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Test–retest reliability of value-driven attentional capture

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Abstract

Attention is biased toward learned predictors of reward. The degree to which attention is automatically drawn to arbitrary reward cues has been linked to a variety of psychopathologies, including drug dependence, HIV-risk behaviors, depressive symptoms, and attention deficit/hyperactivity disorder. In the context of addiction specifically, attentional biases toward drug cues have been related to drug craving and treatment outcomes. Given the potential role of value-based attention in psychopathology, the ability to quantify the magnitude of such bias before and after a treatment intervention in order to assess treatment-related changes in attention allocation would be desirable. However, the test–retest reliability of value-driven attentional capture by arbitrary reward cues has not been established. In the present study, we show that an oculomotor measure of value-driven attentional capture produces highly robust test–retest reliability for a behavioral assessment, whereas the response time (RT) measure more commonly used in the attentional bias literature does not. Our findings provide methodological support for the ability to obtain a reliable measure of susceptibility to value-driven attentional capture at multiple points in time, and they highlight a limitation of RT-based measures that should inform the use of attentional-bias tasks as an assessment tool.

Keywords Selective attention · Reward learning · Eye movements · Test–retest reliability

Associative reward learning can change the attentional priority of visual stimuli, such that learned predictors of reward acquire the ability to automatically capture attention. This phenomenon, referred to as *value-driven attentional capture*, is supported by covert attention measures (e.g., Anderson, Laurent, & Yantis, 2011a, 2011b; Failing & Theeuwes, 2014), eyetracking (e.g., Anderson & Yantis, 2012; Le Pelley, Pearson, Griffiths, & Beesley, 2015; Theeuwes & Belopolsky, 2012), and stimulus-evoked brain activity using a variety of techniques, including functional magnetic resonance imaging (Anderson, 2017; Anderson, Laurent, & Yantis, 2014; Hickey & Peelen, 2015; Krebs, Boehler, Egner, & Woldorff, 2011), electroencephalography (MacLean & Giesbrecht, 2015; Qi, Zeng, Ding, & Li, 2013), positron emission tomography (Anderson, Kuwabara, et al., 2016; Anderson, Kuwabara, et al., 2017), and magnetoencephalography (Donohue et al., 2016; Hopf et al., 2015). Importantly, previously reward-associated cues have been shown to capture attention even when they are currently task-irrelevant and physically nonsalient, suggesting that reward history plays a direct role in the control of attention (see Anderson, 2013, 2016a, for reviews).

Recent evidence points to a possible role for value-driven attention in psychopathology. The degree to which an arbitrary reward cue impairs performance in an attention task has been linked to drug dependence (Anderson, Faulkner, Rilee, Yantis, & Marvel, 2013; Anderson, Kronemer, Rilee, Sacktor, & Marvel, 2016; see Anderson, 2016b, for a review), HIV-risk behaviors (Anderson, Kronemer, et al., 2016), depression (Anderson, Chiu, DiBartolo, & Leal, 2017; Anderson, Leal, et al., 2014), and attention deficit/hyperactivity disorder (Sali, Anderson, Yantis, Mostofsky, & Rosch, 2018). Attention to reward covaries with the presence of these psychopathologies and may play a role in the observed symptomology.

Interest in the use of attentional-bias measures for clinical assessment is not new, and in particular, it has a rich history in the context of addiction research (see Field & Cox, 2008, for a review). Experimental tasks probing addiction-related attentional biases typically involve the use of actual drug cues (e.g., pictures of drug paraphernalia or words describing drug use) and assess the degree to which such drug-related stimuli are processed in patients and drug-naïve controls. A wealth of evidence supports the idea that drug-related stimuli

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automatically capture the attention of drug-dependent patients (Hogarth, Dickinson, & Duka, 2003, 2005; Lubman, Peters, Mogg, Bradley, & Deakin, 2000; Mogg, Bradley, Field, & De Houwer, 2003; Nickolaou, Field, Critchley, & Duka, 2013; Nickolaou, Field, & Duka, 2013; Stormark, Field, Hugdahl, & Horowitz, 1997) and heavy users (Field, Mogg, Zetteler, & Bradley, 2004; Townshend & Duka, 2001), as compared to controls, and are related to drug craving (Field, Mogg, & Bradley, 2005; Field, Mogg, Mann, Bennett, & Bradley, 2013; Field et al., 2004; Franken, Kroon, Wiers, & Jansen, 2000; see Field, Munafò, & Franken, 2009, for a meta-analysis). A number of studies have demonstrated a predictive relationship between the strength of attentional biases toward drug cues and the subsequent treatment outcome (Carpenter, Schreiber, Church, & McDowell, 2006; Cox, Hogan, Kristian, & Race, 2002; Marissen et al., 2006; Powell, Dawkins, West, Powell, & Pickering, 2010; Waters, Shiffman, Sayette, et al., 2003), although the robustness of this relationship is controversial, and several failures to replicate it have also been documented (e.g., Field et al., 2013; Waters, Shiffman, Bradley, & Mogg, 2003).

