KU LEUVEN

FACULTY OF PSYCHOLOGY AND EDUCATIONAL SCIENCES
CENTER FOR THE PSYCHOLOGY OF LEARNING AND EXPERIMENTAL PSYCHOPATHOLOGY
TIENSESTRAAT 102 BOX 3712
3000 LEUVEN
BELGIUM

February 12, 2025

Dear Editor,

We would like to thank you and the three reviewers for your valuable time reviewing our stage-1 registered report entitled “Impact of Acute Stress Exposure on Reactivity to Loss of Control Over Threat”. We greatly appreciate your comments, and we feel that they have allowed us to improve our manuscript considerably. In the following, we address them one by one. The reviewers’ comments are shown in grey, while our responses are prefaced by “Response” in bold and shown in black. All page numbers we mention below refer to the revised manuscript with marked changes.

Sincerely,

Michalina Dudziak

on behalf of all coauthors

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Review 1 by Laura Meine

The authors plan to investigate the effects of exposure to acute stress on perceived, biological, and physiological stress reactions in response to loss of control. The study is relevant, very well thought out, and the methods are described in great detail. I only have a few questions and points that could be addressed to further improve the planned investigation:

Participants:

- Is there a max. age for recruitment?

Response: No maximum age is specified for recruitment in this study. Following consultation with the Ethics Committee Research UZ/KU Leuven, it was determined that age alone does not impose safety restrictions. Instead, eligibility is based on the physical and mental health of prospective participants. Any healthy adult who does not meet the exclusion criteria is eligible for inclusion, regardless of age. However, to minimize the potential influence of age on the outcome variables and ensure a homogeneous sample, we will exclude the data of participants over the age of 45 from the analyses. This data analysis exclusion criterion has now been added to the manuscript (lines 686 to 687). Please note that our sample will primarily consist of first-year psychology students, and therefore, it is likely that only a few sign-ups will need to be excluded.

MAST task:

- In case participants do not make (m)any mistake(s), I assume they are interrupted with the instruction to “count faster”. This could be made a bit clearer.

Response: Further instructions for the mental arithmetic task, tailored to mathematically skilled participants, have now been added to the manuscript (lines 337 to 344). The experimenter will instruct participants who make few or no errors to count faster. In addition, the experimenter can further enhance the task's difficulty by asking participants to count in increments of different numbers, such as 13 or 33.

- Previous studies have shown that experimenter characteristics (sex, race) can influence experimental pain tolerance, so it would probably be good to either keep the sex of the experimenters the same for all participants or at least document it and potentially include it as a covariate in analyses.

Response: White students of different sexes will assist with data collection for the current study. It is, therefore, impossible to keep the biological sex or gender of the experimenter the same for all participants. Given the evidence that men and women might exhibit enhanced stress reactivity or differences in pain tolerance when presented with the opposite sex, we will document the biological sex of the testing experimenters and then create a variable indicating whether each

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participant was tested by an experimenter of the same or opposite gender ("matched" or "unmatched" dyad) and include it as a covariate in supplementary analyses.

US calibration procedure and stimulation:

- Please elaborate a bit more on the calibration
  - Why did you decide on the max. duration of 2.8 s? Will the durations vary during the task? How?

Response: The duration of the electrical stimulus was determined through consultation with experts in the fear conditioning field and pilot testing conducted in 2022. We considered several factors when selecting the stimulus duration. First, it had to be long enough for participants to try various strategies to find the correct button. For example, we wanted to allow participants to press multiple buttons during each trial to improve their chances of selecting the correct one, thus fostering a sense of control. Second, our initial experiment (manuscript currently under review) involved administering the electrical stimulus to the forearm of the dominant hand while participants performed a movement with the computer mouse to terminate the electrical stimulus by clicking the correct button on the screen. Receiving the stimulus while performing a movement substantially slowed reaction time. Stimuli were applied to the dominant hand because skin conductance responses were measured from the palm of the non-dominant hand to decrease movement artifacts. Through piloting, we found that a 2.8-second electrical stimulus combined with a 2.5-second button presentation was sufficient for participants to try different strategies and be able to terminate the stimulus.

