Mere Exposure Effects on Implicit Stimulus Evaluation: The Moderating Role of Evaluation Task, Number of Stimulus Presentations, and Memory for Presentation Frequency

Pieter Van Dessel[1], Gaëtan Mertens,[2] Colin Tucker Smith[3], and Jan De Houwer[1]

[1]Utrecht University, The Netherlands

[2]Ghent University, Belgium

[3]University of Florida, USA

Authors Note: PVD and JDH, Department of Experimental Clinical and Health Psychology, Ghent University. GM, Department of Psychology, Utrecht University. CTS, Department of Psychology, University of Florida. PVD is supported by a Postdoctoral fellowship of the Scientific Research Foundation, Flanders (FWO-Vlaanderen). JDH is supported by Methusalem Grant BOF16/MET_V/002 of Ghent University and by the Interuniversity Attraction Poles Program initiated by the Belgian Science Policy Office (IUAPVII/33). Correspondence concerning this article should be sent to Pieter.vanDessel@UGent.be

Abstract

The mere exposure (ME) effect refers to the well-established finding that people evaluate a stimulus more positively after repeated exposure to it. So far, the vast majority of studies on ME effects have examined changes in explicit stimulus evaluation. We describe the results of three large-scale studies (combined *N* = 3623) that examined ME effects on implicit stimulus evaluation. We looked at three moderators of these effects, the implicit evaluation measure, the number of stimulus presentations, and memory for presentation frequency. We observed ME effects on implicit stimulus evaluations as measured with an Implicit Association Test (IAT) and Affect Misattribution Procedure (AMP), but not an Evaluative Priming Task (EPT). ME effects were more robust when there were relatively few stimulus presentations and when participants had accurate memory for the presentation frequencies. We discuss how these findings relate to ME effects on explicit evaluations as well as theoretical and practical implications.

Keywords: mere exposure, implicit evaluation, frequency memory, IAT, evaluative priming

Mere Exposure Effects on Implicit Stimulus Evaluation: The Moderating Role of Evaluation Task, Number of Stimulus Presentations, and Memory for Presentation Frequency

The mere exposure (ME) effect refers to the finding that people tend to prefer stimuli with which they have more experience (Zajonc, 1968). The ME effect is a robust (Bornstein, 1989) and ubiquitous finding in psychology. For instance, ME effects have been observed in research on novel products (Janiszewski, 1993), food preferences (Pliner, 1982), and racial prejudice (Zebrowitz, White, & Wieneke, 2008). Whereas an abundant number of studies have examined ME effects on explicit stimulus evaluations as captured by self-reported liking and choice preference measures, only a handful of studies have investigated ME effects on automatic (i.e., implicit) stimulus evaluations as captured by implicit evaluation measures (see below for an overview). This is a significant lacuna in ME research because implicit evaluation is often considered to be an important determinant of a wide range of behaviors in different domains of psychology such as consumer purchases, voting choices, or addictive behaviors. Indeed, several reviews and meta-analyses indicate that measures of implicit evaluation complement other measures of (explicit) evaluation and explain important additional variance in behavior under certain conditions (De Houwer, Teige-Mocigemba, Spruyt, & Moors, 2009; Hofmann, Gawronski, Gschwendner, Le, & Schmitt, 2005; Eschenbeck, Heim-Dreger, Steinhilber, & Kohlmann, 2016), especially in the context of more automatic or spontaneous behavior (e.g., Dovidio, Kawakami, & Gaertner, 2002; Eschenbeck et al., 2016; Friese, Hofmann, & Schmitt, 2009). Hence, the practical usefulness of ME for changing behavior might depend on whether, and under what circumstances, ME procedures influence implicit stimulus evaluations.

Furthermore, the question of whether ME can lead to changes in implicit stimulus evaluations is also important for evaluating theoretical accounts of ME effects such as the processing fluency/attribution account (e.g., Bornstein & D’Agostino, 1994). This account postulates that repeated exposure to a stimulus results in facilitated processing fluency. This fluency experience can be misattributed to stimulus properties that a participant is asked to rate (such as valence). In accordance with this account, ME effects have also been observed on rated stimulus dimensions other than valence, such as the prototypicality, truth, or brightness of a stimulus (e.g., Mandler, Nakamura, & Van Zandt, 1987). Importantly, according to certain interpretations of the processing fluency/attribution account, ME does not produce changes in a person's genuine liking of a stimulus but only facilitates changes in overt reports of stimulus evaluation as a consequence of being asked to provide evaluative stimulus ratings (e.g., Whittlesea, 1993). Hence, there are reasons to suspect that ME effects might occur only when participants are required to complete measures of explicit evaluation (allowing for misattribution of the fluency experience to liking), but not when they complete measures of implicit evaluation.

In contrast, alternative accounts of ME effects assume that ME leads to an immediate change in liking that is not critically dependent on the (explicit) measurement of evaluation. For instance, the hedonic-fluency account (e.g., Winkielman & Cacioppo, 2001) assumes that processing fluency is an inherently positive experience and, therefore, repeated exposure to a stimulus leads to a genuine change in the liking of a stimulus. Some have argued that this effect should be more easily observed on implicit evaluation measures because these measures are more sensitive to evaluations that arise from unconscious influences such as fluency experiences (e.g., Kawakami, 2012).

Propositional accounts of ME effects, which assume that ME effects depend on the acquisition of propositional knowledge about the frequency of exposure to a stimulus, also predict ME effects on implicit evaluation (Van Dessel, Mertens, Smith, & De Houwer, 2017). For instance, when participants infer that a frequently occurring stimulus is positive (e.g., because such stimuli are safe and harmless, Zajonc, 2001), this newly acquired information may influence both explicit and implicit stimulus evaluation (see De Houwer, 2014). Such inferences might occur under certain conditions of automaticity (e.g., unaware or uncontrolled; see Van Dessel, Hughes, & De Houwer, 2018).

