UNIVERSITEIT GENT

FACULTEIT PSYCHOLOGIE EN PEDAGOGISCHE WETENSCHAPPEN

Automatic evaluative stimulus processing as a function of feature-specific attention allocation: New directions in measuring and changing implicit evaluations.

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Proefschrift ingediend tot het behalen van de academische graad

van Doctor in de Psychologie

2016

ACKNOWLEDGEMENTS

Four years ago, I entered the world of academia for what turned out to be an exciting and challenging endeavor that would eventually lead to this dissertation. It was an adventure with many ups and downs, through trial and error, but somehow I seem to have made it to the end. Of course, I would not have been able to successfully complete this task without the help and support of so many. Therefore, some overdue thank you's are in order.

Foremost, I would like to thank my promoter *Adriaan Spruyt* without whom I would not have been able to finish this dissertation project. Thank you for your guidance and support, the enlightening talks and your patience when correcting the same errors in my manuscript over and over again throughout the four years. Most importantly, I would like to thank you for all your efforts in guiding me through the process of writing a dissertation and for being a great friend.

Second, I'm also very grateful to my co-promoter *Jan De Houwer* for giving me this opportunity. I would like to thank you for all your helpful suggestions and comments during the project. I'm also highly thankful to the members of my guidance committee. Thanks to *Dr. Anne Gast, Prof. Dr. Wim Notebaert, and Prof. Dr. Markus Kiefer* for their time, ideas, and constructive comments. Additionally, I'm also perpetually indebted to all the members of the *LIPLAB*. Thank you for all the great moments, your patience, ideas, and constructive feedback! Special thanks go out to *Marijke, Gaetan, and Pieter* with whom I shared an office during most of the dissertation-years. Thank you for all the talks and the interesting discussions. Generally, I would just like to thank you guys for all the good times!

Of course, I would never have made it up to this point without the help and loving support of friends and family. I would like to thank some of my closest friends for putting up with me throughout so many years. Thank you for all the phone calls, the countless game- and movie nights, pranks, and mayhem. You guys never fail to light up even the dreariest of days! We have been friends for almost a decade or even longer and I'm sure we will be for many decades to come! Thank you *Silke, Laura, Kea, Elfi, Sjoukje, Bram, and Yuri*!

Most of all I would like to thank my parents, *Luc and Francine*. There exist no words to describe how blessed I am to have been raised by such kind and gentle souls. You have loved and supported me throughout every decision I have made and I could not be more grateful for that. In the same vein, I would like to thank the rest of my family. I think we are truly blessed to have such close family ties. I'm very lucky to have so many people close to me that are so loving and supportive. I would like you to know that I cherish each and every one of you and you all hold a very special place in my heart.

Finally, I would like to thank *Joris.* Thank you for being so supportive and for being able to put up with my messy head and crazy ideas. Thank you for sticking by me when I decide to take on a master in statistics during my dissertation project or when I try to convince you that we do have room for at least one more foster cat. I'd like to thank you for simply being you and the constant sunshine you bring to my life.

Jolien

June 2016

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CHAPTER 1

GENERAL INTRODUCTION

INTRODUCTION

Throughout the history of humankind, people have been fascinated by the factors that drive human behavior. Clinicians try to understand the mechanisms that drive pathological behavior, politicians want to know how they can influence the voting behavior in their electorate, and marketers aim at influencing consumer behavior. As argued by Allport (1935), likes and dislikes are important determinants of behavior. Indeed, our interpersonal interactions, the activities we pursue, the products we buy, etc., are all, to some extent, guided by our personal likes and dislikes. Moreover, there is ample evidence showing that (most) likes and dislikes are acquired over time rather than innate (Rachman, 1977; Walther, Nagengast, & Trasselli, 2005). This means, that we cannot only measure likes and dislikes in order to predict behavior, insights in the mechanisms underlying attitude acquisition and attitude change might also enable us to influence behavior. It may come as no surprise then, that likes and dislikes, also referred to as evaluations or attitudes, have become a central area of interest in psychological research.

Traditionally, likes and dislikes were measured by asking participants to conduct and report a self-assessment of their likes and dislikes, e.g., by means of a questionnaire. Participants might be asked, for example, to evaluate the sentence ”I like fruit” on a scale from one (strongly disagree) to ten (strongly agree). These so-called explicit measures have been criticized, however, because they do not only depend on the goodwill of participants to express their likes and dislikes but also on their ability to express them (Fazio & Olson, 2003; Greenwald & Banaji, 1995). Moreover, during the past decades, it has become increasingly clear that behavior is not only determined by explicit likes and dislikes but also by implicit likes and dislikes, that is, the spontaneous evaluative reactions that objects, persons, or situations evoke. These insights led to the development of a new class of measurement techniques that do not rely on direct verbal reports but aim to capture spontaneous evaluative reactions. The advantage of these so-called implicit measurement techniques is twofold. First, implicit attitude measures make it harder for participants to misreport their likes and dislikes. Second, participants do not have to rely on introspection to assess their own likes and dislikes. As an example, consider the evaluative priming paradigm (Fazio, Sanbonmatsu, Powell, & Kardes, 1986). In this paradigm, participants are typically asked to evaluate a series of target stimuli (e.g., a picture of a cute kitten) as being positive or negative. Each target stimulus is preceded by the presentation of a task-irrelevant prime stimulus (e.g., a picture of a spider). Crucially, the evaluative congruence between prime and target stimuli is manipulated: the primes and the targets share the same evaluative tone on some trials (hereafter referred to as congruent trials, e.g., the prime word 'kitten' followed by the target word 'cute'), but differ in evaluative tone on other trials (hereafter referred to as incongruent trials, e.g., the prime word 'kitten' followed by the target word 'nasty'). Typically, performance is facilitated on congruent trials as compared to incongruent trials (i.e., faster responses and less errors). The evaluative priming paradigm can be used to measure evaluative reactions to attitude objects by presenting attitude-relevant stimuli as primes. For example, by presenting pictures depicting spiders as primes, one could measure likes and dislikes toward spiders. Other, well-known examples of these implicit measurement techniques are the Implicit Association Test (IAT, Greenwald, McGhee, & Schwartz, 1998), the Affect Misattribution procedure (AMP, Payne, Cheng, Govorun, & Stewart, 2005), the Extrinsic Affective Simon task (EAST, De Houwer, 2003), and many others (see Fazio & Olson, 2003; Nosek, Hawkins, & Frazier, 2011 for a review).

In this chapter, I will first elaborate on the hypothesis that evaluative information processing occurs unconditionally (e.g., Arnold, 1960; Barlett, 1932; Lazarus, 1966; Wundt, 1907). Next, I will introduce the feature-specific attention allocation (i.e., FSAA) framework to account for the observation that automatic evaluative stimulus processing is modulated by selective attention allocation. I will then discuss recent studies demonstrating the moderating role of FSAA on the acquisition of likes and dislikes. Finally, based on the FSAA framework, I will present new predictions concerning the measurement and modification of spontaneous likes and dislikes and how these new ideas were tested during my research project.

Feature-Specific Attention Allocation modulates automatic evaluative processing

Various influential researchers have proposed that evaluative processing occurs in an unconditional and automatic fashion (e.g., Arnold, 1960; Barlett, 1932; Lazarus, 1966; Wundt, 1907). However, it wasn't until Zajonc (1980) published his seminal work on the primacy of evaluative judgments that the 'automatic evaluative processing hypothesis' was scrutinized systematically. The majority of knowledge concerning automatic evaluative stimulus processing has been derived from experiments using the evaluative priming paradigm (Fazio et al., 1986). The evaluative priming effect can come about only if participants process the evaluative tone of the task-irrelevant primes and can thus be used as an index of evaluative stimulus processing. By examining the conditions under which this effect can be obtained, one can thus learn about the conditions under which evaluative stimulus processing can occur. Numerous experiments with the evaluative priming paradigm have confirmed that humans can indeed process the evaluative tone of a stimulus under automaticity conditions (Moors & De Houwer, 2006). For example, Hermans, De Houwer, and Eelen (2001) showed that the evaluative priming effect takes place at a very short time interval (i.e., 0 ms) between the onset of the prime stimulus and the onset of the target stimulus. The occurrence of the evaluative priming effect at short stimulus onset asynchronies (i.e., SOA) is generally considered as evidence that stimulus evaluation is a fast process (Klauer, Roßnagel, & Musch, 1997). Moreover, the evaluative priming effect has been found even when participants were not instructed to evaluate stimuli (e.g., Bargh, Chaiken, Raymond, & Hymes, 1996; Spruyt, Hermans, De Houwer, & Eelen, 2002), or when stimulus duration prohibited the conscious perception of the stimulus (Draine & Greenwald, 1998; Greenwald, Klinger, & Schuh, 1995). Finally, the magnitude of the evaluative priming effect is unaffected by a mental load (e.g., remembering a series of digits during performance of the evaluative priming task), suggesting that evaluative processing is not dependent on the availability of cognitive resources (Hermans, Crombez, & Eelen, 2000).

However, despite numerous studies attesting to the unconditional, automatic nature of evaluative processing (see Klauer & Musch, 2003), Spruyt and colleagues demonstrated that the evaluative tone of a prime stimulus is processed only under conditions that maximize selective attention for the evaluative stimulus dimension (Everaert, Spruyt, & De Houwer, 2013; Spruyt, De Houwer, & Hermans, 2009; Spruyt, De Houwer, Hermans, & Eelen, 2007; Spruyt, Klauer, Gast, De Schryver, & De Houwer, 2015; Spruyt & Tibboel, 2015; Spruyt, 2014). For example, in a seminal paper, Spruyt et al. (2009) used a pronunciation task to examine the influence of FSAA on the occurrence of the evaluative priming effect. Because participants are asked to name target stimuli in the pronunciation task, this task does not induce a particular semantic processing mindset. In this sense, the pronunciation task is semantically neutral. Pronunciation trials were embedded in a context of either evaluative or non-evaluative semantic categorization trials. More specifically, participants were asked to categorize target stimuli according to their valence (i.e. positive or negative) or animacy (i.e., animal or object) and were thus encouraged to assign selective attention to evaluative or non-evaluative semantic stimulus features, respectively. Results revealed an evaluative but no non-evaluative priming effect on the pronunciation trials if participants assigned selective attention to evaluative features on the categorization trials. In contrast, a non-evaluative priming effect was found if participants were encouraged to assign selective attention to non-evaluative semantic stimulus features. In line with these findings, other markers of automatic evaluative stimulus processing have also been found to depend on the extent to which attention is assigned to the evaluative stimulus dimension, including the dot probe effect (Everaert et al., 2013), the emotional stroop effect (Everaert et al., 2013), and the P3a evoked by unexpected emotional stimuli (Everaert, Spruyt, Rossi, Pourtois, & De Houwer, 2013). Moreover, similar effects have been found in the domain of non-evaluative semantic processing. In three ERP studies, Kiefer and Martens (2010) showed a masked semantic priming effect (N400) if participants were encouraged to assign attention to (non-evaluative) semantic stimulus features. However, the masked semantic priming effect was abolished if participants were encouraged to assign attention to perceptual stimulus features (for related findings see also Ansorge, Kunde, & Kiefer, 2014; Kiefer & Brendel, 2006; Kiefer, 2012; Martens & Kiefer, 2009).

The modulation of evaluative processing by FSAA is readily explained by the FSAA framework developed by Spruyt and colleagues (Everaert et al., 2013; Spruyt et al., 2009, 2007). Drawing on the Generalized Context Model (GCM) of classification (Nosofsky & Palmeri, 1997; Nosofsky, 1984, 1986; see also Medin & Schaffer, 1978) the FSAA framework states that stimulus dimensions are processed only if and to the extent that they are selectively attended to. In addition, selective attention assignment is assumed to be dependent upon current goals and task demands. The FSAA framework can be conceptualized by representing stimuli in a multidimensional space. Each dimension of this space corresponds to a specific stimulus feature and the structure of the space depends on the focus of selective attention. More specifically, selective attention assignment stretches the space along the attended dimension whereas the space along unattended dimensions shrinks (Nosofsky & Palmeri, 1997; see also Carrol & Wish, 1974; Medin & Schaffer, 1978; Medin, 1983; Nosofsky, 1984; Reed, 1972; Tversky, 1977). As a result, the saliency of the attended dimension will increase. Differences along the attended dimension become more apparent whereas differences along unattended dimensions become less salient. This principle is illustrated in Figure 1 taken from Everaert (2012).

Direct support for the FSAA framework was provided in a unpublished study conducted in our lab (Everaert, 2012) in which participants were asked to categorize computer-generated faces either in terms of emotional expressions (i.e., the emotion group) or in terms of age (i.e., the age group). Intermixed with these categorization trials, participants were presented with similarity judgments

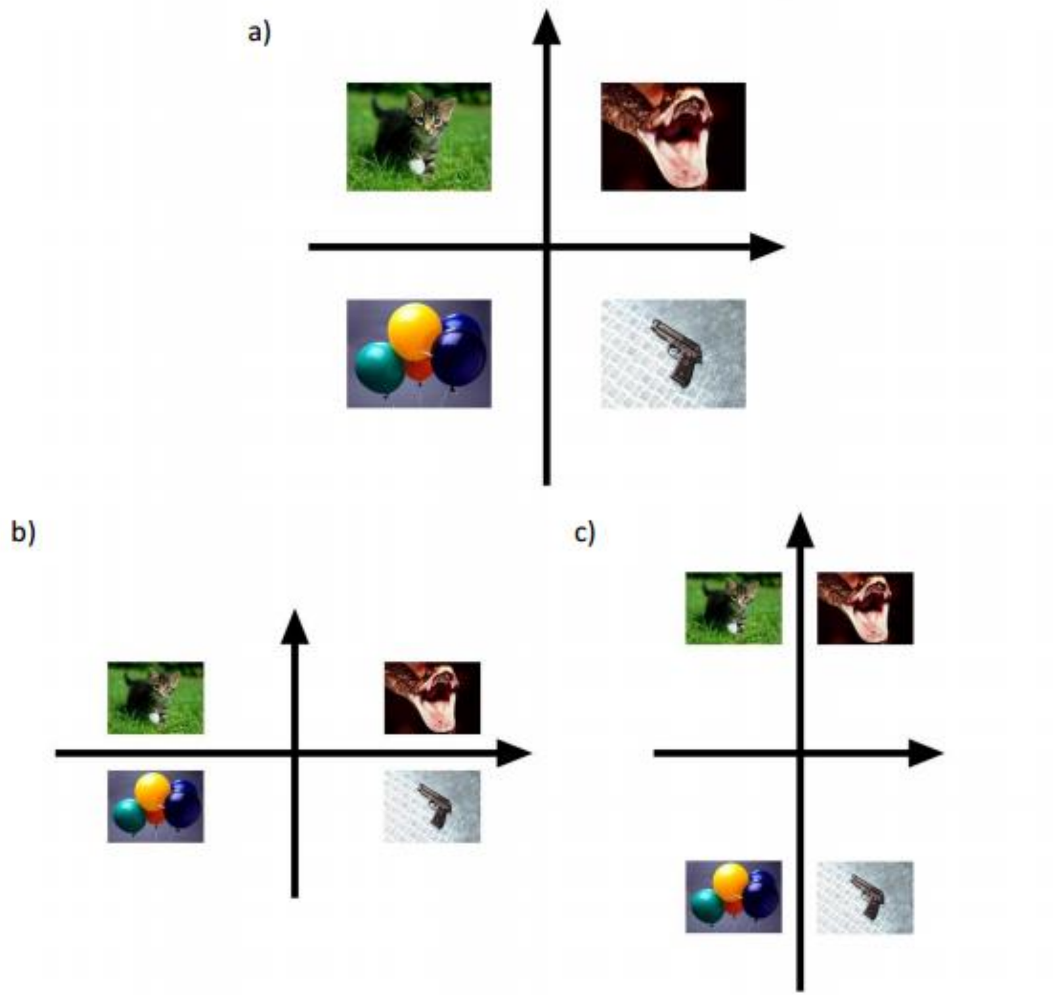

Figure 1. Hypothetical example of the multidimensional representation of feature-specific attention allocation. Each section shows a two-dimensional space in which the horizontal axis reflects the evaluative stimulus dimension whereas the vertical axis reflects the animacy dimension. Figures are stimuli taken from Spruyt et al. (2007). Panel A depicts the two-dimensional space under the assumption that attention is distributed equally across the two stimulus dimensions. Panel B shows the effect of feature-specific attention allocation towards the evaluative stimulus dimension. Panel C shows the effect of feature-specific attention allocation towards the animacy dimension.

trials. During these similarity judgment trials, participants were asked to judge the similarity between pairs of faces. Using INDSCAL, a multidimensional scaling approach, these similarity judgment ratings were transformed into attention weights representing the amount of attention assigned to different stimulus features by a given individual. It was shown that the emotion group and the age group assigned different weights to the two stimulus features. Whereas the emotion group assigned greater weight to the emotion dimension as compared to the age dimensions, the age group assigned greater weight to the age dimension as compared to the emotion dimension. It should be noted that the FSAA framework is not limited to stimuli with an evaluative tone but generalizes to non-evaluative stimuli (e.g., Nosofsky, 1986).

IMPLICATIONS OF THE FEATURE-SPECIFIC ATTENTION ALLOCATION FRAMEWORK FOR THE ACQUISITION, MODIFICATION, AND MEASUREMENT OF LIKES AND DISLIKES: AN OVERVIEW OF THE DISSERTATION

Given that FSAA impacts automatic evaluative processing, the question arises whether FSAA can also modulate the acquisition of likes and dislikes, their measurement, and the degree to which they can be changed. Interestingly, Gast and Rothermund (2011) provided evidence that the acquisition of likes and dislikes can be moderated by FSAA. They examined the influence of attentional focus on the occurrence of the evaluative conditioning (i.e., EC) effect. In a typical EC study, a neutral stimulus is paired with a stimulus that has a clear positive or negative meaning. The EC effect refers to the shift in valence of the initial neutral stimulus (i.e., Conditioned Stimulus or CS) toward the valence of the positive or negative stimulus (i.e., Unconditioned Stimulus or US) with which it was jointly presented. This phenomenon has now been replicated in a large number of studies (for a review, see Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010). In one seminal study, Gast and Rothermund (2011) intermixed EC trials with categorization trials in which participants were asked to judge the CS-US pair based on their evaluative features or a non-evaluative semantic feature. Thus, participants were encouraged to selectively attend either to evaluative or non-evaluative stimulus features, respectively. Results revealed an EC effect if participants allocated selective attention to evaluative features during the categorization trials. However, if participants assigned attention to non-evaluative semantic features, the EC effect was absent. This finding suggests that a person will learn about the evaluative tone of a new stimulus only if the person is in a evaluative mindset. Similar findings have been reported in the domain of classical conditioning. Olson, Kendrick, & Fazio (2009) showed that conditioning can be observed in the non-evaluative domain (e.g. size), if selective attention is allocated to these non-evaluative stimulus features during the conditioning procedure. Interestingly, recent studies of Spruyt et al. (2015) suggest that FSAA not only modulates the acquisition of likes and dislikes but also the generalization of recently acquired likes and dislikes to novel stimuli. In three EC studies, participants were presented with various CSs that could be classified in two subordinate categories (e.g., young men versus old women) based on two stimulus dimensions (i.e., age and gender). Either during (i.e., Experiment 1 and 2) or before (i.e., Experiment 3) the EC phase, participants were encouraged to assign attention to one of these stimulus dimensions. During a subsequent generalization test, novel stimuli were evaluated in a manner that was consistent with the acquired liking of those CSs that were similar in terms of the stimulus dimension that was selectively attended to during the evaluative conditioning phase.

While Gast and Rothermund (2011) provided empirical evidence for the hypothesis that the acquisition of likes and dislikes is modulated by FSAA, the moderating role of FSAA on the measurement and modification of likes and dislikes has not been tested empirically. Therefore, in a first line of research of this project, we examined the modulating role of FSAA on the measurement of spontaneous likes and dislikes. We hypothesized that a higher predictive validity would be found for implicit attitude measurement techniques in which participants are not encouraged to assign attention to evaluative semantic stimulus features relative to implicit measurement techniques in which participants are encouraged to attend to evaluative stimulus features.

This hypothesis was based on the assumption that individuals will not only differ in the extremity or direction of their likes and dislikes but are also likely to differ in the extent to which selective attention assignment is required to automatically process the evaluative tone of attitude objects. For instance, spider-fearful participants might not only evaluate spiders as more negative than control participants (Ellwart, Rinck, & Becker, 2006), they might also show negative reactions to spiders even though current task-demands require them to assign selective attention to non-evaluative stimulus features. In line with this idea, numerous studies have shown that FSAA is governed by personal goals. FSAA can be driven by explicit task instructions (Everaert, 2012; Spruyt et al., 2009) or more subtle aspects of the task design (Everaert, Spruyt, & De Houwer, 2011). In addition, one can expect stable inter-individual differences in the extent to which selective attention is assigned to certain stimulus dimensions. In line with this reasoning, it has been observed that socially anxious individuals show a greater emotional Stroop effect for words related to social threat relative to physical threat whereas individuals experiencing anxiety concerning physical dangers show a greater emotional Stroop effect for words related to physical threat compared to social threat (Mogg, Mathews, & Weinman, 1989). In addition, multidimensional scaling studies have shown that individuals with bulimic symptoms assign greater weight to stimulus dimensions reflecting body size than individuals without bulimic symptoms (Viken, Treat, Nosofsky, McFall, & Palmeri, 2002). Likewise, Cavanagh and Davey (2001) demonstrated that spider-fearful participants assign greater weight to the evaluative stimulus dimension than fearless participants. Fazio and Dunton (1997) observed a correlation between spontaneous racial bias (as measured with the evaluative priming paradigm) and the degree of attention assigned to a race-related stimulus dimension (indexed using the INDSCAL algorithm). In accordance with the FSAA framework, these findings suggest that attitude objects might indeed trigger evaluative processing in an automatic fashion, provided that their personal relevance is sufficiently high. It can thus be hypothesized that inter-individual differences in implicit evaluation will be most outspoken under conditions that promote selective attention for a non-evaluative stimulus feature. This idea is related to theories concerning the importance of attitude accessibility, i.e., the strength of the object-evaluation association (Fazio, 2001;

Fazio, 1990). Likes and dislikes high in accessibility are assumed to be easily activated upon the mere perception of a stimulus whereas likes and dislikes low in accessibility are less likely to become activated from memory upon the perception of a stimulus. Interestingly, it has been shown that highly accessible likes and dislikes are usually personally relevant (Bizer & Krosnick, 2001; Krosnick, 1989). Based on the FSAA framework, it could be suggested that evaluative features of highly accessible likes and dislikes are processed in an automatic and unconditional fashion because the attitude-relevant stimulus features are chronically attended to.

In **Chapter 2**, we discuss two studies designed to investigate the moderating role of FSAA on the measurement of spontaneous likes and dislikes. In the first study, we presented participants with a picture-picture naming task in which targets were to be named on 50% of the trials. This task was used to measure implicit likes and dislikes toward fruit and candy. On the remaining 50 % of the trials, a green or purple rectangle surrounding the target was presented, indicating that a categorization task was to be performed. Participants were either asked to categorize stimuli according to valence (i.e., the evaluative condition), animacy (i.e., the semantic condition), or the color of the surrounding rectangle (i.e., non-semantic condition) and were thus encouraged to assign selective attention to evaluative stimulus features, non-evaluative semantic features or perceptual features, respectively. At the end of the experiment, participants were presented with some pieces of fruit and candy as a little thank-you present and were informed that they could choose one item to take home. We hypothesized to observe higher predictive validity in the semantic and non-semantic condition relative to the evaluative condition.

In the second study, participants performed both an evaluative categorization task and a semantic categorization task. Thus, participants were encouraged to assign attention to either evaluative or non-evaluative semantic stimulus features, respectively. Both tasks were designed to measure spontaneous likes and dislikes of spiders. At the end of the experiment, participants performed a behavioral assessment task (i.e., BAT) to assess how close participants were willing to approach a living spider. Similar to Experiment 1, we expected the predictive validity of semantic categorization task to be higher than the predictive validity of the evaluative categorization task. Importantly, the within-subjects design of Experiment 2 also enabled us to examine a second hypothesis. We predicted that spider-fearful participants will process the evaluative features of attitude-relevant stimuli independent of whether selective attention is focused on evaluative features or non-evaluative semantic stimulus features. Therefore, we predicted no difference between inter-individual difference scores obtained with the evaluative categorization task and inter-individual difference scores obtained with the semantic categorization task for spider fearful-participants. In contrast, for participants who were not afraid of spiders, we expected that the evaluative features of spider-related stimuli would be processed only if selective attention for the evaluative stimulus dimension is maximized (i.e., the evaluative categorization task). Consequently, we expected a significant difference between inter-individual difference scores obtained in the evaluative categorization task and the semantic categorization task in control participants.

One could argue that the participants tested in Chapter 2 were students for whom the tested attitude objects were not necessarily personally relevant. To meet this concern, we decided to test our hypothesis about the impact of FSAA and relevance on the predictive validity of implicit attitude measures using a meta-analytical approach. In **Chapter 3**, we describe a meta-analysis in which we compare the predictive validity of implicit measurement techniques that maximize selective attention assignment towards non-evaluative semantic features relative to implicit measurement techniques that maximize selective attention assignment towards evaluative stimulus features. Moreover, we empirically tested whether the effect of FSAA is modulated by the personal relevance of the attitude object. We did not expect relevance to be a significant moderator of predictive validity in implicit attitude measurements that maximize selective attention for evaluative stimulus features. However, we did expect that personal relevance would influence the effect of FSAA on the predictive validity of implicit measures if participants are not encouraged to assign selective attention to the evaluative stimulus dimension. Finally, we also considered various other moderators that could influence the relationship between implicit likes and dislikes and behavior (e.g., the nature of the response task, the nature of the behavioral outcome, the domain of study).

In a second line of research of the current project, we examined the impact of FSAA on the extinction of likes and dislikes. Research has shown that likes and dislikes are highly resistant to extinction (Craske, 1999; Hallion & Ruscio, 2011; Krypotos, Arnaudova, Effting, Kindt, & Beckers, 2015), potentially because the spontaneous evaluative response that is evoked by an attitude object consistently reaffirms the evaluative tone of this object (Lewicki, Hill, & Czyzewska, 1992; Martin & Levey, 1978). Based on the FSAA framework, it can be hypothesized that an encounter with an attitude object is less likely to result in an evaluative response if attention is directed away from the evaluative stimulus dimension, and hence, that the extinction rate of likes and dislikes must be contingent upon the degree to which attention is assigned to other, non-evaluative (semantic) stimulus features.

In **Chapter 4**, the evaluative conditioning paradigm was used to examine the modulating role of FSAA on the extinction of recently acquired likes and dislikes. In a first experiment, CSs were abstract Gabor patches that varied along two orthogonal, perceptual dimensions (i.e., orientation and spatial frequency). We manipulated the extent to which participants assigned selective attention to one of these dimensions, both during the acquisition phase and the extinction phase of the experiment. During the acquisition phase, one of these perceptual features was predictive of the valence of the USs and participants were asked to categorize the CSs in terms of this feature. Next, participants performed an extinction phase in which CSs were presented alone. In the extinction phase, participants were encouraged to assign selective attention to the valence of the CSs (i.e., the evaluative condition), the perceptual stimulus feature that was correlated with valence in the acquisition phase (i.e., the relevant condition) or the stimulus feature that was unrelated to valence in the acquisition phase (i.e., the irrelevant condition). To measure the effects of our FSAA-manipulation, extinction was assessed both post-acquisition and post-extinction through a measure of implicit evaluation (i.e., the Affect Misattribution Paradigm; Payne, Cheng, Govorun, & Stewart, 2005) and explicit evaluative ratings. Extinction was expected to occur in the irrelevant condition but not in the evaluative condition. Because participants in the relevant condition were encouraged to assign attention to a stimulus feature that was related to valence, we expected extinction to be less pronounced in this condition relative to the irrelevant condition.

In **Chapter 5**, the idea that FSAA impacts the extinction rate of likes and dislikes was again examined using pre-existing attitudes rather than attitudes created in the laboratory by means of an evaluative conditioning procedure. Participants were asked to categorize a series of real-life pictures according to either their valence (i.e., evaluative condition) or their animacy (i.e., semantic condition). Importantly, one fourth of our stimuli depicted spiders, thereby allowing for a test of the hypothesis that a manipulation of FSAA can be exploited as a means to reduce dislikes toward fearful stimuli. Following the exposure phase, participants were asked to complete the Affect Misattribution Procedure (i.e., the AMP), give evaluative ratings, and complete a stimulus-response compatibility (i.e., SRC) task. It was expected that each of these measures would reveal less negative evaluations towards spiders in the Semantic Condition as compared to the Evaluative Condition. In addition, we included (novel) exemplars that were not presented during the exposure phase to examine the extent to which the impact of our manipulation would generalize to novel stimuli.

In **Chapter 6**, we will briefly summarize the main findings of the dissertation project. In addition, we will discuss how our findings can serve to improve implicit measurement techniques of likes and dislikes. Finally, we will address the implications of our findings with regard to the modification of likes and dislikes as well as their therapeutic value.

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CHAPTER 2

THE MODERATING ROLE OF FEATURE-SPECIFIC ATTENTION ALLOCATION ON PREDICTIVE VALIDITY IN THE EVALUATIVE PRIMING TASK.

In two experiments, we examined the impact of feature-specific attention allocation on the predictive validity of implicit attitude scores obtained with the evaluative priming paradigm. In Experiment 1, participants performed a picture-picture naming task. Naming trials were intermixed with categorization trials in which participants were encouraged to selectively attend to either evaluative stimulus features, non-evaluative semantic stimulus features, or perceptual stimulus features. In Experiment 2, participants performed both an evaluative categorization task and a semantic categorization task and were thus encouraged to assign selective attention to evaluative features and non-evaluative semantic features, respectively. Even though the experimental design of these experiments was quite different, their pattern of results were remarkably consistent. First, in both experiments, inter-individual difference scores obtained under conditions that maximized selective attention to evaluative stimulus features were unrelated to outcome measures. Second, we did observe a significant relationship between outcome measures and inter-individuals difference scores obtained under conditions that encouraged selective attention to non-evaluative semantic features. Interestingly, in both experiments, this relationship was opposite to what was predicted. That is, implicit likes were associated with avoidance behavior whereas implicit dislikes were associated *with approach behavior. Potential underlying mechanisms of this unexpected finding are discussed.*

INTRODUCTION

Ever since Allport (1935) claimed that 'attitudes drive behavior', the measurement of likes and dislikes has become a major field of interest in the psychological research community. To capture those likes and dislikes, participants are typically asked to self-asses and report their likes and dislikes. These so-called 'explicit attitude measures' were soon criticized, however, as they allow for intentional misreports and capitalize on the ability of participants to self-assess their own likes and dislikes by means of introspection. Moreover, it has become clear that behavior is not only determined by explicit likes and dislikes but also by automatic evaluative reactions towards stimuli (hereafter referred to as implicit likes and dislikes).

For these reasons, behavioral scientists have developed a class of measurement techniques that allow for the measurement of likes and dislikes in an indirect way, that is without the need to rely on self-reports by the respondent. As an example, consider the evaluative priming task developed by Fazio, Sanbonmatsu, Powell, and Kardes (1986). In this task, participants are asked to evaluate a series of target stimuli as positive or negative. Each target stimulus presentation is preceded by the presentation of a task-irrelevant prime stimulus (e.g., a picture of a spider). Performance is generally better when the evaluative tone of the prime stimulus and the target stimulus is congruent (e.g. spider-unhappy) as compared to when the evaluative tone of the prime-target pair is incongruent (e.g., spider-happy). This phenomenon is usually referred to as the evaluative priming effect and can be exploited as a means to capture implicit likes and dislikes towards the prime stimuli. For example, Fazio, Jackson, Dunton, and Williams (1995) adapted the evaluative priming task to measure inter-individual differences in racial prejudice. Faces of black and white individuals were presented as primes followed by positive and negative target adjectives. The extent to which racial information conveyed by the primes facilitated or interfered with target responding was then used as a measure of the implicit likes and dislikes towards black and white individuals. Importantly, implicit likes and dislikes as measured by the evaluative priming task proved to be predictive of prejudiced behavior (i.e., duration of eye contact with a black person). This seminal finding demonstrated that the evaluative priming task can be used as a valid predictor of behavior and triggered a surge of studies examining the predictive validity of implicit measures in general. However, while demonstrations of the predictive validity of the evaluative priming paradigm continued to appear in the literature (see for example Degner, Wentura, Gniewosz, & Noack, 2007; Dovidio, Kawakami, & Gaertner, 2002; Frings & Wentura, 2003), the usefulness of the evaluative priming paradigm is not unequivocal. For example, it has been shown that the reliability of evaluative priming scores is (often) disappointingly low (Bosson, Swann, & Pennebaker, 2000; Fazio & Olson, 2003, but see Vandromme, Hermans, & Spruyt, 2011). In addition, a recent meta-analysis conducted by Cameron, Brown-Iannuzzi, and Payne (2012) revealed that, taken together, the relationship between attitude-relevant behavior and inter-individual difference scores obtained with the evaluative priming paradigm is relatively modest (i.e., r =.28). Finally, some studies were unable to replicate the relation between inter-individual difference scores obtained with the evaluative priming paradigm and behavior. For example, Spruyt, Hermans, De Houwer, Vandekerckhove, and Eelen (2007) were unable to predict the choice between a piece of fruit or candy at the end of an experimental session on the basis of the implicit likes and dislikes towards fruit and candy as measured by the evaluative priming task. Similarly, Falk, Heine, Takemura, Zhang, and Hsu (2015) observed that implicit self-esteem as measured by the evaluative priming paradigm was unrelated to a wide range of behavioral outcomes (e.g., friend rating of self-competence, friend rating of self-liking, friend rating of self-esteem, or the ambiguous statements task). To account for these inconsistent findings, it has been argued that the (predictive) validity of implicit measures in general and the evaluative priming paradigm in particular is dependent on moderating factors. Examples of such moderating factors include the motivation to control prejudice (Olson & Fazio, 2004), whether attitude measurement were administered in a stereotypically positive context such as a basketball arena or a stereotypically negative context such as a slum (Wittenbrink, Judd, & Park, 2001), and whether the examined stereotypical group seems homogenous or not (Lambert, Payne, Ramsey, & Shaffer, 2005).

The aim of the present study was to examine the influence of one other potential moderator of predictive validity in the evaluative priming paradigm, i.e., feature-specific attention allocation (hereafter referred to as FSAA). Whilst it is typically argued that evaluative stimulus information is processed in an unconditional and automatic fashion (e.g., Arnold, 1960; Barlett, 1932; Lazarus, 1966; Wundt, 1907; Zajonc, 1980), recent evidence suggests that automatic stimulus evaluation is highly dependent on the degree to which attention is assigned to the evaluative stimulus dimension. The evaluative priming effect, for example, is typically obtained only if participants are encouraged to process all incoming stimulus information in an evaluative manner (e.g., Everaert, Spruyt, & De Houwer, 2016; Spruyt, De Houwer, & Hermans, 2009; Spruyt, De Houwer, Everaert, & Hermans, 2012; Spruyt, Klauer, Gast, De Schryver, & De Houwer, 2015; Spruyt, Hermans, De Houwer, Vandromme, & Eelen, 2007; Spruyt & Tibboel, 2015; Spruyt, 2014, but see Becker, Klauer, & Spruyt, 2016). Likewise, FSAA has been shown to impact other markers of automatic evaluative processing, including the dot probe effect (Everaert, Spruyt, & De Houwer, 2013), the emotional Stroop effect (Everaert, et al., 2013), and neuropsychological markers of implicit evaluation (Everaert, Spruyt, Rossi, Pourtois, & De Houwer, 2013). Similar effects have also been shown in the domain of automatic non-evaluative semantic processing (e.g., Kiefer & Brendel, 2006; Kiefer & Martens, 2010; Kiefer, 2012; Martens & Kiefer, 2009).

The observation that automatic evaluative stimulus processing is dependent upon FSAA has huge implications for the measurement and predictive validity of implicit attitude measures because individuals are likely to differ not only in the extremity and direction of their implicit likes and dislikes but also in the extent to which selective attention assignment is required to process the valence of attitude objects in an automatic fashion. For instance, abstaining smokers might not only have more favorable automatic affective reactions towards cigarettes than non-smokers (e.g., Spruyt, Lemaigre, et al., 2015) but might also exhibit positive reactions to cigarettes even when current task-demands require them to selectively attend to non-evaluative stimulus information. It can thus be hypothesized that inter-individual differences in implicit evaluation will be most outspoken under conditions that promote selective attention for a non-evaluative stimulus feature.

Spruyt, Hermans, De Houwer, Vandekerckhove, and Eelen (2007) provided preliminary evidence for the hypothesis that predictive validity of the evaluative priming task depends on FSAA. They showed that inter-individual difference scores obtained with an implicit measure in which participants were not encouraged to engage in evaluative stimulus processing (i.e., the picture-picture naming task) predicted behavior relatively well as compared to inter-individual difference scores obtained with implicit attitude measures in which attention to evaluative stimulus features was maximized (i.e., evaluative decision task and the implicit association test).

The hypothesis that FSAA moderates the predictive validity of the evaluative priming task was tested in two experiments. In Experiment 1, implicit likes and dislikes towards fruit and candy were examined using the picture-picture naming task. We deliberately opted for the picture-picture naming task (hereafter referred to as PPNT) as this task does not induce a particular semantic processing mindset as participants are simply asked to name a series of target pictures. Participants were presented with target pictures that varied on two separable dimensions, i.e., valence and animacy. Target pictures were preceded by prime pictures depicting either fruit or candy. Target pictures were to be named as fast as possible unless they were surrounded by a colored rectangle. In this case, participants were asked to categorize target the pictures either according to their valence (i.e., evaluative condition), animacy (i.e., semantic condition), or the color of the surrounding rectangle (i.e., non-semantic condition). Thus, participants were encouraged to assign selective attention to the evaluative stimulus dimension, to a non-evaluative semantic stimulus dimension, or to a non-semantic, perceptual stimulus dimension, respectively.

