

Personality traits are less stable than people think

Corresponding Author: Dr Amanda Wright

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

Version 0:

Decision Letter:

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Dear Dr Wright,

Thank you for submitting your manuscript titled "Personality traits are less stable than people think" to Communications Psychology. We have given the paper our careful consideration and find it of potential interest. However, due to certain shortcomings we are concerned that sending the current manuscript out to review could lead to unnecessary delays and quite possibly an undesirable outcome of the review process.

In particular, detailed report of the results of the estimated models are missing for both study 1 and 2. While the manuscript refers to supplementary tables, these are unfortunately missing from the uploaded files. Additionally, even if result summaries are provided in supplementary information, it would be helpful to include statistics for key statements (e.g., p. 7 "At the domain level, the Big Five were rated as more stable than all other domains (Figure 1C)." in the results section as well. Please ensure you follow our statistical guidelines when reporting statistics (<https://www.nature.com/commpsychol/submit/submission-guidelines#statistical-guidelines>). We request all statistics be reported in full. That is, for frequentist inferential statistics, we expect reporting as statistic(degrees of freedom) = value, p = value, effect size statistic = value, % Confidence Intervals = values. Please report exact p-values unless $p < 0.001$.

Furthermore, please clarify if you ethic approval was obtained or waived for Study 1.

We would therefore like to invite you to revise your manuscript to address these concerns before we make a final determination on whether to send your manuscript for external review.

We shall hope to receive your revised version as soon as you are able to complete the suggested revisions. If something similar is published in the interim we will have to consider the impact it has on the novelty of a revised manuscript.

If you anticipate a delay of more than four weeks, please let us know. Should your manuscript be substantially delayed without notifying us in advance and your article is eventually published, the received date may be that of the revised, not the original, version.

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

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 when you are prepared to resubmit.
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Thank you for your interest in Communications Psychology.

Best regards,
Yana Fandakova

Yana Fandakova
External Editor

Version 1:

Decision Letter:

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Dear Dr Wright,

Thank you for your patience during the peer-review process. Your manuscript titled "Personality traits are less stable than people think" has now been seen by 2 reviewers, and I include their comments at the end of this message. They find your work of interest but raised some important points. We are interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response to the reviewers. Please highlight all changes in the manuscript text file.

Editorially, we consider the issues around the ergodicity assumption and reliability in study 2 critical and they should be addressed in a revision. The revised manuscript should also substantially revise the discussion and address points and limitations pointed out by the reviewers.

As you revise the manuscript in response to these issues, please also implement all requests in the attached Mandatory Revision Requests document. All requirements listed in this document need to be fully met, or the work will be returned to you for further revisions without peer review. This workflow is in place to increase the likelihood that the paper will be accepted for publication. It reduces the number of rounds of revision (and review) and ensures that the reviewers vet a version of the article that is compliant with journal policies. If you have any questions regarding the required revisions, please contact the journal prior to resubmission to avoid a negative outcome.

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

Please submit the following items:

- Revised manuscript
- Point-by-point response to the referees' comments
- Mandatory Revision Requests Table (attached).
- Cover letter (as a separate document)

via this link: [Link Redacted](#) .

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

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

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

Best regards,

Yana Fandakova

Yana Fandakova
External Editor
Communications Psychology

REVIEWER EXPERTISE:

personality traits and development
longitudinal and metanalytic data analysis

REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

I think this is a great paper and very close to – or entirely ready for – publication.

I only have two comments.

First, I think there is evidence that mean-level age differences in personality traits have been over-estimated in studies solely relying on self-reports. This is probably because people tend to respond in a socially more desirable manner to questionnaire items as they get older (for example, see Ausmees et al. 2022 for empirical evidence and a broader discussion; <https://doi.org/10.1016/j.jrp.2022.104245>). Age differences tend to appear quite a bit smaller in magnitude, albeit similar in pattern, according to reports by other people – that is, people may report/feel that they change (mature) more than they are seen changing by others. If so, the empirical personality trait changes may be (quite substantially) overestimated, whereas the real changes may be more in line with public perceptions. In any case, public perceptions may be conceptually closer to informant's perceptions rather than self-perceptions. I think authors may consider discussing this among the limitations.

Second, the discussion in p. 25 ("Third, ...") that mean-level changes may make people less predictable may require more nuance. One could argue that mean-level personality trait changes that most lay people are familiar with have either no implications for how predictable people are, or may even increase predictability of people's behaviour. For example, people tend to know that themselves and others become, on average, somewhat more conscientious between young and later adulthood (personality trait age stereotypes reflect empirical mean differences), and this knowledge may help them to (somewhat accurately) predict how an older person might behave compared to a young adult.

Reviewer #2 (Remarks to the Author):

Review Com Nat Psycholog

I enjoyed reading the article and very much appreciate the methodological rigor achieved by the excellent preregistration. Nevertheless, I have three major concerns concerning study 2 (I consider study 1 as more of a motivational starting point). Especiall 1. could have been addressed at time of preregistration and I realize that it is not ideal to address these at this stage.

Nevertheless, I believe that the validity of conclusions outweigh the disadvantages of having to deviate substantially from the preregistration.

Major:

1. Violation of Ergodicity: Fixed Effects Do Not Measure Individual Change

The authors' central claim concerns "how much each variable changes across the lifespan."

However, they operationalize change as differences in fixed effect predictions across ages (e.g., predicted value at age 2 minus age 1).

