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BRIEF REPORT

Absence of a Mere-Exposure Effect in Older and Younger Adults

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The mere-exposure effect, in which repeated stimuli are liked more than novel stimuli, is a well-known effect. However, little research has studied adult age differences in mere-exposure effects, despite possible applications in helping older adults transition to new living environments. Here, we report four experiments assessing mere-exposure to neutral-face stimuli in groups of older and younger adult participants tested online. In each experiment, repeated face exposure did not increase liking within either age group; rather, Bayesian evidence favored the null hypothesis of no effect. Older adults reported higher overall liking ratings relative to younger adults, and both groups preferred younger faces, though this tendency was stronger in the younger group. Further exploratory analysis considering factors such as gender or race of the faces and participants did not reveal any consistent results for the mere-exposure effect. We discuss these findings in relation to other recent studies reporting mixed evidence for mere-exposure effects.

Public Significance Statement
We sought to apply repetition-induced increase of liking (known as the "mere-exposure effect") to ease the transition of older adults into group living environments by exposing them to photos of staff and fellow residents' faces prior to moving to an extended-care facility. Surprisingly, we found no evidence across four experiments that repeating faces increased participants' liking of them, suggesting empirical and applied limitations to the mere-exposure effect. These findings set the stage to further explore how the mere-exposure effect may manifest in older adults in comparison to younger adults.

Keywords: mere-exposure, implicit, liking, aging

Repetition effects are pervasive throughout psychology, including neuroscience, perception, cognition, and social behavior. Repetition of the same sounds results in habituation, seen in the brain as reduced neural responsivity or repetition suppression (Buchsbaum et al., 2015; Schacter & Buckner, 1998). Repeating information results in the ability to remember and can make plausible statements seem true (Fazio & Sherry, 2020; Hasher et al., 1977; Karpicke & Bauernschmidt, 2011). Repeatedly meeting someone can even facilitate falling in love (see Aron et al., 1989). Indeed, repetition effects occur in both intentional situations (as in learning; e.g., Gluck et al., 2020) and in incidental or implicit situations (Reber, 2013), such as sequence learning (Hebb, 1961; Turk-Browne et al., 2005), decision-making (Stafford & Grimes, 2012), or knowledge of event occurrence frequency (Zacks & Hasher, 2002). Repetition via practice can lead to expertise across various domains (Ericsson et al., 1993; Macnamara & Maitra, 2019).

Repetition can make stimuli seem more likable. From hearing songs repeated to growing familiar with a particular painting or person's face, a large literature on the *mere-exposure effect* has shown that repeating stimuli increases affection and facilitates cognitive and perceptual processing (e.g., Abakoumkin, 2018; Bornstein, 1989; Montoya et al., 2017; Smit et al., 2022; Zajonc, 1968). For instance, many studies have shown that repeated faces are rated more positively (e.g., happier; Carr et al., 2017) than new

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Stephen Rhodes, Jason K. Chow, and Lynn Hasher developed the study concept. Stephen Rhodes collected data and conducted analysis; Lynn Hasher, Bradley R. Buchsbaum, and Nicholas O. Rule provided supervision. Lynn Hasher and Stephen Rhodes wrote the first draft, and all authors provided critical revisions. All authors approved the final version of the article for submission.

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faces. Moreover, liking increases as a function of the number of exposures (or repetitions), at least up to some limit (de Zilva et al., 2016; Zajonc, 1968).

We originally sought to apply the mere-exposure effect as an intervention to improve the experiences of older individuals moving to new environments. Specifically, we wondered whether repeatedly exposing new residents of extended-care facilities to images of caretakers and fellow residents might promote their adaptation to the new environment and possibly improve their health by mitigating the challenges associated with moving (Brandburg, 2007; Kahn, 1999) and by capitalizing on the beneficial effects of positive mood (Ong et al., 2011). In this context, the mere-exposure effect could be a useful tool for efficiently integrating older adults into their new environments.

