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Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

13th May 24

Dear Professor Ein-Dor,

Thank you for your patience during the peer-review process. Your manuscript titled "The Biological Backbone of Attachment: An Integrated Theoretical, Neurobiological and Epigenetic Framework of Attachment Pattern Formation" has now been seen by 3 reviewers, and I include their comments at the end of this message.

The reviewers are in principle enthusiastic about your work. However, they also mention a number of concerns. We are very interested in the possibility of publishing your manuscript in Communications Psychology, but would like to consider your response to these concerns in the form of a revised manuscript before we make a decision on publication.

In detail, we ask you to prioritize the following issues in revision:

1) Strength of the argument/persuasiveness. The Reviewers highlight some issue with the "epigenetics" account; editorially, we agree that this is the most speculative section. We ask for a very careful revision, avoiding any unsubstantiated inferences and flagging tentative proposals as such. Relatedly, we ask you to follow Reviewer #2's advice and avoid presenting the framework as a theory and we strongly endorse removing the UnBAT terminology (see also other referees' concerns). The substance of the review (and proposal) should be the focus of piece, not its terminology or how it relates to other acronyms. Any self-evaluative statements ("this framework/theory will...") should be strictly avoided.

2) Presentation/Scientific Advance. Your review includes some sections that are considered textbook knowledge or are written in the style of a textbook. The goal for the piece is to be accessible to a broad audience of psychology researchers, including junior researchers; the target audience is not layreaders. Please keep this audience in mind as you balance accessibility and focus on the new scientific proposal.

In sum, we invite you to revise your manuscript taking into account all reviewer and editor comments.

You will find a complete list of formatting requirements following this link:

<https://www.nature.com/documents/commsj-style-formatting-checklist-review-perspective.pdf>

Please use the checklist to prepare your manuscript for resubmission.

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

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We hope to receive your revised paper within 12 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.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

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,

Marike

Marike Schiffer, PhD

Chief Editor

Communications Psychology

REVIEWERS' EXPERTISE:

The Reviewers all have a background in attachment theory and the neurobiology of social/developmental behaviour

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

This review provides a psychobiological bridge between the two lines of literature that are usually addressed separately: attachment theory and social neuroscience.

A significant advantage and point of novelty of this manuscript is the inclusion of different biological levels, namely, the anatomy of brain regions, neurotransmitters, and cellular processes of signal transduction. The social neuroscience literature often lacks this kind of detail, which adds another layer of complexity and mechanistic knowledge. Moreover, the new model adds biological components that are not usually treated as inherent parts of the social brain, adding more biological detail to the framework.

In this sense, the manuscript is timely and called for, can advance the state-of-the-art, and opens novel theoretical and empirical avenues. My main comment is that some parts of the manuscript and figures read like textbook summaries and do not explicitly specify the novelty.

Specific points:

1. The figures: In their current form, the figures do not add any new information to the reader but rather describe neural circuits that were previously described. What is the novel take-home message of each figure? Making it clear, both graphically and in the figures' titles, will improve the readability of the figures, and make them self-explanatory.

Consider including a depiction of the attachment behavior in addition to the brain to make an intuitive connection between them.

The titles of the figures are general and do not express their contribution to the paper, or the main idea of the figure.

Figures 1 and 3—The cortical aspects are detailed in the model and should also be included in the figures.

Figure 1a- abbreviations are missing

2. The first section is clear. It specifies attachment patterns, then lists the key biological candidates involved, and then describes the unifying model (UnBAT).

3. The idea of social flight is interesting but requires further clarification. Specifically, why and in what way is “social flight” specific and different from classical and operant conditioning?

The authors write that “dopamine from the VTA and noradrenaline from the LC being co-released in the hippocampus, and especially in its CA1 subregion during social information retrieval from memory, as shown in animal research irrespective of attachment.” How is this different from retrieving other (i.e., nonsocial) safety cues (or any relevant cue)?