This conflates population-average trajectories with individual change, assuming ergodicity—that the population mean trajectory equals the average individual trajectory.

This assumption is violated when individuals follow heterogeneous developmental paths.

Critically, fixed effects can show zero change even when all individuals change substantially:

50% of individuals: +1.0 SD change

50% of individuals: -1.0 SD change

Fixed effect: 0.0 SD (no population change)

Average individual change: 1.0 SD (substantial individual change)

The opposite, that fixed effect change is present but not individual change is present, can also happen due to attrition, changes in measurement or reliability, or in this specific case purely due to binning of age groups.

The non-ergodic nature of developmental processes means the authors cannot support claims about "how much individuals change" using population-level fixed effects. They should instead report:

* Person-specific change estimates (random slopes from their mixed models)

* The distribution of individual trajectories (mean, SD, range of person-specific slopes)

* The degree of heterogeneity (SD of random slopes relative to fixed effect)

This issue gets compounded for nonlinear trajectories.

I am open to the possibility that I misunderstood the way "change" is operationalized and that the authors actually are calculating average change and not change of the average since I am not familiar with the particular analytic approach and have not had the time to review the analysis code as well.

2. Reliability is ignored

The authors ignore reliability and potentially very different reliabilities between studies.

For the chosen approach of change of the average this underestimates change, for approaches aimed at average change this overestimates change.

3. Discussion goes far beyond the evidence

The question: "Why, then, do people continue to see personality traits as fixed?" is not answered/addressed by the empirical evidence at all but speculation almost fills half of the discussion.

Furthermore, the implications section only discusses changeability not change.

Those should not be equated.

If you were to observe that I never eat greens, hence no change over time, it would be wrong to infer that no intervention could change that.

Or if you observe that older people get more health problems (health changes), it would be unwarranted to assume that hence it is easy to make everyone healthy (changeability).

I suggest that the authors avoid speculation and concentrate on what their evidence shows, which is plenty interesting.

Also I expect that a discussion is open about possible limitations of the study.

Exposing them is not a weakness but shows rigorous thinking and understanding of the subject matter.

Minor:

> Each model estimated mean ratings per level of its included factor variable in 136 standardized units (Cohen's d)

This is not Cohen's D. A rating divided by standard deviation of an individual, then averaged has the unit of "standard deviations" but is not a standardized mean difference between two samples.

> An a priori power analysis indicated that with $n = 140$
1,000, we would have 80% power at $\alpha = .01$ to detect differences of $|d| = 0.108$ in people's 141 change ratings.

This seems rather high. Does this include the p-value adjustment?

> we ran two no-intercept regression models.

Perhaps rather say intercept fixed to zero.

> First, variable-level change ratings were predicted by a factor variable containing 25 levels, one for each variable.

This is an unusual but clever choice. Perhaps the authors should expose the reasoning and state that this is equivalent to calculating 25 means.

> with a median completion time of 4.6 minutes.

I would like to know how many items they answered, how much text they were supposed to read, and if the pay was hourly or for completion.

Signed,
Aaron Peikert

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Version 2:

Decision Letter:

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

Dear Dr Wright,

Thank you for your patience during the peer-review process. Your manuscript titled "Personality traits are less stable than people think" has now been seen by 1 reviewer, and I include their comments at the end of this message. Unfortunately, Reviewer 2 was not available to submit a report. Therefore, we consulted with the lead statistics expert on the editorial board, and their comments are also appended below. You will see that both find your work improved, and we remain interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to the remaining concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response to the reviewers. Please highlight all changes in the manuscript text file.

Editorially, we consider it important that the interpretation of the findings reflect what can be shown by the data including a clear distinction between change and changeability.

As you revise the manuscript in response to these issues, please also implement all requests in the attached Mandatory Revision Requests document. All requirements listed in this document need to be fully met, or the work will be returned to you for further revisions without peer review. This workflow is in place to increase the likelihood that the paper will be accepted for publication. It reduces the number of rounds of revision (and review) and ensures that the reviewers vet a version of the article that is compliant with journal policies. If you have any questions regarding the required revisions, please contact the journal prior to resubmission to avoid a negative outcome.

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- Cover letter (as a separate document)

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that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, provided it still presents a significant contribution to the literature at that stage.

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

Best regards,

Yana Fandakova

Yana Fandakova
External Editor
Communications Psychology

REVIEWER EXPERTISE:
personality traits and development
longitudinal and metanalytic data analysis

REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

I congratulate authors for the interesting work. I have no more comments.

Statistics advisor:

The authors demonstrate genuine methodological sophistication, particularly in the reliability correction and power analysis. However, the ergodicity response navigates around, rather than through, the most fundamental challenge, and the change/changeability distinction remains underaddressed. A careful reviewer would likely accept this revision conditionally, with pointed requests to verify that language throughout the manuscript consistently reflects the population-average framing and that the Discussion explicitly distinguishes change from changeability in the implications. The paper is stronger than the response letter alone suggested, particularly in the reliability analysis. The core findings that laypeople systematically underestimate personality trait change relative to other variables are robustly demonstrated across multiple analytic approaches. The main residual weaknesses are the change/changeability conflation in the discussion and the inherent limitation of not estimating person-specific developmental slopes, both of which are disclosed if not fully resolved.