We anticipated that older adults would show a mere-exposure effect on the assumption that these effects can be implicit or rely on fluency (e.g., Dywan & Jacoby, 1990), both of which have been proposed as mechanisms underlying the effect (e.g., Bornstein & D'Agostino, 1992; Reber et al., 1998). Testing both older and younger adults, we based the present procedures on parameters successfully employed in previous work. We chose faces of different ages, races, and genders with neutral emotional expressions (Meskin et al., 2013). Across four experiments, we varied the exposure duration and repetition count of these faces. As mere-exposure effects have been reported with small-to-medium effect sizes, we carefully ensured that we had sufficient statistical power in each experimental group. This resulted in a well-powered study using a variety of experiment parameters previously shown to successfully induce a mere-exposure effect. We assumed that younger adults would exhibit a mere-exposure effect (with faces seen more often rated more favorably). We also expected older adults to show a mere-exposure effect—and larger than that seen in the younger adults because the increase in fluency with repetition is a particularly strong cue for older participants (Dywan & Jacoby, 1990).

Method

Transparency and Openness

This study, the hypotheses, and analyses were not preregistered. We report all deidentified data, measures, manipulations, and exclusions along with code to replicate the analysis in the associated Open Science Framework (OSF) page (https://doi.org/10.17605/OSF.IO/Q7WMJ). This study and their data have not been reported elsewhere.

Participants

Power analysis was performed using G*Power 3 (Faul et al., 2007). To detect a small-to-medium main effect of repetition in each age group ($f = .20$, $\eta_p^2 = .04$), or a small-to-medium group-by-repetition interaction in the full sample ($f = .14$, $\eta_p^2 = .02$), we used at least 44 participants per age group for 90% power.

Participants were recruited online via prolific.co (Palan & Schitter, 2018) and directed to an experiment created with PsychoPy3 hosted on pavlovia.org (Peirce et al., 2019). Prolific's background questionnaire information assured that no participants reported a history of head injury resulting in loss of consciousness, ongoing mental health conditions, or diagnosis of mild cognitive impairment or dementia; all indicated U.K., U.S., or Canadian nationality. We did not collect information about participants' racial or ethnic backgrounds. We recruited 50 older and 50 younger adults for each of the four experiments but excluded several because of missing data files or problems following instructions (e.g., not responding quickly enough on any trial); final sample sizes, E1: 92, E2: 95, E3: 96, and E4: 96 (Table 1).

Stimuli

We selected 48 neutral faces from Minear and Park (2004) database, balancing self-reported gender and age: 24 younger adults (18–35 years old; half women, half men) and 24 older adults (60–80 years old; half women, half men). Of these, 36 self-identified as White, seven as Black, and five as another race. The same 48 images were used in Experiments 1–3, but a new set was sampled from the same database for Experiment 4 (35 White, 8 Black, 5 other). Twenty-four images appeared during the exposure phase and all 48 images appeared during the rating phase.

Procedure

Each experiment started with an exposure phase. Participants viewed 24 faces individually and judged whether each was female or male via key press (F or M, respectively) as a cover task to maintain attention and assure identity processing without drawing attention to

Table 1
Participant Demographics in Each Experiment

	Experiment 1		Experiment 2		Experiment 3		Experiment 4	
Age group	Older	Younger	Older	Younger	Older	Younger	Older	Younger
N	44	48	45	50	47	49	46	50
n_{female}	21	20	28	21	21	26	24	20
n_{male}	23	28	17	28	26	23	22	30
$n_{\text{U.K.}}$	43	27	35	39	34	37	30	35
$n_{\text{U.S.}}$	1	9	10	10	12	10	15	1
n_{Canada}	0	12	0	1	1	2	1	14
M_{age}	69.80	26.33	69.67	24.90	69.06	25.98	68.98	26.68
SD_{age}	4.38	4.44	4.65	4.67	3.27	4.94	3.99	5.17
Age range	65–83	18–35	65–85	18–35	65–77	18–35	65–80	18–35

specific features related to liking. On each trial, a fixation cross appeared for 400 ms followed by a face for 100 ms (Experiment 1), 500 ms (Experiment 2), or 1,000 ms (Experiments 3 and 4). To give time to respond, a blank screen followed until 1,200 ms elapsed from the onset of the face (e.g., 1,100 ms in Experiment 1). Images in Experiments 1–3 repeated 3, 5, or 7 times (eight images per repetition frequency), and either 1 or 3 times in Experiment 4 to shorten the task (and all for 1,000 ms). The next trial began immediately after the response window. Faces appeared in a pseudorandom order for each participant, constrained such that a face could not immediately repeat. Following the exposure phase, participants completed an online version of the Morningness Eveningness Questionnaire (Horne & Östberg, 1976), inserting approximately 5–10 min between the exposure and rating phases.