The electrical stimulus duration will vary in the loss-of-control task, depending on the phase of the task. In the first phase (trials 1-12, including 6 CS+ trials), the stimulus duration will depend on how quickly participants press the correct button during CS+ trials. The termination of the stimulus will be possible from 300 ms after onset. In the second part of the task (trials 13-24), the duration of the stimulus will not depend on the participants' actions but on the predetermined durations of the stimuli, obtained from the responses of a participant from the previous study. The stimulus duration will never be shorter than 300 ms or longer than 2.8 seconds.

  - What are the intervals for the gradual increase? Are they always the same?

Response: The stimulus intensity will be increased in 1 mA increments, in accordance with standard practice in our lab.

  - If participants know the intensity is gradually increasing, might they not be tempted to report a higher intensity than they actually perceived to ensure less uncomfortable stimulation during the task? I appreciate that you plan to ask them whether they would like to try one level higher, but it might be helpful to jump up and down a bit during the calibration so you

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catch participants who deliberately misreport their perception. It may also be helpful to just emphasise at the start that perception of the stimulus varies a lot between individuals and that it is not a competition about who picks the highest intensity.

Response: We try to ensure that participants choose the highest stimulus level that is "clearly uncomfortable, but not painful" by providing detailed instructions before the calibration procedure. The exact text of these instructions is: "*Because not everyone has the same sensitivity level, you must choose the level of electrical stimulation that is unpleasant specifically for you. For our research, it's important that the electrical stimulus is clearly uncomfortable but not painful. It means that you should put some effort into enduring the stimulus, and the stimulus shouldn't be too easy to deal with. I will start by giving you light stimulation and gradually increase it in small steps. I will tell you whenever I am about to start a new stimulus, and after each stimulus, I will ask you to assess it on a scale from 0 to 10, where 0 means "I feel nothing" and 10 means "This is the maximum level of stimulus I can tolerate". I would like you to select the highest possible stimulus that is clearly uncomfortable for you yet not painful. If you feel that a stimulus is at that level, please let me know. However, I will then ask you to try one stimulus higher. If that stimulus exceeds your highest level of "clearly uncomfortable, but not painful", we can always return to the previous, lower stimulus.*"

These instructions are intended to minimize the number of participants selecting a stimulus level below the "clearly uncomfortable but not painful" threshold. However, we cannot entirely rule out that some participants may choose a relatively low stimulation level or provide inaccurate assessments. In addition to these practices, experimenters avoid directly asking participants if the stimulus is painful, instead allowing participants to evaluate it on their own.

We would like to avoid altering the objective stimulus level during calibration without the participants' knowledge, as such an approach could be perceived as deceptive and stress-provoking. Our goal is to make sure that the calibration process remains relatively stress-free and clearly controllable.

- Will the electrode be attached to participants' non-dominant forearm? I would recommend that so there's less risk of electrodes detaching during task-related movement of the dominant hand.

Response: We agree there is a lower risk of detaching electrodes if they are applied on the non-dominant forearm. However, in this experiment, we plan to use an electrical stimulation band that can be put on the wrist or the forearm like a bracelet. The band contains built-in knobs delivering electrical stimuli to the skin throughout the task. By consistently making sure that the band was securely attached, we have never encountered any instances of the band detaching from the skin in previous experiments.

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We decided to apply the electrical stimulation band to the dominant forearm because participants in the stress group will be first asked to immerse their non-dominant hand (including the wrist) in ice-cold water before performing the computer task with electrical stimulation. Separating the arms (non-dominant vs dominant) for these procedures may help avoid any influence of the temperature change in the arm exposed to ice-cold water on the sensory perception of the electrical stimuli. Furthermore, during piloting, we observed that hand immersion in ice-cold water temporarily caused stiffness and reduced fine motor control in the affected hand. If the setup was reversed - requiring participants to submerge their dominant hand in ice-cold water and then applying electrical stimulation to their non-dominant hand - participants might later struggle to use the computer mouse with their dominant hand, which in consequence might affect their reaction times in the task.