The change/changeability conflation, however, is still visible in the paper. The discussion states that "highlighting the changeability of traits may empower individuals to pursue positive change and encourage policies or interventions that support personal growth". This directly follows from findings about normative developmental change over 75 years, not interventional changeability. The logical leap is present. Similarly, calling personality "less stable than people think" in the title and conclusion, then pivoting to implications for intervention and social programs, imports an assumption that normative age-graded change warrants optimism about directed intervention. This conflation is subtler than the reviewer's garden-hose/health analogy, but it is structurally the same problem. I'd ask the authors to more clearly rewrite this section to explicitly distinguish change from changeability in the implications.

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Version 3:

Decision Letter:

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

Dear Dr Wright,

Your manuscript titled "Personality traits are less stable than people think" has now been editorially reviewed, and I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Psychology.

We therefore invite you to revise your paper one last time to address a list of editorial requests. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table". Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

SUBMISSION INFORMATION:

In order to accept your paper, we require the files listed here <https://www.nature.com/documents/commsj-file-checklist.pdf>.

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*** CODE AVAILABILITY:** All Communications Psychology manuscripts must include a section titled "Code Availability" at the end of the methods section. We require that the custom analysis code supporting your conclusions is made available in a publicly accessible repository at this stage; please choose a repository that generates a digital object identifier (DOI) for the code; the link to the repository and the DOI must be included in the Code Availability statement. Publication as Supplementary Information will not suffice.

* DATA AVAILABILITY:

All Communications Psychology manuscripts must include a section titled "Data Availability" at the end of the Methods section. More information on this policy, is available in the Editorial Requests Table and at <http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf>.

Please use the following link to submit the above items:

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reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Jennifer Bellingtier

Jennifer Bellingtier, PhD
Senior Editor
Communications Psychology

Yana Fandakova
External Editor
Communications Psychology

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November 19th, 2025

Prof. Dr. Yana Fandakova
External Editor, *Communications Psychology*

Dear Prof. Dr. Fandakova,

Thank you for the opportunity to revise our manuscript for further consideration at *Communications Psychology*. We have resolved the issue with the supplementary tables by providing a mixture of our longer tables via supplementary data and our shorter tables via a combined supplementary tables file. Additionally, we have ensured that all model-provided estimates and statistical tests are reported in line with the journal's reporting guidelines. Lastly, we clarified the ethical approval for Studies 1 and 2.

Please see our responses to each comment below. Thank you again for consideration of our manuscript for *Communications Psychology*.

Thank you for submitting your manuscript titled "Personality traits are less stable than people think" to Communications Psychology. We have given the paper our careful consideration and find it of potential interest. However, due to certain shortcomings we are concerned that sending the current manuscript out to review could lead to unnecessary delays and quite possibly an undesirable outcome of the review process.

In particular, detailed report of the results of the estimated models are missing for both study 1 and 2. While the manuscript refers to supplementary tables, these are unfortunately missing from the uploaded files. Additionally, even if result summaries are provided in supplementary information, it would be helpful to include statistics for key statements (e.g., p. 7 "At the domain level, the Big Five were rated as more stable than all other domains (Figure 1C)." in the results section as well. Please ensure you follow our statistical guidelines when reporting statistics (<https://www.nature.com/commpsychol/submit/submission-guidelines#statistical-guidelines>). We request all statistics be reported in full. That is, for frequentist inferential statistics, we expect reporting as statistic(degrees of freedom) = value, p = value, effect size statistic = value, % Confidence Intervals = values. Please report exact p-values unless $p < 0.001$.

RESPONSE: We have uploaded the original supplementary tables that were in aggregate Excel format as a mixture of individual Excel sheets of supplementary data (for the larger tables) and a Word document containing the remaining shorter supplementary tables.

Regarding providing statistics for key statements, we apologize for our previous omission of such statements. For those such statements in which we did not provide accompanying numerical information to support them, we typically instead relied upon a figure that was mentioned alongside these statements to contain and convey this supporting information. For example, in the above quoted sentence on page 7, Figure 1C depicts the contrast estimates for the Big Five versus all other domains, including the point estimate of the contrast and its 99% confidence interval. However, we recognize that some readers may prefer numerical estimates in-text as opposed to relying on figures or supplementary information.

Thus, we have added a table to the manuscript for Study 1 (Table 1; pg. 8) and we additionally provide further numerical information in the text throughout the remainder of the Results section. Due to the inclusion of Table 1 for some of Study 1's results, though, we do not write out this same statistical information in-text so as to keep the results content concise and avoid redundancy with information in Table 1. Namely, when describing specific results for different variables, we write out the effect size but

do not write out the accompanying test statistic, degrees of freedom, p value, nor 99% confidence intervals as this information is included in Table 1. Similarly, for Study 2, we do not repeat the confidence intervals associated with effect sizes of different variables in-text because this information is provided in Table 3. For contrast results on Study 1, though, we do write out all statistical information as well as continue to refer to the supplemental materials for full results (Table S3).

Additionally, we have ensured that in the supplementary information (i.e., tables and data), all statistical information included in the journal's reporting guidelines are presented for every model. Previously, the test statistics and degrees of freedom were omitted from these tables. This has been fixed for the revision.

Furthermore, please clarify if you ethic approval was obtained or waived for Study 1.

RESPONSE: At the end of the manuscript (before the references), we now more clearly specify this under the "Ethics Declarations" header:

"Authors declare no conflicts of interest. Both studies were exempt/waived from ethical review because they collected anonymous survey data (Study 1) or used preexisting anonymized panel data (Study 2)."