During the rating phase, participants viewed all 48 images (half seen during exposure, half new; counterbalanced between participants via six random-assignment lists) individually in random order and were asked to rate "How much would you like this person?" from 1 (*not at all*) to 5 (*quite a bit*) using the number keys on their keyboard. Participants had 4,000 ms to provide each rating with a 500-ms interstimulus interval.

Analysis

Average liking ratings were analyzed via mixed-model analysis of variance using the ez package for R (Lawrence, 2016). To quantify the weight of evidence for or against the effects of interest, we also computed Bayes factors using Jeffreys–Zellner–Siow default priors (Rouder et al., 2012) with a prior *r* scale factor of 0.5, implemented in the BayesFactor package (Morey & Rouder, 2018). Unlike frequentist statistics, Bayesian analysis quantifies the relative evidence between competing hypotheses, with BF_{10} referring to a Bayes factor favoring a particular effect or interaction against a null. To ease interpretation, we also report BF_{01}, which is simply the inverse of BF_{10} and indexes evidence for the null hypothesis against the alternate. Bayes factors are interpretable without cutoffs, but we follow popular conventions (Jeffreys, 1961) where $BF_{10} > 3$ is the threshold for substantial evidence toward a hypothesis, whereas $BF_{10} = 1–3$ is only anecdotal evidence (though still leaning toward the alternate hypothesis), conversely $BF_{10} < 1/3$ is the threshold for substantial evidence toward the null hypothesis.

Results

Average liking ratings are presented in Figure 1. The main effect of repetition did not reach statistical significance in any of the four experiments (Table 2). Moreover, there was substantial evidence ($BF_{01} > 16.41$) against a main effect of repetition on liking ratings in each experiment. Older adults had higher overall liking ratings than younger adults, though the age group's main effect was significant in only Experiments 2–4. Finally, there was no evidence of an age group by repetition interaction with Bayes factors favoring the null ($BF_{01} > 2.17$).

Separate analyses of the data from older and younger adults confirmed the absence of a repetition effect in both groups with BF_{01} values of 6.04, 11.32, 22.98, and 9.70 for older adults and 11.98, 21.82, 1.76, and 10.69 for younger adults in Experiments 1–4, respectively.

Exploratory Analyses

We conducted a series of exploratory analyses to better understand why the mere-exposure effect did not replicate. We first combined the data from Experiments 1–3 and included presentation time (100, 500, and 1,000 ms) during the exposure phase as a between-subjects factor. For the liking score, older adults gave higher ratings than younger adults, $F(1, 277) = 14.71$, $p < .001$, $\eta_p^2 = .05$, $BF_{10} = 128$. The interaction between age and number of repetitions was not significant and the Bayes factor supported the null hypothesis over the alternative, $F(3, 831) = 2.64$, $p = .058$, $\eta_p^2 = .01$, $BF_{01} = 5.96$. No other main effects or interactions approached significance, $F\text{s} < 1.23$, $p\text{s} > .28$, $\eta_p^2 = .01$, $BF_{01} > 6.00$.