Relatively few studies have examined ME effects on implicit evaluation. First, three studies (i.e., Kawakami, 2012; Kawakami & Yoshida, 2015; Smith, Dijksterhuis, & Chaiken, 2008, Experiment 3) demonstrated subliminal ME effects on implicit measures (i.e., the affect misattribution procedure [AMP], single-category implicit association test [SC-IAT], and the evaluative priming task [EPT], respectively). These studies involved 10-13 ms repeated presentations of either face stimuli or Nepalese signs, followed by a mask to prevent conscious reports of the stimuli. Because many recent studies have cast doubt on reported evidence for subliminal perception effects (e.g., Simmons, Nelson, & Simonsohn, 2011; Vadillo, Konstantinidis, & Shanks, 2016; Van Dessel, De Houwer, Roets, & Gast, 2016), we calculated Bayes Factors for the reported tests in these studies. These Bayes factors provide an indication of how strongly the data support either the null hypothesis (BF_0; reflecting the absence of an effect) or the alternative hypothesis (BF_1; reflecting the presence of an effect). BFs between 1 and 3, between 3 and 10, and larger than 10, respectively designate ‘anecdotal evidence’, ‘substantial evidence’, and ‘strong evidence’ for the tested hypothesis – most commonly the null (BF_0) or the alternative hypothesis (BF_1) (Jeffreys, 1961). Overall, evidence in favor of the effects was low ($BF_{1s} < 3$), with the exception of one reported effect in Kawakami & Yoshida (2015, Experiment 2: $BF_1 = 28.42$).

Second, two supraliminal ME studies have also used implicit evaluation measures. However, these studies did not focus on the effect of ME on the evaluation of specific stimuli, but rather on the effects of ME on the evaluation of categories of stimuli (e.g., the implicit evaluation of Japanese writing systems following exposure to exemplars of words written in those writing systems; Kawakami, Sato, & Yoshida, 2010) and on general positive affect (e.g., the overall evaluation of artificial words following exposure to Chinese ideographs: Hicks & King, 2011).

Third, ME effects have been reported on psychophysiological measures such as facial electromyography (e.g., Harmon-Jones & Allen, 2010; Winkielman & Cacioppo, 2001; Winkielman, Halberstadt, Fazendeiro, & Catty, 2006; Witvliet & Vrana, 2007). However, it is unclear to what extent psychophysiological responses reflect implicit evaluation (De Houwer & Moors, 2010). Finally, a recent set of studies demonstrated that ME instructions (i.e., instructions about the number of upcoming presentations of stimuli in the absence of actual presentations) can influence implicit evaluations of individual stimuli (Van Dessel, Mertens, Smith, & De Houwer, 2017). However, it is not clear whether ME instruction effects rely on the same mechanisms as ME effects instantiated through actual stimulus presentations. Hence, currently there is only limited evidence that ME through actual stimulus presentations can influence the implicit evaluation of those stimuli.

In the current study, we investigated whether ME can influence implicit evaluations and additionally assessed three potential boundary conditions of ME effects on implicit evaluation. First, we examined whether ME effects depend on the task that is used to measure implicit stimulus evaluations. More specifically, we examined ME effects on implicit evaluations as measured with the IAT (Greenwald, McGhee, & Schwartz, 1998), EPT (Fazio, Sanbonmatsu, Powell, & Kardes, 1986) and AMP (Payne, Cheng, Govorun, & Stewart, 2005). These three tasks were chosen because (1) they constitute the most widely used tasks to measure implicit evaluation, (2) they are differentially sensitive to a number of factors other than the to-be-measured psychological construct of implicit evaluation (e.g., extra-personal knowledge: Olson & Fazio, 2004; salience asymmetries: Rothermund & Wentura, 2004) and are thus assumed to involve different underlying processes (De Houwer et al., 2009), and (3) they conform with important normative criteria of implicit evaluation measures to a different extent (De Houwer et al., 2009). For the sake of comparison, we have also included a measure of explicit stimulus evaluation (i.e., a self-reported liking rating scale).

Second, we manipulated the number of stimulus presentations in the ME task. Previous research has shown that this can be an important moderator of ME effects on explicit stimulus evaluations (Bornstein, 1989; Van den Bergh & Vrana, 1998). It is typically observed that a minimum number of stimulus presentations is needed to produce a ME effect, yet the ME effect also seems to decrease in size after a relatively small number of stimulus presentations (e.g., 10-20 presentations) (Bornstein, 1989; Montoya, Horton, Vevea, Citkowicz, & Lauber, 2017; Zajonc, Shaver, Tavris, & van Kreveld, 1972). We examined whether this moderation is also observed for ME effects on implicit evaluations.

Third, we investigated whether ME effects depend on participants’ memory for the stimulus presentation frequencies. There is much debate about the importance of this factor for ME effects (Bornstein & D’Agostino, 1992; Newell & Shanks, 2007; Stafford & Grimes, 2012). Whereas some authors have stressed that memory for the presentation frequencies (and even conscious recognition of the stimuli at the time of exposure) is not necessary for, or could even hamper, ME effects (Bornstein, 1989; Monahan, Murphy, & Zajonc, 2000), other authors argue that memory for presentation frequencies is an important moderator of the ME effect (Brooks & Watkins, 1989; Newell & Shanks, 2007; Stafford & Grimes, 2012). This discrepancy may be due to the fact that some studies used small samples thus leading to more unreliable effects (Bornstein, 1989; Stafford & Grimes, 2012). In-line with Bar-Anan, De Houwer, and Nosek (2010), who investigated the relationship between memory of stimulus-stimulus contingencies and evaluative conditioning (EC) in a large sample, we recruited a large number of participants to investigate the relationship between memory of stimulus presentation frequencies and ME effects. To gain information about the strength of evidence for the presence or absence of ME effects for participants with either accurate or inaccurate presentation frequency memory, we supplemented traditional *t*-test analyses with Bayesian analyses (Dienes, 2011; Wagenmakers, 2007).

To address the above-mentioned questions, we conducted three large-scale experiments. All experiments used the same general procedure to manipulate the amount of exposure to different stimuli (Experiment 1 and 2: nonwords; Experiment 3: unknown brands). After the ME phase implicit evaluations were measured with either the IAT (Experiment 1, Experiment 2), EPT (Experiment 2), or AMP (Experiment 3).

Method

Participants

Participants were 892, 1392, and 1339 visitors to the Project Implicit research website (https://implicit.harvard.edu) in Experiments 1, 2, and 3, respectively**.** Prior to data-collection, target sample size of Experiment 3 was pre-registered together with the study design, data-analysis plan and the described hypotheses. These pre-registered plans as well as experiment scripts, stimuli, data, and analysis code of all experiments are available at https://osf.io/dnqcs/. In-line with standard procedures of data-reduction for Project Implicit data (e.g., Smith, De Houwer, & Nosek, 2013), we excluded data of participants who (1) did not complete all tasks (131 participants in Experiment 1: 14.7%; 184 participants in Experiment 2: 13.2%; 171 participants in Experiment 3: 12.8%), (2) had error rates above 30% when considering all critical IAT test blocks or above 40% for any one of these blocks (12 participants in Experiment 1, 1.6%; 9 participants in Experiment 2, 0.8%), (3) had error rates in the EPT that exceeded the population mean by more than 2.5 standard deviations (8 participants in Experiment 2: 0.7%, population mean = 7.2%, *SD* = 10.7%), or (4) used the same response key in the AMP for more than 90% of the trials (211 participants: 15.8%). The analyses were performed on the data of 749 participants (61.2% women, mean age = 35 years, *SD* = 13, range = 18-79) in Experiment 1, 1191 participants (63.6% women, mean age = 32 years, *SD* =

13, range = 18-76) in Experiment 2, and 956 participants (58.1% women, mean age = 31 years, SD = 13, range = 18-77) in Experiment 3.

