

External environment and internal state in relation to life-history behavioural profiles of adolescents in nine countries

Lei Chang, Hui Jing Lu, Jennifer E. Lansford, Marc H. Bornstein, Laurence Steinberg, Bin-Bin Chen, Ann T. Skinner, Kenneth A. Dodge, Kirby Deater-Deckard, Dario Bacchini, Concetta Pastorelli, Liane Peña Alampay, Sombat Tapanya, Emma Sorbring, Paul Oburu, Suha M. Al-Hassan, Laura Di Giunta, Patrick S. Malone, Liliana Maria Uribe Tirado and Saengduean Yotanyamaneewong

Article citation details

Proc. R. Soc. B **286**: 20192097.
<http://dx.doi.org/10.1098/rspb.2019.2097>

Review timeline

Original submission: 10 September 2019
1st revised submission: 31 October 2019
2nd revised submission: 13 November 2019
Final acceptance: 25 November 2019

Note: Reports are unedited and appear as submitted by the referee. The review history appears in chronological order.

Review History

RSPB-2019-2097.R0 (Original submission)

Review form: Reviewer 1

Recommendation

Major revision is needed (please make suggestions in comments)

Scientific importance: Is the manuscript an original and important contribution to its field?

Good

General interest: Is the paper of sufficient general interest?

Excellent

Quality of the paper: Is the overall quality of the paper suitable?

Good

Is the length of the paper justified?

Yes

Should the paper be seen by a specialist statistical reviewer?

No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.

No

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?

N/A

Is it clear?

N/A

Is it adequate?

N/A

Do you have any ethical concerns with this paper?

No

Comments to the Author

I believe that this article is empirically sound. That is, the variables are well measured and analyzed. At the same time, the article is not always theoretically sound. I would recommend that the authors rework some of the theoretical arguments, as follows:

1. "It has been widely observed that harsh and unpredictable external environments shape fast life histories (LH) and LH related traits [1-4]." Stearns is not an appropriate citation for this statement. He does not endorse developmental life history models.
2. "For example, populations of the Trinidadian guppy (*Poecilia reticulata*) living downstream with high predatory risks adopt faster LH by exhibiting an earlier age and smaller size at maturity, shorter inter-brood..." Be clear whether you are referring to evolution or development of life history strategies.
3. "Large data studies reported in the public health literature have reached similar conclusions that adolescents with chronic medical conditions exhibit fast LH, characterized by earlier, more active, or riskier sexual activities, and other risky behaviors, such as smoking, drug use, and antisocial acts [15-18]." Please comment on whether these data suggest that faster LH strategies are causing more medical conditions, or vice versa, or whether the direction of causation cannot be established from the research designs used. This is directly relevant to assessing the internal PAR model, which assumes a pathway from somatic deterioration to faster LH strategies (not vice versa).
4. The paragraph beginning: "The theoretical rationale underpinning these observations follows..." This paragraph has a number of problematic statements. "Emphasizing the time lag between predictive responses to the environment or body state and adaptive LH phenotypes manifested subsequently, this process is also called external or internal predictive adaptive

responses [22,23].” Unclear sentence. “Various bio-energetic trade-offs can be summarized into those between reproduction or early reproduction and prereproductive consumption for somatic development and maintenance, which, when given more, as compared to less, energetic allocation mean slow development and delayed reproduction in contrast to fast development and early reproduction.” This sentence is unintelligible. “For external processes, leading causes of mortality and morbidity are extrinsic risks, such as predation, disease, and introspecific violence.” This sentence is also problematic. What is meant by “for external processes”? “The frequency and variation of extrinsic risks, known as environmental harshness and unpredictability [1], cause mortality and morbidity beyond individuals’ survival efforts.” This again does not make sense. I do not know what the authors are trying to say here. “Corresponding to these two kinds of physical, reproductive strategies, there is related dual sociality.” I do not buy the distinction between physical and social aspects of LH strategies. There are coherent, integrated developmental adaptations to stress. There is no natural distinction between social and physical responses—it is all biobehavioral. In general, the authors provide a poor representation of Ellis et al. (2009), which they are drawing from. One detail after another is wrong or stated in a confusing manner. For a good recent summary of the logic underlying harshness and unpredictability, see Ellis & Del Giudice, (2019). The logic needs to be stated clearly to justify the measurement strategy.

5. “Their results, based on a large longitudinal U. S. American sample, showed that external adversity and internal state were each uniquely predictive of fast LH in the expected directions and the effect size of the external prediction was much larger than that of the internal prediction. Three other studies tested almost identical mediation models and yielded similar findings that external and internal predictors were each uniquely predictive of LH [35-37].” There are many problems with these statements. First, this is an oversimplification of Hartman's findings, which were mixed. Further, two of these studies were nothing like Hartman et al., as health was not included in Ellis & Essieux and health was not tested as a mediating variable in Belsky et al.. Chua et al. was all based on retrospective data that is hard to interpret in this context, as fast LH strategies could be causing health problems, rather than vice versa.

6. Sample. Please describe the sample. Are these well-fed, middle-class participants. Are there substantial differences in the living conditions for participants from less developed vs more developed countries? From what population were participants recruited?

7. The major issue that I have with this paper was the use of the CBCL somatic complaints scale as an indicator of internal somatic condition. This Scale does not map on very well to the theory. Why should we believe that this represents compromised internal state, as in real illness. Given that this scale has been used a million times, there should be data indicating to what extent somatic complaints correlate with actual health measures, such biological aging, oxidative stress, viral infections, asthma, etc. Note that somatic complaints is part of the internalizing scale, which is more often used as an indicator of LH strategy (e.g., Chua et al.). The authors need to provide a compelling argument that the Somatic Complaints subscale is indicative of health deterioration. Otherwise, they are not testing the theory.