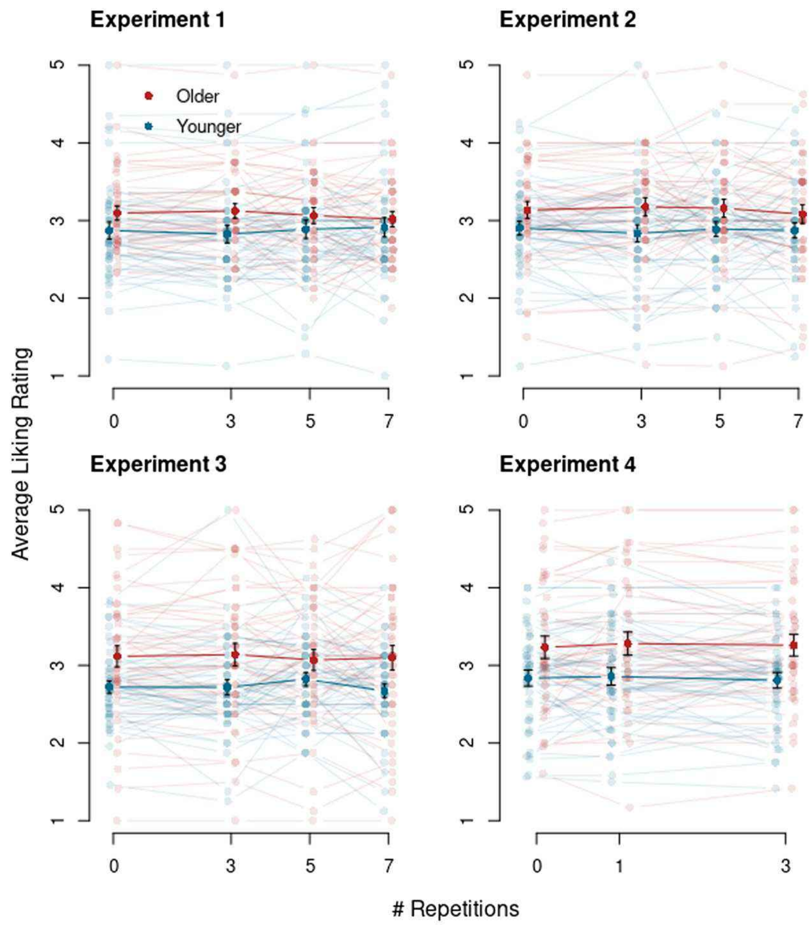
We then conducted several other exploratory analyses that we describe briefly below (see OSF repository for details). First, several participants had low accuracy on the gender-discrimination cover task during the exposure phase, suggesting that they did not attend to the faces or sufficiently engage with the experiment. Excluding participants who failed to correctly identify the targets' gender on 15% or more trials did not change the pattern of results for liking ($F\text{s} < 1.94$, $p\text{s} > .12$, $\eta_p^2 < .003$).

Second, we considered participants' responses to the Morningness Eveningness Questionnaire and their time of participation. Consistent with Horne and Östberg (1976), we categorized participants scoring 41 or lower to have an evening preference and those scoring 59 or greater to have a morning preference; those in between were deemed to have no strong preference. We then used the local time at which the experiment was accessed online to determine who participated during their preference-matched time of day. Experiment 2 exhibited a significant interaction between time-preference group and repetitions, $F(6, 267) = 2.23$, $p = .04$, $\eta_p^2 = .007$, and Experiment 4 exhibited a three-way interaction between age group, time-preference group, and repetition, $F(4, 180) = 3.68$, $p = .007$, $\eta_p^2 = .007$. Other possible mere-exposure effects with time-of-day preference did not reach significance ($F\text{s} < 1.17$, $p\text{s} > .32$, $\eta_p^2 < .003$). Follow-up analysis of Experiments 2 and 4 that repeated the main analysis separately for each time-preference group only resulted in a single significant mere-exposure effect in Experiment 4 among participants who did not participate at their preferred time. However, this age by repetition interaction effect in the group tested at nonpreferred times, $F(2, 26) = 3.41$, $p < .048$, $\eta_p^2 = .07$, only included two older adults. No other effects reached significance ($F\text{s} < 2.07$, $p\text{s} > .13$, $\eta_p^2 < .002$).

Finally, we explored the target faces in more detail. Rather than average the stimuli and aggregate responses, we fit an ordinal regression model to the trial-level liking ratings separately for each experiment (Bürkner & Vuorre, 2019) via the brms package (Bürkner, 2017, 2018) with target gender (Male/Female), race (Black/White/other), age (Younger/Older), number of repetitions, and participant age group as fixed effects, including interactions between the age group of the face and age group of the participant and between the age group of the participant and number of repetitions. The model also contained a random intercept and effect of the number of repetitions for participants, and a random item

Figure 1

Average Liking Rating by Number of Repetitions During the Exposure Phase in Experiments 1–4. Transparent Points Show Individuals and Error Bars Are Within-Subject Standard Errors



Note. See the online article for the color version of this figure.

effect (i.e., for the particular image presented on a given trial). To ease interpretation, we report 95% credibility intervals (95% CI) for regression coefficients. These are similar to frequentist confidence intervals but have a more straightforward interpretation of the true parameter having a 95% probability to lie within the interval.

In every experiment, the regression coefficient 95% CIs include zero for the repetition main effects and the interactions between repetition and participant age, therefore providing evidence for the null hypothesis as in the main analyses.

