nature portfolio

Peer Review File

How mood-related physiological states bias economic decisions

Corresponding Author: Dr Roeland Heerema

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

Dear Dr Heerema,

Thank you for your patience during the peer-review process. Your manuscript titled "How mood-related physiological states bias economic decisions" has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but raised some important concerns. The referees, especially Reviewer #2 raises concerns about the strength of evidence. These concerns arise from a number of issues: the sample size (of individual studies), difference in analysis/exclusion criteria between studies, potential disparity in the influence of separate studies on the results, doubts about the successful induction of emotions, in particular for anger and fear, and a lack of preregistration.

We are interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response to the reviewers. Please highlight all changes in the manuscript text file. In response, we ask you to bear the following editorial requests and guidance in mind:

1) Please explain the rationale for the determination of the sample sizes: how were the sample size of the individual studies determined, why did you decide to run the analysis on the sample from the studies merged together? When addressing power concerns risen by Reviewer#2 please avoid post-doc power analysis, but rather use sensitivity analysis to determine the smallest population effect that you can detect with your current experimental setting.

2) Please carefully address the concerns about the effectiveness of the induction procedure across moods. Reviewer#2 rises a key concern about whether the effectiveness of the mood induction of fear and anger was comparable to the one of happiness and sadness in terms of magnitude. Please also consider that anger (a high arousal affective state) would be expected to have a much higher impact on physiological measures reflecting sympathetic activation compared to sadness (a low arousal affective state). In particular, we would like to see a clear paragraph reporting the manipulation check of the induction of each emotion on the auto-reports and the physiological indicators in the result section of the main manuscript, with their respective plots and inferential statistical analysis. In the present version of the manuscript the physiological responses to fear and anger were excluded from the main analysis because they did not correlate with auto-reports. This criterion seems very stringent, and we would like to see more detailed explanation of this choice.
In the absence of strong evidence for a successful induction of fear and anger, we encourage you to perform a replication study targeting the anger and fear manipulation; the study would need to be powered a priori to detect the smallest theoretically informative effect size at 80% power. Alternatively the interpretation of their effect on choice behaviour is not sufficiently supported. In this case, inferences about the effects of induced fear or anger on choice behaviour would need to be removed from the manuscript.

3) Although we share Reviewer #2’s views that hypothesis driven confirmatory work should be preregistered, this is not a precondition for publication in Communications Psychology (except for randomized clinical trials). However, as explained in the checklist linked below, you must include a preregistration statement.

4) Please re-analyse the data using the same well-justified exclusion criteria for all studies (where specific exclusion criteria logically apply to all studies).

5) Please provide some more information about the weights of the individual studies. You provide descriptive information about this in your figures, but please also address this issue through additional analysis, for example by adding study as a fixed factor in your regression models.

6) Your manuscript requires better elaboration of the theoretical framework on the effects of affect on decision making. Reviewer#2 suggests several relevant papers on the literature between affect and decision making that would need to be integrated. Reviewer#1 also asks for the rationale for the moods that were selected to be used in the present set of studies: why there were selected about the different possible affective states and how are they expected to be reflected in the physiological measures. Reviewers also agree that the terminology between the different affective constructs such as emotions vs. moods needs to be better defined and kept consistent throughout the manuscript.

7) The reviewers also criticize a lack of clarity around your analytical approach. Please clarify and explain the terms of the models and the analytical approach, reviewer#1 and reviewer#3 have constructive suggestions on how to do that. In general, as you revise the interpretation, please consult the linked checklist and template on the journal policies about statistics reporting and interpretation. For example, p values need to be report exact or as p<0.001, not as approximating 0. Non-significant effects derived from NHST cannot be interpreted or taken as evidence for an absence of a difference, which is currently the case in multiple instances.

8) Please address in detail the concerns about the interpretation of the attentional mechanisms risen by reviewer#3

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
Link Redacted

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within 8 weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, provided it still presents a significant contribution to the literature at that stage.

We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Eva R. Pool

Eva R. Pool, PhD
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Communications Psychology
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EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist: Communications Psychology formatting checklist

and also in our style and formatting guide Communications Psychology formatting guide .

* TRANSPARENT PEER REVIEW: Communications Psychology uses a transparent peer review system. This means that we publish the editorial decision letters including Reviewers' comments to the authors and the author rebuttal letters online as a supplementary peer review file. However, on author request, confidential information and data can be removed from the published reviewer reports and rebuttal letters prior to publication. If your manuscript has been previously reviewed at another journal, those Reviewers' comments would not form part of the published peer review file.

* CODE AVAILABILITY: All Communications Psychology manuscripts must include a section titled "Code Availability" at the end of the methods section. In the event of publication, we require that the custom analysis code supporting your conclusions is made available in a publicly accessible repository; at publication, we ask you to choose a repository that provides a DOI for the code; the link to the repository and the DOI will need to be included in the Code Availability statement. Publication as Supplementary Information will not suffice. We ask you to prepare code at this stage, to avoid delays later on in the process.

* DATA AVAILABILITY:
All Communications Psychology manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

At a minimum the Data availability statement must explain how the data can be obtained and whether there are any restrictions on data sharing. Communications Psychology strongly endorses open sharing of data. If you do make your data openly available, please include in the statement:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publicly available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.
REVIEWER EXPERTISE:
Reviewer #1: mood and decision making, physiological measures
Reviewer #2: emotions and decision making
Reviewer #3: computational model decision making

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors compared the effect of induced sadness and happiness on unrelated economic choices between uncostly-but-small and large-but-costly rewards. Across four studies both the subjective ratings on the induced emotions and a proxy constructed from physiological measures predicted the choice irrespective of the cost type (risk, delay or effort). Moreover, gaze tracking revealed an early preference for the congruent option (i.e., costly option following happiness induction). Analyses are novel, impressive, exhaustive, detailed, robust, and well described. Overall, my impression of this manuscript is quite positive. Almost all of the questions about the theoretical background, the methodological approaches and the future implications that I had in mind when reading the manuscript were clarified within the relevant paragraphs or in the discussion.
The paper represents an advance in understanding how mood induction affect economic choices and the paper provide strong evidence for its conclusions.
Below, few minor suggestions and clarification that I feel might improve the manuscript.
1- How did the authors selected the four emotions from the six basic emotions? That is why they excluded Disgust and surprise? How does their finding relates to other line of research on music-induced emotions (e.g., Cowen et al., 2019 doi: 10.1177/1529100619850176; Cowen et al., 2020 doi: 10.1073/pnas.1910704117)
2- More in general the authors should consider the consistency with the use of “mood”, “feelings” and “emotions” especially when parametrising a modulator of a choice bias. See for example the paragraph “Temporal decay of the mood effect on choice” where they correctly note that “Emotions are often reported to be short-lived events”. Please consider wisely when

using “mood” or “mood fluctuations” or “mood rating” instead of “emotions”.
3- Pupil diameter codes not only for the arousal but also for the degree of surprise and volatility (Pulcu and Browning, 2017 doi: 10.7554/eLife.27879) that could be relevant in depression (Pulcu & Browning, 2019 doi:10.1016/j.tics.2019.07.007) and anxiety (Browning et al., 2015 doi: 10.1038/nn.3961). Out of curiosity, did you find any association between pupil diameter and the subjective measures at BDI or STAI? And between pupil diameter and the ‘curiosité’ ratings in Study 3 and 4?
4- The authors recruited n=94 subjects across four studies. Considering the experimental setting (i.e., eye-tracker, EMG, oximeter, skin conductance, the fitness bike etc) it’s a good number but, since the study was not preregistered, I was wondering whether they had an a-priori sample size calculation or a power analyses (e.g., Beekmans et al., 2023 doi: 10.3758/s13428-023-02165-7).
5- The study design (induction --> economic choice --> subjective ratings) seems more suited to evaluate the effect of economic choices on the subjective ratings rather than the other way round. Maybe it’s worth clarify why the authors did not invert the two (e.g., induction --> subjective ratings as a manipulation check --> economic choice) and why the subjective ratings are not a results of the economic choice (e.g., I feel happier after earning more money).
6- In all the four studies the duration of the experiment was nearly one hour and a half (calibration session + 60 trials with 10 sec washout + breaks between blocks. Did the authors notice an overall effect of time (such as fatigue or boredom) on choice, effort and emotional induction?
7- Were the subjective ratings normally distributed? That means did the participants exploited the whole possible range from 0 to 10? If not, any comment on why the mean ratings following positive and negative induction is 0.6?
8- I feel that the Supplementary Materials might be ordered and numbered so that when referencing to them in the main text, it’s easier to find the appropriate paragraph. For example when referring to the debriefing questionnaires supplementary in the main text (Results – subjective ratings), the reader find a discussion on the mood bias parameter that is not yet introduced in the results.
9- In the model-free analyses (Fig 3a) would be nice to know if there’s an effect of study and if so, why. For example the emotions effect on ‘delay’ trials seems driven mostly by the study denoted by the circle. Similarly the effect size even in the ‘risk’ and ‘effort’ for the studies denoted by the triangle and diamond seems pretty small.
10- As a related note, did you find any association between delay and impulsiveness at BIS-11? From a psychopathological perspective is interesting that risk is more associated with happiness and effort with sadness. Manic patients might have an elated mood and be more risky in their decision and depressed might be more abulic but not necessarily more considerate in risky choices. Incidentally, I think that in Figure 3 the legends are inverted for figure b and c.
11- RTs are z-scored within each participant, correct? Looks like the participants are slower following sadness induction when they choose the costly option. How does this relate to your gaze results, where after sadness induction they stare more the uncostly option?
12- In general, the authors did an amazing job in describing the methods, but I feel that it can be improved in term of clarity among terms. At a first read it’s hard to retain the definitions of decision function, decision value, discount(-ing) function, discounted value, and discount factor.
Probably these are explained in the unpublished paper (Heerema et al., under review).
Specifically, in the model-based results would be helpful to define more clearly what each parameter represents and how it affects the sigmoid curve of the logit (decision) function defined as 1/[1+e^-(β0+β1*x)]. Considering the broader readership of the journal, might be easier to see each step the authors implemented, starting from the simple decision function. For example, adding what is the DV term after Equation 1 as depicted in Equations 6, 8 and 10, the slow reader (such as me) can see how the x axis relates to the logit curve (Fig 4b). At this point the author could also better spell out what the discount functions represent and how they relate to the X and Y axis in Figure 4a. Doing so would be easier to understand that the βM*mood is another (negative?) term added to the same equation -(β0+β1*DV-βM*mood). If I got that correctly. Incidentally I’m not sure what’s the difference between the E and PE subscripts in Equations 10 and 11 and there’s probably a typo at line 305 when mentioning Table S2 instead of Table S4.
13- I’m not sure I understand how in study 2 the delay represent an opportunity cost. Can you please clarify? Also the term “intertemporal choices” appear in this paragraph for the first time.
14- Probably would be better to add a paragraph named “calibration session” before the description of the “economic choices” that depict the actual course of the experiment. Here, defining each individual indifference curves for each cost type is an elegant solution. Did the authors find any association between these and the other unstandardised study variables (e.g., subjective ratings, questionnaires or physiological measures)?
15- I believe that few methodological decisions might benefit of appropriate references such as “the zygomaticus and corrugator are commonly dichotomized as signalling positive and negative emotions”; “to reduce the dimensionality of physiological responses…this was done by taking the median of the signal over the entire 0-10s induction window”; “From the resulting corrected response estimates, we considered two dimensions for principled reasons”.
16- There’s a typo in the references: Eran, Eldar, instead of Eran, E.,

Reviewer #2 (Remarks to the Author):

The authors have engaged in highly creative work. In keeping with referee guidelines for Nature: Communications Psychology, I organize my review within the pre-specified questions offered by the journal.

• What are the major claims of the paper?

The authors hypothesize that what they call “mood-related emotions” (happiness and sadness) will bias a variety of unrelated economic decisions whereas what they call “other emotions” (anger and fear) will only evoke actions like fight or flight, which are related to their specific triggers. This is a surprising hypothesis because it seems to overlook the fact that other researchers have already found effects for "other emotions" on economic decisions. For example, here are examples that readily come to mind: Lee and Andrade found that fear influenced financial decision making. Lerner, Small, & Loewenstein found effects of disgust on choice prices and/or selling prices. Gambetti et al found that anger influences investment decisions. Ferrer et al. found that anger influenced risky choices with real monetary incentives. The list could go on.

• Will the paper be of interest to others in the field? Will the paper influence thinking in the field?

The paper would be of interest and be influential if it did the following:
1) Begin the introduction by discussion major theories of emotion and decision making and explaining how your hypotheses might build upon or contradict such theories. Presently, the paper does not address existing theories at all.

2) Conduct a more systematic literature review and discuss studies that run counter to your hypothesis, presenting a more even-handed review of the field.

3) Provide more theoretical background for the idea that some emotions should be considered “moods” rather than emotions. This conceptualization runs counter to the more typical idea that a feeling such as sadness could be an emotion when intense, target-focused, and short lived OR a mood when mild, diffuse, and protracted. Many journal articles and books in affective science have long viewed the relation between emotion and mood in this way (e.g., papers by Ekman, DeSteno, Keltner, Davidson, Clore, Scherer, Ellsworth, Gross, Levenson, etc). It is certainly possible to challenge that view, but one must provide a rationale for doing so.

4) Run studies with much larger sample sizes, improving generalizability and keeping up with norms in the field.

5) Balance gender representation within your studies or at least justify having such a strong skew toward one gender and limit conclusions accordingly.

6) Address the crucial confound in the independent variable: The inductions for anger were not nearly as successful (in terms of magnitude) as were the inductions for the other emotions. Thus, when the authors conclude that, contrary to happiness and sadness inductions, anger and fear inductions did not affect economic decisions, they cannot rule out the possibility that anger was simply weaker than the other emotions.

7) In keeping with scientific norms for the field, please pre-register your hypotheses and analytic plans so that it is clear which data need to be analyzed with post-hoc corrections and so that opportunistic findings are less likely. The present paper, which used different exclusion criteria from one study to the next, is not in keeping with current best practices.

8) Increase the replicability of your work by following guidelines in the Open Science Framework

9) Provide more information on the construct validity of the emotion inductions. Typically in affective science, reading a one sentence description of an event is not considered sufficiently engaging for triggering an emotion per se. Instead, the scenario might be triggering demand effects.

10) Provide far more detail on emotional carry-over effects given the within-subject nature of the manipulations.

11) Provide far more detail on and rational for the specific physiological measures used.

• Are the claims convincing? If not, what further evidence is needed?

Due to the issues described above as well as below (on the Supplement), the claims cannot yet be considered convincing.

• Have they provided sufficient methodological detail that the experiments could be reproduced?

It would be helpful to have far more methodological detail.

• Is the statistical analysis of the data sound?

It is difficult to determine the extent to which the analyses are sound because much detail is missing. For example, the section on links between choices and physiology presents a series of correlational results but it is unclear why they were not analyzed in a simultaneous equation. The rationale for various analyses is unclear throughout the results section.

• Are there any published articles that compromise scientific advance?a

Because the foundational aspects of this paper need improvement, the results are unlikely to be considered a scientific advance.

• Are there other experiments that would strengthen the paper further? How much would they improve it, and how difficult are they likely to be?
Please see discussion above and below for specific suggestions.

• Are the claims appropriately discussed in the context of previous literature?

As mentioned, the paper has not yet laid out a map of existing theories re: emotion and decision making, making it difficult to contextualize the claims.
• Are there any special ethical concerns arising from the use of animals or human subjects?

I did not see any mention of whether the studies obtained IRB approval before running subjects. Perhaps I missed that?

• If the manuscript is unacceptable in its present form, does the study seem sufficiently promising that the authors should be encouraged to consider a resubmission in the future?

The studies could be considered pilot studies but a major overall would be needed.

Supplementary Info

1100-1105 The Open Science Framework recommends that all researchers pre-register their hypotheses and analytic plans including plans for exclusion criteria in order to avoid opportunistic data analysis. Even if one has not pre-registered, it is generally considered good practice to use the same exclusion criteria across studies rather than rejecting candidates based on certain scale scores in one study and then including candidates with similar scale scores in a different study (as the researchers have done here). It is very important to re-analyze the data using consistent application of exclusion criteria across studies.

1105 The authors do not report analyses on personality traits because they found no statistical link with behaviors of interest. Presumably they are referring to outcome variables. But what about the hypothesized moderating effect of Openness from the BFI and of the VVIQ on susceptibility to the emotion inductions? Did the authors examine these moderators? Please report the results.

1108 Given current trends in the field, sample sizes (ranging from N = 18 to N = 36) are unusually low. Moreover, the gender composition of the sample was sometimes always skewed toward females, sometimes highly so. It is hard to consider these samples representative.

1109 Please add standard deviations to the table. Per convention, it is crucial to assess the variation within the measure, not just the confidence estimates around the mean.

1110 In order for this work to be considered replicable, all stimuli should be uploaded and made freely available. Another approach would have been to use stimuli already used within the literature. Why are some stimuli adapted from existing literature but not others?

1139 It seems that the music selected to evoke happiness had far stronger effects on self-reported happiness than the did music selected to evoke anger on self-reported anger. How did the authors account for this imbalance in stimulus strength?

1141 I'm unclear how the short sentences (e.g. ,"you buy a lotter ticket and you win 100 euros instantly") could evoke emotion. Typically emotion researchers use much longer passages.

1152 I'm unclear about how the procedure worked in Study 1. Did participants receive a within-subject (repeated exposure) to the same emotion induction three times? If so, it would be valuable to analyze the data accordingly, showing the impact of the repeated measure.

1160 I'm also unclear about the procedure from Study 2 onwards. What does it mean to alternate emotion categories? Also, why did you decide to "simplify" the design by "…only presenting choices in the gain frame potential loss being only included in the risky decisions, where the costly 1162 option was now defined as a probability to win the large reward versus a probability of losing a 1163 fixed amount (10€)." I'd like to see all of these decisions unpacked and justified based on results and revisions to hypotheses.

1178 Figure S1 implies that all participants received multiple different emotion inductions. Is that correct? How was carryover from one emotion to the next handled? Given that there were five different emotions, why were there only four different orders in S1 and only three different orders on S2? Why not fully randomize?

1178 Figure S1 is extremely hard to follow.

1197 Why was a pulse oximeter used? Which hypotheses were advanced?

1216 Why does Figure S3a report only recordings for anger and fear? What about the other emotions?

1239 "It was expected that the effects of our happiness and sadness inductions would not last long and return shortly to baseline." Why not expect this of the other emotions as well?

1250 The figures in panel a and b are under-reported. Please explain the Y axes and title the figure with the main point.

1259 The section on model comparison would benefit from significant tightening of the rationale and steps taken. Sentences like this one are difficult to understand: “Starting from the base discount functions described in the main text (with one parameter per function), we either did or did not add a weight on reward, and costs either had a fixed power parameter (1 for delay and risk, 2 for effort) or a free one to be estimated through model fitting.”

1276 It would be helpful to add discussion about the limits of retrospective introspection about experienced emotions in specific studies.

Reviewer #3 (Remarks to the Author):

In this study, the authors investigate the impact of mood on economic decision-making in humans. Across 4 experiments and 3 settings with different types of cost (risk, delay, effort), results suggest that economic choices, response times, and attention are biased by mood-related state. After inducing happiness, sadness, anger and fear, the authors show that happiness and sadness result in (opposite) changes in choices towards the costly option, but not anger nor fear. This bias was elegantly captured by a choice-based model which implements the mood-related state as an option bias in the selection (softmax) rule. The work has many positive features, including a very thorough design with many measurements, well-justified analyses and quantitative comparisons of several models. Overall, the paper is very well-written and the findings add a new perspective to the field. Therefore, I only have a few comments and suggestions for the authors, but in general I am very positive about their interesting work.

1) The paragraph about attention (lines 413-427) is a bit misleading and will benefit from more detailed analyses. The authors state that “a predisposition to selecting either choice option should result in attention being preferentially directed towards that option immediately following its presentation”, which would induce a gaze bias after stimulus onset. Yet, the results presented here only focus on gaze patterns following choice onset, which does not directly address the hypothesis and, in my opinion, do not justify the “signature of a prior intention” mentioned in the discussion. From context, and judging from the authors’ interpretation, it is possible that the reported results where following the stimulus onset, not the choice per se, as “choice onset” is an ambiguous formulation. If this is not the case, could the authors comment on the gaze patterns in the “early phase of decision making”, i.e., before the choice?

2) The authors find a mood-related bias in the choices, but also in the response times and gaze patterns. Yet, the model they chose to implement only captures the choice patterns and doesn’t seem to be built to account for RT and attention. These effects could however be captured by sequential sampling models, such as the (attentional-)DDM (Krajbich et al, 2010). I do not think that including such a model in the analyses would drastically improve the paper in its current form, but this should be at least mentioned in the discussion. Relatedly, the authors choose to implement the mood component as an additive feature in the selection rule, but do not discuss any other implementation, nor whether the addition of this parameter might interact with others (e.g., beta_0).

3) It is not clear why the authors chose to implement the weight on rewards as a multiplicative factor and not power parameter, such as in classical forms of Prospect Theory (in addition to their implementation of loss aversion and discount function curvature).

Minor points

4) line 283. The inclusion of the different parameters for the winning model could be briefly justified and explained in greater details for the non-expert reader (temperature and choice biases, weights, power parameters, discount factors).

5) line 370 or Figure 5b. The shape of the valence function seems a bit surprising. One could expect a linear/sigmoid, rather than step-like function. Could the authors comment on this, or explain why they were expecting such a shape?

6) Figure 2. I recommend to define H-N-S in the figure legend. Also, there might be a label mistake in the top right green panel (all inductions) for the x-axis.

7) Figure 4b. It should be specified in the legend whether the measures are absolute or compared to the neutral condition.

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scientific community achieve unambiguous attribution of all scholarly contributions. You can create and link your ORCID from the home page of the Manuscript Tracking System by clicking on ‘Modify my Springer Nature account’ and following the instructions in the link below. Please also inform all co-authors that they can add their ORCIDs to their accounts and that they must do so prior to acceptance.
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Version 1:

Decision Letter:

** Please ensure you delete the link to your author homepage in this e-mail if you wish to forward it to your coauthors **

Dear Dr Heerema,

Thank you for your patience during the peer-review process. Your manuscript titled "How mood-related physiological states bias economic decisions" has now been seen by 2 reviewers, and I include their comments at the end of this message. Reviewers are satisfied with the revised version of the manuscript. We are happy with the quality of the revisions and remain interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to some remaining editorial concerns.

We therefore invite you to revise your manuscript, please highlight all changes in the manuscript text file.

Editorially, we consider that the revised manuscript has been substantially improved and now provides a clearer and more focused message. In the new manuscript, you presented the logic of the studies as following an exploratory and confirmatory sample approach. This is a very solid approach, and could come across more clearly in the revised manuscript. In particular, we would like you to report systematically results the exploratory studies and confirmatory studies separately, so that the internal replication is clear. In the present version of the manuscript that is clear in most of the figures, but is not systematic. If some analysis needs to be performed on the samples merged across of the exploratory and confirmatory studies, then please report these analyses in a distinct section, which starts by explaining the rationale for pooling.
Please do not conduct a post-hoc power analysis based on the observed effect size in your study (cf. Lakens, 2022, https://doi.org/10.1525/collabra.33267). Instead, please provide a sensitivity analysis to demonstrate whether the study had at least 80% power to detect the smallest effect size of interest for the critical comparisons. Please note that a clear rationale for what constitutes the smallest effect size of interest must be provided.
Finally, in the result section do not refer to the p-value of 0.109 as a trend.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

I am attaching an Editorial Requests Table that details critical reporting requirements for the revised manuscript. Please attend to each item and ensure your manuscript is fully compliant. We are requesting that your manuscript aligns with these requirements as this facilitates the evaluation of your manuscript, reducing delays in re-review and potential future acceptance. If your revised manuscript is not aligned with these requests on major issues, such as those concerning statistics, it may be returned to you for further revisions without re-review. Additional information can be found in our style and formatting guide Communications Psychology formatting guide.

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We hope to receive your revised paper within 8 weeks; please let us know if you aren’t able to submit it within this time so that we can discuss how best to proceed. If we don’t hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, provided it still presents a significant contribution to the literature at that stage.

We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning.

We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Eva R. Pool

Eva R. Pool, PhD
Editorial Board Member
Communications Psychology
orcid.org/0000-0001-5929-1007

REVIEWER EXPERTISE:
Reviewer #1 mood and decision making, physiological measures
Reviewer #2 emotions and decision making
REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

The authors replied to all my comments. I don't have further questions.
The manuscript add an important brick to the literature of how mood affects decision making.
If the editor agrees, I believe that the manuscript can be published in Communications Psychology.

Reviewer #2 (Remarks to the Author):

The revised manuscript has clarified most of my original questions.

EDITORIAL POLICIES

We ask that you ensure your manuscript complies with our editorial policies and reporting requirements.

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- Nature Research Reporting Summary
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Version 2:

Decision Letter:

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Dear Dr Heerema,

Your manuscript titled "How mood-related physiological states bias economic decisions". I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Psychology.

We therefore invite you to revise your paper one last time to address a list of editorial requests. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work. Your manuscript is overall in excellent shape, and the remaining requests should be easy to address.

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Reviewer #1 (Remarks to the Author):

REVIEWER EXPERTISE: mood and decision making, physiological measures

The authors compared the effect of induced sadness and happiness on unrelated economic choices between uncostly-but-small and large-but-costly rewards. Across four studies both the subjective ratings on the induced emotions and a proxy constructed from physiological measures predicted the choice irrespective of the cost type (risk, delay or effort). Moreover, gaze tracking revealed an early preference for the congruent option (i.e., costly option following happiness induction).
Analyses are novel, impressive, exhaustive, detailed, robust, and well described. Overall, my impression of this manuscript is quite positive. Almost all of the questions about the theoretical background, the methodological approaches and the future implications that I had in mind when reading the manuscript were clarified within the relevant paragraphs or in the discussion.
The paper represents an advance in understanding how mood induction affect economic choices and the paper provide strong evidence for its conclusions.
Below, few minor suggestions and clarification that I feel might improve the manuscript.

- We thank the Reviewer for the positive evaluation of our work. We believe that, indeed, the suggestions below helped us to improve the manuscript.