Procedure

ME phase. After participants gave informed consent, they were told that they would see one or more stimuli (words in Experiments 1 and 2, novel food brands in Experiment 3) presented on the screen sequentially, that is, one after the other. They were asked to pay close attention to the stimuli because this would be vital for the successful completion of the study. Participants then went through a ME phase in which they saw presentations of two non-existing words "FEVKANI" and "LOKANTA" (Experiments 1-2) or three novel brand names with logos (Empeya, Levida, and Witkap). The stimuli remained on the screen for 1000 ms with a 1000 ms inter-trial interval. The number of stimulus presentations was manipulated between-subjects such that for participants in Experiment 1: (1) one word was presented two times and the other word was never presented (0-2 condition), (2) one word was presented three times and the other word was presented once (1-3 condition), (3) one word was presented six times and the other word was presented once (1-6 condition), or (4) one word was presented twelve times and the infrequent word was presented once (1-12 condition). In Experiment 2, there were only two stimulus pair conditions: the 0-2 condition and the 1-12 condition. These conditions were selected because they were the conditions in which we had observed the strongest ME effects on implicit evaluation in Experiment 1. In Experiment 3, all participants saw one brand twelve times, one brand two times, and one brand was never presented (0-2-12 condition). Which specific word or brand was presented more often was counterbalanced across participants and stimulus pair conditions. The order of the stimulus presentations within the ME phase was randomized.

Explicit evaluation. For half of the participants of Experiments 1 and 3, the ME phase was followed by an explicit evaluation task, which was then followed by the implicit evaluation task. The other participants first completed the implicit evaluation task and then the explicit evaluation task. In Experiment 2, all participants first completed the implicit evaluation tasks and then completed the explicit evaluation task. In the explicit evaluation task, participants indicated liking ratings of each of the two nonwords (Experiments 1 and 2) or three brand names (Experiment 3) by selecting an option on a 9-point Likert scale (1= not liked at all; 9 = completely liked) from a dropdown list on separate pages. The Likert scale for the different stimuli were presented in random order.

IAT (Experiments 1 and 2). In Experiment 1, all participants completed the IAT; in Experiment 2, half of the participants completed an IAT and the other participants completed an EPT. In the IAT, participants were asked to sort stimuli by pressing either the “E” or the “I” on the keyboard. On each trial, a word was presented in the center of the screen until the participant pressed one of the two keys. If the response was correct, the word disappeared and the next word was presented 400 ms later. If the response was incorrect, the word was replaced by a red “X”. The next word appeared 400 ms after participants pushed the correct button. In the first block, participants categorized FEVKANI and LOKANTA as their respective names. To avoid classification of the target stimuli based only on simple perceptual features, the words were presented in different font types (Arial Black and Fixedsys), capitalizations (uppercase and lowercase), and sizes (16pt and 18pt), resulting in 8 different stimuli for each nonword. Category labels were presented in the top left and right corner to aid classification. After 20 trials, participants categorized ten attribute words as ‘Good’ (wonderful, glorious, marvelous, success, peace) or ‘Bad’ (nasty, failure, agony, unpleasant, evil) with the “E” and the “I” buttons for 20 trials. Next, participants completed 20 practice trials and 40 critical trials in which both attribute and target words were categorized and in which FEVKANI and positive stimuli shared the same response key and LOKANTA and negative stimuli shared the other response key (or vice versa). Participants then practiced sorting target words with the response key assignment reversed for 40 trials. Finally, participants completed 20 practice and 40 critical trials with the new response key assignment.

EPT (Experiment 2). At the start of the EPT, participants were told that words would appear one after the other on the screen and that their task was to categorize the words as either "good" or "bad" using the ‘E’ and ‘I’ keys of a computer keyboard as quickly as possible, while making as few mistakes as possible. Participants were further told that they would see words presented before the positive and negative words and that they should not respond to those words. Participants were then shown a list of the 14 positive and 14 negative words that they would have to categorize. In-line with standard procedures (Spruyt, De Houwer, Hermans, & Eelen, 2007), a single trial consisted of a fixation cross presented in white for 500 ms, a blank screen for 500 ms, a prime for 200 ms, a post-prime pause for 50 ms and the presentation of a target word in white font for 1500 ms. The inter-trial interval was set to vary randomly between 500 ms and 1500 ms. There were four types of trials: (1) trials with the word LOKANTA as prime and a positive word as target, (2) trials with the word LOKANTA and a negative target, (3) trials with the word FEVKANI and a positive target, and (4) trials with the word FEVKANI and a negative target. Each type of trials was presented on a quarter of the trials. Participants first completed eight practice trials (two of each of the four types of trials) and then completed 120 trials separated into three blocks of 40 trials, each containing 10 of the four types of trials, presented in random order.

AMP (Experiment 3). In accordance with standard procedures (Payne et al., 2005), the AMP consisted of 3 blocks of 30 trials in which participants were presented with a prime stimulus for 75ms, a blank screen for 125ms, and a Chinese ideograph for 100ms, which was then covered with a black-and-white pattern mask. The three brands Empeya, Levida and Witkap served as prime stimuli. Each trial, participants indicated if they considered the

Chinese ideograph more or less visually pleasant than average by pressing either “I” or “E”, respectively. Participants were asked to ignore the prime stimuli and respond only to the Chinese ideographs.

Stimulus frequency memory measurement. At the end of the experiment, participants were asked to indicate how many times they had seen each of the two words or the three brands during the first (ME) task. Participants could choose a number between 0 and 15 from a dropdown list for each stimulus. The order of the questions was randomized.

Task engagement measurement. In Experiment 3, we assessed task engagement for the ME phase by asking participants to rate their levels of boredom and attention in this task with two 10-point rating scales (short version of the Dundee Stress State Questionnaire; Helton & Naswall, 2015).

