Fear and Anger Prime Effects on Cognitive Performance: The Role of Prime Visibility

Framorando & Gendolla

Reviewer comments history, Part 1:

<u>**Editor's comment:**</u>

After review, we have reached a decision regarding your submission to Swiss Psychology Open: the official journal of the Swiss Psychological Society, "Fear and Anger Prime Effects on Cognitive Performance: The Role of Prime Visibility". Our decision is to request revisions of the manuscript. These revisions will then undergo further peer review and it is not guaranteed that the manuscript will be finally accepted.

A summary of the requested edits from the editorial team can be found below. Please consider these points and revise the file accordingly:

Editorial Revision Requests:

1) As you will see, both reviewers think that your conclusions are not warranted. While the Reviewer 1 is more positive that the issues can be fixed, the Reviewer 2 is less convinced. My personal view is more in line with Reviewer 2, but I am will to accept a revised version if you can provide a convincing argument that your conclusions are warranted or if you are willing to include a follow-up experiment to further support your conclusions.

2) Please share the raw data (individual trial data) and not the aggregated data.

3) Please update the transparency statements according the journal requirements.

4) If you decide the submit a revision, please also include an author contribution statement. As long as the manuscript is not accepted and reviewed anonymously, you can replace the author names with letters (e.g., author A, author B, etc.)

Response: Although we found Reviewer 1's comments were positive and believe that our responses and revisions should result in a positive response from him/her, we are less optimistic about convincing Reviewer 2, whose interpretations differ greatly from ours. Nonetheless, we believe that our arguments are reasonable (all of Reviewer 2's criticisms are carefully addressed below). We have thus decided to give our manuscript a second chance.

We have also uploaded the raw data for response accuracy and reaction times, which can be found here (https://research-psychology.qvolapuk.com). The data for the

physiological measures (SBP and DBP) had already been presented in their raw format (one measure per minute).

In addition, we have added a transparency statement according to the journal requirements.

Finally, as the author contribution statement was already present in the previous submission, we only changed the authors initials with letters (Author A, Author B).

<u>**Reviewer 1:**</u>

Andrews, V., Lipp, O.V., Mallan, K.M., et al. (2011). No evidence for subliminal affective priming with emotional facial expression primes. *Motivation Emotion, 35*, 33–43. https://doi.org/10.1007/s11031-010-9196-3

1) Concerning the non-significant effect in the suboptimal condition, I see two solutions: please a) either temper the interpretation and conclusions of the interaction and consider the not significant effect in the suboptimal condition. This point concerns the title, highlights, abstract, the figure, and discussion of the manuscript. Or b), replicate the results in a larger sample. The new sample-size should be estimated in a power analysis based on the effect size of the suboptimal priming effect observed in the present sample.

Response: As suggested by Reviewer 1. We have toned down the summary, highlights, and especially the discussion. We did this by removing all interpretations that focused on the mean difference between the suboptimal fear and anger conditions as the cells contrast for suboptimal conditions was not significant.

2) Please shift the focus more to the behavioral measures. This point mainly concerns the first paragraph of the introduction, which in my opinion is still focusing too much on physiological results.

Response: We thank Reviewer 1 for this comment. We have revised the first part of the introduction and put more emphasis on nonphysiological behavioral measures. However, we discuss the psychophysiological findings because the present study was conducted in an effort context in which behavioral changes such as response accuracy or reaction times were considered to result from changes in effort. The predicted behavioral effects were grounded in the IAPE model (Gendolla, 2012), a theory of implicit affective influences on resource mobilization that essentially relates to psychophysiological measures. This means that highlighting the effort-related psychophysiological findings is important for understanding potential effects on behavioral performance measures.
References:
Gendolla, G. H. E. (2012). Implicit affect primes effort: A theory and research on cardiovascular response. *International Journal of Psychophysiology***, 86(2), 123-135.**

3) Minor Issues. Describe the minor shortcomings / issues of the presented research, content and structure of the manuscript. Please provide constructive feedback how the shortcomings / issues could be addressed. Enumerate your points with Arabic lowercase letters: a), b) , c) …

:
a)
Page 8: Please describe the prime-stimuli and potential creation of different stimulus lists in more detail. Please report also if / how you controlled for perceptual features (e.g., luminance, frequency).

Response: We thank Reviewer 1 for this comment. Such information was added to the method section. The faces used in the present experiment were highly standardized and stem from database (AKDEF) of facial expression pictures that are averages of 70 individual's expression of anger, fear, and neutral expression to create one unique stimulus for each expression (the stimuli used in the present experiment can be downloaded here: https://research-psychology.qvolapuk.com). We did not control for luminance and spatial frequency ourselves, since this was done by the authors of the AKDEF inventory and averaging 70 individual faces has produced sufficiently equivalent images in terms of perceptual features. Moreover, while we are aware that luminance and spatial frequency should be important for certain measures of effort, such as effort-related EEG components or pupillometry (e.g., Suzuki et al., 2019), this is not the case for cardiovascular activity measures. This was specifically tested in one of our previous studies in which we examined the effects of implicit affect primes alone using AKDEF pictures on cardiovascular activity outside a task context (Framorando & Gendolla, 2019). Participants were presented with masked happiness and sadness faces in an achievement-condition (they were asked to remember a series of letters) or a mere watching condition (they were asked to look at the screen without instructions to memorize). Expected effort-related effects on cardiovascular responses were only found in the achievement-context condition, while no prime effects were found in the watching-condition. This speaks against the possibility that the effects on effort or corresponding effort-related behavioral effects could be caused by artifacts related to the used stimuli.