1- How did the authors selected the four emotions from the six basic emotions? That is why they excluded Disgust and surprise? How does their finding relates to other line of research on music-induced emotions (e.g., Cowen et al., 2019 doi: 10.1177/1529100619850176; Cowen et al., 2020 doi: 10.1073/pnas.1910704117)

- We admit that our choice of emotions was not well explained in the initial manuscript. In fact, we did not start from a theoretical list of basic emotions. Our starting point was our own theory about how mood impacts choices (Pessiglione et al., 2023). In this theory, mood is conceived as an affective state positioned along a single dimension, from happiness to sadness. The theory makes the prediction that an individual should accept more costs to get more rewards when in a happier state. We already validated this prediction using a mood-induction procedure based on feedback in a quiz game (Heerema et al., 2023). For the present paper, the idea was to replicate those findings using an emotional induction procedure that enabled the comparison with other emotions, to test the specificity of the happiness-to-sadness dimension. It is impossible to test all emotions within a same design, as there can be many of them, depending on the classification. For instance, according to the semantic space theory proposed in the articles cited by the Reviewer (Cowen et al., 2019; 2020), there are 23 emotional clusters with fuzzy boundaries. Our intention was not to be exhaustive, i.e. to provide a systematic mapping from all emotions to all choices. The intention was to test whether our predictions regarding mood were specific, in the sense that emotions with the same valence would similarly affect decisions. We picked anger and fear for both practical and theoretical reasons: 1) anger and fear have been implemented in many lab studies, such that there are procedures available to elicit these emotions with the same material as for happiness and sadness, 2) anger and fear have been associated with stereotyped behaviors (fight and flight) in particular contexts (conflict and threat), such that we could expect from theoretical principles that they would not affect unrelated economic decisions, although the intuition may suggest that they have opposite effects on risk taking.

- In the revised manuscript, we have clarified these points and stated explicitly that our ambition was not to discuss the cartography of emotions. We have also acknowledged that other emotions could be tested, with citation of the work mentioned by the Reviewer (Introduction, page 4-5, lines 68-104):

"The second aim of the present study was to generalize our theoretical account of mood effects on economic decisions to more transient states of happiness and sadness that have equally been explored as emotional states. There is no consensual taxonomy of affective states, which form a superordinate category that includes both emotions and moods (Cowen & Keltner, 2021). Emotions are generally considered as multi-faceted phenomena, triggered by well-defined events that give rise to specific subjective feelings, physiological responses, cognitive appraisals, and action plans (Frijda, 1988; Lazarus, 1991; Ridderinkhof, 2017; Scherer, 2009). They have been classically divided into discrete categories (such as anger and fear) that are shared across cultures and species (Ekman, 1992; LeDoux, 2012) and associated with specific behaviours (such as aggression and escape). Moods are usually defined as more diffuse or global affective states that can last long enough to impact incidental decisions across different contexts. In this sense, they would represent temporal extensions of emotions described as transitory sadness/happiness that may follow negative/positive experience. Our working hypothesis is that happiness and sadness should induce the same shift in preference (bonus for bigger but more costly rewards), irrespective of whether their manifestations are best described as short-lived emotions or long-lasting moods. The prediction is therefore that procedures meant to induce emotions of happiness and sadness should result in the same effects on risk, delay and effort discounting that we observed in our previous study when inducing episodes of positive and negative mood (Heerema et al., 2023).

To induce happiness and sadness, we used guided imagery vignettes engaging foreground attention combined with congruent music in the background, a procedure that was previously validated based on subjective experience (Jallais & Gilet, 2010; Mayer et al., 1995). We opted for this procedure because it was reported to be the second most effective one in a meta-analysis (Westermann et al., 1996), the first one being movies, which were discarded because variations in luminance would have blurred measures of pupil size. Vignettes and music can be used as well to induce other basic emotions, such as anger and fear, which we took as controls for the specificity of mood effects on choices. The idea was not to establish an exhaustive mapping of how emotions affect decisions, but to test prototypic examples that can be compared with sadness and opposed to happiness (because of their negative valence). Hereafter, we keep the term mood to designate the position of the affective sate on the sadness-happiness dimension, with the aim of maintaining a link with our theoretical model and empirical observations exposed in previous studies, even if moods in the present study were more transient. We note that such transitory affective states have been called mood by other groups (Rutledge et al., 2014; Eldar et al., 2015), including when using the same induction procedure with text and vignette (Mayer et al., 1995). Although this is clearly not the only possible meaning of the word, this unidimensional definition is pertinent because mood is typically said to vary from 'good' to 'bad' (Frijda, 1993; Morris & Reilly, 1987), and because it provides continuity with mood disorders in psychiatry (extreme states of sadness/happiness being seen in depressive/manic episodes)."

2- More in general the authors should consider the consistency with the use of "mood", "feelings" and "emotions" especially when parametrising a modulator of a choice bias. See for example the paragraph "Temporal decay of the mood effect on choice" where they correctly note that "Emotions are often reported to be short-lived events". Please consider wisely when using "mood" or "mood fluctuations" or "mood rating" instead of "emotions".

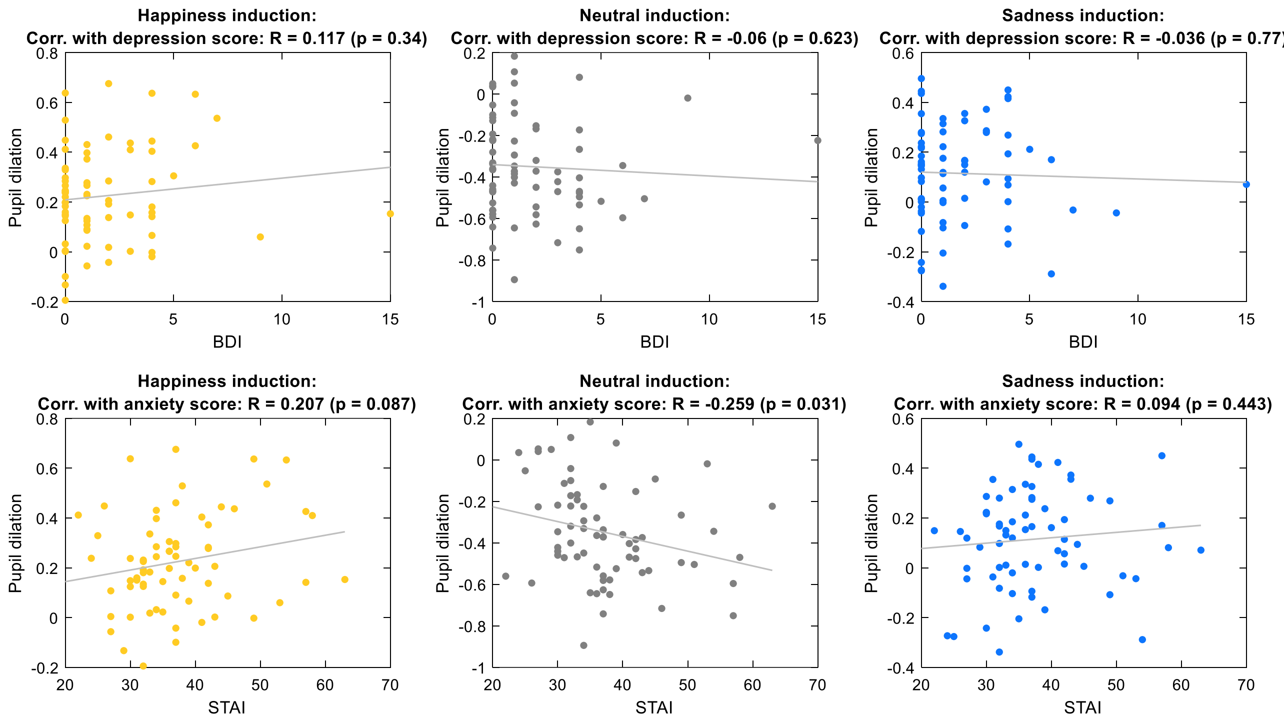
- This is a fair point. We used 'mood' when referring to the position on a happiness-to-sadness dimension, and 'emotion' when referring to the specific category targeted by the induction procedure (i.e., happiness, sadness, anger or fear). We still think that mood is the best term, because it was used in all previous papers on which we built our experimental and theoretical framework (e.g., Rutledge et al., 2014; Eldar et al., 2016; Vinckier et al., 2018; Pessiglione et al., 2023, Heerema et al., 2023), and because it makes the link with states observed in mood disorders (extreme happiness being linked to manic state and extreme sadness to depressive state). However, we acknowledge that using this term misses the difference in time scale between emotions and moods, as outlined in the Discussion (page 24, lines 584-601).

  "In any case, our results suggest that happiness and sadness may not operate on the same processes as other affects listed as basic emotions (Ekman, 1992; LeDoux, 2012), since they have a more pervasive effect on different types of decisions. Indeed, they can be considered as the two poles of a single dimension akin to mood, which is known to bias choices in both real-life behaviours (Edmans et al., 2007; Otto et al., 2016; Saunders, 1993) and lab experiments (Cecchi et al., 2022; Vinckier et al., 2018; Heerema et al., 2023). It is striking that the difference between episodes of positive and negative mood in these studies yielded the same choice bias as observed in the present study with the difference between happiness and sadness inductions. Besides the difference in induction methods (imagination of personally-relevant situations here versus feedbacks on personal performance in previous studies), there is a difference in time scale. In our previous studies, mood fluctuated at the time scale of ~15 minutes, through the integration of recurrent feedback. Here, experience of happiness and sadness lasted about ~1 minute in total, since ratings were related to the last induction only, and tended to fade away before the next induction. We note that mood fluctuations as they spontaneously occur in real life are usually observed at longer time scales, from hours to weeks (Eldar et al., 2018; Villano et al., 2020). A way to bring these results together is to consider that positive/negative mood represents some temporal integration of momentary happiness/sadness (Eldar et al., 2021; Pessiglione et al., 2023), with the same effect on decision-making (a bias toward seeking larger reward, even if costly)."

- In the Introduction (see paragraphs copied in response to point 1), we explain our technical use of term mood. We also made explicit the assumption that happiness and sadness may exert the same bias on choices, whether they are considered as emotions or moods (i.e., irrespective of their time scale). We have checked that we are consistent in our use of the terms emotion and mood. Notably, we have removed the expression 'mood-related emotion', which we realize was a bit awkward. Note that the term 'feeling' was only used when discussing the question of whether participants were truly 'feeling' the emotion targeted by the induction procedure. We have removed this term and replaced it by 'experiencing', to avoid any semantic confusion.

3- Pupil diameter codes not only for the arousal but also for the degree of surprise and volatility (Pulcu and Browning, 2017 doi: 10.7554/eLife.27879) that could be relevant in depression (Pulcu & Browning, 2019 doi:10.1016/j.tics.2019.07.007) and anxiety (Browning et al., 2015 doi: 10.1038/nn.3961). Out of curiosity, did you find any association between pupil diameter and the subjective measures at BDI or STAI? And between pupil diameter and the 'curiosité' ratings in Study 3 and 4?

- We had not tested these associations initially, as they are orthogonal to our main question, but we followed the suggestion to satisfy the Reviewer's curiosity. The figure below shows inter-individual correlations between mean pupil dilation response to the three kinds of inductions (columns) and depression (BDI score, top row) or anxiety (STAI score, bottom row). Dots represent individual participants (N = 69 for whom we collected pupil data). Although there was a trend for anxiety scores (Pearson's *R* and *p*-value in the figure), no correlation would survive correcting for multiple comparisons. Note that the depression and anxiety scores were low in these healthy volunteers, so this absence of significant correlations may not be surprising. Including patients with mood and anxiety disorders would allow for more variance, hence a better power to detect correlations.

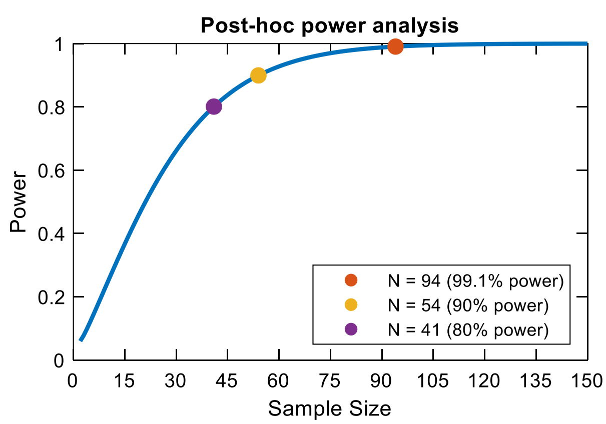


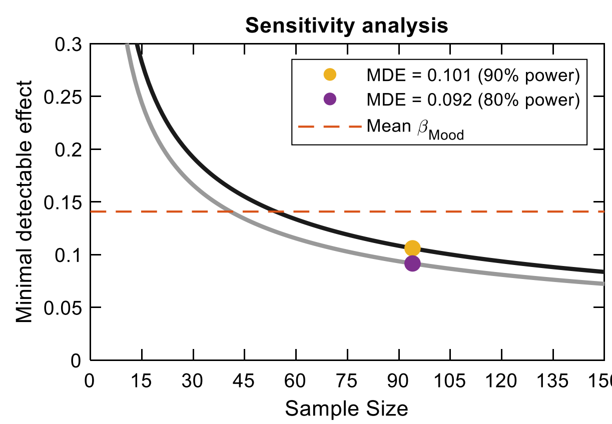
- We also calculated the correlation between pupil dilation response and curiosity ratings across inductions, for each participant (N = 36, in whom both pupil size and curiosity ratings were collected). No significant correlation could be found at the group level (mean $R = 0.01 \pm 0.03$, $t(35) = 0.39$, p = 0.698).

4- The authors recruited n=94 subjects across four studies. Considering the experimental setting (i.e., eye-tracker, EMG, oximeter, skin conductance, the fitness bike etc) it's a good number but, since the study was not preregistered, I was wondering whether they had an a-priori sample size calculation or a power analyses (e.g., Beekmans et al., 2023 doi: 10.3758/s13428-023-02165-7).

- This is a fair question, and we realize we should have been more explicit about it. There was no power calculation for studies 1 and 2, as these were meant as exploratory studies. We were notably unsure about whether anger and fear would affect choices at all. These exploratory studies confirmed a significant effect of mood (rated happiness minus rated sadness), which was expected given our theoretical model (Pessiglione et al., 2023) and our previous experimental dataset (Heerema et al., 2023). However, they showed no effect of anger and fear, whatever the cost type. We therefore dropped anger and fear and intended to replicate the mood effect in studies 3 and 4, which were meant as confirmation studies. We acknowledged that we could, and probably should, have preregistered our design and analysis at this point. In the revised manuscript, we refer to studies 1-2 as exploratory studies (4 emotions plus neutral induction, N = 35) and studies 3-4 as confirmatory studies (2 emotions plus neutral condition, N = 59), so our approach to replication appears more clearly. In the end, we pooled studies together (N = 94), because they all showed the same mood effect on choices.

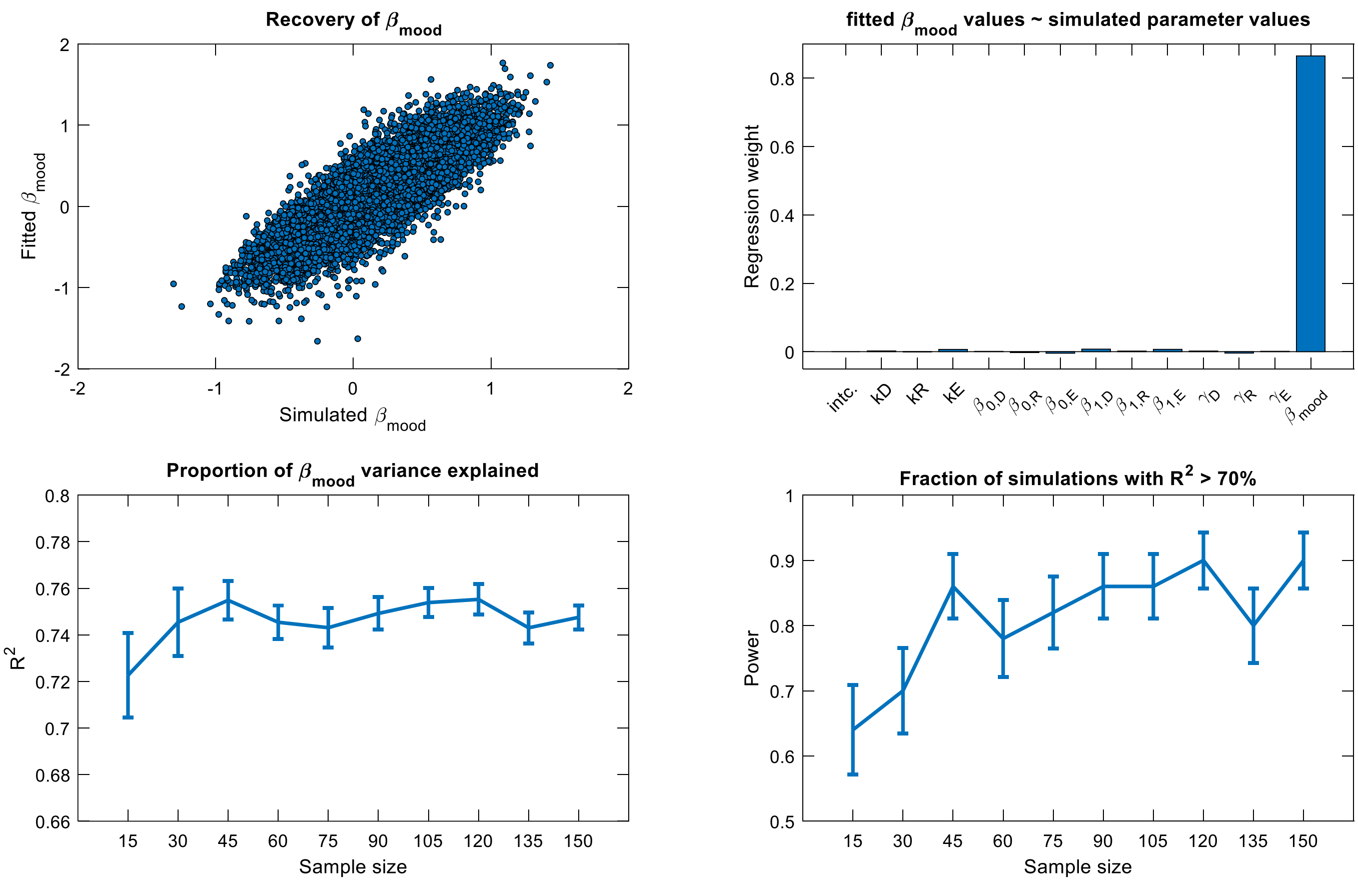
- We conducted a post-hoc power analysis, on the two-tailed t-test of the mood-related choice bias β_{Mood}, with a significance threshold of $p<0.05$, using MATLAB's function *sampsizepwr* (the results being verified with *G*Power*). Across all participants (N = 94), we observed a sample mean $\mu_{\beta\_mood} = 0.141$ and standard deviation $\sigma_{\beta\_mood} = 0.314$, which gives us a Cohen's *d* of 0.45 (approximately medium effect size). In the left figure below, the blue line shows power as a function of sample size, assuming a true effect size of μ_{β_mood}. We achieve a high power of 99.1% for the total sample size (N = 94, red dot). Much smaller samples of N = 54 or N = 41 (yellow and purple dots) would have been required to achieve conventionally adopted levels of 90% or 80% power, respectively. The right figure displays a sensitivity analsysis where we plotted the minimal statistically detectable effect (MDE) with 90% or 80% power (dark and light grey solid lines, respectively). MDEs well below μ_{β_mood} (red dashed line) are found for both conventional power levels (yellow and purple dots), given our sample size $N = 94$.





- We thank the Reviewer for the reference to Beekmans et al. (2023), but we note that the sophisticated COMPASS-toolbox described in this paper can only be used for power analyses on parameters from Rescorla-Wagner models, which we do not employ. Moreover, the toolbox is made to test for power of statistics that do not concern the present study, namely external correlations (e.g. the correlation between a model parameter and a personality trait) or a group difference. Yet, in the spirit of Beekmans et al. (2023), we followed the method of

Wilson & Collins (*eLife,* 2019; DOI), i.e. the paper that Beekmans and colleagues built upon. This method entails a recovery analysis, in which a set of parameter values is drawn from an a priori distribution and used to generate data according to the specifics of the study – it is as if a simulated agent performs the experiment. This simulated dataset is then used to fit the model and compute the proportion of variance explained (R^2). We used the posterior parameter distributions from all 94 participants to draw sets of parameter values and for the simulation of emotion ratings we sampled from the fitted rating probability density functions (cf. response to point 7 below). We ran Monte-Carlo simulations of the experiment (following the design of confirmatory studies) with different sample sizes, varying from 15 to 150 for the sake of coherence with the model-free power analysis above. We repeated this procedure 50 times, i.e. per "experiment" of sample size $N \in (15, 30, \ldots, 150)$ there were 50 repetitions. In total, 41250 virtual agents were simulated (top left panel). Our aim was to gauge the recoverability of their β_{mood} parameter (figure below, top left panel). We first showed that the recoverability of this parameter did not depend on the values of other parameters, by regressing all fitted parameters (after z-scoring) against the simulated β_{mood} values. The regression weight of the simulated β_{mood} values was 0.87, while the absolute weights of all other parameter values were below 0.01 (top right panel). We then obtained the proportion of the variance of fitted β_{mood} values that could be obtained by squaring Pearson coefficients (R) from the correlation between simulated and fitted β_{mood} values. R^2 seemingly levelled off at 75% (bottom left panel). As a criterion, we set $R^2 = 70\%$, informed by the recoverability of the same parameter in Heerema et al. (2023; supplementary material), where we found it to be the lower bound of what online trial generation could achieve. Power, then, was defined as the fraction of the 50 repetitions where this criterion was surpassed (Beekmans et al. 2023). For experiments that include 90 participants, we estimated a power of 86% (bottom right panel).



- In sum, what this model-based power analysis shows if that a power of 80% is already reached with a sample size of N = 45, and the explained variance does not improve beyond this size. Thus, our total sample (N = 94) and even the sample of our confirmatory studies (N=59), was largely enough to detect a significant mood effect on choices. In the revised paper, we have grouped the power and recovery analyses in a section of Supplementary Information (page 60, lines 1369-1409).

"Power and recovery analyses

We conducted a post-hoc power analysis, on the two-tailed t-test of mood-related choice bias parameter (β_M), with a significance threshold of $p<0.05$, using MATLAB's function *sampsizepwr*. Across all participants (N = 94), we observed a mean $\mu_{\beta M} = 0.141$ and standard deviation $\sigma_{\beta M} = 0.314$, which gives us a Cohen's *d* of 0.45 (approximately medium effect size). Assuming that $\mu_{\beta M}$ is an accurate estimate of the true effect size, power can be plotted as function of effect size (Figure S2a). Our sample size (N = 94) offers high power of 99.1%, much smaller samples (N = 54 or N = 41) being sufficient to achieve conventionally adopted power levels (90% or 80%, respectively). We also conducted a sensitivity analysis (Figure S2b) that computes the minimal statistically detectable effect (MDE) as a function of sample size, for different power levels. MDE well below $\mu_{\beta M}$ are obtained for conventional power levels (80% and 90%), given our sample size of $N = 94$.

We also performed a recovery analysis, inspired by the approach proposed in Wilson & Collins (*eLife*, 2019). Data were simulated using the winning computational model, with parameters drawn from posterior distributions obtained in our sample (N = 94). Simulated agents performed our task as designed in confirmatory studies, the sadness and happiness ratings being drawn from the observed mean distribution over all participants. The winning model was fitted to each simulated dataset, and the proportion of variance explained (R^2) was computed. Monte-Carlo simulations were conducted with different sample sizes, varying from 15 to 150 for the sake of coherence with the model-free power analysis above. We repeated this procedure 50 times, for a total of 41250 virtual agents (Figure S2c). To assess recoverability, all simulated parameters (after z-scoring across datasets) were regressed against the fitted β_M (Figure S2d). The regression weight of the simulated β_M was 0.87, while the absolute weights of all other parameter values were below 0.01. Thus, the fitted β_M parameter was essentially determined by the true β_M, with very little contamination of other parameters. We also examined how proportion of explained variance varied with sample size: it increased until reaching a plateau around 75% between N = 30 and N = 45, suggesting that including more participants would not improve the estimation of the β_M parameter."