Results

Data-preparation

IAT ME scores were calculated using the D2-algorithm (Greenwald, Nosek, & Banaji, 2003), such that higher scores indicate a stronger ME effect (i.e., a stronger preference for the frequently-presented word over the infrequently-presented word). EPT ME scores (Experiment 2) were created by (a) subtracting the mean latencies on trials with a positive target and the frequent word prime from the mean latencies on trials with a negative target and the frequent word prime, (b) subtracting the mean latencies on trials with a positive target and the infrequent word prime from the mean latencies on trials with a negative target and the infrequent word prime, and (c) subtracting the second difference score from the first difference score. EPT ME scores were calculated on the basis of EPT trials that remained after exclusion of trials with an incorrect response (3.5%) and trials with reaction times that were at least 2.5 standard deviations removed from an individual’s mean for that type of trial (2.9%).

Three AMP ME scores (Experiment 3) were calculated for each participant by subtracting the proportion of “pleasant” responses on (a) trials with the brand presented 0 times as prime from trials with the brand presented 12 times as prime (0-12 ME score), (b) trials with the brand presented 0 times as prime from trials with the brand presented 2 times as prime (0-2 ME score), and (c) trials with the Brand presented 2 times as prime from trials with the Brand presented 12 times as prime (2-12 ME score). The Spearman-Brown corrected split-half reliability was *r*(748) = .86 (Experiment 1) and *r*(590) = .87 (Experiment 2) for the IAT ME scores, *r*(597) = .43 for the EPT ME scores, and *r*(954) = [.51-.61] for the AMP ME scores.

Explicit rating ME scores were calculated by subtracting participants’ score rating for the infrequent word from their score rating for the frequent word (Experiments 1 and 2). For Experiment 3, three explicit rating ME scores were calculated by subtracting ratings for the infrequent brands from ratings for the frequent brands (0-12 ME score, 0-2 ME score, 2-12 ME scores). Explicit rating ME scores correlated significantly with IAT ME scores (*r*[747] = .32, *p* < .001 [Experiment 1], *r*[590] = .27, *p* < .001 [Experiment 2]), and AMP ME scores (*r*[953] = .22 [0-12 ME scores], *r*[953] = .21 [0-2 ME scores], *r*[953] = .21 [2-12 ME scores], *p*s < .001 [Experiment 3]), but not EPT ME scores (*r*[597] = .05, *p* = .18 [Experiment 2]).

Stimulus frequency memory was coded as accurate for participants who correctly indicated that the frequent word was presented more often than the infrequent word (Experiment 1: 392 participants, 52.3%; Experiment 2: 690 participants, 57.9%; Experiment 3: 0-12 pair: 808 participants, 84.7%; 0-2 pair: 711 participants, 74.5%; 2-12 pair: 719 participants, 75.3%). It was coded as reversed for participants who indicated that the frequent word was presented less often than the infrequent word (Experiment 1: 116 participants, 15.5%; Experiment 2: 176 participants, 14.8%; Experiment 3: 0-12 pair: 90 participants, 9.4%; 0-2 pair: 164 participants, 17.2%; 2-12 pair: 127 participants, 13.3%) and as indiscriminate for participants who indicated that both words had been presented equally often (Experiment 1: 241 participants, 32.2%; Experiment 2: 325 participants, 27.3%; Experiment 3: 0-12 pair: 57 participants, 5.9%; 0-2 pair: 80 participants, 8.3%; 2-12 pair: 109 participants, 11.4%). We also created an index of subjective ME Experience by subtracting the number of reported stimulus presentations for the infrequent word from the number of reported stimulus presentations for the frequent word. In Experiment 1, participants with accurate memory indicated smaller differences in the number of presentations for the 0-2 pair (M = 2.33, SD = 1.48) than for the 1-3 (M = 3.05, SD = 2.03), 1-6 (M = 5.97, SD = 3.62), and 1-12 pair (M = 9.21, SD = 4.17), ps < .014. In Experiment 2, participants with accurate memory indicated smaller differences for the 0-2 (M = 3.21, SD = 3.00) than for the 1-12 pair (M = 8.09, SD = 4.25), $t(688) = 16.82$, $p < .001$. In Experiment 3, participants with accurate memory indicated smaller differences for the 0-2 pair (M = 4.17, SD = 2.62) than for the 2-12 (M = 6.78, SD = 3.37) or 0-12 pair (M = 8.83, SD = 4.22), ps < .001.

Implicit Evaluation

IAT. In Experiment 1, IAT ME scores were significantly higher than zero, indicating a ME effect on IAT performance (M = 0.07, SD = 0.48), $t(748) = 4.14$, $p < .001$, $d_z = 0.15$, 95% confidence interval (CI) = [0.04, 0.11], $BF_1 = 190.51$. We performed an ANOVA on IAT ME scores that included Memory (accurate, indiscriminate, reversed), IAT Block Order (positive words and frequent word categorized with the same key in the first block, positive words and infrequent word categorized with the same key in the first block), Task Order (implicit evaluation task first, explicit evaluation task first), and Stimulus Pair (0-2, 1-3, 1-6, 1-12) as between-subjects factors.[1] This revealed only a main effect of IAT Block Order, $F(1, 701) = 5.21$, $p = .032$, but not any other main or interaction effects, Fs < 3.11, ps > .078.[2] Planned

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[1] For all experiments, we also performed ANOVA's that did not include the Memory factor. These analyses revealed the same significant effects.

[2] Because the main aim of our experiments was to quantify evidence for the presence or absence of an effect in any of the different memory and ME condition groups (and our experiment was specifically designed for this)

one-sample *t*-tests indicated that participants with accurate memory significantly preferred the frequent word (*M* = 0.10, *SD* = 0.48), *t*(391) = 4.21, $p < .001$, $d_z = 0.21$, 95% CI = [0.05, 0.15], $BF_1 = 303.78$. We did not observe a ME effect for participants with indiscriminate or reversed memory, *t*s < 1.49, *p*s > .13, d_zs < 0.10, BF_0s > 4.62. The ME effect for participants with accurate memory was significant only for the 0-2 pair (*M* = 0.16, *SD* = 0.44), *t*(77) = 3.25, *p* = .001, $d_z = 0.37$, 95% CI = [0.06, 0.26], $BF_1 = 15.12$, and the 1-12 pair (*M* = 0.13, *SD* = 0.50), *t*(130) = 2.91, *p* = .004, $d_z = 0.25$, 95% CI = [0.04, 0.21], $BF_1 = 5.44$, but not the 1-3 pair (*M* = 0.13, *SD* = 0.50), *t*(72) = 1.95, *p* = .055, $d_z = 0.23$, 95% CI = [0.00, 0.22], $BF_0 = 1.33$, or the 1-6 pair (*M* = 0.02, *SD* = 0.46), *t*(109) = 0.46, *p* = .64, $d_z = 0.04$, 95% CI = [-0.07, 0.11], $BF_0 = 8.52$. A summary of the *t*-test results is provided in Table 1.