4. Figure 2 and the section on “prolonged proximity seeking” (pages 9-10): The text describes the HPA axis (which is also the name of the figure) in detail. This reads more like a textbook description of the axis and lacks an explanation of the novelty and the relevance of the novel model. For example, what do UnBAT and social flight teach us about the role of the HPA? The general sentence about daily patterns and depression requires an explanation.

5. The section on reunion is particularly good because it provides a much-needed integration between the literature on oxytocin, the HPA axis, and dopamine. The link between oxytocin and opioids is interesting and novel.

6. How do “insecure-anxious and avoidant attachment patterns form a deviation from secure attachment patterns”? This requires an explanation across all levels, from the psychological to the biological and genetic manifestations.

7. The last section states that it focuses on developing attachment patterns through epigenetics. However, the section describes the manifestation of attachment patterns and the potential attachment outcomes of over/under expression and function of certain receptors or the HPA axis. The section lacks a sufficient review of the existing evidence of how maternal/parental behavior impacts epigenetic modifications to relevant genes. Many key papers on this topic are not cited, such as Weaver et al. 2004 Nature, Szyf et al. 2007, and others from Weaver, Frances Champagne, and Michael Meaney. Relatedly, a discussion on epigenetic modifications of maternal care on sex hormones and receptors is missing, given the evidence showing their involvement in social bonding and sensitivity to early life care.

Reviewer #2 (Remarks to the Author):

The manuscript “The Biological Backbone of Attachment: An Integrated Theoretical, Neurobiological and Epigenetic Framework of Attachment Pattern Formation” presents a narrative that implicates specific neural systems, endocrine responses, and gene expression in the complex behavioral and sensory responses that characterize attachment behavior. This provides a cohesive narrative that aids in the conceptualization of attachment formation and variation in attachment relationships.

Comments/Critiques

The repeated referral to a “Unified Biological Attachment Theory (UnBAT)” detracts from the value of the manuscript. The narrative provided has value but setting this up as a theory does not. The neurobiological mechanisms described are in some cases very superficial and oversimplified. They work as a narrative but each element of this narrative could easily be scrutinized and challenged by additional work in the field that is not cited in this manuscript. Moreover, there are species-specific dynamics that are collapsed together in this theory. Finally, there are many biological systems that are not included here, such that “Unified Biological” is an oversimplification. I recommend removing UnBAT entirely and focusing on the narrative itself – which again, is valuable.

Though describing what may be stable gene expression changes involved in variation in attachment has value within the manuscript, invoking “epigenetics” does not. The treatment of epigenetics in this section is highly speculative. Placing focus on how gene expression changes may account for variation should just be considered a molecular level basis for behavior – these can occur through a variety of molecular-level processes. You can certainly suggest that epigenetic mechanisms might be involved in these changes, but that is generally speculative.

The visuals provided in Figures 1 & 3 are not particularly effective at supporting the narrative.

Abstract – Line 34, should be “neurobiological and”

Reviewer #3 (Remarks to the Author):

The paper is well structured and thought-out.

At the beginning you mention the effect of maternal (and paternal) behavior on the child's attachment pattern formation. I was wondering, if you can elaborate this influence more, especially with respect to epigenetics, which you mention at the end of your paper. Furthermore, I recommend to present more attachment theories, than the ones provided (e.g. Hazan & Shaver or Bartholomew & Horowitz), to get a more comprehensive overview about different approaches.

The different steps of the UnBAT are really well explained and the provided figures are helpful for further clarification. Thus, I was wondering why both “wanting” and “liking” is increased after the

reunion with the attachment figure (line 341). “Liking” seems reasonable, but I do not understand why “wanting” should still be high after the reward is gained.

The input about epigenetics is well written; however, here are cases, in which the gene activity is upregulated (depending on the gene and the location of methylation). Furthermore, I was wondering, if you can provide some examples about studies which focus on DNA methylation and attachment.

Overall, the paper is clearly written, and of great interest for the field. The introduction of the UnBAT theory, provides a new perspective about attachment, and will drive a deeper understanding of attachment patterns.