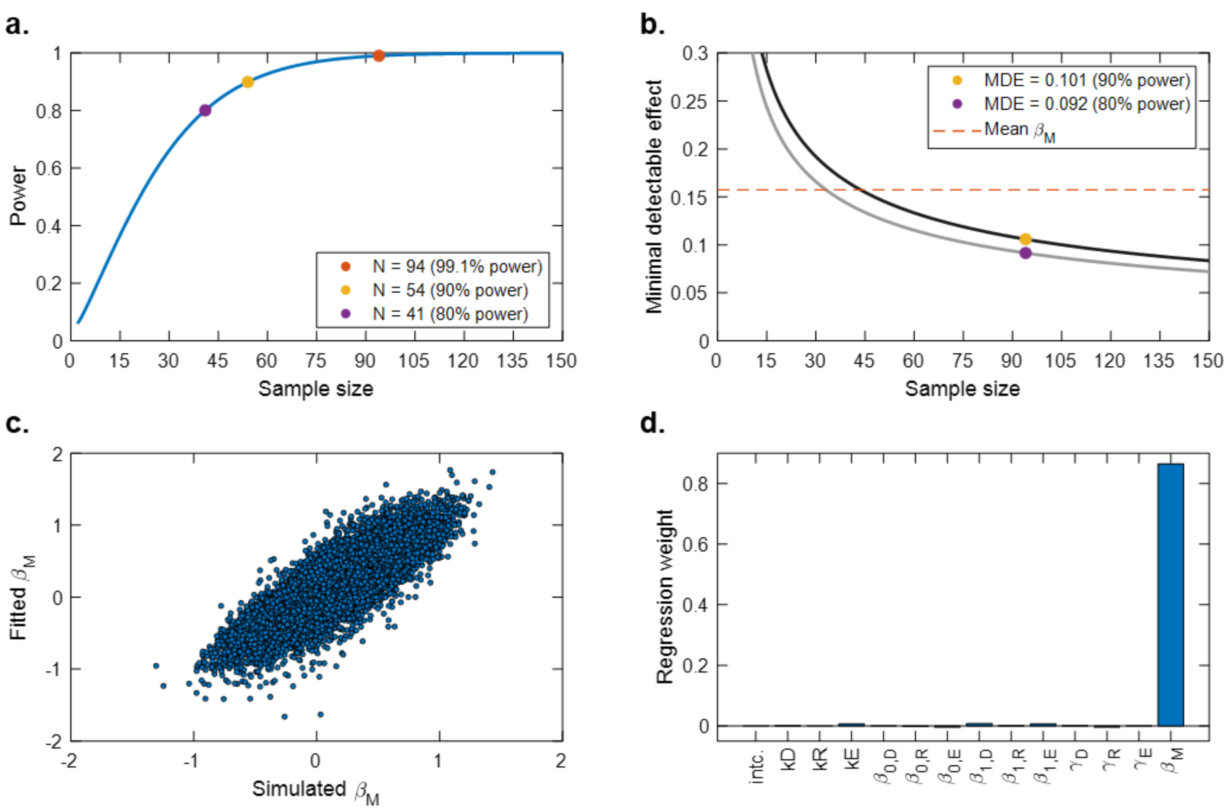


Figure S2: Power and recovery analyses.

a. Post-hoc power analysis. Power is plotted as a function of sample size, with dots comparing the power achieved with our sample size to conventional power levels (80 and 90%).

b. Sensitivity analysis. Minimal detectable effect (MDE) is plotted as a function of sample size. Dots compare the effect size observed in our sample ($\mu_{\beta M}$) to the MDE corresponding to conventional power levels (80 and 90%).

c. Recovery of β_M. Dots represent the β_M fitted to 41250 datasets simulated with various β_M parameters and sample sizes.

d. Regression of fitted β_M against all simulated parameters, across the 41250 simulated datasets. Bars show regression coefficients, i.e. the weight that each simulated parameter had in explaining the fitted parameter.

5- The study design (induction --> economic choice --> subjective ratings) seems more suited to evaluate the effect of economic choices on the subjective ratings rather than the other way round. Maybe it's worth clarify why the authors did not invert the two (e.g., induction --> subjective ratings as a manipulation check --> economic choice) and why the subjective ratings are not a results of the economic choice (e.g., I feel happier after earning more money).

- This is an excellent point that we should have clarified earlier. The main reason is that we did not want the ratings to drive choices, i.e., that participants choose costly options not because they feel happy but because they have declared feeling happy (in their rating). As the main objective of the study was to establish a link between physiological responses to emotion inductions and economic choices, it called for a design where choices directly followed inductions. This way we could have the music played during inductions being prolonged over the decision-making period. We cannot rule out that choices had an impact on ratings, although this may not be very plausible: if subjects feel happier because they accept more costs, then they should always accept more costs, which is not what they do. In any case, even if this effect was present, it could not explain the direct impact of inductions on choices, nor the direct association with physiological measures, since these events (emotional induction and physiological response) happened before economic decisions were made. Note that we followed the same order (choice before rating) in our previous study (Heerema et al., 2023), but the reverse order (rating before choice) in older studies (Vinckier et al., 2018; Cecchi et al., 2022) – a similar effect of mood induction on decisions was observed in all cases. In the revised manuscript, we have clarified the reason for adopting this order in the revised manuscript (Methods, page 29, line 758):

  "The ratings were collected after choices to prevent explicit self-report from interfering with the impact of affective state on decision making."

- We have also explained in the Discussion why this order is not a confound for our main conclusions (page 26, lines 651-658):

  "A more definitive argument comes from physiological responses: while the facial muscles may be contracted on demand, one cannot directly control pupil size or skin conductance. So, the fact that the choice bias could be predicted from physiological responses speaks against a demand effect and for a true emotional involvement. Also, they eliminate the issue of a possible reverse causality: because ratings were made after choices, one could imagine that ratings were impacted by choices, and not the opposite. This reverse interpretation is not possible for the effects of inductions, and the related physiological responses, because the induced mood states were reconstructed from physiological measures recorded before choices were displayed to participants."

6- In all the four studies the duration of the experiment was nearly one hour and a half (calibration session + 60 trials with 10 sec washout + breaks between blocks. Did the authors notice an overall effect of time (such as fatigue or boredom) on choice, effort and emotional induction?

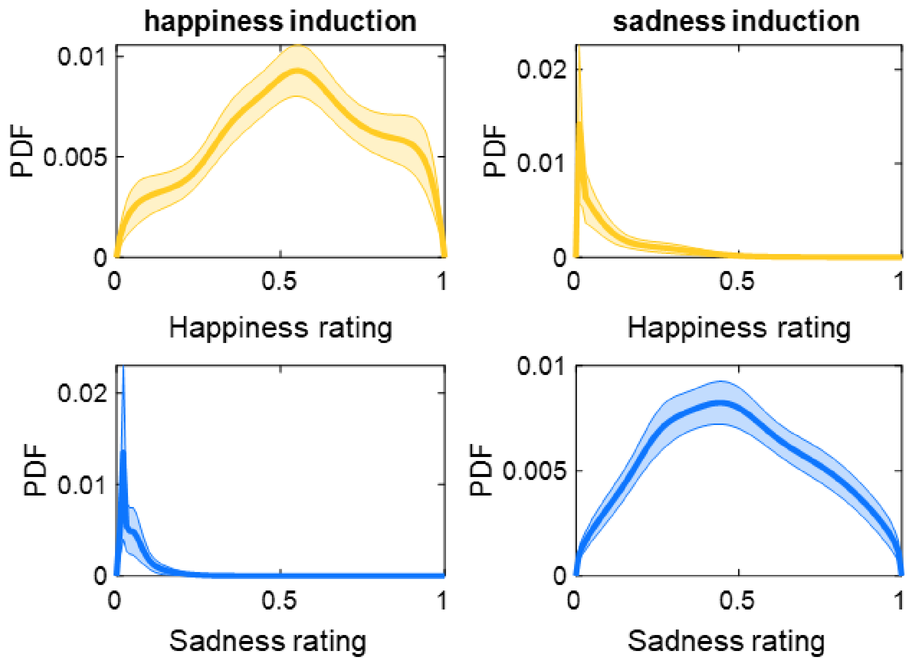
- To examine potential effects of fatigue or boredom, we regressed the trial number against the dependent variables that the Reviewer suggested. We found 1) a slight but significant decline in emotional rating (by 7.2% after 60 trials, on average), meaning that the intensity of reported happiness and sadness were both decreased (b_{happy} = -0.0012±0.0004, $t(93)$ = -3.08, p = 0.003; b_{sad} = -0.0012±0.0003, $t_{sad}(93)$ = -3.55, p = 6e-4), but 2) no significant change in the global choice rate (costly vs. uncostly option). We have added these results in the revised manuscript (page 8, lines 203-208; and pages 12-13, lines 290-292):

  "The impact of emotional inductions was preserved throughout the experiment, although we observed a slight but significant decline (by 7.2% after 60 trials, on average)), meaning that happiness and sadness ratings were both decreased (b_{happy} = -0.0012±0.0004, $t(93)$ = -3.08, p = 0.003; b_{sad} = -0.0012±0.0003, $t_{sad}(93)$ = -3.55, p = 6e-4). This decrease with time on task was orthogonal to our main effect of interest, because inductions of happiness and sadness were regularly alternated along the experiment."

  "There was no significant change in costly choice rate across trials, suggesting that the task was not long enough to induce cognitive fatigue, which we previously found to affect economic decisions (Wiehler et al., 2022)."

7- Were the subjective ratings normally distributed? That means did the participants exploited the whole possible range from 0 to 10? If not, any comment on why the mean ratings following positive and negative induction is 0.6?

- The figure below visualizes the distributions of subjective ratings, for both happiness and sadness inductions (left and right columns) and both happiness and sadness ratings (yellow and blue rows). We plotted the normalized probability density functions, fitted on the individual rating data with beta distributions using MATLAB's function *betapdf*.



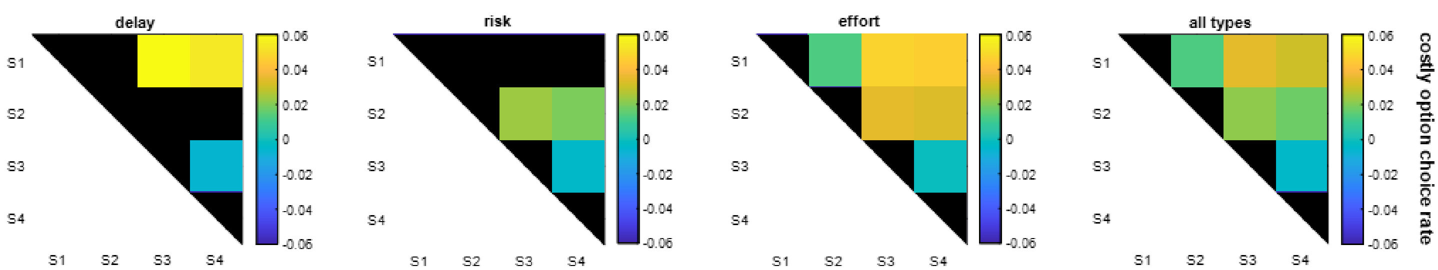
To test for normality, we ran Kolmogorov-Smirnov tests on the ratings for each participant, and counted the number of times the null hypothesis (i.e. that the data is indeed normally distributed) could not be rejected, using MATLAB's *kstest* function with significance threshold $\alpha = 0.05$. This was the case in 87.2% of participants for happiness ratings following happy inductions, and in 93.6% of participants for sadness ratings following sad inductions. We therefore conclude that there is no good reason to suspect that ratings were not normally distributed. Regarding the average rating of 0.6, we note that it is close to the middle of the scale, which was entirely used in a majority of participants, in line with the notion of a normal distribution.

8- I feel that the Supplementary Materials might be ordered and numbered so that when referencing to them in the main text, it's easier to find the appropriate paragraph. For example when referring to the debriefing questionnaires supplementary in the main text (Results – subjective ratings), the reader find a discussion on the mood bias parameter that is not yet introduced in the results.

- We thank the Reviewer for prompting us to rearrange the Supplementary Materials. We have rewritten, simplified and completed Supplementary Materials such that the paragraphs follow the order of the Results section. We have only retained four sections: 1) demographic details of participants, 2) emotion-induction stimuli with their ratings, 3) differences in design between studies, 4) comparison between computational models, 5) responses to debriefing questionnaires.

9- In the model-free analyses (Fig 3a) would be nice to know if there's an effect of study and if so, why. For example the emotions effect on 'delay' trials seems driven mostly by the study denoted by the circle. Similarly the effect size even in the 'risk' and 'effort' for the studies denoted by the triangle and diamond seems pretty small.

➢ We understand the Reviewer's curiosity, but our approach was not adapted to test for differences between studies, notably because the two exploratory studies tested limited samples, and because changes were made for confirmation studies such that some choices are not directly comparable. To us, the important point is that the global mood effect on choices was observed in all four studies, despite their differences. We nevertheless followed the Reviewer's suggestion and calculated the differences in choice rates for the mood effect (happy vs. sad inductions). Note that we omitted the choices involving a risk from study 1 and the choices involving a delay from study 2, as they were framed differently. We then conducted independent-sample t-tests, separately for each cost type as well as for all cost types jointly. The mean differences are visualized in the figure below; as can be seen, the effect sizes are small. None of the differences reached significance even before correcting for multiple comparisons.



10- As a related note, did you find any association between delay and impulsiveness at BIS-11? From a psychopathological perspective is interesting that risk is more associated with happiness and effort with sadness. Manic patients might have an elated mood and be more risky in their decision and depressed might be more abulic but not necessarily more considerate in risky choices. Incidentally, I think that in Figure 3 the legends are inverted for figure b and c.

➢ We had not tested this particular association, because we focused our analyses on the mood effect. To satisfy the Reviewer, we tested the inter-individual correlation between mean choice rate in delay trials and BIS-11 impulsivity score and found no correlation (Pearson's R = 0.005, p = 0.968). Similarly, there was no significant correlation between risky choice rate and hypomania score (R = -0.006, p = 0.966) or between effortful choice rate and depression score (R = -0.127, p = 0.222). Thus, although we agree that the associations mentioned by the Reviewer make sense, there is no evidence to support them in our data.

➢ We thank the Reviewer for spotting that legends were inverted between Figure 3b and 3c. This mistake has been corrected.

11- RTs are z-scored within each participant, correct? Looks like the participants are slower following sadness induction when they choose the costly option. How does this relate to your gaze results, where after sadness induction they stare more the uncostly option?

➢ Yes, it is correct that RTs are z-scored within each participant.

- The relationship between gaze and RT is an interesting question. We thank the Reviewer for pushing us into that direction, as indeed we found interesting links between gaze rate and both choice rate and choice RT.

- We first ran a binomial regression of the costly option gaze rate (during the first second after option display). The regression weight was significantly positive (b_{gaze} = 0.497±0.13, t(57) = 3.79, p = 4e-4), meaning that looking at the costly option when it appears on screen was predictive of a higher likelihood for eventually choosing it.

- We then regressed the first-second costly option gaze rate against choice RT across trials in which that costly option was chosen. Here, the regression weight was significantly negative (b_{gaze} = -0.322±0.08, t(58) = -4.09, p = 1e-4), meaning that costly options are chosen faster when they receive more attention during their first second on screen.

- We have illustrated these links in panels that we have inserted into Figure 7 (panels c and d, see below).

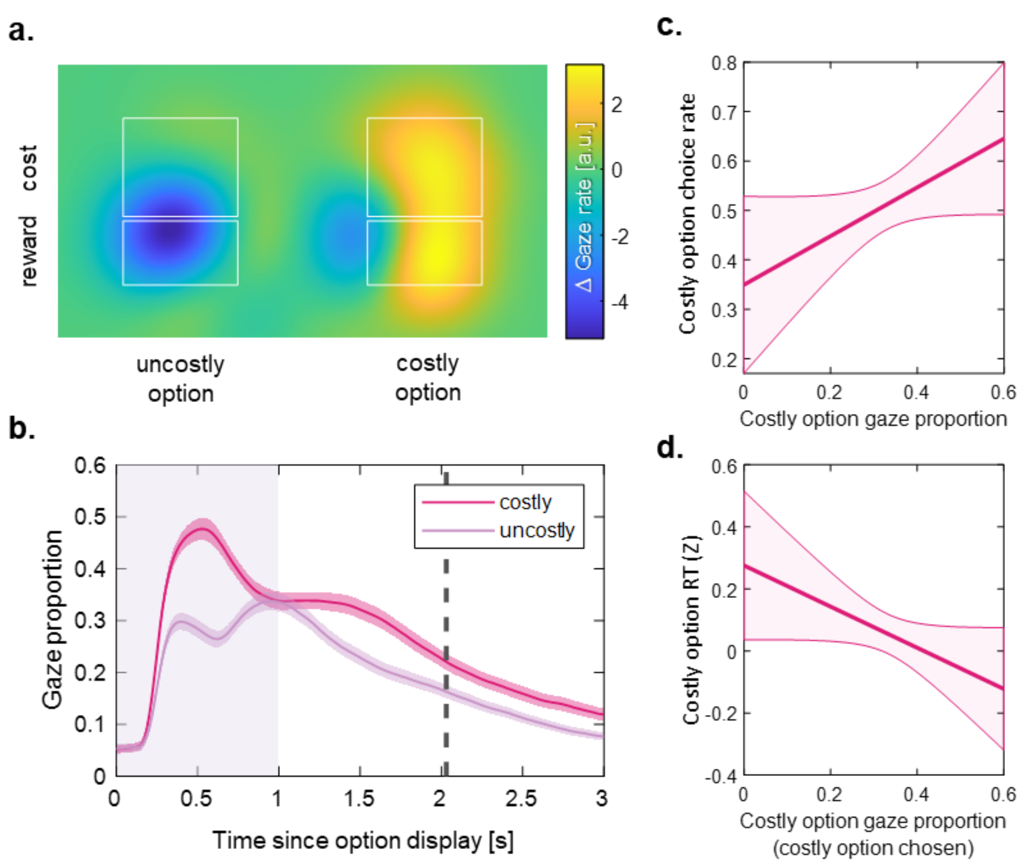


Figure 7. Effects of mood induction on visual attention

a. Difference in gaze distribution between happiness and sadness inductions during the first second following option display (shaded window in the time course below). In the heatmap, screen pixels receiving more gaze after sadness and happiness inductions appear in blue and yellow, respectively. Note that half the trials have been flipped such that the costly option was

always on the right in this analysis, although it was not the case in reality (because the side of costly option display was counterbalanced across trials).
b. Time course of visual attention paid to the two options, within the 0-3s window following option display. Gaze rate is obtained at each time point by dividing the number of samples inside each option frame by the number of trials. It declines with time as decisions are already made in a growing number of trials (median RT is shown by the dotted vertical line). The two curves do not add up to 100% because gaze can be located outside the two option frames. The solid line and shaded area represent the mean and its standard error across participants.
c. Costly option choice rate plotted as a function of costly option gaze proportion (during the first second following option display).
d. Choice RT plotted as a function of costly option gaze proportion (during the first second following option display), specifically for trials in which the costly option was selected.
Both in panels (c) and (d), the solid lines represent linear regression fits and shaded areas the 95% confidence interval across trials and participants.

- These new results are also described in the main text (page 21, lines 520-528):

  "During the first second following option display, more attention was paid to the costly option overall, and even more so after happiness induction relative to sadness induction (**Figure 7a** and **7b**). This pattern was suggestive of a modulation of attention by mood, which was indeed significant in a direct regression of the time spent looking at the costly option (within the first second) against mood score (b_{Mood} = 0.02, $t(58) = 2.50$, $p = 0.015$). In turn, the proportion of time spent during the first second was associated with a higher choice rate ($b_{gaze} = 0.497 \pm 0.13$, $t(57) = 3.79$, $p = 4e-4$) and a shorter RT ($b_{gaze} = -0.322 \pm 0.08$, $t(58) = -4.09$, $p = 1e-4$) when the costly option was eventually selected. Thus, an early attentional bias in favour of the costly option made the selection of this option both more likely and more rapid (**Figure 7c** and **7d**)."

12- In general, the authors did an amazing job in describing the methods, but I feel that it can be improved in term of clarity among terms. At a first read it's hard to retain the definitions of decision function, decision value, discount(-ing) function, discounted value, and discount factor. Probably these are explained in the unpublished paper (Heerema et al., under review). Specifically, in the model-based results would be helpful to define more clearly what each parameter represents and how it affects the sigmoid curve of the logit (decision) function defined as 1/[1+e^-(β0+β1*x)]. Considering the broader readership of the journal, might be easier to see each step the authors implemented, starting from the simple decision function. For example, adding what is the DV term after Equation 1 as depicted in Equations 6, 8 and 10, the slow reader (such as me) can see how the x axis relates to the logit curve (Fig 4b). At this point the author could also better spell out what the discount functions represent and how they relate to the X and Y axis in Figure 4a. Doing so would be easier to understand that the βM*mood is another (negative?) term added to the same equation -(β0+β1*DV-βM*mood). If I got that correctly. Incidentally I'm not sure what's the difference between the E and PE subscripts in Equations 10 and 11 and there's probably a typo at line 305 when mentioning Table S2 instead of Table S4.

- We thank the Reviewer for pointing this difficulty with our depiction of the computational model. Note that the model was actually described twice, with functions using generic notations provided in the Results, and functions using cost-specific notations repeated in the Methods. We have made sure that all definitions are clearly explained in both cases, notably that 1) the decision function is a sigmoid mapping of decision value onto choice probability, 2) the decision value DV is the difference between costly and uncostly option values, 3) option values are computed using a discount function that integrates expected reward with expected cost weighted by a discount factor. It is also explained that the weight on DV (parameter β1) adjusts the sigmoid slope and the bias added to DV (parameter β0) adjusts the sigmoid intercept. Crucially, it is explicitly stated that the additional bias scaled to mood (β_M*Mood) produces a shift in the intercept of the sigmoid. We have copied below the amended depiction of the computational model in the Results section (pages 14-15, lines 342-399):

"We used computational modelling to better capture the role of mood in decision-making and that of the other factors (costs and rewards). We first identified the model that best explained choices independently of mood inductions, and then complemented the winning model by integrating trial-wise mood scores. The equations listed below correspond to the winning model of a Bayesian comparison conducted in another study employing the same choice task (Heerema et al., *2023*) and replicated with the present dataset (see **Methods**).

Choices were modelled with a standard softmax decision function (**Equation 1**) that expresses the selection probability of each option as a sigmoidal transform of decision value *DV* (difference between option values), weighted by an inverse choice temperature β_1 and added to a choice bias β_0. In the equation below, we use a single generic notation for simplicity, even if the choice bias and choice temperature parameters (β_0 and β_1) are actually specific to each cost type (see **Methods** for cost-specific notations).

$$P(costly) = \frac{1}{1+exp(-\beta_0-\beta_1 \cdot DV)} \quad (1)$$

In this decision function, the decision value *DV* is defined as the value difference between the costly and uncostly options, i.e. *V(costly) – V(uncostly)*. the weight and bias parameters (β_1 and β_0) respectively adjusts the slope and the intercept of the sigmoid curve. The slope β_1 captures the consistency of choices (how strictly they follow the decision value), by opposition to their stochasticity (sometimes called temperature). At the extremes, a step function ($\beta_1 = \infty$) would be totally deterministic and a flat function ($\beta_1 = 0$) totally random. The intercept β_0 captures the choice bias, i.e. the inclination to choose one option or the other when the decision value is null (*DV=0*). Here, higher β_0 means greater propensity to choose the costly option.

The subjective option values, *V(costly)* and *V(uncostly)*, are modelled using three different discount functions, depending on the type of cost (risk, delay or effort). Discount functions integrate objective rewards with the costs weighted by a discount factor k_R, k_D, or k_E (**Equations 2-4**). The curvatures of discount functions are controlled by power parameters γ_R, γ_D, and γ_E.

$$V_R = Rew \cdot P - k_R \cdot L \cdot (1-P)^{\gamma_R} \quad (2)$$

where P is the probability of winning the reward *Rew* and L the amount that can be lost by taking the risky option.

$$V_D = Rew \cdot exp(-k_D \cdot D^{\gamma_D}) \text{ where } D \text{ is the delay of reward delivery.} \quad (3)$$

$$V_E = Rew - k_{PE} \cdot E^{\gamma_E} \text{ where } E \text{ is the effort level.} \quad (4)$$

Note that the same discount function is used to compute the value of costly options (for which the reward *Rew* is 30€ and the cost higher than 0), and the value of uncostly options (for which *Rew* varies and cost is 0, such that *V = Rew*).

To check that the set of discount and decision functions described above were best capturing the new choice dataset, we reconducted a Bayesian model comparison. The full space contained 36 different models with systematically varying parametrizations, described in detail in the **Methods** and in **Supplementary Table 2**. We used the Variational Bayesian Analysis toolbox (Daunizeau et al., 2014) to invert the models and compare their log evidence, which offers a tradeoff between accuracy (goodness of fit) and complexity (degrees of freedom). The winning model was submitted to a recovery analysis, which showed good identifiability of all parameters (see **Methods**).

The fitted discount functions were quasilinear for risk, convex for delay and concave for effort (see **Figure 5a**). Thus, subjective option values were always positive for delayed rewards, reflecting the fact that they are always better than no reward at all. In contrast, subjective option values of risky and effortful rewards could be negative, accounting for the possibility that participants would rather give up money than take too great risks or make too demanding efforts.

Psychometric curves (**Figure 5b**) showed that costly choice rates were shifted upwards following happiness induction and downwards after sadness induction, relative to neutral induction. This confirms our theory that mood exerts an additive bias favouring the costly options, on top of the attributes (rewards and costs) that are integrated in the computation of option values. Indeed, mood score significantly explained choice residuals (**Figure 5c**), which represent the variance across trials that was unaccounted for by the model (R = 0.041±0.01, $t(92) = 4.86$, $p = 4.9E\text{-}6$). We therefore updated our model to include mood as a modulator of the choice bias in the softmax function, with parameter β_M shared across cost types.

$$P(costly) = \frac{1}{1+exp(-\ \beta_1 \cdot DV - (\beta_0 + \beta_M \cdot Mood))} \qquad (5)$$

In this augmented softmax function, higher β_M means that positive and negative mood have a greater impact on the choice bias (i.e., irrespective of option values, happiness inclines choices towards costly options and sadness towards uncostly options)."

- In the revised manuscript, we have also amended the legend of Figure 4 (now Figure 5) to use the exact same term as in the model depiction, so the correspondence cannot be missed even by slow readers.