References:
Lundqvist, D., & Litton, J. E. (1998). *The averaged Karolinska directed emotional faces—AKDEF*. Stockholm, Sweden: CD ROM from Department of Clinical Neuroscience, Psychology section, Karolinska Institutet.
Suzuki, Y., Minami, T., Laeng, B., & Nakauchi, S. (2019). Colorful glares: Effects of colors on brightness illusions measured with pupillometry. *Acta Psychologica*, *198*, 102882.

Figures and Tables. Please comment on the use of figures and tables in the manuscript - their relevance in terms of illustrating the arguments and supporting the evidential base, the quality of the formatting and presentation.:
Please start the y-axis at zero. The zoomed graph in the submitted manuscript supports the over-interpretation of the not significant difference between anger and fear primes in the suboptimal condition.
I also recommend showing individual datapoints in addition to the bars.

Response: We thank Reviewer 1 for pointing this out. Although we agree with Reviewer 1 that zoomed graphs might draw the reader's attention to non-significant mean differences, anchoring the y-axis at zero significantly affects the readability of the graph. The mean difference between the fear-prime condition ($M = 97.40\%$, $SE = 1.23$) and the anger-prime condition ($M = 92.97\%$, $SE = 1.20$) is 5%, which is small on a 100-

point scale. In addition, it is common practice to present behavioral results with zoomed scales to improve readability (see Bijleveld et al., 2010; Loersch & Payne, 2012). However, as suggested by Reviewer 1, we added a violin plot to show the distribution of response accuracy for each condition (see Figure 3).

References:
Bijleveld, E., Custers, R., & Aarts, H. (2010). Unconscious reward cues increase invested effort, but do not change speed–accuracy tradeoffs. *Cognition*, *115*(2), 330-335.
Loersch, C., & Payne, B. K. (2012). On mental contamination: The role of (mis) attribution in behavior priming. *Social Cognition*, *30*(2), 241.

Reviewer 2:

1) My main issue is with the task design: unfortunately, prime visibility is confounded with total exposure time and with the prime-target delay time. Based on the methods section, I assume the stimulus sequence was as follows:
[fixation cross: 1000 ms] -> [affect prime: 25 ms OR 775 ms] -> [picture mask: 133 ms] -> [fixation cross: 1000 ms] -> [target stimulus: 1500 ms]
With this design, it is unclear whether actual visibility (or awareness), or just the time available for processing the prime images (25 ms vs. 775 ms) or the mere time between prime onset and target onset (1158 ms vs. 1908 ms) accounted for the behavioral results. Both factors (duration of prime presentation, prime-target delay) are known to moderate the effect and direction of priming. The reaction times in this study indeed were lower for the low vs. high visibility condition (independently of the prime category) which suggests that the prime-target delay significantly modulated performance.

Response: We thank Reviewer 2 for this comment. The priming procedure we have used in the present study builds on the idea that affect primes should activate a global concept of ease/difficulty that feeds into the evaluation of task demand, thus influencing effort and corresponding effects on behavior - in this case, response accuracy. To test this, we ran the same 2 x 2 ANCOVA for the response accuracy during the task but only including the trials with the neutral primes (in the present study, neutral primes occurred in 24 of the 36 trials). Results revealed that the affect prime x visibility interaction remains significant, $F(1,78) = 7.67$, $p = .007$, $\eta2 = 0.09$. Higher accuracy was found for the fear primes condition ($M = 97.5\%$, $SE = 1.13$) than for the anger primes condition ($M = 94.6\%$, $SE = 1.13$) in the suboptimal condition, while the fear primes condition ($M = 93.5\%$, $SE = 1.13$) resulted in lower response accuracy than the anger primes condition ($M = 97.7\%$, $SE = 1.14$) in the optimal condition. These results support the idea that the reported accuracy effect is due to the activation of ease/difficulty concepts during task performance in our between-persons design.
This also speaks against the possibility that the prime-target interval played a role for the prime effects on behavior. Rather, the reverse effects on response accuracy in the optimal conditions are due to participants becoming aware to the presence of the primes, which in turn should induce feelings of being manipulated and trigger behavioral correction to restore autonomy and freedom. This interpretation is consistent with the results of another experiment in which prime awareness was manipulated by prime warning (Framorando & Gendolla, 2019). Instead of presenting primes longer, participants were verbally warned about the occurrence of primes in a suboptimal prime presentation condition, which was sufficient to moderate the effects of affect primes on effort. The entire timing of all trials was identical. This further

supports our interpretation that the present moderation effect is triggered by participants' awareness of the primes rather than by longer intervals between primes and targets.

Regarding the effects on the reaction times, we interpret the longer response times in the optimal condition as reflecting controlled processing triggered by the awareness of the priming procedure. This is based on the idea that fully processed primes should distract attention, because full processing can trigger a state of mind in which individuals consciously reflect on the current situation (e.g., "Why are primes presented that have nothing to do with the task?") - which can impede performance and result in longer reaction times (Gollwitzer, 1990). We have clarified this in the discussion.

