleton on

significantly more trials than expected by chance. This proportion, however, is relatively low.

This has implications for interpreting the current predictable-random difference. Specifically, it

is possible that attempting to inhibit the predictable singleton's location caused attention to be

moved to that location. If spatial attention is more likely to orient to the singleton first when its

location is intentionally suppress, this would naturally lead to slower RTs in the predictable

compared to random condition.

On other hand, other investigators have argued that slower RTs in the random compared

to no additional singleton condition are due to non-spatial filtering costs. In particular, Becker

(2007) has suggested that when there is feature uncertainty in an additional singleton task, the

ability to set a filter to ignore the additional singleton is impeded. This leads to slower RTs in the

random condition that are unrelated to attentional capture. That suggestion is consistent with our

random singleton findings in which neither the distance between the additional singleton and the

target nor whether the item within the additional singleton was compatible with the target

affected RTs. It is unclear, however, how non-spatial filtering costs such as those could be

exacerbated by knowing where the additional singleton would be. In either case, the

foreknowledge hurt performance in a significant way with more work being needed to explain

how it comes to do so.

The current data fits well with the findings from previous studies that indicate that there

is little evidence for a top-down, actively deployable spatial inhibition mechanism.[6] Since our

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[6] This is not say that there is no inhibitory component of spatial attention. A number of studies
have reported evidence of inhibitory mechanisms (Gaspelin, Leonard, & Luck, 2015; 2017;
Gaspar & McDonald, 2014; Sawaki & Luck, 2010). Our argument, however, is that these

experiments only differed in task instructions, any differences in the data of the two experiments

is limited to participants attempting to use such an inhibitory mechanism. We suggest that

previous studies that have claimed to have demonstrated such an active inhibition mechanism

(Chao, 2010; Munneke et al., 2008; Serences, Yantis, Culberson, & Awh, 2004) can be

explained by participants attending to locations they know would not have the distractor rather

than attempting to ignore the distractor (Beck & Hollingworth, 2015; Becker et al., 2016) or by

oculomotor inhibition (Jollie et al., 2016; Klein & Hilchey, 2011). This is not to say that

inhibitory mechanisms do not exist in visual search; rather, inhibition occurs as part of selecting

targets and not purposely inhibiting distractors (Cepeda et al., 1998). When participants do

attempt to inhibit distractors, it appears to instead cause attention to orient to the distractor's

location and increase interference.

Of notable relevance to the current study, Tsal and Makovski (2006) found that probes

appearing at distractor locations are perceived to appear before those appearing elsewhere, and

Moher and Egeth (2012) found that cueing a distractor's color led to slower RTs in a visual

search task. Here we extend these findings in a few ways. First, Tsal and Makovski presented

targets at the distractor's location such that the location was not entirely task irrelevant. We show

that distractor location information continues to hurt search performance even when this is not

the case. Second, while Moher and Egeth showed that giving non-spatial information regarding a

distractor's identity impedes visual search performance, we demonstrate that giving spatial

information does so as well. This is an important demonstration given that Moher and Egeth

proposed that the non-spatial cue was being used in order to find the distractor after which the

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inhibitory mechanisms are not, at least within the given task, capable of being used intentionally.
That is, the observers could not voluntarily inhibit an areas of space in advance of to-be-attended
stimuli.

distractor's *location* was inhibited. Given that proposal, it is possible that giving individuals location information directly would have allowed them to preemptively inhibit the location. This is especially interesting given our intertrial interval was 1,500 ms (or as long as the participant needed; Experiment 3) and they found that presenting a preview placeholder array (including color information) for 800 ms was sufficient for allowing individuals to inhibit the distractor location. Given that, our intertrial interval should have been sufficient for allowing participants to select the area of space and inhibit before the search array occurred. This suggests that the type of inhibition suggested by Moher and Egeth requires a stimulus to operate upon. That is to say that space can be inhibited in these sorts of tasks, but that it becomes inhibited through the inhibition of stimuli in that space.