The University of Zurich (the institution affiliated with all authors) does not oversee nor issue ethics decisions for studies (i.e., due to ruling them as exempt from review) that use secondary data nor that collect anonymous survey data unrelated to any health-related information and that do not attempt to understand diseases or the human body's structure/function (<https://www.research.uzh.ch/en/procedures/research-on-humans.html>).

March 31st, 2026

Prof. Dr. Yana Fandakova
External Editor, *Communications Psychology*

Dear Prof. Dr. Fandakova,

Thank you for the opportunity to revise our manuscript (COMMSPSYCHOL-25-0829A) for further consideration at *Communications Psychology*. Please see our responses to each comment below. Thank you again for consideration of our manuscript for *Communications Psychology*.

EDITOR:

Thank you for your patience during the peer-review process. Your manuscript titled "Personality traits are less stable than people think" has now been seen by 2 reviewers, and I include their comments at the end of this message. They find your work of interest but raised some important points. We are interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response to the reviewers. Please highlight all changes in the manuscript text file.

Editorially, we consider the issues around the ergodicity assumption and reliability in study 2 critical and they should be addressed in a revision. The revised manuscript should also substantially revise the discussion and address points and limitations pointed out by the reviewers.

RESPONSE: Thank you for the opportunity to revise our manuscript. To address the comments we received, we have:

- (1) Revised all analyses for Study 2, thereby better accounting for individual-level variability in single-dataset models and more appropriately estimating dependent age estimates within studies in the meta-analytic models;
- (2) Added further clarifying details to the Methods that were requested in reviewer comments and that also expanded upon our changes we made for the revision;
- (3) Revised the Discussion to remove speculative claims and more clearly frame findings from past research as providing an empirical and theoretical foundation for findings of the present study; and
- (4) Greatly expanded the limitations section to describe the study's limitations in full, connect these to possible routes for future research, and to address any remaining reviewer comments concerning the limitations.

As you revise the manuscript in response to these issues, please also implement all requests in the attached Mandatory Revision Requests document. All requirements listed in this document need to be fully met, or the work will be returned to you for further revisions without peer review. This workflow is in place to increase the likelihood that the paper will be accepted for publication. It reduces the number of rounds of revision (and review) and ensures that the reviewers vet a version of the article that is compliant with journal policies. If you have any questions regarding the required revisions, please contact the journal prior to resubmission to avoid a negative outcome.

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

Please submit the following items:

- *Revised manuscript*
- *Point-by-point response to the referees' comments*
- *Mandatory Revision Requests Table (attached).*
- *Cover letter (as a separate document)*

RESPONSE: We have attached the completed Mandatory Revision Requests document confirming these changes were made, as well as the revised manuscript with highlighted changes, point-by-point responses to reviewers, and a separate cover letter.

Reviewer #1 (Remarks to the Author):

I think this is a great paper and very close to – or entirely ready for – publication. I only have two comments.

RESPONSE: Thank you for the kind words about our study. Please see our responses to your comments below.

First, I think there is evidence that mean-level age differences in personality traits have been over-estimated in studies solely relying on self-reports. This is probably because people tend to respond in a socially more desirable manner to questionnaire items as they get older (for example, see Ausmees et al. 2022 for empirical evidence and a broader discussion; <https://doi.org/10.1016/j.jrp.2022.104245>). Age differences tend to appear quite a bit smaller in magnitude, albeit similar in pattern, according to reports by other people – that is, people may report/feel that they change (mature) more than they are seen changing by others. If so, the empirical personality trait changes may be (quite substantially) overestimated, whereas the real changes may be more in line with public perceptions. In any case, public perceptions may be conceptually closer to informant's perceptions rather than self-perceptions. I think authors may consider discussing this among the limitations.

RESPONSE: We appreciate the reviewer linking an interesting study on this topic. Notably, though, if evaluative biases are present, these should occur for all variables with socially desirable response poles, not just personality. These variables in our study, especially those related to traits like neuroticism (anxiety, self-esteem, depression, life satisfaction, etc.) or extraversion (positive affect), should have the same potential to be impacted by these.

Meta-analyses and cross-sectional, cross-cultural, and longitudinal studies also do not universally favor more socially desirable levels, changes, nor larger changes in general for self-reports (e.g., Allik et al., 2010; Kim et al., 2019; Lenhausen et al., 2024; Oltmanns et al., 2020), with the opposite trend rather often emerging. In Ausmees et al. (2022), 17 facets did decrease for self- to other-reports for cross-sectional age differences, but 13 stayed the same or became more pronounced. Conscientiousness was also highly rated as socially desirable, and its age-related “increases” were equally observed across methods. This trait had the largest changes in our study, suggesting this largest discrepancy of empirical versus perceived changes is robust and would not change.

Regarding change perceptions, we agree our public perceptions are conceptually closer to informant perceptions, as we instructed people to not rate any changes they may have perceived thus far in life. However, people expect others to change more than they will, with these changes being in socially desirable and undesirable directions. For instance, across the lifespan, people expect others to increase more in A, C, & ES + decrease more in E & O than they expect themselves to (Guttral et al., 2022). Similarly, the landmark Quoidbach study found people of all ages don't expect to change on psychological attributes like personality, values, and attitudes (Quoidbach et al., 2013). Overall, agreement likely would be worse if we used self-perceived / expected changes as these tend to be smaller. If self-report, mean-level personality changes are larger than informant-report changes (as the comment suggests), then this alongside using other-focused expected changes should have provided the best chance for laypeople to accurately perceive changes in personality traits. This was still not the case, however, as underestimating personality trait change occurred regardless.