References:
Framorando, D., & Gendolla, G. H. E. (2019). Prime warning moderates implicit affect primes' effect on effort-related cardiac response in men. *Biological Psychology*, *142*, 62-69.
Gollwitzer, P. M. (1990). Action phases and mind-sets. In Higgins, E. T., Sorrentino, R. M. (Eds.), *Handbook of motivation and cognition* (Vol. 2, pp. 53–92). New York, NY: Guilford.
Gendolla, G. H. E. (2012). Implicit affect primes effort: A theory and research on cardiovascular response. *International Journal of Psychophysiology*, *86*(2), 123-135.

What's more, the authors provide no objective assessment of prime visibility or prime awareness. An actual awareness test, where participants have to guess whether there was a prime or not or whether the prime image showed an emotional or a neutral face would be crucial here. Also, using the perceptual awareness scale (PAS) to assess participants' subjective prime awareness on a trial-by-trial base could have helped. The funnel debriefing revealed that at least 20% of participants were aware of the primes in the low visibility condition, and 10% of participants did not notice the primes in the high visibility condition. Hence, prime awareness was not robustly manipulated in all participants. I acknowledge that the authors carefully avoid using the terms "conscious" vs. "unconscious" when referring to their high vs. low visibility conditions. In fact, this crucial because because the presence/absence of awareness in not tested objectively. However, the introduction makes clear that a central part of this study was to avoid prime awareness in the low visibility condition in order to make sure that primes are processed implicitly.

Response: we thank Reviewer 2 for this observation. The link between uncontrolled/controlled processes and the type of priming procedure is complex. Although many studies have based their procedures on the idea that a subliminal priming procedure should ensure uncontrolled processes (Spruyt et al., 2011), others chose to mask the primes with a task that had nothing to do with the primes, e.g., a word completion task (DeMarree et al., 2012). This means that the uncontrolled/controlled processes triggered by the primes are not necessarily related to the visual perception of the primes per se, but rather to the question whether participants believe that the primes influence their thoughts and behavior. We chose to present minimal visual information in our "suboptimal priming" condition that is insufficient to trigger controlled processes but sufficient to activate mental implicit representations of emotional states. The presentation times used in the present experiment were primarily based on previous research on affect priming and effort (e.g., Silvestrini & Gendolla, 2011; Gendolla & Silvestrini, 2011; Chatelain et al., 2015;

Framorando & Gendolla, 2018a, 2018b). This research showed that presenting affect primes for 25 ms influences effort, as predicted by the IAPE model, but was not long enough to trigger controlled processes. In contrast, longer presentation times of primes (783 ms) led to controlled processes manifested by the contrast or zero effect of primes on effort (Framorando & Gendolla, 2018a, 2018b). Because the PAS can only test whether a face was visually perceptible or not, but not whether faces were presented briefly enough to prevent conscious processes, it would have provided no more information than the funnel debriefing we have used in the present experiment. In addition, although the use of a prime awareness scale like the PAS on a trial-by-trial base is an interesting idea, this would have triggered attention to the presence of the primes in the suboptimal condition, which is sufficient to induce conscious processes and behavior correction according to previous studies (e.g., Framorando & Gendolla, 2019; Verwijmeren et al., 2013)—which was clearly not in our intention in the suboptimal condition. In the present study, the funnel debriefing after the task was only used to ensure that the manipulations worked. As expected, results from the funnel debriefing revealed that far more participants in the optimal condition reported having seen an emotional face compared to participants in the suboptimal condition, where the emotional faces were reported by 20% of the participants. This indicates that the affect primes were not clearly visible when presented for 25 ms and backward masked, which is consistent with the use of the term "suboptimal" presentation.

Further, it is of note that the percentage of detected faces in the funnel debriefing only concerns the detection of affective faces but not neutral faces. Although we are aware that 10% of the participants did not report to have seen emotional faces in the optimal conditions, all reported to have seen a face. Seeing a face – even a neutral one - that has nothing to do with the task can be enough to trigger controlled processes.

Finally, as mentioned in our response to point 1, effort effects were also found in another experiment that used prime warning instead of prime visibility (Framorando & Gendolla, 2019) as main manipulation of primes awareness. Importantly, the affect primes were presented for 25 ms and backward masked in *all* conditions. The results (reversed effects of the primes on effort when participants were warned about the occurrence of the primes) demonstrated that 25 ms are enough to elicit the expected implicit processes related to the affect primes (the implicit activation of the ease/difficulty concepts). Further and as already mentioned in our response to point 1, this suggests that what matters is that participants become aware of the presence of the primes, which in turn should create controlled processes and behavioral correction.

Based on Reviewer 2's comment, we have added a footnote at the end of the introduction section to better explain this.

References:
Chatelain, M., & Gendolla, G. H. E. (2015). Implicit fear and effort-related cardiac response. *Biological Psychology*, *111*, 73-82.
DeMarree, K. G., Loersch, C., Briñol, P., Petty, R. E., Payne, B. K., & Rucker, D. D. (2012). From primed construct to motivated behavior: Validation processes in goal pursuit. *Personality and Social Psychology Bulletin, 38*(12), 1659-1670.
Framorando, D., & Gendolla, G. H. E. (2018a). The effect of negative implicit affect, prime visibility, and gender on effort-related cardiac response. *Adaptive Human Behavior and Physiology*, *4*(4), 354-363.