Self-reports/Procedure:

- Please explain how you chose the time points for collection of control expectancy reports. If asked just before the US calibration, do participants refer to control over that or over the MAST? Why not also ask at t40?

Response: The time points for collecting control expectancy ratings are designed to capture participants' predictions about control in the future upon completing different parts of the experiment (e.g., questionnaires, MAST, loss-of-control task, etc.). We aim to determine whether control expectancy varies depending on the preceding task.

Participants are not informed about the order of tasks at the start of the experiment. The control expectancy question is specifically phrased as: *"To what extent do you expect to have control in the **next** part of the task?"* with responses assessed on a scale from 0 ("no control") to 100 ("full control"). When this question is asked, participants are not told what the next task will be, meaning their responses primarily reflect how the previous task influences their expectations of future control.

We will ask the baseline control expectancy question at t40 instead of t30 as suggested, which is right before the first experimental manipulation (MAST) takes place. This has been now changed in the manuscript (line 459).

Yoking:

- I would suggest selecting participants from the previous study who did not consistently show very high accuracy rates and short RTs because then the stimulation would still terminate almost immediately in most trials and participants in the current study might not actually perceive any loss of control.

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Response: We randomly selected data from four participants (one female and one male per counterbalancing condition) from our previous study for yoking purposes. The duration of their electrical stimuli varied among these participants, but the shortest stimulus lasted 530 milliseconds. Identifying participants with low accuracy rates or long reaction times is challenging because we only use data from the second part of the task (last three blocks of four trials). By this stage, participants in the continuous control group had already learned which button terminates the shock. Those who failed to learn the correct button within the first three blocks of the task were excluded from the previous study.

As a reminder about the unavailability of the buttons, a visual cue will be introduced by displaying the buttons crossed out with a red "X." Additionally, the progress bar will be removed from the computer screen to further distinguish between the control and loss-of-control phases.

Data processing:

- Please specify the temperature of the low-temperature freezer

Response: The saliva samples will be stored at -21 °C.

- Will participants with missing blood pressure etc. still be included in the other analyses to avoid losing too much data?

Response: We do not expect much missing data for stress reactivity variables such as blood pressure, as the experimenter will manually measure it using an automated OMRON machine. However, missing data may occur for salivary cortisol and salivary alpha-amylase (sAA) due to undetectable hormone/enzyme activity in the sample or an inadequate amount of saliva in the sample. If cortisol or sAA data are missing for the time points crucial for evaluating exclusion criteria or addressing the main hypotheses (t30, t85, t95 for cortisol; and t30, t60, t75 for sAA), we will exclude the entire participant's data rather than removing them from specific analyses. This specified exclusion criterion has now been added to the manuscript (lines 687-689). The excluded participants will be replaced until we reach a total sample of 128 participants. If missing data occur at other time points (e.g., t50), the participant's data will be only excluded from the analyses involving the missing data point.

Analysis:

- H1d - it might also be interesting to look at changes in PSS

Response: To examine key stress outcomes using a uniform analytical method, we decided to analyze H1d with repeated-measures ANOVA, with group (stress vs. no-stress) and sex-at-birth (female vs. male) as between-subjects factors and time points (t60, t75) as the within-subjects factor (lines 455; 596 to 599).

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- To conclude evidence of absence from null results, it would be better to employ a method such as Bayesian hypothesis testing or frequentist equivalence testing

Response: To enhance the robustness of potential conclusions, Bayesian analyses will be added to complement the frequentist analyses for hypotheses H1-5 (lines 643 to 650).

Other:

- Maybe explain in half a sentence why you decided to include a filler task

Response: We decided to include the filler task to make sure that the time between three measurements is spent in a similar way between participants. Specifically, salivary cortisol measurements need to be collected 10 and 20 minutes after the loss-of-control task to reliably assess the cortisol response after the loss-of-control task. During this interval, we want to keep all participants engaged with the same task to minimize mind-wandering.