Given the potential for attentional-bias measures as a clinical assessment tool, it would be especially interesting to examine whether attentional biases are modulated by treatment interventions or change with the progression of symptoms. For this interesting application to be explored, however, an attentional-bias measure must first be shown to exhibit robust test–retest reliability. If performance upon the first assessment is only weakly predictive of performance at subsequent assessments, any change in measured bias will be difficult to interpret, and the detection of genuine changes in bias will be unlikely. As straightforward as this consideration is, it poses a challenge that has plagued the attention literature.

A major limitation of the commonly used attentional-bias measures is their often weak reliability as a performance indicator. In the addiction literature, attentional-bias measures often lack internal reliability, let alone test–retest reliability (e.g., Ataya et al., 2012; Field, Marhe, & Franken, 2014). As a result, the use of attentional-bias measures as a clinical tool has been strongly cautioned against (Christiansen, Schoenmakers, & Field, 2015; Field et al., 2014). Attentional-bias measures for arbitrary nondrug stimuli have been subject to similar criticisms, with different attention tasks purporting to measure the same attention construct showing little or no relationship in performance (Kawahara & Kihara, 2011; Roque, Wright, & Boot, 2016) and with individual tasks exhibiting low internal reliability (Roque et al., 2016). Such attention measures are not suitable for use as a clinical assessment tool.

In the present study, we explore the test–retest reliability of two different measures of attentional capture by arbitrary reward cues. Given the breadth with which such measures have been linked to different psychopathologies (Anderson et al., 2013; Anderson, Chiu, et al., 2017; Anderson, Kronemer, et al., 2016; Anderson, Leal, et al., 2014; Sali et al., 2018), in addition to the potential nonclinical uses in predicting a broader range of life outcomes, we explored a general measure of reward-related attentional bias rather than a measure that uses stimuli particular to any one psychopathology (e.g., drug cues in a drug-dependent population).

Method

Participants

Thirty participants (18–35 years of age, M = 21.4 years; 20 female, 10 male) were recruited from the Texas A&M University community. Participants were compensated with money earned in the experimental task. All reported normal or corrected-to-normal visual acuity and normal color vision. The data from one participant were replaced due to an inability to reliably track eye position (resulting in a failure to register a target fixation on over 30% of trials). All procedures were approved by the Texas A&M University Institutional Review Board and conformed with the principles outlined in the Declaration of Helsinki.

Apparatus

A Dell OptiPlex equipped with the Matlab software and Psychophysics Toolbox extensions (Brainard, 1997) was used to present the stimuli on a Dell P2717H monitor. The participants viewed the monitor from a distance of approximately 70 cm in a dimly lit room. Eye position was monitored using an EyeLink 1000-plus desktop-mount eyetracker (SR Research, Ottawa, Ontario, Canada). Head position was maintained using an adjustable chin rest (SR Research).

Training phase

Each trial consisted of a fixation display, a search array, and a reward feedback display (see Fig. 1a). The fixation display remained on screen until eye position had been registered within 1.1° of the center of the fixation cross for a continuous period of 500 ms. The search array was then presented for 1,000 ms or until a fixation on the target was registered. The search array consisted of six colored circles, one of which was red or green on each trial. The color of the other five circles was drawn randomly from the set {blue, cyan, purple, orange, yellow, white} on each trial, without replacement. Each circle was approximately 3.6° of visual angle in diameter, placed at equal intervals along an imaginary circle with a radius of 10.2°. The reward feedback display was presented for 1,500 ms and consisted of the money earned on the current trial along with the updated total earnings (if the participant had

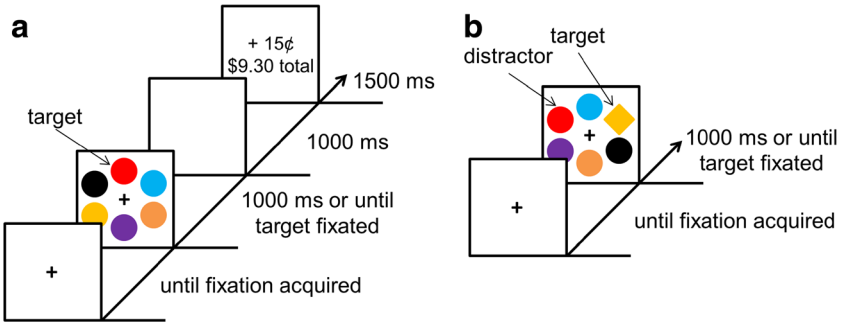

Fig. 1. Sequence and time course of trial events for the training phase (**a**) and the test phase (**b**)

failed to fixate the target before the timeout limit, the word "Miss" was presented in place of the money earned). A 1,000-ms blank screen was inserted between the search and feedback displays, and each trial concluded with a 500-ms blank interval.

