# Implicit Processing During Inattentional Blindness: A Systematic Review and Meta-Analysis

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**Abstract**

The occurrence of implicit processing of visual stimuli during inattentional blindness is still a matter of debate. To assess the evidence available in this debate, we conducted a systematic review of articles that explored whether unexpected visual stimuli presented in inattentional blindness designs are implicitly processed despite not being reported. Additionally, we employed meta-analysis to combine 51 behavioral experiments and investigate the statistical support for such implicit processing across experiments. Results showed that visual stimuli can be processed when unattended and unnoticed. Additionally, we reviewed the tasks used to assess participants' awareness of the unexpected stimuli and used meta-analysis to search for indications of awareness of the unexpected stimuli across experiments. The results showed no evidence that participants were aware of the unexpected stimuli. Furthermore, we observed that a variety of procedures were employed to assess participants' awareness of the stimuli, adopting different criteria which are not always fully described. We discuss the implications for the study of implicit processing and the role of attention in visual cognition.

*Keywords*: attention; implicit processing; inattentional blindness; inattention; unconscious processing; systematic review; meta-analysis.

## 1. Introduction

The relationship between consciousness and attention has been examined by several empirical studies in the last decades, in addition to a body of theoretical work on this matter (Dehaene et al., 2006; Lamme, 2003; Mole, 2008; Pitts et al., 2018; Van Boxtel et al., 2010). Some authors propose that attention and consciousness are completely dissociable (Van Boxtel et al., 2010), with consciousness depending mainly on stimulus features, such as duration or contrast, rather than on attentional factors. Other views assume that the two processes are closely related, often considering attention as necessary for consciousness (see Lamme, 2003, for a discussion of several possible relationships).

Dehaene et al. (2006) proposed a taxonomy for conscious and unconscious states in which both attention and stimulus strength influence whether a stimulus becomes conscious but exert distinct effects. The authors propose that an attended but weak (e.g., masked) stimulus elicits strong feedforward activity, but no recurrent activity, and can be primed only to superficial levels of processing. In contrast, a strong but unattended stimulus elicits local recurrent activation, and priming at multiple levels, but low fronto-parietal activity. In this framework, unconscious stimuli may lead to different neural and behavioral consequences, depending on the method adopted to render them unconscious.

A common approach to investigate the correspondences between attention and consciousness consists in manipulating participants' level of attention. Experimenters attempt to create situations in which stimuli that are consciously perceived in ordinary circumstances are kept unconscious while being unattended by participants. Several paradigms have been developed following this general rationale, such as attentional blink, change blindness, and inattentional blindness.

In the attentional blink, two targets are presented rapidly within streams of visual stimuli, and detection of the second target is hindered by the task of identifying the first target when the stimulus onset asynchrony (SOA) between both targets is between 200 and 500ms (Marois et al., 2004). Using this design, Sergent et al. (2005) investigated the stages of processing in which attention might modulate the perception of undetected stimuli utilizing event-related potentials (ERPs). Whereas the early P1 and N1 waves were not influenced by the occurrence of attentional blink, later components were affected. The observed differences in ERPs suggest that the causes of the attentional blink lie in later stages of processing, such as working memory updating (Luck & Kappenman, 2012).

A recent study (Fahrenfort et al., 2017) contrasted the effects of attention and stimulus strength on visual perception, using multivariate classification analysis of electroencephalogram (EEG) data. The activity evoked by the presentation of Kanisza figures made unconscious by either masking or attentional blink was decoded. They found that EEG could be used to decode both perceptual integration and feature contrast for stimuli during attentional blink; conversely, decoder accuracy for perceptual integration was impaired for masked stimuli, indicating that perceptual integration was disrupted in the latter case (corroborating earlier results, e.g., Fahrenfort et al., 2007). In both studies, the evidence may be interpreted as indicating that perception is largely unaffected by the presence or absence of attention − as operationalized by the attentional blink paradigm.

The phenomenon of change blindness has characteristics in common with the attentional blink, including high stimulus strength with limited and no top-down attention (Rensink, 2000). However, it specifically involves the impaired detection of sudden perceptual changes masked by visual disruptions, such as blank screens, eye blinks, or saccades (Simons & Levin, 1997). An EEG study (Busch et al., 2010) argued that change blindness does not result solely from failures of attention, but also from mechanisms for storing and comparing changes between scenes, which may correspond to working memory.

Finally, the phenomenon of inattentional blindness is characterized by a lack of conscious awareness of an unexpected stimulus (US; also referred to as critical stimulus), which is presented while an observer performs some attentionally demanding task (Mack & Rock, 1998). The US is usually visual (event though there are extensions of inattentional blindness employing stimuli in other modalities, e.g., Raveh & Lavie, 2015), and can be either simple stimuli, like a grating, or a change in stimulus configuration, like Gestalt grouping by similarity (e.g., Moore & Egeth, 1997). In the original version of the inattentional blindness paradigm (Mack & Rock, 1992), participants were explicitly asked if they had noticed the occurrence of the US. When participants could not report its presence correctly, processing of the US was assumed not to have occurred (Mack & Rock, 1992).

A typical inattentional blindness experiment is divided in three phases (Mack & Rock, 1998). The first phase is called inattention phase, since at this point participants have not been informed about the presence of the US. After some trials, participants are alerted about the US, e.g., by being asked about its presence. The following phase is therefore called divided-attention phase, because participants are now aware of the US and are expected to split their attention between the US and the main task. Finally, after the divided-attention phase, participants are requested to ignore the main task and respond to the US. This last phase is called full-attention phase (Mack & Rock, 1998).

Inattentional blindness is often discussed in the literature about the relationship between attention and consciousness (e.g., Kentridge, 2011; Lamme, 2003; Mole, 2008; Pitts et al., 2018), often to argue that attention is necessary for awareness (Prinz, 2011). In part, this is due to the large literature on the phenomenon, including a whole book written by the original proponents of the paradigm (Mack & Rock, 1998). More naturalistic versions of the paradigm have also been developed, such as the "gorillas in our midst" study (Simons & Chabris, 1999), leading to the popularization of the phenomenon of inattentional blindness (Chabris & Simons, 2010). Subsequent studies showed that inattentional blindness has high ecological validity and has important real-world implications (Chabris et al., 2011; Murphy & Greene, 2017; Simons, 2000).

Characteristics of the inattentional blindness distinguish it from the two other paradigms presented above and put it in a unique position to study the relationship between attention and awareness. In contrast to the attentional blink and change blindness, inattentional blindness does not rely on brief exposure times or flickers. Instead, a crucial factor for whether an observer is aware or unaware of a stimulus is expectation or previous knowledge of the stimulus (Braun, 2001; Mack & Rock, 1998; Ward & Scholl, 2015; White & Davies, 2008). This is shown by the observation that, in the overwhelming majority of inattentional blindness experiments**,** most participants become aware of the US after being questioned about it. Thus, in inattentional blindness, the crucial variable that determines whether a stimulus is noticed or not is top-down or voluntary attention. This allows for the study of the effects of attention on stimulus processing dissociated from stimulus-related variables.

Those differences are crucial because attention and stimulus factors are proposed to have distinct roles in the processing of noticed and unnoticed stimuli (Dehaene et al., 2014). Indeed, the differences in design of those paradigms imply distinct underlying mechanisms. Change blindness, for instance, involves perception of transitions between quantities (second-order information; Rensink, 2000). This requires manipulating information in and out of visual short-term memory (visual working memory) to compare inputs and detect changes. In contrast, inattentional blindness involves the perception of the presence of quantities (first-order information; Rensink, 2000) and does not require VSTM. Likewise, working memory has been shown to be involved in the attentional blink (Akyürek et al., 2007; Glennon et al., 2016). These differences suggest that distinct mechanisms are at work in inattentional blindness and those other paradigms (Rensink, 2010).

Inattentional blindness studies assume that attention is necessary for awareness; thus, if a participant's attention is directed away from a US to an attentionally demanding task and the US cannot be reported, it is assumed to be unattended. However, absence of report of a stimulus does not imply that no processing of the stimulus occurred (Wood & Simons, 2019). Decades of investigation with priming (Berkovitch & Dehaene, 2019; Dehaene et al., 2001, 1998; Naccache & Dehaene, 2001), as well as experimental techniques such as continuous flash suppression (Chung & Khuu, 2014; Lin & Yeh, 2016; Mudrik et al., 2011) and masking (Giattino et al., 2018; Peremen & Lamy, 2014), have suggested that unreported stimuli nonetheless influence cognition. This influence may reach considerably high-levels, although some of the evidence for that claim has been disputed (e.g., Moors et al., 2016; Moors et al., 2019). Studies on this issue seek to employ measurements that can detect processing of stimuli even when they cannot be reported. In this paper, we use “unconscious” or “implicit” to refer to stimuli that are not reported by participants in an experimental task.