Reviewer #1

1. The figures: In their current form, the figures do not add any new information to the reader but rather describe neural circuits that were previously described. What is the novel take-home message of each figure? Making it clear, both graphically and in the figures' titles, will improve the readability of the figures, and make them self-explanatory.

Consider including a depiction of the attachment behavior in addition to the brain to make an intuitive connection between them.

The titles of the figures are general and do not express their contribution to the paper, or the main idea of the figure.

Figures 1 and 3—The cortical aspects are detailed in the model and should also be included in the figures.

Figure 1a- abbreviations are missing

Response: Thank you for these insightful comments and recommendations regarding our figures. In keeping with the latter we decided to omit the figures altogether from the current version of the paper.

2. The idea of social flight is interesting but requires further clarification. Specifically, why and in what way is “social flight” specific and different from classical and operant conditioning?

Response: The reviewer raises a very important point here. Thank you. We now elaborate more on the “social flight” concept and link it better to previous accounts of classical and operant conditioning.

The authors write that “dopamine from the VTA and noradrenaline from the LC being co-released in the hippocampus, and especially in its CA1 subregion during social information retrieval from memory, as shown in animal research irrespective of attachment.” How is this different from retrieving other (i.e., nonsocial) safety cues (or any relevant cue)?

Response: We are sorry for this omission. In the revised version of our manuscript, we more extensively elaborate on the specific roles of noradrenaline, dopamine, and oxytocin in memory retrieval to better explain how their actions relate specifically to social memories.

3. Figure 2 and the section on “prolonged proximity seeking” (pages 9-10): The text describes the HPA axis (which is also the name of the figure) in detail. This reads more like a textbook description of the axis and lacks an explanation of the novelty and the

relevance of the novel model. For example, what do UnBAT and social flight teach us about the role of the HPA? The general sentence about daily patterns and depression requires an explanation.

Response: As already indicated above, we decided to omit all figures. Furthermore, we substantially edited the section on the role of the HPA axis.

4. How do “insecure-anxious and avoidant attachment patterns form a deviation from secure attachment patterns”? This requires an explanation across all levels, from the psychological to the biological and genetic manifestations.

Response: Thank you for this very important comment. We edited the section on the individual differences in attachment patterns accordingly.

7. The last section states that it focuses on developing attachment patterns through epigenetics. However, the section describes the manifestation of attachment patterns and the potential attachment outcomes of over/under expression and function of certain receptors or the HPA axis. The section lacks a sufficient review of the existing evidence of how maternal/parental behavior impacts epigenetic modifications to relevant genes. Many key papers on this topic are not cited, such as Weaver et al. 2004 Nature, Szyf et al. 2007, and others from Weaver, Frances Champagne, and Michael Meaney. Relatedly, a discussion on epigenetic modifications of maternal care on sex hormones and receptors is missing, given the evidence showing their involvement in social bonding and sensitivity to early life care.

Response: We are sorry for this omission. We substantially edited the section on individual differences in attachment patterns and epigenetics and now refer to the pioneering work by Meaney and Szyf.

Reviewer #2

The repeated referral to a “Unified Biological Attachment Theory (UnBAT)” detracts from the value of the manuscript. The narrative provided has value but setting this up as a theory does not. The neurobiological mechanisms described are in some cases very superficial and oversimplified. They work as a narrative but each element of this narrative could easily be scrutinized and challenged by additional work in the field that is not cited in this manuscript. Moreover, there are species-specific dynamics that are collapsed together in this theory. Finally, there are many biological systems that are not included here, such that “Unified Biological” is an oversimplification. I recommend removing UnBAT entirely and focusing on the narrative itself – which again, is valuable.

Response: Thank you for this important guidance. We now avoid referring to the UnBAT terminology and to presenting our thinking as a new theory.

Though describing what may be stable gene expression changes involved in variation in attachment has value within the manuscript, invoking “epigenetics” does not. The treatment of epigenetics in this section is highly speculative. Placing focus on how gene expression changes may account for variation should just be considered a molecular level basis for behavior – these can occur through a variety of molecular-level processes. You can certainly suggest that epigenetic mechanisms might be involved in these changes, but that is generally speculative.