Framorando, D., & Gendolla, G. H. E. (2018b). Prime visibility moderates implicit anger and sadness effects on effort-related cardiac response. *Biological Psychology*, *135*, 204-210.

Framorando, D., & Gendolla, G. H. E. (2019). Prime warning moderates implicit affect. primes' effect on effort-related cardiac response in men. *Biological Psychology*, *142*, 62-69.

Gendolla, G. H. E., & Silvestrini, N. (2011). Smiles make it easier and so do frowns: masked affective stimuli influence mental effort. *Emotion*, *11*(2), 320.

Silvestrini, N., & Gendolla, G. H. E. (2011). Masked affective stimuli moderate task difficulty effects on effort-related cardiovascular response. *Psychophysiology*, *48*(8), 1157-1164.

Spruyt, A., Gast, A., & Moors, A. (2011). The sequential priming paradigm: A primer. In K. C. Klauer, A. Voss, & C. Stahl (Eds.), *Cognitive Methods in Social Psychology* (pp. 48 –77). New York, NY: Guilford Press.

Tourangeau, R. (1999). Remembering what happened: Memory errors and survey reports. In *The science of self-report* (pp. 41-60). Psychology Press.

Verwijmeren, T., Karremans, J. C., Bernritter, S. F., Stroebe, W., & Wigboldus, D. H. (2013). Warning: You are being primed! The effect of a warning on the impact of subliminal ads. *Journal of Experimental Social Psychology*, *49*(6), 1124-1129.

Theoretical background:

In the introduction section, the authors make the following statement "Based on the idea that people should prefer autonomy (Ryan & Deci, 2000) and believe to act in accordance with their own thoughts and decisions (Loersch & Payne, 2011), Gendolla (2015) suggested that people should dislike being manipulated and react to the perceived external influences with behavior correction (Brehm, 1966)." This is taken as argument why people should respond differently to conscious vs. unconscious primes. This line of argumentation builds on the premises that the preference for autonomy depends on consciousness, and that it is impossible to unconsciously detect attempts of manipulation or to protect one's interests. In my humble opinion, this premises need further elaboration.

Response: We thank Reviewer 2 also for this comment. We have revised this part of the introduction and better clarified our reasoning. The present study is not based on the assumption that it is not possible to detect unconscious manipulation attempts or that there are no unconscious self-protection mechanisms that could be triggered by primes. Rather, we aimed to test whether the self-protection mechanisms that have been identified in certain priming procedures also hold in effort contexts in which fear vs. anger primes are presented.

Our reasoning is based on two points:

(1) Self-determination theory, which posits that people are highly sensitive to fluctuations in autonomy (a state in which one's behavior is self-organized and volitional) and that people behave differently when they experience their behavior as voluntary than when they feel being controlled/manipulated. In the latter case they show behavioral correction to restore freedom (Brehm, 1966; Loersch & Payne, 2012; Ryan & Deci, 2000; Ryan et al., 2021; Verwijmeren et al., 2013).

(2) Recent findings have identified experimental manipulations – prime visibility and prime warning - that elicit such shifts in behavior: knowing that one is primed should create a feeling of being forced to process certain information and a feeling of being manipulated (e.g., Framorando & Gendolla, 2018a, 2018b, 2019; Loersch & Payne, 2012; Verwijmeren et al., 2013). Taken together, this leads to the idea that manipulating

the visibility of primes, which we did in the present study, should elicit the feelings of being controlled/manipulated and reduced autonomy, which in turn should trigger controlled processes that aiming at behavior correction to re-establish freedom (Brehm, 1966).

References:
Brehm, J. W. (1966). *A theory of psychological reactance*. Academic Press, NewYork.
Framorando, D., & Gendolla, G. H. E. (2018a). The effect of negative implicit affect, prime visibility, and gender on effort-related cardiac response. *Adaptive Human Behavior and Physiology*, *4*(4), 354-363.
Framorando, D., & Gendolla, G. H. E. (2018b). Prime visibility moderates implicit anger and sadness effects on effort-related cardiac response. *Biological Psychology*, *135*, 204-210.
Framorando, D., & Gendolla, G. H. E. (2019). Prime warning moderates implicit affect. primes' effect on effort-related cardiac response in men. *Biological Psychology*, *142*, 62-69.
Loersch, C., & Payne, B. K. (2012). On mental contamination: The role of (mis) attribution in behavior priming. *Social cognition*, *30*(2), 241.
Ryan, R. M., & Deci, E. L. (2000). Self-determination theory and the facilitation of intrinsic motivation, social development, and well-being. *American Psychologist, 55*(1), 68-78. doi:10.1037/0003-066X.55.1.68
Ryan, R. M., Deci, E. L., Vansteenkiste, M., & Soenens, B. (2021). Building a science of motivated persons: Self-determination theory's empirical approach to human experience and the regulation of behavior. *Motivation Science*, *7*(2), 97.
Verwijmeren, T., Karremans, J. C., Bernritter, S. F., Stroebe, W., & Wigboldus, D. H. (2013). Warning: You are being primed! The effect of a warning on the impact of subliminal ads. *Journal of Experimental Social Psychology*, *49*(6), 1124-1129.