8. Risk-taking. Why did the authors average together the 4 ratings. The first rating item directly assess risky behavior. It is not clear to what extent the other 3 ratings do.

Review form: Reviewer 2

Recommendation

Accept with minor revision (please list in comments)

Scientific importance: Is the manuscript an original and important contribution to its field?

Excellent

General interest: Is the paper of sufficient general interest?

Good

Quality of the paper: Is the overall quality of the paper suitable?

Good

Is the length of the paper justified?

Yes

Should the paper be seen by a specialist statistical reviewer?

No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.

No

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?

Yes

Is it clear?

No

Is it adequate?

No

Do you have any ethical concerns with this paper?

No

Comments to the Author

Comments to both author and editor are in the attached PDF. (See Appendix A)

Decision letter (RSPB-2019-2097.R0)

14-Oct-2019

Dear Professor Chang:

Your manuscript has now been peer reviewed and the reviews have been assessed by an Associate Editor. The reviewers' comments (not including confidential comments to the Editor) and the comments from the Associate Editor are included at the end of this email for your reference. As you will see, the reviewers and the Editors have raised some concerns with your manuscript and we would like to invite you to revise your manuscript to address them.

We do not allow multiple rounds of revision so we urge you to make every effort to fully address all of the comments at this stage. If deemed necessary by the Associate Editor, your manuscript will be sent back to one or more of the original reviewers for assessment. If the original reviewers are not available we may invite new reviewers. Please note that we cannot guarantee eventual acceptance of your manuscript at this stage.

To submit your revision please log into <http://mc.manuscriptcentral.com/prsb> and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions", click on "Create a Revision". Your manuscript number has been appended to denote a revision.

When submitting your revision please upload a file under "Response to Referees" - in the "File Upload" section. This should document, point by point, how you have responded to the reviewers' and Editors' comments, and the adjustments you have made to the manuscript. We require a copy of the manuscript with revisions made since the previous version marked as 'tracked changes' to be included in the 'response to referees' document.

Your main manuscript should be submitted as a text file (doc, txt, rtf or tex), not a PDF. Your figures should be submitted as separate files and not included within the main manuscript file.

When revising your manuscript you should also ensure that it adheres to our editorial policies (<https://royalsociety.org/journals/ethics-policies/>). You should pay particular attention to the following:

Research ethics:

If your study contains research on humans please ensure that you detail in the methods section whether you obtained ethical approval from your local research ethics committee and gained informed consent to participate from each of the participants.

Use of animals and field studies:

If your study uses animals please include details in the methods section of any approval and licences given to carry out the study and include full details of how animal welfare standards were ensured. Field studies should be conducted in accordance with local legislation; please include details of the appropriate permission and licences that you obtained to carry out the field work.

Data accessibility and data citation:

It is a condition of publication that you make available the data and research materials supporting the results in the article. Datasets should be deposited in an appropriate publicly available repository and details of the associated accession number, link or DOI to the datasets must be included in the Data Accessibility section of the article (<https://royalsociety.org/journals/ethics-policies/data-sharing-mining/>). Reference(s) to datasets should also be included in the reference list of the article with DOIs (where available).

In order to ensure effective and robust dissemination and appropriate credit to authors the dataset(s) used should also be fully cited and listed in the references.

If you wish to submit your data to Dryad (<http://datadryad.org/>) and have not already done so you can submit your data via this link [http://datadryad.org/submit?journalID=RSPB&manu=\(Document not available\)](http://datadryad.org/submit?journalID=RSPB&manu=(Document not available)), which will take you to your unique entry in the Dryad repository.

If you have already submitted your data to dryad you can make any necessary revisions to your dataset by following the above link.

For more information please see our open data policy <http://royalsocietypublishing.org/data-sharing>.

Electronic supplementary material:

All supplementary materials accompanying an accepted article will be treated as in their final form. They will be published alongside the paper on the journal website and posted on the online figshare repository. Files on figshare will be made available approximately one week before the accompanying article so that the supplementary material can be attributed a unique DOI. Please try to submit all supplementary material as a single file.

Online supplementary material will also carry the title and description provided during submission, so please ensure these are accurate and informative. Note that the Royal Society will not edit or typeset supplementary material and it will be hosted as provided. Please ensure that the supplementary material includes the paper details (authors, title, journal name, article DOI). Your article DOI will be 10.1098/rspb.[paper ID in form xxxx.xxxx e.g. 10.1098/rspb.2016.0049].

Please submit a copy of your revised paper within three weeks. If we do not hear from you within this time your manuscript will be rejected. If you are unable to meet this deadline please let us know as soon as possible, as we may be able to grant a short extension.

Thank you for submitting your manuscript to Proceedings B; we look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Best wishes,
Dr Sasha Dall
mailto:proceedingsb@royalsociety.org

Associate Editor

Comments to Author:

This is an impressive dataset, and the referees support extension of models to explore interactions. Both raise concerns about the use of the somatic complaints scale for measurement of internal state. There are also areas where theoretical arguments are not clear or become a little circular. These and other comments should be addressed in a revision. In addition, referee 2 is curious whether there may be interesting patterns in drop-outs and recruitment that offer further insight.

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)

I believe that this article is empirically sound. That is, the variables are well measured and analyzed. At the same time, the article is not always theoretically sound. I would recommend that the authors rework some of the theoretical arguments, as follows:

1. "It has been widely observed that harsh and unpredictable external environments shape fast life histories (LH) and LH related traits [1-4]." Stearns is not an appropriate citation for this statement. He does not endorse developmental life history models.

2. "For example, populations of the Trinidadian guppy (*Poecilia reticulata*) living downstream with high predatory risks adopt faster LH by exhibiting an earlier age and smaller size at maturity, shorter inter-brood..." Be clear whether you are referring to evolution or development of life history strategies.