In Experiment 2, IAT ME scores also indicated a ME effect on IAT performance (*M* = 0.09, *SD* = 0.49), *t*(591) = 4.41, $p < .001$, $d_z = 0.18$, 95% CI = [0.05, 0.13], $BF_1 = 623.02$. An ANOVA on IAT ME scores with Memory, IAT Block Order and Stimulus Pair as factors revealed a significant main effect of IAT Block Order, *F*(1, 585) = 13.46, $p < .001$, but no other main or interaction effects, *F*s < 2.09, *p*s > .12. We observed a significant ME effect for participants with accurate memory (*M* = 0.12, *SD* = 0.45), *t*(324) = 4.62, $p < .001$, $d_z = 0.26$, 95% CI = [0.07, 0.16], $BF_1 = 1754.95$, but not for participants with indiscriminate or reversed memory, *ts* < 1.82, *ps* > .072, d_z*s* < 0.13, BF_0s > 2.50 (Table 2). Participants with accurate memory exhibited a significant ME effect for the 0-2 pair (*M* = 0.17, *SD* = 0.43), *t*(122) = 4.45, $p < .001$, $d_z = 0.40$, 95% CI = [0.10, 0.25], $BF_1 = 813.57$, and the 1-12 pair (*M* = 0.08,

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we report separate *t*-tests for these different groups despite the fact that the ANOVA did not reveal significant effects of Memory or Stimulus Pair. Bonferroni-correction sets the significance cut-off at *p* = .017 for the *t*-tests examining the effects in the three memory groups and at *p* = .013 for the *t*-tests examining the effects in the four ME condition groups. Multiple comparisons are not a problem for the Bayes Factors (see Dienes, 2016).

SD = 0.46), *t*(201) = 2.48, *p* = .014, d_z = 0.17, 95% CI = [0.02, 0.14], but the evidence for the latter effect was only anecdotal, BF_1 = 1.54.[3]

EPT. Overall, EPT ME scores in Experiment 2 did not differ significantly from zero (*M* = -1.02, *SD* = 120.87), *t*(598) = -0.21, *p* = .84, d_z = 0.01, 95% CI = [-10.72, 8.67], BF_0 = 21.30. An ANOVA on EPT ME scores that included Memory, Task Order and Stimulus Pair as factors revealed a significant main effect of Memory, *F*(2, 593) = 3.98, *p* = .019. We observed a contrast ME effect for participants with indiscriminate memory (*M* = -34.09, *SD* = 162.43), *t*(137) = -2.47, *p* = .015, d_z = -0.21, 95% CI = [-61.43, -6.75], but evidence for this effect was only anecdotal, BF_1 = 1.75. We did not observe significant ME effects for participants with accurate or reversed memory, *t*s < 1.84, *p*s > .068, d_zs < 0.11, BF_0s > 3.26, or for participants with accurate memory for any of the stimulus pairs, *t*s < 1.58, *p*s > .11, d_zs < 0.12, BF_0s > 3.68 (Table 2). Additional between-subjects *t*-tests indicated that standardized IAT ME scores were significantly larger than standardized EPT ME scores for Experiment 2 participants with accurate memory for the 0-2 pair, *t*(294) = 3.00, *p* = .001, BF_1 = 18.04, but not the 1-12 pair, *t*(392) = 0.49, *p* = .31, BF_0 = 5.84.

AMP. AMP ME scores in Experiment 3 were significantly higher than zero, indicating a ME effect on AMP performance (*M* = 1.02%, *SD* = 18.33%), *t*(2867) = 2.97, *p* = .003, d_z = 0.06, 95% confidence interval (CI) = [0.34%, 1.69%], BF_1 = 11.16. An ANOVA on AMP ME scores that included Stimulus Pair (0-12, 0-2, 2-12) as within-subject factor and Memory (accurate, indiscriminate, reversed) and Task Order (implicit evaluation task first, explicit evaluation task first) as between-subjects factors revealed only a main effect of Task Order, $\chi^2(1)$ = 4.22, *p* = .040, indicating bigger ME effects when participants started with the AMP, but not any other main or interaction effects, χ^2s < 6.99, *ps* > .13. Planned follow-up *t*-tests indicated that participants with accurate memory significantly preferred the frequent word (*M*

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[3] Note that the effect was significant at the Bonferroni-corrected *p*-value for two comparisons of *p* = .025.

= 1.29%, *SD* = 17.81%), *t*(2237) = 3.43, *p* < .001, d_z = 0.07, 95% CI = [0.55%, 2.03%], BF_1 = 52.57. We did not observe a ME effect for participants with indiscriminate or reversed memory, *ts* < 0.45, *ps* > .65, $d_z s$ < 0.03, BF_0s > 2.98. The ME effect for participants with accurate memory was significant for the 0-12 pair (*M* = 1.79%, *SD* = 18.96%), *t*(807) = 2.68, *p* = .008, d_z = 0.09, 95% CI = [0.48%, 3.10%], BF_1 = 8.10, and the 0-2 pair (*M* = 1.31%, *SD* = 17.12%), *t*(710) = 2.04, *p* = .042, d_z = 0.08, 95% CI = [0.48%, 2.57%], BF_1 = 1.99, but not the 2-12 pair (*M* = 0.72%, *SD* = 17.14%), *t*(718) = 1.12, *p* = .26, d_z = 0.04, 95% CI = [-0.54%, 1.97%], BF_0 = 2.22 (Table 3).

In-line with our pre-registered data analysis plan, we performed additional analyses excluding participants who reported low task engagement in the ME task (mean score < 3; overall mean = 4.66, *SD* = 2.09). In these analyses, the same effects were significant as in the other analyses with the exception that we now observed a significant interaction effect of Task Order and Memory, $\chi^2(2)$ = 10.63, *p* = .005, indicating a Memory main effect for participants who started with the explicit rating task, $\chi^2(2)$ = 9.18, *p* = .010, but not for participants who started with the AMP, $\chi^2(2)$ = 4.33, *p* = .11.[4]

Compound analysis. The performed ANOVA's on implicit evaluation scores did not provide clear evidence for a moderation of ME effects by Memory or Stimulus Pair. To explore whether this might be due to a lack of statistical power in these analyses, we decided to perform additional ANOVA's on standardized IAT and AMP ME scores (but not EPT ME scores) for participants in all experiments. These analyses also allowed us to compare ME effects on IAT and AMP scores.

First, an ANOVA that included Memory and Implicit Evaluation Task revealed a main effect of Task, *F*(1, 4203) = 6.07, *p* = .014, indicating stronger effects on IAT scores than on AMP scores. More importantly, we also observed a main effect of Memory, *F*(2, 4203) =

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[4] We also performed pre-registered exploratory analyses with different procedures for coding memory accuracy. These analyses generally produced similar results. A report of these analyses can be found on the OSF webpage.