Note that, although participants were encouraged to adopt a non-evaluative processing mindset in both the semantic and the non-semantic condition, these two conditions differ in a significant way. Whereas participants in the non-semantic condition were encouraged to assign attention to the perceptual features of the target stimuli, participants in the semantic condition were encouraged to process non-evaluative, semantic stimulus information while ignoring the evaluative tone of these stimuli. At the end of the experiment, participants were presented with the choice between a piece of fruit or a candy bar. We expected that the relationship between the evaluative priming scores and the choice behavior would be dependent upon the degree to which participants assigned attention to the evaluative stimulus dimension. More specifically, we expected predictive validity of the evaluative priming scores to be higher in the non-semantic semantic condition and semantic condition compared to the evaluative condition. We had two distinct a priori hypotheses about the difference in predictive validity between the non-semantic condition and semantic condition. On the one hand, it is possible that inter-individual differences in automatic evaluative processing will be especially pronounced under conditions that maximize selective attention to non-evaluative semantic features as compared to conditions that maximize attention to semantically neutral stimulus features. In this case, we would expect higher predictive validity in the semantic condition as compared to the non-semantic condition. On the other hand, inter-individual differences in automatic evaluative processing might be suppressed if participants are encouraged to attend to a non-evaluative semantic stimulus feature. Consequently, we would expect higher predictive validity in the non-semantic condition as compared to the semantic condition.

EXPERIMENT 1

Method

Participants

One hundred and twenty-three participants (32 men, 91 women) at Ghent University with a mean age of 21.76 years (*SD* = 2.80 years) took part in the experiment in exchange for a payment of 4 €. In total, nine participants were excluded from the analysis. Two participants were excluded because more than one fourth of their data points (i.e., 26.6 % and 28.1 %) had to be removed due to errors, voice key failures, outliers, or responses given outside of the response window. Four other participants were excluded because their mean reaction time (i.e., 938 ms, 1011 ms, 1013 ms, and 1018 ms) exceeded the outlier criterion of 2.5 standard deviation above the sample mean (i.e., 682 ms). One other participant was excluded as he accidently tampered with the settings of the external voice key device in the middle of the experiment. Finally, two participants were excluded because they clearly stated that the evaluative nature of one of the target pictures was opposite to what was intended by the researcher. One of these participants had an allergy to latex, so she perceived a target picture depicting balloons as very negative. The other participant had a fascination with weapons and perceived a target picture depicting a gun as very positive. Note, however, that results were not contingent upon the inclusion or exclusion of these participants. Participants were randomly assigned to either the evaluative (*n* = 38), the semantic (*n* = 36), or the non-semantic (*n* = 40) condition.

Materials

On the basis of normative data collected by Spruyt, Hermans, De Houwer, & Eelen (2002), eight stimuli were selected to be used as targets. Several of these pictures originated from the International Affective Picture System (i.e., IAPS; Lang, Bradley, & Cuthbert, 1999). Targets varied on two orthogonal dimensions, i.e., valence and animacy. On a scale ranging from very negative (-5) to very positive (+5), the mean evaluative ratings of the positive (*M* = 2.17, *SD* = 0.40) and negative (*M* = -1.99, *SD* = 0.40) target pictures differed significantly, $F(1, 6) = 103.09$, $p < 0.001$. Mean evaluative ratings did not differ between pictures depicting objects (*M* = -0.20, *SD* = 2.47) and pictures depicting living creatures (*M* = 0.38, *SD* = 2.43), $F < 1$. All target pictures could be named with a single word (see Appendix). Two pictures depicting fruit (i.e., an orange and an apple), two pictures depicting candy (i.e., a Snicker and a Mars) and four pictures of neutral geometric stimuli (i.e., a square, a trapezium, a hexagon, and a diamond) were selected to serve as primes.

The experiment was programmed in Affect 4.0 (Spruyt, Clarysse, Vansteenwegen, Baeyens, & Hermans, 2010). Response latencies were registered with an external voice key connected to the parallel port of an Intel Core Duo E8600 computer. All stimuli were presented against the black background of a 19 inch computer monitor (100 Hz) and were 512 pixels wide and 384 pixels high.

Procedure

The experiment consisted of three practice phases followed by the experimental task. In the first practice phase, each target picture was presented together with its corresponding name. Participants were instructed to look attentively at the pictures and to remember the corresponding names as they would need to use these words to name the pictures correctly during the naming task. Each trial started with a 500-ms fixation cross followed by a blank screen for 750 ms. Next, the target picture was presented until participants pressed the spacebar of the keyboard or 3000 ms elapsed. The next trial was initiated after an ITI that varied randomly between 500 and 1500 ms. Note, that the same ITI was used throughout the entire study. The second practice phase was equal to the first practice phase with the exception that the eight target pictures were now presented without the corresponding names written underneath them. Participants were instructed to name the target words as fast and accurately as possible by using the names learned during the first practice phase. Pictures were presented until participants gave a response or 4000 ms elapsed. Incorrect responses were corrected by the experimenter. The experimenter coded whether the microphone was triggered accurately and whether the participant had made a correct response. During the third practice phase, each target picture was presented twice (i.e., 16 trials). In half of these trials, participants were instructed to name the targets (i.e., naming trials). In the other half of the trials, targets were surrounded by a colored rectangle (i.e., induction trials). Participants were instructed to categorize the target if it was surrounded by a colored rectangle and to name the target otherwise. Each target picture was presented once on a naming trial and once on an induction trial. The color of the rectangle was randomized and could either be green or purple. In the evaluative condition, participants were instructed to categorize the targets according to their valence (i.e., positive or negative). Participants in the semantic condition were instructed to categorize targets according to their animacy (i.e., alive or object). Finally, in the non-semantic condition, participants were instructed to categorize targets according to the color of the rectangle (i.e., green or purple). Targets were presented on the screen until a response was detected or 4000 ms elapsed. If an erroneous response was made, the experimenter corrected the participant.

The procedure of the experimental task was identical to that of the third practice phase except that the target pictures were now preceded by prime pictures. Participants were informed that the prime pictures were irrelevant for the task at hand and could be ignored. The task consisted of 192 trials divided into two blocks of 96 trials each. In between blocks participants could take a small break. Within each block, each prime was combined four times with each target picture. Half of the trials required a naming response (i.e., naming trials) whereas a categorization response was required on the other trials (i.e., induction trials). Each trial started with the presentation of a fixation cross in the center of the computer screen for 500 ms, followed by a 500 ms blank screen. Then, a prime picture was presented for 200 ms. After a delay of 50 ms, the target picture was presented, resulting in a stimulus onset asynchrony of 250 ms. The target picture remained on the screen until the participant gave a response or until 4000 ms elapsed.

At the end of the experiment, participants were presented with a Fun-size Snickers candy bar, a Fun-size Mars candy bar, an apple, and an orange. Participants were informed that they could choose one of these objects to take home as a little thank-you present. We did not include self-reports of likes and

Table 1

Number of participants choosing fruit or candy as a function of condition (% between brackets)

	Choice behavior	
Condition	Candy	Fruit
Evaluative	12 (31.58)	26 (68.42)
Non-Semantic	20 (50.00)	20 (50.00)
Semantic	13 (36.11)	23 (63.89)

dislikes towards fruit and candy because we wanted to avoid interference between these explicit reports and choice behavior at the end of the experiment, and vice versa.

Results

Data Reduction and analysis.

Only the data of the naming trials were analyzed. Neutral priming trials (neutral/negative and neutral/positive trials) were considered filler trials and were not included in the analysis. Furthermore, we excluded data from trials on which the voice key was improperly activated (4.53 %), an incorrect response was given (1.80 % in the evaluative condition, 0.66 % in the non-semantic condition, and 1.73 % in the semantic condition), or a response was given after the response deadline (0.04 % in the evaluative condition, 0.16 % in the non-semantic condition, and 0.12 % in the semantic condition). Finally, we discarded all response latencies that deviated more than 2.5 standard deviations from a participant's mean latency in a particular condition (see Ratcliff, 1993; 2.63 % in the evaluative condition, 2.44 % in the non-semantic condition, and 2.80 % in the semantic condition).

For each individual participant, two difference scores were computed.

First, we subtracted the mean response latencies observed on trials consisting of a fruit-related prime and a positive target from the mean response latencies observed on trials consisting of a fruit-related prime and a negative target. Second, we subtracted the mean response latencies observed on trials consisting of candy-related prime and a positive target from the mean response latencies observed on trials consisting of a candy-related prime and negative target

Finally, to obtain a relative preference score, the second difference score was subtracted from the first difference score. Hence, positive priming scores indicate a preference for fruit over candy. Because participants made a dichotomous choice between fruit and candy, logistic regression analyses were performed to investigate the predictive validity of the implicit attitude measure in each condition.

Descriptive statistics

As can be seen in Table 1, participants were more inclined to choose a piece of fruit instead of a piece of candy as a take-home present. Choice behavior did not differ between conditions, $X^2(2) = 3.01$, $p = 0.22$. Overall, the implicit preference score was negative (i.e., -16 ms) and reliable, $t(113) = -2.21$, $p < .05$, $d = -0.21$. In sum, our sample showed an overall implicit preference for candy over fruit. Inter- individual difference scores were not dependent upon the condition factor, $F < 1$.

The relationship between implicit attitudes and behavior

The individual choices between fruit (coded as 1) and candy (coded as 0) were regressed onto the condition factor (evaluative condition vs. non-semantic condition vs. semantic condition) and the inter-individual difference scores. In isolation, neither the condition factor, $X^2(2) = 2.80$, $p = .25$, nor the inter-individual difference scores, $X^2(1) = 1.29$, $p = .26$, were a reliable predictor of behavioral choice at the end of the experiment. However, as anticipated, the interaction between the two predictors did reach significance, $X^2(2) = 6.10$, $p < 0.05$. As expected, inter-individual difference scores as measured by the picture-picture naming task were unrelated to behavior in in the evaluative condition, $X^2(1) = 0.17$, $p = .68$, *odds ratio (unit change)* = 1.00. In contrast, the inter-individual difference scores obtained in the semantic condition did prove to be related to behavior, $X^2(1) = 5.06$, $p < .05$, *odds ratio(unit change)* = 0.99. Surprisingly, however, this effect was in the opposite direction of what one would expect: the more an inter-individual difference score was indicative of a preference for fruit over candy, the higher the probability that participants chose for candy at the end of the experimental session. In the non-semantic condition, there was no relationship between the behavioral outcome measure and the inter-individual difference scores, $X^2(1) = 2.17$, $p = .14$, *odds ratio (unit change)* = 1.01

Discussion

Earlier studies revealed that the extent to which an individual engages in automatic evaluative processing in the absence of an explicit evaluative processing goal depends on the personal relevance of the attitude objects. The higher the personal relevance of a particular attitude object, the more likely that one will process its evaluative connotation. Accordingly, in line with the FSAA framework developed Spruyt and colleagues (Everaert, et al., 2013; Spruyt et al., 2007; 2009), one may predict that measures of implicit evaluation will be less indicative of behavioral outcomes if they are obtained under conditions that maximize selective attention for the evaluative stimulus dimension as compared to when they are obtained under conditions that require participants to focus selective attention on non-evaluative stimulus information. To test this hypothesis, we examined the impact of FSAA on the predictive validity of the PPNT, i.e., a variant of the evaluative priming task that does not induce a semantic processing mindset. Naming trials were intermixed with induction trials in which participants were encouraged to assign selective attention to evaluative features (i.e., evaluative condition), non-evaluative sematic stimulus features (i.e., semantic condition), or perceptual stimulus features (i.e., non-semantic condition). We expected that the predictive validity of the PPNT would be less pronounced in the evaluative condition relative to the semantic condition and the non-semantic condition. Two competing hypothesis could be postulated about the difference in predictive validity between the semantic condition and the non-semantic condition. First, if inter-individual differences are most outspoken under conditions that maximize selective attention to non-evaluative semantic features as compared to conditions that maximize attention to semantically neutral stimulus features, we would expect higher predictive validity in the semantic condition as compared to the non-semantic condition. Second, inter-individual differences in automatic evaluative processing might be abolished if participants are encouraged to process non-evaluative semantic stimulus features but be unaffected if participants are encouraged to process non-semantic stimulus features. In this case, we would expect higher predictive validity in the non-semantic condition compared to the semantic condition.

In line with our hypotheses, inter-individual difference scores obtained with the PPNT did not predict behavior in the evaluative condition. In contrast, the results obtained in the non-semantic condition were not in line with our expectations. Despite the fact that participants were not encouraged to assign attention to a non-evaluative stimulus dimension, no relationship was found between the inter-individual difference scores obtained in the PPNT and subsequent choice behavior. At least three different accounts can be proposed to explain this unexpected finding. First, it is possible that the evaluative tone of the primes was processed but simply did not influence the speed of target responding. According to a response-level account of evaluative processing, the evaluative priming effect occurs because the primes automatically activate response tendencies that interfere or facilitate responses to the target (Klauer, Roßnagel, & Musch, 1997; Rothermund & Wentura, 1998; Wentura, 2000). In the PPNT, such a response-level mechanism is unable to operate as each target stimulus requires a unique response. Therefore, if it is assumed that the evaluative priming effect can emerge only if there is overlap between the response set and the prime set, it is almost a trivial finding that no relationship was found between the outcome behavior and the evaluative priming scores. It must be emphasized, however, that in line with various other studies (e.g., De Houwer, Hermans, & Spruyt, 2001; De Houwer & Randell, 2004; Spruyt, et al., 2002), we did observe an overall preference for candy over fruit. This observation clearly shows that the PPNT did capture implicit evaluations and is therefore incompatible with the idea that evaluative priming effects can emerge only if there is dimensional overlap between the response and the prime set.

Second, one may simply argue that the semantic condition included a relatively large proportion of participants that did not entertain pronounced likes and dislikes towards fruit and candy. Given our reasoning concerning the importance of personal relevance, such a scenario would imply that the evaluative priming scores were driven by implicit evaluations for only a limited number of participants. The likelihood of such a scenario seems rather small, however, given that participants were assigned to different conditions at random.

Therefore, as a third and more likely explanation, one may argue that the requirement to engage in a shallow, non-semantic processing style hampered the semantic analysis of the primes in general. It is important to note that participants were unaware of the nature of the required response (i.e., color decision or picture naming) until the colored rectangle appeared around the target. Semantic information was thus task-irrelevant until the target in the naming trial was presented. One could thus argue that participants did not engage in semantic processing until the target in a naming trial was presented. However, such an explanation is at odds with studies showing processing of evaluative stimulus features even though participants were encouraged to assign attention to perceptual stimulus features (see for example Effting, Salemink, Verschuere, & Beckers, 2016; Peeters et al., 2013). Moreover, this account does not explain why inter-individual difference scores in our sample revealed a significant overall preference for candy over fruit. Indeed, a significant preference for either fruit or candy could have come about only if participants processed the evaluative features of prime stimuli. However, it should be noted that, although no significant difference in inter-individual difference scores between conditions was observed, the mean inter-individual difference score in the semantic condition was much smaller (i.e., - 9 ms) than the mean inter-individual difference scores in the non-semantic and evaluative condition (both -20 ms).

Finally, In accordance with our hypotheses, individual difference scores obtained in the semantic condition were reliable predictors of choice behavior. To our surprise, however, the nature of this relationship was opposite to our predictions. The more inter-individual difference scores revealed a preference of fruit over candy, the higher the probability that participants chose a candy bar instead of a piece of fruit to take home. Given the unexpected nature of this observation, we were initially reluctant to give much weight to this isolated finding. We did, however, decide to conduct a follow-up study in order to verify whether this observation was a chance finding. In addition, we wanted to address a number of shortcomings of Experiment 1.

First, given our reasoning concerning attitude importance, it seems key to examine implicit attitudes in participants for whom the tested attitude objects are of high personal relevance. One may question whether a study focusing on the implicit attitude towards fruit and candy in a random sample of (healthy) students meets this requirement (but see Spruyt, et al., 2007).

Second, in Experiment 1, the behavioral outcome measure was a one-shot, binary decision between a piece of candy or a piece of fruit. Such a measure might be very sensitive to contextual factors. In Experiment 1, for example, we observed that participants were more likely to choose a piece of candy rather than a piece of candy in the morning, $X^2(1) = 6.42$, $p < .05$. In Experiment 2, we opted for a measure that would be less sensitive to external influences in order to increase the reliability of the outcome measure.

Finally, in Experiment 1, we deliberately opted not to administer explicit attitude measures because we did not want these explicit reports to interfere with choice behavior at the end of the experiment and vice versa. As a result, we were unable to ascertain that participant held strong likes and dislikes towards fruit and candy. To remove this problem, we did include explicit measures in Experiment 2.

EXPERIMENT 2

In Experiment 2, we measured implicit likes and dislikes towards spiders in a group of spider-fearful participants and a control group. Participants performed two evaluative priming tasks designed to measure implicit likes and dislikes towards spiders. Importantly, participants were either asked to categorize target stimuli according to their evaluative features (i.e., evaluative categorization task) or according to their non-evaluative semantic features (i.e., semantic categorization task) and were thus encouraged to assign attention to evaluative stimulus features and non-evaluative semantic features, respectively. At the end of the experiment, participants performed a behavioral assessment task (i.e., BAT) to assess avoidance behavior towards a living spider. We expected that inter-individual difference scores obtained with the semantic categorization task would show a more pronounced relation with BAT performance than inter-individual difference score obtained with the evaluative categorization task. In addition, we expected that the degree to which the two tasks would produce similar inter-individual difference scores would be dependent upon the personal relevance of the prime stimuli. That is, we expected spontaneous evaluative processing of attitude objects to be independent of the requirement to focus attention on either non-evaluative or evaluative stimulus features in the spider-fearful group. In contrast, we expected that individuals in the control group would be more likely to process the evaluative features of the attitude objects while performing the evaluative categorization task as compared to the semantic categorization task. In sum, we expected to observe a reliable difference between inter-individual difference scores obtained in the semantic condition and the evaluative condition in the control group only.

Method

Participants

Forty spider-fearful students and 40 non-anxious controls at Ghent University took part in the experiment in exchange for a payment of 5 €.

Participants were selected based on their self-reported spider fear in a screening survey embedded in the web-based Experiment Sign-up site of Ghent University. Two participants were removed from the analyses because their overall error rate in the semantic categorization task (i.e., 18.75 % in both cases) exceeded the cutoff criterion which was set at 2.5 standard deviations of the mean number of errors in the semantic categorization task (i.e., 6.61 %). Two other participants were removed because their mean reaction time (i.e., 681 ms and 693 ms) in the evaluative categorization task exceeded 2.5 standard deviation of the mean reaction time) in the evaluative categorization task (i.e., 499 ms). Finally, one participant was removed because her mean number of errors in the evaluative condition (i.e., 25.78 %) exceeded 2.5 standard deviation of the mean number of errors in the evaluative categorization task (i.e., 7.48 %). Results were not contingent upon the inclusion or exclusion of these participants unless otherwise stated.

Materials

Based on norm data collected by Spruyt et al. (2002), four positive and four negative color pictures referring to either objects or living creatures were selected to be used as targets (See Appendix). Several of these pictures originated from the International Affective Picture System (i.e., IAPS; Lang et al., 1999). On a scale ranging from -5 (“very negative”) to + 5 (“very positive”), the mean valence rating of negative targets was significantly smaller than zero, M = -1.99, SD = 0.72, $t(3)$ = -1.99, $p < .05$. The mean valence rating of positive targets was significantly larger than zero, M = 1.85, SD = 0.72, $t(3)$ = 5.18, $p < .05$. Mean evaluative ratings did not differ between pictures depicting objects (M = -0.52, SD = 2.10) and pictures depicting living creatures (M = 0.38, SD = 2.43), $t < 1$. Stimuli that were used as primes were four pictures depicting spiders, four pictures depicting presents, and two pictures depicting geometric stimuli (i.e., a square and a trapezium). All pictures were 512 pixels wide and 384 pixels high.

Computer tasks were run on a Dell Optiplex GX520 computer. An Affect 4.0 program (Spruyt et al., 2010) controlled the presentation of the stimuli as well as the registration of the responses. Stimuli were presented against the black background of a 19 inch computer monitor (100 Hz).

The Dutch translation of the Fear of Spider Questionnaire (i.e., FSQ, Muris & Merkelbach, 1996; Szymanski & Donohue, 1995) was used to measure the explicit fear of spiders. The FSQ consists of 18 statements (e.g., I think a lot about spiders) which are to be rated on an eight-point Likert scale ranging from zero (Completely disagree) to seven (Completely agree).

Procedure

Participants were asked to complete the FSQ, followed by the evaluative categorization task and the semantic categorization task. The order of the two evaluative priming tasks was counterbalanced. On each trial, a fixation cross appeared for 500 ms, followed by a blank screen for 500 ms. Next, the prime stimulus was displayed for 200 ms. Following and SOA of 250 ms, the target stimulus was presented which remained on the screen until participants responded or 2000 ms had elapsed. If participants did not respond within 2000 ms, a 2000 ms message (i.e., 'Te laat!' which translates as 'Too slow!') was presented. In all tasks, we used an inter-trial interval that varied between 500 and 1500 ms. In the evaluative categorization task, participants were instructed to categorize targets according to their evaluative meaning. In the semantic categorization task, participants were instructed to categorize targets according to their animacy. Each task consisted of 160 trials, divided in two blocks of 80 trials each (10 primes × 8 targets). In between blocks, participants could take a small break.

Each evaluative priming task was preceded by 8 practice trials in which only a target stimulus was presented. Each target stimulus was presented once. On each practice trial, a 500-ms fixation cross was presented, followed by a blank screen for 750 ms. Next, the target stimulus was presented. Practice trials for the evaluative categorization task required participants to categorize the target stimuli in terms of their valence. Practice trials for the semantic categorization task required participants to categorize the target stimuli in terms of their animacy. In case of an erroneous response, a 500-ms error message (i.e., 'FOUT!'

which translates as 'WRONG') appeared.

Upon completion of the categorization tasks, participants were informed about the upcoming BAT. They were told that the experimenter was interested in getting an impression of their level of spider fear. Therefore, they would be asked to enter a room containing a living spider located inside a cardboard box. Next, participants were presented with a photograph of the spider (i.e., Araneus diadematus) and were told that they would approach the spider shown on the photograph. In reality, however, the box did not contain a spider but was simply filled with leaves. Participants were first asked whether they were prepared to start the BAT. Following a positive response, participants were accompanied to an adjacent room that contained the cardboard box. Participants were asked to perform the following six steps: (1) approach the cardboard box, (2) touch the cardboard box, (3) open the cardboard box, (4) pick up the cardboard box, (5) try to uncover the spider by pushing away some leaves with a pencil, and (6) try to uncover the spider using their hands. Before each step was executed, the participant was asked whether he or she was willing to perform the step. Participants were assured that they could refuse to perform a particular step at any time. If a participant refused to perform a step, the instructions for that particular step were repeated. The BAT was terminated following a second refusal. The experimenter neither encouraged nor praised the participant during the performance of the BAT. Finally, participants were once more asked to fill out the FSQ.

Results

Data Reduction and analysis

Neutral priming trials (neutral/negative and neutral/positive trials) were not included in the analysis. Furthermore, we excluded trials in which an incorrect response was given (8.0 % in the evaluative condition, 6.3% in the semantic condition), a response was given past the response deadline (1.6 % in the evaluative condition, 1.6 % in the semantic condition), or if response latencies deviated more than 2.5 standard deviations from a participant's mean latency in a particular condition (see Ratcliff, 1993; 2.9% in the evaluative condition, 3.1% in the semantic condition).

Similar to Experiment 1, two inter-individual difference scores were computed. A first difference score was calculated by subtracting the mean response latencies observed on trials consisting of a present-related prime and a positive target from the mean response latencies observed on trials consisting of a present-related prime and a negative target. Next, we calculated a second difference score by subtracting the mean response latencies on trials consisting of spider-related prime and a positive target from the mean response latencies observed on trials consisting of a spider-related prime and negative target. Finally, priming scores were obtained by subtracting the second difference score from the first difference score. Hence, positive priming scores indicate a preference for presents over spiders.

Descriptive statistics

A reliable mean inter-individual difference score of 15.82 ms was obtained in the evaluative categorization task, $t(74) = 3.20$, $p < .001$, $d = .37$. The mean inter-individual difference score in the semantic categorization task was 3.30 ms and was not statistically different from zero, $t < 1$. A difference score was computed by subtracting inter-individual difference scores obtained in the semantic categorization task from inter-individual difference scores obtained in the evaluative categorization task. This difference score was not dependent on whether participants described themselves as spider-fearful (M = 20.45 ms, SD = 58.06 ms) or being unafraid of spiders (M = 5.01 ms, SD = 48.73 ms) on the prescreening survey embedded in the web-based Experiment Sign-up site of Ghent University, $F(1,73) = 1.57$, $p = .21$, $\eta^2 = 0.02$. This effect was not dependent on the order in which the two categorization tasks were administered, $F < 1$. Interestingly, we did observe a marginal significant difference between inter-individual difference scores obtained in the semantic categorization task and the evaluative categorization task if the spider-fearful group and control group were defined based on a median split of the FSQ scores obtained during the experimental session, $F(1,73) = 3.87$, $p = .05$, $\eta^2 = 0.05$. Interestingly, the difference score was higher in the spider-fearful group (M = 26.27, SD = 54.23) as compared to the control group (M = 2.11, SD = 51.37). The effect was not modulated by the presentation order of the two categorization tasks, $F(1,71) = 1.61$, $p = .21$, $\eta^2 = 0.02$. Follow-up analyses revealed that the difference score reached significance in the spider-fearful group but not in the control group, $t(31) = 2.74$, $p < .05$, $d = 0.48$ and $t < 1$, respectively.

Correlational analyses

Correlational analyses revealed an almost-perfect correlation between FSQ scores measured at the start of the experiment and FSQ scores at the end of the experiment, $t(73) = 31.12$, $p < .001$, $r = .96$. Accordingly, the two FSQ scores were averaged. We did not observe a significant correlation between the mean FSQ score and inter-individual difference scores obtained with the semantic categorization task, $r = -.07$, $t < 1$, or the evaluative categorization task, $r = .19$, $t(73) = 1.65$, $p = .10$, respectively. Inter-individual difference scores obtained in the semantic categorization task and the evaluative categorization task did not correlate with each other, $r = .12$, $t < 1$.

Predictive validity

To examine the relationship between implicit attitudes and the number of steps performed on the BAT, a cumulative logit model (i.e., proportional odds model) was used. Inter-individual difference scores obtained with the evaluative categorization task were unrelated to the number of steps performed on the BAT[1], $X^2 < 1$. This effect was not dependent on the order in which the evaluative categorization task and the semantic categorization task were performed, $X^2 < 1$. In addition, we did not find a significant relationship between inter-individual difference scores obtained in the semantic categorization task and behavior, $X^2 < 1$. However, there was some evidence that the relationship between the number of steps performed on the BAT and the inter-individual difference scores obtained with the semantic categorization task was dependent on the order in

[1] It may be noted that inter-individual difference scores obtained with the evaluative categorization task did predict BAT performance if outliers were *not* removed from analysis, $X^2(1) = 5.94$, $p < .05$.

which the two categorization tasks were performed, $X^2(1) = 2.43$, $p = .12$. Follow-up analyses revealed a significant relationship between the number of steps performed on the BAT and the inter-individual difference scores obtained in the semantic categorization task if this task preceded the evaluative categorization task, $X^2(1) = 4.24$, $p < .05$. In line with Experiment 1, this relationship was again in the opposite direction of what we expected. Participants whose inter-individual difference scores were indicative of a high level of spider fear were more likely to perform a higher number of steps on the BAT than participants whose inter-individual difference scores were indicative of lower level of spider fear (β = 0.03). The effect did not reach significance if the semantic categorization task was performed after the evaluative categorization task, $X^2 < 1$.

Finally, we also examined the relationship between the explicit measure of spider fear and behavior on the BAT. This relationship proved to be significant, β = -0.05, $X^2(1) = 45.13$, $p < .001$. Participants whose FSQ scores indicated a relatively high level of spider fear completed fewer steps on the BAT. A hierarchical regression analysis in which both implicit and explicit measures of spider fear were included as predictors revealed a meaningful relationship between BAT performance and the explicit measure of spider fear only, $X^2 < 1$.

Discussion

In Experiment 1, it was observed that inter-individual difference scores were predictive of choice behavior only if participants were encouraged to assign selective attention to a non-evaluative, semantic stimulus dimension during attitude measurement. The nature of this relationship, however, was unanticipated. Participants whose inter-individual difference scores were indicative of an (implicit) preference for fruit over candy were more likely to select a piece of candy than a piece of fruit during the behavioral choice task. Because of the unexpected nature of this finding, we decided to conduct a second experiment aimed at examining the impact of FSAA on the predictive validity of implicit preference scores obtained with the evaluative priming paradigm.

Replicating the results of Experiment 1, we observed that BAT performance was unrelated to inter-individual difference scores obtained under conditions that promoted selective attention for the evaluative stimulus dimension. However, it should be noted that inter-individual difference scores obtained with the evaluative categorization task did reliably predict behavior if outliers were not removed from the analysis. It could thus be suggested that the absence of predictive validity in the evaluative decision task was due to a lack of power. Also in line with Experiment 1, was the observation that inter-individual difference scores obtained with a non-evaluative semantic categorization task were predictive of a behavioral outcome measure, at least if this task was performed prior to the evaluative decision task. The fact that the predictive validity of this task was contingent upon the task order can be readily accounted for in two ways. First, selective attention assignment can carry-over from one experimental phase to another (Vanaelst, Spruyt, & De Houwer, 2016). Second, this effect might simply be due to fatigue or a sense of boredom as participants were required to complete two evaluative priming tasks in close succession. Most importantly, the relationship between BAT performance and the evaluative priming scores obtained in the non-evaluative semantic categorization task was again reversed. The more individual difference scores were indicative of fear towards spiders, the better BAT performance. It thus seems rather unlikely that the results in Experiment 1 were simply a chance finding. In contrast to our expectation, we did not observe a significant difference between inter-individual difference scores obtained in the semantic categorization task and evaluative categorization task in the control group. However, this difference score did reach significance in the spider-fearful group. A detailed discussion of these unexpected findings is provided in the general discussion.

GENERAL DISCUSSION

In the present study, we examined the hypothesis that measures of implicit evaluation will be less indicative of behavioral outcomes if they are obtained under conditions that maximize selective attention for the evaluative stimulus dimension as compared to when they are obtained under conditions that require participants to focus selective attention on non-evaluative stimulus information. This hypothesis was based on the FSAA framework developed by Spruyt and colleagues (Everaert, et al., 2013; Spruyt et al., 2009; Spruyt, et al., 2007), in which it is stated that the extent to which evaluative stimulus features are processed is dependent on the degree to which attention is assigned to evaluative information. Moreover, one can expect stable inter-individual differences in the extent to which selective attention is assigned to certain stimulus dimensions as numerous studies have shown that FSAA is governed by personal goals. For example, using a multidimensional scaling approach, Fazio and Dunton (1997) observed a significant correlation between a measure of the implicit attitude towards black individuals and the degree to which participants were inclined to assign selective attention to race-related stimulus information (see also Mogg, Mathews, & Weinman, 1989; Viken, Treat, Nosofsky, McFall, & Palmeri, 2002).

To test our hypothesis, we conducted two evaluative priming experiments in which participants were encouraged to assign selective attention to either evaluative stimulus information or to non-evaluative semantic features. Despite the fact that the procedures employed in these studies were quite different, the pattern of results were strikingly similar. First, implicit preference scores obtained with the (classic) evaluative decision task were unrelated to outcome measures (but see below). Second, in both experiments, a reliable relation was found between a behavioral outcome measure and evaluative priming scores obtained under conditions that maximized selective attention for non-evaluative, semantic stimulus features. Third, in both experiments, the nature of this relationship was counterintuitive. Participants were inclined to avoid attitude objects that were associated with a positive implicit preference and to approach attitude objects that were associated with a negative implicit preference, not the other way around.

In sum, our studies provided some initial evidence for the idea that the predictive validity of evaluative priming scores is indeed moderated by FSAA. Nevertheless, several aspects of our findings were surprising and require further discussion. Let us first consider the predictive validity of evaluative priming scores obtained under conditions that maximize selective attention for evaluative stimulus information. In line with our predictions, these evaluative priming scores were unrelated to behavior in Experiment 1. This result was replicated in Experiment 2, but it must be noted that we did observe a reliable relation in this second study if outliers were not removed. Furthermore, in Experiment 2, evaluative priming scores obtained with the evaluative decision task tended to correlate ($p = .10$) with an explicit measure of spider fear (i.e., the FSQ). It could thus be hypothesized that these effects might have reached statistical significance had we included more statistical power. Taken together, then, our results seem to mimic the current state of affairs in the literature. Whereas several studies attesting to the predictive validity of the (classic version of the) evaluative priming paradigm have appeared in the literature (e.g., Degner, et al., 2007; Dovidio, et al., 2002; Frings & Wentura, 2003), several other studies failed to replicate this finding (Falk et al., 2015; Spruyt, et al., 2007). The present null findings are important because they indicate that FSAA is indeed an important moderator of predictive validity in the sequential priming paradigm and could prove to be valuable in understanding the inconsistent findings that appear in the literature.

Second, in contrast to our expectations, in Experiment 2, we did not observe a significant difference between the inter-individual difference scores obtained with the semantic categorization task and the evaluative categorization task in the control group. We did, however, observe a clear-cut difference in the sample of spider-fearful participants. Clearly, this pattern of findings is exactly the opposite of what we had hypothesized a priori. We will address this observation in more detail below.

Finally, we observed a reversed relationship between the behavioral outcome measure and evaluative priming scores obtained under conditions that maximized selective attention for the non-evaluative, semantic stimulus features. This observation is not only counterintuitive, it is also at odds with several studies showing a (normal) positive relationship between behavioral measures and implicit attitude measures obtained in the absence of an evaluative processing mindset (e.g., Dovidio et al., 2002; Frings & Wentura, 2003; Spruyt, et al., 2007; Vandromme et al., 2011). At this point, we see two ways to account for this observation. First, as already noted above, it could simply be argued that the reversed relationship between the implicit attitude scores and the behavioral outcome measure resulted from Type-I errors. The fact, however, that this reversed relationship was observed in two consecutive experiments seems to be incompatible with such an account. Therefore, as an alternative explanation, one might argue that the requirement to maximize selective attention for non-evaluative semantic information somehow resulted in a reversal of the evaluative priming scores (i.e., participants responded faster to incongruent trials compared to congruent trials). Reversed evaluative priming effects have been reported by several authors (for an overview, see Klauer, Teige-Mocigemba, & Spruyt, 2009). For example, in six experiments, Glaser and Banaji (1999) consistently observed reversed evaluative priming effects for extreme primes in a word naming task. Chan, Ybarra, and Schwarz (2006) presented evidence that highly accessible targets are likely to show reversed priming effects. Similar results have also been reported in the non-evaluative domain (Eimer & Schlaghecken, 1998; see also Eimer & Schlaghecken, 2003). Importantly, the idea that the evaluative priming effect captured by the semantic categorization task were reversed could also explain the (unexpected) observation in Experiment 2 that the difference between the mean priming score obtained with the semantic categorization task and the mean priming score obtained with evaluative categorization task was reliable the spider-fearful group. If it is assumed that spider-fearful participants exhibited a normal (assimilative) priming effect in the evaluative categorization task but a reversed evaluative priming effect in the semantic categorization task, one would naturally predict a large difference between the two priming scores.

The question arises, of course, how such reversal of the evaluative priming effect can be accounted for at the mental-process level. Unfortunately, existing accounts of reversed priming effects such as the accuracy motivation account (Glaser & Banaji, 1999; Glaser, 2003) and the psychophysical account (Klauer et al., 2009) have great difficulty explaining the complete pattern of results obtained in our studies. The main problem is that both accounts explain the presence or absence of reversed priming effects in terms of specific procedural details (e.g., the extremity of the primes or the length of the SOA) whereas these procedural details were identical across the different versions of the evaluative priming tasks implemented in our studies. Further research will thus be required to substantiate the idea that the reversed relation between behavioral outcomes and evaluative priming scores observed in the present experiments did indeed result from to a reversal of the evaluative priming scores. We anticipate, however, that this research studies will be a stimulating enterprise as it could potentially advance our understanding of reversed evaluative priming effects in general.

In sum, we conducted two studies aimed at showing that the predictive validity of implicit attitudes as captured by the evaluative priming paradigm might increase as a function of decreasing levels of selective attention for the evaluative stimulus dimension. In line with this reasoning, we found evaluative priming scores to be predictive of behavior only if participants were encouraged to assigned selective attention to a non-evaluative, semantic stimulus dimension. Unexpectedly, in both experiments, this relationship was in the opposite direction of what was predicted. Further research will be needed to establish the generality of this effect and to uncover its underlying mechanisms.