Participants were instructed to fixate ("look directly at") the red or green circle on each trial and were informed that they would earn a small amount of money each time they did this within the time limit. Red and green target circles appeared equally often across trials within a block, with each color appearing equally often in each of the six stimulus positions. Correctly fixating one color target (red or green, counterbalanced across participants) was associated with an 80% probability of a high reward of 15¢ and a 20% probability of a low reward of 3¢ (high-value color), whereas for the other target color these percentages were reversed (low-value color). Each block consisted of 60 trials, the order of which was randomized.

Test phase

Each trial consisted of a fixation display (until fixation had been acquired for a continuous period of 500 ms), a search array (1,000 ms or until a fixation on the target was registered), a 1,000-ms blank interval, and, in the event of an incorrect response, a feedback display (1,000 ms). Each trial concluded with an additional 500-ms blank interval (Fig. 1b). Targets were now defined as the unique shape, either a diamond among circles or a circle among diamonds (equally often), which participants were instructed to fixate. The colors of the shapes were irrelevant to the task, and participants were instructed to ignore color. The feedback display consisted of the word "Miss" presented at the center of the screen.

One of the nontarget shapes was rendered in the color of the formerly high-value target (high-value distractor) on one-third of the trials, and likewise in the color of the formerly low-value target (low-value distractor) on another third of the trials. On the remaining one-third of trials, none of the shapes was rendered in the color of a formerly reward-predictive target (distractor-absent trials). The stimuli other than the critical distractor were drawn from the same color set used for nontargets in the test phase, and the same stimulus positions were used. The targets and distractors appeared equally often in each of the six possible stimulus positions across trials within a block. Each block consisted of 90 trials, the order of which was randomized.

Task procedure

Each participant scheduled both an initial lab visit and a four-week follow-up visit (same time and day of the week) prior to participating, and was allowed to reschedule the follow-up visit for up to one week after the initially scheduled time if the participant indicated an inability to maintain the original appointment. In each of the two lab visits, participants completed four blocks of trials of the training phase, followed by three blocks of trials of the test phase. Both the training and test phases were preceded by interactive instructions that included practice trials with and without the timeout limit. Participants were paid the amount of money earned in the training phase at the completion of the experiment. Other experimental tasks were included during the initial visit, as part of a different study focused on individual differences in susceptibility to value-driven attentional capture, and are not reported here.

Measurement of eye position

Head position was maintained throughout the experiment using an adjustable chin rest that included a bar upon which to rest the forehead (SR Research). Participants were provided a short break between different runs of the task, during which they were allowed to reposition their head to maintain comfort. Eye position was calibrated prior to each block of trials using a 9-point calibration (Anderson & Yantis, 2012) and was manually drift-corrected by the experimenter as necessary (the next trial could not begin until eye position had been registered within 1.1° of the center of the fixation cross for 500 ms; see, e.g., Nissens, Failing, & Theeuwes, 2017). During the presentation of the search array, the X and Y position of the eyes was continuously monitored in real time with

respect to the six stimulus positions, such that fixations were coded online (Le Pelley et al., 2015).

Analysis of fixations and response times

We measured which of the six shape stimuli was initially fixated on each trial, as well as whether the target was fixated before the timeout limit, along with the time required to fixate the target (i.e., the response time, RT). Fixation of a stimulus was registered if eye position remained within a region extending 0.7° around the stimulus for a continuous period of at least 50 ms (100 ms on the target, to trigger the termination of the stimulus array; see, e.g., Le Pelley et al., 2015). On distractor-absent trials, in order to quantify the probability of initially fixating a distractor for the sake of comparison, one of the nontargets was dummy-coded as the critical distractor on each trial, using the same parameters that were used to define the position of the critical distractors on distractor-present trials (i.e., the same counterbalance of position relative to the target position). The RT was measured from the onset of the stimulus array until a valid target fixation had been registered. RTs in fixating the target that exceeded three standard deviations of the mean for a given condition for a given participant were trimmed (Anderson & Yantis, 2012).