3.99, *p* = .019. There was a significant ME effect for participants with accurate memory (*M* = 0.11, *SD* = 0.97), *t*(2954) = 6.02, *p* < .001, d_z = 0.11, 95% CI = [0.07, 0.14], BF_1 >10.000, or with indiscriminate memory (*M* = 0.08, *SD* = 1.06), *t*(676) = 2.06, *p* = .039, d_z = 0.08, 95% CI = [0.004, 0.165], BF_1 = 2.14, but not for participants with reversed memory (*M* = 0.01, *SD* = 1.07), *t*(576) = 0.18, *p* = .86, d_z = 0.01, 95% CI = [-0.08, 0.10], BF_0 = 5.44. Note that the evidence for a ME effect for participants with indiscriminate memory was only anecdotal. However, there is substantial evidence for the absence of a difference in effect sizes between participants with accurate and indiscriminate memory, BF_0 = 5.34.

Second, we performed an ANOVA for participants with accurate memory that included Implicit Evaluation Task (AMP, IAT) as well as 2 Stimulus Pair variables: (1) Infrequent Stimulus Presentation (whether the infrequent stimulus was presented or not) and (2) Difference in Number of Presentations for the Frequent and Infrequent Stimulus (range 2-10). This revealed a main effect of Evaluation Task, *F*(1, 2950) = 17.35, *p* < .001, as well as a main effect of Infrequent Stimulus Presentation, *F*(1, 2951) = 4.01, *p* = .045. The ME effect was stronger when the infrequent stimulus was not presented (*M* = 0.14, *SD* = 0.98) than when it was presented (*M* = 0.04, *SD* = 0.96). Notably, however, ME effects were significant for both types of stimulus pairs, *p*s < .001, BF_1s > 79.

Explicit Evaluation

Experiments 1 and 2. Overall, explicit rating ME scores were significantly higher than zero, indicating a ME effect on explicit ratings (*M* = 0.13, *SD* = 2.01), *t*(1939) = 2.93, *p* = .003, d_z = 0.07, 95% CI = [0.04, 0.22], BF_1 = 11.82. An ANOVA on explicit rating ME scores that included Memory, Task Order and Stimulus Pair as factors did not reveal any main or interaction effects, *F*s < 1.71, *ps* >.16. The ME effect was significant for participants with accurate memory (*M* = 0.19, *SD* = 1.99), *t*(1081) = 3.12, *p* = .002, d_z = 0.09, 95% CI = [0.07, 0.31], BF_1 = 25.44, and non-significant for participants with reversed or indiscriminate memory, $ts < 1.34$, $ps > .18$, $d_z s < 0.06$, $\mathrm{BF}_0 s > 1.52$. We observed a significant ME effect for participants with accurate memory for the 0-2 pair ($M = 0.38$, $SD = 2.03$), $t(373) = 3.63$, $p < .001$, $d_z = 0.19$, 95% CI = [0.17, 0.59], $\mathrm{BF}_1 = 146.28$, but not for any of the other pairs, $ts < 1.06$, $ps > .29$, $d_z s < 0.13$, $\mathrm{BF}_0 s > 4.52$ (Table 1).

Experiment 3. Explicit rating ME scores revealed a ME effect ($M = 0.09$, $SD = 1.84$), $t(2867) = 2.73$, $p = .006$, $d_z = 0.05$, 95% confidence interval (CI) = [0.03, 0.16], $\mathrm{BF}_1 = 5.66$. An ANOVA on explicit rating ME scores that included Memory, Task Order and Stimulus Pair as factors did not reveal any main or interaction effects, $\chi^2 s < 5.67$, $ps > .059$. The ME effect was significant for participants with accurate memory ($M = 0.13$, $SD = 1.85$), $t(2237) = 3.28$, $p = .001$, $d_z = 0.07$, 95% CI = [0.05, 0.20], $\mathrm{BF}_1 = 31.77$, and non-significant for participants with reversed or indiscriminate memory, $ts < -0.03$, $ps > .51$, $d_z s < 0.00$, $\mathrm{BF}_0 s > 4.41$. The ME effect for participants with accurate memory was significant for the 0-12 stimulus pair ($M = 0.15$, $SD = 1.84$), $t(807) = 2.35$, $p = .019$, $d_z = 0.08$, 95% CI = [0.02, 0.28], $\mathrm{BF}_1 = 8.10$, and the 0-2 stimulus pair ($M = 0.19$, $SD = 1.83$), $t(710) = 2.83$, $p = .005$, $d_z = 0.11$, 95% CI = [0.06, 0.33], $\mathrm{BF}_1 = 12.43$, but not for the 2-12 stimulus pair ($M = 0.04$, $SD = 1.88$), $t(718) = 0.52$, $p = .61$, $d_z = 0.02$, 95% CI = [-0.10, 0.17], $\mathrm{BF}_0 = 4.43$.

Compound analysis on implicit and explicit evaluation

An ANOVA on standardized implicit and explicit evaluation ME scores (excluding EPT scores) that included Memory, Task Order, Implicit Evaluation Task, and Type of Evaluation Task (implicit/explicit) revealed a significant main effect of Memory, $\chi^2(2) = 6.67$, $p = .036$, and a main effect of Type of Evaluation Task, $\chi^2(1) = 4.41$, $p = .036$, indicating stronger ME effects on implicit than explicit evaluation tasks.[5]

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[5] We also performed statistical mediation analyses that indicated that the ME effect on implicit evaluation task performance was not mediated by changes in explicit ratings. In contrast, we did observe full mediation of the ME effect on explicit ratings by changes in IAT scores in Experiment 1 and 2 but this pattern did not replicate for AMP scores in Experiment 3.

Discussion

In three experiments, we examined ME effects on implicit stimulus evaluations. Results showed that the frequent and infrequent presentation of non-existing words can produce significant changes in implicit evaluations of these words. The ME effect on implicit evaluations, however, depended on a number of boundary conditions. First, it was dependent on the task that was used to capture implicit evaluations. We obtained strong evidence for a ME effect on IAT scores (BF_1s > 190) and AMP scores ($BF_1 > 11$) and substantial evidence that ME does NOT influence EPT scores ($BF_0 > 21$). Second, we obtained evidence that the ME effect depends on participants' memory of the ME experience. We observed a robust ME effect on IAT and AMP scores only when participants had accurate memory of which stimulus had been presented more often in the ME task. Third, our results indicate that the ME effect depends on the number of stimulus presentations. That is, we only observed a robust ME effect on IAT or AMP scores when the frequent word was presented two or twelve times and the infrequent word was never presented (i.e., for the 0-2/0-12 stimulus pairs). When the infrequent word was presented, ME effects were smaller overall and significant effects were only found when the infrequent word was presented only once and the frequent word was presented more than six times (i.e., for the 1-12 pair but not the 1-3, 1-6, or 2-12 pairs).