Response: We agree with this reviewer that some of our suggestions were very speculative. We therefore thoroughly edited the section on individual differences in attachment patterns and epigenetics.

The visuals provided in Figures 1 & 3 are not particularly effective at supporting the narrative.

Response: As already indicated above, we decided to omit all figures in the current version of the paper according to reviewer’s comments.

Abstract – Line 34, should be “neurobiological and”

Response: We are sorry for this error and corrected the typo.

Reviewer #3

At the beginning you mention the effect of maternal (and paternal) behavior on the child's attachment pattern formation. I was wondering, if you can elaborate this influence more, especially with respect to epigenetics, which you mention at the end of your paper. Furthermore, I recommend to present more attachment theories, than the ones provided (e.g. Hazan & Shaver or Bartholomew & Horowitz), to get a more comprehensive overview about different approaches.

Response: Thank you for this very insightful feedback and recommendation. We substantially edited the corresponding section on individual differences in attachment patterns and epigenetics and now refer to the pioneering work by Meaney and Szyf (see previous comments above). We now also refer to additional attachment theories, such as BOSTA and tend-to-befriend theories.

The different steps of the UnBAT are really well explained and the provided figures are helpful for further clarification. Thus, I was wondering why both “wanting” and “liking” is increased after the reunion with the attachment figure (line 341). “Liking” seems

reasonable, but I do not understand why “wanting” should still be high after the reward is gained.

Response: According to other reviewers’ comments, in the current version of our manuscript, we elaborate more on the role of dopamine, which produces the sensation of “wanting,” in the hope of clearly explaining its crucial role in attachment behavior. Please note that in accordance with the other reviewer’s comments, we decided to remove all figures.

The input about epigenetics is well written; however, here are cases, in which the gene activity is upregulated (depending on the gene and the location of methylation). Furthermore, I was wondering, if you can provide some examples about studies which focus on DNA methylation and attachment.

Response: Based on the comments of the Editor and Reviewers 1 and 2, we thoroughly edited the section on epigenetics, including a discussion of the issues this reviewer mentions.

4th Jul 24

Dear Professor Ein-Dor,

Thank you for submitting a revised version of your manuscript titled "The Biological Backbone of Attachment: A Narrative on the Neurobiological Roots of Attachment-System Functioning".

I have read your revised version and revisited the previously solicited peer-reviewer reports. I appreciate the comprehensive edits, yet, after careful consideration, I have decided that we cannot send the work back to the peer reviewers in its present form.

The journal's review content needs to be presented in a way that is suitable for a broad audience of psychology researchers. Your Perspective proposes a process model of human interaction and describes in great level of detail the underlying neurobiological processes. It currently does not contain any visual display items to aid the reader in understanding the process or its underlying biology. While the text is written in clear language, it is hard to follow for non-expert readers.

For Reviews or Perspectives to be widely read and influential beyond a small group of experts, Figures are often indispensable. Your paper is one example where Figures, and potentially a Box or Table that explains key terminology would significantly broaden the appeal and accessibility of the work.

Because these Figures will require peer review by our expert panel, I am inviting you to undertake this necessary revision now, rather than following re-review, to reduce the overall number of rounds of review (and thus time to final decision).

We hope to receive your revised version as soon as possible. If you anticipate a delay of more than two months, however, please let us know. We will be happy to consider your revision so long as nothing similar has been accepted for publication at Communications Psychology or published elsewhere.

If you are not interested in submitting a suitably revised manuscript in the future please let me know immediately so we can close your file. If you have any questions, please contact me.

Please use the link below to submit a suitably revised manuscript and updated response to referees when they are ready.

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Best regards,

Marike

Marike Schiffer, PhD

Chief Editor

Communications Psychology

3rd Sep 24

Dear Professor Ein-Dor,

Your Perspective titled "The Biological Backbone of Attachment: A Narrative on the Neurobiological Roots of Attachment-System Functioning" has now been seen by the previous 3 referees, whose comments appear below. In the light of their advice I am delighted to say that we are happy, in principle, to publish it in Communications Psychology.