3. "Large data studies reported in the public health literature have reached similar conclusions that adolescents with chronic medical conditions exhibit fast LH, characterized by earlier, more active, or riskier sexual activities, and other risky behaviors, such as smoking, drug use, and antisocial acts [15-18]." Please comment on whether these data suggest that faster LH strategies are causing more medical conditions, or vice versa, or whether the direction of causation cannot be established from the research designs used. This is directly relevant to assessing the internal PAR model, which assumes a pathway from somatic deterioration to faster LH strategies (not vice versa).

4. The paragraph beginning: "The theoretical rationale underpinning these observations follows..." This paragraph has a number of problematic statements. "Emphasizing the time lag between predictive responses to the environment or body state and adaptive LH phenotypes manifested subsequently, this process is also called external or internal predictive adaptive responses [22,23]." Unclear sentence. "Various bio-energetic trade-offs can be summarized into those between reproduction or early reproduction and prereproductive consumption for somatic development and maintenance, which, when given more, as compared to less, energetic allocation mean slow development and delayed reproduction in contrast to fast development and early reproduction." This sentence is unintelligible. "For external processes, leading causes of mortality and morbidity are extrinsic risks, such as predation, disease, and introspecific violence." This sentence is also problematic. What is meant by "for external processes"? "The frequency and variation of extrinsic risks, known as environmental harshness and unpredictability [1], cause mortality and morbidity beyond individuals' survival efforts." This again does not make sense. I do not know what the authors are trying to say here. "Corresponding to these two kinds of physical, reproductive strategies, there is related dual sociality." I do not buy the distinction between physical and social aspects of LH strategies. There are coherent, integrated developmental adaptations to stress. There is no natural distinction between social and physical responses--it is all biobehavioral. In general, the authors provide a poor representation of Ellis et al. (2009), which they are drawing from. One detail after another is wrong or stated in a confusing manner. For a good recent summary of the logic underlying harshness and unpredictability, see Ellis & Del Giudice, (2019). The logic needs to be stated clearly to justify the measurement strategy.

5. "Their results, based on a large longitudinal U. S. American sample, showed that external adversity and internal state were each uniquely predictive of fast LH in the expected directions and the effect size of the external prediction was much larger than that of the internal prediction. Three other studies tested almost identical mediation models and yielded similar findings that external and internal predictors were each uniquely predictive of LH [35-37]." There are many problems with these statements. First, this is an oversimplification of Hartman's findings, which were mixed. Further, two of these studies were nothing like Hartman et al., as health was not included in Ellis & Essiex and health was not tested as a mediating variable in Belsky et al.. Chua et al. was all based on retrospective data that is hard to interpret in this context, as fast LH strategies could be causing health problems, rather than vice versa.

6. Sample. Please describe the sample. Are these well-fed, middle-class participants. Are there substantial differences in the living conditions for participants from less developed vs more developed countries? From what population were participants recruited?

7. The major issue that I have with this paper was the use of the CBCL somatic complaints scale as an indicator of internal somatic condition. This Scale does not map on very well to the theory. Why should we believe that this represents compromised internal state, as in real illness. Given that this scale has been used a million times, there should be data indicating to what extent somatic complaints correlate with actual health measures, such as biological aging, oxidative stress, viral infections, asthma, etc. Note that somatic complaints is part of the internalizing scale, which is more often used as an indicator of LH strategy (e.g., Chua et al.). The authors need to provide a compelling argument that the Somatic Complaints subscale is indicative of health deterioration. Otherwise, they are not testing the theory.

8. Risk-taking. Why did the authors average together the 4 ratings. The first rating item directly assesses risky behavior. It is not clear to what extent the other 3 ratings do.

Referee: 2

Comments to the Author(s)

Comments to both author and editor are in the attached PDF

Author's Response to Decision Letter for (RSPB-2019-2097.R0)

See Appendix B.

Decision letter (RSPB-2019-2097.R1)

11-Nov-2019

Dear Professor Chang

I am pleased to inform you that your manuscript RSPB-2019-2097.R1 entitled "External Environment and Internal State in Relation to Life History Behavioral Profiles of Adolescents in Nine Countries" has been accepted for publication in Proceedings B.

The AE has recommended publication, but also suggests some minor revisions to your manuscript. Therefore, I invite you to respond to the comments and revise your manuscript. Because the schedule for publication is very tight, it is a condition of publication that you submit the revised version of your manuscript within 7 days. If you do not think you will be able to meet this date please let us know.

To revise your manuscript, log into <https://mc.manuscriptcentral.com/prsb> and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision. You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript and upload a new version through your Author Centre.

When submitting your revised manuscript, you will be able to respond to the comments made by the referee(s) and upload a file "Response to Referees". You can use this to document any changes you make to the original manuscript. We require a copy of the manuscript with revisions made since the previous version marked as 'tracked changes' to be included in the 'response to referees' document.

Before uploading your revised files please make sure that you have:

- 1) A text file of the manuscript (doc, txt, rtf or tex), including the references, tables (including captions) and figure captions. Please remove any tracked changes from the text before submission. PDF files are not an accepted format for the "Main Document".
- 2) A separate electronic file of each figure (tiff, EPS or print-quality PDF preferred). The format should be produced directly from original creation package, or original software format. PowerPoint files are not accepted.
- 3) Electronic supplementary material: this should be contained in a separate file and where possible, all ESM should be combined into a single file. All supplementary materials accompanying an accepted article will be treated as in their final form. They will be published alongside the paper on the journal website and posted on the online figshare repository. Files on figshare will be made available approximately one week before the accompanying article so that the supplementary material can be attributed a unique DOI.