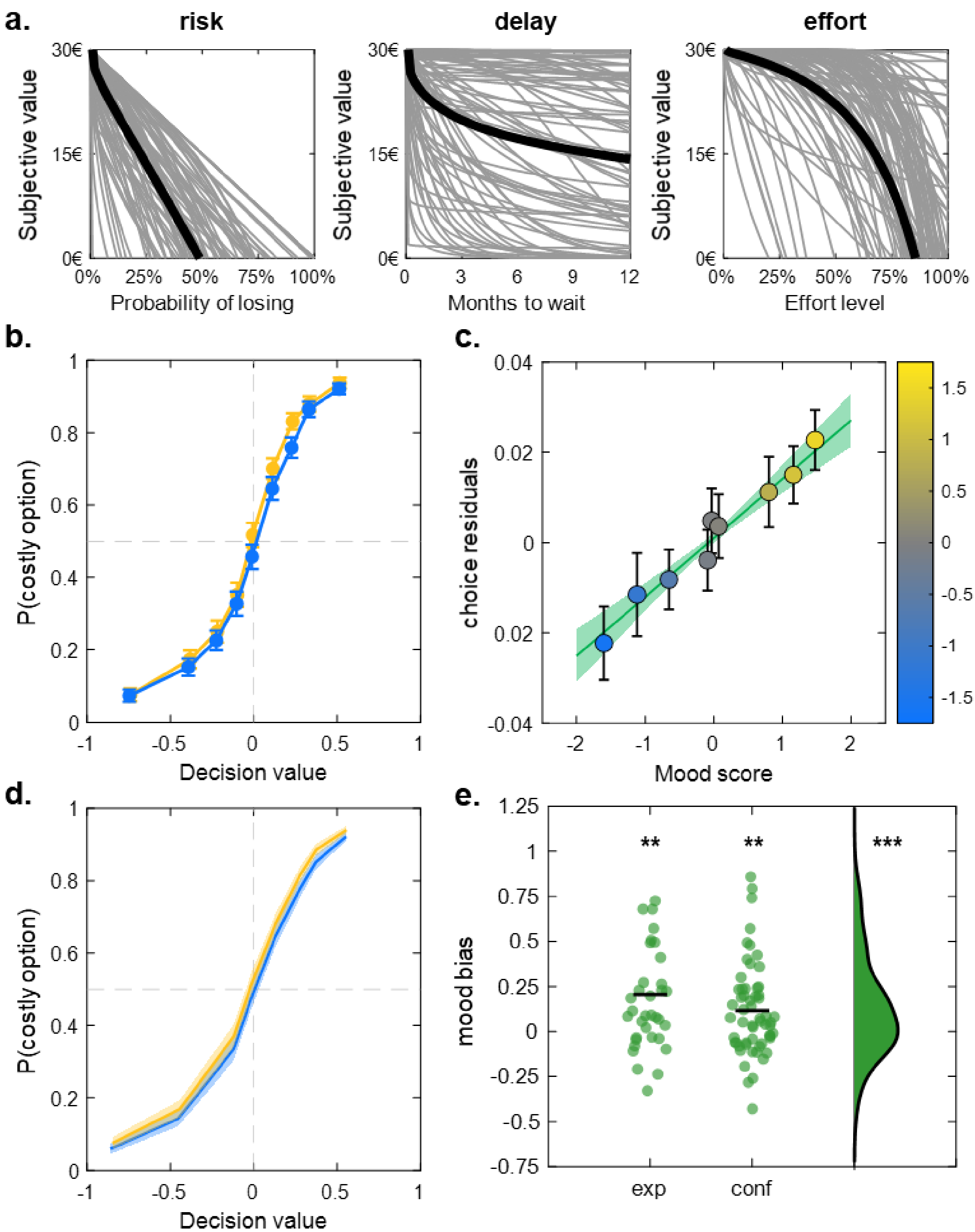


Figure 5. Model-based effects of mood induction on choice behaviour.

a. Discount functions for each of the cost types, obtained from fitting the winning model to the choice data. Each curve describes how the subjective value of the objective big reward (30€) diminishes with increasing costs (risk, delay or effort). Thin grey lines represent individual curves, the thick black line is the mean curve across all participants.

b. Observed psychometric curves. Dots represent costly choice rates following happiness (yellow) and sadness (blue) inductions, as a function of binned decision value (difference between costly and uncostly options), calculated using the discount functions of the winning model. The neutral condition has been removed because it would not be visible between the two curves.

c. Correlation between mood scores and choice residuals. Green line and shaded area display the mean regression fit and its standard error across participants. Choice residuals were obtained by subtracting the probability predicted by the model to the observed choice rate, and then binned according to sorted mood score. Colours of the dots illustrate mood score (same as X-axis) along a dimension going from sadness to happiness.

Both in panels (b) and (c), error bars represent standard errors of the mean.

d. Modelled psychometric curves. Modelled choice rates were obtained here with a softmax decision function that included a bias scaled to mood score. This mood-related choice bias captures the change in the intercept of the psychometric functions between happiness (yellow) and sadness (blue) inductions. Solid lines represent the means and shaded areas represent the standard error of the mean across participants.

e. Distribution of mood-related bias parameters obtained in the exploratory (Exp.) and confirmatory (Conf.) studies. Dots are individual participants. Horizontal lines show the mean across all participants (from both studies). Stars denote distribution density of fitted parameters that differ from the null (zero mood bias). ** $p < 0.01$, *** $p < 0.001$

- About the PE subscript: this is some remaining trace of a previous notation (PE for physical effort) that we have abandoned because it was useless here (because there was no mental effort). We thank the Reviewer for spotting this typo, which has now been corrected.

13- I'm not sure I understand how in study 2 the delay represent an opportunity cost. Can you please clarify? Also the term "intertemporal choices" appear in this paragraph for the first time.

- This is because participants waited in the experimental room until the chosen delay had elapsed. As they could not do anything during this delay (smartphone use was forbidden), it could be considered as an opportunity cost. We realize that our description of exploratory designs was unnecessarily detailed, including conditions that were not retained for confirmatory studies and not even included in the analyses. In the revised manuscript, we focused on the experimental conditions that were included in the analysis and retained for confirmatory studies.

- Intertemporal choice is a standard appellation for choices between rewards delivered at different delays. We removed it nonetheless to avoid any confusion and made sure the term delay (and not time) was used to designate these choices.

14- Probably would be better to add a paragraph named "calibration session" before the description of the "economic choices" that depict the actual course of the experiment. Here, defining each individual indifference curves for each cost type is an elegant solution. Did the authors find any association between these and the other unstandardised study variables (e.g., subjective ratings, questionnaires or physiological measures)?

- Note that the economic choices used in the calibration session were the same as those used in the main experiment. We cannot copy the same text in two separate paragraphs, and we would prefer not to bury the description of choices in a separated calibration paragraph, because this is crucial to understanding the main experiment.

- Regarding the correlations that the Reviewer wants us to test, the issue is that there are tons of possibilities. Each indifference curve is specified by three parameters and there are three curves for the three cost types (risk, delay, effort). Then there are at least two types of subjective ratings (happiness and sadness), five psychometric scores (apathy, impulsivity, hypomania, depression, anxiety) and four physiological measures (skin conductance, pupil size, zygomaticus and corrugator EMG activity). Thus, the request is to test correlations between 9 parameters and 11 measures. It is unlikely that any of these correlations would survive a correction for multiple tests, so we needed specific a priori predictions. Taking inspiration from the Reviewer's point 10, we tested a subset of these correlations (calibration AUCs to impulsivity, depression, hypomania, and apathy score). None was significant after correction for testing multiple correlations.

15- I believe that few methodological decisions might benefit of appropriate references such as "the zygomaticus and corrugator are commonly dichotomized as signalling positive and negative emotions"; "to reduce the dimensionality of physiological responses…this was done by taking the median of the signal over the entire 0-10s induction window"; "From the resulting corrected response estimates, we considered two dimensions for principled reasons".

- Indeed, this is a good suggestion.

- About the zygomaticus and corrugator signalling positive and negative emotions: we already cited Rymarczyk et al. (2011) and Larsen, Norris, & Cacioppo (2003). Note that this is common sense anyway (who would argue that smiling signals a negative emotion?). These citations can be found in the Methods section (page 33, lines 886-889):

  "Since the zygomaticus and corrugator are commonly dichotomized as signalling positive and negative emotions (Larsen et al., 2003; Rymarczyk et al., 2011), we decided to restrict recordings of facial musculature to those muscles after study 1."

- About taking the median of the physiological response: this is common practice to get one data point per trial. We have added references to a couple of papers doing the same (Bradley et al., 2008; Mathôt et al., 2018), although their selection is quite arbitrary. These citations can be found in the Methods section (page 35, lines 946-948):

  "For the pupil dilation and EMG responses, this was done by taking the median of the signal over the entire 0-10s induction window, following common practice (e.g., Bradley et al., 2008; Mathôt et al., 2018)."

- About the principled reasons to decompose emotional responses into two dimensions (valence and arousal), there is an extensive literature. We have selected the following papers: Lang et al., 1998; Russel, 2003; Posner et al., 2005; Henderson et al., 2018. These citations can be found in the Methods section (page 35, line 955):

  "For each physiological response, the estimates were corrected for the induction and session numbers, to account for temporal drifts and changes in recording quality due to sensor readjustment during breaks. From the resulting corrected response estimates, we extracted valence and arousal, the two dimensions onto which emotions are classically mapped (Henderson et al., 2018; Lang et al., 1998; Posner et al., 2005; Russell, 2003)."

16- There's a typo in the references: Eran, Eldar, instead of Eran, E.,

- We thank the Reviewer for spotting this typo (now corrected).

Reviewer #2 (Remarks to the Author):

REVIEWER EXPERTISE: emotions and decision making

The authors have engaged in highly creative work. In keeping with referee guidelines for Nature: Communications Psychology, I organize my review within the pre-specified questions offered by the journal.

• What are the major claims of the paper?

The authors hypothesize that what they call “mood-related emotions” (happiness and sadness) will bias a variety of unrelated economic decisions whereas what they call “other emotions” (anger and fear) will only evoke actions like fight or flight, which are related to their specific triggers. This is a surprising hypothesis because it seems to overlook the fact that other researchers have already found effects for "other emotions" on economic decisions. For example, here are examples that readily come to mind: Lee and Andrade found that fear influenced financial decision making. Lerner, Small, & Loewenstein found effects of disgust on choice prices and/or selling prices. Gambetti et al found that anger influences investment decisions. Ferrer et al. found that anger influenced risky choices with real monetary incentives. The list could go on.

- We apologize if our manuscript was not clear enough, but we note that our claims are misrepresented in the Reviewer’s summary. Let us try to better explain the rationale and the aims of our paper, as the Reviewer may be unaware of our previous work on the topic. Our hypothesis regarding how mood affects choice does not come from nowhere. We have defined mood as an affective state that 1) can be positioned on a single dimension going from happiness to sadness and 2) can last long enough to bias incidental decisions. In our initial studies, we manipulated mood using feedback given to participants in a quiz task, measured mood using subjective rating, and tested mood effects on unrelated risky choices. In two consecutive studies (Vinckier et al., 2018; Cecchi et al., 2022), we found that better mood results in taking more risk for obtaining more reward and identified the neural basis of the underlying mechanisms. Then we elaborated a theory explaining why mood should have such effects. The key idea is that mood provides a valid generalization of reward and cost statistics in auto-correlated environments. Here again we did not pull this idea out of the blue, but built on a model proposed by Eldar and Niv (2015). We further developed the computational basis of the model, using simulations to show that indeed, the bias that mood exerts on choices is adaptive when sources of rewards and costs are correlated between them and across time. Because it assumes that good mood favours foraging behaviours, this model made new predictions: mood should affect not only choices under risk, but also choices that involve other costs, such as delay and effort. We validated these predictions in another empirical study published last year (Heerema et al., 2023), testing the feedback-induced mood changes in N = 102 participants tested twice: when happier, people are more willing to take risk, make effort and wait for delay so they get more reward. The purpose of the present study was 1) to replicate this already well-established finding using a different procedure that would induce happiness and sadness in separate conditions, 2) to show the specificity of happiness and sadness effects by comparing with other emotions, such as anger and fear, induced with the same procedure, 3) to quantify the induced mood state with physiological measures that would directly predict the effect on choices. In our opinion, the last objective was the most important, because it would open an avenue towards predicting choice bias from objective physiological markers, without the need for self-reports, which raises many issues, including the confounds with demand effects. Although it was what we retained for the title ('How mood-related physiological states bias economic decisions'), this point seems to have been missed by the Reviewer.

- Instead, the Reviewer seems focused on the difference with anger and fear. In our opinion, this is not the main part of the paper, which is why we only reported the exploratory studies that tested anger and fear in supplementary materials. We could even remove these studies without undermining our main claim that mood-related physiological states bias economic decisions, but we would prefer to report them, as they were part of our experiments and still informative regarding the specificity of mood effects. We admit that regarding anger and fear, we had no pre-registered predictions, and to be honest, no precise predictions at all. We just anticipated that the effects would be different from mood effects, because there was no theoretical or empirical reason to assume that anger or fear would exert a general bias on all cost-benefit tradeoffs, as was the case for mood. So, our expectations were either to observe no effect or to find effects on a specific type of cost, as has been shown already in the literature, with for example anger and fear respectively increasing and decreasing risk taking. Note that the abstract does not even mention what the Reviewer regards as our surprising hypothesis:

  "When making decisions, humans are susceptible to all sorts of biases, relative to rational norms. An important factor is incidental changes in affective states, such as variations in mood between happiness and sadness. We previously developed a computational model, in which mood affects choice by forming a predisposition to face costs and seek more rewards. Here, we generalized this theory to account for how specific inductions of happiness and sadness affect different types of economic decisions involving a tradeoff between costs (risk, delay, effort) and benefits (financial rewards). Across exploratory and confirmatory studies (N = 94), we observed a consistent bias exerted by transitory mood states, whether they were assessed through self-reports (rated happiness minus rated sadness) or inferred from physiological measures (valence of facial expression times intensity of autonomous arousal). This choice bias was best explained by our computational model, with a mood-scaled bonus added to the value of the more rewarded but more costly option, irrespective of the cost type (risk, delay or effort). In addition, gaze tracking during decision making confirmed that the choice bias was driven by an early preference for the mood-congruent option. Together, these results demonstrate the feasibility of predicting irrational choices from objective measures of affective states."

- Regarding the literature, it is not fair to state that we overlooked the fact that other researchers found effects of emotion inductions on decisions. In the introduction of the initial manuscript, we dedicated a long paragraph to describe results that were previously obtained. We took the example of risk attitude because it has been more explored, and cited contradictory findings for each of the four emotions. We indeed cited many more studies than the three papers picked by the Reviewer (Isen & Patrick, 1983; Arkes et al., 1988; Raghunathan & Pham, 1999; Kugler et al., 2012; Schulreich et al., 2014; Halko et al., 2015; Cohn et al., 2015; Szasz et al., 2016; Ferrer et al., 2017; Calluso et al., 2021). We could have cited even more studies, but being exhaustive would have made the introduction unnecessarily lengthy and redundant. As the Reviewer says, the list could go on, but our citations were sufficient to make our point: previous studies using between-group designs have lead to inconsistent results. For a more exhaustive presentation of the literature, we referred to reviews on the matter coming from different fields: economics (Loewenstein, 2000), psychology (Lerner et al., 2015) and neuroscience (Phelps et al., 2014).

Here is the paragraph where we describe the contradictory reports regarding affects and risk attitude (Introduction, page 4, lines 105-124):

"We are certainly not the first to explore the impact of affective states on cost/benefit tradeoffs. The very idea that incidental emotions can influence judgment and decision-making has been discussed by philosophers for centuries, and empirically tested in different fields of research such as economics (Loewenstein, 2000), psychology (Lerner et al., 2015) and neuroscience (Phelps et al., 2014). However, the evidence is not consistent, as can be illustrated by the literature on risk attitude. Happiness following receipt of a candy gift was reported to increase risk taking (Arkes et al., 1988), but other studies found that the effect was restricted to low-risk gambles, the opposite effect being observed with high-risk gambles (Isen & Patrick, 1983). Anger induced with biographic recall or video clips was once shown to increase risk taking (Szasz et al., 2016), but another study only found the effect in men but not women (Ferrer et al., 2017). Fear induced by threat of electric shocks was said to increase risk aversion in financial decisions (Cohn et al., 2015) but fear induced by a writing task was found to increase risk taking in human interactions (Kugler et al. 2012). In studies inducing negative emotions with written scenarios, some observed that anxious and sad states were respectively driving risk aversion and risk seeking (Raghunathan & Pham, 1999), but others (Calluso et al., 2021) observed that all negative emotions (including anger, fear and sadness) increased risk seeking. A similar degree of inconsistency appears in the literature on how emotions impact decisions that involve delay an effort. As a systematic review would be beyond the scope of this experimental paper, we refer readers to our online table (https://github.com/MBB-team/MoodChoicePhysiology2024) that lists all papers reporting effects of incidental emotions on economic choices involving cost-benefit trade-offs."

- To better show that we did not ignore previous reports but just tried to introduce relevant information in a concise manner, we decided to make our literature overview table public under the CC BY-NC 4.0 license (https://github.com/MBB-team/MoodChoicePhysiology2024). This table (copy-pasted below) lists the papers we found and consulted when we started writing the our manuscript, according to the following inclusion criteria:
  - Original experimental work only (no reviews or meta-analyses)
  - The experiment must contain an incidental affect induction that is orthogonal to the behavioural task (no integral emotions)
  - The observed behaviour must be a choice (not judgement or rating)
  - Specifically, the choice task must feature an economic cost-benefit trade-off (excluding social games)

PUBLICATION		INCIDENTAL AFFECT			DECISION MAKING		RESULT
Authors	Year	Induction method	Induced affect	Comparison	Choice task	Economic cost	Reported effect
Arkes, Herren & Isen	1988	candy bag gift	positive affect	between-subject	buy lottery ticket (gain) or insurance (loss)	risk	positive affect increases willingness to buy lottery tickets and insurance
Augustine & Larsen	2011	word primes, pictures	positive/negative affect	between-subject	choose immediate vs. delayed reward	delay	more delayed choices in positive affect vs. immediate choices in negative affect (especially in low-neurotic people)
Bagneux, Bollon & Dantzer	2011	movie clips	happiness, anger, fear	between-subject	game of dice task	risk	angry and happy participants were less likely to take risk than fearful participants
Calluso et al.	2021	reading text scripts	neutral, disgust (moral/phys.), anger, fear, sadness	between-subject	delay discounting, probability discounting	risk, delay	all negative feelings made participants more myopic and more risk-seeking (sadness mostly so)
Cecchi et al.	2021	feedback during quiz game	positive/negative mood	within-subject	risky motor challenge	risk	more risk taking in positive mood, less risk taking in negative mood
Cohn et al.	2015	stock market boom/bust priming (1) / random electric shocks (2)	fear	(1) between-subject / (2) within-subject	financial investment tasks	risk	more risk aversion after fear induction (in stock market traders)
Desteno et al.	2014	autobiographical recall	gratitude, happiness	between-subject	delay discounting	delay	gratitude but not happiness increases willingness to wait
Ferrer et al.	2016	autobiographical recall, video clip	sadness, anger	between-subject	balloon analog risk task	risk	anger increases risk taking in males, no significant effect of sadness
Halko et al.	2015	liked or disliked music	positive/negative affect	within-subject	gambling	risk	more risk taking during liked music vs. disliked or no music
Halko & Kaustia	2015	liked or disliked music	positive/negative affect	within-subject	gambling	risk	more risk taking during liked vs. disliked music (in adolescents)
Heerema et al.	2023	feedback during quiz game	positive/negative mood	within-subject	delay / risk / effort discounting	risk, delay, effort	bias towards costly but more rewarding options in positive mood, and towards uncostly but less rewarding options in negative mood.
Ifcher & Zarghamee	2011	movie clips	positive affect	between-subject	delay discounting	delay	higher momentary value of the delayed option in positive mood
Isen & Geva	1987	candy bag gift	positive affect	between-subject	gambling	risk	reduced risk preference for high-stakes bets but increased risk preference for low-stakes bets during positive affect
Isen & Patrick	1983	McDonald's gift certificate	positive affect	between-subject	gambling	risk	reduced willingness to accept high-risk bets but increased willingness for low-risk in positive affect
Isen, Nygren & Ashby	1988	candy bag gift	positive affect	between-subject	gambling	risk	more loss aversion in positive affect (no effect of gain subjective utility)
Kugler, Connolly & Ordóñez	2010	writing task	anger/fear/happiness	between-subject	lottery	risk	increased risk-aversion in nonsocial lottery (but decreased risk-aversion in social lottery) with fear compared to happy/angry
Kuhnen & Knutson	2011	picture primes	positive/negative affect	within-subject	stock market game	risk	more risk-seeking following positive inductions and more risk-aversion following negative inductions
Lee & Andrade	2014	movie clips	fear	between-subject	stock market game	risk	more risk aversion in fear-induced group (but context-dependent)
Lee & Andrade	2014	movie clips	fear	between-subject	(1) stock market game, (2) casino game	risk	more risk aversion with fear in stock market game, but more risk seeking in casino game
Lempert et al.	2017	autobiographical recall, imagining scenes	positive/negative affect	between-subject	delay discounting	delay	reduced discounting with positive affect in first experiment, but increased discounting with positive affect in last experiment, no efffect of negative affect
Lerner, Li & Weber	2012	film clips, autobiographical recall	sadness, disgust	between-subject	delay discounting	delay	more impatience with sadness, no effect of disgust
Luo, Ainsli &, Monterosso	2014	emotional face primes	happiness, fear	within-subject	delay discounting	delay	patience higher with fear relative to happiness, but no difference from neutral
Mathar et al.	2023	erotic images	erotic arousal	within-subject	delay discounting	delay	increased delay discounting with erotic arousal
Pyone & Isen	2011	word reading, word recall	positive affect	between-subject	delay discounting	delay	participants in positive affect condition more likely to choose larger later rewards
Raghunathan & Pham	1999	reading text scripts	sadness, anxiety	between-subject	choose low vs. high risk gambles / secure vs. insecure job	risk	bias for high-risk/high-reward options with sadness; bias for low-risk/low-reward options with anxiety
Schulreich et al.	2014	music (eyes closed)	happiness, sadness	within-subject	lottery	risk	more risky choices in happy than in sad condition
Schulreich et al.	2016	emotional face prime	fear	within-subject	gambling	risk	increased risk aversion following fearful faces (attributed to loss aversion)
Schulreich et al.	2020	emotional face prime	fear	within-subject	gambling	risk	increased loss aversion with fearful faces.
Szasz et al.	2016	autobiographic recall	anger, sadness	between-subject	balloon task, Iowa Gambling Task	risk	more risk taking with anger, relative to sadness
Vinckier et al.	2018	feedback during quiz game	positive/negative mood	within-subject	risky motor challenge	risk	more risk taking in positive mood, less risk taking in negative mood
Westbrook et al.	2022	movie clips	sadness	within-subject	n-back task	mental effort	no effect of sadness induction in healthy controls (but increased effort in depression)
Yang et al.	2018	emotional faces	happiness, fear, anger	within-subject	gambling	risk	more risky choices selected when happy, then angry, then scared.
Zhao et al.	2016	emotional pictures	positive/negative affect	within-subject	gambling	risk	more gambling during positive affect; no difference between negative affect and neutral

- About the papers cited by the Reviewer, we would like to make the following comments. The Lee & Andrade (2011, 2014) papers actually strengthen our point, as they report inconsistent effects of fear on risk attitude. In the 2011 paper, fear enhanced risk aversion in a stock market game, but this effect was contingent on several features of the game. In the 2014 paper, fear enhanced risk aversion in a stock market game, but reduced risk aversion in a casino game. We added these citations in our list of contradictory findings that we describe in our introduction. The Lerner, Small, & Loewenstein (2004) paper reported the effects of induced sadness and disgust on the "endowment effect". The result is that participants who received sadness inductions set a higher buy price but a lower sell price, while participants who had received disgust inductions set both a lower buy price and a lower sell price (relative to a third, neutral-induction control group). This experiment is certainly interesting in the context of testing Prospect Theory, but does not involve any tradeoff between reward and costs such as risk, effort and delay, so it cannot be used to derive predictions that would be relevant for our purposes.

The Ferrer, (…), & Lerner (2017) paper was already cited in the introduction as showing that autobiographic recall and video clips used to induce anger increase risk taking in the Balloon Analog Risk Task, but this effect was observed only in males, not females. The Gambetti et al. (2012, 2014) papers are about *trait* anger and anxiety, and featured no emotion induction, so they fall out of our scope. Thus, we thank the Reviewer for suggesting citations, but in the end, these do not point to any relevant insight that we would have missed in the literature.

- **Will the paper be of interest to others in the field? Will the paper influence thinking in the field?**

The paper would be of interest and be influential if it did the following:

1) Begin the introduction by discussion major theories of emotion and decision making and explaining how your hypotheses might build upon or contradict such theories. Presently, the paper does not address existing theories at all.

➢ It is not correct to state that our manuscript did not present any existing theory. Our predictions stem from the model elaborated in our theoretical paper (Pessiglione et al. 2023), itself building on another important model (Eldar & Niv, 2015), which is embedded in the larger frameworks of reinforcement and foraging theory. Our model explains how and why happiness and sadness affect economic decisions, and make predictions at several levels (psychological, computational, clinical). This is explained in the first paragraphs of the introduction (page 2, lines 26-49):

"According to rationality axioms of economic decision theory, our preferences regarding attributes of choice options should be stable and consistent (Von Neumann & Morgenstern, 1944). However, people vary in their attitude towards key attributes, such as risk, showing systematic deviations from rational choice behaviour (Kahneman & Tversky, 1979). Among the sources of variability are changes in internal states, such as mood fluctuations. Field observations have reported spectacular cases in which incidental mood changes spill over onto unrelated decisions. For example, people take more risk when the weather is nice or after a victory of the local sports team, both in personal gambling and on the stock market (Edmans et al., 2007; Otto et al., 2016; Saunders, 1993). These real-world mood effects have been reproduced in experimental settings: mood fluctuations induced by series of positive negative feedbacks incline participants into accepting or declining risky challenges (Cecchi et al., 2022; Vinckier et al., 2018).
Although mood-related changes in preferences are clearly irrational when mood triggers are independent from choice outcomes, they may arise from what would be considered as adaptations from a behavioural ecology perspective, which is notably taken in foraging theory (Stephens & Krebs, 1986). In a recent theoretical work, we have shown, using simulations of foraging behaviour, that mood-related changes in preferences are indeed adaptive when sources of costs and rewards in the environment are correlated, as is the case across seasonal fluctuations (Pessiglione et al., 2023). Mood is defined here as an affective state that can be positioned on a continuum from sadness to happiness and that varies with the experience of positive and negative outcomes. Our model assumes that mood shifts the tradeoff between costs and benefits in the decision to forage for food reward. This assumption is applied not only to risk attitude but also to time preference and effort sensitivity: when in a good mood, theoretical agents are more willing to take more risks, wait for longer delays and exert more strenuous efforts for the prospect of collecting bigger rewards."

- Besides, we respectfully disagree with what the introduction of an empirical paper should include. As is written in Communications Psychology guidelines, an introduction must provide sufficient information from the literature for readers to understand the rationale behind the research question. A discussion of major theories of emotion and decision making would be inappropriate here. However, we are happy to incorporate any specific theory that the Reviewer would like to see, provided it makes relevant predictions about how happiness and sadness affect cost-benefit trade-offs.

2) Conduct a more systematic literature review and discuss studies that run counter to your hypothesis, presenting a more even-handed review of the field.