If the revised paper addresses all reviewer concerns, is in Communications Psychology format, in accessible style and of appropriate length, we shall accept it for publication immediately. I have attached an edited version of your manuscript, and ask you to attend to each comment in detail.

EDITORIAL REQUESTS:

* Please comprehensively address the remaining concerns voiced by Reviewer #1 through suitable revisions. We will not send your revised paper for further review if, in the editors' judgement, the referees' comments on the present version have been addressed. However, we reserve the right to seek further input if the revisions appear to not fully satisfy the referee's requests.

* Please review the changes in the attached copy of your manuscript, which has been edited for style, and address the comments and queries I have added. If using Word, please use the 'track changes' feature to make the process of accepting your manuscript more efficient.

* Please ensure in particular that the style of referencing comprehensively supports all statements (this is frequently missing especially for the newly added neurobiological sections). Statements that have a proposal character must be emphasized as such.

* Please check whether your manuscript contains third-party images, such as figures from the literature, stock photos, clip art or commercial satellite and map data. If any of the display items in your manuscript (figures, tables, boxes or movies) include images that are the same as, or are adaptations of, previously published images, please fill in the Third Party Rights Table, and return to us when you submit your revised manuscript. This information will enable us to obtain the necessary rights to re-use such material. If we are unable to obtain the necessary rights to use or adapt any of the material that you wish to use, we will contact you to discuss alternative options.

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*If you have not done so already, please alert me to any related manuscripts from your group that are under consideration or in press at other journals, or are being written up for submission to other journals (see www.nature.com/authors/editorial_policies/duplicate.html for details).

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You will find a complete list of formatting requirements following this link:
<https://www.nature.com/documents/commsj-style-formatting-checklist-review-perspective.pdf>

Please use the checklist to prepare your manuscript for final submission. In the following, I also highlight some issues of particular importance.

** Figures

Please remove all figures from the main text and upload them individually, one figure per file. To ensure the swift processing of your paper please provide the highest quality, vector format, versions of your images (.ai, .eps, .psd) where available. Text and labelling should be in a separate layer to enable editing during the production process. If vector files are not available then please supply the figures in whichever format they were compiled in and not saved as flat .jpeg or .TIFF files. If your artwork contains any photographic images, please ensure these are at least 300 dpi.

* Figures should be simple and informative — multi-part figures are best avoided. Boxes should occupy no more than half a page in the PDF (less than 500 words) and may include a figure.

* References

References appear as superscript Arabic numerals, in order of mention. The reference list mentions references in the numerical order in which they are mentioned in the main text. If a reference is cited more than once, the same number is used throughout the text and the reference receives a single entry in the reference list.

We ask that you select the most significant 5–10% of references in your list for highlighting, and add a single sentence in bold after each of these references to describe the main result and its significance.

Only papers that have been published or accepted by a named publication should be in the reference list (preprints and citations of datasets are also permitted). Unpublished/Submitted research should not be included in the reference list; it should only be mentioned briefly and parenthetically in the main text. Note that no major arguments should rely on unpublished research.

Published conference abstracts and URLs for web sites should be cited parenthetically in the text, not in the reference list.

Footnotes are not used.

* Competing interests

Please include a "Competing interests" statement after the References. Note that we ask authors to declare both financial and non-financial competing interests. For more details, see <https://www.nature.com/authors/policies/competing.html>. If you have no financial or non-financial competing interests, please state so: "The authors declare no competing interests."

SUBMISSION INFORMATION:

In order to accept your paper, we require the following:

* A cover letter describing your response to our editorial requests.

* A separate document detailing your point-by-point response to any issues raised by our referees (please include the referees' comments in this document).

* The final version of your text as a Word or TeX/LaTeX file, with any tables prepared using the Table menu in Word or the table environment in TeX/LaTeX and using the 'track changes' feature in Word.