Methods:
a) Psychophysical properties of the images used as affective primes are not reported: were fear and anger images equalized regarding luminance, contrast, and spatial frequency content?
b) Mask: Was only one specific image used as mask, or were there multiple mask images that were randomly used for each trial?
c) Task instruction: how was the experiment introduced? Were participants informed about the presence of primes? Why was prime duration always 25 ms for the practice trial (in the high and the low visibility condition)?

Response: We thank Reviewer 2 for pointing this out. We have further elaborated the method section and added the requested information. We have already responded to the prime-related questions above when addressing the points of Reviewer 1.

We used the same mask for all trials. The stimuli used in the present experiment can be downloaded here: **https://research-psychology.qvolapuk.com** and are now also presented in the new Figure 1.

Participants were told that they would first have an 8-minute rest period during which their baseline cardiovascular activity would be recorded, followed by the task instructions and the task, which would last 5 minutes. Participants were not informed about the occurrence of the primes because *prime warning* has been shown to increase

the awareness of the primes' presence, and to induce behavior correction (see point 2 above) (Framorando & Gendolla, 2019; Verwijmeren et al., 2013).

In the practice trials, the primes were only presented for 25 (as in the suboptimal condition of the task) to prevent participants from becoming aware of the primes' presence before task onset. We wanted to keep the procedure as similar as possible in all conditions, and reserved the manipulations for the main task. Everything else, the instructions, examples, practice trials and questions, were identical in all conditions.

References:
Framorando, D., & Gendolla, G. H. E. (2019). Prime warning moderates implicit affect. primes' effect on effort-related cardiac response in men. *Biological Psychology*, *142*, 62-69.
Suzuki, Y., Minami, T., Laeng, B., & Nakauchi, S. (2019). Colorful glares: Effects of colors on brightness illusions measured with pupillometry. *Acta Psychologica*, *198*, 102882.
Verwijmeren, T., Karremans, J. C., Bernritter, S. F., Stroebe, W., & Wigboldus, D. H. (2013). Warning: You are being primed! The effect of a warning on the impact of subliminal ads. *Journal of Experimental Social Psychology*, *49*(6), 1124-1129.

Results:
Reporting of statistical analyses is insufficient: Were all responses analyzed (which would be 36 at the maximum) or only responses following a prime (which would be 12 at the maximum)? What software was used for analysis? Were what type of ANVCOVAs (I/II/III) were performed?
I struggle with the authors' use of performance in the practice task as covariate in the analysis of performance in the main experiment (only 12 trials vs 36, different task: participants were provided feedback at practice, different stimulus sequences). The rationale for doing this is not provided. It is also unclear to me, what a significant interaction between prime visibility and prime affect means after controlling for performance in the practice task. The interpretation of this effect is not trivial. Finally, it is unclear whether there was a significant difference between groups already in the practice task, i.e. at "baseline", before emotional primes were introduced.
Response accuracy was at ceiling and thus probably not a reliable measure of performance. Response latencies were probably the more reliable measure, but they showed no effect of priming at all, which further dampens my confidence in the validity of the findings. Non-parametric tests that were performed in support of the parametric ANCOVAs do not account for a lack of sensitivity of a measure.

Response: we thank Reviewer 2 for pointing this out. We have modified the results section to include the following missing information:

   a) **Regarding the effects on response accuracy and reaction times, we analyzed all 36 trials and not only the trials that included the fear or anger primes. This was based on the idea that the affect primes should have had an effect on the experience of task demand (see point 1 above), which affected the response accuracy in all trials in our between-persons conditions**
   b) **We analyzed the data with SPSS 27 software, and we performed ANCOVAS with type III sum of squares because it is valid for significant interactions (type II is more powerful in case of non-significant interactions and type I is sensitive**

to the order of the variables entered into the model, which affects the results, and which is not what we wanted). This has been clarified in the results section.

c) We had designed the study for cardiovascular effort measurements, which required a between-person design as in our other effort studies. Therefore, as we had briefly stated in the results section, we controlled for individual differences in reaction time and response accuracy in terms of ANCOVAs rather than within-person ANOVAs (which were not possible in our design)—as we did in the study by Framorando and Gendolla (2018b) that also found a prime x visibility effect on response accuracy. Without such a correction of individual differences, performance effects of external factors are difficult to find, which is the reason why response speed and accuracy is usually investigated with within-person designs in cognitive psychology. Moreover, it is of note that the effects on accuracy (interaction effect) and reaction times (main effect) remain significant without including practice trials as a covariate. We conclude that our reported effects are robust.

d) We interpret significant effects on response accuracy in the absence of effects on reaction times as the result of the main strategy chosen by participants, who focused on accuracy rather than speed. It is of note that task instructions emphasized response correctness. Performance effects are strongly dependent on the strategies that are used to perform a task (Glickman et al., 2005). For example, participants may use a strategy that "sacrifices" accuracy to increase their reaction speed, which should result in performance effects on reaction times. In contrast, they can also do the opposite - "sacrifice response times", in which case the effects should be discovered on the basis of response accuracy. In the present experiment, the task was to *respond correctly* as fast as possible which apparently leads to a strategy that focuses on response accuracy.

e) We recognise that high response accuracy may influence the effects of manipulations, which is described as ceiling effects in Reviewer 2's comment. However, ceiling effects become are manifest in the fact that the level of variance of an independent variable is no longer measurable because the task becomes too easy. Following this logic, ceiling effects should mask the effects of the manipulation, rather than the opposite (increasing the likelihood that effects will be found by chance). From this perspective, our results suggest that response accuracy was still sensitive enough to detect the expected effort-related performance effects, even if it was high.