- A systematic literature review is precisely what we had done before writing the manuscript. The overview of the relevant papers given our search criteria (cf. response to point 1 above) are listed in the Excel table that we have made public and referred to in the introduction. We are happy to include any other paper that the Reviewer would suggest and which we would have missed. All reported effects (consistent or not with our theory) are listed there. It was already explained in the initial manuscript that contradictory findings have been reported regarding the effects of emotions on decisions. We also advanced potential explanations that motivated our experimental choices. A notable weakness in many previous studies is a between-subject manipulation combined with a small sample, which is why we opted for a within-subject manipulation in a large sample. Another weakness is the absence of check that the manipulation was effective (i.e., that the targeted affect was truly induced), which is why we collected both subjective ratings and physiological responses. This was explained in the Introduction (page 4-5, lines 125-138):

  "Several factors may have contributed to the non-replicability of the links between emotions and decisions. A first factor is the diversity of methods used to induce emotions and to probe decisions, a reason why we systematically employed the same procedure to induce all emotions, and the same format for all types of cost in our economic choice task. Another issue is the use of comparisons between groups, which may occasion false positives related to individual preferences, especially when combined with a small sample size. Indeed, studies using within-subject designs to compare emotional states induced by music, as we have done here, found more robust results, with more risk seeking in happy states and more risk aversion in sad states (Halko et al., 2015; Schulreich et al., 2014). An even more critical issue is that the emotional state targeted by the induction is rarely assessed, such that decisions are compared between conditions (e.g., between groups that do or do not receive a happiness-enhancing gift). We argue here that decisions should be related to individual experiences, which may differ between participants for a same induction. For this reason, we collected both subjective ratings of experienced emotion and objective markers of intensity and valence."

3) Provide more theoretical background for the idea that some emotions should be considered "moods"rather than emotions. This conceptualization runs counter to the more typical idea that a feeling such as sadness could be an emotion when intense, target-focused, and short lived OR a mood when mild, diffuse, and protracted. Many journal articles and books in affective science have long viewed the relation between emotion and mood in this way (e.g., papers by Ekman, DeSteno,

Keltner, Davidson, Clore, Scherer, Ellsworth, Gross, Levenson, etc). It is certainly possible to challenge that view, but one must provide a rationale for doing so.

- There is a misunderstanding here. We do not pretend that some emotions should be considered as moods. In a way, our theory is agnostic to the distinction between emotion and mood. We are fine with the typical idea put forward by the Reviewer that happiness and sadness can be emotions (when intense, target-focused, and short-lived) or moods (when mild, diffuse, and protracted). What we do pretend is that happiness and sadness exert systematic biases on decision-making, irrespective of whether they manifest as emotions or moods. In our initial manuscript, we designated happiness and sadness as 'mood-related emotions', to acknowledge the facts that 1) they are emotions (not moods) when induced with our procedure, 2) they connect to episodes of mood disorders (depression as extreme sadness and mania as extreme happiness), and 3) they exert the same bias on decisions (alternation between happiness and sadness should mimic the effect of mood fluctuations).

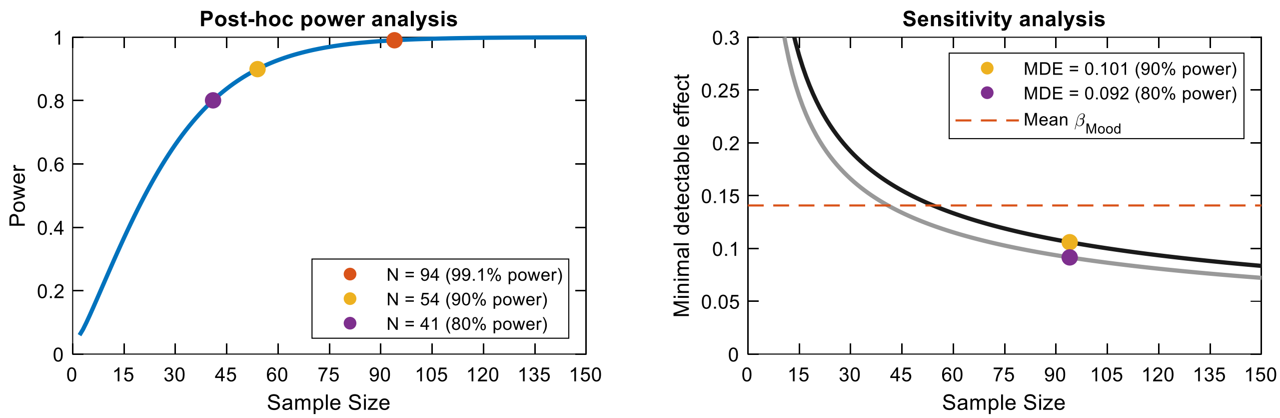
- We are aware that words such as *mood* are polysemic and we understand why our terminology has been misunderstood by this Reviewer. We took a dimensional approach that is standard in psychiatry: we called mood score the position on a dimension that goes from very sad to very happy. Our reasoning was the following: if happiness and sadness are analogous to the states between which mood fluctuates, then the effects of happiness and sadness on decisions should be similar to those of mood fluctuations. As we previously established the behavioural signatures of mood fluctuations on economic choices, we tested whether happiness and sadness would produce the same signatures. The data are strikingly clear (compare the present and our previous study): the pattern is exactly the same (significant effect on choice bias whatever the type of cost). We have revised the introduction to better explain the rationale of our study (pages 2-3 lines 68-104):

  "We have validated our computational model of choice behaviour, using an experimental design where changes in mood were induced by series of positive or negative feedback to the answers that participants provided to general knowledge quiz questions (Heerema et al., 2023). Mood was assessed with a subjective rating on a scale graduated from very bad to very good, and its effects on choices were captured by a bonus parameter added to the value of bigger but more costly rewards. However, subjective ratings have been criticized on the grounds that they are bounded by the quality of insight and susceptible to a potential demand effect (when participants can guess what experimenters expect them to report). The first aim of the present study was to circumvent these limitations by collecting objective physiological markers (pupil diameter and skin conductance) of autonomic arousal, which by definition cannot be voluntarily controlled and therefore remain immune to demand effects (Bach et al., 2010; Bradley et al., 2008; Figner & Murphy, 2011; Joshi & Gold, 2019). As autonomic arousal only reflects the intensity of mood changes (Kreibig, 2010; Siegel et al., 2018), we also recorded the electromyographic activity of the smiling and frowning muscles (zygomaticus and corrugator), which respectively reflect the positive and negative valence of mood states, i.e. the relative expression of happiness and sadness (Brown & Schwartz, 1980; Hu & Wan, 2003; Larsen et al., 2003; Rymarczyk et al., 2011). Thus, the idea was to build and validate an analytic pipeline that would enable inferring mood from objective markers that do not depend on subjective reports.
  The second aim of the present study was to generalize our theoretical account of mood effects on economic decisions to more transient states of happiness and sadness that have equally been explored as emotional states. There is no consensual taxonomy of affective states, which form a superordinate category that includes both emotions and moods (Cowen & Keltner, 2021).

Emotions are generally considered as multi-faceted phenomena, triggered by well-defined events that give rise to specific subjective experience, physiological responses, cognitive appraisals, and action plans (Frijda, 1988; Lazarus, 1991; Ridderinkhof, 2017; Scherer, 2009). They have been classically divided into discrete categories (such as anger and fear) that are shared across cultures and species (Ekman, 1992; LeDoux, 2012) and associated with specific behaviours (such as aggression and escape). Moods are usually defined as more diffuse or global affective states that can last long enough to impact incidental decisions across different contexts. In this sense, they would represent temporal extensions of emotions described as transitory sadness/happiness that may follow negative/positive experience. Our working hypothesis is that happiness and sadness should induce the same shift in preference (bonus for bigger but more costly rewards), irrespective of whether their manifestations are best described as short-lived emotions or long-lasting moods. The prediction is therefore that procedures meant to induce emotions of happiness and sadness should result in the same effects on risk, delay and effort discounting that we observed in our previous study when inducing episodes of positive and negative mood (Heerema et al., 2023).
To induce happiness and sadness, we used guided imagery vignettes engaging foreground attention combined with congruent music in the background, a procedure that was previously validated based on subjective experience (Jallais & Gilet, 2010; Mayer et al., 1995). We opted for this procedure because it was reported to be the second most effective one in a meta-analysis (Westermann et al., 1996), the first one being movies, which we discarded because variations in luminance would have blurred measures of pupil size. Vignettes and music can be used as well to induce other basic emotions, such as anger and fear, which we took as controls for the specificity of mood effects on choices. The idea was not to establish an exhaustive mapping of how emotions affect decisions, but to test prototypic examples that can be compared with sadness and opposed to happiness (because of their negative valence). Hereafter, we keep the term mood to designate the position of the affective state on the sadness-happiness dimension, with the aim of maintaining a link with our theoretical model and empirical observations exposed in previous studies, even if moods in the present study were more transient. We note that such transitory affective states have been called mood by other groups (Eldar & Niv, 2015; Rutledge et al., 2014), including when using the same induction procedure with text and vignette (Mayer et al., 1995). Although this is clearly not the only possible meaning of the word, this unidimensional definition is pertinent because mood is typically said to vary from 'good' to 'bad' (Frijda, 1993; Morris & Reilly, 1987), and because it provides continuity with mood disorders in psychiatry (extreme states of sadness/happiness being seen in depressive/manic episodes)."

4) Run studies with much larger sample sizes, improving generalizability and keeping up with norms in the field.

- The present paper included N = 94 participants. This is largely sufficient to ensure high statistical power in detecting effects of emotions on decisions. Our main effect was the weight of mood score on costly choice rate (mood-related choice bias parameter), with a mean size μ_{β_mood} = 0.141 and standard deviation σ_{β_mood} = 0.314, which gives a Cohen's *d* of 0.45 (medium effect size, roughly). We conducted a post-hoc power analysis on the two-tailed one-sample Student's t-test used to assess whether the choice bias differed from the null. In the left figure below, the blue line shows power as a function of sample size, assuming a true effect size of μ_{β_mood}. We achieve a high power of 99.1% for the total sample size (N = 94, red dot). Much smaller samples of N = 54 or N = 41 (yellow and purple dots) would have been required to achieve conventionally adopted levels of 90% or 80% power, respectively. The right figure displays a sensitivity analasysis where we plotted the minimal statistically detectable effect (MDE) with 90% or 80% power (dark and light grey solid lines, respectively). MDEs well below μ_{β_mood} (red dashed line) are found for both conventional power levels (yellow and purple dots), given our sample size *N* = 94.

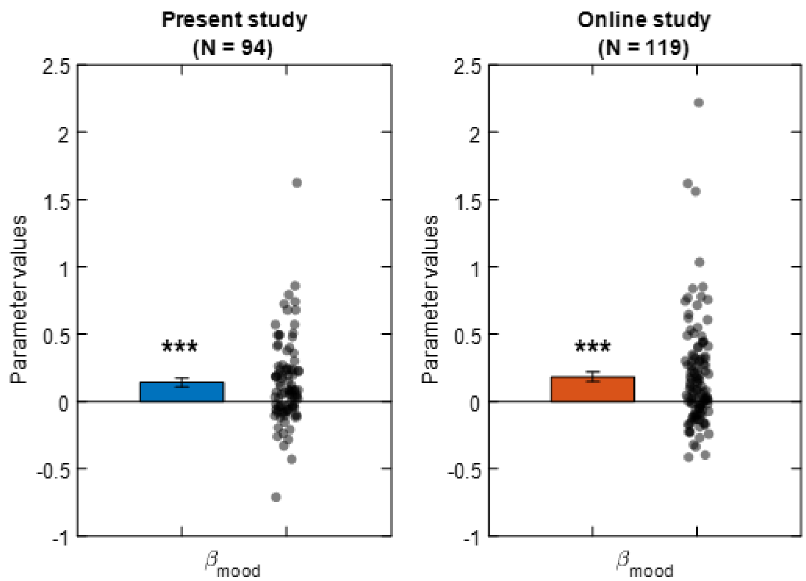


- As the norm in the field is 80% power, N = 41 participants would have been sufficient, so the sample size is not an issue for the validity of our conclusions. If the Reviewer is concerned with the 94 participants being administered 4 tasks with slight variations in the design, let us reiterate that the main effect was significant in each of the 4 subgroups. Observing the same effect despite variations in the design makes the result not less but more robust. It may be worth emphasizing that the probability of observing a significant effect in each of 4 small subgroups under the null is way lower than the probability of observing the effect in the large group made of pooled subgroups.

- However, if what the Reviewer has in mind is the absence of effect for anger and fear, then we fully admit that it could be due to statistical power. We cannot run a power analysis on potential effects of anger and fear because we have no clue about their size and variance, as these effects may not exist at all. What we can do is using the sensitivity analysis: as we only tested anger and fear in exploratory studies (N = 35), the minimal detectable effect is around 0.12, which is close to the effect of mood. Indeed, more participants would be needed to detect smaller effects. Note that we cannot conclude anything from a null result anyway, so we have no claim about the effects of anger and fear. Again, our claim is about the effect of happiness and sadness, which was robust enough with our sample size. It is now clearly said in the discussion (pages 24-25, lines 581-583) that we cannot draw any strong conclusion from the null effect of anger and fear:

  "Yet, null results should be interpreted with caution, and further studies may help specify in what context and on which decisions these and other negative emotions could have an impact."

- Beyond power analysis in the present paper, the robustness of the results is also confirmed with replications (see Appendix 2 below). With a different induction procedure, the same mood effect on choices was shown in our previous paper (Heerema et al., 2023), which included N = 102 participants ($t(101) = 3.54$, p = 6e-4). Moreover, the 102 participants were tested twice, and the result was highly significant in each the two visits, providing an internal replication within this study. Finally, we have run an online study in N = 119 participants with the same induction procedure as in the present paper (happiness and sadness induced by music + vignettes) and again observed a highly significant mood effect on decisions ($t(118)$=5.10, p = 1e-6). These results will be published in a future paper because the online study also included a longitudinal follow-up for which data analysis is still ongoing. Also, physiological measures, which are the focus of the present paper, were obviously not recorded in this online study testing participants at home. Nevertheless, this online replication of the exact same experimental design (see figure below) shows that the effect is extremely robust. If we include our previous paper, this makes 3 highly significant replications in 3 different samples with an average of more than N = 100 participants. To our knowledge, this is way above norms in the field.



5) Balance gender representation within your studies or at least justify having such a strong skew toward one gender and limit conclusions accordingly.

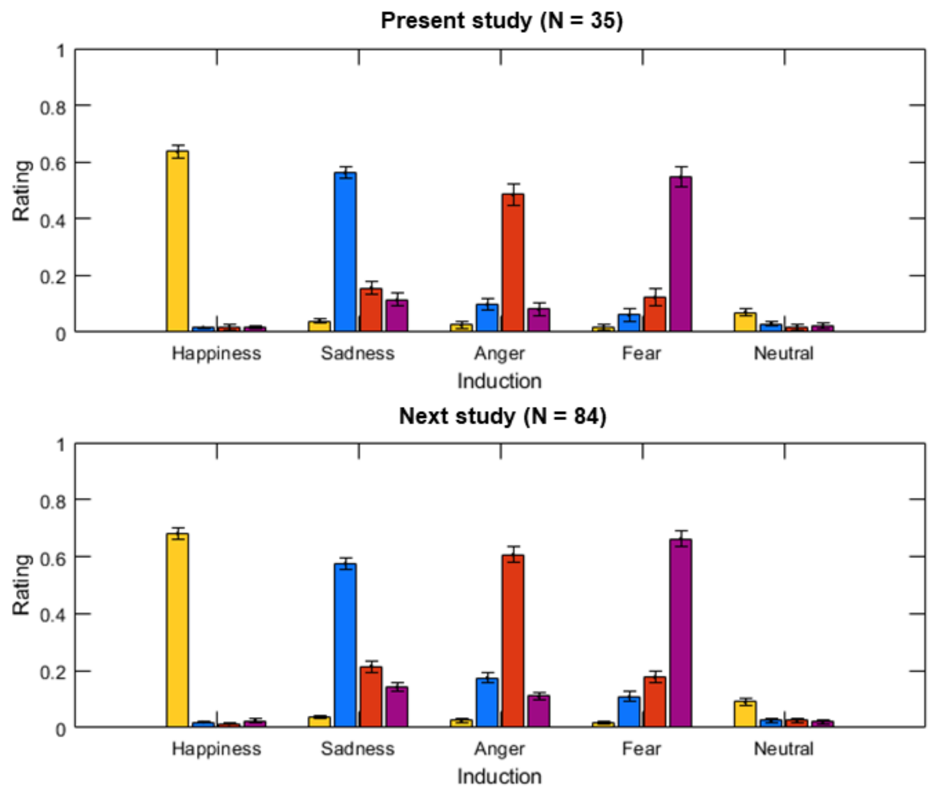
- The gender was indeed unbalanced (59 females and 35 males). This is because we did not select participants, as our ethics committee would consider it to be gender-based discrimination. So we included all participants who applied for participation and satisfied the criteria. It just happened, as is often the case in our experience, that more women volunteered to take part in the study. In this particular case, the requirement to be clean-shaved for facial EMG measurement, as detailed in lines 687-688, might have contributed to less men answering the call for participation. However, there is no reason to believe that gender unbalance could have biased the results, because there was no difference between females and males in our effect of interest, and because the two genders were sufficiently represented to ensure that the effect was significant in both cases ($\beta_{M,men}$ = 0.12±0.05, $\beta_{M,women}$ = 0.16±0.04, $t(34,57) > 2.61$, $p < 0.014$). Indeed, we note that in a meta-analysis comparing the effectiveness of different mood induction methods, Westermann et al. (1996) found no gender difference. Similarly, we found no gender difference in the effects of mood fluctuations on economic decisions in our previous paper (Heerema et al., 2023). We therefore had no reason to suspect that a skewed gender distribution would impact our effects of interest. To check that it was not the case, we compared males to females in terms of reactivity to emotional induction and in terms of mood bias on choice behaviour. For the emotional reactivity, we computed the mean target rating (i.e., self-reported happiness following a happiness induction, and self-reported sadness following a sadness induction). We found no significant difference: μ_{female} = 0.596±0.03, μ_{male} = 0.601±0.03, $t(92)$ = -

0.224, p = 0.823. For the mood bias, we compared the β_{Mood} parameter values and found no significant difference either: μ_{female} = 0.153±0.04, μ_{male} = 0.120±0.05, $t(92)$ = 0.481, p = 0.631. Also, we note that in our online replication study, gender representation was more balanced (61 females and 58 males), and yet the result was exactly the same, with a highly significant effect of mood score on choice bias. All these results concur to demonstrate that unbalanced gender representation in the present study had no weight on our main conclusions.

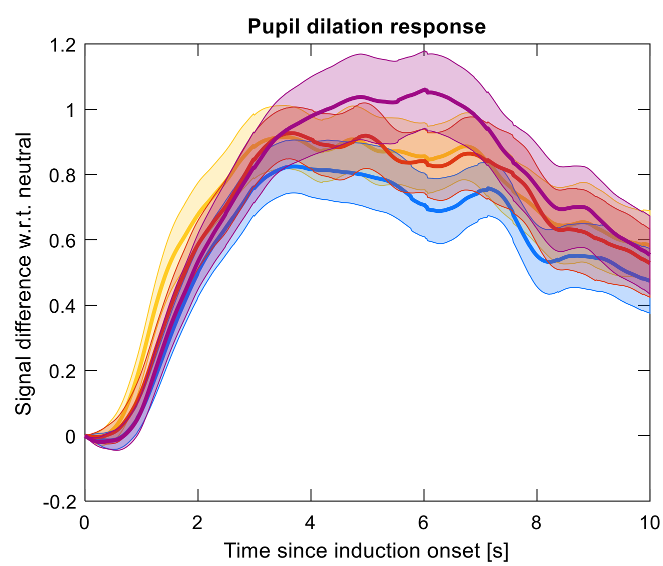
6) Address the crucial confound in the independent variable: The inductions for anger were not nearly as successful (in terms of magnitude) as were the inductions for the other emotions. Thus, when the authors conclude that, contrary to happiness and sadness inductions, anger and fear inductions did not affect economic decisions, they cannot rule out the possibility that anger was simply weaker than the other emotions.

- ➢ Again the results of anger and fear inductions are not crucial in our demonstration, we could remove them without damaging our conclusions, so we are a bit surprised that the Reviewer keeps focus on these results. We are even more surprised by the invention of a 'crucial confound', which sounds like a desperate attempt to kill a claim that we do not even make. What the Reviewer states here is simply not true: there is no evidence that induction of anger and fear were less successfully induced that happiness and sadness, so this cannot be the reason why these emotions had no effect on choices, whether the Reviewer likes it or not.

- ➢ To assess the efficiency of emotional inductions, we examined both subjective ratings and physiological responses. Anger and fear were taken as points of comparison for sadness, to keep the valence negative. When comparing target ratings, there was no significant difference between anger and sadness or between fear and sadness. To show the robustness of inductions on ratings, we reproduce below the results of another study using the exact same induction procedure (but a different choice task), in a larger number of participants (N = 84).



- However, we agree that subjective reports are susceptible to demand effects and can be artificially equalized if participants normalize their rating to use the entire scale. This is precisely why we think that objective physiological measures are important. Critically, there was no indication either of a less successful induction for anger and fear in physiological responses. Numerically, anger and fear (red and purple lines) were accompanied not by a weaker, but a stronger pupil dilation response relative to sadness (blue line). As pupil cannot be voluntarily controlled, this is a good indication that the intensity of anger and fear was no weaker than the intensity of sadness.



- Thus, both subjective and objective measures of emotional experience concur to show the efficiency of our induction procedures for all emotions. Note that even if anger and fear inductions were weaker, it would not really matter for our analyses, because in addition to comparing between conditions, we tested whether emotional states, inferred for each induction in each individual (either from self-report or physiology), have an impact on subsequent choices. In other words, a potential difference in the induction success was already factored in the linear regression analysis. Therefore, there cannot be any confound here.

7) In keeping with scientific norms for the field, please pre-register your hypotheses and analytic plans so that it is clear which data need to be analyzed with post-hoc corrections and so that opportunistic findings are less likely. The present paper, which used different exclusion criteria from one study to the next, is not in keeping with current best practices.

- We agree that pre-registration is a good practice for replication studies. However, predictions and analyses were made clear in our previous publications. This was probably not emphasized enough in our initial manuscript, but the predictions about happiness and sadness modulating the bias for costly options in decision making were announced in our theoretical paper (Pessiglione et al., 2023). Importantly, the computational model capturing the choice bias is exactly the same as the one developed from theoretical principles. In plain words: we have not opportunistically changed the equations to embellish the result. Furthermore, the analytic pipeline is exactly the same as the one used in our paper published last year that used the same choices, just with a different induction procedure (Heerema et al., 2023). Thus, computational models and statistical analysis were not pre-registered, but they were already made public in our previous papers.

- Another important point is that the manuscript includes an internal replication. We have made this more apparent in our revision by clearly separating exploratory studies and replication studies. Our exploratory studies included anger and fear inductions to compare sadness with other negative emotions. In this first sample (N = 35), we found the effect of happiness and sadness that was expected from our published theoretical and empirical papers, but no significant effect of anger and fear. We could not have pre-registered expectations for the effects of anger and fear, because we had no principled predictions and because previous studies implementing these emotions have yielded contradictory results. Then we abandoned anger and fear, which were not conclusive, and conducted a replication study in a second sample (N = 59). We concede that we should have pre-registered our hypotheses and analytic plans at this point, and we will certainly do so for our future confirmation studies. However, the hypothesis was obviously that we would obtain the same results, and we applied the same analytic pipeline.

- It is correct that relative to exploratory studies, we have relaxed the inclusion criteria for replication studies. Note that what was changed were the criteria for testing participants, not for post-hoc inclusion of datasets in the analysis. More precisely, we changed the cut-off on depression and anxiety scores for inviting candidates to participate in the experiment. We simply realized in our exploration studies that these cut-offs had been set too low, for no good reason. Relaxing these cut-offs enabled testing more participants with a higher diversity in depression and anxiety scores. Yet this did not change anything in the results. Applying the same stringent criteria would have led to excluding 11 participants from our replication studies, leaving a total of N = 83 participants. Had we done that, the main effect would stay entirely unchanged: in the subset of 83 participants, $\beta_{mood} = 0.145 \pm 0.04$, $t(82) = 4.10$, p = 1e-4. Although it would not affect our conclusions, we prefer to avoid post-hoc exclusions of participants and present analyses that include all the datasets that were collected.

8) Increase the replicability of your work by following guidelines in the Open Science Framework

- The Reviewer may have missed it, but we did commit to making our data and codes public in the sections "Data Availability" and "Code Availability" (line 867 onwards in the original manuscript; line 997 onwards in the revised manuscript), so anyone can replicate our experiments and analyses.

9) Provide more information on the construct validity of the emotion inductions. Typically in affective science, reading a one sentence description of an event is not considered sufficiently engaging for triggering an emotion per se. Instead, the scenario might be triggering demand effects.

- We are surprised by this comment. In real life examples, one short sentence, such as "Your remarks are bullshit" or "You've always been a prick" is sufficient to induce strong emotions, including anger. This is why researchers in the field have developed this emotional induction procedure using short vignettes. Let us remind the Reviewer that the induction does not just include text but also congruent music, which is universally recognized as a powerful emotional trigger. Note also that we are not the authors of the induction procedure used here; it precisely comes for the field of affective science. More precisely, our inspiration comes from the meta-analysis of Westermann et al. (1996), which reviewed a diverse range of induction methods and found only movies to be more efficient than a combination of text and music. Because of our intention to record pupil dilation, which would have been blurred during movie viewing, we chose luminance-corrected text combined with music. Importantly, our vignettes present short scenarios that are happening to *"you"*, which may be even more efficient than scenarios happening to characters in a movie. We also took guidance from Mayer et al. (1995) and Jallais & Gilet (2010) in our choice of using a combination of scenarios and music. For the choice of specific contents, we have borrowed many stimuli from Mayer et al. (1995) and Schulreich et al. (2014), as detailed in the Supplementary Material, where all text vignettes and music excerpts are listed.