* Production-quality versions of all figures, supplied as separate files. Photographic images should be 300 dpi in RGB format (.jpg, TIFF or native Photoshop format) and any labels/scale bars included in a separate layer from the image. Line art, graphs and schemes should be vector format (.ai, .eps, .pdf); Adobe Illustrator files are preferred and will minimize production time. Any chemical structures or schemes contained within figures should additionally be supplied as separate Chemdraw (.cdx) files.

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We hope to hear from you within two weeks; please let us know if the process may take longer.

Best regards,

Marike

Marike Schiffer, PhD

Chief Editor

Communications Psychology

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

- The figure is quite informative. I have two comments:

1. There is a significant amount of text in the figure. It might be helpful to succinctly present the information.

2. It is unclear what justifies labeling the mechanisms described as social. How do they differ from domain-general sympathetic and endocrine stress responses?

- Defining attachment as fear behavior might not fully capture its complexity. Attachment serves many purposes and is motivated by numerous factors. The fact that individuals seek closeness in fearful situations doesn't necessarily imply the opposite. It is logically unsupported to assume attachment specifically arises from fear. Similarly, the fact that people seek proximity during stress does not inherently make the stress response 'social.' This is an example of the fallacy of affirming the consequent: just because stress leads to attachment behavior does not mean that the mechanisms of stress are inherently social. Stress can lead to various outcomes beyond attachment behaviors, and attachment behavior can arise in different contexts than stress.

How are the dopamine-noradrenaline, HPA, and oxytocinergic mechanisms suggested to be specific for attachment figures acting categorically differently in a social context?

Despite the additional rationale provided in the revised manuscript, I am not convinced. The social labeling of the stress and memory mechanisms as social flight might not be justified and could be potentially misleading.

This aligns with reviewer #2's comment regarding the oversimplified presentation of the provided narrative.

- While the last section articulates hypotheses about epigenetic differences in attachment styles, I didn't find revisions explaining the different biological levels discussed in the paper of insecure and

anxious attachment styles and how distortions in the mechanisms described in the paper lead to them.

Reviewer #2 (Remarks to the Author):

The authors have adequately addressed the prior critiques and both the overall framework and summary figure are a strong contribution to the field.

Reviewer #3 (Remarks to the Author):

Thank you for the well thought out revision. Especially the new figure provides clarification. No further comments.

Reviewer #1

1. There is a significant amount of text in the figure. It might be helpful to succinctly present the information.

Response: As requested, we have revised the figure to present the information more succinctly, ensuring clarity while maintaining the essential details. We aimed to balance the visual elements with concise text to improve readability and convey the key points effectively.

2. It is unclear what justifies labeling the mechanisms described as social. How do they differ from domain-general sympathetic and endocrine stress responses?

Response: Many of the considerations in the current manuscript are based on our previously published neuro-anatomical model of human attachment (NAMA; Long et al., 2020). NAMA clearly postulates that, while attachment behaviour is orchestrated by an extended and interconnected network of brain areas, these areas do many other things. There thus is no dedicated “attachment region” or “attachment network”, and similarly, the brain regions involved in attachment behaviour are not selectively activated within a social context. For example, many threats that yield attachment system activation (via a fear response involving the sympathetic nervous system and the HPA-axis) are of a non-social nature. However, because proximity seeking directed towards attachment figures as an innate response to danger is an integral part of attachment behaviour, most of its components are inherently ‘social’ in nature. Thus, the described mechanisms are labelled as ‘social’ because they arise within the social setting of attachment behaviour. This does, of course, not imply that the same mechanisms cannot have different implications and be involved under different circumstances, many of which are non-social. Nonetheless, to avoid possible confusion, we now changed a subsection heading of Figure 1 into Sustained “Proximity Seeking.”

• Defining attachment as fear behavior might not fully capture its complexity. Attachment serves many purposes and is motivated by numerous factors. The fact that individuals seek closeness in fearful situations doesn’t necessarily imply the opposite. It is logically unsupported to assume attachment specifically arises from fear. Similarly, the fact that people seek proximity during stress does not inherently make the stress response ‘social.’ This is an example of the fallacy of affirming the consequent: just because stress leads to attachment behavior does not mean that the mechanisms of stress are inherently social. Stress can lead to various outcomes beyond attachment behaviors, and attachment behavior can arise in different contexts than stress.