Although the original studies on inattentional blindness suggested that unreported US are not processed (e.g., Mack et al., 1992), most recent research has arrived at the opposite conclusion (e.g., Mack & Rock, 1998; Moore & Egeth, 1997; Pitts et al., 2012). To contribute to a synthesis of the results in this area, in the present study we review the empirical studies that investigate unconscious processing during inattentional blindness. We present a qualitative review of experimental designs and results from a meta-analytic review of effect sizes reflecting implicit processing.

An additional goal of this study concerns the investigation of the related issue of the awareness tests employed to determine if stimuli are implicit. Vadillo et al. (2016) conducted a meta-analysis of 73 studies assessing the power of awareness tests in studies investigating implicit processing in contextual cueing paradigms. To conclude that a stimulus is processed implicitly, an assessment of awareness must exhibit null results in combination with significant results on a behavioral performance test. However, Vadillo et al. (2016) showed that, in many cases, such awareness assessments are actually underpowered to detect small effects of awareness. Additionally, they observed that a large number of studies showed a significant effect in awareness tests, summing up to a proportion of 21.5%. That is above the expected rate of 5% of false positives, assuming that the null hypothesis was false. Lastly, the meta-analytic effect size observed for the probability of awareness was significantly different from chance. Overall, although contextual cueing effects are robust (Chun & Jiang, 1998; Vadillo et al., 2016), the results of their meta-analysis do not support the conclusion that contextual cueing is implicit. Considering those results, it is reasonable to inquire if a similar issue occurs in studies on implicit processing during inattentional blindness. Hence, we also investigated whether the results of the awareness assessments in those studies support the conclusion that processing of the US is implicit.

## 2. Methods

### 2.1. Literature search

We followed the PRISMA guidelines for the selection of papers (Moher et al., 2009). An automated search was conducted in the databases Web of Science, Scopus, PubMED, and PsycINFO, using the search term "inattentional blindness" in combination with the terms "implicit*", "aware*" or "conscious*". This search returned 616 papers. We complemented this automated search with a manual search of reference lists of review and empirical papers, as well as with a survey of the grey literature, which returned 10 additional papers and one additional book chapter. The resulting number of retrieved papers was 628, of which 281 were excluded due to repetition. Our search was performed on 10 July 2020, with no start date.

In the next step, we screened the abstracts of the retained papers. Theoretical papers, reviews and studies on computational modeling were excluded, as they did not report any new behavioral or physiological results. Articles not written in English were also excluded. Conference presentations, theses, dissertations, and book chapters were selected for screening. This screening by abstracts excluded 242 papers not fitting the review's aims. As an exception, we included one meta-analysis (Kreitz et al., 2020) suggested by one of the reviewers. This meta-analysis provides new data from the authors' research group that had not been reported in the original studies. Since data for the individual papers is reported in Kreitz et al.'s (2020) meta-analysis, we included all of those which fitted the aims of the current review. This led to the exclusion of two papers from the 14 reported in the meta-analysis, because both were cross-modal experiments, whereas the current review is restricted to the visual domain.

The remaining 105 papers were fully read and evaluated with respect to fitting the review's aims. This led to the exclusion of 78 papers and 27 papers selected for the qualitative review. As many papers comprised more than one experiment, these 27 papers reflected together a total of 87 experiments. From the 27 papers selected for the qualitative review, 10 papers were not included in the quantitative review: 9 due to the lack of behavioral data and 1 for not providing enough information to compute effect sizes. The final sample for the quantitative review thus consisted of 17 papers. One experiment was removed from one of the selected papers due to the absence of enough data to compute a summary effect size, leading to a total of 51 experiments in the quantitative analysis (we refer interchangeably to the experiments as "studies" in the remainder of the text). In particular, the paper by Most et al. (2005) comprised 7 experiments, which were pooled together by the authors in an eight study dedicated to investigate implicit processing across all experiments. Figure 1 depicts the entire process of paper selection.

**Figure 1.**

*Study selection flowchart*

[Insert Figure 1 here]

We employed a number of criteria to exclude experiments employing experimental paradigms that cannot be strictly characterized as inattentional blindness. First, attention misdirection experiments (e.g., Kuhn & Findlay, 2010) were excluded because, despite conceptual similarities, this paradigm differs in important ways from inattentional blindness, and the two have been argued to configure distinct phenomena (Memmert, 2010; Moran & Brady, 2010). However, the precise mechanisms behind the two phenomena are still largely undetermined, and their definition has been a matter of debate (e.g., Kuhn & Tatler, 2011). However, several differences have been pointed out between them. One crucial point is that, in inattentional blindness, participants are not informed about the critical stimulus and have no expectation of it, while misdirection experiments use concomitant stimulation to direct attention away from the critical stimulus, and participants are informed about the concomitant stimuli. Thus, considering the lack of consensus regarding the relationship between both paradigms, we believe that their differences suffice to warrant separate investigation of each phenomenon.

Experiments employing dual-task paradigms were also excluded. In these experiments, participants are informed about two sets of stimuli/tasks but are instructed to ignore one of them; hence, critical stimuli are not unexpected, just ignored. This leads to a greater likelihood that attention "leaks" to the distractor than in inattentional blindness, in which not informing participants in advance about the critical stimulus contributes to reduce the chance of such "leaking" (Scholte et al., 2006). Expectation of the critical stimulus may significantly modulate implicit processing, and lead to different implicit effects between the two designs. Indeed, dual-task designs are more similar to the divided-attention phase of inattentional blindness experiments, in which participants already know about the unexpected stimulus but are not required to attend to it. Other experiments were also excluded when instructions mentioned the critical stimulus, even if they required participants to ignore it. We decided to exclude those experiments even though some still referred to such designs as inattentional blindness. Such choice was made based on the following observations: 1) inattentional blindness was explicitly created to eliminate voluntary attention from a perceptual situation; and 2) expectation is crucial feature of the inattentional blindness phenomenon, as shown by the generally large increase in noticing rates after the first awareness assessment. In contrast, when subjects are informed about the irrelevant stimulus, they may develop expectations about it, which may change the attentional dynamics of the critical stimulus. For example, it has been suggested that to-be-ignored stimuli are not excluded by attention; instead, they are initially attended and then suppressed (Cunningham & Egeth, 2016; Fukuda & Vogel, 2011). These criteria were applied independently by the authors of the present study, with disagreements adjudicated by all authors. A Cohen's kappa of .77 was obtained, indicating a good agreement between raters (Cohen, 1960).

**2.2. Coding**

All studies were coded by both the first and second author, and each author's coding was checked by the other. The articles were coded according to the following variables:

1. Main task: Type of behavioral task performed on which participants focused their attention during presentation of the US.

   Code: e.g. "letter tracking", "shape discrimination".

2. US relevance (implicit measure): Whether the US interacted with the stimuli in the main task, either by constituting part of the main stimuli or by forming a

configuration with it. The US could be classified as relevant even if they were not mentioned in the instructions.

Code: "irrelevant" or "relevant".

3. Modality (implicit measure): Modality of measure used to assess the implicit processing of the US.

   Code: "behavioral", "eye movements" or "EEG".

4. Static/dynamic: whether the US is static or moves across the screen.

   Code: “static”, “dynamic”.

5. Configuration: whether the US consisted in a change in the configuration of already present stimuli.

   Code: “yes” or “no”.

6. How unaware participants were selected for assessment of implicit processing.

   Code: “post-hoc data selection” or “group assessment of awareness”.

7. Implicit measure: Measure used as dependent variable to quantify implicit processing.

   Code: "RT", "accuracy", "eye fixations" or "ERPs". When a study reported both reaction times (RTs) and accuracy, these were coded separately for the meta-analysis.

8. Online/offline: Whether implicit processing was assessed during the inattentional blindness task (when the US was present) or retroactively, after the US disappeared.

   Code: "online" or "offline".

9. N trials (implicit measurement): Number of trials used to assess implicit processing.

   Code: continuous number.

10. N participants (implicit measurement): Number of participants in the implicit processing measurement.

    Code: continuous number.

11. Awareness task: Type of task used as criterion to categorize participants' awareness of the US.

    Code: "AFC discrimination", "yes-or-no detection", "cued recall", "free recall" or "confidence rating".

12. Objective/subjective (awareness measure): Whether participants' awareness was assessed using an objective measure of awareness (e.g. AFC) or subjective reports provided by participants about their own perceptual experience (e.g. confidence rating).

    Code: "objective", "subjective", or, when both were employed, "objective, subjective".

13. Delay (awareness measure): Whether the delay between the task and the assessment of awareness of the US was fixed or variable (i.e. according to a variable/random criterion or to subjects performance).

    Code: "fixed" or "variable".

14. N trials (awareness measure): Number of trials used to assess awareness.

    Code: continuous.

15. N participants (awareness measure): Number of participants in the awareness measurement.

    Code: continuous.

**2.3. Data analysis**

We estimated the meta-analytic effect size of implicit responses by comparisons of performance in trials with and without US presentation. We used correlations as measures of effect size, which has advantages over effect sizes based on standardized mean differences, such as Cohen's *d*, when studies with distinct degrees of freedom are combined (Rosenthal & DiMatteo, 2001). We computed *r* for each contrast from the means and standard deviations for each condition, when provided. In other cases, we used either *t*-values, *F*-values, or $\chi 2$ values to compute *r*. In a few cases (four contrasts), we converted *r* from the given value of Cohen's *d*.

For results on implicit processing, effect sizes were coded as positive when the observed result was in the direction predicted if the US was implicitly processed, and negative otherwise. Several of the studies reported comparisons for both RT and accuracy. We computed effect sizes separately for each of those contrasts when the corresponding data were available. However, some papers reported enough information to compute effect sizes for only one of those measures. In those cases, we computed the effect sizes for the measure with available data. This resulted in a total of 65 contrasts for implicit processing.

For awareness effect sizes, we computed *r* from the proportion or percentage of participants who reported the US in each experiment. To standardize the sign of effect sizes, we compared the effect size to the number of participants that would be expected to provide correct responses if all participants responded randomly. For example, if the experiment uses a 4AFC to assess participants' awareness, a 25% rate of correct responses would be expected. Importantly, this is only relevant for experiments in which implicit processing is assessed for the whole sample, including both aware and unaware participants (group assessment of awareness). Therefore, we do not include in this analysis experiments that tested for implicit processing in a subgroup of participants considered unaware according to individual awareness results ("post-hoc data selection"). We coded effects in the direction of "awareness" (i.e., more participants than what would be expected by chance noticed the US) as positive, and effects in the direction of "lack of awareness" (i.e., fewer participants than what would be expected by chance noticed the US) as negative. Since awareness tests contributed only one result by experiment for both RT and accuracy contrasts, the meta-analytic model for awareness included 14 fewer contrasts than the model for implicit effects. Hence, 51 contrasts were employed in this analysis.

Most studies included multiple measures of awareness, which often differed in sensitivity. In the majority of cases, participants first had to report if they noticed any additional stimuli or pattern in the critical trial (yes-or-no detection), and then were asked to identify it (e.g. forced-choice, free or cued recall). For most studies, rates of awareness were generally higher when only the first question was considered than when both were considered. We considered the use of only the first question as a "lax" criterion for assessing awareness, and the use of the two questions as a "strict" criteria, roughly corresponding to the definitions of Wood and Simons (2019). The results of the meta-analytic model for awareness effect sizes may change depending on which criterion is employed. To evaluate possible differences in rates of awareness due to the use of lax or strict measures, we built separate meta-analytic models using effect sizes for each criterion, when these were reported separately.

After computing all individual effect sizes, an analogous procedure was conducted for both implicit and awareness analyses. We built the model using Fisher's *z*-transformed correlations using the following formula (Borenstein et al., 2009):

$$z = 0.5 \times \ln\left(\frac{1+r}{1-r}\right) \quad (1)$$

Where *z* is the Fisher's *z*-transformed value, ln is the natural logarithm, and *r* is the correlation value for the contrast.

We employed a random-effects model (Hunter & Schmidt, 2004), given that effect sizes were expected to vary in the population (Field & Gillett, 2010). Since several studies resulted in more than one effect size, we used a three-level model where the additional level refers to effect sizes for outcomes within the same study (Borenstein et al., 2009; Harrer et al., 2019). The formula for the model is given in equation 2:

$$\hat{\theta}_k = \beta_0 + \nu_k + \zeta_{ik} + \varepsilon_{ik} \quad (2)$$

Where $\hat{\theta}_k$ is the θ observed effect size for study k; $\beta_0$ is the overall mean effect size; $\nu_k$ is the random deviation of the mean effect size of study k from the overall mean effect size; $\zeta_{ik}$ is the random deviation of the effect size for outcome i in study k from the mean effect in study k; and $\varepsilon_{ik}$ is the random error due to sampling.

Heterogeneity in the effect sizes was initially assessed using $\tau^2$, defined as the variance of the true effect sizes (Borenstein et al, 2009). Then, we examined the width of confidence intervals (CIs) in forest plots to search for studies with unusually large CIs and outlier studies, defined as those with 95% CIs falling outside the upper or lower bound of the 95% CI of the pooled effect size (Harrer et al., 2019).

A moderator analysis was conducted to explore the existence of subgroups among the studies. We analyzed subgroups clustered according to different criteria (e.g., type of response measures, number of trials). The specific variables used as criteria are described below. In the case of binary categorical variables, we re-ran the meta-analysis separately for each subgroup defined by the levels of the variable. In the case of continuous variables, we conducted a meta-regression including the variable as a predictor.

# 3. Results

## 3.1. Qualitative review

The studies included in the review, along with their main features, are displayed in Table 1.

[Insert Table 1 here]

### *3.1.1. Assessment of implicit processing*

#### 3.1.1.1. ***Behavioral measures***

Most studies evaluated implicit processing by means of behavioral effects of interference caused by the US in main task performance. A common approach was to investigate whether the unnoticed US captured attention implicitly. For example, Most et al. (2005) conducted seven experiments using a dynamic inattentional blindness task, in which participants counted the bounces of moving stimuli and, in the critical trial, a US crossed the screen. The authors pooled together the participants of these seven experiments in an eight study to investigate implicit processing across all experiments. Participants unaware of the US showed an overall decrease in performance in the critical trial compared to the preceding trial in which no US was presented, suggesting implicit shifts of attention to the US. In contrast, no decrement in performance was observed in a control experiment that did not include a US.

Adapted versions of the general procedure of Most et al. (2005) were used in other studies (Beanland & Pammer, 2010; Richards et al., 2012; Pammer & Blink, 2018), which in general present evidence of exogenous orientation of attention by unconscious stimuli. On the other hand, the results of Gabay et al. (2012), showing an implicit top-down orientation of attention by unreported arrows, suggest that attention can also be oriented endogenously by the US.

Among the reviewed studies, a class of processes commonly examined are Gestalt grouping and figure-ground segmentation. In an influential study, Moore and Egeth (1997) conducted two experiments to investigate if perceptual grouping occurs during inattentional blindness. The display was composed of two lines presented in the center among background dots configuring Ponzo (experiment 1) or Müller-Lyer (experiment 3) illusions in critical trials, while participants had to discriminate the length of the lines. Results showed that background configuration interfered with the task: identical lines appeared to have different lengths when the background was configured so as to elicit the illusions, even though the configurations of the dots were never consciously perceived.

Moore and Egeth's (1997) findings of perceptual grouping under inattention have been replicated by Lamy et al. (2006), using the Müller-Lyer illusion, and more recently by Wood and Simons (2019), using both the Müller-Lyer and Ponzo illusions. Lo and Yeh (2008) also used the Ponzo illusion to evaluate implicit texture segregation during inattentional blindness, and found converging evidence. Using a different illusion - the Roelofs effect - Lathrop et al. (2011) reported an effect of spatial mislocalization of targets induced by an unreported frame, in that participants mislocalized target positions as if they had processed the surrounding frame.

Other studies employed similar designs to investigate other Gestalt processes. For example, Moore et al. (2003) used modal and amodal completion as a measure of implicit processing. In their design, a visual display showed either a solid line or a dashed line alongside background pacmen stimuli in the background. These pacmen stimuli could be aligned as to induce the formation of an illusory rectangle through surface completion; this illusory rectangle which was the US. Even though participants were inattentionally blind to the modally-completed rectangle, responses were slower to dashed lines when this rectangle occluded the gaps in the line than when the pacmen stimuli were not aligned and thus did not configure a triangle. This suggests that surface completion can be triggered by unattended stimuli (i.e., the pacmen stimuli).

Similar stimuli were employed by Ariga et al. (2007) to investigate the same-object advantage (Egly et al., 1994), which refers to a reduction in RTs to invalidly-cued stimuli that belong to the same object compared to stimuli belonging to a distinct object. In this study, unattended pacmen stimuli unexpectedly configured illusory objects, which were used to elicit a same-object advantage. Although such an effect was found when participants were aware of the objects formed by the pacmen, the same was not observed for inattentionally blind participants.

Effects of contour integration were investigated by Cheng et al. (2019) using a modified inattentional blindness paradigm with varying levels of perceptual load. They reported a modulatory effect of perceptual load in contour integration dependent on consciousness. Under an unconscious state, only salient circles were successfully integrated under low perceptual load, while both salient circles and S-contours could be integrated consciously.

Perceptual grouping under conditions of inattention has also been examined by studies using the ‘inattention paradigm” (Kimchi & Razpurker-Apfeld, 2004; Russell & Driver, 2005; Razpurker-Apfeld & Pratt, 2008; Kimchi & Peterson, 2008; Rashal et al., 2017). This paradigm differs in some aspects from the typical inattentional blindness design, but follows the same fundamental structure. The inattention paradigm evaluates the effect of changing the configuration of background stimuli on the detection of small changes in a target matrix presented at the center of the screen, compared to a condition in which the background stays the same (see Section 4.1.). Using various configurations of background elements, these studies have generally observed that some types of grouping occurs implicitly under inattention. Furthermore, results from several of those studies suggest that attentional demands differ between perceptual organization processes.

Another type of process that has been investigated under inattentional blindness is response selection. Two studies used similar designs based on a stimulus-response compatibility effect, the Simon effect (Simon & Rudell, 1967), in that participants respond faster when a task-irrelevant stimulus (e.g. a square) appears on the same side as the correct response, compared to when it appears on the opposite side. In conditions in which the irrelevant stimulus was unattended, both studies found no effects of interference on performance, suggesting that response-selection processes do not occur under inattention (Moore et al., 2004; Lo & Yeh, 2008).

Other processes, such as semantic processing, have also been examined in the context of inattentional blindness. Schnuerch et al. (2016) used a number categorization task in which participants had to respond whether a target number presented in the periphery of the screen was smaller or greater than 5. The center of the screen was composed of a distractor array irrelevant to the main task. When the array contained numbers that matched the target, responses were faster than when it contained non-matching numbers. This basic effect was replicated by Pugnaghi et al. (2020) in a design modified to manipulate perceptual load.

In contrast to studies which evaluated implicit processing with online interference measures, Mack and Rock (1998) reported a series of studies using offline measures. These studies used essentially the same design as the original inattentional blindness experiments (Mack et al., 1992; Rock et al., 1992), but included an additional task administered after the critical trial to measure implicit processing; specifically, stem-completion tasks and recognition tests. They observed, similarly to earlier studies on auditory attention (e.g., Bentin et al., 1995), that unattended stimuli are semantically processed even when unnoticed. An study by Wood and Simons (2019) attempted to replicate this word-stem priming effect, but found no converging evidence.

Using the same paradigm as Schnuerch et al. (2016), Pugnaghi et al. (2019) explored possible effects of mere exposure to preconscious stimuli in the array (nonwords or Chinese symbols) in an offline assessment of preference. However, no mere exposure effects indicative processing of preconscious stimuli were observed. A recent meta-analysis investigated preconscious processing in inattentional blindness employing offline measures (Kreitz et al., 2020), in which the authors reanalyzed 16 datasets from studies originally designed to investigate different research questions. The meta-analysis analyzed participants' accuracy in multiple-choice questions about the US and found above-chance guessing accuracy when participants could not report the US, suggesting preconscious processing of the stimuli.

***3.1.1.2. Psychophysiological and eye-tracking measures***

Some studies have employed other types of measures to investigate implicit processing of the US in addition to the main behavioral task. These include eye-tracking, skin-conductance responses (SCRs), electroencephalography (EEG), functional magnetic resonance imaging (fMRI), and magnetoencephalography (MEG).

Eye-tracking studies usually measure implicit processing by examining if inattentionally blind participants fixate a US. Results from Beanland and Pammer (2010) show that both noticers and nonnoticers were equally likely to make saccades toward the US, suggesting that the US covertly captures attention and attracts eye movements independently of participants' awareness. Richards et al. (2012) found that participants that did not notice the US fixated the US less frequently and later in the task compared to noticers. Some inattentionally blind participants appeared to fixate the US but failed to report it, although they might have attended the US covertly. Using SCRs combined with eye-tracking, Wiemer et al. (2013) found converging results with both measures. An arousing US (a spider) elicited higher SCRs and was fixated more often than a neutral US (a flower), even though both stimuli were reported equally often.

Another group of studies used EEG to investigate implicit processing of US during inattentional blindness. Pitts et al. (2012) recorded ERPs in response to the US in multiple critical trials while participants performed a contrast detection task. In their design, the US was a square contour formed by random line segments presented on the background, which were irrelevant to the main task. They found a difference between random and square configurations in a visual ERP component related to texture segregation in inattentional blindness participants.

Shafto and Pitts (2015) employed the same design, with the modification that the line segments in the background sometimes configured a face, used as US. Contrary to Pitts et al. (2012), no significant differences in ERPs between faces and random configurations were observed in inattentionally blindness participants. Finally, in Schelonka et al. (2017), the unexpected stimuli in the background configured words, and ERP results indicated early, implicit processing of those words. Other EEG studies were based on the design of Pitts et al. (2012) with geometrical configurations to investigate neural responses associated with stimulus awareness, such as Schlossmacher et al. (2020), evaluating ERP’s, and Harris et al. (2018), evaluating oscillatory activity. Those studies focused on differences in ERPs between trials with and without a US, and did not compare performance in the main task between those conditions.

Finally, fMRI and MEG recordings were employed by some studies to identify neural signatures of implicit processing during inattentional blindness. In general, results reveal similar patterns of neural responses to the US between participants that were aware and those who were unaware of the stimulus for scene segmentation (Scholte et al., 2005) and the Kanizsa figure illusion (Vandenbroucke et al., 2014).

### *3.1.2. Assessment of awareness*

Regarding the tasks employed to assess awareness, most experiments included more than one task (see Table 1). Often, both a less demanding task (e.g., a yes/no question asking participants if they saw anything besides the main task stimuli) and a more demanding one (e.g., asking participants to describe what they saw) were used. However, in some experiments (e.g., Rashal et al., 2017) multiple similar tasks were employed, such as a forced-choice questions about the organization (columns vs. rows) or shapes (squares vs. circles) formed by the background stimuli. Some papers reported the number of aware participants for each measure separately (Kimchi & Razpurker-Apfeld, 2004; Moore et al., 2004; Razpuker-Apfeld & Pratt, 2008). Others reported only the number of participants that were considered aware in more than one measure (e.g., reporting seeing something besides the the main task stimuli *and* being able to describe it), instead of describing performance separately for each measure (Gabay et al., 2012; Lo & Yeh, 2008; Mack & Rock, 1998; Most et al., 2005; Schnuerch et al., 2016). A small number of papers reported both the results for each measure separately and the number of aware participants considering all measures (Beanland & Pammer, 2010; Wood & Simons, 2019).

In the majority of cases, awareness was measured only once with each measure, i.e., subjects responded to each question only once. When more assessments were performed, this was done using non-behavioral measures such as ERPs or SCRs in addition to the behavioral measures. In those studies, however, the criteria for categorizing participants as aware or unaware was a behavioral measure, with non-behavioral measures being employed with other objectives, such as identifying neural correlates of awareness (e.g., Pitts et al, 2012).

Concerning the number of participants for awareness tests, all studies evaluated participants individually for awareness. However, they differed in how those assessments were used for the investigation of implicit processing. Some studies used awareness measures for post-hoc selection of a subset of unaware participants, who were then tested for implicit processing of the US separately from other participants. Others analyzed participants as a group, testing if the group perceived the US more than would be expected by chance. Then, the whole group was tested for implicit processing.

We also observed differences regarding objective and subjective measures of awareness (Seth et al., 2008). Objective measures assess awareness by the performance on specific tasks that allow to verify mismatches between stimuli and responses (Schmidt & Vorberg, 2006), such as 2-alternative forced-choice (2-AFC) recognition tasks. Subjective measures, on the other hand, rely on participants' report of their own mental states; examples include confidence ratings and the perceptual awareness scale (Sandberg et al., 2011). Of the 51 experiments included in our review, 11 combined objective and subjective measures, 50 used only objective measures, and only one used only subjective measures. The majority of studies (n = 47) employed yes-or-no detection questions as objective measures, while others used forced-choice tasks (n = 33) or free/cued recall questions (n = 32). Most of the studies (n = 49) used more than one task. Although most of the time results for all tasks were reported, it was rarely the case (n = 6; Most et al., 2005; Schnuerch et al., 2016, both experiments; Pitts et al., 2012; Shafto & Pitts, 2015; Schelonka et al., 2017) that more than one task was considered to categorize participants as aware or unaware (for studies using post-hoc data selection) or to determine that awareness was not above chance in a sample. The subjective measures employed were mainly confidence (n = 13) and rarely frequency (n = 3) ratings. Of the experiments using subjective measures, four (Harris et al., 2018; Pitts et al., 2012; Shafto & Pitts, 2015; Schelonka et al., 2017) used those measures to categorize participants as aware or unaware. The remaining experiments (Lo & Yeh, 2008, exps. 1 and 2) classified participants using objective measures, employing the subjective measures to compare confidence between the inattention and divided-attention phases.

## 3.2. Quantitative review (Meta-analysis)

### *3.2.1. Overall meta-analytic results*

Table 2 reports Fisher's *z* correlation values and standard errors for effect sizes included in the meta-analysis.

[Insert Table 2 here]

The random-effect analysis of the overall implicit effect size showed a mean effect size of .33, with a 95% confidence interval (CI) ranging from .21 to .45. This overall effect was significant ($t$ =5.43, $p$ < .001). A heterogeneity test showed a significant result as assessed by the *Q* statistic ($Q(85) = 1679.69$, $p < .001$), with a large heterogeneity value of $I^2 = 89.98\%$. CI limits varied widely across experiments, which prompted us to search for outliers, which were assessed by searching for CIs that fell outside the limits of the pooled effect size with 95% CIs. We also conducted an influence analysis using the leave-one-out method (Viechtbauer & Cheung, 2010) to examine if any of the effect sizes exerted a disproportionate influence on the model. These procedures identified two outliers (Russell & Driver, 2005, exp. 2; Wood & Simons, 2019, exp. 2). Re-fitting the model excluding the outlier led to a similar overall effect size (.35, 95% CI = [.27; .44]), with similar significance level ($t = 8.33$, $p < .001$). The heterogeneity value also fell to $I^2 = 75.81\%$; the heterogeneity test was still significant ($Q(82) = 293.70$, $p < .001$). Whereas the variance estimate within studies was not significant (.0248, $p = .36$), there was a significant variation between studies (0.061, $p = .006$) suggesting clustering by moderating variables between studies. We explore this in the moderation analyses reported later.

To investigate the impact of different criteria to classify participants as aware or unaware, we fit separate meta-analytic models using effect sizes obtained using lax and strict criteria. For lax measures, a two-level model showed an overall effect size of .53, with 95% CI limits of .08 and .80. The overall awareness effect was significant ($z = 2.26$, $p = .02$), indicating that participants' awareness of the US was above chance across studies according to this criterion. In comparison, the model for effect sizes according to a strict criterion showed an overall effect size of .40, with 95% CI limits of -.06 and .72. In contrast to the lax measures, awareness was not significantly above chance using the strict criterion ($z = 1.71$, $p = .09$). The mean percentage of aware participants in the lax and strict criteria were 57% and 37%, respectively. Because rates of noticing are expected to vary widely in inattentional blindness experiments , we did not compute heterogeneity statistics for these models. Overall, these results show that the conclusion about whether processing of a US is explicit or implicit depends on the criterion used to categorize participants as aware or unaware.

### *3.2.2. Publication bias*

To evaluate the presence of publication bias for implicit effects, we visually inspected individual effect sizes on a funnel plot, which shows each effect size against its standard error. In these plots, an asymmetry is indicative of publication bias due to small-study bias (Borenstein et al., 2009). The plot showed an asymmetry (Figure 2), so we employed the Begg and Mazumdar's (1994) rank correlation test to investigate if there was a correlation between individual effect size and sample size. A Kendall's tau of 0.328 ($p < .001$) was observed, indicating a risk of publication bias. We assessed this using the trim-and-fill method (Duval & Tweedie, 2000). This analysis showed 26 studies missing on the left side (Figure 2). After those studies were filled, we fitted the model again to the data, which showed a still significant overall effect ($t(109) = 3.71$, $p < .001$), although the effect size was smaller ($r = .18$, 95% CI = [.08, .28]). Hence, the results suggest that there is a true effect of implicit processing across studies, but there appears to be a bias towards publishing results in the expected direction (i.e., that US are implicitly processed during inattentional blindness).

**Figure 2**

*Funnel plot of implicit effect sizes*

[Insert figure 2 here]

Importantly, we did not conduct the same publication bias analyses for awareness effects. The reason is that several of the studies reported results for multiple measures, with some of those clearly not showing that participants were unaware, as described above. Thus, we have no reason to suppose that results were selected for publication in a biased manner.

#### *3.2.3. Moderator analyses*

In order to explore whether variations in experimental design influenced implicit effect sizes, we conducted separate analyses for clusters of papers defined by moderator variables. Subgroup analyses were performed in the case of categorical variables, and meta-regressions for continuous variables. We selected variables of interest from our coding procedure presented in Table 1, leading to the following list of moderator variables:

1. Relevance of the US (relevant vs. not relevant)
2. Dynamic vs. static
3. Group assessment of awareness (whole sample vs. post-hoc selection)
4. Type of implicit measure (RT or accuracy)
5. Number of trials for implicit test
6. Number of participants for implicit test

In addition to the moderator analysis using the variables above, we also conducted a subgroup analysis of papers using the inattention paradigm, since they present a uniform design and adopt several methodological precautions not present in the other papers (the inattention paradigm is discussed in detail in Section 4.1). Importantly, those studies often aim to sketch a coherent picture of the relationship between grouping and attention. It is interesting, then, to investigate the general picture of these studies and the impacts of using this particular paradigm through a subgroup analysis.

The results showed no significant differences ($t(81) = 1.53$, $p = .13$) in effect size for contrasts with relevant and irrelevant US ($r = .51$, 95% CI [.18, .84] and $r = .34$, 95% CI [.25, .43], respectively). Similarly, no significant differences were observed ($t(81) = 0.62$, $p = .53$) between studies employing a static US ($r = .37$, 95% CI [-.05, .80]) or a dynamic US ($r = .30$, 95% CI [.10, .51]). However, a significant difference was observed ($t(81) = 3.05$, $p = .003$) between studies that assessed implicit effects on the whole sample ($r = .49$, 95% CI [.22, .75]) and those employing post-hoc selection of participants ($r = .24$, CI [.14, .35]), with a larger effect for the former. Finally, the implicit effect size for the subgroup of inattention studies was significantly larger ($t(83) = 2.56$, $p = .01$) than in other studies ($r = .50$, 95% CI [0.2436, 0.764] and $r = .2945$, 95% CI [0.1931, 0.3859], respectively).

Effect sizes were somewhat different for RT and accuracy ($r = .32$, 95% CI [.07, .59] and $r = .38$, 95% CI [.28, .48], respectively), although this difference was not significant ($t(83) = -0.63$, $p = .12$). Tests for continuous moderators showed moderation both by number of trials ($t(81) = 2.26$, $p = .03$) and number of participants ($t(81) = -2.28$, $p = .02$), indicating that effect sizes increase with number of trials, but decrease with number of participants.

**Figure 3**

*Plots of moderation effects for implicit effect sizes by number of trials and number of participants. Black circles represent significant effects; grey circles indicate non-significant effects. Circle sizes are proportional to the precision of the estimated effect.*

[Insert figure 3 here]

## 4. Discussion

In the current study, we reviewed articles that investigated visual processing of unexpected stimuli (US) outside of the participants' awareness while their attention was directed to an attentionally demanding task. We examined multiple methodological aspects of 27 papers and employed meta-analysis to investigate the occurrence of implicit processing in a set of 51 experiments across a subset of 17 papers. We found evidence for implicit processing of the US across studies. Furthermore, we found no evidence for publication bias, indicating that the effect is a true effect.

The studies reviewed include experiments by Mack and Rock (1998) that possess several features commonly found in inattentional blindness designs (.e.g, the studies reviewed in Kreitz et al., 2020): very few critical trials (in their case, only one); a clear separation between the US and stimuli in the main task; and assessment of awareness immediately after presentation of the US. While the first feature may contribute to unreliable measurements (Vadillo et al., 2016), the last two have been pointed out as important procedures to ensure that the US is actually unattended (Driver et al., 2001; Wolfe, 1999).

One noteworthy feature of the experiments by Mack and Rock (1998) is the use of offline measures to evaluate implicit processing, as opposed to testing for interference by the US on the performance in the main task. Offline measures have been criticized for being vulnerable to memory issues, since they are only administered after the US has disappeared (Moore, 2001; Moore & Egeth, 1997). Furthermore, they only allow for one measurement of implicit processing per participant, instead of the multiple measurements that can be performed using online measures.

Despite those criticisms to the design of Mack and Rock (1998), similar findings have been reported in studies using online measures. For example, Schnuerch et al. (2016) also found implicit semantic processing during inattentional blindness using online measures administered in multiple trials. Likewise, six experiments (Kreitz et al., 2015a, exp. 2; Kreitz et al., 2016a; Kreit et al., 2016b; Redlich et al., 2019, exp. 2) reviewed by Kreitz et al. (2020) assessed implicit US processing in a dynamic inattentional blindness task using offline measures. This task was based on Most et al. (2005, exp. 8), who used an online measure (decrement in accuracy in the main task when the US was presented) and found that the US was processed implicitly despite considerable heterogeneity between the stimuli across their experiments. Four of the six experiments (Kreitz et al., 2015b; Kreitz et al., 2016a) reported by Kreitz et al. (2020) found implicit processing of the dynamic US (Table 2). Thus, in spite of being vulnerable to a number of issues, offline measures appear to lead to results that match those obtained with online measures.

Wood and Simons (2019) attempted to replicate the results of Mack and Rock (1998) using a large sample ($n$ = 216) but did not succeed. This replication's effect size was also a clear outlier in our meta-analytic model. However, this experiment incorporated some differences in stimulus selection compared to Mack and Rock (1998). Specifically, very low-frequency words were selected, which might have reduced the priming effect on the stem-completion task and yielding a non-significant result. Moreover, in contrast with Mack and Rock (1998), Wood and Simons (2019) did not conduct control experiments to determine the base rate of choice of the words employed as primes, making it difficult to assess how often they would be chosen in stem-completion tasks without priming.

Moore and Egeth (1997) criticized the use of offline measures, and their study was widely influential on subsequent experiments about implicit processing during inattention: Several studies (Ariga et al., 2007; Lo & Yeh, 2008; Moore et al., 2003; Moore et al., 2004) followed their recommendations regarding the use of online measurements. The results of Moore and Egeth (1997) have been successfully replicated in two studies (Lo, 2018; Wood & Simons, 2019, experiment 1). In particular, Wood and Simons (2019, experiment 1) used a large sample ($n$ = 165) in their replication of Moore and Egeth (1997). The number of participants whose responses were influenced by the illusion was comparable to that of the original study ($r$ = .68, close to the effect size of .70 observed in the original study). This was the case even though Wood and Simons (2019) employed only a single illusion-trial, in contrast to the 16 trials in Moore and Egeth's study.

However, Moore and Egeth's (1997) study was criticized by other authors (Driver et al., 2001; Lamy et al., 2006) on the grounds that, in their experiment, the proximity between the target lines and background dots might have led to the target falling within the attended area; that is, the background might have received spatial attention, so that the grouping was not truly unattended. Additionally, since the dots and the lines were presented in the same color, they might have formed a common group based on color, which might have attracted attention. Thus, it is possible that attentional processes have inadvertently influenced the results of Moore and Egeth (1997). A similar observation had already been made by Mack and Rock (Mack et al., 1992; see also Russell & Driver, 2005), who argued that, for stimuli to be really unattended, they should not only be task irrelevant, but also clearly separated from target items.

Supporting this interpretation are the results of Lamy and colleagues (Lamy et al., 2006). They conducted an experiment similar to experiment 3 of Moore and Egeth (1997) using the Müller-Lyer illusion and comparing the effect of the illusion during inattention between two conditions: in one condition, the lines were in the same color as the dots; in another condition, they were in a different color. They found that the illusion occurred even in the different-color condition, thus removing the confound pointed out earlier. Importantly, however, the illusion was weaker when the lines were different than when they were in the same color, which may be taken in support of an attentional leak to the US.

Lo and Yeh (2008) performed an experiment similar to Moore and Egeth (1997), but found a smaller effect size. Their design incorporated the modification that, instead of dots, the background matrix was composed of Gabors, which could be oriented horizontally or vertically. Even though they reproduced a bias in line-length judgement, this effect was only detected when the stimuli were presented for 500 ms, and not for the 200 ms condition. Intriguingly, the latter stimulus duration corresponded to the duration used in Moore and Egeth's study. This fact may indicate that texture segregation, which is necessary to extract the lines composing the illusion along the Gabors, demands a longer presentation time to occur without attention. Importantly, the effect size observed for the 500 ms experiment ($r$ = .58) was lower than the effect reported by Moore and Egeth (1997; $r$ = .70).

Other studies conceptually based on Moore and Egeth (1997) include: Moore et al. (2003), which provided results in support of the hypothesis that surface completion occurs without attention, but with a much smaller effect; Ariga et al. (2007), which found a same-object advantage induced by illusory figures when participants were aware of the figures (experiment 1), but not when they were unconscious due to inattentional blindness (experiment 2); and Moore et al. (2004) and experiment 2 in Lo and Yeh (2008), which found no evidence that simple unattended visual shapes elicit a Simon effect. These results suggest that surface completion, same-object advantage and response-end processes, such as the Simon effect, all demand attentional resources. However, those studies used small sample sizes (all below 30), and most of them included only a few trials in which the US was presented. Therefore, it is also possible that those studies were simply underpowered to detect the effects. Indeed, our moderator analysis indicated that the effect size increases with the number of trials. However, this effect was very small and should be taken with caution.

It is interesting to compare our results with a previous meta-analysis (Kreitz et al., 2020). That meta-analysis investigated effects of implicit processing of US during inattentional blindness across 16 experiments, 14 of which employed only visual stimuli. They found a small but significant effect ($d$ = 0.153, or $r$ = .076, $p < .01$) across those 14 experiments. In the current review, we extend those findings and provide evidence that the effect observed by Kreitz et al. (2020) appears to generalize to other inattentional blindness tasks. The effect sizes observed indicate that implicit processing extend to perceptual organization processes, such as surface completion by Kanisza configurations (Moore et al., 2003), in which patterns of stimuli that are grouped according to grouping or segmentation rules (Kimchi et al., 2016; Wagemans et al., 2012) change in the background. These are mid-level vision processes that segment input images into objects and background surfaces and precede later processes of visual recognition (Kubilius et al., 2014). Similarly, high-level processing appears to occur for unattended stimuli, such as semantic processing (Mack & Rock, 1998; Schnuerch et al., 2016). The fact that these processes occur during inattention contrasts with what is observed when stimulus strength is manipulated by paradigms such as continuous flash suppression (Moors et al., 2016; Moors & Heyman, 2014). Thus, even though both attention and stimulus strength are necessary for awareness of visual stimuli, limiting either has distinct consequences for the type of processing that can occur for unreported stimuli. This is in line with the framework of neural workspace theory, in which attention and stimulus strength both contribute to consciousness, but influence distinct levels of processing. On the other hand, the few studies (Lo & Yeh, 2008, exp. 2; Moore et al., 2004) that investigated if response interference takes place for unattended stimuli, employing the Simon Effect, found negative results (Table 2). This may indicate that processing of stimuli for response selection cannot occur without attention, but this has not been studied extensively in inattentional blindness experiments.

Even though the overall effect size found here was much higher ($r = 0.35$) than that found by Kreitz et al. (2020), this difference might be partly explained by methodological differences from other studies. Particularly, several studies used the same paradigm (the inattention paradigm) that employs a method to categorize participants as aware or unaware which may lead to higher estimates of implicit processing. We discuss those studies below.

**4.1. Inattention Paradigm**

In addition to the criticisms on Moore and Egeth's (1997) design discussed above, Driver et al. (2001) argued that the close proximity of the presented dots might have led to a confound with the observed effects of grouping, such that the effects might actually reflect the blurring of low spatial frequency channels. In an attempt to solve those methodological problems, Driver and colleagues (2001) proposed a different paradigm, which they named the Inattention Paradigm (Driver et al., 2001; Russell & Driver, 2005). This paradigm is not usually referred to as “inattentional blindness”, but it does follow the same general design.

In those inattention studies, participants perform an attentionally demanding detection task using a central matrix composed of black and white pixels. Specifically, the task requires participants to detect small changes over two sequential presentations of the matrix interspersed with a brief blank screen. The blank screen makes the change more difficult to discriminate, in a similar manner to change blindness experiments (Rensink, 2010). Stimuli are presented in the background surrounding the target in one of several configurations, e.g., grouping in rows or columns. The background configurations either change or stay the same after the blank screen.

The rationale for the inattention paradigm can be described as a combination of change blindness and contextual cueing (Driver et al., 2001): changing the background configuration (the visual context) across frames facilitates the detection of a small change in the matrix, compared to identical background configurations. The specific context may consist, for example, in the presence vs. absence of some form of perceptual grouping. Employing this paradigm, a series of studies (e.g., Russell & Driver, 2005) investigated the effects of a number of variables on the interference exerted by the background on the matrix task, such as distance between the background changes and the matrix, matrix/background presentation time, and large saccades intervening between the two matrix presentations.