We agree that more closely examining these discrepancies (and reasons for these) is a key avenue for future research, though. For the revision, we have restructured and greatly expanded upon our limitations section (pgs. 29-31) to describe these considerations and detail multiple future directions that the present study cannot address. For instance, we mention exploring possible differences in self- versus other-reported (a) longitudinal changes in variables and (b) perceived changes, and how these may differ across variables. Self-expected changes in personality are small (Quoidbach et al., 2013) yet people expect their well-being will change (Lachman et al., 2008), but this may differ for health-related variables, for example. We believe the suggested additions helped enrich the discussion of our study's scope and make clearer its limitations, while also laying out several avenues for future research. Thank you to the reviewer for this suggestion!

Second, the discussion in p. 25 (“Third, ...”) that mean-level changes may make people less predictable may require more nuance. One could argue that mean-level personality trait changes that most lay people are familiar with have either no implications for how predictable people are, or may even increase predictability of people’s behaviour. For example, people tend to know that themselves and others become, on average, somewhat more conscientious between young and later adulthood (personality trait age stereotypes reflect empirical mean differences), and this knowledge may help them to (somewhat accurately) predict how an older person might behave compared to a young adult.

RESPONSE: Thank you for raising this interesting point. We agree that individual people exhibiting normative age trends in personality may have no bearing on how predictable they are to others over long periods. This point was intended to be framed at the level of frequent, short-term interactions, whereby people’s thoughts, feelings, and behaviors are more variable (i.e., dynamic) and other people’s expectations for these would then become more relevant.

However, as our data speak to long-term changes, any implications pertaining to these shorter-term interactions are not relevant / are outside of the scope of our study. We have thus removed this entire “Third” point from the Discussion.

Thank you to Reviewer 1 for their time in reviewing our study, and for helping us improve the discussion of our study and outlining important directions for future research!

Reviewer #2 (Remarks to the Author):

I enjoyed reading the article and very much appreciate the methodological rigor achieved by the excellent preregistration. Nevertheless, I have three major concerns concerning study 2 (I consider study 1 as more of a motivational starting point). Especiall 1. could have been addressed at time of preregistration and I realize that it is not ideal to address these at this stage. Nevertheless, I believe that the validity of conclusions outweigh the disadvantages of having to deviate substantially from the preregistration.

RESPONSE: Thank you for the kind words about our paper and our efforts at preregistration.

Major:

1. Violation of Ergodicity: Fixed Effects Do Not Measure Individual Change

The authors' central claim concerns "how much each variable changes across the lifespan." However, they operationalize change as differences in fixed effect predictions across ages (e.g., predicted value at age 2 minus age 1). This conflates population-average trajectories with individual change, assuming ergodicity—that the population mean trajectory equals the average individual trajectory. This assumption is violated when individuals follow heterogeneous developmental paths. Critically, fixed effects can show zero change even when all individuals change substantially:

50% of individuals: +1.0 SD change

50% of individuals: -1.0 SD change

Fixed effect: 0.0 SD (no population change)

Average individual change: 1.0 SD (substantial individual change)

The opposite, that fixed effect change is present but not individual change is present, can also happen due to attrition, changes in measurement or reliability, or in this specific case purely due to binning of age groups. The non-ergodic nature of developmental processes means the authors cannot support claims about "how much individuals change" using population-level fixed effects. They should instead report:

** Person-specific change estimates (random slopes from their mixed models)*

** The distribution of individual trajectories (mean, SD, range of person-specific slopes)*

** The degree of heterogeneity (SD of random slopes relative to fixed effect)*

This issue gets compounded for nonlinear trajectories.

I am open to the possibility that I misunderstood the way "change" is operationalized and that the authors actually are calculating average change and not change of the average since I am not familiar with the particular analytic approach and have not had the time to review the analysis code as well.

RESPONSE: We agree individual- and population-level changes are not identical. However, this study focused on how people believe different variables change over the lifespan and the (mis)alignment of these beliefs with variables' average lifespan trajectories. It was not the extent to which individual people change and/or whether they believe they themselves change. Moreover, we do not make inferences from the average lifespan change trajectory to any specific individual(s), which is where the ergodicity assumption becomes especially important. The survey in Study 1 intentionally asked people to report how much they believe each variable changed over the lifespan, on average; *not* to report changes they themselves may have experienced up until this point in their lives. Consistent with this framing, we then analyzed average trajectories of variables.

For the revision, we have revised our analyses in the following ways to ensure we best captured lifespan changes in our variables:

- (1) Redid all single-dataset analyses to include person-specific random effects (see pgs. 13-14) and
- (2) Jointly meta-analyzed all age-specific estimates across studies with nested random effects structures to account for dependency in individuals' age-specific values over time (see pg. 14).

Our single-dataset models thus now provide estimates of individual-level heterogeneity in person-typical ratings for variables per dataset. Age estimates are then weighted averages of responses, adjusting for systematic mean-level differences across people, periods, and cohorts, that also account for within-cluster correlations across time inherent in longitudinal data.