References:
Framorando, D., & Gendolla, G. H. E. (2018b). Prime visibility moderates implicit anger and sadness effects on effort-related cardiac response. *Biological Psychology*, *135*, 204-210.
Glickman, M. E., Gray, J. R., & Morales, C. J. (2005). Combining speed and accuracy to assess error-free cognitive processes. *Psychometrika, 70*(3), 405-425. doi:10.1007/s11336- 002-0999-3

Minor Issues. Describe the minor shortcomings / issues of the presented research, content and structure of the manuscript. Please provide constructive feedback how the shortcomings / issues could be addressed. Enumerate your points with Arabic lowercase letters: a), b) , c) …
:
Chapter "Methods" > "Participants and Design" => the text provided for the footnote 1 does

not really make sense in this context.

Response: We thank Reviewer 2 for this comment. Footnote 1 appeared twice. This has been corrected. Now, Footnote 1 refers to the Task Performance section in the results section.

Figures and Tables. Please comment on the use of figures and tables in the manuscript - their relevance in terms of illustrating the arguments and supporting the evidential base, the quality of the formatting and presentation.:
Figures and tables are fine.

A figure illustrating the image sequence for one d2 trial with a prime present and one trial without prime would be nice.

Response: Based on Reviewer 2's comment, we have added the new Figure 1 illustrating the sequence of an experimental trial. The primes were presented in all trials. However, in some trials the primes were faces expressing sadness or anger, while in the remaining trials neutral faces were shown.

Furthermore, figures illustrating the distribution of accuracy scores and response latencies (e.g. violin plots) would help gain trust in the results.

Response: We thank Reviewer 2 also for this comment. We have created and added the new violin plots in Figure 3 (depicting response accuracy during task performance in the four conditions).

Reviewer comments history, Part 2:

Editors' comment:

Both reviewers feel that the main claim of the manuscript is still not fully supported by the data. I am supportive of Reviewer #2 that the issue can be solved with additional analyses and a limitation section in the discussion. Moreover, both reviewers suggest that you should add individual datapoints to your plots, which I strongly encourage.
Please also add the following information to be consistent with the transparency guidelines of Swiss Psychology Open:

- Please indicate your stopping criterion more clearly. It does not matter whether you tested until you reached a given number or until the semester etc. However, it is important to transparently report which criterion you actually applied.

- Please include a statement on pre-registration. That is, if and which parts of the study were pre-registered. It does not matter if this was not the case, but please state it transparently.

- Please include a statement on whether or not your exclusion criteria where determined a priori.

Reviewer #1 indicates that the provided raw data cannot be merged on the basis of a subject ID. Please provide the required information in the datasets that they can be merged.

Response: Reviewers still have expressed concerns about the robustness of the results due to data distribution. As suggested by Reviewer 2, we performed a nonparametric version of the ANCOVA for response accuracy using permutations (see Reviewer 2's responses). Importantly, this additional analysis confirms the results of the parametric ANCOVA we had reported before and strengthens the confidence in the findings of our present study. We also included a short paragraph in the Discussion section considering the data distribution as a potential limitation of the present study.

Moreover, all other minor points have been carefully addressed as well. These include additional information about the sample size determination (dropout criterion), participant exclusion criteria, information about preregistration, and the addition of individual data points to the violin plots.

We have also added the participants' ID in the main data file (Data_Experiment_D1_Final.xlsx), which can be found here (https://research-psychology.qvolapuk.com). This should allow investigators to replicate the analyses performed on response accuracy and reaction times.

Below, we also provide a point-to-point response to the comments of Reviewers 1 and 2.

Editorial Revision Requests:

Both reviewers feel that the main claim of the manuscript is still not fully supported by the data. I am supportive of Reviewer #2 that the issue can be solved with additional analyses and a limitation section in the discussion. Moreover, both reviewers suggest that you should add individual datapoints to your plots, which I strongly encourage.

Please also add the following information to be consistent with the transparency guidelines of Swiss Psychology Open:

-       Please indicate your stopping criterion more clearly. It does not matter whether you tested until you reached a given number or until the semester etc. However, it is important to transparently report which criterion you actually applied.

-       Please include a statement on pre-registration. That is, if and which parts of the study were pre-registered. It does not matter if this was not the case, but please state it transparently.

-       Please include a statement on whether or not your exclusion criteria where determined a priori.

Reviewer #1 indicates that the provided raw data cannot be merged on the basis of a subject ID. Please provide the required information in the datasets that they can be merged.

Response: We thank the Editor for this helpful and constructive advice. As outlined above and below, we have addressed all of these points in the present revision.

Reviewer 1

The authors responded to almost all my concerns. However, I am still unsure about the main finding, the interaction, on which all conclusions are based. Therefore, I tend not to recommend the manuscript for publication with the presented sample size.