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APPENDIX

Experiment 1: description of target pictures

- Picture depicting positive living beings: poesje (kitten) and bruid (bride)
- Pictures depicting positive objects: ballonnen (balloons) and bloem (flower)
- Pictures depicting negative living beings: slang (snake) and soldaat (soldier)
- Pictures depicting negative objects: pistool (pistol) and vuilnis (garbage)

Experiment 2: description of target pictures

- Picture depicting positive living beings: poesje (kitten) and bruid (bride)
- Pictures depicting positive objects: ballonnen (balloons) and lolly (popsicle)
- Pictures depicting negative living beings: slang (snake) and soldaat (soldier)
- Pictures depicting negative objects: pistol (pistol) and vuilnis (garbage)

CHAPTER 3

A META-ANALYSIS OF THE EFFECT OF FEATURE-SPECIFIC ATTENTION ALLOCATION ON THE PREDICTIVE VALIDITY OF IMPLICIT ATTITUDE MEASUREMENTS

Despite numerous studies attesting the predictive validity of implicit attitude measures, several authors have reported data in which the relationship between inter-individual differences scores obtained with these measures and behavioral outcome measures proved to be unreliable. To shed further light on the conditions under which implicit measures can be used to predict behavior, we examined the extent to which the predictive validity of these measures is dependent upon feature-specific attention allocation. We present a meta-analysis of 57 studies in which the predictive validity of either the evaluative priming paradigm or the approach-avoidance paradigm was examined. A small-to-moderate relation between behavioral outcome measures and implicit attitude scores was obtained with the evaluative priming paradigm (r = .22) as well as the approach-avoidance paradigm (r = .16). In contrast to our expectations, however, there was no corroborating evidence for the idea that the predictive validity of implicit attitude measures depends on feature-specific attention allocation.

INTRODUCTION

Ever since Allport (1935) proclaimed that personal likes and dislikes bear a strong relationship with behavior, behavioral scientists have sought reliable instruments to measure inter-individual differences in those likes and dislikes. Together with increasing evidence that attitude objects can be evaluated in an unconditional and automatic fashion (e.g., Arnold, 1960; Barlett, 1932; Lazarus, 1966; Wundt, 1907), so called ‘implicit’ measurement techniques were developed to measure the spontaneous automatic reactions elicited by persons, objects, and situations. For example, in the evaluative priming paradigm (Fazio, Sanbonmatsu, Powell, & Kardes, 1986), the influence of a task-irrelevant prime on the evaluative categorization of a subsequently presented target is examined. Crucially, the evaluative congruence between prime and target stimuli is manipulated. Evaluative categorization responses are typically facilitated (i.e., lesser errors and faster response times) when the prime and the target share the same connotation relative to when the evaluative connotation of the prime-target pairs is incongruent (but see Chan, Ybarra, & Schwarz, 2006; Glaser & Banaji, 1999; Klauer, Teige-Mocigemba, & Spruyt, 2009). One can thus use the evaluative priming procedure to measure spontaneous likes and dislikes by displaying attitude-relevant stimuli as prime stimuli. For example, to measure spontaneous evaluative reactions toward spiders, pictures of spiders can be presented as primes. A typical observation is that spider-fearful participants show a benefit in both speed and accuracy if the picture of spider is followed by a negative word relative to when the picture is followed by a positive word (e.g., Klein et al., 2012). Other, well-known examples of implicit measurement techniques are the Implicit Association Test (IAT, Greenwald, McGhee, & Schwartz, 1998), the relational responding task (i.e., RRT, De Houwer, Heider, Spruyt, Roets, & Hughes, 2015), and the Approach- Avoidance task (i.e., AAT; Solarz, 1960). These techniques are especially useful in situations in which the validity of self-reports is debatable, either because likes and likes are not accessible through introspection or because there is considerable doubt that participants will report their true likes and dislikes (see Fazio & Olson, 2003; Greenwald & Banaji, 1995).

Numerous studies have shown that implicit attitude scores can be used to predict relevant behavioral outcomes (see for example, Degner & Wentura, 2009; Descheemaeker, Spruyt, & Hermans, 2014; Klein et al., 2012; Spruyt et al., 2015). Corroborating these results, recent meta-analytic reports have corroborated the reliable predictive validity of both the evaluative priming paradigm (Cameron, Brown-Iannuzzi, & Payne, 2012) and the IAT (Greenwald, et al., 2009). However, the magnitude of the relationship between behavioral outcomes and implicit attitudes as captured by the evaluative priming paradigm (Cameron et al., 2012) or the IAT (Greenwald, Poehlman, Uhlmann, & Banaji, 2009) is only small to medium ($r = .28$ and $r = .27$, respectively). Moreover, despite several studies attesting the predictive validity of implicit measures (see for example Degner, Wentura, Gniewosz, & Noack, 2007; Dovidio, Kawakami, & Gaertner, 2002; Frings & Wentura, 2003), other studies have failed to replicate the existence of a meaningful relationship between behavioral outcome measures and inter-individual difference scores obtained with the evaluative priming paradigm (e.g., Falk, Heine, Takemura, Zhang, & Hsu, 2015; Spruyt, Hermans, De Houwer, Vandekerckhove, & Eelen, 2007), the AAT (e.g., Neimeijer, de Jong, & Roefs, 2015), or the IAT (e.g., Blanton et al., 2009). One way to account for these inconsistent findings is to assume that the predictive validity of implicit attitude measures is dependent upon moderating variables.

The main aim of the present meta-analysis was to examine the moderating influence of feature-specific attention allocation (i.e., FSAA) on the predictive validity of implicit measurement techniques. Although many studies supported the assumption that evaluative processing can take place in an unconditional and automatic fashion (e.g., Bargh, Chaiken, Raymond, & Hymes, 1996; Draine & Greenwald, 1998; Fazio, 2001; Greenwald, Klinger, & Schuh, 1995; Hermans, Crombez, & Eelen, 2000; Klauer, Roßnagel, & Musch, 1997; Spruyt, Hermans, De Houwer, & Eelen, 2002; Vuilleumier, 2005; Zajonc, 1980), recent reports suggest that automatic evaluative stimulus processing is typically reduced under conditions that promote selective attention for non-evaluative semantic stimulus features. This observation has now been confirmed using several markers of automatic stimulus processing, including the evaluative priming effect (e.g., Spruyt, De Houwer, & Hermans, 2009; Spruyt, De Houwer, Everaert, & Hermans, 2012), the affect misattribution paradigm (Everaert, Spruyt, and De Houwer, 2016), the dot probe effect (Everaert, Spruyt, & De Houwer, 2013), and the emotional stroop effect (Everaert et al., 2013). Similar effects have also been found in the domain of automatic non-evaluative semantic processing (see Kiefer & Brendel, 2006; Kiefer & Martens, 2010; Kiefer, 2012; Martens & Kiefer, 2009). These findings are accounted for by the feature-specific attention allocation framework developed by Spruyt and colleagues (Everaert et al., 2013; Spruyt et al., 2009; Spruyt, De Houwer, Hermans, & Eelen, 2007) in which it is stated that stimulus features are processed only if and to the extent that they are selectively attended to. Importantly, FSAA might not only be driven by explicit task instructions (Everaert, 2012; Spruyt et al., 2009) or subtle aspects of the procedure (Everaert, Spruyt, & De Houwer, 2011), but might also be dependent on personal goals. That is, individuals might not only differ in the extremity and direction of their implicit likes and dislikes but also in the extent to which selective attention assignment is required to automatically process the evaluative tone of attitude objects. Various studies support the idea that selective attention to evaluative stimulus features is dependent on the personal relevance of attitude objects. For example, using a multidimensional scaling approach, Cavanagh and Davey (2001) showed that spider-fearful participants add greater weight to the evaluative stimulus dimension of pictures depicting spiders than non-fearful persons. Likewise, Viken, Treat, Nosofsky, McFall, and Palmeri (2002) observed that women with high levels of bulimic symptoms are more attentive to body size information and less attentive to facial affect information (see also Fazio & Dunton, 1997; Mogg, Mathews, & Weinman, 1989). These results suggest that the degree to which participants process the evaluative features of attitude-relevant stimuli automatically is dependent upon the personal relevance of these stimuli. It can thus be hypothesized that inter-individual differences in implicit evaluation will be particularly outspoken under conditions that promote selective attention for a non-evaluative stimulus feature. However, for most implicit measurement techniques, it has been a common practice to maximize the extent to which participants assign attention to the evaluative features of task-relevant stimuli. Participants are typically asked to categorize (at least a subset of) stimuli according to their evaluative features (e.g., the evaluative priming paradigm; Sanbonmatsu, Posavac, Vanous, Ho, & Fazio, 2007; and the IAT; Greenwald, et al., 1998), the decide whether targets are more or less positive than an average target stimulus (e.g., the Affect misattribution procedure; Payne, Cheng, Govorun, & Stewart, 2005), or instructed to approach or avoid stimuli based on their evaluative connotation (e.g., the AAT; Solarz, 1960).

Preliminary evidence for the hypothesis that the predictive validity of implicit attitude measures is moderated by FSAA, was observed in a study by Spruyt, et al. (2007) using the picture-picture naming task, a variant of the evaluative priming paradigm. The picture-picture naming task is semantically neutral because participants are simply asked to name the target pictures and are, therefore, not encouraged to selectively attend to a specific semantic stimulus dimension. Spruyt, et al. (2007) showed that individual difference scores obtained with the picture-picture naming task could predict participants' choice between fruit or candy relatively well compared to inter-individual difference scores obtained with implicit attitude measures in which attention to evaluative features was maximized (i.e., the evaluative priming paradigm with the standard evaluative categorization task and the IAT, which both produced null-findings).

To allow for a direct examination of the impact of FSAA on the predictive validity of implicit attitude measures, we only included implicit measurement tasks in which participants can either be asked to respond based on evaluative stimulus features or on the basis of non-evaluative stimulus features. One example of such a measure is the AAT in which participants have to approach or avoid a valenced stimulus. (Krieglmeyer, Deutsch, De Houwer, & De Raedt, 2010; Solarz, 1960). The AAT is based on the assumption that positive stimuli tend to evoke approach tendencies whereas negative stimuli tend to evoke avoidance tendencies. Generally, approach behavior is facilitated (i.e., faster responses and/or less errors) in response to positive stimuli and inhibited (i.e., slower responses and/or less errors) in response to negative stimuli. Likewise, avoidance behavior is facilitated (i.e., faster responses and/or less errors) in response to negative stimuli and inhibited (i.e., slower responses and/or less errors) in response to positive stimuli. Importantly, participants can be asked to respond to task-relevant stimuli either based on their evaluative features (i.e., valence; e.g., Churchill & Jessop, 2011) or based on their non-evaluative features (i.e., format or orientation of stimuli; e.g., Reinecke, Becker, & Rinck, 2010). Another attitude measurement paradigm that meets our inclusion criterion is the evaluative priming paradigm. In most evaluative priming studies participants are asked to respond on the basis of the evaluative features of target stimuli (e.g., Fazio, Jackson, Dunton, & Williams, 1995), but this is by no means a requirement. In the piciture-picture naming task developed by Spruyt, et al. (2007), for example, participants are instructed to simply name the targets. In contrast, an example of an implicit measurement technique that does not meet the inclusion criterion is the affect misattribution procedure (i.e., AMP; Payne et al., 2005). In this task, participants are presented with a valenced prime stimulus followed by the presentation of Chinese pictograph. Participants are asked to indicate whether the Chinese pictograph is less pleasant or more pleasant than the average Chinese pictograph. Typically, participants are more inclined to categorize the Chinese pictograph as less pleasant than the average Chinese pictograph following a negative primes stimulus as compared to a positive prime stimulus and vice versa. As such, it is inherent to the AMP that participants evaluate the task-relevant stimuli. For the same reason, we did not include studies in which the IAT was used to capture implicit evaluations (Greenwald et al., 1998).

In addition, we examined whether the effect of FSAA on the predictive validity of implicit measures is modulated by the personal relevance of the to-be-measured implicit attitudes. It can be expected that implicit measures that minimize selective attention assignment towards evaluative features will fail to pick up reliable inter-individual differences if the to-be-measured evaluation is of low importance to the participants. In contrast, for participants for whom the to-be-measured attitudes are personally relevant, we expected implicit measures to pick up implicit evaluations regardless of attentional deployment. Thus, inter-individual differences between participants for whom the attitude object is relevant and for whom the attitude object is irrelevant will be largest under conditions that minimize attention towards evaluative stimulus features. Therefore, we expected that it would be specifically advantageous to use a measure that minimizes selective attention assignment towards evaluative stimulus features as compared to a measure that maximizes selective attention assignment towards evaluative stimulus features in a group consisting of people for whom attitude objects were or were not of personal importance.

Finally, the present meta-analysis is important for a number of other reasons. To the best of our knowledge, we are the first to examine the magnitude of the relation between behavioral outcome measures and inter-individual difference scores obtained in the AAT. Accordingly, we ran a number of exploratory analyses in order to examine the moderating effect of various procedural variables that might impact the predictive validity of the AAT, including the type of task (i.e., the abstract manikin task, the joystick task, or the joystick feedback task), the type of the behavioral outcome (i.e., concurrent self-reported behavior, retrospective self-reported behavior, concurrent objective behavior, and outcomes that summarize objective behavior omitted during the past), the domain of the study (i.e., prejudice, consumer preferences, personality, impulsive behavior, clinical psychology, or other for studies that did not fit any of the other domains), target modality (i.e., pictures or words), or the number of trials used in the study. Similarly, we examined several procedural variables that could influence predictive validity in the evaluative priming paradigm, including the type of task (i.e., the evaluative decision task, the semantic decision taks, the lexical decision task, or the naming taks), type of behavioral outcome, stimulus modality (i.e., pictures or words), and prime visibility (i.e., supraliminal or subliminal presentation).

METHOD

Inclusion criteria

Studies were included if they met a number of criteria. (1) We only included studies examining implicit likes and dislikes. Studies were not included if implicit measures were used to capture automatic non-evaluative semantic processing. For example, neither did we include measures that examined individual differences in the perceived relationship between alcohol and aggression nor did we include studies that examined individual differences in the perceived relationship between sex and power. (2) Only studies in which the evaluative priming paradigm or the approach-avoidance paradigm was used were included. Evaluative priming studies in which participants were asked to evaluate the targets (Fazio et al., 1986) were classified as instances of studies in which participants focused attention on evaluative stimulus information. Evaluative priming studies in which the semantic decision task (e.g., Banaji & Hardin, 1995), the naming task (Bargh et al., 1996; Hermans, De Houwer, & Eelen, 1994), or the lexical decision task (Meyer & Schvaneveldt, 1971) were used were included as instances of studies in which participants did not focus attention on non-evaluative stimulus information. To examine the predictive validity of the AAT, we included the abstract manikin task (De Houwer, Crombez, Baeyens, & Hermans, 2001), the joystick task (Chen & Bargh, 1999), the joystick feedback task (Seibt, Neumann, Nussinson, & Strack, 2008), and the vertical three button task (Rotteveel & Phaf, 2004). In each of these tasks, participants can be asked to respond to target stimuli based on their evaluative stimulus features (e.g., valence) or based on their non-evaluative stimulus features (e.g., orientation or format of target stimuli; see Kersbergen, Woud, & Field, 2014; Machulska, Zlomuzica, Adolph, Rinck, & Margraf, 2015). (3) We restricted the literature search to studies that focused on behavior as an outcome measure. That is, we only include outcome measures that resembled observable behavior or behavior that, in principle, could have been observed. Thus, we did not include correlations between two implicit measures of likes and dislikes or between an implicit and an explicit measure of likes and dislikes. For example, the intention of a participant to perform a certain behavior was not considered to be an adequate outcome measure as intention is as a subjective self-assessment of an unobservable construct. In this sense, the intention variable should be regarded as an indirect self-report measure of likes and dislikes instead of a proxy of behavior. In the same vein, we did not include studies in which the outcome variable consisted of a close friend of the participants reporting about the participants' self-esteem or self-liking for two reasons (e.g., Falk et al., 2015). First, these judgments are considered to reflect likes and dislikes instead of observable behavior. Second, judgments of behavior by close friends can be biased by how participants perceive themselves. Indeed, it is likely that judgments of behavior by close friends will be in accordance with the manner in which participants present themselves. (4) Studies were not included if the relationship between the attitude measure and the outcome was qualified by a moderator variable. This criterium was adopted because we were primarily interested in the direct relationship between attitude and outcome measures. Furthermore, the necessity to use subsets of data to account for within-study moderators would have considerably complicated data-analysis. (5) Studies were written in English and published in a peer-reviewed journal. Authors were contacted for further information if studies did not provide adequate statistical information for derivation of effect sizes.

Search Procedure

A literature search was conducted until February 2016. Searches were executed in three databases (i.e., ISI Web of Science, PubMed, and PsycINFO) using the following keywords: implicit, measure (or measures), and behavior. Additional articles were obtained from the reference sections of these articles as well as the reference sections of an earlier meta-analysis concerning the predictive validity of the evaluative priming paradigm (i.e., Cameron et al., 2012). This initial search rendered a list of 474 studies to be considered for inclusion in the meta-analysis. Of these studies, 38 were excluded because the implicit measure was not correlated with an adequate outcome variable. Another, 337 studies were excluded because of the use of an implicit measure that was did not meet our inclusion criteria. Finally, 10 studies were excluded because we were unable to retrieve the statistical information needed for the calculation of the appropriate effect size, despite several requests for additional data from the original authors. In sum, 87 studies were retained for the analysis. These studies originated from 57 different articles and yielded 175 relevant effect sizes to be analyzed. Thirty of these studies concerned the predictive validity of the evaluative priming paradigm, with a total of 95 effect sizes. The approach-avoidance paradigm was examined in 28 studies, with 80 effect sizes in total.

Moderators

One researcher coded all studies whereas a second researcher coded one fourth of them. Interrater reliability between the two researchers was assessed using the intraclass correlation coefficient for continuous measures and Cohen's kappa in the case of categorical variables. These measures are reported below for each individual moderator.

Transformation of effect size. Studies were coded with respect to whether the effect size was provided in the published report or had to be calculated on the basis of statistical information reported in the report. Cohen's Kappa equaled 1.

Type of implicit measurement. Studies were categorized based on the type of implicit measurement that was used to measure implicit evaluations: the evaluative priming procedure or the approach-avoidance paradigm. Cohen's kappa equaled 0.92.

Feature-Specific Attention Allocation. Effect sizes were classified in two, broad categories depending on whether the corresponding implicit measure involved an explicit evaluative processing mindset. Evaluative priming studies in which a naming task, a lexical decision task, or a non-evaluative decision task was used were all classified as studies in which participants were not required to focus attention on the evaluative stimulus dimension. Evaluative priming studies in which participants performed an evaluative decision task were classified as studies in which participants assigned selective attention to evaluative features. The AAT was considered to maximize attention to evaluative stimulus features if participants were asked to respond to target stimuli based on an attitude-relevant stimulus features. For example, participants might be asked to respond to target stimuli based on whether stimuli depicted an alcoholic beverage or a non-alcoholic beverage. Since this stimulus feature is clearly related to likes and dislikes of alcohol, this type of task was considered as a task in which participants were encouraged to assign attention to evaluative stimulus features. However, if participants were asked to respond on the basis of an attitude-irrelevant feature (e.g., the shape of the target stimulus; Cousijn, Goudriaan, & Wiers, 2011) the AAT was considered to maximize attention to non-evaluative stimulus features. As mentioned above, we expected the predictive validity to be higher in tasks in which attention to non-evaluative stimulus features was maximized as compared to tasks in which attention to evaluative features was maximized. Cohen's kappa for this variable equaled 1.

Relevance. Studies were classified as 'relevant' if the implicit attitude measure was administered in a sample of participants for whom the presented attitude objects were personally relevant. For example, one can expect spider-related stimuli or nicotine-related stimuli to be highly relevant for spider-fearful participants or (abstaining) smokers, respectively. Effect sizes obtained in samples of participants for whom the critical attitude objects were not particularly relevant were coded as 'irrelevant'. If a sample consisted of a mixture of participants for whom the tested attitude objects were or were not personally relevant, studies were coded as 'mixed relevance'. We expected a significant effect of relevance under conditions that maximized attention to non-evaluative stimulus features. In contrast, we did not expect that relevance would moderate the predictive validity of tasks in which attention to evaluative stimulus features was maximized. Cohen's Kappa equaled 0.82.

Type of Behavior. A distinction was made between behavior observed by the experimenter and self-report measures. In addition, we coded whether behavior took place at the time of the measurement itself (i.e., concurrent report) or whether behavior was reported after it took place (i.e., retrospective report) (see Schwarz, 1999; Schwarz, 2007). Accordingly, our coding scheme differentiated between four types of outcome measures: concurrent self-reported behavior (e.g., self-reported choice behavior), retrospectively self-reported behavior (e.g., a judgment of the average number of cigarettes smoked during the last month), concurrent objective behavior (e.g., behavior on a behavioral assessment task), and outcomes that summarized behavior omitted during the past in an objective way (e.g., Body Mass Index). As retrospective reports are often considered to be biased (Schwarz, 1999; Schwarz, 2007), we expected a stronger relation between implicit likes and dislikes and concurrent reports of behavior as compared to retrospective reports of behavior. Similarly, as self-reports are often contaminated by impression management strategies (see Fazio & Olson, 2003), we predicted higher predictive validity if objective behavior was used as an outcome measure than if self-reports were used. Cohen's Kappa equaled 0.83.

Type of research domain. A distinction was made between six different research domains: prejudice (based on gender, race, or groups), consumer preferences (e.g., time watching big brother), personality (e.g., self-esteem), impulsive behavior (non-clinical levels of behavior such as drinking, smoking, eating, fear, …), clinical psychology (studies among clinical populations such as persons with anorexia nervosa), and the category 'other' for studies that did not fit one of the above-mentioned categories. Since, this variable was added for purely exploratory reasons, we did not have an a priori hypothesis about the direction of the effect. Cohen's kappa equaled 0.78.

Publication year. The oldest study included in the meta-analysis was published in 1995. Given the accumulation of knowledge over time, one might predict (or hope) that that the predictive validity of implicit attitude measures must have improved over time as the result of methodological innovations (for an example, see Wentura & Degner, 2010). The intraclass correlation coefficient equaled 0.99.

Time of behavioral assessment. We coded whether the behavioral outcome assessment and the implicit attitude assessment took place in close temporal succession. This variable was included because the relationship between an implicit attitude measure and a behavioral outcome measure may change as a function of the delay between the these measures, for example because (implicit) evaluations themselves might change over time (e.g., Reinecke et al., 2010; Teachman & Woody, 2003). We expected predictive validity to be highest if attitude assessment and behavioral outcome assessment took place in close temporal succession as compared to when the two measures were assessed at different moments in time. Cohen's kappa equaled 0.88.

Methodological parameters.

Evaluative priming paradigm. To examine the extent to which the predictive validity of implicit priming measures is affected by procedural parameters, we took into account the following list of variables: the number of trials, stimulus modality (pictures vs. words), the duration of the presentation time of the primes and targets, the length of the interval between the onset of the primes and the onset of the targets (i.e., stimulus-onset asynchrony; SOA), the interval between two adjacent trials (i.e., inter-trial interval; ITI), and whether the presentation time of the target stimulus and the ITI was fixed or variable. In addition, we tested whether the relationship between inter-individual difference scores and behavior was influenced by whether primes were presented subliminal or supraliminal. Finally, we coded the type of evaluative priming paradigm that was used to measure implicit evaluations: the evaluative decision task, the semantic decision task, the lexical decision task, or the naming task.

Approach-avoidance paradigm. Procedural parameters taken into account were: target modality (i.e., pictures or words) and number of trials. We also coded whether the abstract manikin task, the joystick task, or the joystick feedback task was used. In the abstract manikin task, approach-avoidance behavior is measured on an abstract level. That is, participants move a manikin towards or away from the target stimulus with the use of a key on the computer key board. In contrast, in the joystick task, approach-avoidance behavior is operationalized as the actual horizontal movement of a vertically positioned joystick or lever. The joystick feedback task is identical to the joystick task except that visual feedback is given; i.e., the target stimulus comes closer or disappears upon movement of the joystick or lever. Finally, in the three button task, three one-button boxed are placed vertically on a stand. Participants push either the lower or the top button in response to a target stimulus. Note, however, that the vertical three button task was not recorded as an extra level of this moderator as none of the included studies made use of this particular task (see Phaf, Mohr, Rotteveel, & Wicherts, 2014 for an in depth analysis of the different types of approach-avoidance tasks). Finally, we also differentiated between different scoring algorithms that are used to compute inter-individual difference scores in the AAT. A first algorithm to be considered is the RT-based compatibility score that is computed by subtracting mean reaction times observed in the compatible condition from mean reaction times observed in the incompatible condition (e.g., Spruyt et al., 2013). Second, we considered the 'D-measure' which is computed by dividing the RT-based compatibility score by the pooled standard deviation (see Greenwald, Nosek, & Banaji, 2003; Wiers, Eberl, Rinck, Becker, & Lindenmeyer, 2011). Third, we included constrained RT-based compatibility effects in which the RT-base compatibility score is based on a subset of the presented stimuli (e.g., Peeters et al., 2013). For example, to obtain likes and dislikes toward alcohol, one would subtract the mean RT observed on trials in which alcohol-related stimuli have to be pulled from the mean RT observed on trials in which alcohol-related stimuli have to be pushed. Finally, we also coded studies in which the constrained RT-based compatibility effect was corrected for responses on neutral trials. In these studies an approach bias score was calculated for neutral stimuli by subtracting the mean RT on approach-neutral trials from the mean RT on avoid-neutral trials. Next, this approach bias for neutral stimuli was subtracted from the constrained RT-based compatibility effect for relevant stimuli (e.g., Neimeijer et al., 2015). Target duration and ITI were not coded as most studies did not report these parameters

Effect Size Calculation

All effect sizes (i.e., odds ratios, *t*-statistics, *F*-statistics, X^2-statistics) were converted to Pearson Correlation coefficients. As Pearson Correlation coefficients have some undesirable statistical properties (e.g., a problematic standard error formulation; Rosenthal, 1994), correlations were converted to Fisher's z-scores (Hedges & Olkin, 1985). After the analysis, Fisher's z-scores were converted back to their original scale. For the majority of studies, information about the reliability of the implicit measure or/and outcome measure was missing. Accordingly, no correction for attenuation of the correlations due to the unreliability of the involved variables was applied. The interrater reliability equaled .99.

Analytic Model

Data analysis was performed in the statistical software package R (version 3.1.2.; R Core Team, 2014) using the metafor package (Viechtbauer, 2010). A multilevel (i.e., three-level random effects model) meta-analysis was performed to deal with dependency between effect sizes (Van den Noortgate, López-López, Marín-Martínez, & Sánchez-Meca, 2014). We chose to perform a multi-level analysis as this method enabled us to perform an analysis on non-aggregated data. In contrast to a meta-analysis performed on aggregated datasets, the multi-level approach allows for (a) the use of all available data points and (b) an examination of all important moderating variables.

It should be noted that analyses were performed on the absolute values of the effect sizes. We deliberately chose for this approach because we were primarily interested in the size of the attitude-behavior relationship and not in the direction of this relation. Moreover, for a considerable amount of experiments, the authors did not specify the expected direction of the relationship between behavior and implicit evaluations. As a result, it was often unclear whether the correlation was in the expected direction or not. Sensitivity analysis were performed to identify outliers and influential data points. Effect sizes were considered influential if standardized residuals exceeded 3 and leverages were higher than two times the average leverage.

Publication Bias

An important concern for each meta-analysis is the question whether there is evidence for a publication bias. Publication bias is mostly due to the underrepresentation of studies with small subject samples. The low statistical power of small sample studies often leads to non-significant results. Importantly, these null results are less likely to get published as compared to significant effects. Consequently, the literature will contain a disproportional amount of studies with small sample sizes and large effect sizes as compared to studies with small sample sizes ad small effect sizes. If publication bias is present, the studies included in the meta-analysis represent a biased selection of studies with large effects. Consequently, the results of the meta-analysis will provide an overestimation of the true effect size. To examine whether publication bias influenced our results, we examined the shape of a funnel plot in which a measure of the accuracy of the study (i.e., the standard deviation of the effect size) is plotted against the effect size (Elvik, 1998; Light, Singer, & Willet, 1994; Sterne & Egger, 2005). If publication bias is absent, effect sizes are arranged in a symmetric, pyramid shape. However, it must be noted that funnel plots in a multilevel meta-analysis should be interpreted with caution as these plots do not take into account the dependencies between effect sizes. Therefore, the occurrence of publication bias was also tested by using Egger's regression test (Egger, Smith, Schneider, & Minder, 1997; Sterne & Egger, 2005). The multilevel random effects model was modified to include the variance of the effect sizes as a moderator. The intercept of this analysis provides a measure of asymmetry, i.e., studies with small sample sizes reveal effect sizes that differ systematically form the effects of larger studies. Publication bias is assumed if the intercept of the regression deviates significantly from zero. The significance of the intercept was tested at $p < .1$ (see Egger et al., 1997).

RESULTS

Effect sizes

Correlations (*k* = 175) ranged from 0 (by definition, given the use of absolute values) to 0.59. The multilevel model revealed a significant average weighted effect size of *r* = .19, *p* < .001, *95% CI* = .17 to .22. There was a clear indication of heterogeneity in the effect sizes, $Q(174)$ = 244.44, *p* <.001. The estimated proportion of within-study variation equaled $I^2_{(2)}$ = 0.00 whereas the estimated proportion of between-study variation equaled $I^2_{(3)}$ = 0.23. Thus, whereas the heterogeneity between effect sizes originating from the same study was negligible, about one fourth of the heterogeneity in the data could be accounted for by differences in effect sizes between studies. We did not observe influential (outlying) data points in the dataset.

Moderator Analysis

Transformation of effect size. Effect sizes derived on the basis of statistical information reported in the study did not differ significantly from effect sizes that could be extracted directly from the article, $Q_m(1) < 1$.

Type of implicit measurement. The mean average effect size was significantly qualified by an interaction with type of implicit measurement, $Q_m(1)$ = 7.34, *p* < .01. The mean average effect size in the evaluative priming paradigm (*r* = .22, *p* < .001, *95% CI* = .19 to .26, *k* = 95) was higher than the average effect size in the approach-avoidance paradigm, (*r* = .16, *p* < .001, *95% CI* = .12 to .19, *k* = 80).

Relevance. The test of the moderator relevance was not significant, $Q_m(2)$ = 1.77, *p* = 0.41. The estimated average correlation did not differ reliably between participants for whom attitude objects were irrelevant (*r* = .20, *p* < .001, *95% CI* = .17 to .23, *k* = 119), participants for whom the attitude objects were relevant (*r* = .19, *p* < .001, *95% CI* = .09 to .29, *k* = 28), or for mixed-relevance groups (r = .19, p < .001, *95% CI* = .14 to .23, k = 28). The effect of relevance also did not differ between the evaluative priming procedure and approach-avoidance paradigm, $Q_m(5) = 9.09$, $p = .11$.

Feature-Specific Attention Allocation. Results did not reveal a significant difference between the estimated aggregated correlation in studies in which participants were encouraged to assign selective attention toward evaluative features (r = .21, p < .001, *95% CI* = .18 to .25, k = 95) relative to studies in which participants were not encouraged to assign selective attention to evaluative features (r = .17, p < .001, *95% CI* = .13 to .21, k = 80), $Q_m(1) = 2.49$, $p = .11$.

We did observe that the effect of FSAA differed between the evaluative priming procedure and the approach-avoidance paradigm, $Q_m(3) = 9.36$, $p < .05$. Follow-up analyses revealed, however, that the effect of attention allocation failed to reach significance both for the evaluative priming paradigm, $Q_m(1) < 1$, and the approach-avoidance task, $Q_m(1) = 2.10$, $p = .15$. In addition, we did not observe a significant interaction between FSAA and relevance, $Q_m(5) = 8.90$, p = .11. Indeed, follow-up analyses revealed that the effect of relevance was not statistically different in tasks that did, $Q_m(2) = 2.39$, $p = .30$, or did not, $Q_m(2)$ = 3.89, p = 0.14, encourage participants to assign attention to the evaluative features stimulus dimension.

Next, we examined the three-way interaction between FSAA, relevance, and the type of implicit measurement. Interestingly, this three-way interaction reached significance, $Q_m(10) = 18.99$, $p < .05$. The two-way interaction between relevance and FSAA was not significant in the evaluative priming paradigm, $Q_m(4) < 1$. However, the interaction between relevance and FSAA did reach significance in the approach-avoidance task, $Q_m(5) = 15.76$, $p < .01$. Follow-up analyses revealed that the effect of relevance in the AAT was significant if participants assigned selective attention to non-evaluative features, $Q_m(2)$ = 7.38, $p < .05$. Predictive validity did not differ between the mixed-relevance group and the irrelevant-evaluations group, $Q_m(1) < 1$, whereas the mixed-relevance group and the irrelevant-evaluation group both differed from the relevant-evaluations group, $Q_m(1) = 3.73$, $p = .05$, and $Q_m(1) = 4.16$, $p < .05$, respectively. In contrast to our expectations, the estimated correlation between inter-individual difference scores and behavior was smaller in the relevant-evaluations group compared to the mixed-relevance and the irrelevant-evaluations group. For participants who attended to evaluative stimulus features during attitude assessment, the main effect of relevance was only marginally significant, $Q_m(2) = 5.37$, $p = .07$. Follow-up analyses revealed a significant difference in predictive validity between studies in which likes and dislikes were personally relevant and studies in which likes and dislikes were irrelevant, $Q_m(1) = 5.54$, $p < .05$. Indeed, predictive validity was notably higher in the relevant-evaluations as compared to the irrelevant-evaluations group. The difference in predictive validity between the mixed relevance group and the relevant-evaluations group, $Q_m(1) = 2.26$, $p = .13$, as well as the difference in predictive validity between the mixed group and the irrelevant-evaluations group, $Q_m(1) < 1$, did not reach significance (see Table 1).

Finally, the four-way interaction between attention allocation, relevance, type of implicit measurement, and type of behavior reached significance, $Q_m(26) = 48.21$, $p < .01$. However, we decided not to give too much weight to this finding as most cells of the interaction contained a only a few or even no observations at all.

Type of Behavior. Overall, the estimated average effect size was not moderated by the nature of the behavioral outcome, $Q_m(3) = 3.41$, $p = .33$. The effects sizes for concurrent objective behavior ($r = .22$, $p < .001$, *95% CI* = .18 to .26, $k = 76$), retrospective self-reports of behavior ($r = .16$, $p < .001$, *95% CI* = .19 to .18, $k = 75$), concurrent self-reports of behavior ($r = .22$, $p < .001$, *95% CI* = .09 to .34, $k = 7$), and objective outcomes summarizing past behavior ($r = .18$, $p < .001$, *95% CI* = .10 to .26, $k = 17$) were all very similar. Interestingly, the effect of domain of study was qualified by an interaction with type of implicit measurement, $Q_m(7) = 23.04$, $p < .01$. Follow-up analysis revealed, that the effect of type of behavior did not reach significance in the evaluative priming paradigm, $Q_m(3) = 6.34$, $p = .10$, but did reach significance in the approach avoidance task, $Q_m(3) = 8.46$, $p < .05$. Follow-up analyses revealed that predictive

Table 1. Predictive validity in function of type of implicit measurement, FSAA, and relevance (CI in parentheses).

FSAA: attention to valence	Relevance	Type of implicit measurement: Evaluative priming task *r* (95 % CI)	*k*	Approach Avoidance task *r* (95 % CI)	*k*
yes	Relevant	NA	NA	.35** (.17 to .51)	5
	Irrelevant	.23** (.18 to .27)	59	.14** (.10 to .19)	16
	Mixed	.24** (.12 to .35)	4	.21** (.09 to .32)	10
No	Relevant	.22** (.04 to .39)	6	.09* (.01 to .17)	20
	Irrelevant	.22** (.15 to .29)	27	.15** (.10 to .21)	17
	Mixed	NA	NA	.17** (.12 to .22)	12

Note: *k* = number of effect sizes; $*p < 0.05$, $**p < 0.001$

Table 2. Predictive validity in the AAT in function of type of behavior (CI in parentheses).

Type of behavior	*r* (95 % CI)	*k*
Objective past behavior	**.09** (.02 to .16)[a]	12
Retrospective self-reported behavior	**.13** (.10 to .17)[a]	50
Concurrent objective behavior	**.24** (.14 to .33)[b]	17
Concurrent self-reported behavior	**.22** (.09 to .34)[b]	7

Note. *k* = number of effect sizes; correlations printed in bold reached significance ($p < .05$). The difference in predictive validity between types of behavior reached significance if superscripts were different but not when they were identical.

validity in the AAT depends on whether behavior occurred at the same time of implicit measurement or whether it took place at an earlier time (see Table 2 and table 3).

Type of research domain. There was no significant heterogeneity in the effect sizes based on the research domain in which the study was performed, $Q_m(5) = 8.75$, $p = .12$. Effects sizes between studies investigating prejudice ($r = .24$, $p < .001$, *95% CI* = .17 to .31, $k = 25$), consumer preferences ($r = .20$, $p < .01$, *95% CI* = .10 to .28, $k = 9$), personality traits ($r = .19$, $p < .001$, *95% CI* = .10 to .28, $k = 23$), impulsivity ($r = .17$, $p < .001$, *95% CI* = .14 to .20, $k = 79$), clinical psychology ($r = .14$, $p < .001$, *95% CI* = .07 to .21, $k = 24$), and other topics ($r = .24$, $p < .001$, *95% CI* = .17 to .31, $k = 15$) were very similar. Interestingly, the effect of domain of study was qualified by type of implicit measurement, $Q_m(8) = 24.88$, $p < .01$. The effect of type of research domain was not significant in the evaluative priming paradigm, $Q_m(4) = 3.48$, $p = .48$; but did reach significance in the approach-avoidance task, $Q_m(3) = 11.41$, $p < .01$. Studies examining studies belonging to the ‘other’ category differed significantly from studies examining clinical behavior, $Q_m(1) = 8.87$, $p < .01$ or impulsive behavior, $Q_m(1) = 6.58$, $p < .05$. The difference in predictive validity between studies examining clinical behavior and impulsive behavior did not reach significance, $Q_m(1) < 1$. Indeed, studies classified as belonging to the ‘other’ category ($r = .36$, $p < .001$, *95% CI* =

.23 to .47, *k* = 4) revealed the highest effect size, followed by impulsive behavior (*r* = .15, *p* < .001, *95% CI* = .11 to .18, *k* = 51), and clinical psychology (*r* = .14, *p* < .001, *95% CI* = .07 to .21, *k* = 24). It may be noted that strong effect found for non-specified topics was based on just four effect sizes, originating from just two different studies. We are therefore reluctant to assign much weight to this finding. For studies concerning prejudice, consumer behavior, or personality traits, the number of studies in which the approach-avoidance task was used was simply too low (i.e., 0 or 1 study) to allow for reliable analyses.