ME influences implicit evaluation

The presence of a ME effect on implicit stimulus evaluations is important for a number of reasons. First, due to the generality of the ME effect, the widespread application of ME procedures for changing stimulus evaluation, and the relevance of implicit evaluation for behavior, this finding may have practical importance. Second, it also has important theoretical implications. More specifically, it contrasts with an important assumption of the processing fluency/attribution account of ME effects that these effects depend on the explicit measurement of evaluation. In Experiments 1 and 3, ME influenced IAT scores independent of whether the IAT was performed before or after participants provided their explicit liking ratings. In Experiment 2, ME influenced IAT scores even though the IAT always preceded the explicit rating task. In other words, participants showed evidence of a ME effect on IAT scores without first actively reporting on the quality of their explicit evaluations. These findings accord with the assumption of other theoretical accounts that ME leads to an immediate change in stimulus liking even when participants do not have the task to rate their liking of the crucial stimuli (e.g., the hedonic/fluency account, propositional accounts). Our results also show that ME can influence implicit evaluations despite the fact that stimuli were exposed many times during the IAT, EPT, and AMP procedures. To further explore this issue, we performed additional analyses which showed that (1) effects on explicit evaluations were observed even for participants who first completed implicit evaluation tasks and (2) ME effects were observed on AMP scores in Experiment 3 even when excluding the first block of AMP trials. This resilience to re-exposure accords with the idea that propositional knowledge of stimulus frequencies during the ME phase drives ME effects rather than fluency experiences that result from repeated exposure (and therefore should not survive the following exposures).

Our results also suggested that ME effects were stronger on implicit evaluations (except for the EPT effects) than on explicit evaluations. In accordance, we found evidence for a full mediation of ME effects on explicit rating scores by effects on IAT scores At first glance, these results are in-line with previous theorizing that ME effects are the result of implicit processes (e.g., in the sense of unaware) and that such evaluations can be more easily probed with implicit evaluation measures (Kawakami, 2012). However, this does not fit with findings indicating an important role for memory for stimulus frequencies in ME effects. An alternative explanation is that explicit evaluation measures emphasize validation which might lead some participants to refrain from using the frequency of stimulus presentations as a basis for evaluation. For instance, participants who learn that there is a difference in the number of stimulus frequencies might easily infer liking on the basis of this regularity. However, when asked to explicitly report their liking they might refrain from using this information because they do not consider it a good enough reason for changing liking. As we discuss below, this idea also accords with our findings regarding the moderating role of the number of stimulus exposures on ME effects on implicit and explicit evaluation. Note, however, that the overall difference in effect sizes between effects on implicit and explicit evaluations was very small and evidence for this difference in effect sizes was only anecdotal.

Moderators of ME effects on implicit (and explicit) evaluation

The observation that a ME effect on implicit evaluations was observed only under certain conditions also raises many interesting issues. First, the dissociation between ME effects on the EPT versus AMP and IAT could be due to the fact that effects on the EPT tend to be smaller and more unreliable than those on IAT and AMP (De Houwer et al., 2009; Wittenbrink, 2007).[6] Hence, the EPT might simply have failed to capture implicit evaluations. In-line with this idea, correlational analyses revealed only a non-significant correlation between EPT scores and explicit rating scores. This finding is noteworthy because especially for novel stimuli such as unfamiliar non-existing words stronger correlations between implicit and explicit evaluations are typically observed (Nosek, 2005). Another possibility is that EPT procedures hamper the observation of ME effects specifically. In accordance, it has been observed that although other evaluative learning procedures that include novel stimuli can lead to reliable EPT effects (e.g., approach-avoid learning: Van Dessel, De Houwer, Gast, & Smith, 2015), ME instructions do not (see Van Dessel et al., 2017 for a discussion). One

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[6] To further investigate ME effects on EPT scores, we also performed analyses with item-based linear mixed effects models (Bates, Mächler, Bolker, & Walker, 2014). This approach allowed us to investigate participants' raw reaction times (RTs) rather than an index of their performance as combined in one (unreliable) EPT ME score and to control for possible effects of counterbalancing factors such as the target words or prime words that were used. Importantly, however, the linear mixed effects regression analyses supported the conclusion of the main analyses that EPT performance was not influenced by ME.

potential explanation might be that evaluative priming effects result from semantic relatedness between target and prime stimuli rather than evaluative congruency (see Werner, Von Ramin, Spruyt & Rothermund, 2018). Whereas other evaluative learning procedures might allow novel stimuli to become semantically related to EPT targets (e.g., a novel stimulus that is repeatedly avoided might become related to target words such as ‘Loss’ or ‘Lonely’ which might allow for an evaluative priming effect), this might not be the case for ME (e.g., because presentation frequency does not readily relate to any of the EPT targets). Though further research is needed to test such explanations, it is clear that the current results highlight the importance of using multiple implicit measures of attitudes to avoid equating implicit evaluations with any one measurement procedure.

Another important finding of our studies is that ME produced robust effects only for participants with accurate memory. This result is at odds with the proposal by Bornstein (1989) that memory of stimulus presentations is an important *inhibitor* of ME effects and reduces the size of ME effects. Rather, these results are in line with those of earlier studies showing that the ME effect necessarily involves conscious awareness of (1) the stimulus presentations (Brooks & Watkins, 1989; de Zilva, Vu, Newell, & Pearson, 2013; Newell & Shanks, 2007; Szpunar, Schellenberg, & Pliner, 2004) and (2) the frequency of occurrence of the stimuli (Stafford & Grimes, 2012). Our results extend these findings by showing that recognition memory also moderates ME effects on implicit evaluations. This contrasts with the assumption that ME effects observed on implicit evaluation measures more strongly reflect fluency-based processes that do not depend on conscious knowledge of stimulus frequencies (e.g., Kawakami, 2012). Moreover, results strongly resemble findings that contingency awareness is a potent moderator of EC and approach-avoidance effects on implicit and explicit evaluation (see Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010; Van Dessel, De Houwer, & Gast, 2016).