Online supplementary material will also carry the title and description provided during submission, so please ensure these are accurate and informative. Note that the Royal Society will not edit or typeset supplementary material and it will be hosted as provided. Please ensure that the supplementary material includes the paper details (authors, title, journal name, article DOI). Your article DOI will be 10.1098/rspb.[paper ID in form xxxx.xxxx e.g. 10.1098/rspb.2016.0049].

- 4) A media summary: a short non-technical summary (up to 100 words) of the key findings/importance of your manuscript.

- 5) Data accessibility section and data citation

It is a condition of publication that data supporting your paper are made available either in the electronic supplementary material or through an appropriate repository.

In order to ensure effective and robust dissemination and appropriate credit to authors the dataset(s) used should be fully cited. To ensure archived data are available to readers, authors should include a 'data accessibility' section immediately after the acknowledgements section. This should list the database and accession number for all data from the article that has been made publicly available, for instance:

- DNA sequences: Genbank accessions F234391-F234402
- Phylogenetic data: TreeBASE accession number S9123
- Final DNA sequence assembly uploaded as online supplemental material
- Climate data and MaxEnt input files: Dryad doi:10.5521/dryad.12311

NB. From April 1 2013, peer reviewed articles based on research funded wholly or partly by RCUK must include, if applicable, a statement on how the underlying research materials – such as data, samples or models – can be accessed. This statement should be included in the data accessibility section.

If you wish to submit your data to Dryad (<http://datadryad.org/>) and have not already done so you can submit your data via this link

[http://datadryad.org/submit?journalID=RSPB&manu=\(Document not available\)](http://datadryad.org/submit?journalID=RSPB&manu=(Document+not+available)) which will take you to your unique entry in the Dryad repository. If you have already submitted your data to dryad you can make any necessary revisions to your dataset by following the above link. Please see <https://royalsociety.org/journals/ethics-policies/data-sharing-mining/> for more details.

6) For more information on our Licence to Publish, Open Access, Cover images and Media summaries, please visit <https://royalsociety.org/journals/authors/author-guidelines/>.

Once again, thank you for submitting your manuscript to Proceedings B and I look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Sincerely,

Dr Sasha Dall
Editor, Proceedings B
<mailto:proceedingsb@royalsociety.org>

Associate Editor:

Board Member

Comments to Author:

The authors' have further strengthened the manuscript with revisions. Particularly the sampling methods, where I would also recommend quoting the statistic provided in the comments on attrition rates relative to household income.

The referees' concerns about self-reported measures of internal state (validity of the scale; correlations between interoception and LH strategy) haven't been fully addressed, however.

Given the consistency between referees and hence possibility that readers will share these views, just a few in-depth and critical sentences on each point would be beneficial. With regards the CBCL scale, briefly summarizing the response already provided in the comments would be sufficient, on two points: specific markers/disease correlates in children, and it's relative validity compared to the internalizing scale. With regards interoception, an explicit comment is required on the possibility that it varies with individual differences in LH (rather than individual differences generally). Referee 2 has provided a reference that may be useful.

Decision letter (RSPB-2019-2097.R2)

25-Nov-2019

Dear Professor Chang

I am pleased to inform you that your manuscript entitled "External Environment and Internal State in Relation to Life History Behavioral Profiles of Adolescents in Nine Countries" has been accepted for publication in Proceedings B.

You can expect to receive a proof of your article from our Production office in due course, please check your spam filter if you do not receive it. PLEASE NOTE: you will be given the exact page length of your paper which may be different from the estimation from Editorial and you may be asked to reduce your paper if it goes over the 10 page limit.

If you are likely to be away from e-mail contact please let us know. Due to rapid publication and an extremely tight schedule, if comments are not received, we may publish the paper as it stands.

If you have any queries regarding the production of your final article or the publication date please contact procb_proofs@royalsociety.org

Open Access

You are invited to opt for Open Access, making your freely available to all as soon as it is ready for publication under a CCBY licence. Our article processing charge for Open Access is £1700.

Corresponding authors from member institutions

(<http://royalsocietypublishing.org/site/librarians/allmembers.xhtml>) receive a 25% discount to these charges. For more information please visit <http://royalsocietypublishing.org/open-access>.

Your article has been estimated as being 10 pages long. Our Production Office will be able to confirm the exact length at proof stage.

Paper charges

An e-mail request for payment of any related charges will be sent out after proof stage (within approximately 2-6 weeks). The preferred payment method is by credit card; however, other payment options are available

Electronic supplementary material:

All supplementary materials accompanying an accepted article will be treated as in their final form. They will be published alongside the paper on the journal website and posted on the online figshare repository. Files on figshare will be made available approximately one week before the accompanying article so that the supplementary material can be attributed a unique DOI.

Thank you for your fine contribution. On behalf of the Editors of the Proceedings B, we look forward to your continued contributions to the Journal.

Sincerely,

Dr Sasha Dall

Editor, Proceedings B

<mailto:proceedingsb@royalsociety.org>

Appendix A

Review of Manuscript RSPB-2019-2097

External Environment and Internal State in Relation to Life History Behavioral Profiles of Adolescents in Nine Countries

The current paper seeks to expand on existing life history literature by examining the role that internal state plays, when statistically tested in synergy with external environmental cues of harshness and unpredictability. The authors have collected cross-cultural, longitudinal data and employed SEM to test the independent and mutual influences that internal and external environments play in calibrating life history relevant behaviors. Although the hypotheses in this paper are not a great theoretical leap from some of the recent studies discussed in the introduction, this paper still adds significantly to the literature by employing a different theoretical model, and by introducing data from a more diverse sample. In short, there is much to like about this paper. I believe that with some minor-to-moderate revision (in the introduction, methods, and discussion), this paper would be suitable for publication in Proceedings B.