Publication year. The estimated correlation between implicit measures and behavior was moderated by the year of publication, $Q_m(1)$ = 7.93, *p* < .01, *β* = -0.01. Interestingly, predictive validity decreased with increasing publication year. This effect was qualified by an interaction with type of implicit measurement, $Q_m(3)$ = 21.71, *p* < .01, with the effect reaching significance in the approach-avoidance paradigm, $Q_m(1)$ = 13.92, *p* < .001, *β* = -0.03, but not in the evaluative priming paradigm, $Q_m(1)$ < 1.

Time of behavioral assessment. Effect sizes were not dependent on whether behavior was assessed at the same time of the implicit evaluation assessment or at a later point in time, $Q_m(1)$ < 1. The effect of behavioral assessment did differ between the evaluative priming procedure and the approach-avoidance procedure, $Q_m(3)$ = 10.20, *p* < .05. However, follow-up analyses revealed that the effect of time of assessment failed to reach significance both in the evaluative priming paradigm, $Q_m(1)$ = 1.10, *p* = .29, and the approach avoidance task, $Q_m(1)$ = 1.31, *p* = .25 (see Table 4).

Methodological parameters.

Evaluative priming paradigm. The relationship between inter-individual difference score as measured with the evaluative priming paradigm and behavior was not moderated by prime duration (ranging between 15 ms and 1000 ms), $Q_m(1)$ < 1, whether the prime was presented subliminally or supraliminally, $Q_m(1)$ = 2.05, *p* = .15, target modality, $Q_m(1)$ < 1, type of evaluative priming paradigm, $Q_m(3)$ = 3.99, *p* = .26, and whether the presentation time of the target stimulus and ITI were fixed or not, $Q_m(1)$ = 1.24,

Table 3. Predictive validity in function of type of implicit measurement, FSAA, and Type of behavior (CI in parentheses).

FSAA: attention to valence	Type of behavior	Type of implicit measurement: Evaluative priming task r (95 % CI)	k	Approach Avoidance task r (95 % CI)	k
yes	Objective past behavior	.36** (.25 to .46)	5	.09^{+} (-.00 to .18)5	5
	Retrospective self-reported behavior	.20** (.14 to .26)	17	.14** (.10 to .19)	17
	Concurrent objective behavior	.22** (.16 to .28)	38	.32** (.19 to .43)	8
	Concurrent self-reported behavior	.12^{+} (-.01 to .24)	4	NA	NA
No	Objective past behavior	NA	NA	.10^{+} (-.02 to .21)	7
	Retrospective self-reported behavior	.22** (.13 to .30)	8	.13** (.08 to .17)	33
	Concurrent objective behavior	.21** (.13 to .29)	21	.17** (.06 to .27)	9
	Concurrent self-reported behavior	.32** (.13 to 49)	2	NA	NA

Note. k = number of effect sizes; $^{+}$ $p < .1$, * $p < .01$, ** $p < .01$

Table 4. Predictive validity as a function of type of implicit measurement and time of behavioral assessment (Confidence intervals in parentheses).

Type of implicit measurement task t	Behavioral assessment at the same time of implicit attitude assessment? Yes		No	
	r (95 % CI)	*k*	*r* (95 % CI)	*k*
Evaluative priming paradigm	**.23** (.19 to .27)	72	**.17** (.11 to .23)	23
Approach- Avoidance task	**.15** (.12 to .19)	70	**.20** (.12 to .29)	10

Note: *k* = number of effect sizes; correlations printed in bold reached significance ($p < .05$).

p = .27 and $Q_m(1) = 2.86$, $p = .09$, respectively. Interestingly, we did find a significant effect of SOA, $\beta = -0.0001$, $Q_m(1) = 8.82$, $p < .01$. The longer the time interval between prime and target (ranging between 36 ms and 2015 ms), the lower the correlation between inter-individual difference scores and behavior. In addition, we also observed that a higher number of trials (ranging between 20 and 432 trials), $\beta = 0.001$, $Q_m(1) = 6.00$, $p < .05$, a longer target duration (ranging between 220 ms and 750 ms), $\beta = 0.001$, $Q_m(1) = 4.47$, $p < .05$, and a longer ITI (ranging between 0 ms and 2500 ms), $\beta = 0.0001$, $Q_m(1) = 6.64$, $p < .01$, increased the correlation between inter-individual difference scores and behavior. Finally, this correlation was also more pronounced when pictures ($r = .23$, $p < .001$, *95% CI* = .20 to .27, $k = 59$) instead of words ($r = .20$, $p < .001$, *95% CI* = .13 to .26, $k = 36$) were used as primes, $Q_m(1) = 3.99$, $p < .05$. For exploratory purposes, we examined the interaction between the above mentioned moderators, FSAA and, relevance. The results of these analyses are reported in the Appendix (Table 4 – 11). If the three-way interaction between moderator, FSAA, and relevance did not reach significance, we report the effect of FSAA. We do not report the main effect of relevance because this effect only reached significance for the variable SOA ($p = .05$). However, this effect was not interpretable due to insufficient data.

Approach-avoidance paradigm. We found a significant effect of number of trials, *β* = 0.001, $Q_m(1) = 6.35$, *p* < .05. Results revealed an increasing degree of association between inter-individual difference scores and behavior with an increasing number of trials (ranging between 12 and 256 trials). Neither the effect of target modality, $Q_m(1) < 1$, nor the effect of type of approach-avoidance task, $Q_m(2) < 1$, reached significance. Interestingly, the estimated correlation in the AAT was moderated by scoring algorithm, $Q_m(3) = 34.21$, *p* < .001. Predictive validity was highest if an RT based compatibility score was used (*r* = .25, *p* < .001, *95% CI* = .19 to .32, *k* = 21) and differed significantly from the predictive validity when inter-individual difference scores were calculated via the restricted RT based compatibility score, $Q_m(1) = 7.96$, *p* < .01, the D-measure, $Q_m(1) = 15.80$, *p* < .001, or a restricted RT based compatibility score that was corrected for the approach bias to neutral stimuli, $Q_m(1) = 18.75$, *p* < .001. The second highest relation between inter-individual difference scores and behavior was obtained, using the restricted RT based compatibility score, (*r* = .15, *p* < .001, *95% CI* = .12 to .18, *k* = 36). This correlation was significantly different from the estimated correlation using the D- measure, $Q_m(1) = 10.07$, *p* < .01, or the restricted RT based compatibility score corrected for the approach bias to neutral stimuli, $Q_m(1) = 5.56$, *p* < .05. Finally, we did not find a difference between the estimated correlation when using the D-measure (*r* = .06, *p* = .10, *95% CI* = -.01 to .13, *k* = 9) or the restricted RT based compatibility corrected for responses to neutral stimuli (*r* = .08, *p* < .01, *95% CI* = .03 to .13, *k* = 14), $Q_m(1) = 1.17$, *p* = .28. Note, however, that the latter two results were based on 9 and 14 effect sizes originating from just three and two different studies, respectively. Therefore, it is hard to make strong claims on the basis of these observations. Similar to the evaluative priming task, we examined the three-way interaction between each of these moderators, FSAA and relevance. Results are reported in the Appendix (Table 12 – 17).

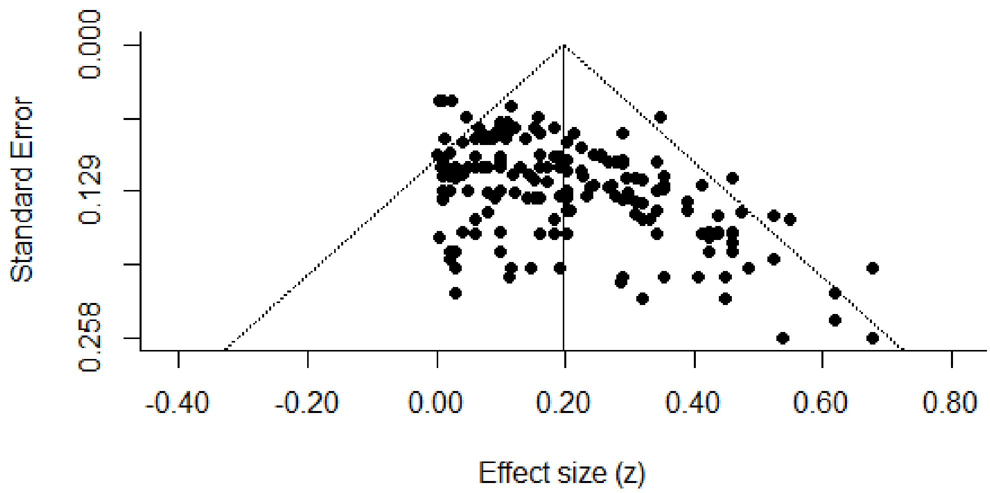

Figure 1. Funnel plot based on the complete dataset, with Fisher's z transformations of the effect size on the x-axis and standard errors on the y-axis. Dotted lines represent the 95% confidence interval around the mean.

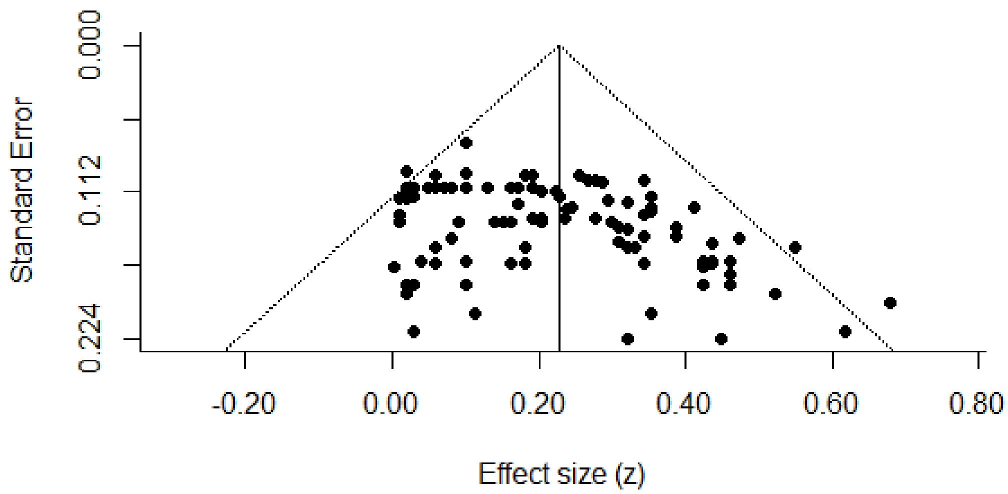

Figure 2. Funnel plot based on studies examining the predictive validity of the evaluative priming paradigm, with Fisher's z transformations of the effect size on the x-axis and standard errors on the y-axis. Dotted lines represent the 95% confidence interval around the mean.

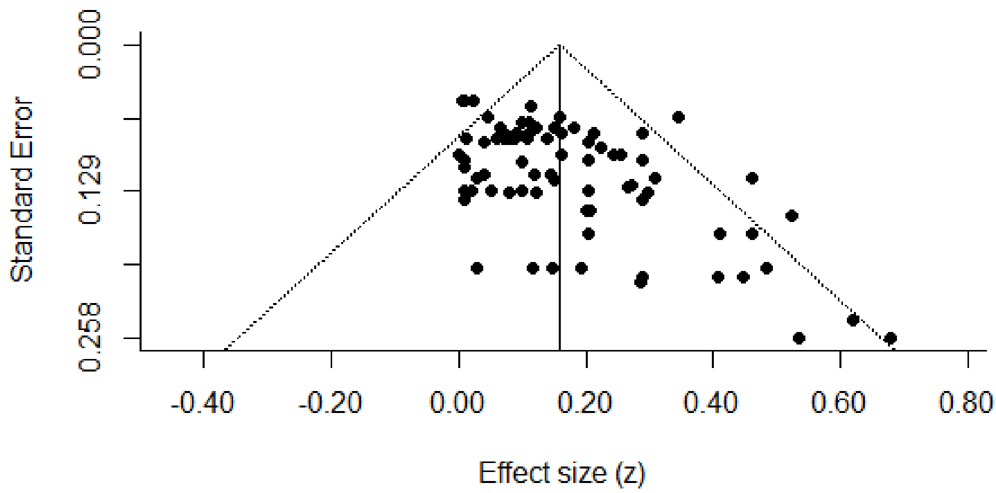

Figure 3. Funnel plot based on studies examining the predictive validity of the approach-avoidance paradigm, with Fisher's z transformations of the effect size on the x-axis and standard errors on the y-axis. Dotted lines represent the 95% confidence interval around the mean.

The impact of Publication Bias

The funnel plot based on the entire dataset is given in Figure 1 and appears to be asymmetric. The interpretation of the funnel plots was confirmed by the results of the Egger's test which revealed a significant intercept, β = 0.08, z = 4.44, p < .001. Publication bias was found both for studies examining the predictive validity of the evaluative priming procedure, β = 0.11, z = 3.13, p < .01, and studies examining the predictive validity of the approach-avoidance paradigm, β = 0.07, z = 3.30, p < .01 (see Figure 2 and 3, respectively).

DISCUSSION

The main aim of the present meta-analysis was to examine the influence of FSAA on the predictive validity of implicit measurement techniques. It was hypothesized that predictive validity would be higher if participants were encouraged to attend to non-evaluative stimulus features during implicit attitude measurement as compared to implicit measurement tasks in which attention to evaluative stimulus features is maximized, especially in participants for whom the attitudes under examination were personally relevant. The results of this meta-analysis did not confirm our main hypotheses, in several ways. First, there was no evidence for an overall dependency of predictive validity of implicit attitude measures on FSAA. Second, while we did find evidence for a significant interaction between FSAA and personal relevance in the approach-avoidance task, the nature of this interaction was opposite to our expectations. More specifically, whereas the predictive validity of the AAT that maximizes attention to non-evaluative stimulus features did not differ between the mixed-relevance group and the irrelevant-evaluations group, predictive validity was significantly lower in the relevant-evaluations group as compared to the mixed-relevance group and the irrelevant-evaluations group. It must be noted, however, that this finding does not necessarily imply that predictive validity is less outspoken in participants for whom the attitude object is personally relevant. After all, this finding might as well be an artifact of a reduced amount of variance in the inter-individual difference scores in this group. If most participants in the relevant group had very similar likes and dislikes toward the attitude object, than this low amount of variability could have attenuated the correlation between likes and dislikes and behavior.

In sum, the results of the present meta-analysis do not favor our hypothesis that predictive validity is influenced by FSAA. Nonetheless, at least for the evaluative priming paradigm, our meta-analysis might at best only provide inconclusive evidence. Note, that we hypothesized that the moderating role of FSAA on predictive validity in implicit attitude measurements would be particularly outspoken in the mixed relevance and relevant groups. Yet, the majority of studies that examined the predictive validity of the evaluative priming paradigm examined likes and dislikes that were not personally relevant (i.e. 90.5 %). Thus, only 9.5 % (i.e., nine effect sizes) of the included studies examined the predictive validity of the evaluative priming paradigm in groups in which the attitude object was personally relevant for at least a significant part of participants (i.e., the relevant-evaluations group or mixed-relevance group). Five of these effect sizes were obtained in studies in which participants were encouraged to assign selective attention to evaluative features. The other four effect sizes were obtained in studies that encouraged participants to attend to non-evaluative semantic features. Thus, it could be argued that we simply lacked sufficient statistical power to test the modulating influence of FSAA on the predictive validity of the evaluative priming paradigm.

This state of affairs indicates much more research is needed on the predictive validity of the evaluative priming paradigm in individuals for whom the attitude object is personally relevant. Implicit measurement techniques were developed to understand and predict meaningful behavior. That is, we are primarily interested whether implicit measurement techniques can be employed to predict, for example, drinking behavior in alcoholics or dietary behavior in individuals suffering from anorexia. In contrast, it seems less informative to predict drinking behavior in non-alcohol abusing students, or to predict dietary behavior in a student population. Moreover, one could argue that both the predictive validity and reliability of implicit measurement techniques will be underestimated if examined in individuals for whom these evaluations are not personally relevant. It seems therefore essential that the reliability and validity of these techniques are assessed in samples for whom the measured likes and dislikes are highly meaningful.

Finally, it is important to stress that the current findings should be interpreted with caution as inspection of the funnel plots as well as results of the Egger's test indicated the presence of publication bias. The presence of publication bias in our sample might not only have amplified our estimates of the predictive validity of implicit attitude measurements, it also impedes the conclusions that can be drawn about the moderating role of various procedural variables. Importantly, it is possible that the reported publication bias specifically effected our analyses about the moderating role of FSAA in the evaluative priming task. That is, a selective underreporting of null findings in the evaluative priming task in which attention to evaluative stimulus features is maximized, would specifically enhance the estimate of the predictive validity in these sort of tasks. Consequently, the estimated difference in predictive validity between evaluative priming tasks that maximize attention to non-evaluative stimulus features and evaluative priming tasks that maximize attention to evaluative stimulus features, would be biased. Note, that this scenario is not unlikely as far more studies have examined the predictive validity of the evaluative priming paradigm in which selective attention to evaluative features was maximized as compared to the evaluative priming paradigm in which selective attention to non-evaluative features was maximized. Unfortunately, we were unable to use the trim-an-fill method (Duval & Tweedie, 2000) to correct for publication bias as this method has not yet been developed for multilevel meta-analyses. The presence of publication bias was probably aggravated or even induced because we did not include any unpublished work in our analysis. Taken together, our observation strongly suggest that the field of implicit attitude measurement suffers from selective publishing of significant results while ignoring non-significant findings. It thus becomes critical for the advancement of this field to publish the null-findings that have probably disappeared in file drawers.

The evaluative priming paradigm

Based on 30 studies (95 effect sizes), we observed an estimated correlation of .22 between implicit likes and dislikes obtained with the evaluative priming paradigm and behavior. This correlation is somewhat smaller than the analogous correlation of .28 reported by Cameron et al. (2012). At least two possible account can be proposed to explain why these two meta-analysis arrived at different estimates. First, in the present meta-analysis only a subset of the priming tasks considered in the meta-analysis of Cameron et al. (2012) were included. For example, we did not include the AMP, the Eriksen Flanker task, or the shooter task. These tasks were not included because they did not meet our inclusion criteria. That is, none of these tasks can be administered both under conditions that maximize attention to evaluative stimulus features and conditions that minimize attention to evaluative stimulus features. Second, we adopted a very rigorous definition of behavior in the sense that we restricted our meta-analysis to behavior that was or could have been observed by an independent observer. We adopted this definition to avoid the inclusion of behaviors that were in part explicit measures of evaluations. For example, the intention to perform a certain act (e.g., to quit smoking) was conceived of as a self-assessment of an unobservable construct, not as a behavior in the strict sense. Likewise, we discarded self-reports of anxiety (Spalding & Hardin, 1999) or marital satisfaction (Mcnulty, Olson, Meltzer, & Shaffer, 2013) because these outcome measures are quite likely to be influenced by explicit evaluations.

Although the correlation between behavior and inter-individual difference scores in our study is only small to moderate, our findings suggests that the priming paradigm is indeed a valid measure of implicit likes and dislikes. Importantly, our results also reveal that predictive validity in the evaluative priming paradigm is a very general phenomenon, given that it is unaffected by variables such as the type of evaluative priming paradigm, the research topic, as well as the type of behavioral outcome measure. Corroborating the results of Cameron et al. (2002), we also did not observe any moderation by target modality, time of behavioral assessment, prime duration, or whether primes were presented subliminal or supraliminal. Moreover, as was observed by Cameron et al. (2002), predictive validity was found to be moderated by target duration, i.e., the predictive validity of evaluative priming scores increased with increasing target duration. Interestingly, the effect of target duration observed by Cameron et al. (2012) was opposite to the effect of target duration in the present meta-analysis. Cameron et al. (2012) observed that increasing target durations was associated with decreased predictive validity. The authors attributed this effect to increased processing of the prime stimulus in order to help disambiguate a briefly presented target (see De Houwer, Hermans, & Spruyt, 2001 for related findings). This discrepant finding could be explained by comparing the variability observed for the variable target duration in the study of Cameron et al. (2012) and in our study. Cameron et al. (2012) observed a lot more variance in the variable target duration (*SD* = 2280.94 ms) than we did in the present meta-analysis (*SD* = 217.61 ms). It can be concluded, that Cameron et al. (2012) included a much broader range of target durations in their meta-analysis than we included in the present study which could explain our opposite findings.

Interestingly, some variables proved to be significant moderators of predictive validity in our study but failed to reach significance in the study of Cameron et al. (2012). First, in the present meta-analysis, predictive validity decreased with increasing duration of the interval between prime onset and target onset. This finding is in line with multiple studies demonstrating that evaluative priming effects are observed at short SOA's but not at long SOA's (e.g., Fazio et al., 1986; Hermans, De Houwer, & Eelen, 2001). Second, predictive validity was depended on the ITI. A longer ITI was predictive of a higher correlation between inter-individual difference scores and the behavioral outcome measures. Third, we observed that a higher number of trials was associated with an increase in predictive validity. This finding is unsurprising as it has been generally observed that a higher number of trials increases the reliability of the measurement which leads to stronger relations between implicit attitude measures and behavior (Wentura & Degner, 2010). Fourth, we observed higher predictive validity if pictures instead of words were used as prime stimuli. This results is in line with the memory model of Glaser and Glaser (1989) stating that pictorial information has privileged access to the semantic system as compared to words (see also Spruyt, et al., 2002)

The approach-avoidance paradigm.

Although many studies have used the AAT to measure implicit likes and dislikes, there have been no quantitative reviews about the predictive validity of this task. The present meta-analysis revealed an estimated correlation of .16 between inter-individual difference scores and behavioral outcome measures based on 28 studies and 80 effect sizes. While the estimated correlation is rather small, the available data suggests that the AAT is a valid measure of implicit likes and dislikes. Moreover, the relation between inter-individual difference scores and behavior revealed to be fairly general. That is, predictive validity was insensitive to variations in the type of AAT that was used (i.e., abstract manikin task, the joystick task, or the joystick feedback task), target modality, and whether behavior was assessed at the same time of attitude assessment or not.

Interestingly, we did observe some variables that significantly moderated predictive validity in the AAT. First, the estimated correlation between inter-individual difference scores and behavior was dependent on the domain of study. More specifically, predictive validity was smaller in studies that examined clinical behavior or impulsive behavior as compared to studies that examined topics that were unrelated to either clinical behavior, impulsive behavior, politics, consumer behavior, prejudice, or personality traits. However, because the moderating effect of type of study domain was based on only a small amount of studies, we are reluctant to assign much weight to this finding. Second, in correspondence with our findings in the evaluative priming paradigm, predictive validity in the AAT increased with an increasing number of trials. This finding suggests that the amount of trials used in the AAT will influence the reliability of the task. As mentioned above, the reliability of a task will positively affect the predictive validity of this task (see Wentura & Degner, 2010). Third, we observed that the predictive validity in the AAT was dependent on the scoring algorithm that was used to compute inter-individual difference scores. In terms of predictive validity, it seems that the RT-based compatibility measure (i.e., the difference score between congruent and incongruent blocks) outperforms the restricted RT-based compatibility measure (i.e., an RT based compatibility measure that is based on only a subset of the presented stimuli). Unfortunately, only a few studies used the D-measure (i.e., 3 studies) or the restricted RT based compatibility measure corrected for responses to neutral stimuli (i.e., 2 studies). Consequently, we lacked sufficient power to make strong claims about the usefulness of these algorithms to measure inter-individual difference scores in the AAT. Fourth, our findings suggest that the association between inter-individual difference scores obtained with the AAT and behavior decreases as the time between the moment of behavior was measured and the moment behavior took place increases. Indeed, the relation between inter-individual difference scores and retrospective reports of behavior was smaller as compared to the relation between inter- individual difference scores and concurrent reports of behavior. This finding is in line with Schwarz (2007) who stated that retrospective reports of behavior are often biased memories that lack detail and precision (see also Robinson & Clore, 2002; Schwarz, 1999; Strube, 1987). Finally, the predictive validity of the AAT was moderated by the publication year of the experiment. Predictive validity decreased over time suggesting that initial reports about the predictive validity of the AAT were too optimistic. More recent reports might provide a more realistic perspective on the predictive power of the AAT.

Importantly, the results of the present meta-analysis revealed that the magnitude of the relationship between behavioral outcome measures and behavior was significantly lower in the AAT as compared to the evaluative priming paradigm. However, it would be preliminary to conclude that the evaluative priming paradigm should be preferred over the AAT if one wants to measure implicit likes and dislikes in order to predict behavior. Although predictive validity in both tasks was fairly general, we did observe various procedural variables that significantly moderated predictive validity in the AAT and the evaluative priming paradigms. Consequently, an evaluative priming design using detrimental procedural parameters might not necessarily show a higher predictive validity than an AAT in which the most optimal procedural parameters are used. Therefore, more research is necessary to determine the optimal design both for the AAT and the evaluative priming paradigm before strong conclusions can be drawn about the relative magnitude of the relationship between behavior and inter-individual difference scores obtained with these two tasks.

Conclusion

The results of the present meta-analysis reveal a small to moderate relation between inter-individual difference scores as measured with the evaluative priming paradigm or the AAT. While the estimated correlation in the

AAT was significantly lower than the estimated correlation in the evaluative priming paradigm, it seems premature to conclude that the evaluative priming paradigm outperforms the AAT in terms of predictive validity because a host of moderating variables proved to impact the predictive validity both in the evaluative priming paradigm and AAT.

Importantly, the hypothesis that the predictive validity of implicit measurement techniques is moderated by feature-specific attention allocation was not confirmed. Predictive validity did not differ between tasks in which participants were or were not encouraged to assign selective attention to evaluative features. It should be noted, however, that we were unable to identify a substantial amount of studies in which the predictive validity of the evaluative priming paradigm was studied in individuals for whom the to-be-measured likes and dislikes were personally relevant. In sum, more research is needed to test the influence of FSAA on the predictive validity of the evaluative priming scores. Importantly, we also observed strong evidence indicative of a publication bias. Researchers active in this field are thus encouraged to report unpublished null-findings to increase confidence in future meta-analyses on this topic.

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APPENDIX

Table 5. The effect of number of trials on predictive validity in the evaluative priming task as a function of FSAA and Relevance

	FSAA: attention to valence					
	No			Yes		
Relevance	*β*	Q_m(df)	*k*	*β*	Q_m(df)	*k*
Irrelevant group	**-0.002**	5.13(1)	25	**0.001**	9.28 (1)	52
Relevant Group	0.23	4.60(2)	3	NA	NA	NA
Mixed group	NA	NA	NA	0.003	.24 (1)	5

Note. *k* = number of effect sizes; beta-coefficients printed in bold reached significance (p < .05). The three-way interaction between FSAA, relevance, and number of trials reached significance, Q_m (7) = 16.36, p =.02, k = 86. Follow-up analyses revealed that the two-way interaction between relevance and number of trials reached significance if selective attention was assigned to evaluative features, Q_m(3) = 10.37, p = .02, k = 57, but not when selective attention was assigned to non-evaluative semantic features, Q_m(3) = 5.13, p = .16, k = 29.

Table 6. The effect of prime duration on predictive validity in the evaluative priming task as a function of FSAA

FSAA: attention to valence	*β*	Q_m(df)	*k*
No	-0.00	.04(1)	31
Yes	0.0001	.17(1)	60

Note. *k* = number of effect sizes; beta-coefficients printed in bold reached significance (p < .05). The two-way interaction between FSAA and prime duration did not reach significance, Q_m(3) < 1.

Table 7. The effect of target duration on predictive validity in the evaluative priming task as a function of FSAA and Relevance

	FSAA: attention to valence					
	No			Yes		
Relevance	*β*	Q_m(df)	*k*	*β*	Q_m(df)	*k*
Irrelevant group	**0.0006**	8.23(1)	8	0.004	.49(1)	7
Relevant Group	NA	NA	NA	NA	NA	NA
Mixed group	NA	NA	NA	NA	NA	NA

Note. *k* = number of effect sizes; beta-coefficients printed in bold reached significance ($p < .05$). The three-way interaction between FSAA, relevance, and target duration reached significance, Q_m (4) = 20.20, $p < .001$, $k = 16$. Follow-up analyses revealed that the two-way interaction between relevance and target duration reached significance if selective attention was assigned to non-evaluative semantic features, $Q_m(2) = 8.33$, $p < .05$, $k = 9$. The two-way interaction between relevance and target duration did not reach significance if selective attention was assigned to evaluative stimulus features, $Q_m(1) < .1$.

Table 8. The effect of SOA on predictive validity in the evaluative priming task as a function of FSAA

FSAA: attention to valence	*β*	Q_m(df)	*k*
No	**-0.0001**	5.78(1)	31
Yes	-0.0001	.55(1)	35

Note. *k* = number of effect sizes; beta-coefficients printed in bold reached significance ($p < .05$). The two-way interaction between FSAA and prime duration reached significance, $Q_m(3) = 9.47$, $p < .05$, $k = 66$.

Table 9. The effect of ITI on predictive validity in the evaluative priming task as a function of FSAA and Relevance

	FSAA: attention to valence					
	No			Yes		
Relevance	β	Q_m(df)	k	β	Q_m(df)	k
Irrelevant group	**0.0002**	9.87(1)	20	0.00	1.44(1)	34
Relevant Group	**0.23**	2.43(1)	3	NA	NA	NA
Mixed group	NA	NA	NA	-0.0001	.24(1)	5

Note. *k* = number of effect sizes; beta-coefficients printed in bold reached significance (p < .05). The three-way interaction between FSAA, relevance, and ITI reached significance, Q_m (6) = 13.34, p < .05, *k* = 62. Follow-up analyses revealed that the two-way interaction between relevance and ITI reached significance if selective attention was assigned to evaluative features, Q_m(3) = 1.92, p = .59, *k* = 39, and if selective attention was assigned to non-evaluative semantic features, Q_m(2) = 9.94, p < .01, *k* = 23

Table 10. The effect of prime visibility on predictive validity in the evaluative priming task as a function of FSAA

	FSAA: attention to valence					
	No			Yes		
Prime visibility	*r* (95 % CI)	z	*k*	*r* (95 % CI)	z	*k*
Subliminal presentation	**.20** (.09 to .30)	3.59	14	**.31** (.22 to .40)	6.09	16
Supraliminal presentation	**.23** (.16 to .29)	6.91	17	**.19** (.15 to .23)	8.61	48

Note. *k* = number of effect sizes; correlations printed in bold reached significance (p < .05). The two-way interaction between FSAA and prime visibility reached significance, Q_m(3) = 5.90, p = .13, *k* = 95. The effect of prime visibility reached significance if selective attention was assigned to evaluative features, Q_m(1) = 4.94, p < .05, *k* = 64, but not when selective attention was assigned to non-evaluative semantic features, Q_m(1) < 1.

Table 11. The effect of prime modality on predictive validity in the evaluative priming task as a function of FSAA

	FSAA: attention to valence					
	No			Yes		
Prime visibility	*r* (95 % CI)	z	*k*	*r* (95 % CI)	z	*k*
Pictures	**.23** (.17 to .29)	7.35	22	**.24** (.20 to .28)	10.18	37
Words	**.20** (.05 to .33)	2.66	9	**.19** (.12 to .27)	5.01	27

Note. *k* = number of effect sizes; correlations printed in bold reached significance (p < .05). The two-way interaction between FSAA and prime modality did not reach reach significance, Q_m(3) = 4.02, *p* = .26, *k* = 95. Follow-up analyses revealed that the effect of prime modality did not reach significance if selective attention was assigned to evaluative features, Q_m(1) = 2.13, *p* = .14, *k* = 64, nor when selective attention was assigned to non-evaluative semantic features, Q_m(1) = 1.07, *p* = .30, *k* = 31.

Table 12. The effect of target modality on predictive validity in the evaluative priming task as a function of FSAA

	FSAA: attention to valence					
	No			Yes		
Prime visibility	*r* (95 % CI)	z	*k*	*r* (95 % CI)	z	*k*
Pictures	**.22** (.15 to .29)	5.85	14	**.21** (.11 to .31)	3.95	12
Words	**.21** (.12 to .29)	4.97	17	**.23** (.19 to .28)	9.85	52

Note. *k* = number of effect sizes; correlations printed in bold reached significance (p < .05). The two-way interaction between FSAA and target modality reached significance, Q_m(3) = 1.26, *p* = .74, *k* = 95. The effect of target modality did not reach significance if selective attention was assigned to evaluative features, Q_m(1) < 1, nor when selective attention was assigned to non-evaluative semantic features, Q_m(1) < 1.

Table 13. The effect of variable or fixed target duration on predictive validity in the approach avoidance task as a function of FSAA and Relevance

	FSAA: attention to valence											
	Yes						No					
	Variable target duration			Fixed target duration			Variable target duration			Fixed target duration		
Relevance	*r* (95 % CI)	*z*	*k*	*r* (95 % CI)	*z*	*k*	*r* (95 % CI)	*z*	*k*	r (95 % CI)	*z*	*k*
Irrelevant group	**.21** (.16 to .25)[a]	8.85	49	**.40** (30 to .49)[b]	7.12	7	**.24** (.17 to .30)[a]	7.11	19	.15 (-.03 to .31)[a]	1.66	8
Relevant Group	NA	NA	NA	NA	NA	NA	**.22** (.04 to .39)	2.43	3	NA	NA	NA
Mixed group	**.24** (.12 to .35)	3.96	5	NA	NA	NA	NA	NA	NA	NA	NA	NA

Note. *k* = number of effect sizes; correlations printed in bold reached significance ($p < .05$). The three-way interaction between FSAA, relevance, and variable or fixed target duration reached significance, Q_m (6) = 13.13, $p < .05$, $k = 92$. The two-way interaction between relevance and variable or fixed target duration reached marginal significance if selective attention was assigned to evaluative features, Q_m(2) = 10.86, $p < .01$, $k = 61$, but failed to reach significance when selective attention was assigned to non-evaluative semantic features, Q_m(3) = 1.81, $p = .61$, $k = 31$. The difference in predictive validity between variable and fixed target duration reach significance if superscript were different but not when they were identical.

Table 14. The effect of fixed or variable ITI on predictive validity in the evaluative priming task as a function of FSAA

	FSAA: attention to valence					
	No			Yes		
ITI	*r* (95 % CI)	z	*k*	*r* (95 % CI)	z	*k*
variable	**.21** (.13 to .28)	5.25	10	.08 (-.05 to .22)	1.19	4
fixed	.**20** (.08 to .32)	3.17	13	**.26** (.22 to .30)	11.51	35

Note. *k* = number of effect sizes; correlations printed in bold reached significance (p < .05). The two-way interaction between FSAA and fixed or variable ITI reached significance, $Q_m(3) = 7.02$, $p = .07$, $k = 62$. The effect of fixed or variable ITI did not reach significance if selective attention was assigned to evaluative features, $Q_m(1) = 5.95$, $p < .05$, $k = 39$, nor when selective attention was assigned to non-evaluative semantic features, $Q_m(1) < 1$, $k = 23$.

Table 15. The effect of number of trials on predictive validity in the approach avoidance task as a function of FSAA and Relevance

	FSAA: attention to valence					
	No			Yes		
Relevance	*β*	Q_m(df)	*k*	*β*	Q_m(df)	*k*
Irrelevant group	**0.001**	4.38(1)	15	0.001	1.02(1)	14
Relevant Group	0.0004	.13(1)	20	**0.009**	3.79(1)	5
Mixed group	0.001	1.54(1)	12	**0.002**	5.54 (1)	10

Note. *k* = number of effect sizes; beta-coefficients printed in bold reached significance (p < .05). The three-way interaction between FSAA, relevance, and number of trials reached significance, Q_m (11) = 42.55, $p < .001$, $k = 76$. Follow-up analyses revealed that the two-way interaction between relevance and number of trials reached significance if selective attention was assigned to evaluative features, $Q_m(5) = 15.96$, $p < .01$, $k = 29$ but not when selective attention was assigned to non-evaluative semantic features, $Q_m(5) = 21.38$, $p < .001$, $k = 47$.

Table 16. The effect of target modality on predictive validity in the approach avoidance task as a function of FSAA and Relevance

	FSAA: attention to valence											
	Yes						No					
	Pictures			Words			Pictures			Words		
Relevance	*r* (95 % CI)	*z*	*k*	*r* (95 % CI)	*z*	*k*	*r* (95 % CI)	*z*	*k*	*r* (95 % CI)	*z*	*k*
Irrelevant group	**.15** (.09 to .20)[a]	5.12	12	**.13** (.03 to .23)[a]	2.64	4	**.15** (.10 to .21)	5.67	17	NA	NA	NA
Relevant Group	**.27** (.10 to .42)[a]	3.14	3	**.54** (.24 to .75)[a]	3.32	2	**.09** (.01 to .17)	2.20	20	NA	NA	NA
Mixed group	NA	NA	NA	**NA**	NA	NA	**.17** (.12 to .22)	6.13	12	NA	NA	NA

Note. *k* = number of effect sizes; correlations printed in bold reached significance ($p < .05$). The three-way interaction between FSAA, relevance, and target modality reached significance, Q_m (7) = 19.81, $p < .01$, $k = 80$. Follow-up analyses revealed that the two-way interaction between relevance and target modality reached marginal significance if selective attention was assigned to evaluative features, Q_m(4) = 8.32, $p = .08$, $k = 31$. We did not observe enough data to examine the two-way interaction between relevance and target modality when selective attention was assigned to non-evaluative semantic features.