Only a single effect relating to participant age was consistently observed across the four experiments: an interaction between participant age and face age, E1: $\beta = 0.07$, 95% CI [0.02, 0.13]; E2: $\beta = 0.10$, 95% CI [0.05, 0.16]; E3: $\beta = 0.13$, 95% CI [0.08, 0.19]; and E4: $\beta = 0.06$, 95% CI [0.01, 0.12].

Both groups rated younger faces more positively than older faces, particularly for younger adults. This was evidence for a main effect of face age in E1 (2.86 vs. 3.09, $\beta = -0.27$ 95% CI [−0.44, −0.10]), E2 (2.89 vs. 3.11, $\beta = -0.19$, 95% CI [−0.37, −0.03]), and E3 (2.77 vs. 3.05, $\beta = -0.30$, 95% CI [−0.49, −0.12]); the main effect only trended toward a true effect in E4 (2.93 vs. 3.15, $\beta = -0.17$, 95% CI [−0.39, 0.05]). Other stimulus-demographic effects were inconsistent (see Online Supplemental Materials).

Discussion

With the goal of ultimately developing an intervention to improve the social and physical well-being of older adults moving into new residences, we conducted four studies to establish that the mere-exposure effect extends to older adults. We failed to find evidence for a mere-exposure effect across all four experiments; however, even among the younger adult participants intended to serve as validating controls for the older adult participants. In fact, we found strong Bayesian evidence supporting the null hypothesis of no mere-exposure effect.

Despite following methods and procedures previously reported for mere-exposure effects (e.g., Bornstein, 1989; Montoya et al., 2017), we were unable to detect the existence of an effect. Indeed, we invested considerable effort to create a task that should successfully reproduce the mere-exposure effect for later adaptation to an older adult population. Specifically, we used emotionally neutral

Table 2
ANOVA Results for Average Liking Rating by Experiment

Effect	*df*	*F*	*p*	BF_{10}	BF_{01}	η_p^2
Experiment 1						
Age	1, 90	2.64	.11	0.94	1.06	.03
Repetitions	3, 270	0.07	.96	0.01	79.35	<.001
Age × Repetitions	3, 270	2.33	.09	0.46	2.17	.003
Experiment 2						
Age	1, 93	4.84	.03	1.99	0.50	.04
Repetitions	3, 279	0.48	.67	0.02	48.30	.001
Age × Repetitions	3, 279	0.95	.41	0.08	12.45	.002
Experiment 3						
Age	1, 94	7.60	.01	5.65	0.18	.07
Repetitions	3, 282	0.77	.49	0.03	33.36	.001
Age × Repetitions	3, 282	1.94	.14	0.15	6.54	.003
Experiment 4						
Age	1, 94	9.51	.003	11.20	0.09	.08
Repetitions	2, 188	0.56	.56	0.06	16.41	.001
Age × Repetitions	2, 188	0.29	.74	0.08	11.98	<.001

Note. ANOVA = analysis of variance; BF = Bayes factor.

faces, short exposure durations during encoding, a task judgment that has been used before, and a brief delay between the end of the exposure phase and the beginning of the rating phase. Using these proven procedures, alongside some variations to further rule out edge cases, we did not elicit a mere-exposure effect. However, we replicate previous research demonstrating that younger faces are more liked than older faces (Ebner, 2008), demonstrating that our procedures can detect an experimental effect and our nonreplication is unlikely due to an error in the task or an anomaly in the participants.

In addition, we note that we had higher statistical power than previous research considering age and the mere-exposure effect. Of the four studies previously conducted, two compared age-matched participants with or without an Alzheimer's diagnosis (Willems et al., 2002; Winograd et al., 1999) and two compared normally aging older adults to young adults (Palumbo et al., 2021; Wiggs, 1993). Those studies had a range of sample sizes, from 10 (Willems et al., 2002) to 27 (Palumbo et al., 2021) participants, versus the nearly 100 in each of the experiments reported here. Our failure to find a mere-exposure effect with older adults, despite having larger samples, thereby represents the highest powered study within the small set of published findings on age and the mere-exposure effect. Considering the present and previous work together, further research is needed using different stimuli, exposure protocols, and rating procedures to assess whether older adults would exhibit a mere-exposure effect and, if so, how its magnitude compares to that seen in younger adults. For example, some studies include nonface unfamiliar stimuli (nonwords, Palumbo et al., 2021; Japanese ideograms, Wiggs, 1993), vary exposure cover task (identifying specific facial features, Winograd et al., 1999; passive viewing, Wiggs, 1993), or even ask participants to repeatedly rate faces during exposure (Palumbo et al., 2021). A specific combination of these procedures may be important for the mere-exposure effect in older adults.