Major Issues. Describe the major shortcomings / issues of the presented research, content and structure of the manuscript. Please provide constructive feedback how the shortcomings / issues could be addressed. Enumerate your points with Arabic numerals: 1), 2), 3) ...:

    1) I am still unsure about the main finding, the interaction, on which all conclusions are based. With the present sample size, it appears difficult to draw reliable conclusions. More precisely, I'm not completely convinced that the observed interaction is not merely based on artifacts in the data. The authors now provide violin plots, what I appreciate a lot (you might still think on providing individual data points, please). Looking at the new graphs, performance seems to be at least close to ceiling and performance in the "optimal fear" condition might be biased by performance of one or two outlier participants.

Response: The violin plots in this revised version show individual data points. To deal with the non-normal distribution of response accuracy and to ensure robustness of the main results, we performed an additional permutation ANCOVA for response accuracy. Cell mean differences between fear and anger primes under suboptimal and optimal prime presentation conditions were also additionally tested using permutation t-tests. Importantly, the results fully confirm our main findings in the parametric analyses. This argues against the idea that the present results may be affected by artefacts in the data and increases the reliability and conclusiveness of the present results. Nevertheless, we added a paragraph to the Discussion section mentioning the data distribution as a possible limitation of the present study.

2) Please state, from which condition the two participants were excluded due to accuracy below 60. If they were all in the condition "optimal fear" it would strengthen reliability of the main results (if they were in any other conditions, it would speak rather against drawing conclusions from these data). Moreover, I was wandering why you chose a threshold of 60 and not 67 (because you have a 1/3 proportion of target trials). You might also think about calculating d prime scores and leave in all participants?

Response: (a) The two excluded participants were in the "fear/optimal" and "anger/optimal" conditions. We have added this information to the Participants and Design section. (b) The exclusion criterion was set a priori. The threshold of 60% for response accuracy was chosen because we believe that a response accuracy of less than 60% generally tends towards chance-level accuracy, suggesting that participants either misunderstood the task instructions or responded "blindly". Moreover, both excluded values (56% for P= 48 | 50% for P = 69) were 3 SDs below the grand mean (a widely accepted exclusion criterion), while the value of 67% was not.

3) I was not able to retrace the analyses, as the raw data file lacks all condition variables and the other file lacks a variable to identify individual participants (to link the data with the raw

data). Please add these variables to the files.

Response: We thank Reviewer 1 for this comment. The raw data file has been modified to include the variable ID. Reviewer 1 should now be able to link the main data file to the raw data for the response accuracy.

Minor Issues. Describe the minor shortcomings / issues of the presented research, content and structure of the manuscript. Please provide constructive feedback how the shortcomings / issues could be addressed. Enumerate your points with Arabic lowercase letters: a), b) , c) … :

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Figures and Tables. Please comment on the use of figures and tables in the manuscript - their relevance in terms of illustrating the arguments and supporting the evidential base, the quality of the formatting and presentation.:

Thank you for the violin plots. Please consider to show individual data points.

Response: We have reprocessed the violin plots and added individual data points.

Reviewer 2

The question addressed in this study is interesting and relevant, but I feel that the main claim of the manuscript is still not fully supported by the data. I am confident, however, that the authors can address my remaining issues with additional analyses, a limitations paragraph in the discussion section, and a few changes to the manuscript.

Major Issues. Describe the major shortcomings / issues of the presented research, content and structure of the manuscript. Please provide constructive feedback how the shortcomings / issues could be addressed. Enumerate your points with Arabic numerals: 1), 2), 3) ...:

The authors have done a great job ad addressing most of my concerns in the manuscript and the the response letter.

These are my remaining concerns:

No significant effect in the low visibility condition.
In the introduction, the authors clearly lay out their hypothesis that "[…] suboptimally presented fear primes should lead to the mobilization of higher cognitive resources than suboptimally presented anger primes [...]", and that "[…] this effect should be corrected and either disappear or be reversed when the affect primes are clearly visible […]". The main focus was thus on the impact of implicitly processed affect stimuli. However, the study only provides somewhat robust evidence for the claim that fear vs. anger stimuli had different impact on cognitive performance in the high visibility condition. Whether there was any reliable implicit processing of fear vs. anger primes at all in the low visibility condition is unclear.
This limitation and the mismatch with the initial hypotheses needs to be elaborated more clearly in the discussion section. I acknowledge that the authors already toned down their interpretation of the findings.
Ideally, the authors could add Bayesian analyses and report how much evidence there is for a difference between fear and anger primes in the low visibility condition. This is, of course, optional.

Response: We thank Reviewer 2 for this comment. We consider the lack of significant differences in the suboptimal prime presentation condition (p = .08 with permutation test) as a call for replication with hopefully stronger additional evidence, rather than a result that contradicts our original hypothesis. The pattern of means is consistent with the expected pattern at PEP and the t-test for permutation tended towards significance. It is likely that this effect would have been more pronounced with a larger sample. We have discussed this issue at the end of the first paragraph in the Discussion section.

Ceiling effect for key outcome variable (response accuracy)
The authors only found a significant effect for response accuracy. But performance for this measures was at ceiling. Hence variability was minimal, and the performance measure was not normally distributed. As a consequence, even a few outliers / extreme values in one condition could have led to spurious but significant effects (i.e., false positives). Importantly, the violin plots in figure 3 clearly suggest that a few outliers in the optimal fear condition could account for the significant effect.