Results

Baseline performance

RTs differed across the three distractor conditions, $F(2, 58) = 5.88$, $p = .005$, $\eta_p^2 = .169$ (see Fig. 2a). RTs were slower on high-value distractor trials than on both low-value distractor trials, $t(29) = 2.72$, $p = .011$, $d = 0.50$, and distractor-absent trials, $t(29) = 3.00$, $p = .006$, $d = 0.55$, but did not differ between low-value distractor and distractor-absent trials, $t(29) = 0.98$, $p = .337$. Errant fixations also differed significantly by distractor condition, $F(2, 58) = 8.53$, $p = .001$, $\eta_p^2 = .227$ (Fig. 2b). Both the high-value distractor, $t(29) = 3.70$, $p = .001$, $d = 0.67$, and the low-value distractor, $t(29) = 2.38$, $p = .024$, $d = 0.44$, drew initial fixations more frequently than did a nontarget on distractor-absent trials. The difference in the frequencies of initial fixations between the high-value and low-value distractors was also significant, $t(29) = 2.11$, $p = .044$, $d = 0.39$. A valid fixation was registered on the target on over 95% of all trials, which did not differ across the three distractor conditions, $F(2, 58) = 0.11$, $p = .895$ (high-value, 95.3%; low-value, 95.4%; distractor-absent, 95.2%).

Test–retest reliability

In all, 76.67% of the participants returned to the lab to repeat the training and test phases, whereas the remaining participants were lost to follow-up (i.e., they did not complete their originally scheduled second visit and were uninterested in rescheduling). For the returning participants, the cost in RTs associated with the high-value distractor (relative to distractor-absent trials) at Visit 2 was uncorrelated with the RT cost observed at Visit 1, $r = .113$, $p = .607$ (Fig. 3a). How frequently the high-value distractor was fixated, however, was robustly correlated across visits, $r = .798$, $p < .001$ (Fig. 3b). The ability to predict Visit 2 distractor fixations from Visit 1 distractor fixations remained robust when we included as a covariate the percentage of trials on which a nontarget was fixated on distractor-absent trials during Visit 1, $\beta = .762$, $p < .001$, suggesting that the observed relationship was driven by individual differences in value-based distraction over and above difficulty fixating the target more generally.

Discussion

Our results establish the test–retest reliability of value-driven attentional capture as an individual-differences measure. Such reliability is a necessary precondition for use of the measure to track performance before and after a treatment intervention or a change in the status or state of an individual (e.g., changes in symptomology). In this way, tracking the percentage of

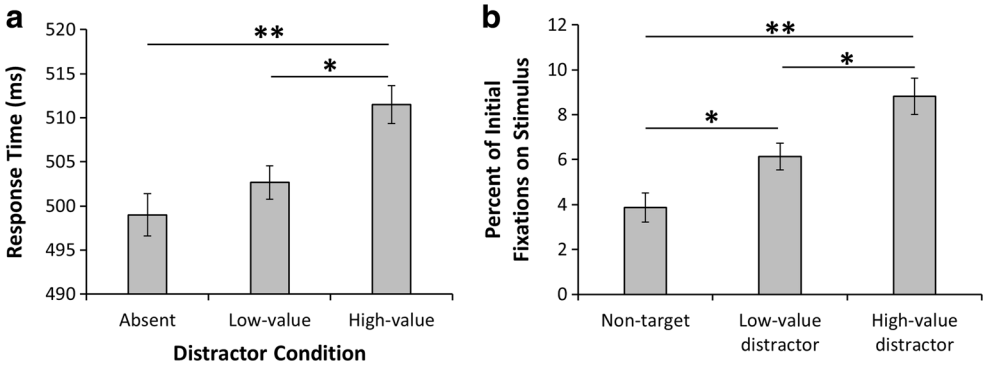

Fig. 2. Response times in fixating the target (**a**) and proportions of errant saccades (**b**) by distraction condition in the test phase. Error bars reflect within-subjects confidence intervals. $^{*}p < .05$, $^{**}p < .01$