- In addition, our induction procedure was validated by participants' self-reports, who declared the targeted emotion as their dominant affective reaction. We fully agree with the Reviewer that self-reports are susceptible to demand effects. This is why we further validated the induction procedure with physiological measures. All emotional stimuli triggered a strong pupil dilation and skin conductance response, compared to neutral stimuli. As by definition these responses of the autonomic nervous system cannot be voluntarily controlled, the physiological results demonstrate that emotions were actually experienced by participants (and not just reported). In addition, we validated that our happiness and sadness induction stimuli had opposite valence by showing opposite effects on smiling and frowning muscles (zygomaticus and corrugator electromyographic activity). We note that most studies implementing emotion induction do not provide such an exhaustive manipulation check. Thus, claiming that our stimuli cannot induce emotions is like stating a dogma in the face of overwhelming empirical evidence.

10) Provide far more detail on emotional carry-over effects given the within-subject nature of the manipulations.

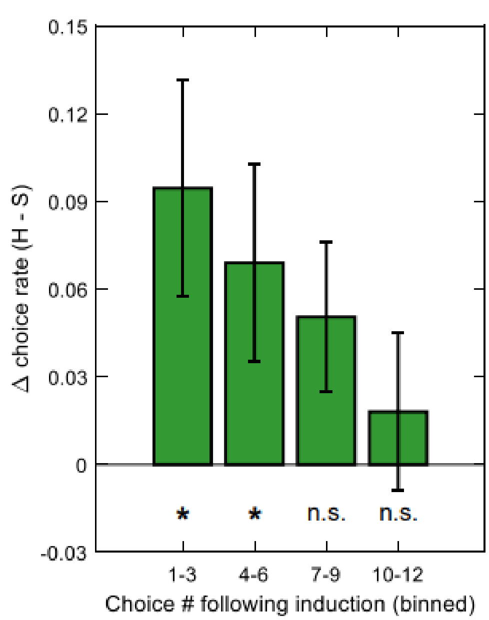
- The reviewer might have missed it, but we did report an analysis of the potential carry-over effects in the initial manuscript. In brief, there was no significant effect of previous induction, as is mentioned in the Results section (page 8, lines 192-194):

  "When regressing target ratings against ratings of the same emotion on the subsequent trial, we found no significant correlation for any of the categories (all $p > 0.20$), suggesting that the 10-second washout period between trials sufficed to extinguish the induced affective state."

- We also report in the supplementary information that the impact of happiness and sadness inductions vanishes across choice trials (hence, before the next induction). This is the reason why we reduced the number of choice trials in confirmatory studies, and the reason why we included trial number in our mood proxy, as explained in the Results section (page 19, lines 468-470):

  "Because the effect of mood was found to decline across the series of choices (see **Supplementary Information**), we also integrated post-induction trial number *N* to account for temporal dissipation of the affective state."

- Therefore, there was no carry-over effects on choices either, because the effect of a given induction tended to extinguish before the next induction was implemented. To better visualise this phenomenon, we plot mood effect as a function of choice trial number in the figure below.



- Thus, we conclude to an absence of carry-over effect. We are nonetheless happy to add any specific detail that the Reviewer would deem relevant.

11) Provide far more detail on and rational for the specific physiological measures used.

- Our behavioral testing facility (PRISME) is equipped with standard BIOPAC modules, which we used for recording electromyographic activity and skin conductance, and standard EyeTribe eyetrackers, which we used to record pupil dilation. These measures have been validated by the papers from which we borrowed the procedures:
    - E. J. Moody, D. N. McIntosh, L. J. Mann, K. R. Weisser, More than mere mimicry? The influence of emotion on rapid facial reactions to faces. Emotion. 7, 447–457 (2007).
    - R. Soussignan, M. Chadwick, L. Philip, L. Conty, G. Dezecache, J. Grèzes, Self-relevance appraisal of gaze direction and dynamic facial expressions: Effects on facial electromyographic and autonomic reactions. Emotion. 13, 330–337 (2013).
    - J. Künecke, A. Hildebrandt, G. Recio, W. Sommer, O. Wilhelm, Facial EMG responses to emotional expressions are related to emotion perception ability. PLoS One. 9 (2014), doi:10.1371/journal.pone.0084053.
    - C. W. Korn, D. R. Bach, A solid frame for the window on cognition: Modeling event-related pupil responses. J. Vis. 16, 28 (2016).
    - A. R. Mitz, R. V. Chacko, P. T. Putnam, P. H. Rudebeck, E. A. Murray, Using pupil size and heart rate to infer affective states during behavioral neurophysiology and neuropsychology experiments. J. Neurosci. Methods (2017), doi:10.1016/j.jneumeth.2017.01.004.
    - C. L. Stephens, I. C. Christie, B. H. Friedman, Autonomic specificity of basic emotions: evidence from pattern classification and cluster analysis. *Biol Psychol*. 84, 463–473 (2010).
    - S. D. Kreibig, Autonomic nervous system activity in emotion: a review. Biol Psychol. 84, 394–421 (2010).
    - E. H. Siegel, M. K. Sands, W. Van den Noortgate, P. Condon, Y. Chang, J. Dy, K. S. Quigley, L. F. Barrett, Emotion fingerprints or emotion populations? A meta-analytic investigation of autonomic features of emotion categories. *Psychol. Bull.* **144**, 343–393 (2018).

- Although we only used standard physiological measures, we describe them in details in dedicated Methods sections (pages 33-36, lines 875-958), with references to the papers that have validated our procedures:

"Data collection

In a subset of participants, sensors from a BIOPAC® MP 150 kit were placed for physiological recordings. On the left hand, one Ag-AgCl (signal) electrode was placed on the fingertip of the middle finger and another (baseline) on the back of the ring finger to measure galvanic skin responses. On the left thumb, a pulse oximeter was placed to obtain a photoplethysmogram for heart period measurement (not reported). Participants placed their left hand on the lab desk and were instructed to avoid moving it during the study. We further placed electrode pairs on the zygomaticus, corrugator, frontalis and depressor muscles on the left part of the face for electromyography. These facial muscles are respectively used for smiling, frowning, pouting, and raising one's eyebrows. Electrodes placed on the depressor had the tendency to loosen and signals from the frontalis showed considerable crosstalk from the corrugator's activity.

Since the zygomaticus and corrugator are commonly dichotomized as signalling positive and negative emotions (Larsen et al., 2003; Rymarczyk et al., 2011), we decided to restrict recordings of facial musculature to those muscles after study 1. Signals were collected at a 1000Hz sampling rate using BIOPAC's *AcqKnowledge®* software.

Once all sensors were placed, participants were seated comfortably at approximately 60 cm from a large computer screen (HP S230tm, 50.9 × 28.6 cm active area). A headrest was placed against the back of the head to reduce movement for studies in which pupil size was recorded. An infrared eye-tracking device from The Eye Tribe® (sampling rate: 60Hz) was positioned under the computer screen and centred on the participant's eyes. Pupil dilation responses were measured in all cases, but tracking gaze position requires a short calibration during which participants follow a moving dot with their eyes, which was only performed in confirmatory studies. For a full overview of the different physiological recordings per study, see **Figure S1e**.

The physiological recordings common to exploratory and confirmatory studies consisted of pupillometry, electrodermal activity, pulse oximetry, and electromyographic (EMG) activity of the zygomaticus, and corrugator muscles. Two full datasets were lost (one saving error and one early interruption), and three EMG datasets were discarded after quality check (too many artefacts), leaving 69 participants for pupil size and skin conductance, and 66 participants for zygomaticus and corrugator EMG activity.

Data preprocessing

Below we describe the preprocessing steps for each of the physiological signals. Data filtering and smoothing was done using functions from Fieldtrip (Oostenveld et al., 2011; see http://fieldtriptoolbox.org).

Skin conductance time series were consecutively bandpass-filtered between 1/40Hz and 1Hz (4^{th}-order Butterworth filter), smoothed with a 1s boxcar kernel, down-sampled to 10Hz, and epoched to the full duration of the induction (10 seconds). A few participants moved their hand, which caused artefacts in the electrodermal signals. All signals measured during artefactual inductions (0.8±0.4%) were visually detected and removed. The remaining time series were standardized across inductions and baseline-corrected by subtracting the first data point after induction started.

EMG time series of the zygomaticus (smiling) and corrugator (frowning) were high-pass filtered at 25Hz. Line noise was filtered out with a 48-52Hz stopband filter, after which the signal was rectified by taking the analytic envelope. The signal was then epoched to the induction duration and visually inspected. Inductions with large, prolonged artefacts (1.7±0.4%) were rejected. The signal was then down-sampled to 50Hz and transformed by taking its natural logarithm, so that a normal distribution of the data was approached. Remaining outliers were automatically detected if their value was more than 3 standard deviations above the mean; these samples were removed and the signal was interpolated. Then, the signal was smoothed with a 2s boxcar kernel and standardized across all inductions, separately for each muscle. Finally, the signals were baseline-corrected.

Eye-tracking time series were preprocessed following the pipeline described in Wiehler et al. (2022). Pupil diameter was estimated from eye-tracking data and corrected for the estimated distance between eyes and screen (although headrest minimized changes in this distance). Samples that were not within the median±5 standard deviations of the signal were discarded as outliers. Eye blinks were detected and labelled by the eye-tracker, along with the 6 samples (100ms) preceding and following the blink. Signal samples containing outliers, blinks, or absence of a detected pupil were removed and interpolated but flagged for the analysis (see below). Next, a bandpass filter between 1/128Hz and 1Hz was applied and the signal was smoothed with a 0.5s boxcar kernel. The pupil diameter was averaged between the two eyes and the resulting signal was standardized. Next, the signal was epoched to the induction duration and baseline-corrected.

Inductions with more than 2/3 of samples being originally flagged as artefacts (4.1±1.3%) were excluded from analyses.

Statistical analysis

Significant clusters in the time window corresponding to induction duration were detected using the VBA toolbox to correct for multiple comparisons (i.e., multiple time points). The correction applied here controls for family-wise error (FWE) based on random field theory, given the estimated temporal dependency of successive samples in the autocorrelated signal. This procedure was used to detect time clusters showing a difference between emotional and neutral inductions, or between happiness and sadness inductions. For assessing the links with ratings and choices, we needed to reduce the dimensionality of physiological responses to one estimate per induction. For the pupil dilation and EMG responses, this was done by taking the median of the signal over the entire 0-10s induction window, following on common practice (e.g., Bradley et al., 2008; Mathôt et al., 2018). The amplitude of the skin conductance response was estimated using PsPM (Bach et al., 2013, 2018), which first extracts a canonical response function and then uses it to deconvolve the skin conductance signal by fitting a general linear model. For each physiological response, the estimates were corrected for the induction and session numbers, to account for temporal drifts and changes in recording quality due to sensor readjustment during breaks. From the resulting corrected response estimates, we extracted valence and arousal, the two dimensions onto which emotions are classically mapped (Henderson et al., 2018; Lang et al., 1998; Posner et al., 2005; Russell, 2003). The proxy for valence was construed as a net facial expression signal (standardized difference between the zygomaticus and corrugator responses). The proxy for arousal was construed as a mean autonomic signal (average of pupil dilation and skin conductance response)."

- In sum, we do not see which important information might be missing, but we are happy to provide any detail if the Reviewer makes a specific request.

Supplementary Info

1100-1105 The Open Science Framework recommends that all researchers pre-register their hypotheses and analytic plans including plans for exclusion criteria in order to avoid opportunistic data analysis. Even if one has not pre-registered, it is generally considered good practice to use the same exclusion criteria across studies rather than rejecting candidates based on certain scale scores in one study and then including candidates with similar scale scores in a different study (as the researchers have done here). It is very important to re-analyze the data using consistent application of exclusion criteria across studies.

- Again (see our responses to points 7 and 8 above), this is a misunderstanding. We have not applied any post-hoc exclusion criteria, meaning that we have included in our dataset all the participants who were tested. What we did is setting more inclusive criteria for the confirmatory study relative to the exploratory study, such that more volunteers could be tested. What the Reviewer is asking us to do (excluding participants of confirmatory studies based on criteria set for exploratory studies) is precisely what should not be done, if we are to follow good practice. Anyway, doing so would not change a thing to our conclusions, because the impact mood score on choice rate is highly significant, both in exploratory and confirmatory studies, and whether or not we exclude participants as the Reviewer suggests. To reiterate, applying the same inclusion criteria would have led us to not test 11 participants in our confirmatory studies, leaving a total of N = 83 participants. If we post-hoc exclude those 11 participants, the mood-related choice bias would be unchanged and still highly significant: $\beta_{mood} = 0.145 \pm 0.04$, $t(82) = 4.10$, $p = 1e\text{-}4$.

1105 The authors do not report analyses on personality traits because they found no statistical link with behaviors of interest. Presumably they are referring to outcome variables. But what about the hypothesized moderating effect of Openness from the BFI and of the VVIQ on susceptibility to the emotion inductions? Did the authors examine these moderators? Please report the results.

- Yes we did test the links between personality scores and subjective ratings of emotional experience. This is stated in Supplemntary Information:

  "We do not present any further analysis on personality traits because we found no statistical link with the behaviours of interest (i.e., subjective ratings and economic choices)."

  Just to satisfy the Reviewer's curiosity: the correlation between the Openness and VVIQ scores and the mean target ratings as far from significance level, even before apllying the correction for multiple testing ($R_{openness} = 0.11$, $p = 0.66$; $R_{VVIQ} = 0.02$, $p = 0.89$). As explained in Supplementary Information, these scales were not retained for the confirmatory studies, because they failed to predict any behaviour of interest.

1108 Given current trends in the field, sample sizes (ranging from N = 18 to N = 36) are unusually low. Moreover, the gender composition of the sample was sometimes always skewed toward females, sometimes highly so. It is hard to consider these samples representative.

- Please refer to our responses to comment 4 regarding sample size and to comment 5 regarding gender balance. In short, sample size is actually quite high (N=94) and the gender imbalance (59 females for 35 males) cannot change the conclusion because the impact of mood on choice was significant in both males and females. The fact that the same effect (mood impact on choice) was significant in every subsample of the N = 94 total does not make the result less robust but more robust. The Reviewer seems to fall for a reasoning fallacy. Under the null, it is much less likely to get four times a significant result in 4 sub-samples than to have one significant result across the total pooled sample.

1109 Please add standard deviations to the table. Per convention, it is crucial to assess the variation within the measure, not just the confidence estimates around the mean.

- What was reported was the standard error of the mean (SEM), which quantifies the reliability of the estimate, not just variance. Although we still think that SEM is more informative than standard deviation (SD), we realise that readers can still compute it with the number of participants. Thus, to satisfy the Reviewer's request, we have replaced SEM by SD in the table.

1110 In order for this work to be considered replicable, all stimuli should be uploaded and made freely available. Another approach would have been to use stimuli already used within the literature. Why are some stimuli adapted from existing literature but not others?

- We thank the Reviewer for this remark, as we must admit that our statement on data availability was not explicit enough about our stimuli being available online. They are freely downloadable, -(see folder 'Stimuli'), from our GitHub repository under the CC BY-NC 4.0 license (https://github.com/MBB-team/MoodChoicePhysiology2024). Anecdotally, researchers from labs at other universities have contacted us personally to request our music fragments, and are currently using them in their studies.

- The vignette stimuli had the same format as the ones used by Mayer et al. (1995). However, they provided only 8 vignettes per emotional category, and none for neutral induction. As we implemented many more inductions, we had to create new stimuli. Moreover, some of their vignettes were specific to the time in which they were conceived, others were somewhat culturally specific to the United States. Therefore, we made our selection from their vignettes based on cultural/temporal relevance and created the rest. The process of stimuli selection / creation, for both vignettes and music extracts, is described in the Methods (page 29, lines 733-753).

"During instructions, participants viewed a few vignettes for familiarisation with the emotion induction and rating procedures prior to the main experiment. The vignettes consisted of short, personally relevant sentences, such as (for happiness): "You leave for a holiday. Upon your arrival, your destination looks like paradise.", or (for sadness): "Your pet that you were very attached to just passed away." For the neutral induction, the vignettes were impersonal, encyclopaedic sentences adopted from Wikipedia, for example: "The novel is a literary genre that is essentially characterized by a fictional narrative." We initially collected 24 vignettes per category and had them rate by an independent pilot group of participants (see **Table S3 for** a full overview). Some of the vignettes were adapted from Mayer et al. (1995), but most of them were created because we needed many more for all our inductions.

The vignettes were paired with emotionally laden pieces of music in all but the neutral induction. The music extracts consisted of diverse fragments of classical music from Baroque to contemporary composers (notably including compositions for movies and video games). Classical music is a genre that can be associated with each of our intended emotion categories, which made it suitable for our purposes. We included only instrumental music because we did not want lyrics to interfere with the text contents of the vignettes. We avoided overly rhythmical pieces that could incite the participant to tap along, which could possibly interfere with the physiological recordings. Per emotion category, we initially collected 16 pieces (see **Table S2** for a full overview), some of which were adapted from Mayer et al. (1995) and Schulreich et al. (2014), others were created for the occasion. Each music extract had the sound peak amplitude normalized to -1dB and was introduced/concluded by a 1s fade in/out. We used Audacity® for sound editing."

- In Supplementary information, we provide a table with an exhaustive list, together with item-specific mean ratings.

"From all the emotion stimuli that we collected or created, only a subset was used in the decision-making experiments. To select the best, we had previously run a pilot study of 37 participants (24 females, mean age 28.3, SD = 7.6), who rated each of the stimuli. Text vignettes and music extracts were assessed separately: vignettes were presented on screen with no sound for 10 seconds; music was presented in clips of 25 seconds while the screen was black. After each stimulus, the participant answered the question "To which extent did you feel each of the following emotions?" by rating their experienced happiness, sadness, anger, and fear on a continuous scale from 0 ("Not at all") to

10 ("Maximally"). These ratings served to compute a specificity score for each stimulus as follows: *(target emotion) – mean(other emotions)*. In the supplementary tables below, all stimuli are listed by their specificity scores. Colours refer to the specificity for happiness (yellow), sadness (blue), anger (red), and fear (purple). Based on this criterion, we selected the most categorically specific vignettes and music for the choice studies.

We collected 16 pieces of music for each of the emotion inductions except the neutral one, where vignettes were presented without music. The stimuli are listed in **Table S2** below with their composer and the start time of the sound clips. The stimuli flagged with ([1]) are adapted from Mayer et al. (1995); the stimuli flagged with ([2]) are adapted from Schulreich et al. (2014). Pieces 3 and 4 from the sad music clips were no longer used after exploratory studies, because we had received anecdotal feedback from participants stating that this music did not pair well with all text vignettes. For confirmatory studies, we selected the next most specific pieces of music instead.

In addition to the music stimuli, we collected 24 vignettes for each of the five emotion inductions. For the sake of brevity, they are only listed in English in **Table S3**, though the original stimuli were in French (which is a gendered language, so we prepared both a male declination and a female declination of the vignette list). The stimuli flagged with (*) are adapted from Mayer et al. (1995). The neutral stimuli were adopted from Wikipedia. As there was no target emotion for these vignettes, they are ranked according to their average rating (fifth column), from lowest to highest."

<u>1139</u> It seems that the music selected to evoke happiness had far stronger effects on self-reported happiness than the did music selected to evoke anger on self-reported anger. How did the authors account for this imbalance in stimulus strength?

- This is a false impression: what is reported in Table S3 is the *specificity* of each stimulus, not the strength. As explained in Supplementary Information (see paragraphs copied in response to previous point), we used the specificity score (target rating minus mean of other ratings) to select our stimuli. As we had 1 positive emotion and 3 negative emotions, the specificity of happiness is by construction higher, even if the intensity of happiness is not. On the contrary, a stimulus triggering intense anger will also induce some degree of sadness and fear. The correlation of negative emotions, and the anti-correlation between positive and negative emotions, is a well-documented phenomenon. The lesser specificity of negative emotions, even when the intensity was no different than happiness, was repeatedly observed across our datasets (see results plotted in response to point 6). Note that it cannot be an issue when contrasting happiness to sadness, and we ended up doing in our confirmatory studies.

<u>1141</u> I'm unclear how the short sentences (e.g. ,"you buy a lotter ticket and you win 100 euros instantly") could evoke emotion. Typically emotion researchers use much longer passages.

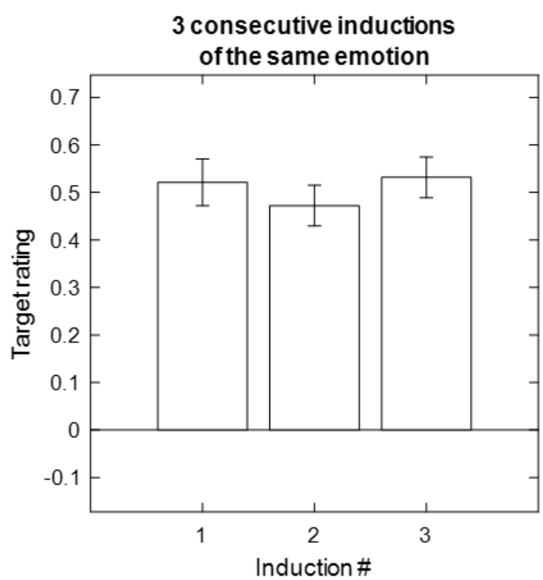
- Again, we have not invented this induction procedure, it was actually developed and validated by emotion researchers (Mayer et al., 1995). We provide substantial evidence, in both subjective reports and physiological responses, that the procedure does work. Perhaps what the Reviewer is missing is that participants are asked to imagine living the situation described. Also, the sentences were accompanied by music, which by itself is a powerful emotional trigger. The combination of short text with music was indeed deemed the second more powerful mood induction method after movies in a meta-analysis (Westermann et al., 1996).

1152 I'm unclear about how the procedure worked in Study 1. Did participants receive a within-subject (repeated exposure) to the same emotion induction three times? If so, it would be valuable to analyze the data accordingly, showing the impact of the repeated measure.

- Note that all our experiments implement within-participant manipulations, as we clearly explain right from the introduction that between-participant designs are one reason for the presence of inconsistent results in the literature on emotions and decisions (see response to point 2). The Reviewer is correct about the same induction being repeated three times and the necessity to test the effect of repetitions – this is precisely what we have done and reported. We initially assumed that the effect of inductions would accumulate, but there was in fact no effect of repetition (meaning that each of the 3 inductions produced a similar effect). So, for the next studies, we just alternated the different types of induction, following a pseudo-randomized order. This was explained in Supplementary Information (page 55, lines 1329-1334):

  "Note that exploratory studies can be divided into sub-studies 1 and 2, with a difference in the order of emotional inductions. In study 1 we assumed that emotions might ramp up, so we presented three consecutive inductions of the same emotion. In study 2 (and later in confirmatory studies), we alternated emotion categories (meaning that two consecutive inductions targeted different emotions), because we observed no build-up in subjective ratings across successive inductions."

- To test the effect of repetitions, we regressed target rating against induction number, and found no significant increase or decrease ($k_{ind\#} = 0.005 \pm 0.009$, $t(17) = 0.59$, $p = 0.562$). As there may be other ways for testing this effect, we just plot the target rating below for the Reviewer to check that it does not increase or decrease with repetition:



1160 I'm also unclear about the procedure from Study 2 onwards. What does it mean to alternate emotion categories? Also, why did you decide to "simplify" the design by "…only presenting choices in the gain frame potential loss being only included in the risky decisions, where the costly 1162 option was now defined as a probability to win the large reward versus a probability of losing a 1163 fixed amount (10€).” I’d like to see all of these decisions unpacked and justified based on results and revisions to hypotheses.

- Alternating emotion categories means that we imposed in the pseudo-randomisation that two successive inductions cannot be taken from the same category (happiness, sadness, anger, fear, neutral). This is explained in the text (see response to previous point) and illustrated in the figure summarizing the differences between designs (copied below).

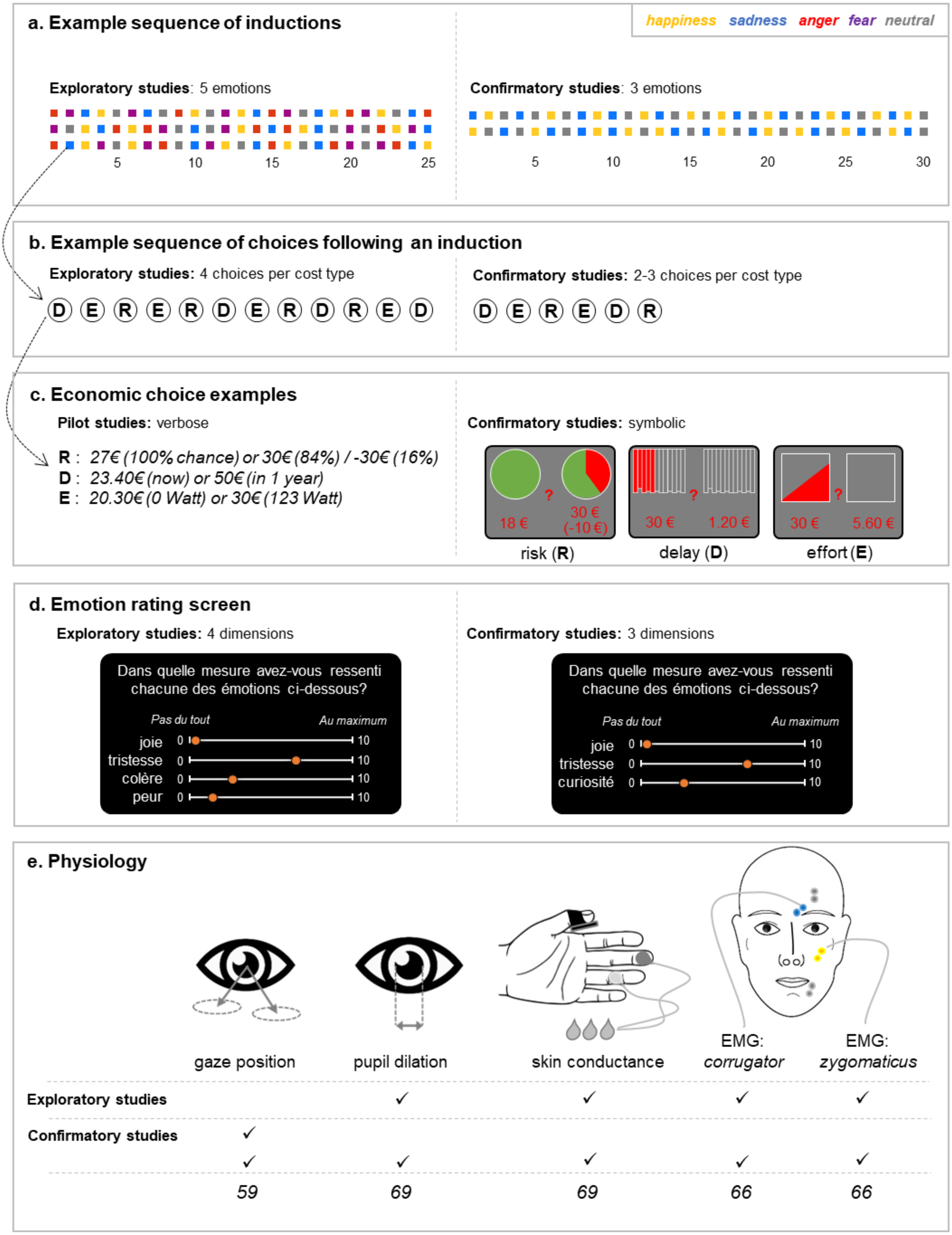
- We initially provided all details that differed between the experiments, for the sake of transparency, including conditions that were not even analysed (such as the loss condition). We realise after reading Reviewer 2’s comments this was too much to take. In the revised manuscript, we simplified our description and only provide details that matter for the data that were in fine analysed. This includes all details of the experimental conditions that were retained for confirmatory studies. Note that confirmatory studies follow the exact same design, the difference between sub-studies 3 and 4 being the collection of physiological responses. The description (Supplementary Information, page 55, lines 1325-1346) now reads as follows:

  “Data were collected across two sets of studies with variations at both the levels of emotion inductions and of economic choices.