The authors argue that "[...] ceiling effects should mask the effects of the manipulation, rather than the opposite (increasing the likelihood that effects will be found by chance). From this perspective, our results suggest that response accuracy was still sensitive enough to detect the expected effort-related performance effects, even if it was high."

In my humble opinion, the first part of this argument is incorrect: low variability means that a few outliers/extreme values can lead to significant differences. The second part is tautological (the fact that there is a significant result does not exclude the possibility that it is a false positive).

In sum, the authors still do not provide convincing evidence for the validity of the findings to warrant publication. I could recommend publication of the manuscript if the authors :

   • The authors confirm the robustness of their findings with non-parametric tests, ideally permutation- or rsampling-based tests (e.g., the "perm.anova()" function in the r-package "RVAideMemoire").

   • The authors mention the ceiling effect for accuracy and admit that a few outliers / extreme values could have led to the spurious finding as limitation of the study in the discussion section.

   • The authors add dots for each accuracy value in the violin plots (Figure 3) so that it is clearly visible how the values were distributed.

Response: We thank Reviewer 2 for these comments.

(1) In the previous version of our manuscript, we dealt with the skewed distribution with a non-parametric ANCOVA based on rank-transformed response accuracy scores. Importantly, results corroborated the main results of the parametric ANCOVA. Following Reviewer's 2 comment, we have decided to replace the rank-transformed non-parametric ANCOVA with a permutation ANCOVA (aovperm test – permuco package in R, with 5000 permutations). We also tested cell mean differences with permutation t-tests (perm.t.test from RVAideMemoire package). Importantly, the prime x visibility interaction remained highly significant, $F = 8.62$, $p = .004$, $\eta 2 = .10$. Also, the mean difference between anger and fear primes in the optimal conditions remained significant ($t = 2.29$, $p = .02$, $\eta^2 = .06$). Results can be found in Footnote 4. These results confirm the robustness of the main findings on response accuracy.

(2) We discussed the role of data distribution as a potential limitation of the present study in a limitations paragraph (see above).

(3) **We have added individual data points to the violin graphs (see above).**

Same duration of primes (25 ms) in the practice trials for both visibility conditions

In the response letter, the authors explained the reasoning for using the same, short prime duration (25 ms) for the low- and the high-visibility condition. This information should be included in the methods section, ideally with references that support the authors' reasoning.

Response: We thank Reviewer 2 also for this comment. We now inform about the reasons for the suboptimal prime presentation times in the "Procedure" section.

Prime awareness

It would help tremendously if the authors could explicitly mention somewhere in the introduction that prime visibility was experimentally manipulated not to ensure vs. exclude conscious awareness of the primes (subliminal vs. supraliminal), but to alter the degree of implicit vs. explicit processing.

Response: We thank Reviewer 2 for this comment. We have updated the "The Present Experiment" section and added this information.

Rationale for a between-subject design

Could the authors also elaborate in the manuscript why a between subject design was necessary?

Response: We thank Reviewer 2 for this comment. This information had been added in Footnote 2 in the manuscript.

Minor Issues. Describe the minor shortcomings / issues of the presented research, content and structure of the manuscript. Please provide constructive feedback how the shortcomings / issues could be addressed. Enumerate your points with Arabic lowercase letters: a), b) , c) …
:
        Chapter Results > Task performance: paragraph #2: Why are these "preliminary" ANCOVAs?

Response: We have labelled these ANOVAs "preliminary" because they were conducted prior to the main analysis. The reason for this is that we wanted to test whether reaction times and response accuracy for the practice trials should be included as covariates in the main analysis to control for individual differences in general response speed and accuracy (which is automatically controlled in within-persons designs). That way, the results of these "preliminary" ANOVAs serve to inform about the subsequent analysis.

End of same paragraph: the statement that this was a between-person design was removed in the revised version of the manuscript. In my humble opinion, providing this information here again as a reminder was helpful.

Response: We thank Reviewer 2 for this comment. This information is still present in the revised manuscript but had been moved a few lines above ("Two preliminary 2 (prime) × 2 (visibility) ANCOVAs were conducted for both response accuracy and reaction times, using the response accuracy and reaction times of the practice trials, respectively, as covariates - this allowed to control for individual differences in response speed and accuracy in our between-persons design").

Figures and Tables. Please comment on the use of figures and tables in the manuscript - their relevance in terms of illustrating the arguments and supporting the evidential base, the quality of the formatting and presentation.:

Figures and tables are fine.

Figure 3: please add single points for each participant's accuracy value to the violin plot (jitter plot).

Response: Figure 3 has been modified and includes now data points in each violin plot.

Open Science Practice.

Does the manuscript include a statement how the sample size was determined? (Yes / No? What is missing?)
:

Sample size was determined based on the general "guideline to at collect at least 20 participants per condition". Based on the methods, the reader must assume that the authors did not perform an a priori power analysis.
It is unclear why the authors aimed for a sample size of 85 for a 2x2 between subject design.

Does the manuscript include the stopping criterion for sampling data? (Yes / No? What is missing?):

Yes. The stopping criterion was 85 participants. However, it is unclear how this number was determined for a 2x2 design.

Response: We stopped recruiting participants at N=85 to ensure at least 20 participants per condition. This is because we assumed a priori that up to 5 participants could have to be excluded because of technical issues with the physiological measures. We have revised the "Participants and Design" section and included this information.