- "BS" is a bit of an unfortunate abbreviation, I think :)

Response: We agree. This abbreviation is now deleted from the manuscript.

- Typo in line 465: I think it should be "their BELIEF of the deception"

Response: The sentence was now changed to "Prior to the debriefing, participants will be asked a few open- and closed-ended questions to assess their experiences of the deceptive tasks and how believable they found the deception used in the study" (lines 496 to 498).

- If you have time, it might be interesting to also collect baseline data on participants perceived self-efficacy/locus of control which should be related to control expectancy and could provide interesting results in terms of inter-individual differences

Response: Thank you for this insightful suggestion. Previous literature suggests that self-efficacy might influence our expectations for managing stressful situations (De Raedt & Hooley, 2016). Therefore, exploring the association between self-efficacy and control predictions in the experiment seems relevant. We have added the General Self-Efficacy Scale (GSE; Schwarzer & Jerusalem, 1995) to assess global perceived self-efficacy to the proposed study (lines 486 to 493). We will investigate the association between self-efficacy and control expectancy in exploratory analyses (lines 659 to 661).

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Review 2 by Genisius Hartanto

Dudziak and colleagues proposed a well-thought-out study plan to investigate stress and its role in low-control contexts involving an aversive stimulus. They provided an extensive yet clear motivation to address questions related to instrumental conditioning, loss of control, and stress, thereby bridging classical associative learning with clinical perspectives. The hypotheses are solid and supported by a relatively broad range of literature. The authors' analysis strategies are excellent. I also appreciate that the authors openly included the exclusion criteria and addressed the possibility of the data being abnormally distributed by providing non-parametric tests as alternatives to the proposed ones.

I have a few comments and suggestions:

1. **Loss-of-Control Task:** The newly developed task has really interesting components and has been described extensively. It is mentioned in lines 371–372 that presenting crossed-out buttons would ameliorate the perceived loss of control. I'm not entirely sure if that would achieve the intended effect. I would think that seeing crossed-out buttons might actually reinforce the idea that the shocks are unstoppable, thereby decreasing motivation to try (they might still try anyway, given the yoked duration). Consider this: wouldn't the perceived loss of control be more pronounced if participants saw a normal button (not crossed out), tried to press it, but failed to stop the shock? Wouldn't that reset their expectations, making them re-learn their perception of control? Looking back at original learned helplessness studies (e.g., Hiroto & Seligman, 1975; Burger and Arkin, 1980), the tools were not labeled "unsolvable" or "unstoppable," most likely for these reasons.

Response: Thank you for this insightful comment. We realize now that we should have provided a clearer explanation of our rationale for implementing the crossed-out buttons in the current study. In our previous study, we observed that some participants in the loss of control group experienced an illusion of control on loss-of-control trials. During shock delivery, participants from the loss of control group saw a blank screen, but their computer mouse remained active. As a result, some participants continued pressing the area where the correct button had previously appeared, and in some cases, their presses coincided with the predetermined (yoked) stimulus termination. This behavior aligns with Skinner's concept of superstitious behavior, where a particular outcome coincides with a specific action, leading to the formation of false beliefs about personal influence. In the case of our study, this unintended alignment may have reinforced a false sense of control. As previous research has shown, the perception of control can have a stronger impact on cognition and behavior than actual control (Zvolensky et al., 2000). To prevent the induction of an illusion of control, we decided to display crossed-out buttons in the corner of the screen as a clear, explicit indication that participants could not terminate the stimuli themselves.

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We recognize the potential value of allowing participants to press the buttons and learn through direct experience that they no longer influence the stimulus duration. However, our main concern when designing the task was that, due to the yoked stimulus duration (with the shortest shock lasting 500ms), participants in the loss of control group might still believe they had control if the buttons were present but non-functional. To mitigate this risk, we opted for a more explicit approach that distinctly signals the transition between having and losing control.