Please see below for my questions and suggestions:

Introduction:

1. (Pages 1- 4) Although the authors do quite well at explaining life history theory, some of this information comes a little late in the introduction for a novice reader who is not as familiar with Life history or its terminology. Could some of this basic definitional information (e.g., the beginning of paragraph 2 of page 4) come earlier in the introduction? I understand the rationale and terminology well, but a lay reader or out of area reader may not.
2. Did you predict in advance that external environment would be a greater predictor than internal state (hence the ordinal interaction)? Why? Is your logic based on the four external-internal integrative studies that you cited? If so, please elaborate on this point a little more.

Methods:

1. You have a wonderfully diverse sample that seems to be well balanced across all collection sites. I appreciate the complexity that goes into collecting this kind of data. I also appreciate that you conducted attrition analyses. I am curious: do you have any information on whether participants who dropped out differed from participants who persisted in 1) family income (or some other indicator of SES outside of parent education), or 2) by collection site (i.e., if more of the drop-outs were from a particular site)?
2. I'd be interested to know a little bit more about the general socioeconomic characteristics of your sample (aside from parent education) if you have that data, since you mention that children were recruited from school programs. I'd wager that in some of the countries you collected (especially the poorest ones), children from the lowest

socioeconomic demographics are less likely to be enrolled in or attend school. Thus, maybe there is the potential that your sample in those countries may be somewhat limited in terms of variability in overall environmental harshness, potentially diminishing your or altering your effects slightly.

3. The measures used to represent external environments, internal states, and LH are appropriately chosen. Many are well-cited measures (e.g., CHAOS, BIS-11) that conceptually approximate past conceptualizations of these theoretical constructs.

Data Analysis and Results:

1. This is one of the clearest written and explained sections for SEM that I've read in quite some time. Each theoretical and analytical choice is well rationalized. The theoretical model, estimator decision, and analytical methods used to test it seem appropriate to the research questions and data at hand. Fit statistics are slightly diminished, but adequate for cross-lag and multi-rater data.

Discussion:

1. I appreciate that you took the time to discuss some of the limitations with how we measure external environmental harshness in modern human populations. However, effects of internal state could also be diminished in this sample due to the way that internal state was operationalized. I appreciate that in longitudinal data, self and other report measures are much less challenging to collect (especially with children) than other types of data. However, self-report measures assume that someone has full and accurate interoception of their internal state. Some past research (see e.g., Proffitt-Leyva & Hill, 2018) has suggested that people pursuing faster LH may be less interoceptive of their internal state than slower LH strategists. This suggests that although self-report measures generally line up fairly well with physiological biomarkers of damage or allostatic load (e.g., oxidative stress, inflammatory markers, stress level), there may be more yet that physical biomarkers are able to tell us about the role internal state plays in modulating the effects of external environment. Alternately, it would be fascinating to see if biomarkers alone do not yield significant predictive effects (e.g., some interoception of poor internal state is necessary for poor internal state to hasten the effects of external environment) when thrown into an interaction model with external environment. Although this is clearly out of the scope for the current project, it may be worth giving these possibilities a nod in your discussion (perhaps in a future directions section).

Minor comments:

1. (Page 6, paragraph 2) Reference 23 is double-cited.

2. Stylistic note: you use animal and human interchangeably at times throughout the manuscript. Although humans are an animal, using both terms makes the logic a little more confusing to follow at times. Thus, it might be simpler to just keep the term human throughout when discussing your predictions, model, and implications.

3. I appreciate your transparency in sharing data. Given that these are more complex analyses, it may aid future replication attempts if you were to also share your analysis code (at least for MPlus), or at the very least, a codebook with more information about the variable names.

Dear Editor Dall,

Thank you and the two reviewers for reviewing our manuscript, External environment and internal state in relation to life history behavioral profiles of adolescents in nine countries. We have made almost all the suggested changes, and believe the revised manuscript is greatly improved and will make an interesting and useful addition to the literature. Below please find our point to point response to reviewers' comments. Thank you again for reviewing and improving our work.

Sincerely,
Authors

Reviewer 1:

1. "It has been widely observed that harsh and unpredictable external environments shape fast life histories (LH) and LH related traits [1-4]." Stearns is not an appropriate citation for this statement. He does not endorse developmental life history models.

Stearns is removed from the references here.

2. "For example, populations of the Trinidadian guppy (*Poecilia reticulata*) living downstream with high predatory risks adopt faster LH by exhibiting an earlier age and smaller size at maturity, shorter inter-brood..." Be clear whether you are referring to evolution or development of life history strategies.

We replaced this one with a new example that is clearly about LH development but not evolution.

3. "Large data studies reported in the public health literature have reached similar conclusions that adolescents with chronic medical conditions exhibit fast LH, characterized by earlier, more active, or riskier sexual activities, and other risky behaviors, such as smoking, drug use, and antisocial acts [15-18]." Please comment on whether these data suggest that faster LH strategies are causing more medical conditions, or vice versa, or whether the direction of causation cannot be established from the research designs used. This is directly relevant to assessing the internal PAR model, which assumes a pathway from somatic deterioration to faster LH strategies (not vice versa).

These are correlational studies focusing on social and sexual behaviors of adolescents suffering from chronic illness. The studies all indicate that the participating children and adolescents had a chronic illness before the participants entered the studies. Thus, the direction is from illness to behaviors, many of which are characteristic of faster LH. We made the causal direction clearer in the revised paper.