Response: We did not intend to define attachment as a specific fear behavior. Rather, we cited John Bowlby’s exact words, where he stated that flight and proximity-seeking behaviors could be categorized as fear behaviors. As Bowlby is the originator of attachment theory, we believe his words are highly relevant to the discussion, and we respectfully request that the citation be retained. Additionally, the term “social flight” is well-established in the attachment literature, defined by White and colleagues (2023),

and supported by observational (e.g., Mawson), experimental (e.g., Ein-Dor), and cognitive studies (e.g., Mikulincer). Given its importance, we would like to retain the term “social flight,” as it accurately describes the behavior of moving away from threats (not solely stress) toward an attachment figure as part of the innate and primary attachment behavioral response, which is a social act.

Furthermore, we have added additional citations and revised our wording to strengthen our arguments and provide further support for our suggestions. While we agree with the reviewer that attachment behavior can occur without the sensation of stress, we maintain, as Bowlby noted, that it often involves the perception of “natural signs of danger.” Additionally, we emphasize that the activation of “fight-or-flight” provides the energy necessary for action. We do not imply that stress is the outcome of “fight-or-flight” activation.

Please also see our response to the above comment about labeling the mechanisms associated with attachment as ‘social’.

How are the dopamine-noradrenaline, HPA, and oxytocinergic mechanisms suggested to be specific for attachment figures acting categorically differently in a social context?

Response: The reviewer is correct in noting that, to date, the literature lacks direct evidence for the process that leads to the attachment figure emerging in memory. Accordingly, we have added the following statement: “Here, we must emphasize that direct empirical evidence for this mechanism is still lacking, and further research is needed to validate these ideas.”

Despite the additional rationale provided in the revised manuscript, I am not convinced. The social labeling of the stress and memory mechanisms as social flight might not be justified and could be potentially misleading.

Response: As we responded earlier, the term “social flight” is well-established in the attachment literature, defined by White and colleagues (2023), and supported by observational (e.g., Mawson), experimental (e.g., Ein-Dor), and cognitive studies (e.g., Mikulincer). Given its importance, we would like to retain the term “social flight,” as it accurately describes the behavior of moving away from threats (not solely stress) toward an attachment figure as part of the innate and primary attachment behavioral response, which is a social act. This does not imply that these mechanisms are selectively ‘social’, but that in the context of attachment, they are recruited for a social purpose.

- While the last section articulates hypotheses about epigenetic differences in attachment styles, I didn’t find revisions explaining the different biological levels discussed in the paper of insecure and anxious attachment styles and how distortions in the mechanisms described in the paper lead to them.

Response: We appreciate the reviewer’s feedback. In our last revision, we completely restructured the section to refrain from presenting any hypotheses, as per previous revision instructions. The revised section now focuses solely on reviewing the observed differences in epigenetic modifications without speculating on their implications. We have also discussed relevant mechanisms, such as the HPA axis and oxytocin, as

described in the paper, and highlighted how these biological systems differ between insecure and anxious attachment styles.

Our intention is to stimulate further research by offering a framework based on the potential of epigenetic modifications in attachment styles. We hope that researchers can use this perspective to guide their studies in this area. In line with the previous rounds of revision, we avoided speculating on the results, ensuring that our discussion remains grounded in current evidence.

Reviewer #2

The authors have adequately addressed the prior critiques and both the overall framework and summary figure are a strong contribution to the field.

Response: We sincerely thank the reviewer for their thoughtful feedback and positive evaluation. We are pleased to hear that the revised framework and summary figure are seen as a strong contribution to the field. We greatly appreciate the constructive critiques provided throughout the review process, as they have significantly improved the clarity and impact of our work.

Reviewer #3

Thank you for the well thought out revision. Especially the new figure provides clarification. No further comments.

Response: Thank you for your kind words and for recognizing the efforts put into the revision. We are glad that the new figure has provided the needed clarification. We truly appreciate your feedback throughout the process, which has helped enhance the manuscript.