Several other possibilities for the present failures to find a mere-exposure effect can be offered. For one thing, the social importance of faces may make them more resistant to the mere-exposure effect and account for the inconsistency in findings across multiple studies. More socially conscious participants may be reluctant to arbitrarily rate some faces as more likable than others, causing a restriction of range and potentially masking a mere-exposure effect. As well, and based on a debriefing survey, we did not succeed in making our task implicit. Most participants noticed that faces in the exposure phase repeated and were used again in the rating phase. However, a mere-exposure effect has been observed alongside explicit recognition in older adults (Wiggs, 1993; Willems et al., 2002; Winograd et al., 1999).

Adding to this, the absence of a time-of-day effect converges toward the conclusion that mere-exposure effects did not occur in these experiments. Specifically, the effect of match versus mismatch between circadian preferences and test time might have been expected on the assumption that mere-exposure is a largely implicit effect (e.g., Pugnaghi et al., 2019) and that implicit effects or intuitive processes are typically greater at off-peak times of day than are explicit or analytic ones (Bodenhausen, 1990; May et al., 2005). Not finding a time-of-day difference within the data here thus suggests that implicit processes (such as the mere-exposure effect) did not occur.

It is possible that there are limitations to the mere-exposure effect for faces. For example, small changes in context between presentation and testing can eliminate the mere-exposure effect for faces (e.g., de Zilva et al., 2013). However, a few recent reports have failed to find mere-exposure effects with other stimuli despite also having used large samples (Pugnaghi et al., 2019). Some researchers have reported mixed findings: Inoue et al. (2018) failed to find mere-exposure effects in one of four reported experiments, and Mrkva and Van Boven (2020) failed to find a mere-exposure effect in one of five studies.

Is it possible that our failure to detect a mere-exposure effect is due to the online testing format used here? Where this issue has been investigated, performance on online cognitive assessments has returned results similar to those observed in the laboratory (e.g., Cyr et al., 2021; Germine et al., 2012). We also note that most participants had no difficulty identifying the targets' gender during the exposure phase (see Online Supplemental Materials), suggesting that the participants were motivated and attentive, and excluding the lowest performing participants did not change the results. Thus, although we cannot exclude that a lab-based version of these experiments would have resulted in different findings, failure to find a mere-exposure effect seems unlikely to be attributable to online testing, suggesting the possibility that exposing new faces online to older adults prior to a move to a new facility (our original goal) may not be effective for increasing liking.

A remaining explanation for the present findings may be that mere-exposure represents an instance of the decline effect (Schooler, 2014), such that effects reduce in magnitude or disappear since first report. This has occurred for the verbal overshadowing effect (e.g., Schooler, 2014), among others (see de Bruin & Della Sala, 2015). Indeed, declines in effect size have been reported in a range of scientific fields (including psychology, medicine, and biology), and the studies with mixed reports of a mere-exposure effect might also fit within such an explanation (Inoue et al., 2018; Mrkva & Van Boven, 2020). Often, effects seem to decline in magnitude because testing in broader populations reveals nuances not observed in the original work. Thus, as more and more diverse individuals contribute data, the effect slowly vanishes into the noise introduced by additional variables. Although this might explain why we did not observe a mere-exposure effect among older adults here, it does less to explain the findings for younger adults, for which other features of the decline effect may claim responsibility (e.g., popular knowledge about mere-exposure may have undermined its efficacy).

Thus, although people seem to respond to repetition in a variety of ways across tasks relating to perception, cognition, neuroscience, learning, practice, and interpersonal attraction, repetition may influence thought and behavior unevenly. Here, much to our surprise, we did not find evidence for the mere-exposure effect, despite persistent attempts, careful procedures, and with larger samples than have typically been used in past research. This disappointment notwithstanding, we hope that others see its absence as an opportunity to better understand the efficacy and incidence of the mere-exposure effect and, accordingly, develop a sharper understanding of how repetition and familiarity influence liking of other people's faces.

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