The task version we plan to use was previously implemented in our study examining how loss of control over threat influences stress reactivity in humans (in preparation; for details, please see preregistration https://osf.io/b72yk). In the proposed study, we aim to explore the reverse relationship. For this reason, we believe it is most appropriate to use the same task. Additionally, maintaining the same task design ensures accurate yoking between studies.

2. **Duration and Yoking Procedure:** It is great to see the yoking procedure being implemented in this task, even though it may be computationally complicated. The authors might want to consider reporting the mean duration participants took to stop the shock during controllable trials (blocks 1–3) and comparing the latencies between the stress and non-stress groups. If there were differences, it could be an interesting effect to report.

Response: That is an interesting suggestion indeed. Since the PsychoPy program records the time participants take to stop the electrical stimulus, we can easily extract these data and we will include them in exploratory analyses.

3. **Blocking:** Four trials per block were intended in the experiment. I wonder whether asking participants to rate how stressed and fearful they feel after every block might distract them from the learning process.

Response: Assessing fear or distress intermittently (i.e., after blocks of trials) is a common practice in fear learning research (Lonsdorf et al., 2017). Studies have generally shown that collecting fear ratings intermittently does not significantly affect CS discrimination or CS-US contingency learning (Blechert et al., 2008; Sjouwerman et al., 2016). Moreover, our previous experiments (N = 188) indicate that learning rates were unaffected. Specifically, all participants successfully acquired CS-US contingencies during the fear acquisition phase.

4. **Physiological Responses:** The authors outlined plans to analyze blood pressure and heart rate at specific times. I am also wondering about the possibility of including continuous physiological recordings during the experiment, as they could complement the ratings of stress and loss of control. Parameters like SCR or any ECG-related measures (e.g., HRV or heart period) could also be interesting in this context.

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Response: Including continuous ECG-related measures would be a valuable addition and could potentially enhance our study. We have carefully considered this suggestion. However, given the range of physiological measures we are already planning to collect, we have decided not to include ECG at this stage. Implementing these measures would complicate the execution of an already extensive study, and we prefer to focus on the planned measures to ensure feasibility. Nonetheless, we sincerely appreciate this suggestion and will consider it for future research.

Lastly, I appreciate the authors' interest in investigating the relationship between the aforementioned variables and childhood adversity. This could have significant clinical implications. Good luck with the data collection!

Signed,
Genisius Hartanto

Review 3 by Mariela Mihaylova

The current paper is an interesting look into how acute stress impacts later loss of control with clear implications for anxiety disorders and real-world settings. Despite the extensive research on stress, the current study explores a novel and niche area within the broader literature. The paper is clear, well written, well-structured and follows a logical line of reasoning. The introduction is particularly strong, with a good blend of theoretical and empirical evidence. I also commend the authors for listing out their exclusion criteria so well and for increasing transparency by pre-registering their study.

A few points of improvement and recommendations from me listed per section of the paper.

Introduction:

- Although this is well-written and well-researched, I found the connection between sex differences more and loss of control less clear. The research presented is more about responses to fear and threats rather than how (and if) loss of control is handled differently depending on sex. Are there any papers on this? It might be good to substantiate, as it will then better inform your hypothesis on this.

Response: To the best of our knowledge, no studies have explicitly assessed sex differences in the experience of loss of control over threat in healthy humans. However, in a previous study (manuscript in preparation), we observed that the experience of loss of control led to heightened perceived stress in females but not in males in a subsequent acute stress task. Given these results, it seems important to investigate potential sex differences in the experience of loss of control also in the current study. Furthermore, the literature has repeatedly demonstrated sex differences in responses to acute stress, with women usually reporting higher perceived distress, and men displaying greater increase in cortisol (Kelly et al., 2008; Taylor et al., 2013; Handa et al., 2022;

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Geva et al., 2022). Given that acute stress induction will precede the loss-of-control task, it is not unlikely that these differences might extend to the experience of losing control.