Our moderator analysis showed that studies employing the inattention paradigm displayed much larger effects of implicit processing than other studies ($r$ = .50 vs. $r$ = .29, respectively). The reason for this difference is not clear. Overall, inattention paradigms present several advantages when compared to other studies in the review: they employ online measures instead of retrospective measures of implicit processing; they include multiple critical trials, which results in larger effect sizes, according to our moderator analysis; and the US is clearly distinct from the stimuli in the main task. However, it is also possible that this difference is an artifact of how participants are selected in this paradigm: all inattention studies test for implicit effects using the whole sample (group assessment of awareness), including both aware and unaware participants for testing. As we argue below (Section 4.2), it is possible that this method leads to inflated rates of implicit processing.

A second issue is that the change in background configuration for some types of grouping might simply not be salient enough to interfere with the main task (Rashal et al., 2017). Concerning this point, Razpurker-Apfeld and Pratt (2008) used an inattention design and ERPs to investigate implicit similarity grouping in columns and rows (simple grouping), or in triangle/arrow shapes (complex grouping). They observed that, despite their behavioral results, suggesting that only simple grouping occurs during inattentional blindness, ERP differences between random and grouped stimuli were observed for both types of grouping. Thus, it may be the case that grouping processes differ in detectability by behavioral measures, which complicates a comparison between types of grouping based on patterns of significant vs. non-significant results. This issue is especially important because inattention studies propose to distinguish between different types of grouping processes regarding their attentional demands, often finding that some types of grouping occur during inattention whereas others do not (Rashal et al., 2017; Razpurker-Apfeld & Pratt, 2008).

**4.2. Measures of awareness**

In addition to the meta-analysis of implicit effects, we also used meta-analytic models to examine the impact of different criteria to categorize participants as aware or unaware in studies which tested for awareness in the whole sample instead of testing participants individually. We followed the distinction used by Wood and Simons (2019) between "lax" and "strict" measures. For lax measures, results suggest that participants were aware of the US, which implies that the processing of the US was explicit and not implicit. For strict measures, such processing did not differ from chance. Thus, the conclusions of studies may change depending on the criterion to categorize participants as aware or unaware.

Measures of awareness can be distinguished between objective and subjective measures (Seth et al., 2008). Objective measures are based on performance, assuming that a stimulus is conscious if the subject performs above chance in a detection or categorization task such as forced-choice recognition (Dehaene & Changeux, 2011). In those tasks, responses can be categorized as correct or incorrect (Kingdom & Prins, 2010). Subjective measures of awareness rely on subjective reports such as ratings of visibility (e.g., the Perceptual Awareness Scale, or PAS) or higher-order reports such as confidence ratings (Dehaene & Changeux, 2011; Sandberg et al., 2011). In subjective tasks, responses cannot be evaluated as correct or incorrect (Kingdom & Prins, 2010). The issue of whether objective or subjective measures are preferable to assess awareness is a matter of current debate (Zehetleitner et al., 2015). It has been argued that objective (e.g., forced-choice recognition tests) and subjective (e.g., confidence ratings) measures of awareness differ in sensitivity (Sandberg et al., 2011). Subjective measures have been criticized for being contaminated by changes in response criterion (Schmidt & Vorberg, 2006). On the other hand, objective measures, such as forced-choice tasks, might yield above-chance performance even when participants cannot report stimuli in plain sight (Seth et al., 2008; Mack & Rock, 1998), and thus overestimate consciousness. Others have proposed that tasks like forced-choice recognition benefit from both implicit and explicit processes (Bressan & Pizzighello, 2008; Jacoby, 1991; Roediger III et al., 2007; Seth et al., 2008) and therefore are unable to distinguish between conscious processing and implicit processing (Dehaene & Changeaux, 2011). Furthermore, these tasks may be accomplished by familiarity (Roediger III et al., 2007), which, even if considered under the spectrum of consciousness, is qualitatively different from consciousness as observed in recollection and in conscious perceptual awareness.

Another important issue in assessing awareness concerns the use of retrospective measures, i.e., when there is a considerable temporal gap between the critical trial and the inquiry about the awareness assessment. Delays between exposure to a stimulus and reports about awareness of that stimulus allow for forgetting, interference, and intrusion of extraneous content (Ericsson & Simon, 1984). In those cases, alternative explanations for negative responses to awareness measures are possible, e.g., memory issues (“inattentional amnesia”, Wolfe, 1999), with participants who were aware of the critical stimulus during its presentation are erroneously categorized as unaware. As a result, processes which can only occur when a participant is aware of a stimulus may be erroneously assumed to occur without awareness. In this review, several studies employed long delays after US presentation to assess awareness (see Table 1). Testing time may be more or less critical depending on the type of awareness measure employed. Specific details about visual stimuli may be less successfully encoded and more prone to forgetting than gist memory (Ahmad et al., 2017; Rocha et al., 2013). This is important for studies on implicit processing during inattentional blindness, since the tasks used to categorize participants as aware or unaware of the US varied considerably across studies and may assess distinct types of memory. For example, memory of the US may be worse for detailed information (e.g., "what were the patterns in the background?") than for coarse information (e.g., "were there patterns in the background?").

Memory issues may not be entirely avoidable in inattentional blindness experiments. Nonetheless, they may be more severe when long blocks replace the single critical trial of more typical inattentional blindness designs (e.g., Mack & Rock, 1998). Long blocks of inattention trials may increase statistical power for tests of implicit US processing, avoiding the need for large samples when designs such as those of the classical studies by Mack and Rock (1998) are employed (Gabay et al., 2012). It also allows for the use of measures that might detect effects not accessible to behavioral measures, but which demand large numbers of trials, such as ERPs (Pitts et al., 2012; Razpuerker-Apfeld & Pratt, 2008). However, those block designs come with the associated cost of not allowing to determine the point of the experiment when participants became aware of the US, since awareness is only probed once and at the end of the block. This may result in grouping together aware and unaware trials, which may lead to an explicit effect being erroneously interpreted as implicit. Additionally, using long blocks may also increase the chance that participants briefly perceive the US but forget about it when probed (Mack & Rock, 1998).

Thus, although immediate testing after the first appearance of the US results in a limit of one critical trial by participant, this design might be preferable to avoid mixing of implicit and explicit results.

This has consequences not only for the awareness status of the US, but also for the conclusion of such studies on the necessity of attention for the type of processing under investigation. Studies on inattentional blindness manipulate attention to make participants unaware of the US, implicitly assuming that attention is necessary for awareness, a view often put forward in the literature on consciousness (e.g., Cohen et al., 2012; Dehaene et al., 2006). Although it is difficult to assert that an US is truly unattended, since there is a possibility of attention “spillover” to background US even though participants are focusing on an unrelated main task (Rashal et al., 2017), studies often conclude that, when participants are unaware of the US, not enough attention was directed to the US to allow it to enter consciousness. However, the use of unreliable measures compromises this inference, since participants might be aware of the US, and thus be directing attention to it.

Variations in the details that participants are required to recall may also have an impact in awareness assessments carried out immediately after US presentation. Requirements to describe the US in detail may not detect awareness of high-level, abstract properties of the stimuli, also known as ensemble representations (Whitney & Yamanashi Leib, 2018). Recently, Ward et al. (2016) showed that, when appropriate questions are posed, individuals can report statistical ensemble properties of color diversity for visual stimuli but are unable to report individual colors. Similar observations have been reported for size discrimination, face recognition and other visual features (see Whitney & Yamanashi Leib, 2018, for a discussion). Thus, asking participants for details about the US may not detect awareness of high-level properties, which in some cases might suffice to exert an effect on measures of implicit processing, e.g., interference of background configuration on performance in a central task. This is the case even if explicit ensemble representations do not demand (explicit or implicit) representation of individual features (Ward et al., 2016). The numerous observations of ensemble representations formed in the absence of attention (e.g., Corbett & Oriet, 2011; Peng et al., 2019; Whitney & Yamanashi Leib, 2018) provide empirical support to this suggestion.

Along these lines, some studies (Razpurker-Apfeld & Pratt, 2008; Russell & Driver, 2005) reported that participants responded affirmatively to questions about whether they perceived something in the background, even if they could not report what it was. However, participants were still treated as unaware of the stimuli if they could not identify the US in a forced-choice task, for example. In other studies, participants were considered aware only if, in addition to reporting noticing a pattern, they could also describe it. Studies with such requirements often found low rates of noticing. For example, Schnuerch et al. (2016, exps 1 and 2) reported noticing rates of 6 and 8 percent, which were 1.65 and 1.72 standard deviations below the mean noticing rate of 50.7 percent.

To investigate the impact of such variations, Wood and Simons (2019) employed two distinct criteria to group participants on the basis of awareness: a lax criterion, according to which they had to report noticing a pattern even if they could not recall that pattern; and a strict criterion, according to which they also needed to select the correct pattern in a forced-choice task. The proportion of participants who were aware of the patterns by the lax criterion was much higher than by the strict criterion (32% vs, 16.5% in experiment 1; 51.8% vs. 31.3% in experiment 2).