One notable exception is that we did not observe reversed ME effects for participants with reversed memory, whereas reversed effects been observed in EC and approach-avoidance research (e.g., Van Dessel et al., 2016). Though this could be due to a lack of power to observe (typically smaller) reversed effects in our studies, it could also indicate that ME effects have different characteristics compared to EC and approach-avoidance effects (e.g., with regard to automaticity features). Indeed, our results do not provide definitive evidence that ME effects are non-automatic (e.g., in the sense of controlled or conscious). For instance, we measured participants’ memory of which of two stimuli occurs most often with a single question that followed the ME task and evaluation tasks. It is possible that a third variable determines both ME effects and participants’ answer to this question such that more robust ME effects are observed when participants have accurate memory. For instance, fluency experiences or familiarity feelings might not only facilitate stimulus liking but also a higher frequency response in the memory test. Hence, it is at least possible that participants exhibited ME effects and accurate memory yet were unaware of the number of presentations during evaluation. Another possibility is that participants with accurate memory were more attentive or engaged in the experiment and this moderated both ME effects and memory test performance. In contrast with this attention explanation, however, Experiment 3 found a moderation of AMP ME effects by memory even for participants who reported high ME task engagement. In addition, Wang and Chang (2004) found that participants preferred a stimulus they were not familiar with - but that they mistakenly classified as being familiar - over a stimulus that they had been exposed to before - but that they mistakenly classified as unfamiliar. Thus, the judged old/new status of a stimulus was more important to determine the liking of a stimulus than the actual previous exposure to the stimulus. This result suggests that memory is an important *causal* factor for the ME effect, rather than merely being *correlated* with it. The current results thus add to the cumulating evidence that memory of the stimuli and the stimulus presentations is an important precondition, rather than a limiting factor, for the ME effect (see also Montoya et al., 2017).

Interestingly, our results provided (anecdotal) evidence for a ME effect for participants with indiscriminate memory and substantial evidence for the absence of a difference in effect sizes between participants with accurate and indiscriminate memory. Though this might indicate that a proportion of the ME effect is not dependent on frequency memory, it could also reflect a ME effect for (1) participants who were able to retrieve frequency memory during the evaluation task but not the memory task or (2) participants who misinterpreted the memory task.

Finally, our results also revealed another important boundary condition of ME effects, that is, the number of stimulus exposures. ME effects were generally bigger when the infrequent stimulus was never presented. Moreover, for the 0-2 pair we consistently found a robust ME effect implicit and explicit evaluation, but not for pairs with larger differences in the number of stimulus presentations (except for a relatively smaller ME effect for the 0-12 and 1-12 pairs). This downturn in the frequency-affect curve has been observed in other ME studies as well (Harrison, 1977; Zajonc, Shaver, Tavris, & van Kreveld, 1972; see Montoya et al., 2017 for a review). One popular explanation of this observation is that participants may engage in a correction process and consciously revise their initial evaluation when they become more strongly aware of the differences in occurrences (Gilbert, 1989; Trope, 1986). In-line with this idea, we obtained initial evidence that the negative impact of exposure frequency on ME effects depends on the task that is used to measure evaluations. More specifically, we found robust evidence for ME effects on implicit evaluations not only for the 0-2 pair but also for the 1-6, 1-12, and 0-12 pair. In contrast, we only found strong evidence for an effect on explicit evaluations for the 0-2 pair. One possible explanation for this dissociation is that for the 0-2 pair participants have less motivation to control against changes in liking because the frequent stimulus is only presented on two occasions (which might, for instance, prevent boredom due to overexposure; see Van den Bergh & Vrana, 1998). The current results thus suggest that implicit evaluation measures can be an important addition to explicit evaluation measures in the context of ME effects in that they might sometimes capture ME effects that are not registered with explicit evaluation measures (e.g., due to controlled correction processes related to overexposure; see Kawakami et al., 2010 for corroborative evidence). This accords with recent evidence that certain learning procedures such as approach-avoidance training sometimes influence implicit but not explicit evaluation when participants do not consider the learned regularity a good basis for their evaluation (Van Dessel, De Houwer, Gast, Smith, & De Schryver, 2016).

Limitations

An important limitation of our studies is that they do not provide clarity about the reasons why ME effects on implicit evaluations were influenced by certain moderators such as the number of stimulus presentations or the nature of the implicit evaluation task. Our study was originally set up with a propositional account of ME effects in mind, for which we found initial support in a study that revealed effects of ME instructions on implicit evaluations (Van Dessel et al., 2017). It is noteworthy that the current results seem to mirror those of the ME instruction study. In that study, effects of ME instructions were observed only on implicit evaluations measured with an IAT and AMP and not with an EPT. Moreover, ME instructions influenced evaluations only when participants could correctly report which of the two words would occur most often and ME instruction effects were bigger on implicit than on explicit evaluation measures. These similarities might be viewed as indirect support for the idea that ME effects (in part) depend on similar (propositional) mechanisms as ME instruction effects. On the other hand, the observed similarities between ME and ME instruction effects could also arise because the specific moderators influence the (distinct) mechanisms underlying ME and ME instruction effects to a similar extent.

Another important limitation is that we did not probe the relation between ME effects on implicit evaluation and real-world behavior. This requires further study especially given recent evidence that changes in implicit evaluations (as measured with the IAT) sometimes do not mediate changes in other relevant behavior (e.g., Forscher et al., 2017; but see: Friese et al., 2009). Finally, it is important to note that all the observed ME effects were of small effect size (all d_zs < 0.41). Of course that was not unique to effects on implicit measures; our data suggests that, if anything, effects are larger on implicit measures than on explicit measures. Hence, though our experiments were well-powered to find an overall ME effect, they had less statistical power to find robust evidence for effects of the different between-subjects factors and their interactions.

The current results thus provide many clues for future research that might look into ME (and ME instruction) effects on (implicit) evaluation and further test moderation by (1) the number of presentation frequencies, (2) memory, and (3) (implicit vs. explicit) evaluation measurement tasks (e.g., in different domains). These studies will not only allow us to gain more insight into the mental processes underlying ME effects but also implicit (and explicit) evaluation, memory, or human cognition in general. For instance, research examining why ME does not influence EPT effects can help us understand the cognitive underpinnings of priming-related mental processes whereas research on dissociative effects of the number of presentation frequencies on implicit and explicit evaluation might provide information about the controllability of ME and the (automaticity of) processes underlying reactance responses.

Importantly, however, the present studies do allow us to already make at least two important new conclusions with a high degree of confidence (1) ME procedures can influence implicit evaluations as measured with an IAT and AMP, and (2) this effect can occur even in the absence of explicit evaluation. This is important information that might shed new light on the mental mechanisms underlying ME (which has proven difficult so far; see Montoya et al., 2017). Moreover, the fact that our data raise many new questions is important because those questions are likely to stimulate new research. We therefore hope that our studies will provide the basis for many important future discoveries.

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