Table 17. The effect of type of task on predictive validity in the approach avoidance task as a function of FSAA and Relevance

	FSAA to valence					
	Yes			No		
Type of AAT	*r* (95 % CI)	*z*	*k*	*r* (95 % CI)	*z*	*k*
			Irrelevant-evaluations group			
Manikin task	**.14** (.08 to .20)[a]	4.60	12	.07 (-.10 to .24)[a]	.84	2
Joystick task	**.18** (.06 to .30)[a]	2.90	6	NA	NA	NA
Joystick feedback task	NA	NA	NA	.16 (.10 to .22)[a]	5.55	15
			Relevant-evaluations group			
Manikin task	NA	NA	NA	**.08** (.02 to .14)[a]	2.75	8
Joystick task	NA	NA	NA	NA	NA	NA
Joystick feedback task	NA	NA	NA	.12 (-.02 to.26)[a]	1.70	12
			Mixed-relevance group			
Manikin task	.**14** (.06 to .22)	5.12	9	**.15** (.09 to .21)[a]	4.94	8
Joystick task	NA	NA	NA	NA	NA	NA
Joystick feedback task	NA	NA	NA	.**28** (.14 to .40)[a]	4.03	4

Note. *k* = number of effect sizes; correlations printed in bold reached significance (p < .05). The three-way interaction between FSAA, relevance, and type of task reached significance, Q_m (11) = 35.01, p < .001, k = 80. Follow-up analyses revealed that the two-way interaction between relevance and number of trials reached significance if selective attention was assigned to evaluative features, Q_m(5) = 11.70, p < .05, k = 31, and when selective attention was assigned to non-evaluative semantic features, Q_m(5) = 18.56, p < .001, k = 49. The difference in predictive validity between type of tasks reach significance if superscripts were different but not when they were identical.

Table 18. The effect of type of scoring algorithm on predictive validity in the approach - avoidance task as a function of FSAA and Relevance

	FSAA to valence					
	Yes			No		
Type of scoring algorithm	*r* (95 % CI)	*z*	*k*	*r* (95 % CI)	*z*	*k*
				Irrelevant-evaluations group		
RT based compatibility task	**.22** (.10 to .34)[a]	3.52	4	**.37** (.22 to .50)[a]	4.62	2
Restricted RT based compatibility task	**.13** (.07 to .18)[a]	4.65	10	**.14** (.09 to .19)[a]	5.10	13
D-measure	.13 (-.03 to .28)[a]	1.64	2	.08 (-.08 to .23)[b]	.99	2
Restricted RT based compatibility task controlled for approach bias to neutral stimuli	NA	NA	NA	NA	NA	NA
				Relevant-evaluations group		
RT based compatibility task	**.48** (.23 to .65)[a]	4.28	3	**.24** (.09 to .37)[a]	3.16	7
Restricted RT based compatibility task	NA	NA	NA	.23 (-.16 to .55)[b]	1.15	2
D-measure	**.20** (.00 to .39)[b]	1.99	2	.01 (-.04 to .07)[b]	.43	3
Restricted RT based compatibility task controlled for approach bias to neutral stimuli	NA	NA	NA	**.08** (.02 to .14)[a]	2.75	8
				Mixed-relevance group		
RT based compatibility task	NA	NA	NA	**.19** (.06 to .30)[a]	2.98	4
Restricted RT based compatibility task	**.29** (.17 to .41)[a]	4.54	3	**.16** (.10 to .22)[a]	5.36	8
D-measure	NA	NA	NA	NA	NA	NA
Restricted RT based compatibility task controlled for approach bias to neutral stimuli	.08 (-.02 to .18)[b]	1.53	6	NA	NA	NA

Note. *k* = number of effect sizes; correlations printed in bold reached significance ($p < .05$). The three-way interaction between FSAA, relevance, and type of task reached significance, Q_m (16) = 54.93, $p < .001$, $k = 80$. The two-way interaction between relevance and number of trials reached significance if selective attention was assigned to evaluative features, $Q_m(7) = 18.32$, $p < .05$, $k = 31$, and when selective attention was assigned to non-evaluative semantic features, $Q_m(8) = 33.32$, $p < .001$, $k = 49$. The difference in predictive validity between type of scoring algorithms reach significance if superscripts were identical but not when they were different.

4

CHAPTER

THE EFFECT OF FEATURE-SPECIFIC ATTENTION ALLOCATION ON EXTINCTION OF RECENTLY ACQUIRED LIKES AND DISLIKES

Evaluative Conditioning (EC) refers to the change in the liking of a neutral stimulus (CS) due to its pairing with another stimulus that has a clear evaluative meaning (US). In two experiments, we examined whether extinction of EC effects is moderated by feature-specific attention allocation (FSAA). In both experiments, CSs were abstract Gabor patches that varied along two orthogonal, perceptual dimensions (i.e., spatial frequency and orientation). During the acquisition phase, one of these dimensions was predictive of the valence of the USs and participants were encouraged to assign selective attention to this dimension. During the extinction phase, CSs were presented alone and participants were asked to categorize the CSs either according to their valence (evaluative condition), the perceptual dimension that was task-relevant during the acquisition phase (relevant condition), or a perceptual dimension that was task-irrelevant during the acquisition phase (irrelevant condition). As predicted, both implicit and explicit measures of stimulus valence revealed a linear increase in the extinction rate of the EC effect as participants were encouraged to assign selective attention to non-evaluative stimulus features of the CSs during the extinction phase.

INTRODUCTION

Attitudes drive behavior (Allport, 1935). Our interpersonal interactions, the activities we pursue, the products we buy, etc., are all, to some extent, guided by our personal likes and dislikes. Because (most) attitudes are acquired during the lifetime of an organism (Walther, Nagengast, & Trasselli, 2005) it is of great theoretical and practical relevance to study the mechanisms that drive attitude acquisition and attitude change.

One way in which attitudes can be acquired or changed is through Evaluative Conditioning (i.e., EC). In this paradigm, a neutral stimulus is paired with a stimulus that has a clear positive or negative meaning. Typically, this procedure causes the valence of the initially neutral stimulus (i.e., Conditioned Stimulus or CS) to shift toward the valence of the positive or negative stimulus with which it was paired (i.e., Unconditioned Stimulus or US). This phenomenon, also referred to as the Evaluative Conditioning effect (i.e., EC effect), has now been replicated in a large number of studies (for a review, see Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010). From a classical-conditioning perspective, one might expect that a CS will lose its valence and become neutral again when it is repeatedly presented alone during an extinction session (i.e., CS-only trials). In line with this viewpoint, Lipp, Oughton, and LeLievre (2003) demonstrated that the unpleasantness ratings of a CS that was predictive of an aversive electric stimulus (i.e., US) increased during acquisition but decreased back to neutral during extinction (see also Hofmann et al., 2010). Other researchers, however, reported evidence showing that the EC effect is highly resistant to extinction. Consider, for example, the findings of De Houwer, Baeyens, Vansteenwegen, and Eelen (2000). These authors assigned participants to either a standard EC group or an extinction group. Participants in the conditioning group were exposed to seven presentations of eight CS-US pairs each. The extinction group received the same set of CS-US pairs but these were followed by five CS-only trials. De Houwer et al. (2000) found no significant difference between the size of the EC effect in the conditioning group and the extinction group. One way to account for these inconsistent findings is to assume that the degree to which the EC effect is sensitive to extinction is dependent upon moderating variables.

The aim of the present research was to examine the extent to which the extinction of EC effects depends upon one potential moderator, i.e. Feature-Specific Attention Allocation (FSAA). This research question was inspired by the FSAA framework developed by Spruyt and colleagues (Everaert, Spruyt, & De Houwer, 2013; Everaert, Spruyt, Rossi, Pourtois, & De Houwer, 2013; Spruyt, De Houwer, & Hermans, 2009; Spruyt, De Houwer, Hermans, & Eelen, 2007). Drawing on earlier work by Nosofsky (1986), this framework includes two central assumptions. First, it is assumed that stimulus dimensions are processed only if and to the extent that they are selectively attended to. Second, selective attention assignment is assumed to be dependent upon current goals and task demands. In line with this idea, various markers of automatic evaluative stimulus processing have been found to depend on the extent to which attention is assigned to the evaluative stimulus dimension, including the dot probe effect (Everaert et al., 2013), the emotional Stroop effect (Everaert et al., 2013), and the evaluative priming effect (Spruyt et al., 2009).

Building further on these findings, we hypothesized that the degree to which the EC effect is resistant to extinction must also depend on the degree to which the evaluative stimulus dimension is selectively attended to during extinction. A crucial element in our reasoning is the assumption that the resistance to extinction of the EC effect results from the fact that CSs, once they have acquired a clear valence, evoke a spontaneous evaluative response that is in line with the information that was acquired during the evaluative learning phase (Martin & Levey, 1978). In fact, it could be argued that this spontaneous evaluative response maintains or even strengthens the acquired valence because it co-occurs with the evoking CS and thus confirms the emotional nature of the CS (see Lewicki, Hill, & Czyzewska, 1992). According to the FSAA account, however, participants will process the valence of CSs to a lesser degree if they assign attention to another (non-evaluative) stimulus dimension. Hence, when attending to non-evaluative properties of the CSs during extinction, the presentation of a CS is no longer accompanied by a spontaneous evaluative response, which could result in novel learning that the CS is neutral.

To test this hypothesis, we manipulated the extent to which participants assigned attention to specific features of CSs, both during the acquisition phase and the extinction phase of the experiment. CSs were artificial, grayscale figures (Gabor patches, see below) that varied on two perceptually separable dimensions (i.e., spatial frequency and orientation). In the acquisition phase, participants were encouraged to assign selective attention to one of these dimensions because they were required to categorize the CSs according to either frequency or orientation (see Spruyt, Klauer, Gast, De Schryver, & De Houwer, 2015). Crucially, the to-be-judged dimension was predictive of the valence of the USs so that one CS category was always paired with negative USs while the other CS category was always paired with positive USs. Next, during the extinction phase, participants were asked to categorize CSs either according to the evaluative dimension (evaluative condition), the same perceptual dimension (relevant condition), or the other perceptual dimension (irrelevant condition). Participants thus assigned selective attention to valence in the evaluative condition, to a feature that was correlated with valence in the relevant condition, and to a nonevaluative semantic dimension in the irrelevant condition.

Extinction was expected to occur in the irrelevant condition but not in the evaluative condition. Because participants in the relevant condition were encouraged to assign attention to a perceptual feature that was still related to valence, we expected extinction to be less pronounced in this condition relative to the irrelevant condition. To assess the efficacy of this new approach, extinction was assessed by means of two different attitude measures. First, evaluative ratings were administered to capture more deliberate evaluations. Second, more spontaneous evaluations were captured by the Affect Misattribution Paradigm (i.e., AMP; Payne, Cheng, Govorun, & Stewart, 2005).

EXPERIMENT 1

Method

Participants

Ninety-six students (28 men, 68 women) at Ghent University with a mean age of 21.45 years (*SD* = 4.6 years) participated for course credit or were paid €5. Three participants made a large number of errors during the acquisition phase of the experiment (i.e., 32.5 %, 37.5 %, and 52.5 %) and were therefore exposed to just 67.5 %, 62.5 %, and 47.5 %, respectively, of the CS-US pairings (see below). Because these participants were clearly outliers compared to the complete sample (*M* = 7.63 %, *SD* = 8.46 %), the data of these participants were excluded from the analyses. Two additional participants were excluded because they responded exceptionally fast (i.e., less 200 ms) on a large number of AMP trials (i.e., 15.63 % and 25.00 %). Again, these participants were clear-cut outliers in comparison with the complete sample (*M* = 1.01 %, *SD* = 4.70 %). Note, however, that the conclusions reported below were not contingent upon the inclusion or exclusion of these participants.

Materials

Based on norm data collected by Spruyt, Hermans, De Houwer, and Eelen (2002), 15 positive and 15 negative color pictures were selected to be used as USs (all 512 pixels wide and 384 pixels high). Several of these pictures originated from the International Affective Picture System (i.e., IAPS; Lang, Bradley, & Cuthbert, 2008). On a scale ranging from -5 (“very negative”) to + 5 (“very positive”), the mean valence rating of negative USs was significantly smaller than zero, $M = -3.08$, $SE = 0.20$, $t(14) = -15.34$, $p < .001$. The mean valence rating of positive USs was significantly larger than zero, $M = 2.36$, $SE = 0.18$, $t(14) = 12.78$, $p < .001$. Eight grayscale Gabor patches (384 × 384 pixels) were used as CSs. These Gabor patches varied on two orthogonal, perceptual dimensions: spatial frequency and orientation (see Figure 1). Values used for the spatial frequency dimension were 4.25, 5.5, 10.5, and 11.75 cycles. Values used for the orientation

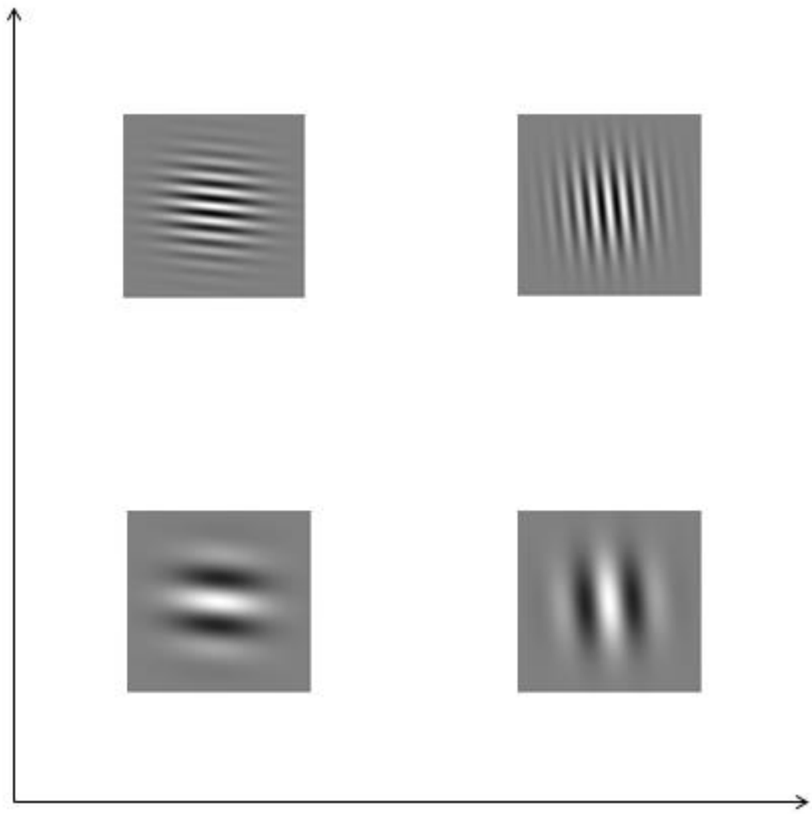

Figure 1. Examples of Gabor Patches.

dimension were 11.25, 22.5, 67.5, and 78.75 degrees. For the AMP, 200 different Chinese pictographs were used as targets. All Chinese pictographs were presented in white and were 256 pixels wide and 256 pixels high.

An Affect 4.0 program (Spruyt, Clarysse, Vansteenwegen, Baeyens, & Hermans, 2010) controlled the presentation of the stimuli as well as the registration of the responses. The experiment was run on a Dell Optiplex GX520 computer. All stimuli were presented against the black background of a 19 inch computer monitor (100 Hz).

Procedure

For the acquisition phase, participants from each experimental condition (see extinction phase) were divided in two counterbalancing groups. The first group was encouraged to assign attention to the spatial frequency of CSs (i.e., frequency Group). Participants in the second group were encouraged to selectively attend to the orientation of CSs (i.e., orientation Group). To manipulate FSAA, we used the procedures developed by Spruyt et al. (2014). More specifically, we asked participants to categorize the CSs in two arbitrary categories, i.e., “Category A” and “Category B”. In the frequency group, participants were informed that assigning attention to “the number of lines” would help them discriminate between the two CS categories. In the orientation group, participants were informed that assigning attention to “the orientation of the lines” would be an efficient strategy to optimize their performance. The cutoff values for assigning a particular CS to either Category A or Category B were 8 cycles and 45 degrees, for the frequency and orientation group respectively.

For each participant separately, the computer program selected five positive and five negative USs from the complete list of available USs (random sampling without replacement). In the frequency group, the presentation of a positive or negative US was contingent upon the spatial frequency of the CSs. In the orientation group, the occurrence of a positive or negative US was contingent upon the orientation of the CSs. Four CSs (e.g., spatial frequency below eight cycles) were paired with all the positive USs and four CSs (e.g., spatial frequency above eight cycles) were paired with all the negative USs, leading to 40 EC trials. As such, each CS category was paired 20 times with either a positive or a negative US. The assignment of a specific CS category to a specific US category was counterbalanced across participants.

Each trial started with the presentation of a CS, which participants were asked to categorize as fast as possible. In case of an erroneous response, a 3000-ms error message (i.e., “FOUT!”) appeared. In case of a correct response, the US was presented for 3000 ms. CSs were displayed until a classification response was registered and participants were asked to learn which CS belonged to which category by relying on the feedback. Participants were thus required to guess on the first trial but quickly learned to classify CSs correctly (overall error rate = 6.56 %). The inter-trial interval varied randomly between 1500 ms and 2500 ms.

Following the acquisition phase, participants completed two rating phases in which each CS was presented once in a random order. First, participants were asked to provide valence ratings for all CSs using a 21-point rating scale ranging from -10 to + 10. Second, they were asked to indicate, for each picture separately, whether they thought it would be followed, at that particular moment in time, by a negative US, a positive US, or by neither of these. Finally, participants completed a series of 16 AMP trials, modeled after the recommendations of Payne et al. (2005). Each trial started with a 500-ms presentation of a fixation cross. Next, 500 ms after the offset of the fixation cross, a CS was presented for 75 ms, followed by a blank screen for 125 ms, and the presentation of a Chinese pictograph for 100 ms. Following the Chinese pictograph, a black-and-white masking stimulus was presented until a response was registered. Participants were instructed to press the left key if they considered the Chinese pictograph to be less pleasant than average and the right key if they considered the Chinese pictograph to be more pleasant than average. The inter-trial interval varied randomly between 500 ms and 1500 ms.

The AMP was followed by an extinction phase in which each CS was presented alone for five times (i.e., 40 trials). Participants were divided in three conditions and were again asked to categorize the CSs. In the relevant condition, participants were asked to categorize CSs according to the same CS dimension as in the acquisition phase. Thus, the orientation Group categorized CSs according to orientation whereas the spatial frequency group categorized CSs according to spatial frequency. In the irrelevant condition, participants were encouraged to focus attention to the perceptual dimension of CSs that was irrelevant during the acquisition phase. So, participants in the orientation group were now asked to categorize CSs according to spatial frequency whereas participants in the frequency group were now asked to categorize CSs according to orientation. In the evaluation condition, participants were asked to judge the evaluative meaning of CSs (i.e., positive vs. negative). Each CS was presented until a response was registered. During the extinction phase, participants did not receive any feedback concerning their performance. The inter-trial interval varied between 500 and 1500 ms.

Table 1

Mean valence ratings, AMP scores, expectancy ratings, EC effects, and extinction effects as a function of Condition, moment, and CS type in Experiment 1 (SD's in parentheses).

	Acquisition			Extinction			
	CS type			CS type			
Condition	Positive	Negative	EC	Positive	Negative	EC	Extinction-effect
			Expectancy ratings				
Irrelevant	.93 (.20)	-.96 (.11)	1.89 (0.28)	0.87 (.22)	-0.91 (.21)	1.78 (0.40)	.12 (.32)
Relevant	.85 (.30)	-.84(.30)	1.69 (0.56)	0.87 (.30)	-0.92 (.22)	1.79 (0.49)	-.09 (.43)
Evaluative	.93 (.18)	-.93 (.17)	1.87 (0.33)	0.88 (.22)	-0.93 (.15)	1.81 (0.31)	.06 (.19)
			Valence ratings				
Irrelevant	2.73 (3.59)	-2.54 (4.18)	5.28 (7.22)	1.30 (2.92)	-1.22 (2.20)	2.53 (4.53)	2.75 (6.39)
Relevant	3.39 (3.17)	-3.61 (3.28)	7.00 (5.49)	3.27 (3.12)	-2.64 (3.61)	5.91 (5.82)	1.09 (3.50)
Evaluative	4.36 (2.57)	-4.04 (2.49	8.39 (4.35)	4.26 (3.22)	-4.91 (2.88)	9.17 (5.84)	-0.78 (4.25)
			AMP scores				
Irrelevant	.72 (.21)	.27 (.28)	.45 (.46)	.65 (.21)	.36 (.27)	.29 (.44)	.16 (.51)
Relevant	.68 (.24)	.33 (.27)	.35 (.45)	.70 (.23)	.34 (.30)	.36 (.51)	-.01 (.26)
Evaluative	.75 (.25)	.31 (.27)	.44 (.48)	.76 (.28)	.27 (.30)	.49 (.54)	-.05 (.46)

Finally, participants were again asked to give evaluative ratings, expectancy ratings of CSs, and to complete the AMP.

Results

Acquisition effects

A significant difference was found between expectancy ratings of positive CSs (M = 0.85, SD = 0.30) and negative CSs (M = -0.87, SD = 0.28), $F(1, 90)$ = 857.4, $p < .001$, $\eta^2 = .91$. Likewise, we found a significant difference between the mean valence ratings of positive CSs (M = 2.89, SD = 3.51) and negative CSs (M = -2.77, SD = 3.72), $F(1, 90)$ = 65.99, $p < .001$, $\eta^2 = .42$. The AMP data corroborated these findings. The proportion of pleasant judgments was significantly higher on positive-CS trials (M = .69, SD = .24) than on negative-CS trials (M = .32, SD = .27), $F(1, 90)$ = 57.62, $p < .001$, $\eta^2 = .39$. None of these effects was qualified by an interaction with counterbalancing group (attention to either orientation or spatial frequency during the acquisition phase), all F's < 3.14, nor did any of these EC effects differ between conditions, all F's < 1.81. For ten participants, at least two of the dependent measures (i.e., expectancy ratings, valence ratings, or AMP scores) did not reveal an EC effect in the expected direction after the acquisition phase (i.e., CS paired with a positive US were to be evaluated more positively than CS paired with a negative US). As it makes little sense to study extinction effects in the absence of a normal EC effect, the data of these eleven participants were excluded from all further analyses.

Extinction effects

For each participant, an extinction effect was calculated by subtracting the EC effect obtained after the extinction phase from the EC effect obtained after the acquisition phase. Extinction effects were calculated for each dependent measure and were subjected to a one way ANOVA with condition (irrelevant condition vs. relevant condition vs. evaluative condition) as a between subjects factor. Because we expected a gradual decrease in the magnitude of the extinction effect from the evaluative condition over the relevant condition to the irrelevant condition, the between-subject factor “condition” was treated as a linear factor. Expectancy ratings did not reveal a significant difference in the extinction effect between the three experimental conditions, $F < 1$. The valence ratings, however, revealed a clear effect of condition, $F(1, 78) = 6.99$, $p < .01$, $\eta^2 = .08$. As predicted, follow-up analyses showed a significant extinction effect in the irrelevant condition, $t(27) = 2.28$, $p < .05$, $d = 0.43$. As can be seen in Table 1, the EC effect was smaller after the extinction phase than after the acquisition phase. In contrast, the EC effect was more or less unaffected by the extinction procedure in both the relevant condition, $t(26) = 1.62$, $p = 0.12$, $d = 0.31$, and the evaluative condition, $t < 1$. In fact, as can be seen in Table 1, the evaluative condition revealed a small increase in the EC effect from post-acquisition to post-extinction. The AMP data1[1] mimic these results, albeit the main effect of condition just missed conventional significance levels, $F(1, 78) = 3.37$, $p = .07$, $\eta^2 = .04$. Follow-up analyses revealed that the extinction effect was far from significant in both the relevant condition and the evaluative condition, *t*’s < 1. The extinction effect was also non-significant in the irrelevant condition, but numerically there was a trend in the anticipated direction, $t(27) = 1.66$, $p = .11$, $d = 0.31$[2].

Discussion

The results of Experiment 1 are straightforward. Exposing participants to CS only trials resulted in a reduction of the EC effect when participants attended to an irrelevant CS feature during the extinction phase of the experiment (i.e., irrelevant condition) but not when participants attended to stimulus valence

[1] There is some debate concerning the extent to which AMP effects are driven by participants who rate the primes intentionally (see Bar-Anan & Nosek, 2012). We examined this possibility using the procedures described by Payne et al. (2013). In both experiments, AMP scores did not differ between participants who claimed that they had rated the primes intentionally and participants who did not claim that they had rated the primes intentionally, all *t*’s < 1.

[2] When we included the ten participants who did not show an EC effect following acquisition, the valence ratings still revealed a significant effect of Condition , $F(1, 88) = 4.07$, $p < .05$, $\eta^2 = .04$. Significant effects were absent in the AMP, $F(1, 88) = 1.32$, $p = .25$, $\eta^2 = .01$, and the expectancy ratings, $F < 1$

(i.e., evaluative condition) or to a CS feature that was correlated with stimulus valence during the acquisition phase (i.e., relevant condition). Both the explicit and implicit valence measure revealed this data pattern. The results of Experiment 1 thus provide initial support for the hypothesis that extinction of EC effects is moderated by FSAA.

It must be noted, however, that the critical effect of condition just missed conventional significance levels for the AMP data (p = .07). For a number of reasons, we are inclined to attribute this null-finding to a Type-II error. First, AMP scores were numerically in perfect accordance with our expectations (see Table 1). Second, implicit measures are typically more noisy than explicit measures (e.g., Cunningham, Preacher, & Banaji, 2001). Accordingly, given that the number of AMP trials was limited to just 16 trials, it could be argued that we simply lacked sufficient power to capture a significant modulation of the extinction effect with the AMP. Note, however, that we deliberately opted for the use of a small number of AMP trials because the AMP requires participants to assign attention to the evaluative stimulus dimension. The administration of the AMP might thus have interfered with the FSAA manipulation had we used a higher number of trials. Finally, we examined whether the extinction effects as measured by the AMP and the explicit valence measure were correlated across participants. Reassuringly, this correlation was reliable, r = .23, p < .05, adding further weight to the idea that the effects captured by the AMP were indeed meaningful.

In sum, the results of Experiment 1 are consistent with the hypothesis that FSAA is a key moderator of the extinction effect. Still, because a direct measurement of FSAA was not included in the design of Experiment 1, the argument that we manipulated FSAA is purely based on the fact that our experimental procedures produced the anticipated effects. One may thus argue that the findings of Experiment 1 are insufficient to conclude that our effects were causally driven by variations in FSAA. Accordingly, to further corroborate and extend the findings of Experiment 1, we conducted a replication study in which a direct measure of FSAA was administered at the very end of the experiment. More specifically, participants were presented with pairs of CSs and for each pair they were asked to judge the similarity of the two CSs. The INDSCAL algorithm, a multidimensional scaling approach that allows for the estimation of individual attention weights (Carroll & Chang, 1970; Carroll & Wish, 1974), was then used to verify whether the FSAA manipulation had been successful. Overall, participants in the relevant and irrelevant condition were expected to assign selective attention to the relevant and irrelevant stimulus dimension, respectively. Participants in the evaluative condition were expected to assign selective attention to the dimension that was relevant during the acquisition phase because the acquired valence of the CSs and the relevant dimension were perfectly confounded. Finally, we expected to find a reliable extinction effect only in participants who were able to shift their attention from the relevant to the irrelevant stimulus dimension.

EXPERIMENT 2

Method

Participants

Eighty-five female and eleven male Ghent University students with a mean age of 22.24 years (*SD* = 3.35 years) were paid €5 in exchange for their participation. Two participants made a large number of errors during the acquisition phase of the experiment (i.e., 37.5 % and 35.0 %) and were thus exposed to a limited number of CS-US pairings (i.e., 62.5 % and 65.0 %, respectively). Because these participants were clearly outliers compared to the complete sample (*M* = 5.76 %, *SD* = 6.09 %), their data were excluded from the analyses. We also excluded the data of one participant who was familiar with the Chinese language and knew the meaning of the Chinese ideographs used in the AMP. Finally, we excluded the data of two participants who responded exceptionally fast (i.e., less 200 ms) on a large number of AMP trials (i.e., 34.38 % and 37.50 %). Again, these participants were clear-cut outliers in comparison with the complete sample (M = 1.27 %, SD = 5.36 %). Note, however, that none of the critical extinction effects reported below were contingent upon inclusion or exclusion of these participants.

Materials and Procedure

The materials and procedures used in Experiment 2 were identical to those used in Experiment 1, except for the inclusion of a similarity judgment task at the very end of the experiment. Each trial of the similarity judgment task started with the presentation of a fixation cross in the center of the screen for 500 ms that was immediately followed by the presentation of two identical black-and-white masking stimuli (420 × 420 pixels) displayed 296 pixels apart on the left and the right side of the fixation cross. After 200 ms, the masks were replaced by two different Gabor patches that were presented for 500 ms. Each Gabor patch was then replaced with a mask that was displayed for 200 ms. Participants were asked to rate the similarity of the two Gabor patches on a four-point scale using an (AZERTY) keyboard. The scale ranged from very similar (response key “x”) over slightly similar (response key “v”) and slightly different (response key “n”) to very different (response key “;”). Participants were asked to respond within 2 seconds. If participants failed to respond within this deadline, a 300-ms message informed them that they were too slow (i.e., “TE TRAAG!”, meaning “too slow” in Dutch). The inter-trial interval varied between 500 and 1500 ms. Every possible pairing (28 pairs) of the eight CSs was presented twice, leading to a total of 56 similarity judgment trials.

Results

Acquisition effects

The overall error rate in the acquisition phase equaled 5.05 %. Expectancy ratings revealed a significant difference between positive CSs (M = 0.91, SD = 0.19) and negative CSs (M = -0.94, SD = 0.17), $F(1, 90) = 2830.9$, $p < .001$, $\eta^2 = .97$. In addition, a significant difference was found between the mean valence ratings of positive CSs (M = 2.84, SD = 2.86) and negative CSs (M = -3.12, SD = 3.65), $F(1, 90) = 86.67$, $p < .001$, $\eta^2 = .49$. The AMP data corroborated these findings. The proportion of pleasant judgments was significantly higher on positive-CS trials (*M* = .72, *SD* = .24) than on negative-CS trials (*M* = .35, *SD* = .25), $F(1, 90) = 61.68$, $p < .001$, $\eta^2 = .41$. Reassuringly, these EC effects were not moderated by the condition factor, all *F*'s < 2.12. Likewise, neither the EC effect in the valence ratings, $F(1, 89) = 3.35$, $p = .07$, $\eta^2 = .04$, nor the EC effect in the AMP data, $F(1, 89) = 1.04$, $p = .31$, $\eta^2 = .01$, was dependent upon the counterbalancing group that participants were assigned to (i.e., attention allocation to the orientation or spatial frequency dimension during the acquisition phase). The expectancy ratings, however, did show a larger EC effect for participants who attended to spatial frequency (*M* = 1.93, *SD* = 0.17) as compared to participants who attended to orientation (*M* = 1.76, *SD* = 0.42), $F(1, 89) = 6.13$, $p < .05$, $\eta^2 = .06$. For six participants, at least two of the dependent measures (i.e., expectancy ratings, valence ratings, or AMP scores) did not reveal an EC effect in the expected direction after the acquisition phase. The data of these six participants were excluded from all further analyses.

Extinction effects

For each participant and each dependent measure, extinction effects were calculated by subtracting the EC scores obtained after the extinction phase from the EC scores obtained after the acquisition phase. Individual extinction effects were then subjected to a one way ANOVA with condition (irrelevant condition vs. relevant condition vs. evaluative condition) as a between subjects factor. As in Experiment 1, the between-subject factor condition was treated as a linear factor.

Whereas the expectancy ratings revealed no effect of condition, $F < 1$, the valence ratings were clearly affected by the condition factor, $F(1, 82) = 11.61$, $p < .01$, $\eta^2 = .12$. As expected, follow-up analyses revealed a reliable extinction effect in the irrelevant condition, $t(29) = 5.02$, $p < .001$, $d = 0.92$, but not in the relevant condition, $t < 1$ (for summary statistics, see Table 2).

A different degree of extinction across different conditions was not picked up, however, by the AMP, $F(1, 82) = 1.92$, $p = 0.17$, $\eta^2 = .02$. Nevertheless, given our a priori hypothesis, follow-up analyses were performed to examine the extinction effect in each experimental condition. The extinction effect was far from significant both in the irrelevant condition, $t < 1$, and the relevant condition, $t(25) = -1.16$, $p = .26$, $d = -0.13$. Interestingly, a reversed extinction effect emerged in the evaluative condition, $t(28) = -2.76$, $p < .05$, $d = -0.51$. In line with the idea that reactivation of the CS-US relationship can strengthen the acquired valence of the CS (see Lewicki et al., 1992), the EC effect following extinction was larger, not smaller, than the EC effect after acquisition. Moreover, an exploratory analysis revealed that the effects obtained with the AMP were highly contingent on the extent to which the CSs were clear-cut instances of the experimental stimulus categories. Remember that participants were presented with eight Gabor patches, only four of which were characterized by extreme values both on the spatial frequency dimension and the orientation dimension (see Figure 2).

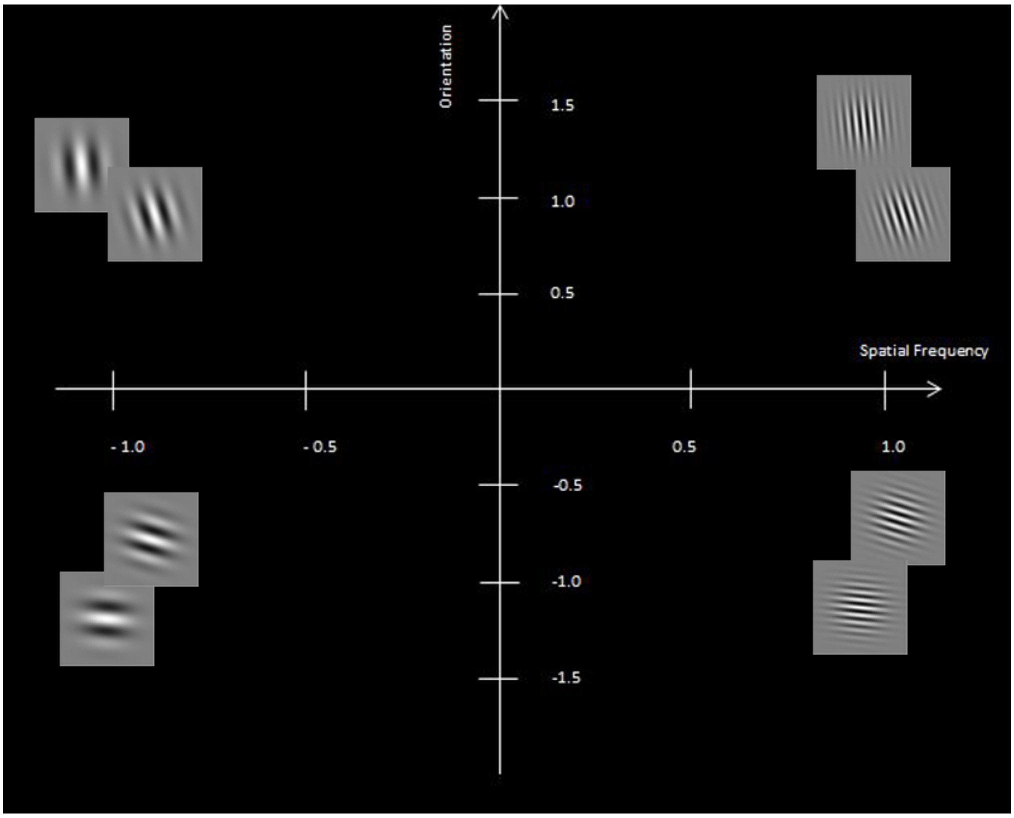

Figure 2. Multidimensional representation derived from participants' similarity judgments.

Table 2

Mean valence ratings, AMP scores, expectancy ratings, EC effects, and extinction effects as a function of Condition, moment, and CS type in Experiment 2 (SD's in parentheses).

	Acquisition			Extinction			
	CS type			CS type			
Condition	Positive	Negative	EC	Positive	Negative	EC	Extinction-effect
			Expectancy ratings				
Irrelevant	.89 (.17)	-.96 (.13)	1.85 (0.26)	.87 (.22)	-.85 (.24)	1.72 (0.40)	.13 (.40)
Relevant	.93 (.13)	-.97 (.11)	1.90 (0.20)	.89 (.21)	-.94 (.16)	1.84 (0.32)	.07 (.34)
Evaluative	.96 (.12)	-.94 (.11)	1.90 (0.19)	.90 (.22)	-.93 (.15)	1.83 (0.29)	.07 (.24)
			Valence ratings				
Irrelevant	2.94 (2.21)	-3.25 (2.95)	6.19 (4.62)	1.87 (1.82)	-1.99 (2.76)	3.86 (4.29)	2.33 (2.55)
Relevant	2.84 (2.59)	-2.58 (3.61)	5.41 (5.76)	2.38 (1.99)	-2.43 (3.11)	4.82 (4.93)	0.60 (3.46)
Evaluative	3.53 (3.32)	-4.57 (3.60)	8.10 (6.58)	3.99 (2.95)	-4.31 (3.39)	8.30 (6.13)	-0.20 (2.53)
			AMP scores				
Irrelevant	.71 (.23)	.34 (.26)	.38 (.44)	.72 (.27)	.32 (.24)	.40 (.47)	-.03 (.44)
Relevant	.75 (.24)	.38 (.23)	.37 (.45)	.76 (.21)	.27 (.29)	.50 (.48)	-.13 (.55)

Given that the Gabor patches were presented for just 75 ms during the AMP, one might argue that participants may have been unable to discriminate between different stimulus categories unless these categories were instantiated by salient exemplars. In line with this reasoning, the two-way interaction between stimulus salience and experimental condition was reliable, $F(1, 82) = 4.21$, $p < .05$, $\eta^2 = .05$. Follow-up analyses confirmed that the anticipated moderation of the extinction effect was reliable when restricting the analyses to the AMP trials with salient stimuli, $F(1, 82) = 6.11$, $p < .05$, $\eta^2 = .07$. When the analyses were restricted to the AMP trials with non-salient stimuli, there was no indication whatsoever of a differential extinction rate across different experimental conditions, $F < 1$. A similar moderation by stimulus salience was not picked up by the explicit measures.