4. The paragraph beginning: "The theoretical rationale underpinning these observations follows..." This paragraph has a number of problematic statements. "Emphasizing the time lag between predictive responses to the environment or body state and adaptive LH phenotypes manifested subsequently, this process is also called external or internal predictive adaptive responses [22,23]." Unclear sentence. "Various bio-energetic trade-offs can be summarized into those between reproduction or early reproduction and prereproductive consumption for somatic development and maintenance, which, when given more, as compared to less, energetic allocation mean slow development and delayed reproduction in contrast to fast development and early reproduction." This sentence is unintelligible. "For external processes, leading causes of mortality and morbidity are extrinsic risks, such as predation, disease, and intraspecific violence." This sentence is also problematic. What is meant by "for external processes"? "The frequency and variation of extrinsic risks, known as environmental harshness and unpredictability [1], cause mortality and morbidity beyond individuals' survival efforts." This again does not make sense. I do not know what the

authors are trying to say here. “Corresponding to these two kinds of physical, reproductive strategies, there is related dual sociality.” I do not buy the distinction between physical and social aspects of LH strategies. There are coherent, integrated developmental adaptations to stress. There is no natural distinction between social and physical responses--it is all biobehavioral. In general, the authors provide a poor representation of Ellis et al. (2009), which they are drawing from. One detail after another is wrong or stated in a confusing manner. For a good recent summary of the logic underlying harshness and unpredictability, see Ellis & Del Giudice, (2019). The logic needs to be stated clearly to justify the measurement strategy.

We thank the reviewer for improving our writing and thinking. We corrected the odd sentences. We also re-wrote part of this section to eliminate the distinction between physical and behavioral manifestations of LH and, following the reviewer’s suggestion, write about different biobehavioral responses as coherent developmental adaptations. We read and cited the new review paper by Ellis and Del Giudice (2019).

5. “Their results, based on a large longitudinal U. S. American sample, showed that external adversity and internal state were each uniquely predictive of fast LH in the expected directions and the effect size of the external prediction was much larger than that of the internal prediction. Three other studies tested almost identical mediation models and yielded similar findings that external and internal predictors were each uniquely predictive of LH [35-37].” There are many problems with these statements. First, this is an oversimplification of Hartman's findings, which were mixed. Further, two of these studies were nothing like Hartman et al., as health was not included in Ellis & Essiex and health was not tested as a mediating variable in Belsky et al.. Chua et al. was all based on retrospective data that is hard to interpret in this context, as fast LH strategies could be causing health problems, rather than vice versa.

We took out Chua et al. (2016) because, as the reviewer pointed out, it is retrospective.

Ellis and Essex (2007) used BMI as a health variable and tested a mediation model: Parental Conflict → Child BMI → Child Pubertal Development.

Belsky et al. (2015) also tested a mediation model: Negative Parenting and Maternal Depression → Child Cortisol → Pubertal Development.

We now added a third study (Brumbach et al., 2009) which also contains a mediation analysis among other analyses, involving general health as the mediator in the relation between environmental unpredictability and life history.

However, as the reviewer pointed out or implied, Hartman et al. (2017) is the only study that is explicitly framed within internal vs. external PARs, and none of these studies including Hartman et al. focused on comparing the coefficients to determine which had a stronger effect. In the revised paper, we dropped the discussion about external prediction being stronger than the internal one, but focus on the potential contribution of our study, which is to test an interaction model by integrating but not replacing the existing mediation model.

6. Sample. Please describe the sample. Are these well-fed, middle-class participants. Are there substantial differences in the living conditions for participants from less developed vs more developed countries? From what population were participants recruited?

Children were recruited from both public and private schools serving high-, middle-, and low-income families approximately proportional to each site city’s socioeconomic strata. For example, Colombia has six well-defined socioeconomic strata; we sampled families from each of the six strata in the city of Medellín. Like Medellín, all the sites are large cities of a country. The sampling procedure yielded a sample with wide economic diversity that is comparable across sites. We added some of this information in the sample descriptions.

7. The major issue that I have with this paper was the use of the CBCL somatic complaints scale as an indicator of internal somatic condition. This Scale does not map on very well to the theory. Why should we believe that this represents compromised internal state, as in real illness. Given that this scale has been used a million times, there should be data indicating to what extent somatic complaints correlate with actual health measures, such as biological aging, oxidative stress, viral infections, asthma, etc. Note that somatic complaints is part of the internalizing scale, which is more often used as an indicator of LH strategy (e.g., Chua et al.). The authors need to provide a compelling argument that the Somatic Complaints subscale is indicative of health deterioration. Otherwise, they are not testing the theory.

There is much literature documenting validity information of the CBCL somatic complaints scale. For example, the scale is either significantly correlated with symptoms of a disease or can reliably separate sufferers of the disease from healthy age cohorts. For the age range of the present study, we found the said discriminant validity evidence with asthma (Gupta, Crawford, & Mitchell, 2006), diabetes (Holmes, Respass, Greer, & Frentz, 1998), recurrent abdominal pain (Dufton, Dunn, & Compas, 2008), inflammation and oxidative stress (Cunha et al., 2016), inflammatory bowel diseases (Väistö et al., 2009), and spinal bifida (Friedman, Bryant, & Holmbeck, 2007). There are additional meta-analyses summarizing similar validity evidence either in a specific disease (e.g., epilepsy, Rodenburg et al., 2005) or across a range of chronic illnesses (Pinquart & Shen, 2011). In these reports, externalizing and internalizing scales of the CBCL are also correlated with disease status, a finding that is itself consistent with the LH predictions of our paper. However, the correlations involving the somatic complaints scale are of a much larger magnitude that is on average 2 to 3 times of those involving externalizing and internalizing scales, further supporting the somatic validity (Pinquart & Shen, 2011; Rodenburg et al., 2005). Overall, the literature provides strong support for the validity of the somatic complaints scale as a measure of compromised internal state. In the measurement section of the revised paper, we added brief statements about the validity of the somatic complaints scale by citing the two meta-analyses, but not other references mentioned here, for space consideration. Of course, despite strong validity evidence, our measures are multi-informant reporting of internal states, as well as multi-informant reporting of external environments. In the revised paper, we acknowledge this as a weakness and as a unique feature of LH studies involving human participants with implications for future research.

8. Risk-taking. Why did the authors average together the 4 ratings. The first rating item directly assesses risky behavior. It is not clear to what extent the other 3 ratings do.

We used the four rating items in keeping with the original design of the scale, which is widely used in the literature, all in its original form, to assess specifically the adolescent population. The other items pertain to risk perception, or assessing consequences, and risk preference, or weighing costs and benefits (Gardner & Steinberg, 2005). Having more items helps our effort to capture the latent construct of fast LH behavioral profile, which is currently indicated by three observed variables – aggression, impulsivity, and risk taking.

Reviewer 2:

Introduction:

1. (Pages 1- 4) Although the authors do quite well at explaining life history theory, some of this information comes a little late in the introduction for a novice reader who is not as familiar with Life history or its terminology. Could some of this basic definitional information (e.g., the beginning of paragraph 2 of page 4) come earlier in the introduction? I understand the rationale and terminology well, but a lay reader or out of area reader may not.

We would like to provide descriptive examples first and then explain the logics of these examples in setting the theoretical framework of the paper. The examples are easy to understand except for the use of one phrase, “harshness and unpredictability.” We removed that phrase and simply presented the examples as representing external environments vs internal body states.

2. Did you predict in advance that external environment would be a greater predictor than internal state (hence the ordinal interaction)? Why? Is your logic based on the four external-internal integrative studies that you cited? If so, please elaborate on this point a little more.

We have changed this part in response to Reviewer 1. We no longer compare the predictive power between external and internal but emphasize testing the interaction between these two conditions as our potential contribution.

Methods:

1. You have a wonderfully diverse sample that seems to be well balanced across all collection sites. I appreciate the complexity that goes into collecting this kind of data. I also appreciate that you conducted attrition analyses. I am curious: do you have any information on whether participants who dropped out differed from participants who persisted in 1) family income (or some other indicator of SES outside of parent education), or 2) by collection site (i.e., if more of the drop-outs were from a particular site)?

Families who continued in the study did not differ in initial household income levels from families who attrited, $F(1,1081) = 1.181, p = .277$. Attrition did differ by site but was overall low, with 87% of the initial sample continuing at the last time point used in the current analyses (five and a half years after initial recruitment), and they did not differ from the initial sample on family income, education, and other variables. We added some of this information in the method section of the revised paper.

2. I'd be interested to know a little bit more about the general socioeconomic characteristics of your sample (aside from parent education) if you have that data, since you mention that children were recruited from school programs. I'd wager that in some of the countries you collected (especially the poorest ones), children from the lowest socioeconomic demographics are less likely to be enrolled in or attend school. Thus, maybe there is the potential that your sample in those countries may be somewhat limited in terms of variability in overall environmental harshness, potentially diminishing your or altering your effects slightly.

Participants were recruited through public and private schools (to increase socioeconomic diversity and representativeness of the sample) in all nine countries. Children were sampled from schools serving high-, middle-, and low-income families in the approximate proportion to which these income groups were represented in the local population. These sampling procedures resulted in an economically diverse sample that ranged from low income to high income within each site. For example, Colombia has six well-defined socioeconomic strata; we sampled families from each of the six strata in the city of Medellín (our data collection site) in the approximate proportion of their representation in the city. These are convenience samples, which despite their limitations in terms of population-wide generalizability, have several advantages in longitudinal, developmental research (Jager, Putnick, & Bornstein, 2017).

The reviewer is correct that school enrollment rates differ across the countries in our sample, although the enrollment rates differ much more in secondary school than in primary school. For example, in the Philippines, the net enrollment rate is 96% in primary school but decreases to 74% in secondary school (Philippine Statistics Authority, 2018b). The participants in our study initially were recruited when school is compulsory and free (at least

in theory) in all nine of the participating countries. For example, by the time we recruited our sample in Kenya, the government had passed reforms to make education compulsory and free from the age of 7 to 14. Costs of books, school uniforms, and the like can be prohibitively expensive for the lowest socioeconomic groups in some countries, even if school is “free,” and can be barriers to attendance (for a discussion of these issues in Kenya, see Oburu & Mbagaya, 2019). Again, we first recruited the children from primary schools that are compulsory and then followed the participants individually. We added a brief discussion of these issues in the text of the manuscript.

3. The measures used to represent external environments, internal states, and LH are appropriately chosen. Many are well-cited measures (e.g., CHAOS, BIS-11) that conceptually approximate past conceptualizations of these theoretical constructs.

We thank the reviewer for the confirmation.

Data Analysis and Results:

1. This is one of the clearest written and explained sections for SEM that I’ve read in quite some time. Each theoretical and analytical choice is well rationalized. The theoretical model, estimator decision, and analytical methods used to test it seem appropriate to the research questions and data at hand. Fit statistics are slightly diminished, but adequate for cross-lag and multi-rater data.

We thank the reviewer for the positive words. We believe this is great multi-country, multi-informant, longitudinal data best suited but rarely available for testing evolutionary hypotheses.

Discussion:

1. I appreciate that you took the time to discuss some of the limitations with how we measure external environmental harshness in modern human populations. However, effects of internal state could also be diminished in this sample due to the way that internal state was operationalized. I appreciate that in longitudinal data, self and other report measures are much less challenging to collect (especially with children) than other types of data. However, self-report measures assume that someone has full and accurate interoception of their internal state. Some past research (see e.g., Proffitt-Leyva & Hill, 2018) has suggested that people pursuing faster LH may be less interoceptive of their internal state than slower LH strategists. This suggests that although self-report measures generally line up fairly well with physiological biomarkers of damage or allostatic load (e.g., oxidative stress, inflammatory markers, stress level), there may be more yet that physical biomarkers are able to tell us about the role internal state plays in modulating the effects of external environment. Alternately, it would be fascinating to see if biomarkers alone do not yield significant predictive effects (e.g., some interoception of poor internal state is necessary for poor internal state to hasten the effects of external environment) when thrown into an interaction model with external environment. Although this is clearly out of the scope for the current project, it may be worth giving these possibilities a nod in your discussion (perhaps in a future directions section).

We thank the reviewer for the insight. Following the reviewer’s suggestion, we now added a brief statement in the last paragraph of the revised paper to acknowledge the limitation of using multi-informant reporting (because individual differences in interoception may confound the effects of body conditions) and discuss its implications for LH research (because cognitive assessment of both internal and external stressors may become integral parts of human LH calibration).

Minor comments:

1. (Page 6, paragraph 2) Reference 23 is double-cited.

We corrected this.

2. Stylistic note: you use animal and human interchangeably at times throughout the manuscript. Although humans are an animal, using both terms makes the logic a little more confusing to follow at times. Thus, it might be simpler to just keep the term human throughout when discussing your predictions, model, and implications.

We attended to this during the revision.

3. I appreciate your transparency in sharing data. Given that these are more complex analyses, it may aid future replication attempts if you were to also share your analysis code (at least for MPlus), or at the very least, a codebook with more information about the variable names.

We provided Mplus code and self-explanatory information for variable names in the data file.

- Belsky, J., Ruttle, P. L., Boyce, W. T., Armstrong, J. M., & Essex, M. J. (2015). Early adversity, elevated stress physiology, accelerated sexual maturation, and poor health in females. *Developmental Psychology, 51*, 816–822. doi:10.1037/dev0000017
- Brumbach, B. H., Figueredo, A. J., & Ellis, B. J. (2009). Effects of harsh and unpredictable environments in adolescence on development of life history strategies. *Human Nature, 20*, 25-51.
- Chua, K. J., Lukaszewski, A. W., Grant, D. M., & Sng, O. (2016). Human life history strategies: Calibrated to external or internal cues?. *Evolutionary Psychology, 15*(1), 1474704916677342. Doi: 10.1177/1474704916677342
- Cunha, G. R., Asevedo, E., Mansur, R. B., Zugman, A., Pan, P. M., Gadelha, A., ... & Cogomora, H. (2016). Inflammation, neurotrophism and oxidative stress and childhood psychopathology in a large community sample. *Acta Psychiatrica Scandinavica, 133*, 122-132.
- Dufton, L. M., Dunn, M. J., & Compas, B. E. (2008). Anxiety and somatic complaints in children with recurrent abdominal pain and anxiety disorders. *Journal of pediatric psychology, 34*, 176-186.
- Ellis, B. J., & Essex, M. J. (2007). Family environments, adrenarche, and sexual maturation: A longitudinal test of a life history model. *Child Development, 78*, 1799–1817. doi:10.1111/j.1467-8624.2007.01092.x
- Friedman, D., Bryant, F. B., & Holmbeck, G. N. (2007). Testing the factorial invariance of the CBCL somatic complaints scale as a measure of internalizing symptoms for children with and without chronic illness. *Journal of Pediatric Psychology, 32*, 512–516.
- Gardner, M., & Steinberg, L. (2005). Peer influence on risk taking, risk preference, and risky decision making in adolescence and adulthood: an experimental study. *Developmental psychology, 41*, 625.
- Gupta, S., Crawford, S. G., & Mitchell, I. (2006). Screening children with asthma for psychosocial adjustment problems: a tool for health care professionals. *Journal of Asthma, 43*, 543-548.
- Hartman, S., Li, Z., Nettle, D., & Belsky, J. (2017). External-environmental and internal-health early-life predictors of adolescent development. *Development and Psychopathology, 29*, 1839–1849. Doi: 10.1017/S0954579417001432
- Holmes, C. S., Respass, D., Greer, T., & Frentz, J. (1998). Behavior problems in children with diabetes: Disentangling possible scoring confounds on the Child Behavior Checklist. *Journal of Pediatric Psychology, 23*, 179-185.
- Jager, J., Putnick, D. L., & Bornstein, M. H. (2017). More than just convenient: The scientific merits of homogeneous convenience samples. *Monographs of the Society for Research in Child Development, 82*, 13-30. doi: 10.1111/mono.12296

- Oburu, P., & Mbagaya, C. (2019). Education and parenting in Kenya. In E. Sorbring & J. E. Lansford (Eds.), *School systems, parent behavior, and academic achievement: An international perspective* (pp. 67-78). Cham, Switzerland: Springer.
- Philippine Statistics Authority (2018b). SDG watch Philippines. Available from <https://psa.gov.ph/sdg/Philippines/baselinedata/4%20Quality%20Education>
- Pinquart, M., & Shen, Y. (2011). Behavior problems in children and adolescents with chronic physical illness: a meta-analysis. *Journal of pediatric psychology*, *36*, 1003-1016.
- Rodenburg, R., Stams, G. J., Meijer, A. M., Aldenkamp, A. P., & Dekovic, M. (2005). Psychopathology in children with epilepsy: A meta-analysis. *Journal of Pediatric Psychology*, *30*, 453-468.
- Väistö, T., Aronen, E. T., Simola, P., Ashorn, M., & Kolho, K. L. (2009). Psychosocial symptoms and competence among adolescents with inflammatory bowel disease and their peers. *Inflammatory Bowel Diseases*, *16*, 27-35.