A caveat to the “average” focus is that with MLMs, average trajectories are based on weighted averages of fixed effects across nesting variables in a model's random-effects structure. In our case, for the revision, this now also included individuals. Moreover, we did not bin multiple ages into groups. Estimates were provided per (year of) age, weighted across people, years, and cohorts. Estimating trajectories in a no-intercept framework via a series of person-level means (i.e., intercepts) per age is statistically identical to estimating them with a grand intercept (e.g., average level at age 40) and slope parameters indexing change at each age relative to the grand intercept (e.g., difference in average values at age 40 versus age 50). Thus, independent fixed effects were not subtracted from each other to obtain change estimates. Rather, dependent estimates across ages and datasets were used to obtain a single lifespan trajectory.

The complexity of lifespan change—and to ensure we did not simplify forms of change—guided our decision to fit flexible trajectories across all ages. It is not possible to estimate random intercepts per age nor random slopes across ages, and a potential loss of individual-level information is inherent to a nomothetic approach focused on typical trends.

The reviewer is correct however that this masks nuanced person-level findings. We added content in the limitations about our broad focus and the added value of more individualized approaches, by also specifically linking this with other metrics of change:

“Third, mean-level stability is only one way to conceptualize change. Incorporating other metrics like rank-order stability⁵ or ipsative consistency⁷⁷ would offer a more holistic view of lifespan development. Fourth, we focused on average trajectories, which neglect more nuanced individual-level changes. Utilizing nomothetic and idiographic approaches⁷⁸ would offer the most comprehensive insight on perceived versus “actual” lifespan change. For instance, life events, on average, have small and inconsistent effects on rank-order stability⁸¹, mean-level trait change^{22,79,80}, and ipsative consistency^{82,83}. However, there is heterogeneity in the latter two, though, such that some changes do occur, and research tends to find that people's perceived changes, for both event-related and normative change, align with observed changes^{84,85}. Thus, people who recognize personality change in themselves may also rate personality as, in general, changing more than people who do not perceive such changes. Identifying differences in average and person-level alignment of perceived and observed changes may help speak to mechanisms driving these beliefs.” (pg, 30)

For our study, though, the correspondence between the nomothetic focus of our research question and the chosen analytic approach is a key consideration. We hope our revisions to account for person-level heterogeneity in trends and correlated values over time remedy the key concerns in the above comment.

2. Reliability is ignored

The authors ignore reliability and potentially very different reliabilities between studies.

For the chosen approach of change of the average this underestimates change, for approaches aimed at average change this overestimates change.

RESPONSE: Thank you for bringing this up. For the revision, we now provide meta-analytic models that correct

for dataset-specific reliability in our 14 variables assessed with composite scales. Specifically, we corrected for reliability by adjusting for the internal consistency of these variables' scales in each dataset. For each variable, observed effect sizes from each single-dataset model were divided by the square root of the dataset's average Cronbach's α (i.e., as an index for reliability) across waves. Sampling variability for corrected estimates was obtained by dividing the original sampling variability by the raw Cronbach's α .

We continue to provide original, uncorrected estimates from all meta-analytic models, too. We explain these corrected models in the Method for Study 2 (pgs. 11-12). The primary results in the paper for Study 2 now use reliability-adjusted values, and we provide a new section at the end of the Results to detail the robustness of our findings across the reliability-corrected and uncorrected models (pgs. 24-25 & Fig. 5). We additionally provide comparisons in these values for the US-only models (pgs. 25-26). Lastly, we include a limitation about the reliability-corrected values in the Discussion and a suggestion for future research (pgs. 29-30).

Ultimately, individual estimates changed in highly systematic, predictable ways when adjusting for reliability, with little overall impact on variables' relative standings of change estimates. Importantly, the correction also had no impact on the degree of misalignment between perceived versus empirical changes in personality traits.

3. Discussion goes far beyond the evidence

The question: "Why, then, do people continue to see personality traits as fixed?" is not answered/addressed by the empirical evidence at all but speculation almost fills half of the discussion. Furthermore, the implications section only discusses changeability not change. Those should not be equated. If you were to observe that I never eat greens, hence no change over time, it would be wrong to infer that no intervention could change that. Or if you observe that older people get more health problems (health changes), it would be unwarranted to assume that hence it is easy to make everyone healthy (changeability).

I suggest that the authors avoid speculation and concentrate on what their evidence shows, which is plenty interesting. Also I expect that a discussion is open about possible limitations of the study. Exposing them is not a weakness but shows rigorous thinking and understanding of the subject matter.

RESPONSE: For the revision, we removed and tempered claims in the Discussion that were speculative or beyond the scope of our study. For example, we removed the "Why, then, do people..." question to not give readers the impression that we can provide answers to that question. Additionally, we completely removed a prior discussion point that was beyond our focus and the scope of our long-term data (see response to comment R1.2).