Manipulation Check: FSAA after extinction

The similarity data were entered into the SPSS V22.0 statistical package (SPSS Inc., 1997) and were analyzed using the INDSCAL algorithm (Carroll & Chang, 1970; Carroll & Wish, 1974). Given the use of two-dimensional stimuli (i.e., Gabor patches that varied in terms of orientation and spatial frequency), the analysis was constrained to two dimensions. The algorithm reached convergence after 11 iterations (i.e., S-stress decrease smaller than 0.0001). The eventual representation had an S-stress of .19, and explained, on average, 79.1% of the variance of each individual participant. In sum, both measures of model fit showed that a two-dimensional model reached an acceptable fit. As can be seen in Figure 2, the dimensions of this two-dimensional space correspond to the orientation dimension and the spatial frequency dimension.

Individual attention weights were coded such that negative values signaled selective attention for the orientation dimension and positive values signaled selective attention for the spatial frequency dimension. We thus expected the mean attention weight to be negative in participants who were required to focus attention on the orientation dimension during the extinction phase. Likewise, the mean attention weight was expected to be positive in participants who were required to focus attention on the spatial frequency dimension. Participants who were asked to evaluate the CSs during the extinction phase (i.e., evaluative condition) were expected to focus their attention upon the stimulus dimension that was predictive of the valence of the USs during the acquisition phase. Accordingly, depending on whether the orientation dimension or the spatial frequency dimension was predictive of the valence of the USs, we expected the attention weights in this group to be negative or positive, respectively.

Individual attention weights were subjected to a 2 (FSAA group: attention to orientation versus attention to spatial frequency) × 3 (condition: irrelevant condition vs. relevant condition vs. evaluative condition) ANOVA. While the main effect of FSAA reached significance, $F(1, 79) = 9.26$, $p < .05$, $\eta^2 = .10$, a significant interaction between FSAA group and condition revealed that the impact of the FSAA manipulation was dependent upon condition, $F(2, 79) = 10.46$, $p < .01$, $\eta^2 = .21$. As can be seen in Table 3, the mean attention weights were in line with the FSAA manipulation both in the relevant condition, $F(1, 24) = 14.22$, $p < .001$, $\eta^2 = .37$, and the evaluative condition, $F(1, 27) = 11.46$, $p < .01$, $\eta^2 = .30$. In the irrelevant condition, however, the mean attention weights were numerically in the opposite direction, albeit not significantly so, $F(1, 28) = 3.94$, $p = 0.06$, $\eta^2 = .12$. We will discuss this observation at length shortly.

Table 3

Mean attention weights as a function of FSAA during the extinction phase and condition in Experiment 2 (SD's in parentheses).

Condition	Attention to frequency	Attention to orientation
Irrelevant	-0.21 (0.98)	0.39 (0.66)
Relevant	0.66 (0.68)	-0.59 (0.99)
Evaluative	0.43 (0.73)	-0.76 (1.12)

Note: Positive weights signal selective attention for the spatial frequency dimension whereas negative weights signal selective attention for the orientation dimension.

Extinction effects as a function FSAA.

Based on individual attention weights obtained through the MDS approach, participants were divided in two groups. Participants were assigned to the successful group if individual attention weight concurred with the FSAA manipulation during the extinction phase. Participants were assigned to the unsuccessful group if their attention weights revealed attention assignment opposite to the FSAA manipulation during the extinction phase. For each of the three dependent measures (i.e., expectancy ratings, explicit valence ratings, and AMP scores), extinction scores were subjected to a two-way ANOVA with condition (irrelevant condition vs. relevant condition vs. evaluative condition) and manipulation check (successful vs. unsuccessful FSAA manipulation) as between subjects factors. Results showed that the interaction between manipulation check and condition was neither reliable for the expectancy ratings, $F < 1$, nor for the evaluative ratings, $F(1, 79) = 2.40$, $p = .13$, $\eta^2 = .03$, or the AMP scores, $F < 1$.

Discussion

Taken together, the present results replicate the results obtained in Experiment 1. As in Experiment 1, the evaluative ratings revealed that the extinction rate of the EC effect was dependent upon variations in FSAA during the extinction phase. In line with our hypotheses, exposure to a series of CS-only trials resulted in a reliable drop of the EC effect only if participants were encouraged to assign selective attention to a stimulus dimension that was orthogonal to evaluative stimulus information (i.e., the irrelevant condition).

The results obtained with the AMP corroborate and extend this observation in two ways. First, while the overall analyses of the AMP data failed to reveal a clear-cut effect of the FSAA manipulation on the extinction rate of the EC effect, the AMP scores did reveal a significant increase in the EC effect from post-acquisition to post-extinction in the evaluative condition. This observation is in perfect accordance with the hypothesis that reactivation of the CS-US relationship can strengthen the acquired valence of the CS (see Lewicki et al., 1992). Second, exploratory analyses showed that the anticipated pattern of results was reliable if the analyses were restricted to CSs that were clear-cut instances of the experimental stimulus categories. This data pattern is readily accounted for if one assumes that the very brief presentation time of the CSs (i.e., 75 ms) prevented participants from discriminating between different stimulus categories unless these categories were instantiated by salient exemplars.

Importantly, the present experiment included a direct assessment of FSAA at the very end of the experiment. As anticipated, both in the relevant condition and evaluative condition, participants were inclined to assign attention to the stimulus dimension that was task-relevant during the preceding extinction phase. In the irrelevant condition, however, the mean attention weights were numerically in the opposite direction. That is, participants who were asked to judge the orientation dimension during the extinction phase were, on average, inclined to assign attention to the spatial frequency dimension. Likewise, if the extinction phase required participants to judge the CSs in terms of spatial frequency, attention weights captured at the very end of the experiment were indicative of selective attention for the orientation dimension. To account for this (unexpected) data pattern, the order in which participants completed the different measures is key. Remember that, immediately prior to the measurement of FSAA, participants were asked to provide expectancy ratings for each of the CSs. For participants in the irrelevant condition, by definition, the stimulus dimension that was predictive of the USs was orthogonal to the stimulus dimension that was task-relevant during the extinction phase. It can thus be argued that the requirement to retrieve knowledge about the CS-US relationship may have triggered selective attention for the stimulus dimension that was task-relevant during the acquisition phase, as was evidenced by the FSAA scores obtained in this group.

Clearly, this reasoning implies that the FSAA scores were not an accurate reflection of selective attention assignment during the extinction phase. It is therefore not surprising that, despite our initial hypotheses, no relationship was found between the FSAA scores and the extent to which our experimental manipulation impacted the extinction rate of the EC effect in different conditions. Still, the pattern of results obtained with the FSAA measure is important, for two reasons. First, it shows that FSAA is highly volatile and that relatively subtle procedural details can be sufficient to induce changes in FSAA. Given that FSAA has been shown to modulate automatic evaluative stimulus processing (Everaert, Spruyt, & De Houwer, 2011; Spruyt et al., 2009, Spruyt, De Houwer, Everaert, & Hermans, 2012), spatial attention allocation (Everaert, et al., 2013), as well as the generalization of EC effect (Spruyt, Klauer, Gast, De Schryver, & De Houwer, 2015), it could thus be worthwhile to scrutinize in future research the (situational and/or person specific) factors that determine (the flexibility of) FSAA. Second, both in Experiment 1 and Experiment 2, the AMP was administered after the explicit ratings. It may thus be hypothesized that stronger AMP effects may have occurred had we administered the explicit ratings after the AMP. As will become clear in the General Discussion, such an observation would also be highly informative about the nature of the underlying mechanism that is responsible for the observation that FSAA modulates the extinction rate of EC effects.

GENERAL DISCUSSION

Whereas some researchers have argued that the EC effect is highly resistant to extinction, others have argued that repeated presentations of a CS in the absence of a US do lead to a reduction of the EC effect. Based on the FSAA framework developed by Spruyt and colleagues (Everaert et al., 2013; Spruyt et al., 2007, 2009, 2012; Spruyt, 2014; Spruyt & Tibboel 2015), we hypothesized that the extinction rate of the EC effect must be dependent upon the degree to which selective attention is assigned to the evaluative stimulus dimension during extinction. To test this hypothesis, we conducted two experiments in which participants were asked to focus their attention on different aspects of CSs during the extinction phase. Participants were either asked to assign selective attention to the evaluative tone of the CSs (i.e., evaluative condition), to a (perceptual) stimulus dimension that was related to stimulus valence (i.e., relevant condition), or to a (perceptual) stimulus dimension that was unrelated to stimulus valence (i.e., irrelevant condition). Both experiments were identical, except for the fact that, in Experiment 2, an attempt was made to include a direct measure of FSAA at the end of the extinction phase.

In line with our expectations, the explicit valence ratings obtained in Experiment 1 revealed a reduction of the EC effect in the irrelevant condition, but not in evaluative condition or the relevant condition. The same pattern was observed for the AMP data, albeit the effect just missed conventional significance levels (p = .06). Experiment 2 corroborated and extended these findings in two ways. First, as in Experiment 1, the explicit valence ratings revealed a significant reduction of the EC effect in the irrelevant condition only. Second, the results obtained with the AMP suggested that this effect was mediated by the extent to which the CS were clear-cut instances of the experimental stimulus categories. The anticipated moderation of the extinction effect was reliable for salient but not for non-salient stimuli.

Interestingly, our findings also hint to the possibility that the impact of FSAA on the extinction rate of the EC effect might be two-fold. First, assigning selective attention to a stimulus dimension that is orthogonal to valence seems to promote a rapid decay of the EC effect. Second, both in Experiment 1 (i.e., evaluative ratings) and Experiment 2 (i.e., AMP), a reliable increase (not a decrease) of the EC effect was observed in the evaluative condition. That is, repeated evaluation of a CS seems to result in a strengthening of its evaluative tone (see Lewicki et al., 1992). Further research would be required, though, to substantiate the generality of this interesting finding.

Likewise, further research would be needed to shed light on the mechanism(s) underlying our effects. In fact, at least five different accounts can be given for the observation that the magnitude of the EC effect was affected by the nature of the classification task that was performed during the extinction phase of the experiment. As a first possibility, given that a clear-cut data pattern was obtained with the evaluative ratings but not with the AMP, one might simply argue that our results resulted from demand effects. For two reasons, however, this possibility seems unlikely. First, the anticipated pattern of results was present in the evaluative ratings but not in the US expectancy ratings. An explanation in terms of demand effects would predict the same pattern of results for both explicit measures as there is no obvious reason why participants would use their assumptions about the critical hypotheses to strategically bias their evaluative ratings but not their US expectancy ratings.

Second, additional linear mixed effect analyses showed that the EC effect in Experiment 1 (but not in Experiment 2) researched significance even if the US expectancy ratings were not in line with the actual CS-US pairings during the acquisition phase (see Pleyers, Corneille, Luminet, & Yzerbyt, 2007)[3]. It must be noted, however, that the vast majority of the US expectancy ratings did correspond with the actual CS-US parings (i.e., 90 %), so these results should be interpreted with caution. A second explanation of our findings implies that the association between a CS and its corresponding summary evaluation is subject to decay only if and to the extent that evaluative stimulus processing is hampered. Because the degree to which evaluative stimulus processing occurs is known to depend upon FSAA (see above) and assuming that decay of associations is promoted by events in which CSs are experienced without a concurrent emotional response, this account can readily explain why significant extinction emerged in the irrelevant condition only.

Third, it might be argued that occasion setting was responsible for the

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[3] To examine whether the EC effect was dependent upon (explicit) US expectancy, valence ratings were subjected to a linear mixed effects model. Fixed effects were CS type (positive vs. negative), US expectancy (in line with actual pairings vs. not in line with actual pairings), and the interaction between these factors. Participants and stimuli were defined as crossed random effects. The mixed-model F tests were computed using Kenward-Roger's adjusted degrees of freedom (Kenward & Roger, 1997). The analysis revealed a strong in interaction between CS and US expectancy both in Experiment 1, $F(1, 1486.07) = 252.15$, $p < 0.001$, and in Experiment 2, $F(1,1526.02) = 102.43$, $p < 0.001$. In Experiment 1, however, a significant EC effect emerged irrespective of whether the US expectancy ratings were in line with the actual CS-US pairings, $F(1, 1197.26) = 785.32$, $p < 0.001$, or were not in line with the actual CS-US pairings, $F(1, 241.93) = 24.18$, $p < .001$. In Experiment 2, a reliable EC effect was found only if the US expectancy ratings were in line with the actual CS-US pairings, $F(1, 1297.57) = 1142.80$, $p < 0.001$.

extinction effect observed in the irrelevant condition (see Rydell & Gawronski, 2009). According to this viewpoint, the requirement to assign attention to different stimulus properties during the acquisition and extinction phase of the experiment resulted in the formation of contextualized representations of CSs. Accordingly, it might be hypothesized that the EC effect might have surfaced again had we tested participants under conditions that promoted selective attention for the stimulus dimension that was task-relevant during acquisition (i.e., ABA renewal). The results obtained with the FSAA measure suggest that this possibility is certainly a viable route for future studies.

A fourth account is based on exemplar-based models of categorization and memory (Hintzman, 1984; Smith & Zarate, 1992). According to these models, when memory is probed with a target stimulus (e.g., a CS), memory traces of specific objects, persons, or experiences contribute to the overall memory response as a function of their similarity to the target stimulus. The stronger the overlap between a target stimulus and a particular exemplar representation, the stronger the influence of that exemplar representation on the memory response. Crucially, FSAA is assumed to determine the weight of each stimulus dimension in the computation of the similarity between the target stimulus and the exemplar representations (Smith & Zarate, 1992). One can thus expect a significant reduction of the EC effect for two reasons. First, because positive and negative CS categories were equivalent in terms of the stimulus dimension that was task-irrelevant during the acquisition phase, both positive and negative exemplars contributed to the overall memory response as soon as participants focused their attention on this stimulus dimension. Second, given that evaluative stimulus processing is reduced under conditions that promote selective attention for a neutral stimulus dimension, exemplar information stored during the extinction phase must have been relatively neutral in the irrelevant condition as compared to the relevant condition and the evaluative condition. In sum, the net memory response at the time of testing would thus be based on a mixture of neutral and non-neutral memory traces, thereby reducing the EC effect.

Finally, our findings may also be accounted for in terms of a propositional model of EC (see De Houwer, 2009). According to this framework, EC effects are mediated by propositional knowledge about the CS-US relation. One might thus argue that the emergence of an extinction effect must involve the formation of new propositions about the (absence of a) relationship between CSs and USs and/or the valence of the CSs. Crucially, such a corrective process would be more likely to occur in the irrelevant condition as compared to the relevant condition and the evaluative condition because participants experience the CS without a concurrent evaluative response only in the former condition.

In sum, while our findings suggest that the EC effect can be reduced by an extinction regimen that requires participants to assign attention to a stimulus dimension that is unrelated to stimulus valence, it remains an open question how this effect can be accounted for at the mental-process level. Nevertheless, our findings are important as EC procedures are increasingly used to modify likes and dislikes in applied settings (e.g., Houben, Schoenmakers, & Wiers, 2010). It seems particularly interesting, for example, to verify whether the outcome of exposure treatment programs is dependent upon the extent to which patients are encouraged to assign selective attention to nonevaluative stimulus information. It thus seems a viable approach to further scrutinize the underlying mechanisms and operating conditions of the extinction effect reported in the present paper.

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CHAPTER 5

Implicit evaluations as a function of Feature-Specific attention allocation[1]

We demonstrate that feature-specific attention allocation influences the way in which repeated exposure modulates implicit and explicit evaluations towards fear-related stimuli. During an exposure procedure, participants were encouraged to assign selective attention either to the evaluative meaning (i.e., Evaluative Condition) or a non-evaluative, semantic feature (i.e., Semantic Condition) of fear-related stimuli. The influence of the exposure procedure was captured by means of a measure of implicit evaluation, explicit evaluative ratings, and a measure of automatic approach/avoidance tendencies. As predicted, the implicit measure of evaluation revealed a reduced expression of evaluations in the Semantic Condition as compared to the Evaluative Condition. Moreover, this effect generalized toward novel objects that were never presented during the exposure procedure. The explicit measure of evaluation mimicked this effect, although it failed to reach conventional levels of statistical significance. No effects were found in terms of automatic approach/avoidance tendencies. Potential implications for the treatment of anxiety disorders are discussed.

[1] Based on Vanaelst, J., Spruyt, A., & De Houwer J. (2016). How to modify (implicit) evaluations of fear-related stimuli: Effects of feature-specific attention allocation. *Frontiers in Psychology*, 7, 717 doi: 10.3389/fpsyg.2016.00717

INTRODUCTION

Attitudes drive behavior (Allport, 1935) and are therefore often targeted as a leverage point for behavioral change. Importantly, behavior is determined not only by carefully constructed opinions of what we like or dislike but also by spontaneous evaluations that can take place under automaticity conditions (Fazio, 1990; Greenwald & Banaji, 1995). To promote behavioral change, it may thus be beneficial or even necessary to develop intervention strategies that allow for a change of these implicit evaluations. In line with this reasoning, it has been demonstrated that experimentally induced changes in the automatic evaluation of alcohol-related stimuli can result in a corresponding change in alcohol consumption (e.g., Houben, Havermans, & Wiers, 2010). Similar findings have been reported in the domain of implicit self-esteem (e.g., Baccus, Baldwin, & Packer, 2004; Clerkin & Teachman, 2010; Conner & Barrett, 2005), consumer research (e.g., Gibson, 2008) and social cognition (e.g., Rydell & Mcconnell, 2006).

In the present research, we examined the viability of a novel strategy to reduce implicit evaluations towards fear-related stimuli. This new approach is based on the observation that automatic evaluative stimulus processing is dependent upon Feature-Specific Attention Allocation (i.e., FSAA), that is, the amount of attention assigned to a specific stimulus feature such as valence, threat-value, gender, size, etc. As an example, consider the evaluative priming studies by Spruyt, De Houwer, & Hermans (2009; for related findings see Spruyt, De Houwer, Hermans, & Eelen, 2007; Spruyt, Klauer, Gast, De Schryver, & De Houwer, 2015; Spruyt & Tibboel, 2015; Spruyt, 2014; see also Kiefer & Brendel, 2006; Kiefer & Martens, 2010; Spruyt, De Houwer, Everaert, & Hermans, 2012). Evaluative priming studies typically consist of a series of trials in which participants are asked to respond to a target stimulus (e.g., a picture of a cute baby) that is preceded by a briefly presented prime stimulus (e.g., a picture of a spider). Crucially, the evaluative congruence of the prime-target pairs is manipulated: whereas both stimuli share the same evaluative connotation on some trials (e.g., a positive prime followed by a positive target), other trials consist of incongruent prime-target pairs (e.g., a positive prime followed by a negative target). A typical observation is a performance benefit in speed and/or accuracy for congruent trials relative to incongruent trials. This effect can come about only if participants process the evaluative tone of the primes and can therefore be used as an index of stimulus evaluation. Despite numerous studies attesting to the unconditional, automatic nature of this so-called 'evaluative priming effect' (see Klauer & Musch, 2003), Spruyt and colleagues demonstrated that the occurrence of this effect is restricted to conditions that maximize selective attention for the evaluative stimulus dimension. Moreover, adding to the generality of the FSAA framework, a number of recent studies confirmed that FSAA exerts similar effects on various other behavioral (Everaert, Spruyt, & De Houwer, 2013) and neuropsychological markers (Everaert, Spruyt, Rossi, Pourtois, & De Houwer, 2013) of implicit evaluation.

Based on the FSAA framework, one can identify two different pathways to reduce implicit evaluations towards fear-related stimuli. First, it may be hypothesized that experimentally induced changes in FSAA at time 1 can determine the likelihood that one engages in automatic processing of a stimulus feature at time 2. More specifically, the FSAA framework naturally predicts that evaluative responses towards fear-related stimuli at time 2 are less likely to come about in individuals who have learned to refrain from evaluative stimulus processing at time 1. Second, the impact of FSAA upon implicit evaluations may be exploited as a means to increase the efficacy of an extinction treatment. Research has repeatedly shown that evaluative responses are highly resistant to extinction (Craske, 1999; De Houwer, Thomas, & Frank, 2001; Hallion & Ruscio, 2011; Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010; Krypotos, Arnaudova, Effting, Kindt, & Beckers, 2015). This resistance-to-extinction could be due to the fact that, during an extinction treatment, the attitude object automatically evokes an evaluative response that consistently reaffirms the information acquired during the preceding evaluative learning episodes (Lewicki, Hill, & Czyzewska, 1992; Martin & Levey, 1978). The FSAA framework predicts, however, that an encounter with an attitude object is less likely to result in an evaluative response if attention is directed away from the evaluative stimulus dimension, thereby allowing for a potential disconfirmation of the preceding evaluative learning episodes (e.g., Lovibond, 2011, see also Sanbonmatsu, Posavac, Vanous, Ho, & Fazio, 2007). Accordingly, one may predict that the extinction rate of evaluative responses toward fear-related stimuli must be contingent upon the degree to which attention is assigned to other, non-evaluative (semantic) stimulus features.

To shed light on these issues, we conducted an exposure study in which FSAA was either directed towards or away from the evaluative stimulus dimension during the exposure phase. Participants were presented with a series of real-life pictures, the content of which varied along two orthogonal semantic dimensions: valence (positive vs. negative) and animacy (living vs. nonliving). Participants were asked to categorize all stimuli either as living vs. nonliving (i.e., the Semantic Condition) or as positive vs. negative (i.e., the Evaluative Condition). Participants in the Semantic Condition were thus encouraged to assign selective attention to the non-evaluative semantic features of the stimulus materials whereas participants in the Evaluative Condition were encouraged to assign selective attention to the evaluative tone of the stimulus materials. Crucially, the category of negative, living stimuli included pictures of spiders only, thereby allowing for a test of the hypothesis that a manipulation of FSAA can be exploited as a means to reduce evaluations towards fear-related stimuli.

To register the impact of this intervention strategy, we used both a measure of implicit evaluation (i.e., the Affect Misattribution Paradigm; Payne, Cheng, Govorun, & Stewart, 2005) and explicit evaluative ratings. In addition, because positive and negative evaluations are assumed to promote automatic approach and avoidance behavior, respectively (Krieglmeyer, Deutsch, De Houwer, & De Raedt, 2010; Solarz, 1960), we also included a Relevant-Stimulus Response Compatibility task aimed at capturing these motivational response tendencies (i.e., the R-SRC task; Mogg, Bradley, Field, & Houwer, 2003). We hypothesized that each of these measures would reveal less negative evaluations towards spiders in the Semantic Condition as compared to the Evaluative Condition. In addition, we included (novel) exemplars that were not presented during the exposure phase to examine the extent to which the impact of our manipulation would generalize to novel (transfer) stimuli.

METHOD

Participants

Sixty-one students of Ghent University (13 men, 48 women) participated in the experiment and received €5 in exchange for their help. In total, two participants in the Evaluative Condition and two participants in the Semantic Condition were excluded from analysis. One participant was excluded due to a technical error. Two other participants were excluded because their error rates in the R-SRC task (i.e., 22.66 % and 21.09 %) exceeded the outlier criterion of 2.5 standard deviations above the sample mean (*M* = 8.49 %, *SD* = 4.69 %). Finally, one participant was excluded because her mean reaction time in the R-SRC-task (i.e., 992 ms) exceeded the outlier criterion of 2.5 standard deviations above the grand mean (*M* = 713 ms, *SD* = 100 ms). Unless otherwise mentioned, results were not contingent upon inclusion or exclusion of these participants. The final sample consisted of 11 men and 46 women ranging between 18 and 36 years of age (*M* = 23.39, *SD* = 3.27). Power analyses revealed that, given this sample size, the power to detect a small effect (i.e., *Cohen's d* = 0.2), a medium-sized effect (i.e., *Cohen's d* = 0.5), or a large effect (i.e., *Cohen's d* = 0.8) was .12, .46, or .84, respectively. The reported research was conducted in accordance with the ethical standards of the institutional ethics committee and with the 1964 Helsinki declaration and its later amendments. All participants gave their informed consent prior to their inclusion in the study.

Materials

The stimulus materials used for the main dependent measures were eight positive and eight negative color pictures (328 pixels wide and 246 pixels high), 13 of which (i.e., eight positive and five negative) were chosen based on norm data collected by Spruyt, Hermans, De Houwer, and Eelen (2002). Several of these pictures originated from the International Affective Picture System (i.e., IAPS; Lang, Bradley, & Cuthbert, 2005). On a scale ranging from -5 (“very negative”) to + 5 (“very positive”), the mean valence rating of the negative stimuli was significantly smaller than zero, $M = -2.08$, $SD = 1.05$, $t(4) = -4.44$, $p < .05$. The mean valence rating of positive stimuli was significantly larger than zero, $M = 2.00$, SD = 0.72, $t(7) = 7.85$, $p < .001$. In addition to these IAPS pictures, three pictures of spiders were included. The final sample of 16 stimuli varied on two orthogonal semantic dimensions (i.e., valence and animacy), creating four stimulus categories. The category of living, negative stimuli was represented by four pictures of spiders. Each of the other three categories was represented by a mixture of pictures depicting different themes (see Appendix). For each individual participant, these 16 pictures were split in two semi-random subsets, each consisting of two pictures from each stimulus category. One of these subsets was used during the exposure phase of the experiment (hereafter referred to as experimental stimuli). The second set was used to test for transfer effects after the exposure phase (hereafter referred to as transfer stimuli).

For the AMP, 200 different Chinese pictographs served as target stimuli. All Chinese pictographs were presented in white and were 256 pixels wide and 256 pixels high. During the R-SCR-task, participants were asked to make a (white) manikin move away or towards the stimuli presented in the center of the computer screen (see below). The manikin was about 51 pixels wide and 79 pixels high.

All computer tasks were run on a Dell Optiplex GX520 computer. An Affect 4.0 program (Spruyt, Clarysse, Vansteenwegen, Baeyens, & Hermans, 2010) controlled the presentation of the stimuli as well as the registration of the responses. All stimuli were presented against the black background of a 19 inch computer monitor (100 Hz).

For exploratory reasons, we also administered a series of questionnaires. First, the Depression Anxiety Stress Scale (DASS, Lovibond & Lovibond, 1995) was used to measure levels of depression, anxiety and stress in the week preceding the experiment. The DASS consists of 42 statements (e.g., I found it difficult to relax) which are to be rated on a four-point Likert Scale ranging from 0 (not at all) to 3 (very much). The internal consistency of the DASS is typically very good, with Cronbach's alpha's for the different subscales ranging between .83 and .91 (de Beurs, Van Dyck, Marquenie, Lange, & Blonk, 2001). In the present sample, Cronbach's alpha's were .86, .89, and .88 for the anxiety, stress, and depression subscales, respectively. Second, to capture the extent to which participants tended to experience, on average, a positive or negative mood, they were asked to complete the Positive and Negative Affect Schedules (PANAS, Watson & Clark, 1988). Each mood scale included 10 mood descriptors (e.g., proud, guilty) and participants were asked to rate each item on a five-point Likert scale ranging from 1 (not at all) to 5 (extremely). The internal consistency of the PANAS is high, both for the English version (i.e., Cronbach's alpha's equal or larger than .80; Watson & Clark, 1988) and the Dutch version (i.e., Cronbach's alpha's equal or larger than .79; Engelen, De Peuter, Victoir, Van Diest, & Van Den Bergh, 2006). In the present sample, Cronbach's alpha equaled .85 for the positive subscale and .78 for the negative subscale. Third, to capture state anxiety, participants completed the Dutch version of the state anxiety subscale of the State Trait Anxiety Inventory (STAI-S, Spielberger, Gorsuch, & Lushene, 1970; Van der Ploeg, Defares, & Spielberger, 2000). Each item of the STAI-S (e.g., I feel frightened) was scored on a four-point Likert scale ranging from 1 (not at all) to 4 (very much). Both the original and the Dutch version of the STAI-S exhibit good internal consistency (i.e., Cronbach's alpha's equals or larger than 0.89; Barnes, Harp, & Jung, 2002; Van der Ploeg et al., 2000). In the present sample, Cronbach's alpha was .92. Finally, the Fear of Spiders Questionnaire (FSQ, Szymanski & Donohue, 1995) was administered to assess spider fear. The FSQ consists of 18 statements (e.g., I do anything to avoid a spider) which are to be rated on an eight-point Likert scale ranging from 0 (completely disagree) to 7 (completely agree). Both Szymanski and Donohue (1995) and Muris and Merkelbach (1996) reported very high internal consistency estimates for the FSQ (i.e., Cronbach's alpha's equal or larger than .92). Likewise, Cronbach's alpha in the present sample equaled .97.

Procedure

The experiment consisted of an exposure phase followed by an assessment phase (see Figure 1). During the exposure phase, the experimental stimuli were each presented 8 times in a random order (i.e., 64 trials). Participants were randomly assigned to either the Evaluative Condition (n = 28) or the Semantic Condition (n = 29). Participants assigned to the Evaluative Condition were asked to categorize these stimuli on the basis of their evaluative meaning (i.e., positive vs. negative). Participants assigned to the Semantic Condition were asked to categorize these stimuli in terms of the animacy dimension (i.e., living or not living). Selective attention for the evaluative stimulus dimension was thus maximized in the Evaluative Condition and minimized in the Semantic Condition.

Each trial started with the presentation of a fixation cross for 500 ms. Next, after an inter-stimulus interval of 500 ms, a stimulus was presented until a response was registered. Participants in the Evaluative Condition pressed the left key if the stimulus was negative and the right key if the stimulus was positive. Participants in the Semantic Condition pressed the left key if the stimulus depicted an object and the right key if the stimulus depicted a living creature. In case of an erroneous response, a 500-ms error message (i.e., 'FOUT!') appeared. The inter-trial interval varied randomly between 500 ms and 1500 ms.

During the subsequent measurement phase, participants first completed an AMP, modeled after the recommendations of Payne et al. (2005). Both the experimental stimuli and the transfer stimuli were used as primes and were presented once in an intermixed, random order (i.e., 16 trials in total). It may be

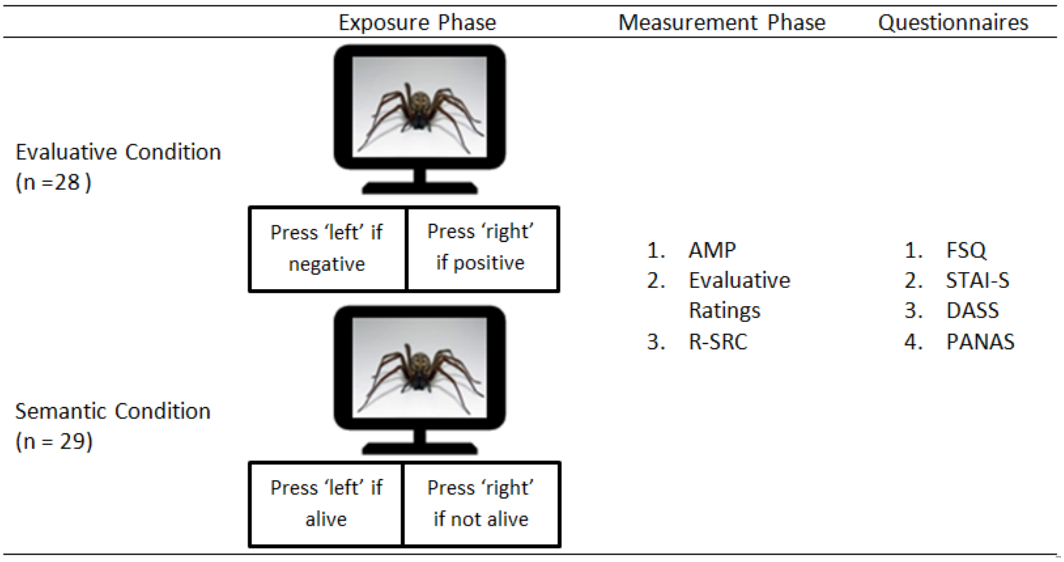

Figure 1. Schematic overview of the experimental procedure.

noted that we deliberately chose to implement a small number of AMP trials as the AMP requires participants to evaluate stimuli. Using a higher number of trials might thus have interfered with the attention manipulation. Each trial started with the presentation of a fixation cross for 500 ms, followed by an inter-stimulus interval of 500 ms and a 75-ms presentation of a prime stimulus. Next, 125 ms after the offset of the prime stimulus, a randomly selected Chinese pictograph was presented for 100 ms. Finally, immediately following the presentation of the Chinese pictograph, a black-and-white masking stimulus was presented until a response was registered. Participants were instructed to press the left key if they considered the Chinese pictograph to be less pleasant than the average Chinese pictograph and the right key if they considered the Chinese pictograph to be more pleasant than average. The inter-trial interval varied randomly between 500 ms and 1500 ms.

Following the AMP, participants were asked to rate the evaluative meaning of the experimental and the transfer stimuli using a rating scale ranging from -100 to + 100. Each stimulus was presented until a response was triggered and the trial list was completely random. The inter-trial interval varied randomly between 500 ms and 1500 ms.

Next, participants completed the R-SRC-task, modeled after Spruyt et al. (2013). On each trial, either an experimental stimulus or a transfer stimulus was presented in the middle of the computer screen. Simultaneously, a manikin was presented either below or above the position of the stimulus (i.e., counterbalanced across trials and individual stimuli). During a first block of trials, participants were asked to move the manikin away from positive stimuli and toward negative stimuli (i.e., incongruent trials) using the arrow keys of a standard computer keyboard. In a second block of trials, participants were asked to move the manikin away from negative stimuli and toward positive stimuli (i.e., congruent trials). They were allowed to move the manikin in any direction, but a loud beeping sound was delivered if the initial movement of the manikin was incorrect. A trial ended if the manikin reached either its highest or its lowest possible position in the accurate direction (i.e., the upper/lower edge of the computer or picture, ten steps in each direction). Each stimulus was presented exactly twice during each block, leading to a total of 64 trials. The inter-trial interval varied randomly between 500 ms and 1500 ms.

Finally, at the end of the experiment, participants were asked to complete the FSQ, the STAI-S, the DASS, and the PANAS (fixed order).

RESULTS

Preliminary analyses revealed that none of the critical effects was qualified by an interaction with stimulus type (i.e., experimental versus transfer stimuli) or animacy (i.e., living versus non-living stimuli), all *F*’s < 2.70. Accordingly, the data were collapsed across these variables. Note, however, that summary statistics for each cell of the design are provided in Table 1.

AMP scores were calculated by subtracting the proportion of pleasant judgments on trials depicting negative stimuli from the proportion of pleasant judgments on trials depicting positive stimuli. Likewise, evaluative rating scores were calculated by subtracting the mean rating of negative stimuli from the mean rating of positive stimuli. For the R-SRC task, individual scores were obtained using the so-called D600 algorithm (Greenwald, Nosek, & Banaji, 2003). First, all reaction times slower than 300 ms and higher than 10,000 ms were removed (0.16 %). Second, for each block of trials, reaction times observed on error trials (8.47 %) were replaced by the mean of correct latencies plus a 600-ms penalty. Third, a pooled SD was calculated based on correct trials and corrected error trials. Fourth, the mean response latency observed on congruent trials was subtracted from the mean response latency observed on incongruent trials. Finally, this difference score was divided by the pooled SD. For all dependent measures, higher values correspond with a more marked difference between positive and negative stimuli.

A bootstrapping approach was adopted to examine the reliability of the AMP effect and the R-SRC effect. For each measure and for each of 10,000 runs, the data of each individual participant were split in two equally-sized, random sets. Two AMP scores and two R-SRC scores were then calculated, one for each subset. Next, for each measure and for each individual run, the correlation between the two scores was computed and Spearman-Brown corrected. The split-half reliability coefficient was then obtained by computing the average of these 10,000 correlations. For the AMP effect, the split-half reliability coefficient equaled .76. The split-half reliability coefficient for the R-SRC effect was.71.

Overall, each of the three dependent measures revealed a more favorable evaluation of positive stimuli as compared to negative stimuli, $t(56) = 4.08$, $p < .001$, $d = 0.54$, $t(56) = 30.30$, $p < .001$, $d = 4.01$, $t(56) = 9.02$, $p < .001$, $d = 1.19$, for the AMP, the evaluative ratings and the R-SRC, respectively. More importantly, a one-way ANOVA with Condition as a between subjects factor revealed that the AMP scores were reliably different in both conditions, $F(1, 55) = 9.74$, $p < .01$, $\eta^2 = 0.15$. Follow-up analysis revealed a significant AMP effect in the Evaluative Condition, $t(27) = 4.70$, $p < .001$, $d = 0.89$, but not in the Semantic Condition, $t(28) = 1.10$, $p = .28$, $d = 0.20$.

Table 1.

Mean Scores for dependent measures as a function of Condition (SD's in parentheses).