  In exploratory studies, we induced four emotions (happiness, sadness, anger and fear), in addition to the neutral induction. Indifference points for the economic choices were determined by a staircase calibration procedure. Note that exploratory studies can be divided into sub-studies 1 and 2, with a difference in the order of emotional inductions. In study 1 we assumed that emotions might ramp up, so we presented three consecutive inductions of the same emotion. In study 2 (and later in confirmatory studies), we alternated emotion categories (meaning that two consecutive inductions targeted different emotions), because we observed no build-up in subjective ratings across successive inductions.

  In confirmatory studies, we only induced happiness, sadness, omitting anger and fear, which had no influence on choices (see below). For comparison, we introduced a curiosity rating in relation to the encyclopaedic text vignettes used in the neutral induction. Another difference is that costs were presented symbolically rather than numerically, to avoid reading and thereby facilitate gaze tracking, which was only introduced in confirmatory studies. Yet another difference is that choice calibration procedure involved an online trial generation algorithm which was more efficient in the estimation of indifference points (as demonstrated in Heerema et al., 2023). A last difference was that the number of choices following each emotion induction was reduced, because we observed a temporal decay in mood effects on decisions. Note that confirmatory studies can be divided into sub-studies 3 and 4, the difference between that physiological responses were recorded in study 4 but not study 3.

  An overview of these design differences is summarized in **Figure S1**.



a. Example sequence of inductions
happiness sadness anger fear neutral
Exploratory studies: 5 emotions
5 10 15 20 25
Confirmatory studies: 3 emotions
5 10 15 20 25 30
b. Example sequence of choices following an induction
Exploratory studies: 4 choices per cost type
D E R E R D E R D R E D
Confirmatory studies: 2-3 choices per cost type
D E R E D R
c. Economic choice examples
Pilot studies: verbose
R : 27€ (100% chance) or 30€ (84%) / -30€ (16%)
D : 23.40€ (now) or 50€ (in 1 year)
E : 20.30€ (0 Watt) or 30€ (123 Watt)
Confirmatory studies: symbolic
18 € ? 30 € (-10 €)
30 € ? 1.20 €
30 € ? 5.60 €
risk (R) delay (D) effort (E)
d. Emotion rating screen
Exploratory studies: 4 dimensions
Dans quelle mesure avez-vous ressenti chacune des émotions ci-dessous?
Pas du tout Au maximum
joie 0 10
tristesse 0 10
colère 0 10
peur 0 10
Confirmatory studies: 3 dimensions
Dans quelle mesure avez-vous ressenti chacune des émotions ci-dessous?
Pas du tout Au maximum
joie 0 10
tristesse 0 10
curiosité 0 10
e. Physiology
gaze position
pupil dilation
skin conductance
EMG: corrugator
EMG: zygomaticus
Exploratory studies ✓ ✓ ✓ ✓
Confirmatory studies ✓
✓ ✓ ✓ ✓ ✓
59 69 69 66 66

Figure S1: Overview of experimental designs across the 4 studies.

a. Example sequence of emotion inductions. Text vignettes with congruent music were used to induce happiness (yellow), sadness (blue), anger (red), fear (purple), or a neutral state (grey). The experiments were divided in sessions (rows) of consecutive inductions (squares), divided by short breaks. Anger and fear were abandoned in confirmatory studies because they had no impact on choices. **b.** Example sequence of economic choices. Each induction was followed by a battery of choices (circles) involving monetary rewards with either delay (D), risk (R), or effort (E). The number of choices was reduced in confirmatory studies because we observed a temporal decay in the impact of emotional inductions.
c. Example choice trials. In exploratory studies (left), choice options were presented textually, whereas in confirmatory studies (right), choices were presented symbolically (with colours adjusted to the luminance of the grey background). The change was made because symbolic presentation facilitated eye-traking, which was introduced to assess the distribution of attention between the two options.
d. Subjective rating. After the choice battery, participants rated their experienced emotions on visual analog scales, shown here in their original French version for exploratory (left) and confirmatory studies (right), in which curiosity was added as a control dimension. Cursors automatically followed the mouse, so that participants just had to click on the scale to confirm their rating.
e. Physiological measurements. The drawing of the left hand displays the placement of the pulse oxymeter on the thumb and electrodes for galvanic skin response measurements on the palm side of the middle finger (signal) and on the back side of the ring finger (reference). The drawing of the face displays the placement of electrodes for electromyography of four facial muscles. Per recorded signal, the total number of datasets included in the analyses are listed.

1178 Figure S1 implies that all participants received multiple different emotion inductions. Is that correct? How was carryover from one emotion to the next handled? Given that there were five different emotions, why were there only four different orders in S1 and only three different orders on S2? Why not fully randomize?

- Yes this is correct. We are surprised by the Reviewer doubting that the comparison is done within participants, as this is stated in multiple instances. Right from the introduction, we explain that we used a within-participant design because we assume that between-participant designs might be a reason why contradictory findings have been reported in the literature.

- As described in the methods, the order of emotions was fully randomized, under the constraint that two successive inductions should target different emotions. This is repeated in the legend of Figure 1:

  "**a.** Example of a 60-induction sequence, in which a series of 3 inductions presented each of the 3 emotions, following a pseudo-random order that avoided repeating the same emotion on two consecutive inductions".

- Note that the order of inductions illustrated in Figure S1 (copied above) is only an example sequence, with different rows representing within-experiment blocks separated by a short break.

1178 Figure S1 is extremely hard to follow.

- We acknowledge that there was probably too much information in this figure. Yet there is a tension between the request for additional details and the request for readability of procedures. We intended to reach a better compromise by eliminating irrelevant details, as explained in response to previous points above (please see the new Figure S1).

1197 Why was a pulse oximeter used? Which hypotheses were advanced?

- A pulse oximeter was used to monitor heart rate. We mention this for the sake of transparency, but it plays no role in our demonstration. In exploratory studies, we plugged the different components of our Biopac systems to test which would be reliable enough to follow autonomic arousal. It appeared that the pulse oximeter signal was not reliable, so we simply dropped it for our confirmatory studies.

1216 Why does Figure S3a report only recordings for anger and fear? What about the other emotions?

- The other emotions were shown in Figure 2 of the main paper. Since the signals are z-scored and baseline-corrected, effect sizes can be compared between figures. Nevertheless, we reorganized the figures so the physiological responses to the 4 emotions are reported in the same figure (copied below).

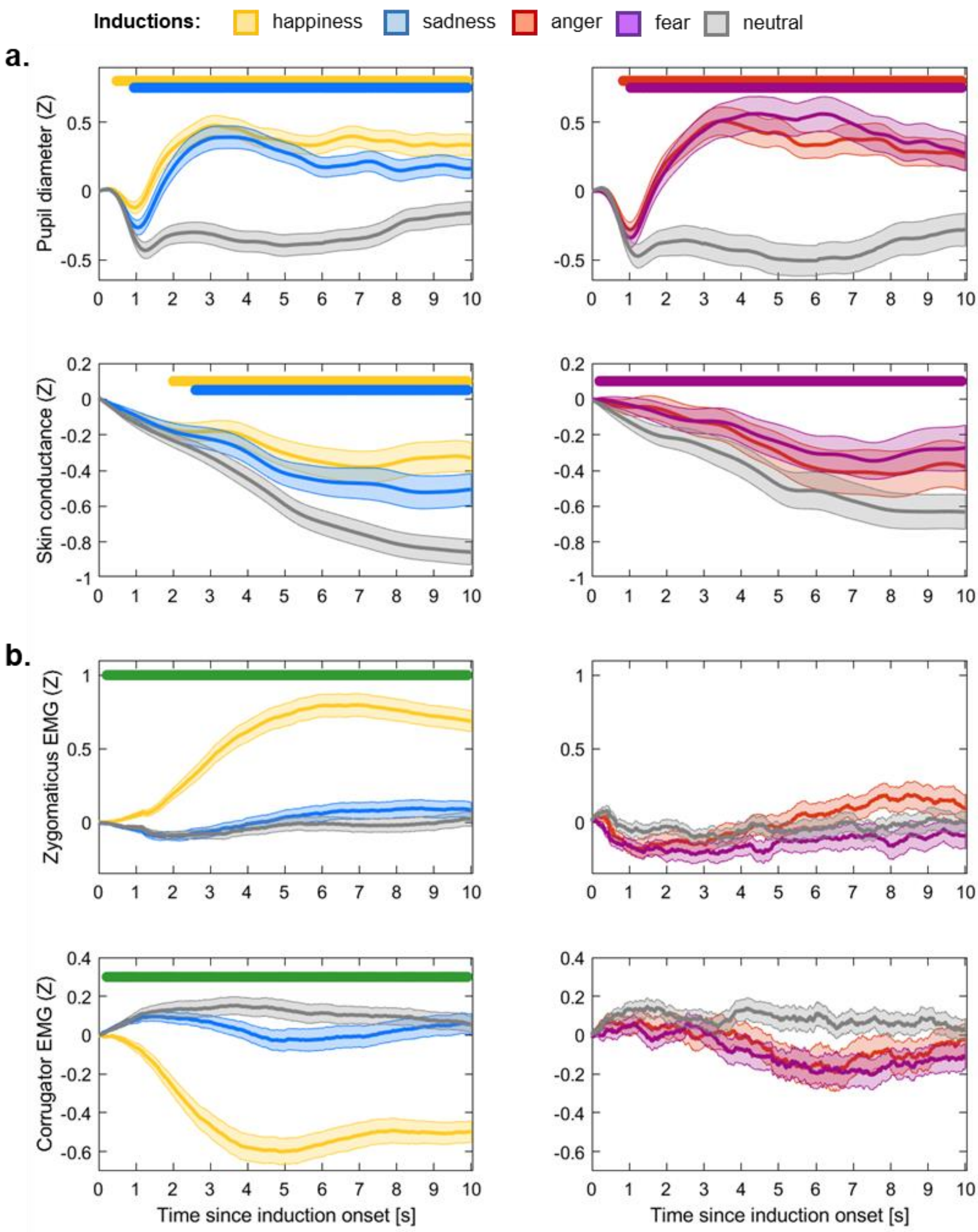


Figure 3. Validation of emotion induction by physiological responses.

Physiological signals of arousal (**a.**, pupil diameter and skin conductance) and physiological signals of valence (**b.**, zygomaticus and corrugator EMG activity) were measured in a subset of participants after happiness and sadness inductions (N = 69, left column) and after anger and fear inductions (N = 35, right column). Time-resolved baseline-corrected responses are shown over the 10-second duration of the induction, locked to text/music onset. Solid lines and shaded areas represent mean responses and their standard errors. Time clusters with significant differences between inductions, after correction for multiple comparisons using Random Field Theory, are denoted by horizontal lines for both arousal measures (yellow: happiness versus neutral, blue: sadness versus neutral) and valence measures (green: happiness versus sadness).

1239 "It was expected that the effects of our happiness and sadness inductions would not last long and return shortly to baseline." Why not expect this of the other emotions as well?

- We agree with the Reviewer: we should have named the four emotions in this sentence (which has been removed when reorganizing the manuscript).

1250 The figures in panel a and b are under-reported. Please explain the Y axes and title the figure with the main point.

- Figures have been reorganised to address all the concerns raised. We have checked that legends provide all necessary information. We apologise if there is still some missing detail, and obviously we would incorporate it on request.

1259 The section on model comparison would benefit from significant tightening of the rationale and steps taken. Sentences like this one are difficult to understand: "Starting from the base discount functions described in the main text (with one parameter per function), we either did or did not add a weight on reward, and costs either had a fixed power parameter (1 for delay and risk, 2 for effort) or a free one to be estimated through model fitting."

- We have amended our description of computational modelling, so readers unfamiliar with this approach can get a more intuitive sense of the different equations. The incriminated sentence has been simplified. Note that the model comparison is exactly the same (with eventually the same winning model) as the one implemented in our previous paper (Heerema et al., 2023). Also, the model is depicted in the results section (page 15-16, lines 342-399), with an explanation of how the different functions and parameters works:

  "We used computational modelling to better capture the role of mood in decision-making and that of the other factors (costs and rewards). We first identified the model that best explained choices independently of mood inductions, and then complemented the winning model by integrating trial-wise mood scores. The equations listed below correspond to the winning model of a Bayesian comparison conducted in another study employing the same choice task (Heerema et al., *2023*) and replicated with the present dataset (see **Methods**).

  Choices were modelled with a standard softmax decision function (**Equation 1**) that expresses the selection probability of each option as a sigmoidal transform of decision value *DV* (difference between option values), weighted by an inverse choice temperature β_1 and added to a choice bias β_0. In the equation below, we use a single generic notation for simplicity, even if the choice bias and choice temperature parameters (β_0 and β_1) are actually specific to each cost type (see **Methods** for cost-specific notations).

$$P(costly) = \frac{1}{1+exp(-\beta_0-\beta_1 \cdot DV)}$$

In this decision function, the decision value *DV* is defined as the value difference between the costly and uncostly options, i.e. *V(costly) – V(uncostly)*. the weight and bias parameters (β_1 and β_0) respectively adjusts the slope and the intercept of the sigmoid curve. The slope β_1 captures the consistency of choices (how strictly they follow the decision value), by opposition to their stochasticity (sometimes called temperature). At the extremes, a step function ($\beta_1 = \infty$) would be totally deterministic and a flat function (β_1 = 0) totally random. The intercept β_0 captures the choice bias, i.e. the inclination to choose one option or the other when the decision value is null (*DV=0*). Here, higher β_0 means greater propensity to choose the costly option.

The subjective option values, *V(costly)* and *V(uncostly)*, are modelled using three different discount functions, depending on the type of cost (risk, delay or effort). Discount functions integrate objective rewards with the costs weighted by a discount factor k_R, k_D, or k_E (**Equations 2-4**). The curvatures of discount functions are controlled by power parameters γ_R, γ_D, and γ_E.

$$V_R = Rew \cdot P - k_R \cdot L \cdot (1-P)^{\gamma_R} \qquad (2)$$

where P is the probability of winning the reward Rew and L the amount that can be lost by taking the risky option.

$$V_D = Rew \cdot exp(-k_D \cdot D^{\gamma_D}) \text{ where } D \text{ is the delay of reward delivery.} \qquad (3)$$

$V_E = Rew - k_{PE} \cdot E^{\gamma_E}$ where E is the effort level. *(4)*

Note that the same discount function is used to compute the value of costly options (for which the reward *Rew* is 30€ and the cost higher than 0), and the value of uncostly options (for which *Rew* varies and cost is 0, such that *V = Rew*).

To check that the set of discount and decision functions described above were best capturing the new choice dataset, we reconducted a Bayesian model comparison. The full space contained 36 different models with systematically varying parametrizations, described in detail in the **Methods** and in **Supplementary Table 2**. We used the Variational Bayesian Analysis toolbox (Daunizeau et al., 2014) to invert the models and compare their log evidence, which offers a tradeoff between accuracy (goodness of fit) and complexity (degrees of freedom). The winning model was submitted to a recovery analysis, which showed good identifiability of all parameters (see **Methods**).

The fitted discount functions were quasilinear for risk, convex for delay and concave for effort (see **Figure 5a**). Thus, subjective option values were always positive for delayed rewards, reflecting the fact that they are always better than no reward at all. In contrast, subjective option values of risky and effortful rewards could be negative, accounting for the possibility that participants would rather give up money than take too great risks or make too demanding efforts.

Psychometric curves (**Figure 5b**) showed that costly choice rates were shifted upwards following happiness induction and downwards after sadness induction, relative to neutral induction. This confirms our theory that mood exerts an additive bias favouring the costly options, on top of the attributes (rewards and costs) that are integrated in the computation of option values. Indeed, mood score significantly explained choice residuals (**Figure 5c**), which represent the variance across trials that was unaccounted for by the model (R = 0.041±0.01, *t*(92)

= 4.86, p = 4.9E-6). We therefore updated our model to include mood as a modulator of the choice bias in the softmax function, with parameter β_M shared across cost types.

$$P(costly) = \frac{1}{1+exp(-\beta_1 \cdot DV - (\beta_0 + \beta_M \cdot Mood))} \qquad (5)$$

In this augmented softmax function, higher β_M means that positive and negative mood have a greater impact on the choice bias (i.e., irrespective of option values, happiness inclines choices towards costly options and sadness towards uncostly options)."

1276 It would be helpful to add discussion about the limits of retrospective introspection about experienced emotions in specific studies.

- We agree that introspection is limited, and retrospective introspection even more limited. This is why we included objective measures of physiological responses recorded when emotions are experienced (not retrospectively). These considerations are mentioned both in the introduction (page 2-3, lines 55-61)

  "However, subjective ratings have been criticized on the grounds that they are bounded by the quality of insight and susceptible to a potential demand effect (when participants can guess what experimenters expect them to report). The first aim of the present study was to circumvent these limitations by collecting objective physiological markers (pupil diameter and skin conductance) of autonomic arousal, which by definition cannot be voluntarily controlled and therefore remain immune to demand effects (Bach et al., 2010; Bradley et al., 2008; Figner & Murphy, 2011; Joshi & Gold, 2019)."

- And in the discussion (page 25, lines 637-650):

  "One potential limitation of our study is that the within-subject design makes the induction explicit: participants easily understand that they are supposed to alternate between happiness, neutral and sadness states, depending on the vignette. This could make the manipulation vulnerable to demand effects, i.e., participants could simply comply with the experimenter's expectations and report being happy/sad after obvious happiness/sadness inductions. Yet this would not explain variations in the intensity of the emotional state within happiness and sadness categories. Also, although it is easy to guess what type of rating is expected, it is not so easy to guess what effect on choices might be expected (even experts disagree, depending on which papers they have read). One could argue that taking less risk after a fear induction is an obvious expected behaviour, but precisely, participants did not conform to that expectation. Moreover, participants who retrospectively reported in a debriefing questionnaire that they felt very emotional, or understood the purpose of the study, or noticed the effects of inductions on their choices, did not exhibit a greater choice bias than those who declared not having a clue. However, retrospective introspection is notoriously limited, so self-reports alone may not be taken as conclusive evidence."

Reviewer #3 (Remarks to the Author):

REVIEWER EXPERTISE: computational model decision making

In this study, the authors investigate the impact of mood on economic decision-making in humans. Across 4 experiments and 3 settings with different types of cost (risk, delay, effort), results suggest that economic choices, response times, and attention are biased by mood-related state. After inducing happiness, sadness, anger and fear, the authors show that happiness and sadness result in (opposite) changes in choices towards the costly option, but not anger nor fear. This bias was elegantly captured by a choice-based model which implements the mood-related state as an option bias in the selection (softmax) rule. The work has many positive features, including a very thorough design with many measurements, well-justified analyses and quantitative comparisons of several models. Overall, the paper is very well-written and the findings add a new perspective to the field. Therefore, I only have a few comments and suggestions for the authors, but in general I am very positive about their interesting work.

- ➢ We thank the Reviewer for the positive evaluation of our work. We believe that, indeed, the suggestions below helped us to improve the manuscript.

1) The paragraph about attention (lines 413-427) is a bit misleading and will benefit from more detailed analyses. The authors state that "a predisposition to selecting either choice option should result in attention being preferentially directed towards that option immediately following its presentation", which would induce a gaze bias after stimulus onset. Yet, the results presented here only focus on gaze patterns following choice onset, which does not directly address the hypothesis and, in my opinion, do not justify the "signature of a prior intention" mentioned in the discussion. From context, and judging from the authors' interpretation, it is possible that the reported results where following the stimulus onset, not the choice per se, as "choice onset" is an ambiguous formulation. If this is not the case, could the authors comment on the gaze patterns in the "early phase of decision making", i.e., before the choice?

- ➢ We fully agree with the reasoning: the Reviewer is correct that a predisposition should result in a gaze bias observable just after stimulus onset. This is actually what we observed, but apparently what we failed to express. Indeed, our phrasing ('choice onset') was ambiguous. What we meant is 'time when choice is started' (i.e., when options appear on screen), but it can be understood as 'time when choice is made'. To avoid any misunderstanding, we have replaced 'choice onset' by 'option display', so it is clear that the gaze bias was observed in the early phase of decision making, in accordance with the interpretation advanced in the discussion: depending on their mood, participants formed a predisposition to selecting one or the other option.

- ➢ We have also added analyses that strengthen our interpretation. Regressions against choice rate and choice RT showed that early attention predicted the likelihood of choosing the costly option and how quickly it was chosen. These new results are mentioned in the main text (page 21, lines 520-528) and illustrated in the new panels of Figure 5 copied below.

"During the first second following option display, more attention was paid to the costly option overall, and even more so after happiness induction relative to sadness induction (**Figure 7a** and **7b**). This pattern was suggestive of a modulation of attention by mood, which was indeed significant in a direct regression of the time spent looking at the costly option (within the first second) against mood score (b_{Mood} = 0.02, $t(58)$ = 2.50, p = 0.015). In turn, the proportion of time spent during the first second was associated with a higher choice rate (b_{gaze} = 0.497±0.13, t(57) = 3.79, p = 4e-4) and a shorter RT (b_{gaze} = -0.322±0.08, t(58) = -4.09, p = 1e-4) when the costly option was eventually selected. Thus, an early attentional bias in favour of the costly option made the selection of this option both more likely and more rapid (**Figure 7c** and **7d**)."

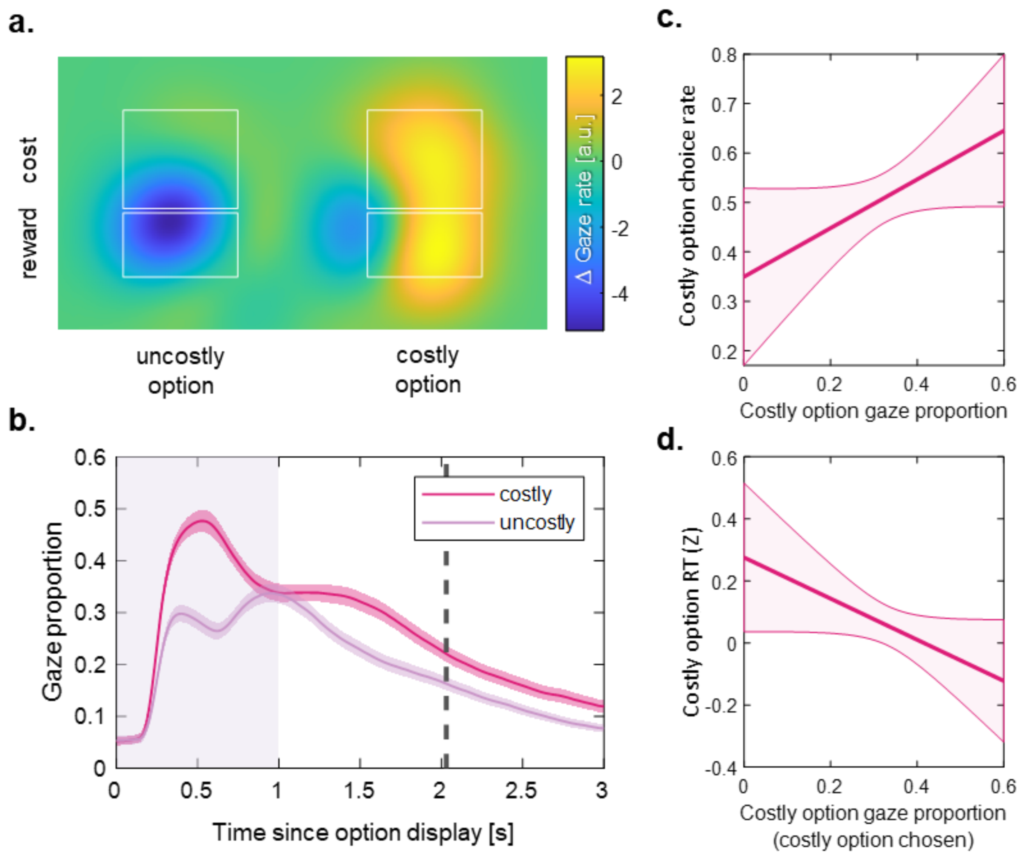


Figure 7. Effects of mood induction on visual attention

a. Difference in gaze distribution between happiness and sadness inductions during the first second following option display (shaded window in the time course below). In the heatmap, screen pixels receiving more gaze after sadness and happiness inductions appear in blue and yellow, respectively. Note that half the trials have been flipped such that the costly option was always on the right in this analysis, although it was not the case in reality (because the side of costly option display was counterbalanced across trials).

b. Time course of visual attention paid to the two options, within the 0-3s window following option display. Gaze rate is obtained at each time point by dividing the number of samples inside each option frame by the number of trials. It declines with time as decisions are already made in a growing number of trials (median RT is shown by the dotted vertical line). The two curves do not add up to 100% because gaze can be located outside the two option frames. The solid line and shaded area represent the mean and its standard error across participants.

c. Costly option choice rate plotted as a function of costly option gaze proportion (during the first second following option display).

d. Choice RT plotted as a function of costly option gaze proportion (during the first second following option display), specifically for trials in which the costly option was selected.
Both in panels (c) and (d), the solid lines represent linear regression fits and shaded areas the 95% confidence interval across trials and participants.