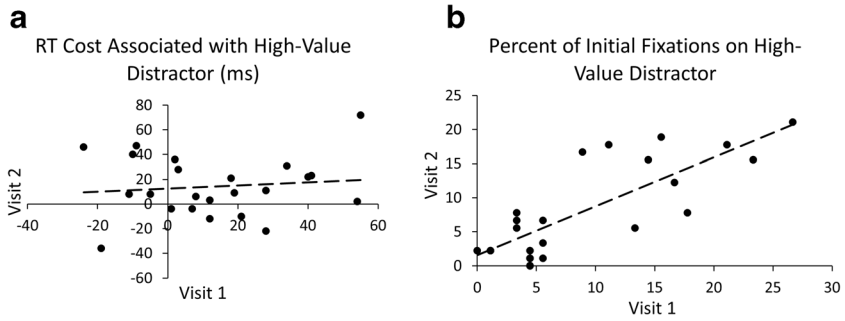

Fig. 3. Correlations between the measures of value-driven attentional capture obtained at Visit 1 and four to five weeks later at Visit 2. (**a**) Correlations between the RT costs associated with the high-value distractor (relative to distractor-absent trials), observed at Visit 1 and Visit 2. (**b**) Correlations between the percentages of trials on which the high-value distractor was the initially fixated stimulus, observed at Visit 1 and Visit 2

fixations to a previously reward-associated stimulus could offer a window on meaningful changes in susceptibility to value-driven attentional capture over time, a construct that has been linked to a variety of psychopathologies (Anderson et al., 2013; Anderson, Chiu, et al., 2017; Anderson, Kronemer, et al., 2016; Anderson, Leal, et al., 2014; Sali et al., 2018).

Our findings also caution against the use of RT measures of attentional capture in tracking changes in individual performance over time. An RT measure, which is commonly used to quantify performance at the group level in both the clinical (Field & Cox, 2008; Field et al., 2014) and nonclinical (Anderson et al., 2013, 2016a, 2016b) attentional-bias literatures, produced weak test–retest reliability. This finding is consistent with criticisms that have been raised against commonly used attentional-bias measures (Ataya et al., 2012; Christiansen et al., 2015; Field et al., 2014) and further highlights the enhanced reliability offered by the spatial precision of eyetracking. Indeed, in a recent review Field et al. (2014) speculated that "These criticisms [including "poor internal reliability"] may not apply to more direct measurements of attentional bias, such as eye movement monitoring" (p. 227; see also Field & Christiansen, 2012). Our findings confirm this idea and validate a means by which the potential of attentional-bias measures as a clinical assessment tool can be fruitfully explored.

It is important to note that, although our findings support the reliability of an oculomotor measure of attentional capture, it is not advised that this measure be used to make strong conclusions at the level of an individual. As with any behavioral assessment, there will be variability in performance that is intrinsic to the measurement. Even with test–retest reliability as high as it was in the present study, attentional capture at Visit 1 still accounted for only 63.7% of the variance in attentional capture by the same reward cue during Visit 2. More fruitful would be to include this attentional measure within the context of a clinical trial or a longitudinal outcome-based study, in order to make claims about whether changes in symptomology correspond to changes in attentional performance at the group level.

In the present study we have explored the test–retest reliability of a measure of attentional capture driven by reward associations that are learned in the context of a laboratory task. Difficulty ignoring such reward cues has been linked to drug dependence (Anderson, 2016b; Anderson et al., 2013; Anderson, Kronemer, et al., 2016), and tracking possible changes in susceptibility to value-driven attentional capture over the course of an addiction would be an interesting direction for future research. The time course of the learning and motivational aspects of drug use (Berridge & Robinson, 1998; Robinson & Berridge, 1993) is of course different from that of the relatively brief reward training employed in the present study. As such, it is unclear whether similar test–retest reliability would be evident for eye movement measures of attentional biases toward actual drug cues, a question that should be explored in future clinical research. Another limitation of the present study concerns the sample size, which was not large and was subject to attrition. Although the test–retest reliability of our eye movement measure of value-driven attentional capture was robust, the precise strength of the correlation across visits may not be as strong as the strength we obtained with the present sample. An additional limitation that we note concerns the absence of a context manipulation. Attentional capture by reward cues has been shown to be context-dependent, occurring selectively within the contexts in which a particular cue has previously been rewarded (Anderson, 2015a, 2015b), and such contextual dependencies may more closely reflect the manner in which reward learning biases attention in everyday life (including drug addiction). The degree to which such contextual dependencies in the control of attention are similarly reliable across testing sessions remains to be explored.

In summary, the findings of the present study provide empirical support for a reliable means of assessing susceptibility to value-driven attentional capture at two different points in time. By measuring eye movements directed toward irrelevant reward cues, a stable measure of attentional bias can be obtained, which contrasts with the weak reliability of RT-based measures that has previously been noted (Ataya et al., 2012; Christiansen et al., 2015; Field et al., 2014).

Author note B.A.A. developed the study concept, and B.A.A. and H.K. designed and programmed the experimental task. H.K. coded the data, which B.A.A. subsequently analyzed. Both B.A.A. and H.K. contributed to writing the manuscript. The authors declare no conflicts of interest. Special thanks to Mark Britton and Ming-Ray Liao for assistance with

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