Condition	Stimulus Type				
		Experimental Stimuli		Transfer Stimuli	
	All Stimuli	Living Creatures	Objects	Living Creatures	Objects
			AMP Scores		
Semantic	0.07 (0.36)**	-.03 (.63)*	.10 (.52)†	0.12 (.55)†	.10 (.54)**
Evaluative	0.42 (0.47)**	.41 (.65)*	.38 (.66)†	0.38 (.55)†	.52 (.55)**
			Evaluative Ratings		
Semantic	118.38 (30.55)	126.78 (54.53)	106.47 (30.21)	128.34 (40.24)	111.91 (37.87)
Evaluative	131.29 (30.77)	137.55 (41.29)	121.34 (39.18)	138.36 (37.78)	127.89 (36.73)
			R-SRC Scores (D600)		
Semantic	.46 (.44)	.71 (.67)†	.44 (.68)	.47 (.54)	.42(.56)
Evaluative	.49 (.36)	.42 (.59)†	.62 (.67)	.35 (.53)	.67(.72)

Note. †$p < .10$, *$p < 0.05$, **$p < 0.01$

Numerically, the rating data mimic these results, but the main effect of Condition failed to reach statistical significance, $F(1, 55) = 2.53$, $p = .12$, $\eta^2 = 0.04$. In contrast, the R-SRC scores did not reveal a reliable difference between the two conditions, $F < 1$ (see Table 1). For each of the dependent measures (i.e., evaluative ratings, AMP scores and R-SRC scores), we also examined whether the effect of the attention manipulation was moderated by inter-individual differences as measured by the questionnaires. While there was no evidence for such a moderation for the PANAS, the DASS, and the STAI-S, all *F*'s < 1, an ANCOVA did reveal a significant interaction between the Condition factor and the FSQ score, at least for the AMP data, $F(1, 53) = 4.72$, $p < .05$, $\eta^2 = .08$. Reassuringly, more extreme levels of spider fear were associated with a more pronounced difference in AMP Scores between the Evaluative and the Semantic Condition. A similar effect did not emerge for the evaluative ratings and the R-SRC data.

Finally, correlational analyses revealed a significant correlation ($r = 0.36$) between the AMP scores and the evaluative ratings, $t(55) = 2.86$, $p < .01$. In contrast, neither the AMP scores nor the evaluative ratings correlated with the R-SRC scores, *t'*s < 1. Interestingly, the correlation between the FSQ scores and the AMP scores was substantial in the Evaluative Condition, $r = 0.47$, $t(26) = 2.70$, $p < .05$. More extreme levels of spiders fear were associated with more extreme AMP Scores. The correlation between the AMP scores and the FSQ scores did not reach significance in the Semantic Condition, $r = .06$, $t < 1$.

DISCUSSION

The aim of the present research was to test the viability of a new method to reduce the negativity of the (implicit) evaluation of fear-related stimuli. Based on the FSAA-framework developed by Spruyt and colleagues (Everaert, et al., 2013; Spruyt et al., 2007, 2009), it was hypothesized that the requirement to engage in a non-evaluative processing style during an exposure procedure would impact measures of evaluation during a subsequent measurement phase, for two reasons. First, the attentional focus on non-evaluative stimulus information may carry over from the exposure phase to the test phase, thereby reducing the likelihood and/or intensity of the evaluative response towards fear-related stimuli. Second, participants may be more likely to experience corrective emotional information during an exposure procedure if the likelihood and/or intensity of an evaluative response is minimized during the exposure phase.

To obtain a proof-of-principle for these ideas, we conducted an exposure study in which participants were asked to categorize fear-related pictures (e.g., pictures depicting spiders) either in terms of their evaluative meaning (i.e., Evaluative Condition) or in terms of the animacy dimension (i.e., Semantic Condition). Participants were thus encouraged to assign selective attention to the evaluative and non-evaluative semantic features of stimuli, respectively. In line with our predictions, we observed that implicit evaluations as measured by the AMP were less pronounced in the Semantic Condition as compared to the Evaluative Condition, both for stimuli used during the manipulation phase and novel transfer stimuli. A similar result was obtained with the explicit evaluative ratings, although it must be noted that this effect was statistically not unequivocal (p = .12, but see Footnote 1). Given that the AMP and the explicit evaluative ratings were substantially correlated, however, we are inclined to attribute the absence of a reliable effect in the explicit valence ratings to a Type-II error.

Interestingly, we also observed that the correlation between the AMP scores and the FSQ scores (i.e., an explicit measure of spider fear) was dependent upon our experimental manipulation. Whereas a strong correlation was found in the Evaluative Condition (r = .47), there was no evidence for such a relationship in the Semantic Condition (r = .06). This finding strengthens our claim that FSAA can modulate the automatic evaluation of fear-related stimuli as it suggests that individual differences in automatic evaluation were picked up reliability by the AMP in the Evaluative Condition but not in the Semantic Condition.

The current findings are important because they shed new light on the mixed results that have been reported in the field of Attention Bias Modification (i.e., ABM; Beard, Sawyer, & Hofmann, 2012; Hertel & Mathews, 2011; MacLeod, Rutherford, Campbell, Ebsworthy, & Holker, 2002). In a typical ABM procedure, participants are encouraged to divert their spatial attention away from fear-related stimuli using an adapted version of the dot-probe task (MacLeod, Mathews, & Tata, 1986). Participants are presented with two briefly presented stimuli (i.e., cues), one of which is a fear-relevant stimulus whereas the other is emotionally neutral. On the majority of the trials, the emotionally neutral stimulus is replaced by a visual probe that requires a response. On the remaining trials, the probe is preceded by the fear-related stimulus. It is expected that the predictive relationship between the nature of the cue and the probe causes participants to selectively direct their spatial attention away from threatening stimuli (Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg, & van IJzendoorn, 2007), thereby promoting therapeutic change. Whereas several studies attesting to the therapeutic value of ABM training have appeared in the literature (e.g., Amir, Weber, Beard, Bomyea, & Taylor, 2008), some authors reported that they were unable to obtain supporting evidence for the idea that ABM training can reduce attention bias and subsequent vulnerability to psychological stressors (e.g., Julian, Beard, Schmidt, Powers, & Smits, 2013). Recent meta-analytical studies also raised concern about the therapeutic efficacy of ABM training (Hallion & Ruscio, 2011; Heeren, Mogoase, McNally, Schmitz, & Philippot, 2015; Mogoaşe, David, & Koster, 2014).

Importantly, this mixed pattern of results is readily accounted for on the basis of the FSAA framework. According to this framework, different stimulus dimensions attract attention as a function of current goals and task demands. In a traditional ABM training, attending to the threat value of the cues is beneficial for the task at hand as soon as the difference between threatening and neutral cues is predictive for the location of the target probes. As a result, somewhat ironically, one can expect participants to assign selective attention to the difference between threatening and neutral stimuli as soon as they pick up a contingency between the threat value of the cues and the location of the targets. The observation that successful attempts to change attention bias were not always accompanied by corresponding changes in symptoms (Browning, Holmes, Murphy, Goodwin, & Harmer, 2010) or even increased reported symptomatology (Baert, De Raedt, Schacht, & Koster, 2010) is consistent with this viewpoint.

The logic developed here differs from the ABM approach in the sense that participants are (a) encouraged to assign spatial attention to fear-relevant stimuli while (b) prioritizing non-evaluative (semantic) stimulus processing over evaluative stimulus processing. Likewise, there is a marked difference between the current approach and the Emotional Processing Theory (i.e., EPT) of exposure therapy (Foa & Kozak, 1986). According to EPT, fear is represented in a fear structure that can be modified only if it is activated. Accordingly, therapeutic sessions often comprise a controlled confrontation with the fear-evoking stimulus, either in vivo or in vitro. Therapeutic change is then expected to occur only if and to the extent that participants can integrate corrective information during such an experience. In line with such an approach, the intervention developed here requires participants to focus spatial attention on a threat-evoking stimulus. Nevertheless, our approach is novel in the sense that participants were encouraged to selectively process non-evaluative instead of evaluative stimulus features. As demonstrated by the present findings, this approach may provide an additional means to combat pathological fear, but we hasten to confirm that more research would be needed to firmly substantiate this claims.

Further research would also be needed to deal with two limitations of our study. First, it is insufficiently clear why exactly the R-SRC task failed to pick up a difference between the Evaluative Condition and the Semantic Condition. Importantly, given that the overall R-SRC effect did reach significance in both conditions (i.e., performance was consistently better in the compatible block as compared to the incompatible block), we can safely rule out the possibility that the specific version of the R-SRC task used in this study was simply unsuited to detect automatic approach/avoidance tendencies. It also seems unlikely that a (successful) manipulation of FSAA would selectively affect the (implicit) evaluation of a stimulus but not the degree to which this stimulus triggers automatic approach/avoidance tendencies. After all, the (automatic) evaluation of a stimulus can be defined as a necessary precursor of the (automatic) tendency to approach or to avoidance that stimulus (Deutsch & Strack, 2006; Gawronski & Bodenhausen, 2006). Therefore, as an alternative explanation, we suspect that the temporal order of the implicit measures may have been critical. The AMP was always performed first, followed by the evaluative rating task and the SRC-task, respectively. We deliberately opted for a fixed order of assessment tasks because (a) we were primarily interested in the influence of FSAA on implicit evaluations (i.e., the AMP) and (b) we wanted to avoid carry-over effects from other tasks while participants completed the AMP. However, because all three dependent measures required participants to evaluate stimuli, one might argue that each of these tasks may have counteracted the effects of the experimental manipulation to some degree. As a logical consequence, it could be argued that the effects of our exposure procedure were abolished by the time participants completed the R-SRC- task. It would thus be interesting to replicate the present experiment while counterbalancing the order of the measurement tasks. Alternatively, it could be worthwhile to use an adaptation of the R-SRC that is semantically neutral. For example, participants might be asked to respond on the basis of the picture format of the target stimuli (e.g., portrait vs. landscape, see Reinecke, Becker, & Rinck, 2010).

As a second limitation of our study, one may argue we restricted our sample to non-clinical, unselected participants. It thus remains an open question whether the current findings would replicate in a clinical sample. It may be noted, however, that the effect of FSAA was slightly larger, not smaller, when the analyses were restricted to data stemming from participants with elevated levels of (self-reported) fear of spiders (i.e., FSQ Scores > 55, see Huijding and de Jong (2006), $F(1, 19) = 15.41$, $p < .001$, $\eta^2 = .45$. More research will be necessary, however, to document the clinical validity of the current findings as well as the life-time of the extinction effect observed in the present study.

These limitations notwithstanding, our findings support the idea that (implicit) evaluations become less intense if participants are encouraged to assign attention to (non-evaluative) semantic stimulus information. This effect was found for generic evaluative stimuli and fear-related stimuli alike and transferred to non-trained exemplars. More research is needed, however, to establish the generality of this effect, its boundary conditions, and underlying mechanisms.

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APPENDIX

Description of stimulus materials used for the main dependent measures

Pictures depicting negative objects: car, gun, garbage, fire

Pictures depicting negative beings: spider, spider, spider, spider

Pictures depicting positive objects: balloons, lollipop, present, air balloon

Pictures depicting negative beings: dolphin, squirrel, baby, kitten

CHAPTER 6

GENERAL DISCUSSION

INTRODUCTION

Ever since Zajonc (1980) proclaimed that 'preferences need no inferences', numerous researchers have systematically investigated the hypothesis that all incoming evaluative information is processed in an automatic and unconditional fashion. (e.g., Arnold, 1960; Barlett, 1932; Lazarus, 1966; Wundt, 1907). Experimental support for the automatic evaluation hypothesis has grown steadily over the last decades, both in the behavioral sciences (e.g., Fazio, 2001) and the affective neurosciences (e.g., Vuilleumier, 2005). Recent studies have shown, however, that automatic evaluative processing is not unconditional but is moderated by feature-specific attention allocation (hereafter referred to as FSAA; Spruyt, De Houwer, & Hermans, 2009; Spruyt, De Houwer, Hermans, & Eelen, 2007; Spruyt, Klauer, Gast, De Schryver, & De Houwer, 2015; Spruyt & Tibboel, 2015; Spruyt, 2014). Generally, the FSAA framework states that stimulus dimensions are processed only if and to the extent that they are selectively attended to. Thus, evaluative processing will only take place to the extent that selective attention is allocated to evaluative stimulus features. Corroborating the FSAA framework, Spruyt et al. (2009) showed that the evaluative priming effect, a classic marker of automatic evaluative stimulus processing, occurs only if attention to the evaluative stimulus dimension is maximized. Since this seminal finding, it has been shown that FSAA exerts similar effects on various other markers of automatic evaluative stimulus processing (Everaert, Spruyt, & De Houwer, 2011, 2013, 2016; Everaert, Spruyt, Rossi, Pourtois, & De Houwer, 2013; Spruyt, De Houwer, Everaert, & Hermans, 2012; see also Kiefer & Brendel, 2006; Kiefer & Martens, 2010; Kiefer, 2012; Martens & Kiefer, 2009 for related findings in the non-evaluative semantic domain).

The idea that automatic evaluative processing is moderated by FSAA implies that FSAA may also moderate phenomena that are known to depend on evaluative processing and the extant literature is consistent with this viewpoint. For example, Gast and Rothermund (2011) showed that the acquisition of new likes and dislikes through evaluative conditioning (i.e., EC) can be modulated by

FSAA. Participants were presented with a mixture of EC trials and categorization trials. During the categorization trials, participants were asked to categorize the CS-US pair based on their evaluative features or non-evaluative semantic features and were thus encouraged to selectively attend either to evaluative or non-evaluative stimulus features, respectively. The authors observed a reliable EC effect if participants assigned selective attention to evaluative features during the conditioning phase. However, the EC effect was absent if participants assigned attention to non-evaluative stimulus features (see also Olson, Kendrick, & Fazio, 2009). In this dissertation project, we examined the moderating role of FSAA on both the measurement and modification of (implicit) likes and dislikes. First, it was hypothesized that FSAA would be a moderating factor in the predictive validity of implicit attitude measures as it has been show that FSAA is not only influenced by explicit task instructions (Everaert, 2012; Spruyt et al., 2009) but also by personal goals. Several studies have revealed stable inter-individual differences in the extent to which FSAA is deployed to the evaluative meaning of attitude-relevant stimuli. For example, it has been shown that spider-fearful participants assign greater weight to the evaluative meaning of spider-related objects than control participants (Cavanagh & Davey, 2001). In addition, individuals with symptoms of bulimia assign more weight to stimulus dimensions reflecting body size compared to control participants (Viken, Treat, Nosofsky, McFall, & Palmeri, 2002, see also Fazio & Dunton, 1997; Mogg, Mathews, & Weinman, 1989). Thus, there seem to exist important inter-individual differences in the extent to which evaluative stimulus features of attitude-relevant objects are automatically processed. In a second research line, we examined whether FSAA moderates the extinction rate of implicit likes and likes, and therefore the treatment of certain classes of psychopathology (i.e., specific phobia). Various studies have shown that likes and likes are resistant to extinction (Craske, 1999; Hallion & Ruscio, 2011; Krypotos, Arnaudova, Effting, Kindt, & Beckers, 2015). One possible reason is that the spontaneous evaluative response evoked by an attitude object consistently reaffirms the evaluative tone of this object (Lewicki, Hill, & Czyzewska, 1992; Martin & Levey, 1978). Based on the FSAA-framework one can predict that participants will process the valence of an attitude-object to a lesser degree if they assign attention to another (non-evaluative) stimulus dimension. Consequently, the degree of extinction of likes and dislikes would be dependent on the degree to which participants assign selective attention to non-evaluative stimulus dimensions.

In sum, based on the FSAA framework of Spruyt and colleagues, we hypothesized that FSAA would moderate various phenomena that are dependent on evaluative processing. Over the course of this dissertation project, we systematically scrutinized the moderating role of FSAA on both the measurement and modification of (implicit) likes and dislikes. First, I will give a short overview of the experiments we conducted and the data we collected during the dissertation project. Second, I will interpret the data in the discussion section of this Chapter.

OVERVIEW

After the general introduction presented in **Chapter 1**, we described two experiments aimed at testing the hypothesis that FSAA moderates the predictive validity of evaluative priming scores (**Chapter 2**). In Experiment 1, a picture-picture naming task designed to measure likes and dislikes towards fruit and candy, was presented on half of the trials. On the other half of the trials, target pictures were surrounded with a colored rectangle. In these induction trials, participants were asked to categorize target pictures either in terms of valence (i.e., evaluative condition), animacy (i.e., semantic condition), or the color of the rectangle (i.e., non-semantic condition). Participants were thus encouraged to assign attention to evaluative stimulus features, non-evaluative semantic features, or perceptual features, respectively. At the end of the experiment, participants were given the choice between a piece of fruit or a piece of candy as a little thank-you-present. As expected, we observed that predictive validity was more pronounced in the semantic condition than in the non-semantic condition or evaluative condition. Surprisingly, however, the relation between inter-individual difference scores obtained in the semantic condition and behavior was in the opposite direction of what was predicted. The more inter-individual difference scores revealed a preference for fruit over candy, the higher the likelihood that participants would choose a piece of candy at the end of the experiment.

In Experiment 2, participants performed both an evaluative categorization task and a semantic categorization task aimed at examining implicit evaluations towards spiders. Thus, participants were encouraged to assign selective attention to evaluative stimulus features in one task and to (non-evaluative) semantic stimulus features in the other task. At the end of the experiment, participants performed a behavioral assessment task (i.e., BAT) designed to measure behavioral avoidance towards a spider. In line with the findings in Experiment 1, we observed higher predictive validity in the semantic decision task as compared to the evaluative decision task, at least if the semantic decision task was performed prior to the evaluative decision task. Interestingly, the relationship between inter-individual difference scores and behavior was again counterintuitive. The more inter-individual difference scores indicated a profound dislike of spiders, the more likely that participants were prepared to approach an (allegedly living) spider. The within-subjects design of Experiment 2 also enabled us to test a second hypothesis. Building further on the FSAA framework, we hypothesized that spider-fearful participants would process the evaluative features of spider-related stimuli independent of whether they were encouraged to assign selective attention to evaluative stimulus features or to non-evaluative stimulus features. Consequently, we did not expect a significant difference between inter-individual difference scores obtained in the evaluative categorization task and the semantic categorization task in spider-fearful participants. In contrast, we hypothesized that individuals in the control group would process the evaluative features of stimuli depicting spiders only if and to the extent that they were encouraged to assign attention to these evaluative features. Therefore, we did expect a reliable difference between inter-individual difference scores obtained in the evaluative categorization task and semantic categorization task in the control group. Interestingly, results were again opposite to what we had expected. Whereas we did not observe a significant difference between inter-individual difference scores obtained in the semantic and the evaluative categorization task in the control group, a reliable difference between these scores was found in the spider-fearful group.

In sum, the results in Chapter 2 were twofold. On the one hand, we observed preliminary evidence that predictive validity in the evaluative priming task is dependent on FSAA. Whereas inter-individual difference score obtained in the evaluative decision task and outcome measures were unrelated, we did observe a reliable relation between outcome measures and inter-individual difference scores obtained in a non-evaluative semantic decision task. On the other hand, the predictive relationship in the non-evaluative semantic decision task was opposite to what we had expected. Inter-individual difference scores indicative of a clear liking were associated with avoidance behavior whereas inter-individual difference scores indicating clear dislikes were associated with approach behavior.

In **Chapter 3**, we describe a meta-analysis in which we again examined the hypothesis that FSAA moderates the predictive validity of implicit measurement techniques. To this end, predictive validity in both the evaluative priming paradigm (e.g., Fazio, Sanbonmatsu, Powell, & Kardes, 1986) and the approach-avoidance task was examined (i.e., AAT; Solarz, 1960). Note that we deliberately opted to include only these two paradigms in the analysis as we wanted to compare implicit measures that involve an evaluative focus with implicit measures that prevent such a processing goal. Both the evaluative priming paradigm and the AAT meet this criterion because participants can be asked to respond to task-relevant stimuli either based on their evaluative features or based on their non-evaluative features. An example of an implicit measure that does not meet the criterion is the implicit association task (i.e., IAT, Greenwald, McGhee, & Schwartz, 1998) as it is inherent to this paradigm that participants evaluate (at least a number of) task-relevant stimuli. Again, we predicted that the predictive validity would be higher for implicit measures that maximize selective attention assignment to non-evaluative stimulus features as compared to tasks that encourage participants to assign attention to evaluative stimulus features. Moreover, we examined whether the effect of FSAA on predictive validity was modulated by the personal relevance of attitude objects. Whereas we did not expect relevance to be an important moderator of the predictive validity in assessment tasks that maximize attention to evaluative stimulus features, we did hypothesize that personal relevance would impact predictive validity in attitude measurement tasks that maximize selective attention for non-evaluative stimulus features.

Overall, the meta-analysis revealed no evidence supporting the hypothesis that the predictive validity of implicit attitude measures is contingent upon the degree to which participants are encouraged to assign selective attention to evaluative features. Moreover, the effect of FSAA was not moderated by the personal relevance of the attitude objects. These results were obtained both for the AAT and the evaluative priming paradigm. However, it should be noted, that the number of studies focusing on implicit attitudes towards goal-relevant attitude objects were extremely limited. Therefore, one could argue that we lacked sufficient power to evaluate the hypothesis that FSAA moderates the predictive validity of the evaluative priming task. In sum, Chapter 2 and Chapter 3 provided mixed evidence concerning the moderating role of FSAA on the predictive validity of implicit attitude measurement techniques. These findings will be discussed in detail in the discussion section of the current Chapter.

In **Chapter 4**, we set out to demonstrate that FSAA can moderate the extinction rate of recently acquired likes and dislikes. In two experiments, participants learned new likes and dislikes through an evaluative conditioning procedure. Conditioned Stimuli (i.e., CSs) were abstract Gabor patches that varied along two orthogonal, perceptual dimensions; i.e., orientation and spatial frequency. During the acquisition phase, one of these dimensions was predictive of the valence of the unconditioned stimuli (i.e., US) and participants were thus encouraged to assign selective attention to this dimension. In a following attention manipulation phase, CS were presented alone. Participants were asked to categorize CSs according to their evaluative features (i.e. evaluative condition), the stimulus dimension that was correlated with valence during the acquisition phase (i.e., relevant condition), or the stimulus dimension that was unrelated to valence (i.e. irrelevant condition). Experiment 1 and Experiment 2 were identical, except for the fact that, in Experiment 2, a direct measure of FSAA was included at the end of the experiment. We expected a reduction in the expression of likes and dislikes to take place in the irrelevant condition but not in the evaluative condition. Because participants in the relevant condition were encouraged to assign attention to a perceptual feature that was correlated with valence, we expected the reduction in the expression of likes and dislikes to be less pronounced in the relevant condition relative to the irrelevant condition.

In line with our hypotheses, explicit measures revealed a linear reduction of the EC effect as selective attention to non-evaluative stimulus features of the CSs was maximized during the attention manipulation phase. In both experiments, we observed a reduction in the expression of likes and dislikes in the irrelevant condition, but not in evaluative condition or the relevant condition. In Experiment 1, the implicit attitude measure corroborated this finding, although the linear effect just missed conventional significance levels (p = .06). The results of Experiment 2 suggested that the effect of FSAA on the implicit measure of likes and dislikes was moderated by the extent to which the CS were clear-cut instances of the experimental stimulus categories in the sense that the anticipated effect of FSAA was reliable for salient but not for non-salient stimuli. Interestingly, both in Experiment 1 (i.e., explicit measure) and Experiment 2 (i.e., implicit measure), a significant increase of the EC effect from post-acquisition to post-attention manipulation was observed in the evaluative condition. This observation is in perfect accordance with the hypothesis that reactivation of emotional tone of the CS can strengthen the acquired valence of the CS (see Lewicki, Hill, & Czyzewska, 1992).

Building further on the findings in Chapter 4, we examined whether FSAA can also impact implicit evaluations of fear-related stimuli (**Chapter 5)**. Participants were presented with a series of real-life pictures depicting generic positive or negative contents. Stimuli varied on two orthogonal dimensions:

valence (positive versus negative) and animacy (living versus nonliving). Crucially, the category of negative, living stimuli included pictures of spiders only. During an attention manipulation procedure, participants were asked to categorize stimuli either according to their valence (i.e. evaluative condition) or animacy (i.e. semantic condition) and were thus encouraged to assign selective attention to evaluative stimulus features or non-evaluative stimulus features, respectively. We expected reduced expression of likes and dislikes in the semantic condition compared to the evaluative condition.

Confirming our hypothesis, implicit likes and dislikes as captured by the AMP were less pronounced in the semantic condition as compared to the evaluative condition. Importantly, this effect generalized toward novel stimuli that were never presented during the attention manipulation procedure. An explicit measure of likes and dislikes corroborated this finding, although it missed conventional levels of statistical significance (p = .12). However, a relevant stimulus-response compatibility tasks designed to capture automatic approach-avoidance tendencies failed to pick up a difference between the evaluative condition and the semantic condition.

In sum, Chapter 4 and Chapter 5 provide convincing evidence that FSAA can impact the extinction of likes and dislikes. However, the effects of the FSAA manipulation seem to be short-lived as they were reliable only when the attitude measurement was administered immediately following the manipulation. These findings as well as possible implications for therapeutic interventions are discussed at length in the following section.

DISCUSSION

The effect of FSAA on the predictive validity of implicit attitude measures

In a first line of research, we examined the moderating role of FSAA on the relationship between inter-individual difference scores obtained with implicit attitude measures and behavioral outcome measures. Taken together, our findings do not allow for a firm conclusion regarding the impact of FSAA on the predictive validity of implicit attitude measures, for three reasons. First, while we did observe an impact of FSAA on the predictive validity of evaluative priming scores in two empirical studies, the relationship between behavior and the evaluative priming scores observed in these studies was opposite to what we had expected (i.e., Chapter 2). Implicit likes were associated with avoidance behavior whereas implicit dislikes were associated with approach behavior. Second, we hypothesized that the extent to which evaluative stimulus features are spontaneously processed depends upon the personal relevance of the attitude objects. For goal-relevant objects, evaluative stimulus processing was expected to take place regardless of whether participants focused attention on the evaluative stimulus dimension. Conversely, attitude objects that are personally unimportant were expected to trigger automatic evaluative stimulus processing only under conditions that promoted selective attention for the evaluative stimulus dimension. However, the opposite data pattern was observed in Chapter 2. That is, FSAA impacted the implicit evaluation of goal-relevant attitude objects, not the implicit evaluation of goal-irrelevant attitude objects. Third, a meta-analysis revealed no impact of FSAA on the relationship between behavior and inter-individual difference scores obtained with either the AAT or the evaluative priming paradigm (i.e., Chapter 3).

However, there are a few caveats that should be discussed before one can conclude that FSAA does not moderate the predictive validity of the evaluative priming paradigm. First, the number of studies reported in the literature in which the evaluative priming paradigm was used to capture implicit attitudes towards goal-relevant attitude objects is extremely scarce. It could thus be argued that a different pattern may have occurred had we been able to include more studies in the meta-analysis. In this respect, it is important to emphasize that we obtained strong evidence in support of the hypothesis that the literature concerning the predictive validity of evaluative priming measures is plagued by a publication bias. This observation not only limits the scope of the meta-analysis, it also sheds new light on the absence of a meaningful relationship between behavior and the evaluative priming scores obtained with the evaluative decision task in Chapter 2. Instead of two unlucky failures to replicate, our observation might very well reflect the true state of affairs, i.e., that the evaluative priming scores obtained with the evaluative decision task lack the validity currently suggested in the literature.

Second, it deserves note that, in two independent experiments, the predictive validity of evaluative priming scores was found to be more pronounced under conditions that promoted participants to assign selective attention to non-evaluative semantic stimulus features (i.e., animacy) as compared to conditions that promoted participants to assign selective attention to evaluative stimulus information (i.e., Chapter 2). On the one hand, the results of the meta-analysis seem to contradict this pattern as no effects of FSAA were found. On the other hand, it must be noted that the vast majority of studies in which evaluative priming scores were obtained in the absence of an evaluative processing goal were studies in which a non-evaluative semantic processing goal was also absent. In the lexical decision task (Campos-Melady & Smith, 2012), for example, participants, are simply asked to decide whether target stimuli are words or not. Overall, predictive validity might thus be more pronounced in studies in which participants are encouraged to assign attention to non-evaluative semantic features compared to studies in which participants are encouraged to assign selective attention to evaluative stimulus features or non-evaluative non-semantic stimulus features.

Third, our results did not confirm the hypothesis that FSAA moderates the predictive validity of the AAT. A meta-analysis did not reveal a significant difference in predictive validity between studies in which attention to evaluative features was maximized and studies in which attention to non-evaluative stimulus features was maximized (Chapter 3). However, the AAT studies that met the criteria for inclusion in the meta-analysis were either studies in which selective attention to evaluative stimulus was maximized or studies in which selective attention to non-evaluative non semantic stimulus features (i.e., the format or orientation of stimuli) was maximized. Thus, in accordance with our findings in the evaluative priming task, it is possible that predictive validity in the AAT would be most pronounced under conditions that maximize attention to non-evaluative semantic stimulus features as compared to conditions that maximize selective attention to evaluative stimulus features or non-evaluative non-semantic stimulus features.

In sum, in order to conclude that FSAA does *not* moderate the predictive validity of implicit measurement techniques, much more research would be needed. First, more research is needed to substantiate the idea that the predictive validity of evaluative priming scores is dependent upon the degree of goal-relevance of the attitude objects. Second, additional research is necessary to examine the predictive validity of implicit attitude measures in which selective attention to non-evaluative semantic stimulus features was maximized. Finally, as the relationship between behavior and inter-individual difference scores obtained in the semantic decision task was in the opposite direction of what was expected, further research will be needed to systematically scrutinize the underlying mechanisms of this effect as well as its boundary conditions. As was discussed in Chapter 2, one explanation for this unexpected findings is that the requirement to assign selective attention to non-evaluative semantic stimulus features resulted in a reversal of the evaluative priming effect (i.e., participants responded faster to incongruent trials compared to congruent trials, see Klauer, Teige-Mocigemba, & Spruyt, 2009 for an overview). To ascertain that the counterintuitive relation between behavior and inter-individual difference scores in the semantic decision task was indeed due to a reversed priming effect in attitude-relevant stimuli, one could experimentally manipulate the relevance of generic positive and negative stimuli, for example, by pairing stimuli with either a high or low reward (see Shen and Chung, 2011). One would expect a reversed evaluative priming effect in the semantic decision task for goal-relevant stimuli but a standard evaluative priming effect for goal-irrelevant stimuli.

Lifetime of the FSAA manipulation: spontaneous extinction or reinstatement?

During the dissertation project, we provided convincing evidence that FSAA can modulate the extinction of likes and dislikes. Participants performed an attention manipulation phase in which they were encouraged to repeatedly process evaluative stimuli in a non-evaluative manner or an evaluative manner. Next, likes and dislikes were captured by an explicit and an implicit attitude measure. In Chapter 4, we observed a reduction of the evaluative conditioning effect (i.e., EC) obtained by an explicit attitude measure (i.e. evaluative ratings) if participant attended to non-evaluative semantic features during the attention manipulation phase. In contrast, we did not observe a reduction of the EC effect if participants were encouraged to assign attention to evaluative information. The same pattern was observed using an implicit measure of likes and dislikes (i.e., the AMP), although the effect missed conventional levels of significance (i.e., Experiment 1) or emerged only if the analysis was restricted to CSs that were clear-cut examples of the stimulus categories (i.e., Experiment 2). Interestingly, the results of Chapter 5 revealed a clear extinction of likes and dislikes measured by an implicit measure of likes and dislikes (i.e., the AMP) if participants repeatedly categorized stimuli according to their non-evaluative stimulus features during the preceding attention manipulation phase. However, the effect of the FSAA manipulation failed to reach conventional levels of significance on a measure of explicit evaluations (i.e., evaluative ratings). Thus, whilst the effect of the attention manipulation was most pronounced in an explicit measure of likes and dislikes in Chapter 4, the attention manipulation effect was most pronounced in an implicit measure of likes and dislikes in Chapter 5. These inconsistent findings could be explained if one takes the order in which the explicit and implicit attitude measures were administered into account. In Chapter 4, participants first performed an explicit measure of likes and dislikes (i.e., evaluative ratings) before completing an implicit attitude measure (i.e., the AMP). In Chapter 5, the order of these two tasks was reversed. Participants first performed an implicit attitude measure (i.e., the AMP) which was immediately followed by an explicit attitude measure (i.e., evaluative ratings). Thus, it seems that the effect of the FSAA manipulation is most pronounced in the attitude measure that is administered immediately following this manipulation, suggesting that the attention manipulation only induced short-term effects. However, further research will be necessary to ascertain that the effect of the attention manipulation was indeed dependent on the order in which attitude measurements were administered. For example, one might counterbalance the presentation order of the attitude measurement tasks in our studies. The hypothesis that the effect of FSAA was moderated by the presentation order of the attitude measures would be supported if the effect of the attention manipulation would be moderated by the counterbalancing factor in both the explicit and implicit measure.

Similar volatile effects of attention manipulation have been observed in an unpublished study of Spruyt and Van Bockstaele. The authors used an adapted version of the dot probe paradigm (see Van Bockstaele, Koster, Verschuere, Crombez, & De Houwer, 2012) to simultaneously measure and influence FSAA. In the standard dot probe task (MacLeod, Mathews, & Tata, 1986), two cue stimuli are presented simultaneously at different locations on the computer screen. One of the cues is threatening whereas the other is not. After the cues disappear, a target stimulus is presented at the location of the neutral cue (i.e., incongruent trial) or the location of the threatening cue (i.e., congruent trial). Typically, responses to target stimuli are faster and lead to less errors in congruent as compared to incongruent trials. In the adapted dot-probe version of Spruyt and Van Bockstaele, cues were presented either in landscape or portrait format. During the attention manipulation phase, the format of the cue was predictive of the target location. Thus, participants were encouraged to assign attention to the non-evaluative feature of the cues. Immediately following the attention manipulation phase, participants performed a measurement phase in which the format of the cue were no longer predictive of the target location. Results revealed that the attention manipulation was successful in inducing selective attention to the format of the cues during the attention manipulation phase. However, selective attention to the format of the cue disappeared as soon as cue-format was no longer predictive of target location.

Interestingly, there are indications that selective attention allocation to non-evaluative stimulus features during an attention manipulation phase could also lead to long-term extinction effects. Sanbonmatsu, Posavac, Vanous, Ho, and Fazio (2007) observed extinction effects the day following an attention manipulation phase. The authors presented participants with a slowly unmasking stimulus and asked them to press a fixed key upon recognition of the word. The following day, an evaluative priming task was administered in which the previously recognized words (i.e., the experimental stimuli) as well as new words (i.e., the control stimuli) were used as primes. Results revealed a significant evaluative priming effect for the control stimuli whereas the evaluative priming effect was absent for the experimental stimuli. Thus, while the design of Sanbonmatsu et al. (2007) led to a long-term reduction in likes and dislikes, the effect was limited to stimuli that were presented during the attention manipulation phase. Sanbonmatsu et al. (2007) attributed their effects to the repeated absence of evaluative processing of attitude-objects. Note, that such an account fits perfectly with the FSAA framework as it could be argued that the absence of a requirement to evaluatively process attitude objects discourages participants to assign selective attention to evaluative stimulus features. One possible reason for the difference in the lifespan of the attention manipulation effect between the study of Sanbonmatsu et al. (2007) and our studies, could be found in the number of trials presented during the attention manipulation phase. In the study of Sanbonmatsu et al. (2007), each experimental stimulus was presented 40 times during the recognition task. In contrast, in our studies, each experimental stimulus was presented either five times (i.e., Chapter 4) or eight times (i.e., Chapter 5) during the attention manipulation phase. Clearly, the number of stimulus presentations during the attention manipulation phase could explain the marked difference in the lifetime of the attention manipulation effects observed in our studies and the studies of Sanbonmatsu et al. (2007) (see Kalish, 1954).

At least three different accounts can explain why our attention manipulation would only induce short-term effects. First, it is possible that the effect of our attention manipulations just reduced due to the passing of time. One could simply test this account by manipulating the length of the interval between the attention manipulation and the measurement phase. The account would be supported if the magnitude of the attention manipulations effect is dependent on the length of this interval. Second, it seems only logical that the lifespan of the FSAA manipulation effect would be dependent on FSAA. More specifically, it is possible that participants were attending to evaluative stimulus features during the measurement phase thereby abolishing the effects of an attention manipulation in which attention to non-evaluative stimulus features was maximized. During the measurement phase participants are asked to evaluate stimuli. This procedure might encourage participants to pay selective attention to evaluative stimulus features (see Everaert et al., 2011). However, this process might take some time to complete, thus influencing the attitude measure that is performed second while leaving the effect of the attention manipulation intact on the attitude measure that is performed immediately following the attention manipulation. Third, the attention manipulation effect might be due to occasion setting (see, for example, Bouton, 2004, Rydell & Gawronski, 2009). During a measurement phase, participants are generally asked to evaluative stimuli and are therefore encouraged to assign attention to evaluative stimulus features. In contrast, during the attention manipulation phase, participants are encouraged to assign selective attention to non-evaluative stimulus features. The requirement to assign attention to different stimulus properties during the measurement phase and attention manipulation phase of the experiment could result in the formation of contextualized representations of stimuli. It could be hypothesized that likes and dislikes will be reduced in a context that maximizes attention to evaluative stimulus features but will resurface in a context that maximizes attention to non-evaluative stimulus features. Note, that this account is in accordance with numerous studies showing that the Pavlovian extinction effect is context-dependent (Bouton, 2004). The latter two accounts could be examined by adopting a 'renewal-procedure' which is frequently used to study context-effects in Pavlovian extinction (see Bouton, 2004). In this procedure, participants first perform an evaluative conditioning phase in context A (e.g., a blue background), which is then followed by an extinction phase in context B (e.g., a yellow background).