2) The authors find a mood-related bias in the choices, but also in the response times and gaze patterns. Yet, the model they chose to implement only captures the choice patterns and doesn't seem to be built to account for RT and attention. These effects could however be captured by sequential sampling models, such as the (attentional-)DDM (Krajbich et al, 2010). I do not think that including such a model in the analyses would drastically improve the paper in its current form, but this should be at least mentioned in the discussion. Relatedly, the authors choose to implement the mood component as an additive feature in the selection rule, but do not discuss any other implementation, nor whether the addition of this parameter might interact with others (e.g., beta_0).

- Capturing RT distribution with an aDDM informed by gaze pattern is indeed an excellent idea (note that Krajbich's paper was already cited in the Discussion). The reason why we have not included such model-based analysis is because it did not capture the pattern of RT results. As sadness biases gaze towards the uncostly option, aDDM predicts that this option should be chosen faster (compared to happiness), but this was not the case. We admit that we are still missing a mechanistic model that would link mood effects on gaze and RT. We are working on this, with the aim of providing a model that would globally account for the effects reported in our different papers, but this would go beyond the scope of this single manuscript. The mention of aDDM in the Discussion (page 25, lines 616-631) reads as follows:

  "The additive effect of mood was also reflected in the interaction (between emotional states and final decisions) observed on choice response time. Relative to uncostly actions, the decision to take the costly actions was faster when in line with the default option (in states of happiness) than when against the default option (in states of sadness). Moreover, the notion of mood as a prior preference for large-but-costly rewards is supported by the effects of emotional states on looking patterns. There is ample evidence that people look first at the option they choose in the end (Krajbich et al., 2010; Onuma et al., 2017), although the direction of causality is not clear. We found evidence indeed that, during the early phase of decision making, participants spent more time looking at the costly (versus uncostly) option in states of happiness (versus sadness). In turn, the attention paid to the costly option was associated with a higher chance that this option would be chosen and a shorter RT when this option was chosen, as would be predicted by the attentional drift-diffusion model (aDDM, Krajbich, 2019). Yet, the pattern of results observed here was not entirely compatible with aDDM, because the uncostly option was not chosen faster after sadness inductions, even if it was more looked at (relative to happiness inductions). Nevertheless, the early effect on gaze direction denotes a prior intention to take the action favoured by the affective state, before even seeing the specifics of choice options."

- Regarding the implementation of the mood component as an additive feature in the selection rule, it is not true that we do not discuss any other implementation. In the initial manuscript, we even provided a comparison with a model where mood was implemented as a multiplicative feature (modulating the weight on reward). The additive model came out as the most plausible with 100% exceedance probability), as explained in the Results (page 16, lines 400-403):

"We compared this model to an alternative that accounts for mood effects by modulating the weight on reward in discount functions. In a Bayesian model comparison, the additive variant in **Equation 5** was selected as the most plausible, with a 94.2% model frequency and 100% exceedance probability."

- Regarding possible interactions with other parameters, we had done a recovery analysis, also mentioned in the Results. We have now included this analysis in a sub-section of Supplementary Information (copied below). Briefly, we simulated datasets with parameters drawn from the observed distributions and fitted new parameters on these data. Among the parameters used for simulation, only β_{mood} explained the variance in the fitted β_{mood} parameter (see panel d in the figure below).

"We also performed a recovery analysis, inspired by the approach of Wilson and Collins (2019). Data were simulated using the winning computational model, with parameters drawn from posterior distributions obtained in our sample (N = 94). Simulated agents performed our task as designed in confirmatory studies, the sadness and happiness ratings being drawn from the observed mean distribution over all participants. The winning model was fitted to each simulated dataset, and the proportion of variance explained (R^2) was computed. Monte-Carlo simulations were conducted with different sample sizes, varying from 15 to 150 for the sake of coherence with the model-free power analysis above. We repeated this procedure 50 times, for a total of 41250 virtual agents (Figure S2c). To assess recoverability, all simulated parameters (after z-scoring across datasets) were regressed against the fitted β_M (Figure S2d). The regression weight of the simulated β_M was 0.87, while the absolute weights of all other parameter values were below 0.01. Thus, the fitted β_M parameter was essentially determined by the true β_M, with very little contamination of other parameters. We also examined how proportion of explained variance varied with sample size: it increased until reaching a plateau around 75% between N = 30 and N = 45, suggesting that including more participants would not improve the estimation of the β_M parameter."

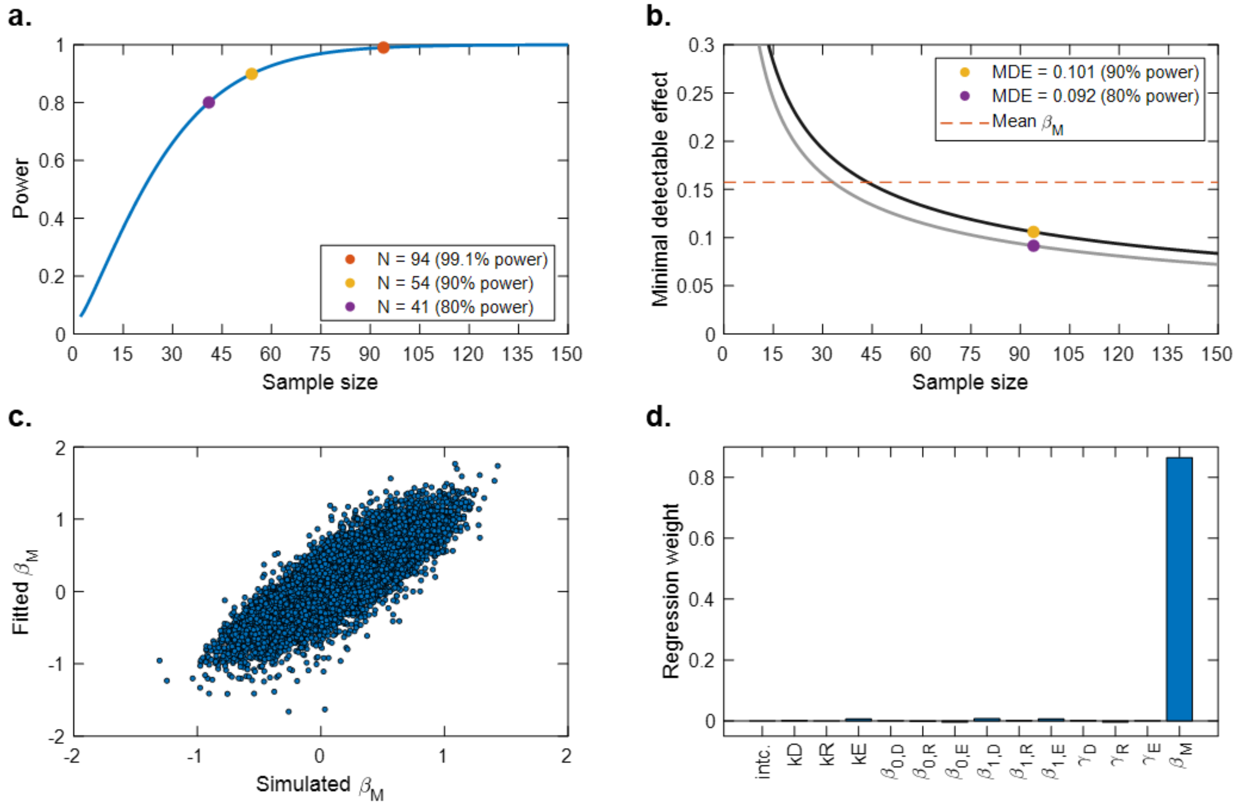


Figure S2: Power and recovery analyses.

a. Post-hoc power analysis. Power is plotted as a function of sample size, with dots comparing the power achieved with our sample size to conventional power levels (80 and 90%).

b. Sensitivity analysis. Minimal detectable effect (MDE) is plotted as a function of sample size. Dots compare the effect size observed in our sample ($\mu_{\beta M}$) to the MDE corresponding to conventional power levels (80 and 90%).

c. Recovery of β_M. Dots represent the β_M fitted to 41250 datasets simulated with various β_M parameters and sample sizes.

d. Regression of fitted β_M against all simulated parameters, across the 41250 simulated datasets. Bars show regression coefficients, i.e. the weight that each simulated parameter had in explaining the fitted parameter.

3) It is not clear why the authors chose to implement the weight on rewards as a multiplicative factor and not power parameter, such as in classical forms of Prospect Theory (in addition to their implementation of loss aversion and discount function curvature).

- This is a fair concern. The choice of a multiplicative factor dates back to our first study of mood effects on economic decisions (Vinckier et al., 2018). Power and weight parameters were both included in the factorial model space, but the winning model only included a weight parameter. Since then, we have retained this implementation with a multiplicative factor. As our argument is that we can replicate with happiness and sadness inductions the results obtained with mood manipulations, we prefer to keep the same model space as in our previous paper (Heerema et al., 2023). Note that our winning model, in both present and previous papers, did not include a variable weight on reward (k_{Rew} was fixed to 1).

- Nevertheless, for the Reviewer's information, we ran a direct model comparison between the winning model reported in the paper (with k_{Rew} = 1), and the suggested variant with a power parameter on reward. The fixed-weight model won the comparison with an exceedance probability of 91.4% (versus 8.6%) and a model frequency of 57% (versus 43%). We are aware that in prospect theory, the power parameter is deemed necessary to capture the curvature of the subjective expected value function. We suspect the reason why it is not needed for explaining our data is because it would only affect small rewards (as the big reward is fixed), which typically amounts to a few euros. The curvature of subjective value might only be observable at much higher amounts.

Minor points

4) line 283. The inclusion of the different parameters for the winning model could be briefly justified and explained in greater details for the non-expert reader (temperature and choice biases, weights, power parameters, discount factors).

- We thank the Reviewer for this excellent advice. We have completed our depiction of the winning model in the Results section (even if a fully detailed depiction can be found on the Methods). Note that, as we explain in this paragraph, the model is justified by the Bayesian comparison conducted in our previous study (Heerema et al., 2023) and replicated with the present dataset (see Supplementary Information). Here is the amended depiction of the winning model in the results section (pages 15-16, 342-399):

  "We used computational modelling to better capture the role of mood in decision-making and that of the other factors (costs and rewards). We first identified the model that best explained choices independently of mood inductions, and then complemented the winning model by integrating trial-wise mood scores. The equations listed below correspond to the winning model of a Bayesian comparison conducted in another study employing the same choice task (Heerema et al., *2023*) and replicated with the present dataset (see **Methods**).

  Choices were modelled with a standard softmax decision function (**Equation 1**) that expresses the selection probability of each option as a sigmoidal transform of decision value *DV* (difference between option values), weighted by an inverse choice temperature β_1 and added to a choice bias β_0. In the equation below, we use a single generic notation for simplicity, even if the choice bias and choice temperature parameters (β_0 and β_1) are actually specific to each cost type (see **Methods** for cost-specific notations).

$$P(costly) = \frac{1}{1+exp(-\beta_0-\beta_1 \cdot DV)} \qquad (1)$$

  In this decision function, the decision value *DV* is defined as the value difference between the costly and uncostly options, i.e. *V(costly) – V(uncostly)*. The weight and bias parameters (β_1 and β_0) respectively adjusts the slope and the intercept of the sigmoid curve. The slope β_1 captures the consistency of choices (how strictly they follow the decision value), by opposition to their stochasticity (sometimes called temperature). At the extremes, a step function (β_1 = ∞) would be totally deterministic and a flat function (β_1 = 0) totally random. The intercept β_0

captures the choice bias, i.e. the inclination to choose one option or the other when the decision value is null (*DV=0*). Here, higher β_0 means greater propensity to choose the costly option.

The subjective option values, *V(costly)* and *V(uncostly)*, are modelled using three different discount functions, depending on the type of cost (risk, delay or effort). Discount functions integrate objective rewards with the costs weighted by a discount factor k_R, k_D, or k_E (**Equations 2-4**). The curvatures of discount functions are controlled by power parameters γ_R, γ_D, and γ_E.

$$V_R = Rew \cdot P - k_R \cdot L \cdot (1 - P)^{\gamma_R} \qquad (2)$$

where P is the probability of winning the reward *Rew* and L the amount that can be lost by taking the risky option.

$$V_D = Rew \cdot exp(-k_D \cdot D^{\gamma_D}) \text{ where } D \text{ is the delay of reward delivery.} \qquad (3)$$

$$V_E = Rew - k_{PE} \cdot E^{\gamma_E} \text{ where } E \text{ is the effort level.} \qquad (4)$$

Note that the same discount function is used to compute the value of costly options (for which the reward *Rew* is 30€ and the cost higher than 0), and the value of uncostly options (for which *Rew* varies and cost is 0, such that *V = Rew*).

To check that the set of discount and decision functions described above were best capturing the new choice dataset, we reconducted a Bayesian model comparison. The full space contained 36 different models with systematically varying parametrizations, described in detail in the **Methods** and in **Supplementary Table 2**. We used the Variational Bayesian Analysis toolbox (Daunizeau et al., 2014) to invert the models and compare their log evidence, which offers a tradeoff between accuracy (goodness of fit) and complexity (degrees of freedom). The winning model was submitted to a recovery analysis, which showed good identifiability of all parameters (see **Methods**).

The fitted discount functions were quasilinear for risk, convex for delay and concave for effort (see **Figure 5a**). Thus, subjective option values were always positive for delayed rewards, reflecting the fact that they are always better than no reward at all. In contrast, subjective option values of risky and effortful rewards could be negative, accounting for the possibility that participants would rather give up money than take too great risks or make too demanding efforts.

Psychometric curves (**Figure 5b**) showed that costly choice rates were shifted upwards following happiness induction and downwards after sadness induction, relative to neutral induction. This confirms our theory that mood exerts an additive bias favouring the costly options, on top of the attributes (rewards and costs) that are integrated in the computation of option values. Indeed, mood score significantly explained choice residuals (**Figure 5c**), which represent the variance across trials that was unaccounted for by the model ($R = 0.041 \pm 0.01$, $t(92) = 4.86$, $p = 4.9E-6$). We therefore updated our model to include mood as a modulator of the choice bias in the softmax function, with parameter β_M shared across cost types.

$$P(costly) = \frac{1}{1+exp(-\beta_1 \cdot DV - (\beta_0 + \beta_M \cdot Mood))} \qquad (5)$$

In this augmented softmax function, higher β_M means that positive and negative mood have a greater impact on the choice bias (i.e., irrespective of option values, happiness inclines choices towards costly options and sadness towards uncostly options)."

5) line 370 or Figure 5b. The shape of the valence function seems a bit surprising. One could expect a linear/sigmoid, rather than step-like function. Could the authors comment on this, or explain why they were expecting such a shape?

- The shape is actually compatible with a sigmoid function, even if the transition is sharp. The sharp transition is due to the dichotomy of inductions (negative valence and mood score following sadness versus positive valence and mood score following happiness). If the Reviewer concern is why neutral mood score (after neutral induction) is associated with negative valence, then we fully admit this was not anticipated. Our guess is that participants tend to frown when reading difficult encyclopaedic texts, which generates some activity in the corrugator muscle.

6) Figure 2. I recommend to define H-N-S in the figure legend. Also, there might be a label mistake in the top right green panel (all inductions) for the x-axis.

- We thank the Reviewer for the suggestion. This figure has been entirely changed (see below). We have added specified the emotional dimension (Happiness, Sadness, Anger, Fear) denoted by the different letters in the figure legend.

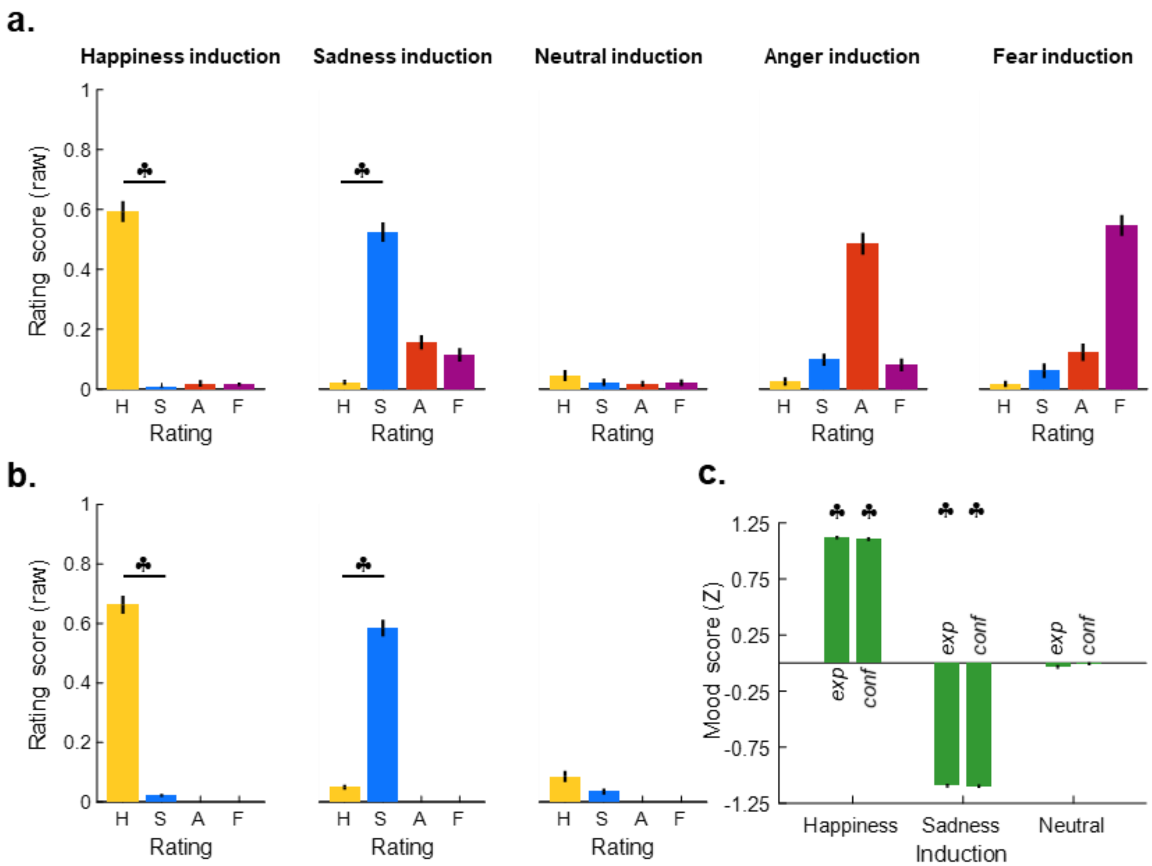


Figure 2. Validation of emotion induction by subjective ratings.

a. and **b.** Subjective ratings in the exploratory studies (top) and confirmatory studies (bottom). Each graph includes all the ratings made after the induction of a specific emotion. Each bar shows the rating of a specific emotional dimension (H = happiness, S = sadness, A = Anger, F = Fear). Statistical results are only shown for the main comparison (happiness vs. sadness ratings).

c. Mood scores (happiness minus sadness ratings). Bars show mood scores obtained in the exploratory (exp.) and confirmatory (conf.) studies, for each of the main emotional inductions (happiness, sadness and neutral inductions). Statistical tests compare mood scores to zero.

In all graphs, error bars represent the standard error of the mean. ♣ denotes p < 1E-20.

7) Figure 4b. It should be specified in the legend whether the measures are absolute or compared to the neutral condition.

- It is unclear to us how the measure could be relative (compared to the neutral condition). The graph in panel 4b (now 5b) describes choice rate, which varies between 0 and 100%, hitting 50% when decision value is 0 (indifference point). Only choices following happiness and sadness inductions are plotted, the neutral condition is omitted because it would be hardly visible between the two curves. If the question is whether we have subtracted the neutral condition, the answer is obviously no (otherwise the curve would be flat on the 50% horizontal line). We now mention in the legends (of panel 5b below) that the neutral condition is just omitted (not subtracted).

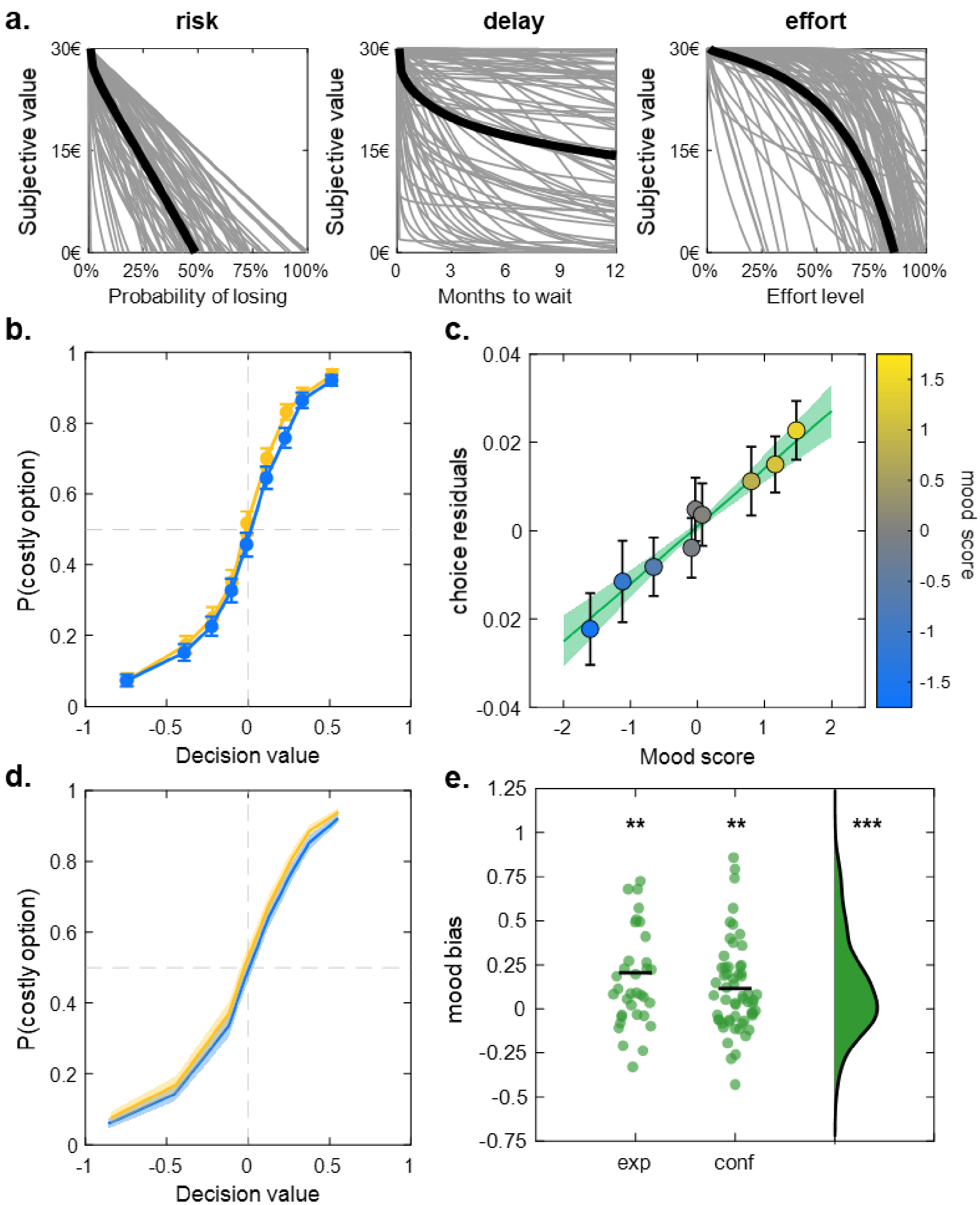


Figure 5. Model-based effects of mood induction on choice behaviour.

a. Discount functions for each of the cost types, obtained from fitting the winning model to the choice data. Each curve describes how the subjective value of the objective big reward (30€) diminishes with increasing costs (risk, delay or effort). Thin grey lines represent individual curves, the thick black line is the mean curve across all participants.

b. Observed psychometric curves. Dots represent costly choice rates following happiness (yellow) and sadness (blue) inductions, as a function of binned decision value (difference between costly and uncostly options), calculated using the discount functions of the winning model. The neutral condition has been removed because it would not be visible between the two curves.

c. Correlation between mood scores and choice residuals. Green line and shaded area display the mean regression fit and its standard error across participants. Choice residuals were obtained by subtracting the probability predicted by the model to the observed choice rate, and then binned according to sorted mood score. Colours of the dots illustrate mood score (same as X-axis) along a dimension going from sadness to happiness.

Both in panels (b) and (c), error bars represent standard errors of the mean.

d. Modelled psychometric curves. Modelled choice rates were obtained here with a softmax decision function that included a bias scaled to mood score. This mood-related choice bias captures the change in the intercept of the psychometric functions between happiness (yellow) and sadness (blue) inductions. Solid lines represent the means and shaded areas represent the standard error of the mean across participants.

e. Distribution of mood-related bias parameters obtained in the exploratory (Exp.) and confirmatory (Conf.) studies. Dots are individual participants. Horizontal lines show the mean across all participants (from both studies). Stars denote distribution density of fitted parameters that differ from the null (zero mood bias). ** $p < 0.01$, *** $p < 0.001$

Rebuttal letter to the Reviewers

Reviewer #1 (Remarks to the Author):

The authors replied to all my comments. I don't have further questions.
The manuscript add an important brick to the literature of how mood affects decision making.
If the editor agrees, I believe that the manuscript can be published in Communications Psychology.

We are glad to hear this and we thank the Reviewer for their valuable contributions.

Reviewer #2 (Remarks to the Author):

The revised manuscript has clarified most of my original questions.

We are glad to hear this and we thank the Reviewer for their valuable contributions.