The studies described above suggest that forced-choice measures may lead to very different conclusions about awareness than simple recall questions. In the case of Razpurker-Apfeld and Pratt's (2008) study, for example, all participants detected something else in the background, while forced-choice discrimination was low (1 participant out of 14). Other studies (Moore & Egeth, 1997; Moore et al., 2003; Moore et al., 2004; Lo & Yeh, 2008; Rashal et al., 2017) also assessed awareness with multiple questions. However, those studies did not analyze the impact of each measure in their results. Instead, when significance tests showed that the proportion of noticers did not deviate from chance according to at least one of the measures, the analysis of implicit processing was carried out assuming lack of awareness or attention. Although measuring awareness is notoriously difficult, using the more demanding measure from a set of awareness measures to classify participants as aware or unaware may lead to incorrect classification. On the other hand, measures with very low demands, such as asking "Have you noticed anything else in the background?", might be susceptible to effects of experimenter effects (Rosenthal, 1966) if participants feel they are supposed to respond affirmatively. They may also result in high rates of false positives due to false alarms, leading to large numbers of exclusions and increasing the number of participants necessary to achieve a sufficient sample size for analysis.

Fortunately, this may not be an issue for all designs. Wood and Simons compared implicit effects for unaware participants selected according to their lax and strict criteria and observed that the proportions of participants responding according to the illusion was comparable across conditions (84.2% vs. 80.7% in experiment 1 for lax and strict criteria, respectively; 0.9% vs. 1.0% in experiment 2). When a distinct procedure is used to assess awareness, however, the sensitivity of awareness measures may be a more serious issue. Specifically, some experiments performed what we called “group assessment of awareness”: instead of selecting participants by individual awareness results, they tested if the sample as a whole noticed the US more often than chance. When they did not, the whole sample (including both aware and unaware participants) was analyzed for processing of the US. If the analysis showed that the US was processed, the study concluded that the US was implicitly processed during inattention. However, this selection procedure leads to another problem: it is possible that processing of the US occurs only for aware participants, and that, if analyzed separately, aware and unaware participants might exhibit distinct results concerning processing of the US. However, when both aware and unaware participants are pooled together and considered unaware as a group, testing for implicit processing in the group might yield significant results due to the aware participants only. In this case, the study would arrive at the incorrect conclusion that there is implicit processing of the US for unaware participants when there is none.

On the other hand, the same process might occur for both aware and unaware participants, but the magnitude of its effect on responses to the task might be larger in the first group than in the second. In this case, pooling aware and unaware participants might inflate the results for implicit processing, leading to overestimates of effect sizes. Our moderator analysis of group assessment of awareness on implicit processing suggests that this might be the case: studies that employed this method resulted in larger effect sizes than studies that employ post-hoc selection of participants ($r$ = .48 vs. $r$ = .24, respectively). This issue does not occur for experiments that select participants for analysis according to their individual performance on awareness tests (“post-hoc data selection”; e.g., Mack &

Rock, 1998; Schnuerch et al., 2016). This method has its own problems, however, discussed in section 4.3.

**4.3. Regression to the mean and implicit processing**

Several papers in this review are affected by regression to the mean, as a consequence of using post-hoc data selection to investigate unconscious processing (Shanks, 2017). This occurs when experimenters select a subgroup from the study sample according to some baseline score or the result of a post-hoc statistical test for awareness of a stimulus. The performance of participants in this subgroup is then tested in a task that demands processing of that stimulus. A level of performance above chance is taken as evidence of processing without awareness. The issue of regression to the mean arises because awareness is imperfectly correlated with measures of performance. Cohen et al. (2003) explains how regression to the mean is a necessary consequence when two variables exhibit a correlation below 1.0 and extreme values are selected from one variable. In the case of post-hoc data selection for awareness, when subgroups from one extreme of the sample scores on the awareness test are selected, the scores on the performance measures will regress toward the mean. If the mean of performance scores of the whole sample is above 0, then the subgroup's average performance will regress toward that level, possibly resulting in results falsely indicating implicit processing. Not all papers in this review employed methods that are vulnerable to this particular issue. However, those studies employed group assessment of awareness, which has problems of its own, discussed in the previous section.

Another issue with this procedure is related to the statistical power of awareness tests. Studies on implicit processing typically assess awareness with specific measures, such as recognition or recall tests, and evaluate deviations from chance with null hypothesis significance tests (NHST). Participants are usually considered unaware when null results are thereby obtained. However, the outcome of these tests depends on their statistical power, which in turn depends on the number of trials for the assessment (Vadillo et al., 2016). Crucially, as seen in Table 1, most awareness assessments in the studies in this review had only one trial. If statistical power is low due to a small number of trials, the tests might not detect a significant deviation from chance even if most participants are actually aware of the US. In contrast, the number of trials to assess implicit processing varied from one to 600.

Additionally, as previously discussed, while the awareness assessments were always retrospective, most of the implicit measurements were performed online, thus likely providing higher sensitivity than the offline awareness measures (Lovibond & Shanks, 2002). In that case, statistical testing based on the scores obtained with the implicit measures have higher power to detect an effect, and thus provide a significant result, than testing with awareness scores. Importantly, this is the case even if an implicit effect is exclusive to aware participants. Hence, a combination of high-powered implicit tests and low-powered awareness tests may lead to a null result for awareness, even if some participants are aware, and to a significant result for implicit processing, even if only aware participants process the US. In that case, the study would arrive at a false conclusion of implicit processing.

Given the above arguments, it may be preferable to avoid group assessment of awareness as well as post-hoc data selection altogether. Shanks (2017) lists three alternatives to post-hoc data selection. The first consists in using two different measures of awareness, one to select unaware participants and the other to estimate awareness in that subsample. The example suggested by Shanks (2017) of selecting odd and single trials is not possible in IB experiments like the ones reviewed here, where participants are typically assessed for awareness only at a single moment, even if a variety of measures are employed. It might be possible to use multiple tasks in conjunction, but this was rarely done in the papers reviewed here, which used multiple tasks to assess awareness. A second option is to compare the performance of unaware participants against the performance predicted by regression to the mean alone or correcting for regression to the mean. Lastly, Shanks (2017) suggests that researchers avoid post-hoc data selection completely, and instead select unaware participants using experimental manipulations instead of measured variables. However, even though this is a possibility in investigations employing methods such as CFS and masking (Breitmeyer, 2015), attention is usually not amenable to reliable manipulation, so this suggestion is probably not an alternative for studies on inattention.

Finally, studies on implicit processing in inattentional blindness might benefit from extending Lovibond and Shanks' (2002) recommendations for assessment of the relationship between conditioning and awareness to the investigation of implicit processing during inattentional blindness and awareness: a) a null outcome between aware and unaware subjects in process of the US; and b) a significant outcome in a test between conditions of this processing in unaware participants alone. This may allow experimenters to address not only the question of whether processing of the US requires attention, but also the related question of how attention influences such processing.

## 5. Final considerations

The issue of whether stimuli that are processed under inattentional blindness are indeed unattended and unconscious is an intricate question. It has been suggested, for example, that an unexpected stimulus, which would trigger an attentional shift in regular conditions, may unconsciously capture a small amount of attention even if subjects are inattentionally blind (Bressan & Pizzighello, 2008; Rashal et al., 2017). The investigation of this relationship is constrained by the limitations of the current state of knowledge on attention, which comprises several subtypes that are not clearly distinguished and cannot be measured unambiguously. Moreover, a lack of consensus on how to appropriately measure awareness opens the conclusions often made about unconscious processing to methodological criticism.

Overall, the current meta-analytic results for implicit processing during inattentional blindness indicate a real effect, despite the variability in the studies reviewed here. However, the conclusions for awareness are less straightforward. Although measuring awareness is far from simple (Sandberg et al., 2010; Zehetleitner & Rausch, 2013), we believe that the current review contributes to highlight the importance of employing adequate methods to assess awareness when designing studies on implicit processing during inattentional blindness.

Lastly, we would like to consider the possible implications of our discussion to a particular theoretical distinction that has been vigorously debated in recent years. This distinction proposes two types of consciousness: phenomenal consciousness vs. access consciousness (Block, 2007; Lamme, 2003, 2004). Phenomenal consciousness refers to the subjective experience of perception, for example, seeing, whereas access consciousness consists of cognitive contents that are selected by attention for further processing, for example, reporting or decision-making. Since measures of awareness require participants to report a stimulus, they are believed to measure only access consciousness (Block, 2007). Hence, it is possible that inattentionally blind participants experience the US (i.e., are phenomenally conscious of the US), but the stimulus fails to be selected for further processing. Despite its plausibility, it is nevertheless difficult to conceive how this sort of consciousness might be measured.

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