Extinction is shown to be context-dependent if evaluative responding to the CSs returns when CSs are presented in the original conditioning context (i.e., ABA renewal) or in an entirely new context (i.e., ABC renewal) but not when the CS is presented in the same context as during extinction (i.e., context B) (for example, see Bouton & Peck, 1989). To differentiate between the 'occasion setting account' and the 'FSAA account', one could encourage participants to assign attention to evaluative stimulus features in a context A (e.g., a blue background) during an acquisition phase. Next, during an extinction phase, participants could be encouraged to assign selective attention to a non-evaluative stimulus dimension in a context B (e.g., a yellow background). Finally, participants would be asked to perform an evaluatively neutral measurement task (e.g., a naming task or an irrelevant stimulus-response compatibility task) in either context A (i.e., ABA condition) or context B (i.e., ABB condition). Based on the occasion setting account, we would predict a significant difference between likes and dislikes in the ABA condition and the ABB condition. Because participants are not encouraged to assign attention to evaluative stimulus features during the measurement phase, we would not expect a difference in likes and dislikes between the ABA condition and the ABB condition based on the FSAA-account.

In sum, the attention manipulation paradigm discussed in Chapter 4 and Chapter 5 led to a short-term reduction in likes and dislikes. However, a study of Sanbonmatsu et al. (2007) suggests that attention manipulation can also lead to a long-term reduction in likes and dislikes. Further research should systematically examine which procedural variables influence the lifespan as well as the generalizability of the attention manipulation effect. These studies could also offer important insights about the underlying mechanisms and boundary conditions of the attention manipulation effect.

Psychopathology

The finding that FSAA moderates the extinction of likes and dislikes, raises the question whether FSAA could also play an important role in the treatment of psychopathology. As mentioned above, individuals might differ in the extent to which selective attention is chronically assigned to the evaluative features of attitude objects (Cavanagh & Davey, 2001; Viken et al., 2002). This chronic deployment of selective attention might even lead to an attention bias (Everaert, et al., 2013) which is claimed to play an important role in the etiology, maintenance and treatment of anxiety disorders (Beck & Clark, 1997; Van Bockstaele et al., 2014). Various therapeutic interventions have been developed aimed at reducing attention bias in order to reduce pathological symptoms. In a typical Attention Bias Modification procedure (i.e., ABM, Beard, Sawyer, & Hofmann, 2012; Hertel & Mathews, 2011; MacLeod, Rutherford, Campbell, Ebsworthy, & Holker, 2002, see Bar-Haim, 2010; Browning, Holmes, & Harmer, 2010 for a recent review), participants are presented with a modified version of the standard attention bias assessment task, i.e. the dot-probe task (MacLeod et al., 1986). In the ABM procedure, attention bias is manipulated by encouraging participants to divert their attention away from threatening information by presenting them with a high number of incongruent trials. This procedure leads to a reduction in attention bias thereby inducing therapeutic change. However, despite various studies attesting to the therapeutic value of the ABM procedure (e.g., Amir, Weber, Beard, Bomyea, & Taylor, 2008; Bar-Haim, 2010), several other studies raised doubts about the robustness of this effect (see Hallion & Ruscio, 2011; Heeren, Mogoase, McNally, Schmitz, & Philippot, 2015; Mogoaşe, David, & Koster, 2014; Van Bockstaele et al., 2014). These inconsistent findings can be readily explained if one takes into account the effects of FSAA. Note, that FSAA manipulation approach differs markedly from the ABM procedure. Whilst in the FSAA approach, participants are encouraged to attend to non-evaluative features of attitude-objects , participants in the ABM procedure are encouraged to divert their attention from attitude objects as a whole. However, one can only divert attention from a stimulus if that stimulus has been processed. Furthermore, in the dot probe paradigm, it becomes beneficial to attend to the threat value of the cue as soon as the content of the cue is predictive of the target location, that is, as soon as the proportion of congruent and incongruent trials is different from 50 %. This could explain why successful attempts to change attention bias were not always accompanied by corresponding changes in symptoms (Browning, Holmes, Murphy, Goodwin, & Harmer, 2010) or even led to an increase in reported symptomatology (Baert, De Raedt, Schacht, & Koster, 2010).

The FSAA manipulation approach also differs from the Emotional Processing Theory (i.e., EPT) of exposure therapy (Foa & Kozak, 1986). Exposure therapy typically involves controlled exposure to a feared stimulus, either in vitro or in vivo. During the intervention, participants are encouraged to attend to the threatening stimulus in order to activate corresponding memory traces. Therapeutic change is expected to occur if participants adjust these memory traces with corrective information during the exposure session. Thus, while participants are encouraged to direct attention to attitude objects as a whole during exposure therapy, participants in the FSAA manipulation approach are encouraged to attend to the non-evaluative features. However, as the underlying mechanisms of exposure therapy are not well understood (Hofmann, 2008), we do not know exactly what participants are doing during the exposure session. For example, it might well be that exposure therapy is particularly effective in participants who spontaneously attend to the non-evaluative features of threatening stimuli when confronted with these stimuli during an exposure session.

Exposure therapy has proven to be a very successful intervention technique for treating, for example, specific phobias (Parsons & Rizzo, 2008), post-traumatic stress disorder (Taylor et al., 2003), and social phobia (Heimberg & Barlow, 1988) (see also Deacon & Abramowitz, 2004; Foa, Keane, & Friedman, 2000). However, in many cases it is impossible to present the original fear-evoking stimuli during an exposure session (e.g., an aggressive dog). Therapist therefore usually rely on the presentation of aperceptual similar fear-evoking stimuli (e.g., the image of an aggressive dog or an imagined aggressive dog). Unfortunately, the extinction of fear does not always generalize to the original fear-evoking stimulus or related fear-evoking stimuli (Barry, Griffith, Vervliet, & Hermans, 2016, Rowe, & Craske, 1998; Vervliet, et al., 2005). Whether or not generalization takes place seems to be dependent on the perceptual similarity between the original stimulus and the stimulus presented during the exposure session, with higher similarity increasing the probability of generalization (Vervliet, Vansteenwegen, & Eelen, 2006). A recent study of Barry and Hermans (2016) examined whether the reduction of fear in generalization stimuli could be attenuated by encouraging participants to attend to the commonalities between stimuli used during the exposure session and generalization stimuli. First, fear was acquired by pairing a stimulus with an ectro-cutaneaous shock. Next, during an exposure session, participants were encouraged to assign attention to the communalities (e.g., color) between the acquisition stimulus and exposure stimulus (i.e., common condition) or to the unique features of the exposure stimulus (i.e., unique condition). Barry and Hermans (2016) observed that fear in response to a generalization stimulus in terms of skin conductance returned in the unique condition but not in the common condition. However, there was no difference between conditions when fear was measured via self-report ratings of shock expectancy or via fear potentiated startle. A similar mechanism could explain why the effect of our FSAA manipulation generalized to stimuli that were not presented during the attention manipulation phase (i.e., Chapter 6). That is, one could suggest that participants were encouraged to assign selective attention to the communalities between generalization stimuli and stimuli presented during the attention manipulation phase in terms of a higher order categorization (i.e., animacy). Therefore, one could expect that the effect of the FSAA manipulation would generalize to all stimuli that fall within this categorization.

Taking into account the limitations of both the EPT and ABM, the FSSA approach might well provide a promising new avenue for combating pathological fear and anxiety in addition to the EPT and ABM. The studies discussed in Chapter 4 and Chapter 5 provide preliminary support this idea, showing that the FSAA manipulation can indeed lead to changes in likes and dislikes. Moreover, the importance of FSAA in combating anxiety and fear was supported by an unpublished study of Spruyt and Van Bockstaele in which the authors tested the efficacy of a modified dot probe paradigm designed to maximize attention to non-evaluative stimulus features. Half of the participants were assigned to an Avoid Training condition in which the threat value of the cue was predictive for the location of the target. The other half of the participants was assigned to an Irrelevant training condition in which a threat-unrelated dimension of the cues (i.e., the format of the cues) was predictive of the location of the target stimulus. Interestingly, participants in the Irrelevant training condition exhibited less emotional reactivity to a stressful situation than participants in the Avoid Training condition. Of course, much more research will be needed to confirm the potential use of the FSAA manipulation approach as a new intervention technique. There are at least three limitations of the studies discussed in Chapter 4 and Chapter 5 that should be addressed.

First, the FSAA manipulation only led to changes in likes and dislikes. Future research should ascertain that the FSAA manipulation also leads to a reduction in behavior and/ or pathological symptoms. Second, we examined likes and dislikes in students. Therefore it is yet to be seen whether the FSAA manipulation can affect likes and dislikes as well as behavior in clinical samples. Third, the FSAA manipulation in our studies only caused short-term changes in likes and dislikes. Further research should examine whether the FSAA manipulation can also lead to long-term changes in likes and dislikes as well as symptomology.

CONCLUSION

In this dissertation project, we systematically tested whether the measurement and modification of (implicit) likes and dislikes are dependent on FSAA. According to the FSAA framework (Spruyt, et al., 2007, 2009, 2015; Spruyt & Tibboel, 2015; Spruyt, 2014), evaluative stimulus features are processed only and to the extent that they are selectively attended. Moreover, it seems likely that individuals differ in the extent to which selective attention is required to automatically process the evaluative features of attitude objects. Based on this assumption, it was hypothesized that inter-individual differences in implicit evaluation will be most pronounced if individuals are encouraged to allocate selective attention to a non-evaluative stimulus feature. Our studies concerning the moderating role of FSAA on the predictive validity of implicit attitude measurement techniques yielded mixed results. In two studies, predictive validity was most pronounced in an evaluative priming task in which attention to non-evaluative semantic features was maximized compared to an evaluative priming task in which attention to evaluative semantic features was maximized. However, a meta-analysis revealed no significant impact of FSAA on the predictive validity of implicit measures. Importantly, the meta-analysis indicated that more research is necessary about the combined effects of personal relevance and FSAA onthe predictive validity of implicit measures before strong claims can be made about the moderating role of FSAA on the predictive validity of implicit attitude measures.

The second part of the dissertation project was more successful. That is, we provided convincing evidence that FSAA can modulate the modification of likes and dislikes. We observed extinction of both recently acquired likes and dislikes as well as likes and dislikes toward fearful stimuli under conditions that maximized attention to non-evaluative stimulus features. The expression of likes and dislikes remained unchanged or even increased under conditions that maximized attention to evaluative stimulus features. However, more research will be necessary to examine the lifespan of this effect, its underlying mechanisms and boundary conditions as well as its therapeutic value.

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NEDERLANDSTALIGE SAMENVATTING

INLEIDING

Het is een klassieke hypothese dat de verwerking van evaluatieve informatie op een automatische en onvoorwaardelijke manier kan verlopen (Arnold, 1960; Barlett, 1932; Lazarus, 1966; Wundt, 1907). Het is echter pas sinds het invloedrijke werk van Zajonc (1980) dat evaluatieve prikkelverwerking op een systematische wijze onderzocht wordt. In het merendeel van dit onderzoek wordt gebruik gemaakt van het evaluatieve priming paradigma (Fazio, Sanbonmatsu, Powell, & Kardes, 1986). In dit paradigma worden deelnemers doorgaans gevraagd om een serie van doelprikkels (bv., een afbeelding van een kitten) te categoriseren als zijnde 'positief' of 'negatief'. Deze doelprikkels worden voorafgegaan door de presentatie van een taak-irrelevante primeprikkel (bv., de afbeelding van een spin). De evaluatieve congruentie van de doelprikkels en primeprikkels wordt gemanipuleerd zodat deze prikkels dezelfde evaluatieve connotatie hebben op sommige proefbeurten (i.e., congruente proefbeurten) maar verschillen in evaluatieve connotatie op andere proefbeurten (i.e., incongruente proefbeurten). Deelnemers reageren doorgaans sneller en maken minder fouten op congruente proefbeurten dan op incongruente proefbeurten (i.e., het evaluatieve primingeffect). Het is belangrijk om te begrijpen dat dit effect enkel kan optreden indien de evaluatieve connotatie van de irrelevante primeprikkels wordt verwerkt. Het evaluatieve primingeffect wordt dan ook vaak gebruikt als een index voor evaluative stimulusverwerking. Door te onderzoeken onder welke condities evaluative primingeffect voorkomt, kan men dus veel leren over de condities waaronder evaluatieve verwerking plaatsvindt. De laatste decennia is de experimentele evidentie voor de 'automaticiteithypothese' enkel gegroeid, zowel in de gedragswetenschappen (bv., Fazio, 2001) als in de neurowetenschappen (bv., Vuilleumier, 2005). Niettemin toonde recent onderzoek aan dat automatische evaluatieve prikkelverwerking geen onvoorwaardelijk fenomeen is maar afhankelijk is van kenmerkspecifieke aandachtallocatie. (i.e., KSAA; Spruyt, De Houwer, & Hermans, 2009; Spruyt, De Houwer, Hermans, & Eelen, 2007; Spruyt, Klauer, Gast, De Schryver, & De Houwer, 2015; Spruyt & Tibboel, 2015; Spruyt, 2014). Spruyt en collegas toonden aan dat de mate waarin evaluatieve stimuluskenmerken automatisch worden verwerkt een functie is van de mate waarin deelnemers selectieve aandacht aan deze kenmerken schenken. Bijvoorbeeld, Spruyt et al. (2009) gebruikten een beeld-beeld benoemingstaak om de invloed van KSAA op het evaluatieve primingeffect te onderzoeken. De auteurs maakten gebruik van de beeld-beeld benoemingstaak omdat deelnemers in deze taak de doelstimuli louter moeten benoemen waardoor de taak doorgaans als semantisch neutraal wordt aanschouwd. Benoemingsproefbeurten werden samen met categorisatieproefbeurten aangeboden. In de categorisatieproefbeurten werden deelnemers gevraagd om doelstimuli te categoriseren met betrekking tot hun evaluatieve kenmerken (i.e., positief of negatief) of met betrekking tot levendheid (i.e., levend of object). Deelnemers werden dus aangespoord om selectieve aandacht te besteden aan respectievelijk evaluatieve of niet-evaluatieve semantische stimuluskenmerken. Spruyt et al. (2009) vonden een evaluatief primingeffect maar geen niet-evaluatief semantisch primingeffect op de benoemingsproefbeurten indien deelnemers aandacht besteedden aan evaluatieve stimuluskenmerken gedurende de categorisatietrials. Er werd echter een niet-evaluatief semantisch primingeffect gevonden maar geen evaluatief primingeffect indien deelnemers werden aangemoedigd om aandacht te besteden aan niet-evaluatieve semantische stimuluskenmerken. Ook andere indices van automatische evaluatieve verwerking blijken afhankelijk te zijn van de mate waarin selectieve aandacht wordt besteed aan de evaluatieve stimulusdimensies, zoals het dot probe effect (Everaert et al., 2013), het evaluatieve stroop effect (Everaert et al., 2013), en de P3a uitgelokt door onverwachte emotionele stimuli (Everaert, Spruyt, Rossi, Pourtois, & De Houwer, 2013). Gelijkaardige bevindingen werden ook gerapporteerd in het domein van niet-evaluatieve semantische stimulusverwerking (zie Ansorge, Kunde, & Kiefer, 2014; Kiefer & Brendel, 2006; Kiefer & Martens, 2010; Kiefer, 2012; Martens &

Kiefer, 2009).

Samen met het stijgend aantal studies die aantonen dat evaluatieve verwerking afhankelijk is van KSAA, rijst de vraag of KSAA ook een modererende factor is in fenomenen die afhankelijk zijn van evaluatieve verwerking. Zo toonden Gast and Rothermund (2011) reeds aan dat de acquisitie van voor- en afkeuren via evaluatieve conditionering afhankelijk is van KSAA. In het huidige doctoraatsproject onderzochten we systematisch of KSAA ook het meten en het veranderen van (spontane) voor-en afkeuren beïnvloedt.

OVERZICHT

De invloed van KSAA op het meten van impliciete voor- en afkeuren.

Gedrag is een functie van voor-en afkeuren (Allport, 1935). Het is dan ook niet verrassend dat veelvuldig onderzoek gewijd werd aan het ontdekken van valide en betrouwbare meetinstrumenten van voor-en afkeuren. Oorspronkelijk werden voor-en afkeuren gemeten via expliciete attitudematen, i.e., proefpersonen werden gevraagd om hun voor-en afkeuren weer te geven via zelf-rapportering. Deze expliciete maten werden echter bekritiseerd omdat ze gebaseerd zijn op deelnemers hun capaciteiten om hun voor-en afkeuren via introspectie te bepalen. Er is echter geen garantie dat deelnemers hiertoe in staat zijn. Daarnaast is het zeer makkelijk voor deelnemers om hun voor-en afkeuren onjuist weer te geven op deze expliciete maten (zie Fazio & Olson, 2003; Greenwald & Banaji, 1995). In de voorbije decennia werd het ook steeds duidelijker dat gedrag niet enkel bepaald wordt door expliciete voor-en afkeuren maar ook door impliciete voor-en afkeuren, i.e., de spontane evaluatieve reacties die stimuli oproepen. Deze inzichten leidden tot de ontwikkeling van nieuwe meetinstrumenten die erop gericht zijn om deze spontane voor- en afkeuren te meten. Een voorbeeld van een dergelijk impliciet meetinstrument is de evaluatieve primingtaak. Door het gebruik van attituderelevante objecten als primestimuli kan de evaluatieve primingtaak aangewend worden als instrument voor het meten van spontane voor- en afkeuren. Bijvoorbeeld, om spontane reacties ten opzichte van spinnen te meten, kan men afbeeldingen van spinnen als primestimuli gebruiken. Een typische observatie is dat personen die angst hebben voor spinnen sneller reageren en minder fouten maken indien de afbeelding van een spin gevolgd wordt door een negatieve doelstimulus dan wanneer de afbeelding gevolgd wordt door een positieve doelstimulus (bv., Klein et al., 2012). Andere welbekende taken voor het meten van spontane voor- en afkeuren zijn de Impliciete associatietest (i.e., IAT, Greenwald, McGhee, & Schwartz, 1998), de relational responding taak (i.e., RRT, De Houwer, Heider, Spruyt, Roets, & Hughes, 2015), en de Approach-Avoidance taak (i.e., AAT; Solarz, 1960).

Ondanks dat verschillende studies hebben aangetoond dat interindividuele verschilscores gemeten via impliciete attitudematen kunnen gebruikt worden om gedrag te voorspellen (zie bijvoorbeeld, Degner & Wentura, 2009; Descheemaeker, Spruyt, & Hermans, 2014; Klein et al., 2012; Spruyt et al., 2015), bleken verscheidene onderzoekers niet in staat om dit effect te repliceren (Blanton et al., 2009; Falk, Heine, Takemura, Zhang, & Hsu, 2015; Neimeijer, de Jong, & Roefs, 2015; Spruyt, Hermans, De Houwer, Vandekerckhove, & Eelen, 2007). Eén manier om deze inconsistente bevindingen te verklaren is om te veronderstellen dat de voorspellende validiteit van impliciete attitudematen beïnvloed wordt door modererende variabelen. In een eerste onderzoekslijn van het doctoraatsproject onderzochten we of KSAA een modererende rol speelt in het meten van spontane voor- en afkeuren. Deze hypothese is gebaseerd op de assumptie dat individuen niet enkel verschillen in de extremiteit of/en de richting van hun voor- en afkeuren maar ook in de mate waarin selectieve aandachtallocatie vereist is om de evaluatieve connotatie van attitudeobjecten automatisch te verwerken. Bijvoorbeeld, individuen die angst hebben voor spinnen zullen spinnen niet enkel als negatiever ervaren dan controlepersonen (Ellwart, Rinck, & Becker, 2006), maar zij zullen de negatieve connotatie van spinnen ook verwerken indien zij aangespoord worden om aandacht te besteden aan niet-evaluatieve stimulusdimensies. Verschillende studies tonen inderdaad aan dat KSAA niet enkel afhankelijk is van expliciete taakinstructies (Spruyt et al., 2009) of meer subtiele aspecten van de taak (Everaert, Spruyt, & De Houwer, 2011) maar ook van de persoonlijke relevantie van een attitudeobject (Cavanagh & Davey, 2001; Fazio & Dunton, 1997; Viken, Treat, Nosofsky, McFall, & Palmeri, 2002). Men kan dus voorspellen dat interindividuele verschillen in automatische evaluatieve verwerking het meest uitgesproken zullen zijn onder condities die selectieve aandacht voor niet-evaluatieve stimulusdimensies stimuleren. Meer bepaald voorspellen we dat de voorspellende validiteit van impliciete attitudematen groter zal zijn wanneer deelnemers worden aangemoedigd om selectieve aandacht te besteden aan niet-evaluatieve stimuluskenmerken dan wanneer zij worden aangemoedigd om selectieve aandacht te besteden aan evaluatieve stimuluskenmerken.

In **Hoofdstuk 2** beschrijven we twee studies waarin deze hypothese werd getest. In Experiment 1 werden deelnemers gevraagd om doelstimuli te benoemen in een beeld-beeld benoemingstaak in de helft van de proefbeurten (see Spruyt et al., 2009). Deze beeld-beeld benoemingstaak werd ontwikkeld om de spontane voor- en afkeuren ten opzichte van fruit en snoep te meten. In de andere helft van de proefbeurten, werden de doelstimuli omringd door een gekleurde rechthoek. In deze proefbeurten werden deelnemers gevraagd om de doelstimuli te categoriseren op basis van hun evaluatieve connotatie (i.e., evaluatieve conditie), hun levendheid (i.e., semantische conditie), of de kleur van de rechthoek (i.e., de niet-semantische conditie). Deelnemers werden dus aangemoedigd om aandacht te besteden aan respectievelijk de evaluatieve stimulusdimensie, een niet-evaluatieve semantische stimulusdimensie, of een niet-evaluative niet-semantische stimulusdimensie. Op het einde van de sessie werden deelnemers gevraagd om te kiezen tussen een stuk fruit of een stuk snoep als een bedankje voor hun deelname aan het experiment. Conform onze hypothesen, was voorspellende validiteit hoger in de semantische conditie dan in de niet-semantische conditie of de evaluatieve conditie. De geobserveerde relatie tussen het gedrag en de interindividuele verschilscores in de semantische conditie was echter tegen intuïtief. Hoe meer interindividuele verschilscores indicatief waren voor een preferentie voor fruit relatief ten opzichte van snoep, hoe hoger de kans dat deelnemers een stuk snoep kozen op het einde van het experiment.

In Experiment 2 voerden deelnemers zowel een evaluatieve categorisatietaak als een semantische categorisatietaak uit. Deelnemers werden dus aangemoedigd om selectieve aandacht te besteden aan evaluatieve stimulusdimensies en niet-evaluatieve semantische stimulusdimensies, respectievelijk. Beide taken werden ontwikkeld om voor- en afkeuren ten opzichte van spinnen te meten. Op het einde van de experimentele sessie werden deelnemers gevraagd om een gedragsbeoordelingstaak uit te voeren om zo hun vermijdingsgedrag ten opzichte van een spin te meten. We observeerden een hogere voorspellende validiteit indien de impliciete attitudemaat werd uitgevoerd onder condities die selectieve aandacht voor niet-evaluatieve semantische stimulusdimensies stimuleren vergeleken met condities die selectieve aandacht voor evaluatieve stimulusdimensies stimuleren, ten minste indien de semantische categorisatietaak voor de evaluatieve categorisatietaak werd aangeboden. Net zoals in Experiment 1 werd vastgesteld dat de relatie tussen gedrag en interindividuele verschilscores gemeten in de semantische categorisatietaak tegen intuïtief was. Hoe meer interindividuele verschilscores een afkeur voor spinnen weergaven, hoe groter de kans dat deelnemers een levende spin durfden benaderen.

Het binnen-subjects design dat gebruikt werd in Experiment 2 stelde ons ook in staat om een tweede hypothese te testen, i.e., we voorspelden dat deelnemers die angst hadden voor spinnen, de evaluatieve stimuluskenmerken van spin-gerelateerde stimuli zouden verwerken onafhankelijk van taakinstructies. We verwachtten dus geen verschil tussen individuele verschilscores gemeten in de evaluatieve categorisatietaak en de semantische categorisatietaak bij deelnemers met spinnenangst. Een controlegroep, daarentegen, zou de evaluatieve stimuluskenmerken van spingerelateerde stimuli enkel verwerken mits inductie van selectieve aandacht voor evaluatieve stimuluskenmerken. We verwachtten dus een betrouwbaar verschil tussen interindividuele verschilscores gemeten in de evaluatieve categorisatietaak en de niet-evaluatieve semantische categorisatietaak in een controle groep. De resultaten waren echter opnieuw tegen intuïtief. Het verschil tussen interindividuele verschilscores gemeten in de evaluatieve categorisatietaak en de niet-evaluatieve semantische categorisatietaak was significant in individuen die angst hadden voor spinnen maar niet in de controle groep.

In **Hoofdstuk 3** beschrijven we een meta-analyse waarin we opnieuw de hypothese onderzoeken dat KSAA een modererende rol speelt in de voorspellende validiteit van impliciete attitudematen. We onderzochten voorspellende validiteit in het evaluatieve priming paradigma (bv., Fazio, Sanbonmatsu, Powell, & Kardes, 1986) en de approach-avoidance taak (i.e., AAT; Solarz, 1960). In beide taken kunnen deelnemers gevraagd worden om op de doelstimuli te reageren op basis van hun evaluatieve stimulus connotatie of op basis van niet-evaluatieve stimuluskenmerken. Deze flexibiliteit in aandachtallocatie was één van de inclusiecriteria waaraan taken moesten voldoen om te worden opgenomen in de meta-analyse. De IAT, bijvoorbeeld, werd niet opgenomen omdat het inherent is aan deze taak dat deelnemers ten minste een deel van de taakrelevante stimuli evalueren. Naast het effect van KSAA op de voorspellende validiteit van impliciete attitudematen, onderzochten we in deze meta-analyse ook of het effect van KSAA gemodereerd wordt door de persoonlijke relevantie van de onderzochte attitudeobjecten. We voorspelden dat persoonlijke relevantie geen moderator zou zijn in impliciete attitudematen waarin selectieve aandacht voor evaluatieve stimulusdimensies wordt gemaximaliseerd. Persoonlijke relevantie zou echter wel een modererende rol spelen in impliciete attitudematen waarin selectieve aandacht voor niet-evaluatieve stimulusdimensies wordt gemaximaliseerd.

In tegenstelling tot onze verwachtingen was voorspellende validiteit onafhankelijk van KSAA. Het effect van KSAA werd ook niet gemodereerd door de persoonlijke relevantie van de attitudeobjecten. Het moet echter vermeld worden dat we slechts enkele studies observeerden waarin de voorspellende validiteit van de evaluatieve priming taak werd onderzocht in individuen voor wie het bestudeerd attitudeobjecten persoonlijk relevant was. Het is dus mogelijk dat we onvoldoende statistische gegevens hadden om de modulerende invloed van KSAA op de voorspellende validiteit van de evaluatieve priming taak te onderzoeken. Daarnaast werd er in de literatuur geen enkele studie gerapporteerd waarin de voorspellende validiteit van de AAT werd gemeten onder condities die selectieve aandacht voor niet-evaluatieve semantische stimuluskenmerken stimuleerde (e.g., levendheid). Verder onderzoek is dus noodzakelijk naar (1) de impact van persoonlijke relevantie op de modulerende rol van KSAA op voorspellende validiteit in de evaluatieve primingtaak en (2) de voorspellende validiteit van de AAT onder condities die selectieve aandacht voor niet-evaluatieve semantische stimuluskenmerken stimuleren.

De invloed van KSAA op het veranderen van spontane voor- en afkeuren.

In een tweede onderzoekslijn van het huidige doctoraatsproject onderzochten we of KSAA een modererende rol kan spelen in het veranderen van (spontane) voor- en afkeuren. Onderzoek heeft aangetoond dat voor- en afkeuren zeer resistent zijn tegen uitdoving (Craske, 1999; Hallion & Ruscio, 2011; Krypotos, Arnaudova, Effting, Kindt, & Beckers, 2015). Een mogelijke reden hiervoor is dat de spontane evalutieve respons uitgelokt door een attitudeobject, de evaluatieve connotatie van dit object steeds bevestigt (Lewicki, Hill, & Czyzewska, 1992; Martin & Levey, 1978). Op basis van het KSAA-kader kan men echter voorspellen dat de kans dat het aanschouwen van een attitudeobject tot een evaluatieve respons leidt kleiner is indien selectieve aandacht wordt weggericht van evaluative stimuluskenmerken. De mate waarin er uitdoving van voor- en afkeuren optreedt zou dus afhankelijk zijn van de mate waarin individuen hun aandacht richten op niet-evaluatieve (semantische) stimuluskenmerken. We voorspelden een reductie in voor- en afkeuren indien deelnemers selectieve aandacht besteden aan niet-evaluatieve stimulusdimensies tijdens een aandachtmanipulatietaak. De expressie van voor- en afkeuren zou niet gereduceerd zijn of zelfs meer uitgesproken zijn indien deelnemers selectieve aandacht besteden aan evaluatieve stimulusdimensies tijdens een aandachtmanipulatietaak.

In **Hoofdstuk 4** voerden we twee experimenten uit waarin deelnemers voor- en afkeuren aanleerden via een evaluatieve conditioneringsprocedure. Geconditioneerde stimuli (i.e., CSs) bestonden uit abstracte Gabor patches die varieerden op basis van twee orthogonale, perceptuele stimuluskenmerken, i.e., oriëntatie en spatiale frequentie. Gedurende de acquisitiefase was één van deze perceptuele kenmerken voorspellend voor de evaluatieve toon van de ongeconditioneerde stimulus (i.e., US). Deelnemers werden dus aangemoedigd om selectieve aandacht te besteden aan dit perceptueel kenmerk. In een volgende extinctiefase werden de CSs alleen aangeboden. Deelnemers werden gevraagd om de CSs te categoriseren op basis van hun evaluatieve stimuluskenmerken (i.e., evaluatieve conditie), het stimuluskenmerk dat correleerde met valentie gedurende de acquisitiefase (i.e., de relevante conditie), of het stimuluskenmerk dat niet gerelateerd was met valentie gedurende de acquisitiefase (i.e., irrelevant conditie). Experiment 1 en Experiment 2 waren identiek behalve dat op het einde van Experiment 2 een directe maat van KSAA werd toegevoegd. We verwachtten uitdoving van voor- en afkeuren in de irrelevante conditie maar niet in de evaluatieve conditie. Omdat deelnemers in de relevante conditie gestimuleerd werden om selectieve aandacht te schenken aan een perceptueel stimuluskenmerk dat gecorreleerd was met valentie voorspelden we dat de uitdoving van voor- en afkeuren in deze conditie minder uitgesproken zou zijn dan in de irrelevante conditie.

Conform onze hypothesen, observeerden we een lineaire reductie in het EC effect gemeten in een expliciete attitudemaat in functie van selectieve aandacht voor niet-evaluatieve stimuluskenmerken. In beide experimenten, stelden we uitdoving van voor- en afkeuren vast in de irrelevante conditie maar niet in de evaluatieve conditie of de relevante conditie. De impliciete attitudemaat in Experiment 1 bevestigde deze bevinding al was het lineaire effect net niet significant (p = .06). De resultaten van Experiment 2 suggereerden dat het effect van KSAA op impliciete maten van voor- en afkeuren afhankelijk is van de mate waarin de CSs duidelijke exemplaren van de experimentele stimuluscategorieën zijn. Het effect van KSAA was significant in uitgesproken exemplaren maar niet in onuitgesproken exemplaren. In zowel Experiment 1 (i.e., expliciete maat) als Experiment 2 (i.e., impliciete maat) observeerden we eveneens een significante stijging in de expressie van voor- en afkeuren in de evaluatieve conditie. Deze observatie is helemaal in overeenstemming met de hypothese dat de reactivatie van de emotionele connotatie van de CS, de verworven valentie van de CS kan versterken (zie Lewicki, Hill, & Czyzewska, 1992).

In **Hoofdstuk 5** onderzochten we of KSAA ook een belangrijke moderator is in de uitdoving van voor- en afkeuren ten opzichte van angstopwekkende stimuli. Deelnemers kregen een serie positieve en negatieve stimuli te zien die varieerden op basis van twee orthogonale dimensies: valentie (positief versus negatief) en levendheid (levend versus object). Om voor- en afkeuren ten opzichte van angstopwekkende stimuli te onderzoeken, bevatte de categorie van negatieve, levende stimuli enkel afbeeldingen van spinnen. Gedurende een aandachtmanipulatiefase werden deelnemers gevraagd om stimuli te categoriseren op basis van hun valentie (i.e., evaluatieve conditie) of hun levendheid (i.e., semantische conditie). Deelnemers werden dus gestimuleerd om selectieve aandacht te schenken aan evaluatieve stimulusdimensies of niet-evaluatieve semantische stimulusdimensies, respectievelijk. We voorspelden minder uitgesproken voor- en afkeuren in de semantische conditie in vergelijking met de evaluatieve conditie.

In lijn met onze hypothese, waren voor- en afkeuren gemeten door een impliciete attitudemaat minder uitgesproken in de semantische conditie in vergelijking met de evaluatieve conditie. Dit effect generaliseerde naar nieuwe stimuli die niet werden aangeboden gedurende de aandachtmanipulatiefase. De expliciete maat bevestigde deze bevinding al was het effect niet significant (p = .12). We vonden echter geen verschil tussen de evaluatieve conditie en de semantische conditie in automatisch toenaderings- en vermijdingsgedrag gemeten door en relevant stimulus-respons compatibiliteittaak.

CONCLUSIE

In het huidige doctoraatsproject onderzochten we systematisch of het meten en het veranderen van (impliciete) voor- en afkeuren afhankelijk was van kenmerk-specifieke aandachtallocatie. In een eerste onderzoekslijn onderzochten we de hypothese dat interindividuele verschillen in impliciete voor- en afkeuren het meest uitgesproken zijn onder condities die aandacht voor niet-evaluatieve stimuluskenmerken stimuleren. Dit onderzoek leverde gemengde resultaten op. In twee studies vonden we dat voorspellende validiteit het hoogst was in een evaluatieve primingtaak waarin selectieve aandacht voor niet-evaluatieve semantische stimuluskenmerken werd gemaximaliseerd in vergelijking met een evaluatieve primingtaak waarin selectieve aandacht voor evaluatieve stimuluskenmerken werd gemaximaliseerd. Een meta-analyse toonde echter geen enkel verschil aan in voorspellende validiteit tussen impliciete maten waarin selectieve aandacht voor evaluatieve semantische stimuluskenmerken werd gemaximaliseerd en impliciete maten waarin geen semantische verwerkingsdoel werd geïnduceerd. Niettemin, al deze studies gaven aan dat meer onderzoek naar de impact van persoonlijke relevantie van attitudeobjecten op de relatie tussen KSAA en voorspellende validiteit nodig is vooraleer er sterke claims kunnen gemaakt worden over de modererende rol van KSAA op de voorspellende validiteit van impliciete attitudematen.

In een tweede onderzoekslijn vonden we evidentie voor de hypothese dat KSAA een modulerende rol speelt in het veranderen van voor- en afkeuren. Zowel recent verworven voor- en afkeuren als voor- en afkeuren ten opzichte van angstopwekkende stimuli waren minder uitgesproken onder condities waarin selectieve aandacht voor niet-evaluatieve stimuluskenmerken werd gestimuleerd. Voor- en afkeuren waren onveranderd of meer uitgesproken onder condities waarin selectieve aandacht voor evaluatieve stimuluskenmerken werd gestimuleerd. Verdere studies zijn echter nodig om de levensduur van dit effect, de onderliggende mechanismen en de therapeutische waarde ervan te onderzoeken.

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1a. Main researcher

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1b. Responsible Staff Member (ZAP)

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If a response is not received when using the above contact details, please send an email to data.pp@ugent.be or contact Data Management, Faculty of Psychology and Educational Sciences, Henri Dunantlaan 2, 9000 Ghent, Belgium.

2. Information about the datasets to which this sheet applies
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* Reference of the publication in which the datasets are reported: Vanaelst, J., Spruyt, A., & De Houwer, J. (2016). How to modify (implicit) evaluations of fear-related stimuli: Effects of feature-specific attention allocation. Frontiers in Psychology: Psychopathology

* Which datasets in that publication does this sheet apply to?: the sheet applies to all the data used in the publication

3. Information about the files that have been stored
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3a. Raw data
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* Have the raw data been stored by the main researcher? [X] YES / [ ] NO
If NO, please justify:

* On which platform are the raw data stored?
 - [X] researcher PC
 - [X] research group file server
 - [X] research group file server via DICT

* Who has direct access to the raw data (i.e., without intervention of another person)?
 - [X] main researcher
 - [X] responsible ZAP
 - [X] all members of the research group
 - [ ] all members of UGent

- [ ] other (specify): ...

3b. Other files

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* Which other files have been stored?
- [X] file(s) describing the transition from raw data to reported results. Specify: Information is available in the files: Aggregate_JV_2015_8.R
- [X] file(s) containing processed data. Specify: full.txt: resulting output. This file can also be obtained by running the R script.
- [X] file(s) containing analyses. Specify: Analyse_JV_2015_8
- [ ] files(s) containing information about informed consent
- [ ] a file specifying legal and ethical provisions
- [X] file(s) that describe the content of the stored files and how this content should be interpreted. Specify: The 'read_me' file contains clear discriptions of all stored files and syntax used for the aggregated dataset 'full.txt' and the raw data.
- [X] other files. Specify: The file 'stim_ratings' contains ratings for the stimuli used in the experiment. The files 'Reliability_AMP.R' and 'Reliability_RSCR.R' contain code for computing the reliability of the AMP and RSCR-task, respectively

* On which platform are these other files stored?
- [X] individual PC
- [X] research group file server
- [ ] other: ...

* Who has direct access to these other files (i.e., without intervention of another person)?
- [X] main researcher
- [X] responsible ZAP
- [X] all members of the research group
- [ ] all members of UGent
- [ ] other (specify): ...

4. Reproduction

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* Have the results been reproduced independently?: [ ] YES / [X] NO

* If yes, by whom (add if multiple):
  - name:
  - address:
  - affiliation:
  - e-mail: