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A Meta-Analysis of Contingent-Capture Effects

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Abstract

The present meta-analyses investigated the widely used contingent-capture protocol. Contingent-capture theory postulates that only top-down matching stimuli capture attention. Evidence comes from the contingent-capture protocol, in which participants search for a predefined target stimulus preceded by a spatial cue. The cue is typically uninformative of the target's position but either presented at target position (valid condition) or away from the target (invalid condition). The common finding is that seemingly only top-down matching cues capture attention as shown by a selective cueing effect (faster responses in valid than invalid conditions) for cues with a feature similar to the searched-for target only, but not for cues without target-similar feature. The origin of this "contingent-capture effect" is, however, debated. One alternative explanation is that intertrial priming–the priming of attention capture by the cue in a given trial by attending to a feature-similar target in the preceding trial–mediates the contingent-capture effect. Alternatively, the rapid-disengagement account argues that all salient stimuli capture attention initially, but that the disengagement from non-matching cues is rapid. The present meta-analyses shed light on this debate by (a) identifying moderators of the size of reported contingent-capture effects (64 experiments) and (b) analyzing pure (blocked) versus mixed presentation of different targets as well as summarizing results of published intertrial priming studies (12 experiments) in the contingent-capture protocol. We found target-singleton versus non-singleton status and pure versus mixed presentation of different targets to be reliable moderators. Furthermore, results indicated the presence of publication bias. Otherwise, the contingent-capture theory was supported, but we discuss additional factors that must be taken into account for a full account of the results.

Keywords: contingent capture, intertrial priming, meta-analysis, publication bias

At each moment in time, humans select only a limited part of the visual input for in-depth processing. This is called *visual attention*, which is of importance for cognitive performance, such as driving, reading, or navigating. One of the fundamental questions in visual-attention research is which principles govern the capture of attention (Bundesen, 1990; Treisman & Gelade, 1980; Wolfe, 1994). At least three factors have been identified: top-down influences, bottom-up salience, and memory (Awh, Belopolsky, & Theeuwes, 2012).

Laying proof of the influence of top-down control on attention, during search for predefined visual targets, irrelevant distractors interfere with search efficiency to the degree that targets and distractors share features. This reflects the shifting of attention to distractors that match the participants' top-down feature templates used to search for targets (Duncan & Humphreys, 1989).

Regarding bottom-up influences, many studies demonstrated that attention is captured by salient stimuli that stand out among their surroundings by a local difference in features, such as color, luminance, or orientation (Itti, Koch, & Niebur, 1998; Nothdurft, 1993). A popular instance of bottom-up capture is the interference by a color-singleton distractor presented away from a searched-for shape target (Theeuwes, 1991, 1992). In this context, a color-singleton is a stimulus that has a color different from all other stimuli that are presented together with the color-singleton and which are (more) similar to each other in their colors. Typically, search for a shape-singleton target is delayed by the presence of such a color-singleton distractor (Theeuwes, 1991, 1992).

Some authors have argued that such capture could likewise be another instance of top-down contingent capture. These researchers observed that if the shape-defined target is not a singleton, such that participants have to search for the target shape to find the target, irrelevant color-singletons no longer interfered, leading researchers to argue the singleton status of the targets can lead participants to adopt top-down singleton search mode, by similarity to which's template color-singleton distractors would then capture attention (Bacon & Egeth, 1994). However, there are definitely cases in which top-down singleton search cannot account for capture by irrelevant singletons and that, thus, support the conclusion that bottom-up capture of attention is a reality (e.g., Schoeberl, Fuchs, Theeuwes, & Ansorge, 2015; Weichselbaum & Ansorge, in press).

Finally, reflective of an influence of memory is intertrial priming: If participants search for a singleton target of uncertain color, repeating the color of the target from one trial to the next facilitates searching for the target in the second of these trials, even if participants would have expected a change of target color (Maljkovic & Nakayama, 1994).

All of these influences have been studied and discussed in a variant of the peripheral cueing procedure that we labelled the *contingent-capture protocol* (which is short for a protocol to measure the top-down contingent capture of attention). Accordingly, in the following, we speak of the *contingent-capture effect* when we refer to the central result in this protocol. We stick to these labels simply because most research with this protocol seemingly supported the top-down dependence of attention capture. However, we emphasize that this is a hypothesis, and up until today, the correct interpretation of the contingent-capture effect is contested. Because of the widespread usage of the contingent-capture protocol and its important bearings on theories of visual attention, in the present study, we conducted two meta-analyses to examine whether the pattern of results across studies supports the major interpretation of the contingent-capture effect and to characterize its critical side conditions. This will be explained next.

Top-Down Contingent Capture

In contingent-capture experiments, participants search for a predefined target, and peripheral cues, not predictive of the most likely target position, are presented, with a short interval[1] before the target. The contingent-capture effect is defined as a (stronger) cueing effect when the cue matches to respondents' top-down search settings, and as an absent (or significantly reduced) cueing effect for cues not matching to the top-down settings. Top-down matching–but not (or less so) non-matching–cues at target position (valid condition) capture attention and decrease search times–relative to top-down matching cues presented away from the target (invalid condition) and, thus, misguiding attention (Folk & Remington, 1998[1]; Folk, Remington, & Johnston, 1992; for reviews, see Burnham, 2007; Rauschenberger, 2003). Statistically, this is expressed as an interaction between top-down match of a cue and cue validity, or sometimes a three-way interaction between cue properties, target properties, and cue validity. For example, if participants search for a (red) color target, a contingent-capture effect consists of a cueing effect with color cues, but no cueing effect with (white) onset cues (Folk et al., 1992). According to the involuntary top-down contingent orienting hypothesis, respondents establish top-down control settings to search for the targets by some of the target’s defining features, and cues with features matching to these search settings capture attention.

Bottom-Up Capture in the Contingent-Capture Protocol

However, as the contingent-capture effect has been demonstrated with singleton cues, the contingent-capture hypothesis is contested. Some researchers have argued that attention could be initially captured in a bottom-up way by any salient cue and that top-down control kicks in only later, following bottom-up capture (Theeuwes, Atchley, & Kramer, 2000). To account for contingent-capture effects, Theeuwes et al. (2000) assumed that, in the interval between cue and target, attention would first be bottom-up captured by all salient stimuli, matching and non-matching singleton cues, but that soon after this, respondents would realize that the non-matching cues are task-irrelevant, such that attention could readily be disengaged from these non-matching cues. In contrast, it would take longer for respondents to discriminate between a matching cue and a target, with the consequence that attention, once captured by the salient matching cue, would be more difficult to disengage from this cue and would linger at cue position until the target arrives.

Related, but locating the impact of attentional disengagement in the target-display, is the attentional dwelling hypothesis of Gaspelin, Ruthruff, and Lien (2016). They argued that task-irrelevant (in their case: onset) cues might capture attention in a bottom-up way and attention then dwells at cue position. Only following the target display onset, participants would reject an item at the cued location if it is a distractor. This rejection would be faster with low than with high target-distractor similarity, so that (more) evidence of bottom-up capture would be found in difficult search tasks. Thus, the cueing effect could increase where it is more difficult to discriminate target and distractors. However, this search-difficulty manipulation should not modulate the contingent-capture effect, as a similar boost of the cueing effect of top-down matching cues in difficult relative to easy searches could simply add to this cue’s contingent capture.

Yet, more or less evidence for the contingent-capture effect could be due to other search (difficulty) manipulations, such as the number of stimuli in the display (i.e., the *set size*; Liao & Yeh, 2013), the target display type – that is, whether the feature-defined target is a singleton or a non-singleton (Lamy & Egeth, 2003), or the number of different target features that have to be searched for. Regarding the number of stimuli in the display, for example, with a larger number of homogenous non-singletons in the display, the salience of a singleton (cue) could increase. This could foster the bottom-up effect and decrease the contingent-capture effect (Liao & Yeh, 2013). Concerning the target display, if a target is defined by a feature (e.g., its color), but also is a singleton (e.g., the one red stimulus among several green nontargets), it is possible that respondents do not (always) search for targets by their defining feature, but rather (sometimes) search for targets by their status as singletons (Bacon & Egeth, 1994). In this situation, all singleton cues could be top-down matching, regardless of the particular features that carry their singleton status. Finally, regarding the number of searched-for target features, a top-down set would be more effortful to maintain and top-down search more difficult, if more features are to be searched for than if only a single feature (or few features) is (are) searched for (Folk & Anderson, 2010; Grubert & Eimer, 2013; cf. Duncan & Humphreys, 1989). For example, participants could fail more often in maintaining each of several top-down search settings than in maintaining a search setting for a single target feature. As a result, less evidence for contingent-capture effects might be observed where participants have to search for several features rather than only one feature.

Such unresolved debates based on divergent results of different studies are ideal material to address with a meta-analysis. We therefore conducted a meta-analysis that was designed to investigate whether varying cue-target stimulus-onset asynchronies (SOAs) or exact target-search conditions (e.g., if the target was or was not a singleton) across studies are correlated with the contingent-capture effect. For example, if the deallocation account holds true, the cue-target SOA used should be a significant moderator of the contingent-capture effect in our meta-regression. However, if the contingent-capture account is valid, there is no reason to expect a significant moderating influence of cue-target interval on the size or prevalence of the contingent-capture effect. Likewise, if, for example, participants tend to search for singletons once the target is a singleton, less evidence for contingent capture based on whether the singleton cues do or do not share target features could be observed. However, if participants stick to top-down search templates for target features even during search for target singletons, the status of the target as a singleton or a non-singleton might not moderate the contingent-capture effect.

Memory: Priming of capture

Researchers have also argued that the contingent-capture effect could be an expression of the third major principle driving attention, namely memory or selection history (Awh, Belopolsky, & Theeuwes, 2012). To understand this, note that the matching cue in a trial *n* is often (if not, as in studies with pure [blocked] target feature designs, always) more similar to the target of the previous trial *n-1* than the non-matching cue. This is important, as the memory of *n-1*'s attended-to target features speeds up directing attention to and searching for a stimulus with similar features in the directly following trial *n* (cf. Kristjánsson & Campana, 2010; Maljkovic & Nakayama, 1994; for a review see Lamy & Kristjánsson, 2013). Some researchers have attributed the entire contingent-capture effect to intertrial priming (Theeuwes, 2013). To test this possibility, past studies deliberately used two searched-for target features in mixed block designs and analyzed whether cueing effects of *n-1* primed matching cues were stronger than those of *n-1* unprimed matching cues (Ansorge & Horstmann, 2007; Folk & Remington, 2008). Again, results varied depending on the study in question. Some authors found evidence that intertrial priming accounted for the contingent-capture effect (e.g., Belopolsky, Schreij, & Theeuwes, 2010) while others confirmed that intertrial priming contributed to the contingent-capture effect but was unable to explain it entirely (Folk & Remington, 2008) and yet a third group of studies found little evidence of intertrial priming at all (e.g., Ansorge, Kiss, & Eimer, 2009). Awh et al. (2012) argued that pure or blocked single-feature target blocks could lead to a "cumulating intertrial priming effect" building up across trials (Krujne, Brascamp, Kristjánsson, & Meeter, 2015; Maljkovic & Nakayama, 1994) and, hence, selective cueing effects for target-similar cues.

Again, this is ideal material for the present meta-analysis that also assessed this hypothesis in a more representative and powerful manner: most straightforwardly by simultaneous comparison across and aggregation of all contingent-capture studies of the priming-of-attention hypothesis. If intertrial priming accounts for the contingent-capture effect, we expected to see a significant interaction between priming (i.e., whether or not a cue in trial *n* was of the same feature as the target in trial *n-1*) and the contingent-capture effect. However, if the contingent-capture explanation holds true, the contingent-capture effect should be found, regardless of whether or not the top-down matching cue has been primed by the target in trial *n-1*. All that matters for contingent capture to take effect is a match between the search templates and the cue. In this case, no significant interaction between priming and contingent-capture effect was expected.

In addition, to test the same question, for our meta-analyses, we coded whether a design used pure (blocked) single-feature targets or mixed targets of different features within a block. Since only a small number of studies has explicitly investigated the role of intertrial priming in the contingent-capture protocol and since priming could also accumulate across trials, the inclusion of pure versus mixed target blocks as a possible moderator in all studies investigating contingent-capture effects adds information regarding this question. To note, only in pure target blocks, each top-down matching cue would also be primed by a similar target in the preceding trial. In contrast, mixing different feature targets within a block typically implies that each top-down matching cue is only primed in a subset of all trials. If selection history is (at least partly) responsible for contingent-capture effects, we would thus expect our meta-regression model to identify pure versus mixed target blocks to be a moderator of contingent-capture effects. More specifically, in line with Awh et al.'s (2012) reasoning, we would expect stronger contingent-capture effects in pure target blocks and weaker contingent-capture effects in mixed target blocks.

Before we continue with a brief outline of the present research, we review one additional factor possibly responsible for the strength of the contingent-capture effect, namely, the particular cue feature. Originally, Folk et al. (1992) studied two different types of features, static features (color) versus dynamic features (abrupt onset). Since this pioneering study, typically the contingent-capture effect has been found to be numerically stronger during search for static (usually color) targets than during search for dynamic (usually onset) targets (Folk et al., 1992; Goller & Ansorge, 2015). In addition, abrupt onsets as irrelevant features could be more difficult to ignore than salient but statically defined singletons (Yantis & Jonides, 1984). Both of these findings imply that the contingent-capture effect could be of different size during search for static than during search for dynamic features, especially if the non-matching cue during search for static targets is an abrupt-onset cue. The present meta-analysis appropriately makes use of the maximum of the available data to study this influence, too.

Present research

The above review illustrates the need for a comprehensive and critical re-evaluation of the evidence regarding the contingent-capture effect. Some influences on attentional capture in contingent-capture experiments have been discussed (i.e., the number of stimuli per display; the target status as a singleton vs. a non-singleton; or the number of to-be-searched for target features). With the current meta-analyses, a new light is shed on these and other potentially decisive conditions (e.g., the duration of the target displays; see Kiss, Grubert, Petersen, & Eimer, 2012). If some of the given explanations are true, all of these potential influences could show up as significant moderating influences for the size of the contingent-capture effect in our meta-regression.

Essentially, in meta-analyses, experiments, not persons, are the subjects of investigation. The experiments are statistically weighted by their size (i.e., information content): Larger and, thus, more powerful experiments have more influence on the estimated overall effects. Because a meta-analysis aggregates data over a vast number of participants, the estimated effect should be relatively precise. Such data aggregation also leads to a high analytic power (Borenstein, Hedges, Higgins, & Rothstein, 2009).

In addition, the current meta-analyses will also allow for more fundamental conclusions about the research field scrutinized. Integrating different experiments into models of particular independent variables as moderators in a meta-regression model of the contingent-capture effect will enable to see which independent variables varied sufficiently often between studies to allow meaningful conclusions and which independent variables were rather kept stable across studies. In this respect, meta-analysis provides a perspective on the blind spots of this research field and provides clues as to what should be done in future research to more systematically address the research questions. Finally, the present meta-analyses also addressed the question of potential publication bias. Here, a skewed distribution of effect sizes as a function of the *SE* of the effect sizes would be diagnostic of a publication bias because studies with larger samples are more precise and, therefore, closer to a calculated common effect. In the present study, the possibility of a publication bias was tested with the help of funnel plot inspections (showing effects sizes as function of the *SE* of the effect sizes), as well as with various related statistical approaches (see below).

Methods

Literature search

A cited-reference search of the groundbreaking study of Folk et al. (1992) was conducted in the Web of Science and PsycINFO databases, in order to ensure the inclusion of the majority of representative journal articles. Additionally, a corresponding search in the Scopus database was conducted to include even more journals, and researchers (Martin Eimer, Charles Folk, Anna Grubert, Dominique Lamy, Roger Remington, and Jan Theeuwes) were approached by e-mail requests for information on any unpublished experiments. The cited-reference search yielded 1,390 hits in the Web of Science, 354 in PsycINFO, and 1,453 in Scopus (on August 18th, 2017).

Study inclusion criteria

In order to keep between-study variance as low as possible and to only include comparable studies, the following study inclusion criteria were applied:

1. Included studies had to report original data from healthy individuals. Reviews, commentaries, and experiments with clinical samples were excluded.
2. Studies had to use visual cues carrying spatial information about a possible target location in the form of their own positions. If a word was used as a cue, a study was classified as a semantic cueing study and, therefore, excluded from analysis. The reason was that semantic cueing most likely works differently than location cueing proper

(Jonides, 1981).

3. Per each trial of an experiment, the targets had to be presented in “one display” only. If a single trial contained several targets in succession, conclusions regarding attention capture by cues are complicated by the workings of temporal attention to different targets (Olivers & Meeter, 2008; Raymond, Shapiro, & Arnell, 1992). This criterion led to the exclusion of experiments using rapid serial visual presentation (RSVP; e.g., Folk, Leber, & Egeth, 2002; Irons & Remington, 2013).
4. For their uncertain relation to the capture of spatial attention (cf. nonspatial filtering; Folk & Remington, 1998), versions of the contingent-capture protocol in which the cue was presented at the same time as the target were not included (e.g., Ansorge & Horstmann, 2007).
5. Due to reasons discussed in more detail below, only effects resulting from within-subject designs (i.e., varying cue- and target-properties as a within-subject factor) were coded. This led to the exclusion of, for example, the first two experiments of Folk et al. (1992), as well as of some other studies (e.g., Liao & Yeh, 2013). If, however, different steps of other factors, such as target display type, were realized as a between-subjects factor, the different steps were coded as two separate experiments and the analyses of these steps were conducted separately for the two steps of the factor in question (i.e., these were treated as independent measurements; e.g., Carmel & Lamy, 2014, Experiment 2).
6. If a study investigated new facets of contingent capture and stood out because of the complex nature of the cueing displays, it was excluded due to a lack of comparable experimental protocols. This was the case for all available unpublished experiments, but also for some published experiments (e.g., a double cue experiment by Grubert, Righi, & Eimer, 2013).
7. Included studies had to report the relevant two-way interaction (i.e., Validity $\times$ Matching) or three-way interaction (i.e., Cue Properties $\times$ Target Properties $\times$ Validity). Missing data were not substituted by estimates to avoid using artificial data. However,

unspecified outcomes (e.g., $F < 1.00$) were coded as 0.

In this way, out of the initially screened papers citing the contingent-capture protocol of Folk et al. (1992), 64 experiments remained for the present meta-analysis of contingent-capture effects (see Table 1) and 12 for a further meta-analysis on intertrial priming in the context of the contingent-capture protocol (see Table 2).

--- insert Table 1 about here ---

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Study coding

The first author screened and coded studies. Another researcher independently coded a selection of 17 studies, in order to estimate interrater reliability. Assessed were publication year and sample sizes, as well as relevant data regarding the experimental design, including type of target display (i.e., whether the target was a singleton or a non-singleton), all durations (SOA, fixation, cue display presentation time, target display presentation time), properties of cue and target (dynamic vs. static discontinuities or features), set size of the target display, the size of the top-down search set (whether one or more features had to be remembered and searched-for simultaneously), and whether the target feature was mixed or blocked. If SOAs, the number of targets, or cue- or target-properties varied within subject, the mean of all possible SOAs, and so forth, of the respective experiment was imputed because otherwise these measurements would not be independent but rather results from the same respondents (e.g., Ansorge & Heumann, 2004; Liao & Yeh, 2013).

With respect to SOAs, most experiments used a single SOA of a length below 300 ms (see Table 1). However, as collapsing across SOAs was necessary in three studies, one of them including an SOA of more than 300 ms, we first scrutinized their results for potential inhibition of return (IOR) effects (Klein, 2000). IOR effects – inverted cueing effects (i.e., benefits in invalid compared to valid conditions) for the longest SOAs –, were of no interest in the current study. Yet, as can be seen in Table 3 only few SOA conditions showed small inverted cueing effects in non-matching conditions. As inverted cueing effects are sometimes also found with short SOAs, especially in non-matching conditions, the averages of these studies were used rather than the corresponding studies excluded.

--- insert Table 3 about here ---

Most of the coded variables are well motivated by what was explained above. Publication year was of interest for testing whether there is a decline effect in the published evidence (Schooler, 2011). The decline effect reflects that many initial reports show stronger findings that tend to diminish over replications and time, possibly due to selective reporting of only the significant replications following an initial (chance) finding. The variable *Author* was used for testing for effect-size differences between different groups of researchers.

One variable that could also have been of theoretical interest and that initially was coded is missing in the above list: the exact type of task. The reason is that there were only few exceptions from the most common task: a compound search task based on a feature search for finding the targets and requiring the discrimination of a second (independently varying) feature for the selection of the correct response. Exceptions from this most commonly found task included general search for color discontinuities rather than a specific color (Folk et al., 1992, Experiment 4); feature conjunction search rather than single-feature search (Adamo, Pun, & Ferber, 2010; Gaspelin et al., 2016); responses to the target's location rather than to a to-be-discriminated target feature (Ansorge & Heumann, 2004); or target detection rather than target discrimination (Adamo, Pun, et al., 2010). However, these exceptions were too rare to allow for meaningful inclusion as separate predictors in a meta-regression. This lack of variability of the task is an interesting insight in itself and shows that the realm in which contingent capture takes effect could be rather limited and that future studies would benefit from utilizing more varied tasks to learn more about the contingent-capture effect.

Since practically every analysis performed in the reviewed research followed an analysis of variance (ANOVA) model, effects were usually reported as *F* values. For the meta-analysis, these *F* values were transformed to Cohen's *dz*, an estimate for standardized mean differences for within-subject designs, via:

$$dz = \sqrt{\frac{F}{n}} \tag{1}$$

(for a more detailed description, see Lakens, 2013). Only outcome variables with *F* values with one degree of freedom in the enumerator were assessed (in order to compare two conditions). This further restriction led to the exclusion of more complex experimental designs, such as utilized by Irons, Folk, and Remington (2012).

It is important to note that, with the available data from the experiments, this way of deriving Cohen's *dz* is the only possible effect size that could be calculated for repeated-measurements designs. However, this approach is only a guesstimate of the true effect sizes. A precise calculation of Cohen's *d* for repeated-measurements designs would have to take the correlation of the dependent variable (reaction times; RTs) between baseline and experimental conditions into account (Lakens, 2013; Morris & DeShon, 2002). Since no study reported this correlation, Cohen's (1988) approach to transform standardized Cohen's *d*s (i.e., *dz*) into Cohen's *d*s comparable to the well-known Cohen's $d_=$ (i.e., the difference of means divided by the pooled *SD*s) was chosen:

$$d = dz \cdot \sqrt{2}$$ (2)[3]

To control for small sample sizes, Cohen's *d*s were converted into Hedges' *g*s via:

$$Hedge\,s'\,g = d \cdot \left(1 - \frac{3}{4(n_1 + n_2) - 9}\right) \tag{3}$$

In addition to complications regarding the exact estimation of true effect sizes, not all researchers did analyze and report contingent-capture effects in the same way. Contingent-capture effects could either be reported as a two-way interaction (i.e., as a Validity × Matching interaction; e.g., Ansorge & Heumann, 2004) or as a three-way interaction (i.e., as a Cue Properties × Target Properties × Validity interaction; e.g., Folk et al., 1992). To check for the influence of the way in which the contingent-capture effect was calculated, the number of factors used for the calculation was coded and the correlation between reported effect sizes and number of factors computed.

Data analysis

Due to many small sample sizes of studies included in the meta-analysis, the originally computed effect sizes were corrected for the small-sample bias of Cohen's *d* and Hedges *g* used for analysis. Since the majority of the coded experiments comprised some sort of variation (e.g., time intervals, different stimuli, other manipulations), the assumption of one true effect size was dismissed and the meta-analyses were conducted using random-effects models. To assess excess variability of effect sizes beyond expectation (i.e., mere sampling error), I^2 was computed. I^2 quantifies the proportion of the estimated true effect to the observed effects (Borenstein, Higgins, Hedges, & Rothstein, 2017). Borenstein et al. (2017) plead that I^2 is not to be interpreted as an absolute value but as the "proportion of the observed variance [that] would remain if we could eliminate the sampling error"(p. 7). The analyses were performed using RStudio 1.0.383 (RSudio Team, 2016) and the R package metafor (Viechtbauer, 2010).

Meta-regression

To allow a deeper understanding of possible moderator variables and their influence on the contingent-capture effect, meta-regression models were performed, considering all the coded variables (see above). Due to fewer experiments investigating intertrial priming effects in the context of the contingent-capture protocols, just a selection of moderators was included in the meta-regression model regarding intertrial priming effects (see below).

Subgroup analysis

Since research in the field of visual attention is divided along theoretical and methodological lines, a subgroup analysis was conducted with researcher group serving as the effect moderator. Researchers represented in the meta-analysis with at least four experiments were counted as a separate group. If researchers often published together, the group was named according to the scientist with the most publications included in these analyses. As a result, the following groups of researchers were coded: Adamo, Ansorge, Eimer, Folk, Lamy, Liao, Lien, others.

Publication bias

Since publication bias is a well-known and ubiquitous problem of evidence distortion in available empirical research (for the cognitive sciences, see Ioannidis, Munafò, Fusar-Poli, Nosek, & David, 2014), the following procedures were applied. Funnel plots were visually inspected. In a funnel plot, effect sizes are depicted in relation to the calculated common and weighted effect size from the random-effects model. While effect sizes are displayed on the *x* axis, the *y* axis shows the *SE* of effect sizes, which is a function of *N* and is smaller with larger sample sizes. Asymmetry in a funnel plot is indicative of publication bias. In order to quantify funnel plot asymmetry, Egger's linear regression method was applied to contingent-capture effects and to intertrial priming in contingent-capture protocols (Egger, Smith, Schneider, & Minder, 1997; Sterne & Egger, 2005). This method essentially is a linear-regression approach to quantify funnel plot asymmetry. If the resulting standardized coefficient of the regression model differs significantly from zero, this is taken as an indicator of funnel-plot asymmetry and, consequently, as an indicator for publication bias. In addition, the trim-and-fill analysis by Duval and Tweedie (2000) was conducted. In the trim-and-fill analysis, funnel-plot asymmetry is aimed to be removed by trimming away the most extreme results and filling in (simulated) studies with directionally opposite results into the funnel plot. If the trim-and-fill method does not result in any or just a small number of added simulated effect sizes, this indicates that the real data do not speak for a publication bias. Finally, Begg and Mazumdar's (1994) rank-correlation method was applied (see Kühberger, Fritz, & Scherndl, 2014). The rationale behind this nonparametric approach to assess funnel-plot asymmetry is that effect sizes and sample sizes should be unrelated. A correlation significantly different from zero either indicates publication bias or distortion of the overall effect by smaller and possibly underpowered studies.

Results

Interrater reliability for effect sizes was calculated based on 17 randomly selected studies coded by a second coder. The intraclass correlation amounted to .99 for coded effect sizes, indicating near-perfect intercoder agreement.

Random-effects models

Two random-effects models were calculated. The random-effects model of 64 experiments (*N* = 1,621; see Appendix) reporting contingent-capture effects yielded a highly significant overall effect of *g* = 1.78 (*p* < .001; 95% CI [1.53, 2.04]; Figure 1). A random-effects model with 12 reported effects of intertrial priming within the contingent-capture protocol (*N* = 225) resulted in an estimated overall effect of *g* = 0.24 (*p* = .058; 95% CI [-0.004, 0.475]; Figure 2). That is, the weighted and combined contingent-capture effect sizes differed significantly from zero. However, for intertrial priming within the contingent-capture protocol this was not the case.

Heterogeneity

Although the random-effects model of the average contingent-capture effect yielded a highly significant result, there was heterogeneity between the different experiments' contingent-capture effects (I^2 = 89.15%). Intertrial priming in the contingent-capture protocol showed moderate heterogeneity with, I^2 = 37.07%. I^2 values of 25%, 50%, and 75% commonly are interpreted as small, moderate, or considerable heterogeneity, respectively (Borenstein et al., 2017). In light of the heterogeneity, meta-regressions were performed to investigate the amount of variability possibly attributable to effect moderators.

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Meta-regression

Contingent-capture effects. The results of the meta-regression models are shown in Table 4. Sample size was not included as a moderator because it was used to estimate the study variance and further to weigh the studies accordingly. As described above, the contingent-capture effect was calculated in different ways, either as a two- or three-way interaction. Since the number of factors for calculating the contingent-capture effect correlated with the strength of the found effect (*r* = −.27, *p* = .03), it was included as a predictor in the subsequent meta-regression. Furthermore, to avoid multicollinearity (i.e., correlations among predictor variables), *size of top-down set* was dropped from the analysis due to its high correlations with the variables *target-display type* (*r* = .43, *p* < .001) and *pure versus mixed target blocks* (*r* = .48, *p* < .001). *Target-display type* and *pure versus* *mixed target blocks* correlated as well, however, considerably weaker (r = .26, p = .04).

The meta-regression with respect to contingent-capture effects identified a significant moderating influence of the target-display type (i.e., whether the target was a singleton or not) on the size of the contingent-capture effects (b = 0.975, p = .002). This effect was due to an increased contingent-capture effect for non-singleton as compared to singleton targets. Furthermore, whether experiments varied target identity within blocks or not (i.e., mixed or pure target blocks, respectively) was a significant moderator (b = – 0.825, p = .007). This effect was due to stronger contingent-capture effects in pure target block experiments. All other moderators failed to reach nominal significance (ps > .07; see Table 4).

Intertrial priming in the contingent-capture protocol. Since only 12 experiments[4] investigated intertrial priming within the contingent-capture protocol, the number of predictors was reduced. Year of publication was not included in this meta-regression. Also, because every experiment included used cue durations of 50 ms, this predictor was dropped as well. Whether the target was a singleton or a non-singleton was also dropped because whether or not respondents searched for a singleton or non-singleton strongly covaried with the size of the top-down set (r = .81, p = .001) and because top-down set could have conveyed more information than the dichotomous singleton/non-singleton classification. Finally, cue and target properties correlated perfectly and therefore target property was dropped as a predictor as well.

The results of the meta-regression suggested the size of the top-down set to be a significant moderator (b = −0.588, p = .049). Essentially, the study with the strongest evidence for intertrial priming as a significant influence on contingent-capture effects was the one of Belopolsky et al. (2010), which used a singleton-target display. This study might have invited singleton search for an additional reason: the fact that Belopolsky et al. calculated priming of capture effects only for what they called neutral trials, in which respondents were not informed in advance about the exact feature that characterized the upcoming target and hence no top-down set could be established. In this situation, it is uncertain if respondents searched for a feature in the first place. In contrast, presenting non-singleton targets, thereby, enforcing feature search, tended to yield less evidence of intertrial priming as an alternative origin of the contingent-capture effect. All other predictors failed to reach significance (*p*s > .059, see Table 4).

--- insert Table 4 about here ---

Sensitivity analyses

Sensitivity analyses were conducted for each of the studies reporting contingent capture and intertrial priming effects in the contingent-capture protocol. These analyses revealed that no exclusion of any experiment would lead to a significant change of contingent-capture effects or inter-trial priming of capture effects.

Subgroup analyses

Because this research field is divided along theoretical points of view held by different researchers, a subgroup analysis was conducted, comparing the results of different researcher groups. The subgroup analysis revealed a significant influence of research group ($p < .001$). Contingent-capture effects of Liao and of Adamo were the smallest (see Figure 3). However, this result should be viewed with caution. The heterogeneity of effect sizes is noticeable (see for instance, the work of Ansorge and of Lien in Figure 3), and even arbitrarily chosen groups could therefore differ significantly by chance (see Figure 4).

--- insert Figure 3 about here ---

--- insert Figure 4 about here ---

Publication bias

To investigate publication bias, funnel plots (Figure 5) were visually inspected for asymmetry. The funnel plots suggested strong asymmetry in contingent-capture effects and also a slight asymmetry in the funnel plot for intertrial priming effects in the contingent-capture protocol. Regression tests for funnel-plot asymmetry confirmed strong asymmetries in the funnel plot for contingent-capture ($p < .001$) and a borderline non-significant asymmetry in the funnel plot for intertrial priming ($p = .051$). Note, however, that the regression test may have lacked power due to the relatively small number of intertrial priming studies and this particular result could, thus, either be due to a real asymmetry or to chance (cf. Sterne et al., 2011). Contingent-capture effects were of special interest for this meta-analysis, hence, Duval and Tweedie`s (2000) trim-and-fill method was applied for these effects (see Figure 6). This showed 15 necessary simulated effect sizes and, thus, suggested the presence of publication bias. Based on the results of the trim-and-fill method, the new meta-analytical estimate of the size of the contingent-capture effect, still, was highly significant with $g = 1.44$ ($p < .001$).

--- insert Figure 5 about here ---

--- insert Figure 6 about here ---

Begg and Mazumdar (1994) proposed to calculate rank-correlation coefficients between effect sizes and sample sizes. If larger effects are not due to less precision in measurements, these correlations should fall around zero (for a similar logic, see Kühberger et al., 2014). Rank-correlation tests were significant for reported contingent-capture effects ($r_\tau = .31$, $p < .001$). The correlation between the number of respondents and effect sizes in intertrial priming also reached significance ($r_\tau = .81$, $p < .001$). These correlations indicated a trendtowards larger effects as sample sizes decreased, both for contingent-capture effects and intertrial priming within the contingent-capture protocol.

The majority of methods investigating the presence of publication bias led to the novel conclusion that, for some reason, the results of contingent-capture effects in the literature of the contingent-capture protocol appear to be biased. A similar trend exists for intertrial priming effects within the contingent-capture protocol, although we draw this conclusion with caution, since not all analyses support this claim. As noted above, these diverging results likely are due to a lack of statistical power because of the small number of studies investigating the influence of intertrial priming on the contingent-capture effect.

An additional rank-correlation between effect sizes of contingent-capture effects and publication year resulted in a perhaps counterintuitive, although not nominally significant correlation of $r_\tau = .17$ ($p = .06$), suggestive of reported effects tending to gain magnitude over time. A directionally opposite effect was found for the correlation between publication year and intertrial priming effects (r_τ = −.56, p = .037), resembling the commonly found decline effect in the strength of research findings (e.g., Schooler, 2011).

Discussion

The aim of the present study was to analyze results of existing contingent-capture experiments via meta-analysis, to discuss and to evaluate alternative accounts for these effects, and to investigate potentially moderating variables. We included an additional meta-analysis of intertrial priming effects within the contingent-capture protocol to test one potential explanation for more capture by matching than non-matching cues in terms of priming of only the matching cue's capture by its similarity to the most recently attended-to target.

Main results

Random-effects models resulted in a highly significant overall effect for contingent capture, whereas intertrial priming failed to reach significance. In the meta-regression models, it became clear that intertrial priming was stronger for smaller top-down sets. In fact, the effect was strongly influenced by a single study (Belopolsky et al., 2010). This study not only used singleton targets but might have encouraged singleton search as Belopolsky et al. (2010) measured the intertrial priming effect in neutral trials only, in which respondents were not informed in advance about the feature by which the next target could be searched for. In summary, the aggregated evidence does not support intertrial priming as the sole or major explanation of the contingent-capture effect.

Influences of target-singleton status and pure versus mixed target blocks. In addition, meta-regression models also indicated greater contingent-capture effects with non-singleton targets and pure target blocks. The non-singleton target displays enforced feature search. Together with no evidence for intertrial priming in the same non-singleton target displays, the results suggested a dissociation of the most favorable side conditions for contingent-capture effects on the one hand and for intertrial priming on the other hand. Now it might seem as if the significant moderator of "pure versus mixed target blocks" would be in line with an influence of priming (or selection history) on contingent-capture effects, as contingent-capture effects tended to be stronger in pure target blocks, compared to mixed target blocks, a result that could be due to priming effects cumulating across several trials (Krujne et al., 2015; Maljkovic & Martini, 2015). However, as said, trial-by-trial priming was not seen as an overall significant moderator although the first repetition usually creates substantial priming of capture (e.g., Krujne et al., 2015). Therefore, it is likely that the influence of pure versus mixed targets on the contingent-capture effects was due to another influence, such as increased attentional weighting of the relevant target feature (Wolfe, Butcher, Lee, & Hyle, 2003) or facilitated maintenance of attentional control sets for a single relevant target color compared to two or more target colors (Liu & Jigo, 2017; but see Grubert & Eimer, 2016; Irons, Folk, & Remington, 2012).

Influences of target singleton versus non-singleton status.

Singleton targets yielded less pronounced contingent-capture effects. This is best in line with Bacon and Egeth's (1994) perspective: When a target is defined as a singleton, participants can search for a singleton and a target-dissimilar (i.e., feature non-matching) cue would capture attention by a match to this template for singletons (see also, e.g., Carmel & Lamy, 2015; Lamy & Egeth, 2003; Lamy, Leber, & Egeth, 2004). In contrast, feature search and, hence, a contingent-capture effect based on features, would have only been enforced in target non-singleton conditions. This finding echoes results of single studies. In particular, Lamy and colleagues found relatively strong differences between stimulus-driven distractor[5] effects in visual search tasks enforcing feature search mode and tasks allowing singleton search. For example, in their fifth experiment, Lamy and Egeth (2003) found that an irrelevant color distractor only captured attention during feature search for a different feature, when participants had used a singleton-detection mode earlier in the experiment (for related results, see Leber & Egeth, 2006).

Lacking influence of stimulus-onset asynchrony. A further factor that was of high theoretical interest but had no significant influence on the contingent-capture effect in the present meta-analysis was SOA: It did not matter much how long the exact cue-target interval was. Thus, the current study did not yield positive evidence in favor of the disengagement hypothesis, which would have predicted that, with very short SOAs, more capture by non-matching cues should have obtained and, hence, the contingent-capture effect should have been weaker. The lacking influence of the SOA, however, is in line with conclusions from studies that varied the cue-target interval within the contingent-capture protocol and found the effect even with short cue lead times (Ansorge & Heumann, 2003; Chen & Mordkoff, 2007; Remington, Folk, & McLean, 2001). The lack of influence of the SOA on the contingent-capture effect is also in line with many event-related potential (ERP) studies that showed little evidence of bottom-up capture by non-matching cues during the cue-target SOA, although bottom-up capture should have been observed with the ERPs if attention would have been initially captured by salient and non-matching cues (e.g., Eimer & Kiss, 2008).

Lacking influences of relevant target features and of irrelevant cue features. Based on the existing literature, one might have also expected differences in the strength of the contingent-capture effect during search for dynamic (onset) versus for static (color) feature targets, for instance, more bottom-up capture and less contingent capture by irrelevant onset cues during color search (than by irrelevant color cues during search for a different color). A clue to understanding this lacking effect was provided by the moderating influence of target-singleton status on contingent-capture effects. Above, we have seen that contingent-capture effects tended to be smaller in singleton-target studies, possibly due to the fact that during search for singletons, both matching and non-matching cues would have captured attention to some extent. As most studies with irrelevant onset cues used color-singleton targets, this mitigating search-mode effect on contingent capture might have masked an additional moderating influence of the type of the irrelevant feature.

Selective reporting. Visual inspections of funnel plots, tests for funnel-plot asymmetry, trim-and-fill analysis, and correlational methods indicated the presence of publication bias in the literature concerning contingent-capture effects. Publication bias is a long-known problem in psychology in general (Rosenthal, 1979; Sterling, 1959), and research in cognitive psychology in particular has shown a variety of biases, too (for overview and discussion, see Ioannidis, Munafò et al., 2014). Publication bias is the phenomenon of selectively publishing significant results and, hence, biasing the available research literature. The current findings are compatible with the assumption that many unsuccessful attempts to replicate the contingent-capture effect went into the filedrawers and were never published. This is just one more reason to also encourage the publication of null results; here, regarding the contingent-capture effect.

Additional findings

Another interesting finding was that contingent-capture effects tended to increase with year of publication. This is the opposite of what would generally be expected: the typically observed decline effect, showing the strongest (and, assumingly, most inflated) effects in the earliest publications and weaker (and, perhaps, more realistic and accurate) effect sizes in subsequent replications. In the present study, the decline effect was only found for the priming of capture effect in the contingent-capture protocol. The fact that contingent-capture effects themselves increased with year of publication could have to do with the increasing popularity of the most powerful designs among researchers active in this field. For instance, at the beginning, Ansorge and Heumann (2003, 2004) used onset targets and onset cues that either matched or did not match to a top-down search setting for target color. In these studies, even the non-matching cue would have provided a partial match to the top-down control settings for onsets, so it seems conceivable that the same authors achieved stronger contingent-capture effects in later years, when they used static color singleton cues, like Folk and Remington (1998), to measure contingent-capture effects (Goller & Ansorge, 2015; Goller, Ditye, & Ansorge, 2016). Another reason for increasing contingent-capture effect sizes across time could be due to the increasing research interest in interactions with the contingent-capture effect. While the initial studies were demonstrations of the contingent-capture effect itself and, hence, two-way or three-way interactions, factors, such as cue-target interval (for tests of the deallocation account) and of target-to-cue similarity (for tests of the intertrial priming explanation) illustrate the need for increasingly more factors in designs. Possibly, attempts to study these higher-order interactions more often failed and remained unpublished if the effect sizes of the contingent-capture effect itself were not sufficiently large enough. The corresponding studies with their lower contingent-capture effect sizes would then have not been published because the original research intention was not borne out by the data. As journals often ask for novel findings, the failure to achieve an intended higher-order interaction might therefore be the common reason for the tendency of the published contingent-capture effect sizes to increase across time and for the funnel plot asymmetries.

Directions for future research

A shortcoming of the present meta-analysis is the lack of reported effect sizes in the primary studies that forced us to estimate true effect sizes. It is becoming increasingly popular to report effect sizes, and this development will hopefully lead to more concentration on the actual size of an effect and its precision, instead on the dichotomous classification of results as significant versus not. The need for more focus on actual effect sizes is obvious from the present meta-analysis. Some authors (e.g., Cumming, 2014) even argued that, instead of *p* values, CIs should be reported. The reasons are clear: Whether or not an effect is significant can be immediately seen by whether or not CI includes null, and, even more importantly, the precision of a measurement is made visible, too. Hence, this shortcoming of past contingent-capture studies amounts to a strong plea to the research community to also report effect sizes.

We also want to give a more specific recommendation. Published articles on contingent capture usually consist of several experiments. This fact provides researchers with a powerful instrument to both focus more on actual effect sizes and to directly tackle the problem of publication bias via the approach of continuously cumulating meta-analyses (CCMA; Braver, Thoemmes, & Rosenthal, 2014; Goh, Hall, & Rosenthal, 2016). In a CCMA, the researcher meta-analyzes the results of all experiments reported within the same paper. This leads to an increased power of the combined and weighted overall effect size, allows to estimate the influence of changed parameters between the single experiments, and, most importantly, since some studies might miss the significance level due to being underpowered, non-significant studies could be included, reported and, hence, be spared the fate of the file-drawer problem (Braver et al., 2014). In principle, CCMA works just like an ordinary meta-analysis. However, after each replication (i.e., within a paper or in a new work) a new, updated meta-analytic estimate is calculated, thereby continuously increasing precision.

Most critically, however, on a theoretical level, it would be important to study new forms of how top-down control is exerted in contingent-capture studies. For example, results, such as that of

Gaspelin et al. (2016) or of Ansorge, Priess, and Kerzel (2016), already imply that not only top-down contingent capture but also top-down contingent suppression of non-matching cues could contribute to contingent-capture effects. Future studies could, thus, for example, investigate if in contingent-capture experiments these forms of top-down suppression depend on certainty about the irrelevant feature as it was found to be the case with capture by singletons in a non-target dimension (e.g., by color singletons during search for shape singletons; Kerzel & Barras, 2016). Likewise, above we have discussed the possibility that cumulative priming might have boosted the contingent-capture effect. However, it is much less clear if priming of capture itself is truly top-down independent (Ansorge & Becker, 2012; Fecteau, 2007), and this question should also be studied in the contingent-capture protocol. Ultimately, new experiments should be conducted to get a more realistic picture of different forms of top-down control that all contribute to contingent-capture effects.

Also, search tasks (i.e., target detection, localization, compound search) should be varied more frequently. Roque, Wright, and Boot (2016) demonstrated that the most frequently used experimental protocols in attention research do not correlate highly. Roque et al. suggested that much of the differences between the various experimental paradigms may stem from differing task demands or strategies employed. A direct comparison of results and inferences from different experimental designs may therefore erroneously condense a variety of factors down to bottom-up versus top-down influences. Hence the creation of new or the combination of existing experimental designs may offer a deeper and more holistic understanding of the interaction between bottom-up and top-down processes in visual search.

Limitations

A limitation of the present meta-analysis is the loss of information due to necessary simplifications in the coding process. As discussed above, research inspired by the contingent-capture protocol of Folk et al. (1992) established a dichotomization between matching and non-matching features. We have also used this dichotomy, albeit its coarseness. Relatively sophisticated experimental manipulations, such as conducted in relational search studies, highlighted that contingent-capture effects should partly be reinterpreted, but the way in which the relational-search theory of Becker (2010) reinterpreted the meanings of matching and non-matching features were not reflected in our coding procedure and in this meta-analysis. In addition, besides all aforementioned factors, such as bottom-up capture plus suppression and intertrial priming, recent research suggested that selective object-file updating costs in non-matching valid conditions could mask bottom-up capture and, thus, contribute to the contingent-capture effect, too (Carmel & Lamy, 2014, 2015; Schoeberl, Ditye, & Ansorge, 2017). These contributions to the contingent-capture effect could not be made visible with the current procedure. Another limitation is the absence of unpublished results in this meta-analysis. Above, we hinted at the reasons: The unpublished experiments that we received used relatively novel and unorthodox protocols. However, in general, publication bias would be easier to demonstrate by comparing published and unpublished experiments.

In the present study, methodological reasons led to the exclusion of many other experiments as well. Here, to estimate a trend, we needed to compare two groups, resulting in one degree of freedom in the numerator. Consequently, more complex experimental designs, like the one by Harris, Becker, and Remington (2015), with their comparison of cueing effects of many cue properties at once, could not be included in this meta-analysis. Similarly, studies with deviations from the selection criteria described above could potentially have provided more interesting insights. Yet, they were excluded to ensure as much comparability as possible between the included studies.

The focus of this meta-analysis was on behavioral data, more specifically, on RTs. However, ERPs are becoming increasingly popular and might show a different picture than RTs (e.g., Grubert & Eimer, 2013, vs. Irons et al., 2012). Therefore, an aggregation of effect sizes found in ERP studies of the contingent-capture effect would be useful as well. This could be the scope of a future meta-analysis. The same holds true for eyetracking data. In addition, including error rates would have been interesting. Still, we had reasons for not including error rates in this meta-analysis: In many, if not most, cases, effects on error rates were not described as elaborated as RT effects. Hence, an analysis of error rates could have been even more selective, since probably more peculiar effects in error rates would have been reported.

As limited as some of the available data for this meta-analysis might be, as informative are the conclusions of the current meta-analysis, highlighting previously unnoticed methodological issues in this research literature. The proposed solutions will hopefully inspire future research and make research more comparable than it is right now.

Conclusion

In summary, the present meta-analysis confirmed that contingent-capture effects deserve their label: They cannot easily be explained by disengagement or by intertrial priming alone. However, the meta-analysis also nurtured the suspicion that singleton capture and number of to-be-searched for target features could undermine contingent-capture effects based on target features, depending on the exact side conditions (e.g., with singleton targets; by blocking of targets).

In addition, it appears that published reports of the contingent-capture effect could be selective in that up to now, low-effect size and null findings have more rarely been published, possibly due to the fact that lower contingent-capture effect sizes are not optimal side conditions for the increasingly more sophisticated experimental designs. This, however, does more concern the effect size of the contingent-capture effect and does not cast doubt on the existence of the contingent-capture effect.

Footnotes

Footnote 1. With longer cue-target intervals exceeding about 300 ms, cueing effects often revert (for an overview, see Klein, 2000). However, regarding the contingent-capture effect, this reversion is typically only found with top-down matching abrupt onset cues and with non-matching cues but not with top-down matching color cues (Gibson & Amelio, 2000).

Footnote 2. It should be noted here that the three-way interactions between cue color, target color, and validity (or cue location) yielded the same result in both Experiments 2 (N = 24) and 3 (N = 16): F(1, 22) = 34.33 of Folk and Remington (1998), an impossible result in the second case (see degrees of freedom). Although visual inspection of the figures does not imply an overly different result, the second F value appears to be a copy-and-paste error.

Footnote 3. An alternative would have been to find a typical correlation between RTs of the conditions and then use this correlation to correct the effect size for each experiment. This alternative was not chosen for two reasons: First, the imputed correlation would again have been an estimate and hence probably not correct for each experiment; and, second, order of effect sizes from small to large would have remained unaffected. Therefore, a meta-analysis based on imputed correlations would have yielded relatively similar results.

Footnote 4. For the interested reader, the interactions concerning the influence of intertrial priming on contingent-capture effects are easy to spot in the Results sections of all but one study, the study by Eimer and Kiss (2010). In Eimer and Kiss (2010), the interactions are reported in the Discussion sections on p. 956 (Experiment 1) and p. 960 (Experiment 2), respectively.

Footnote 5. Lamy and colleagues labelled the cues *distractors*, arguing that they do not indicate the correct target position and, hence, are a distraction.

Conflict of interest: The authors declare that they do not have any conflict of interest.

Ethical approval: This article does not contain any original studies with human participants performed by any of the authors.

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Table captions:

Table 1. Raw data of coded contingent capture experiments

Note: In the columns “Cue” and “Target” “stat.” denotes a static discontinuity and “dyn.” denotes a dynamic discontinuity. If, e.g., cues could either be dynamic *and* static, it was coded as 0.5.

[a]The actual number of participants in this experiment was 16. However, half of the respondents searched for orange and the other half searched for “yorange” targets. Only the effect reported for orange targets could be included in the analysis because of more than one degree of freedom in the enumerator.

[b]Whenever condtions are mentioned in the study names, they were between-subjects factors and their respective effect sizes were coded separately if the contingent-capture effect was reported separately per condition, due to stochastic independence.

Table 2. Raw data of coded intertrial priming experiments in the contingent-capture protocol

Table 3. Cueing Effects (Invalid Reaction Times Minus Valid Reaction Times) Separately for Different Stimulus Onset Asynchronies (SOAs) in Three Studies for Which an Average Cueing Effect Across SOAs was Imputed

Table 4. Results of the meta-regressions.

Note. LBCI (UBCI) = lower (upper) limit of 95% confidence interval. ** p < .01

Figure captions:

Figure 1. Forest plot of all coded contingent-capture effects plus their corresponding effect sizes and *CI*s (95%). The diamond at the bottom of the figure represents the estimated overall effect.

Figure 2. Forest plot of all coded intertrial priming of capture effects in contingent-capture protocols. The diamond at the bottom depicts the overall

Figure 3. Forest plot depicting the results of random-effects models for each coded research group. The diamonds under the separate groups depict the effect found by each group.

Figure 4. Funnel plot of contingent capture effects by the respective researchers.

Figure 5. Funnel plots depicting the coded contingent-capture effects (left) and intertrial priming of capture in the contingent-capture protocol. The effect sizes are plotted against their respective *SE*s (*y* axis). The white funnel represents the area within which all (or most) effects should lie if the between-study heterogeneity was just due to sampling error.

Figure 6. The result of the trim-and-fill analysis. The white circles represent the studies that should exist to erase funnelplot asymmetry, that is, if no publication bias would be expected.