- I also thought that lines 65-100 could be made a bit more succinct by focusing on the broader trends more than the individual studies but I do see the relevance of the mentioned studies

Response: We agree with the reviewer that the mentioned paragraphs are rather extensive. The reason why we decided to list specific studies was two-fold. First, in the initial paragraph (lines 65 to 78), we aimed to highlight the complexity of investigating the relationship between threat (un)controllability and stress reactivity, given the inconsistent findings in the literature. Second, in the following paragraph (lines 79 to 103), we provided a detailed summary of research on the effects of stress on fear learning, as there is a lack of studies directly examining the impact of acute stress on loss of control in humans. Since our loss-of-control task is embedded within a fear conditioning paradigm, we found the connection to fear learning to be the most relevant framework for discussing the concept of loss of control over threat.

- I noted the connection to anxiety disorders, learned helplessness and stress sensitization. Can these be made more explicit to better explain the underlying mechanisms at play here?

Response: The connection between anxiety disorders, stress sensitization, and learned helplessness can be understood through the cognitive model of PTSD (Ehlers & Clark, 2000). When individuals experience a traumatic event, they may develop a persistent sense of threat, which sustains PTSD symptoms. Due to stress sensitization, their stress response becomes heightened and easily triggered, making them more reactive to even minor stressors in everyday life. This constant feeling of danger often arises from negative appraisals about the trauma, the self, and its consequences (e.g., *"I can never control what happens to me"*). These maladaptive beliefs foster feelings of helplessness, promoting passivity, which in turn reduces the motivation to engage in adaptive coping strategies. As a result, individuals often rely on maladaptive coping strategies such as avoidance, which prevents emotional processing of the trauma and reinforces negative beliefs**,** ultimately hindering recovery and maintaining PTSD symptoms.

Based on the outcomes of the present study, we plan to explore how stress-induced sensitization to the effects of loss of control may provide further insights into the mechanisms underlying anxiety disorders.

- The hypothesis are generally clear and grounded in research, but I found the connection to the main aim of the study (loss of control) less clear especially hypotheses 6-8. Can the

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authors make the connection to the loss of control here more direct rather than a by-product of stress?

- Also, I don't really see the relevance of Hypothesis 8 or how it's connected to loss of control or what the authors are expecting to find. Does childhood adversity increase sensitivity specifically to losing control, or to stressors in general? I don't really see why it would be investigated here – there are endless factors that contribute so I'm not sure any meaningful connections can be drawn even if you do see a relationship. I would instead suggest a less is more approach in studies - focus on the main hypotheses and interests rather than tying in other topics.

Response: We acknowledge that we should have been more explicit in stating that hypotheses 6 to 8 are secondary, and that the primary focus of this study is on the first five hypotheses. While secondary, the hypotheses related to general perceived stress and childhood adversity can potentially provide valuable insights into stress-induced sensitization to loss of control. Reviewer 2 highlighted this aspect as a strength of the study. For instance, while laboratory-induced stress may affect the experience of losing control, existing literature suggests that stress experienced in the past month may itself influence the response to acute stress and thus, in turn, potentially influence the relationship between stress exposure and loss of control over threat.

We also recognize the reviewer's concerns regarding the indirect link between childhood adversity and loss of control and acknowledge that this relationship may be influenced by various confounding factors. However, our study aims to take an initial step in exploring this association while recognizing that fully delineating its underlying mechanisms is beyond its current scope. Accordingly, we hypothesize that participants with higher childhood adversity, as measured with the Childhood Trauma Questionnaire - Short Form (CTQ-SF; Bernstein et al., 2003), will report higher perceived stress in response to the acute stress induction procedure (hypothesis 8a; H_{8a}), and the loss-of-control task (hypothesis 8b; H_{8b}) (lines 203 to 209; lines 639 to 642).

- I think the intro would also benefit from more of a connection to the wider real-world relevance of the current paper (what are the implications of the work you're doing?)

Response: We have tried to further clarify the clinical implications of studying stress-induced sensitization of the reactivity to loss of control in the introduction. A long-term implication not previously mentioned in the introduction is that if stress exacerbates the negative effects of losing control, individuals regularly exposed to stressors, e.g., in a workplace setting, may benefit from interventions designed to help them cope with loss of control in a more adaptive way.

We recognize the importance of the reviewer's comment and plan to further elaborate on the broader real-world implications of stress and loss of control in the discussion of the stage-2 manuscript, incorporating insights from the results of our study.

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Methods:

- Sample size. You state a between subjects design but power is calculated for a repeated measures ANOVA. It would be advisable to conduct power analysis based on the type of design you have. This will help give you more reliable findings. You can use the Superpower package in R, for example: https://cran.r-project.org/web/packages/Superpower/vignettes/intro_to_superpower.html#specifying-the-design-using-design

    - I would recommend this over using G*Power, which has been shown to not be very robust in all cases. For more info and for more accurate sample size estimates for designs, see Brysbaert, 2019.

Response: We agree with the reviewer's comment regarding the discrepancy between the stated between-subjects design and the power calculation based on a repeated-measures ANOVA. In response, and in order to be able to apply the same analyses for the key stress hypotheses, we have restructured all stress outcome analyses as repeated-measures ANOVAs, with group (stress vs. no-stress) and sex-at-birth (female vs. male) as between-subjects factors and timepoint as within-subjects factor (lines 593 to 599).

While we attempted to use the Superpower package as recommended, we encountered several challenges. The primary issue is that the effect size we planned to use for the power calculation comes from a study (Bhanji et al., 2016) with a different design (one-tailed t-test). Unfortunately, Superpower package requires means and standard deviations for each group, but the original study did not collect such data separately for men and women. Since we expect sex differences in our study, this lack of sex-specific data makes it difficult to apply the Superpower package accurately. We also explored alternative R packages for calculating effect sizes for mixed ANOVAs but they also required additional data we do not possess. Given these limitations, we decided to proceed with G*Power for the power analysis, fully acknowledging its limitations.

Based on the literature, including the Brysbaert (2019) paper recommended by the reviewer, we understand that effect sizes for between-within interaction designs tend to be smaller than for main effects. Therefore, we adjusted the effect size from Bhanji et al. (2016) to a more conservative estimate of $f = 0.15$ to reflect an expected small-to-medium effect size.

An a-priori sample size calculation, using power $(1 - \beta) = 0.80$, $\alpha = 0.05$, and $f = 0.15$ for a within-between interaction in a repeated-measures ANOVA, indicated that a sample size of 128 participants would be required (lines 230 to 242). In light of this calculation, we have increased our planned sample size. The sample size we settled on now (128 participants after replacements) is ambitious given the very labor-intensive nature of the data collection, but we will make it work.

- Page 13 - the authors note "a prior study" - can you provide a reference for said study?

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Response: The study referenced in the 'Counterbalancing and Yoking Procedure' section is our recently completed study that is currently being prepared for publication. However, full details - including the sample size, hypotheses, measures, study protocol, and data analysis plan - are available on the Open Science Framework (https://osf.io/b72yk).

- The authors mention women will need to note down the last day of their cycle and whether or not they are on the pill if they answer. Are the authors planning to do anything with this information? Knowing how stress impacts differently depending on the phase of the cycle (e.g., Montero-Lopez et al., 2018 but many other papers on this exist), it might be worthwhile for the authors to rethink how to better approach this.

Response: In designing the current study, we carefully considered how to account for the menstrual cycle phase in female participants. Based on our experience, testing female participants in a specific menstrual cycle phase (e.g., luteal phase) would not be feasible, as it would massively complicate data collection. However, we will account for the menstrual cycle phase at the time of testing. Specifically, we will test whether the distribution of female participants over menstrual cycle phases differs significantly between the stress and no-stress groups (lines 520 to 521). If significant differences between groups are detected, we will account for their influence in the analyses involving cortisol responses (lines 718 to 725).

We acknowledge the importance of the menstrual cycle phase and hormonal contraceptive use in stress research. However, findings in the literature remain inconsistent regarding their effects on acute stress responses. For example, Herbison et al. (2016) found that while females on oral contraceptives exhibited higher total plasma cortisol levels, their salivary cortisol responses to stress did not significantly differ from those of non-users. Similarly, their study did not find significant differences in plasma cortisol or salivary cortisol responses across menstrual cycle phases. Given these inconsistencies and the logistical challenges of strict cycle phase testing, our approach allows us to control for hormonal influences on stress while maintaining efficient data collection.

- The loss of control task is stated to be developed by the authors. Has this been pilot tested or validated? I'm not familiar with this literature, but do other loss of control tasks not exist? More information justification or validation for this is needed.

Response: To the best of our knowledge, our task is the first to successfully manipulate the loss of control over threat in humans within a fear conditioning procedure. By incorporating a yoking procedure, we aimed to bridge insights from animal research on threat (un)controllability to human studies.

KU LEUVEN

FACULTY OF PSYCHOLOGY AND EDUCATIONAL SCIENCES
CENTER FOR THE PSYCHOLOGY OF LEARNING AND EXPERIMENTAL PSYCHOPATHOLOGY
TIENSESTRAAT 102 BOX 3712
3000 LEUVEN
BELGIUM

The task in its current form has been tested on 188 participants across two studies - one reported in a manuscript currently under review and another one in a manuscript currently being prepared for submission. In both studies, participants in the loss-of-control condition consistently reported lower perceived controllability at the end of the manipulation phase compared to those in the continuous control condition. Additionally, the task reliably induced heightened fear and lower control expectations in the loss-of-control group compared to the continuous control group. We believe that these findings support the effectiveness of our approach in inducing loss of control over threat.

- Loss of control is assessed after the loss of control task (makes sense) but also after the stress task (why?). Are you expecting a correlation between control levels in the stress/no stress group and the loss of control task? If so, this should be a hypothesis with reasoning.

Response: We do not explicitly anticipate a correlation between perceived control in the acute stress induction task and the loss-of-control task. Our focus is on examining how biological, physiological, and perceived stress responses induced by the acute stress task (MAST) influence key outcomes in the loss-of-control task, such as stress levels, perceived fear, perceived uncontrollability, and control expectations. We included a measure of perceived controllability after the stress task to assess potential differences between the stress and no-stress groups. This serves as an additional manipulation check, given that, as highlighted in the meta-analysis by Dickerson & Kemeny (2004), acute stress tasks are most likely to elicit stress responses when they involve elements of uncontrollability and social-evaluative threat.

- For the loss of control task in general. How is this different from learned helplessness?

Response: Loss of control and learned helplessness are related but distinct concepts. The key difference is that learned helplessness develops when individuals are repeatedly exposed to uncontrollable negative events, which leads them to believe that their actions have no effect on the outcomes. This belief can then generalize to other situations, even when control is possible. On the other hand, loss of control occurs when individuals suddenly lose the ability to influence outcomes that they were previously able to exert control over. Some theories (e.g., Mineka & Kihlstrom, 1978) suggest that losing control after having experienced it can be more detrimental than a consistent lack of control, where no control is ever perceived.

Thank you for the opportunity to review this paper. I hope the suggestions will be well received by the authors. In case of questions, feel free to email me: mariela.mihaylova@etu.unige.ch

KU LEUVEN

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CENTER FOR THE PSYCHOLOGY OF LEARNING AND EXPERIMENTAL PSYCHOPATHOLOGY
TIENSESTRAAT 102 BOX 3712
3000 LEUVEN
BELGIUM

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KU LEUVEN

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CENTER FOR THE PSYCHOLOGY OF LEARNING AND EXPERIMENTAL PSYCHOPATHOLOGY
TIENSESTRAAT 102 BOX 3712
3000 LEUVEN
BELGIUM

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