Additionally, we added further context and framing for the details we do discuss as being what past research has examined regarding beliefs about change versus stability, potential drivers of these beliefs, and their associations with personality specifically. Examples of these additions are copied and pasted below:

"Consistent with past work^{54,55}, this suggests the way people think about certain personal attributes is distinct from other albeit related qualities. Our study expanded upon this idea by showing that beliefs related to stability versus changeability of personality traits are uniquely different from variables belonging to more distinct life domains (e.g., finances, leisure) and that are more closely related to personality traits (e.g., life satisfaction, affect). Informed by prior theoretical and empirical work, it is likely that these widespread, personality-specific views are driven and reinforced by a mix of cognitive, social, and cultural factors." (pg. 27)

"Empirically, such essentialist beliefs are found to uniquely apply to personality traits^{54,56}. For example, people's ratings for how discrete, deeply inherent, immutable, biologically based, consistent, and consequential different characteristics are systematically differ for personality traits and other attributes⁵⁴." (pg. 27)

"For instance, essentialist personality beliefs are empirically linked to endorsements of inaccurate stereotypes

and harmful beliefs about others' life circumstances⁵⁶. Such views may reinforce tendencies to attribute others' intrinsic qualities as being responsible for negative circumstances in their lives⁶¹ and thus deter support for programs that offer social assistance.” (pg. 29)

Overall, though, a Discussion should detail potential explanations for and implications of a study's observed effects as informed by past work. We do thus describe past research examining beliefs about change, factors associated with these, and their consequences (e.g., Furnham & Sherman, 2023; Gutral et al., 2022; Haslam et al., 2007; Levy et al., 1998; Quoidbach et al., 2013). For example, the connection between changeability and change is that substantial research finds meaningful links between beliefs about the possibility/feasibility of change and subsequent change, general adjustment, and self-/other-focused attitudes (e.g., Dweck, 2008; Furnham & Sherman, 2023; Haslam et al., 2004; Hennecke et al., 2014; Robins et al., 2005; Yeager et al., 2014). We did not falsely equate changeability and change; rather, we discussed our findings about change (Study 2), beliefs about changeability (Study 1), and the relevant empirical and practical considerations tied to these findings.

The reasons put forth for why people continue to see personality as fixed are speculative to the degree that they apply to our specific data. Their empirical existence and general applicability to personality are not speculative, though, but are inherent in existing studies cited alongside them. The cumulative nature of research hinges on the idea that past work can and should inform current and future findings.

In addition to our noted revisions above, we have also added to the limitations section that future research is needed to examine these ideas in the context of our specific study/data to confirm their applicability (pg. 31: *Sixth, and relatedly, although past research has examined beliefs about change, factors associated with these beliefs, and consequences of these beliefs, these findings may not generalize to our specific data. Together, research that incorporates multiple report and comparison methods for different types of variables and that jointly examines beliefs about change, reasons for these beliefs, and discrepancies between “actual” and perceived changes in different variables is needed.”*).

Minor:

> *Each model estimated mean ratings per level of its included factor variable in standardized units (Cohen's d). This is not Cohen's D. A rating divided by standard deviation of an individual, then averaged has the unit of "standard deviations" but is not a standardized mean difference between two samples.*

RESPONSE: We agree that our effect size is not a standardized mean difference between two samples. However, Cohen's d is not specific to quantifying a standardized mean difference between two samples. Cohen's d broadly refers to the family of effect sizes obtained by taking the difference between two means and dividing by the relevant standard deviation of the target dividend, with Cohen (1988) including subscripts to differentiate possible d effect sizes. It is a common effect size metric for mean differences and change scores in both independent group and repeated measures designs (Lakens, 2013; Morris & DeShon, 2002). We use subscripts to differentiate our specific quantities that are captured by the Cohen's d effect sizes (i.e., cumulative versus net changes).

> *An a priori power analysis indicated that with $n = 1,000$, we would have 80% power at $\alpha = .01$ to detect differences of $|d| = 0.108$ in people's change ratings. This seems rather high. Does this include the p -value adjustment?*

RESPONSE: This does not include the p value adjustment. This was for a two-tailed test, though. It is less clear how to calculate power when dealing with Tukey-HSD-adjusted p values, but we explored further to determine our likely effective sample size. Based on the largest effect that was not significant in our contrasts ($d = 0.179$ at $p = .0101$), we obtained the required sample size to observe an effect of $d = 0.18$ at $\alpha = .01$ with 80% power. This was $n = 364$. Relative to our final sample of 887, this estimates a relative efficiency of 0.41. To verify this

number, we then used this sample size to obtain the minimum detectable effect size at $\alpha = .01$ with 80% power. This resulted in an effect of $d = 0.17995$, suggesting our derived effective sample size when applying the Tukey HSD correction is accurate. We have added this on page 7 of the Methods.

*> we ran two no-intercept regression models.
Perhaps rather say intercept fixed to zero.*

RESPONSE: We have added the suggested content as a clarifying point (pg. 6: “*With these values, we ran two no-intercept regression models (i.e., default intercept fixed to 0).*”).

*> First, variable-level change ratings were predicted by a factor variable containing 25 levels, one for each variable.
This is an unusual but clever choice. Perhaps the authors should expose the reasoning and state that this is equivalent to calculating 25 means.*

RESPONSE: We have added that our approach is equivalent to calculating 25 means (pg. 6: “*With the no-intercept specification, this provided 25 estimated means, or one per outcome.*”).

*> with a median completion time of 4.6 minutes.
I would like to know how many items they answered, how much text they were supposed to read, and if the pay was hourly or for completion.*

RESPONSE: Participants responded to 26 items in the “main” survey: 25 variable ratings and 1 attention-check item asking them to select a specific response option. Then, they responded to 2 demographic items. An example display screen for participants when completing the 26 main ratings is shown in Figure S1, and Table S2 contains the full introductory text, question stem, and item content for all 26 items. This is noted on page 6 (“*For survey content, please see Table S2 and Fig. S1.*”).