What does underlie the current effect? One possibility is that participants' attention was involuntarily shifted towards the area of space that they were trying to ignore. Once the singleton appeared at that location they were then obliged to process it because of their attention being focused there, thus slowing down the mean RTs. If that were the case, why was there not evidence of a target – distractor compatibility effect? It could be the case that the line within the additional singleton was not processed in the depth necessary to lead to a compatibility effect once it was identified as a distractor (which was facilitated as the item there was known to be a distractor ahead of time. What about the lack of a target-distractor distance effect? One possibility is that because the target is the only remaining singleton attention is moved in a highly efficiently to it such that RTs as highly similar regardless of the target-distractor distance.

A distinction should be made between hypothesized mechanism under study currently and another form of foreknowledge; specifically, Yantis and Jonides (1990) demonstrated that cueing the location of a upcoming target negated the distracting effect of an onset stimulus. The

reason for this being that cueing targets allows the participants to focus their attention on the

target location (for a related idea in the additional singleton task, see: Belopolsky & Theeuwes,

2010; Belopolsky, Zwaan, Theeuwes, & Kramer, 2007). This demonstrates that attention can be

oriented intentionally to locations. Given our data, however, it does not seem like the inhibitory

component of attention can be similarly intentionally allocated to locations.

It is also clear that distractor inhibition does occur in visual search tasks similar to the one

used here. The difference between the current study and previous studies that have shown

distractor inhibition is that we asked participants to intentionally inhibit the additional singleton,

whereas previous studies have looked at implicit memory based distractor inhibition. For

example, the distractor location from trial n – 1 becomes inhibited on trial n. This is supported by

the observation that RTs in these tasks are fastest when the target appears at the previous target

location, slowest when the target appears at the previous distractor location, and in the middle

when the target appears at a neutral location (Geyer, et al., 2010; Gokce et al., 2015). Geyer et al.

also demonstrate that the inhibition effect occurs when a three item search set is used on 80% of

trials with the remaining 20% using a six item set, but not when the probabilities are reversed.

This demonstrates a role of expectation in the distractor inhibition effect, conceptual relevant to

the current study. It is still different than the current study, however, in that the predictability

manipulation was through using probability rather than the current study which asks participants

to intentional inhibit upcoming distractors (which cannot appear at the same location twice in the

predictable condition). Taken together, there appears to be strong support for implicit, memory

based distractor inhibition, but little support from the previous studies or the current experiments

that this inhibition can occur intentionally

It is also noteworthy that our design did not use any sort of cue, which allowed us to isolate the influence of top-down attentional control. When studies use cues that are considered top-down, typically arrows, there often is a visual asymmetry between the different cue displays such that stimulus driven contributions cannot be completely ruled out (e.g., Hommel, Pratt, Colzato, & Godijn, 2001). We believe that the use of instruction-based manipulations is the most powerful way to isolate the effects of top-down control from other factors (such as bottom-up salience and selection history; Awh et al., 2010).

While these experiments were intended to test the possibility of top-down inhibition in search, we note that they also exemplify the possibility of studying high-level cognitive phenomena in visual cognition tasks typically used to study lower-level phenomena (e.g., Rajsic, Wilson, & Pratt, 2015). Our current data is consistent with phenomenon known as ironic processing in which thinking about not doing something makes people more likely to do that very thing (Wegner, 2009). For example, trying not to overshoot a golf putt makes people more likely to overshoot the hole (Wegner, Ansfield, & Pilloff, 1998). That a relatively low-level task might be used to study higher level cognitive effects is consistent with the idea that the brain implements similar mechanisms across the processing hierarchy (Egner, 2008; Hasson, Chen, & Honey, 2015). The advantage of using visual search to study higher level cognitive effects is that it is highly controlled, but also flexible with a well-modelled parameter space (e.g. Bundesen, Vangkilde, & Petersen, 2014; Cave, 1999; Zelinsky, 2008) that will allow for testing a diverse range of processes related to ironic processing theory.

**Compliance with ethical standards**

**Conflict of interest:** The author declares that he has no conflict of interest.

**Ethical approval:** All procedures performed were in accordance with the ethical standards of

the institutional and/or national research committee and with the 1964 Helsinki declaration and

its later amendments or comparable ethical standards.

**Informed consent:** All participants provided informed consent before participating in the study.

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