Before the main survey, participants read a study overview and other content needed for informed consent (e.g., study’s goal, stated as understanding how people think about change over the lifespan; any associated risks; and expected compensation). The most reading done by participants was likely reading variable definitions, if they chose to do so. Definitions were given to ensure variables were equally understood across people in case of any confusion; however, we did expect that most, if not all, were common individual differences, things, or activities people would already be familiar with. We note this on page 6 as well: “*We included variables that were likely relevant/familiar and thus easy to rate, available in panel data, commonly studied in social science research, and/or often examined alongside the Big Five^{24,25}.*”

Compensation was approximately 11 USD per hour (exact pay of 9 GBP per hour). We have noted this on page 5 in the Methods section (“*Approximate compensation was 11 USD per hour, with a median completion time of 4.6 minutes.*”).

*Signed,
Aaron Peikert*

Thank you to Reviewer 2 for the time spent providing a detailed and helpful review for our study that helped us revise our analytic strategy to most appropriately model change and better expand upon our study’s limitations.

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June 26th, 2026

Prof. Dr. Yana Fandakova
External Editor, *Communications Psychology*

Dear Prof. Dr. Fandakova,

Thank you for the opportunity to further revise our manuscript (COMMSPSYCHOL-25-0829B) for consideration at *Communications Psychology*. Please see our responses to the comments below. Thank you again for your continued time spent on our manuscript.

EDITOR:

Dear Dr Wright,

Thank you for your patience during the peer-review process. Your manuscript titled "Personality traits are less stable than people think" has now been seen by 1 reviewer, and I include their comments at the end of this message. Unfortunately, Reviewer 2 was not available to submit a report. Therefore, we consulted with the lead statistics expert on the editorial board, and their comments are also appended below. You will see that both find your work improved, and we remain interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to the remaining concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response to the reviewers. Please highlight all changes in the manuscript text file.

Editorially, we consider it important that the interpretation of the findings reflect what can be shown by the data including a clear distinction between change and changeability.

RESPONSE: Thank you for the opportunity to further revise our manuscript. Please see our responses to all comments below.

As you revise the manuscript in response to these issues, please also implement all requests in the attached Mandatory Revision Requests document. All requirements listed in this document need to be fully met, or the work will be returned to you for further revisions without peer review. This workflow is in place to increase the likelihood that the paper will be accepted for publication. It reduces the number of rounds of revision (and review) and ensures that the reviewers vet a version of the article that is compliant with journal policies. If you have any questions regarding the required revisions, please contact the journal prior to resubmission to avoid a negative outcome.

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

Please submit the following items:

- *Revised manuscript*
- *Point-by-point response to the referees' comments*
- *Mandatory Revision Requests Table (attached).*
- *Cover letter (as a separate document)*

RESPONSE: We have prepared and will submit the completed Mandatory Revision Requests document confirming that the two remaining changes were made, as well as the revised manuscript with highlighted changes, point-by-point responses to reviewers, and a separate cover letter.

Reviewer #1 (Remarks to the Author):

I congratulate authors for the interesting work. I have no more comments.

RESPONSE: Thank you again for the kind words about our study and for your continued time that you invested as a reviewer!

Statistics advisor:

The authors demonstrate genuine methodological sophistication, particularly in the reliability correction and power analysis. However, the ergodicity response navigates around, rather than through, the most fundamental challenge, and the change/changeability distinction remains underaddressed. A careful reviewer would likely accept this revision conditionally, with pointed requests to verify that language throughout the manuscript consistently reflects the population-average framing and that the Discussion explicitly distinguishes change from changeability in the implications. The paper is stronger than the response letter alone suggested, particularly in the reliability analysis. The core findings that laypeople systematically underestimate personality trait change relative to other variables are robustly demonstrated across multiple analytic approaches. The main residual weaknesses are the change/changeability conflation in the discussion and the inherent limitation of not estimating person-specific developmental slopes, both of which are disclosed if not fully resolved.

RESPONSE: Thank you for the positive feedback about our study and for clearly outlining the remaining concerns about the manuscript.

The change/changeability conflation, however, is still visible in the paper. The discussion states that "highlighting the changeability of traits may empower individuals to pursue positive change and encourage policies or interventions that support personal growth". This directly follows from findings about normative developmental change over 75 years, not interventional changeability. The logical leap is present. Similarly, calling personality "less stable than people think" in the title and conclusion, then pivoting to implications for intervention and social programs, imports an assumption that normative age-graded change warrants optimism about directed intervention. This conflation is subtler than the reviewer's garden-hose/health analogy, but it is structurally the same problem. I'd ask the authors to more clearly rewrite this section to explicitly distinguish change from changeability in the implications.

RESPONSE: To remedy the change/changeability conflation in the Discussion, we have explicitly separated these two ideas for readers and provided two clear examples detailing why natural malleability is important for intervention efforts. This new content is copied and pasted below:

“The developmental malleability of personality traits is consequential for intervention changeability for two reasons. First, although not strictly necessary nor sufficient, an attribute being naturally malleable and exhibiting bidirectional, non-monotonic changes over a long-term period lends support for intentional changeability. For instance, eye color’s genetic origins and largely fixed nature over the lifespan⁷¹ precludes self-directed efforts to change it, whereas weight, a developmentally malleable quality greatly influenced by both genetics and the environment⁷², can be directly modified through diet and exercise⁷³. Similarly, for non-physical attributes, the spectrum of natural malleability for intelligence⁷⁴, personality⁵, and well-being²⁴ corresponds with the level of evidence for their intervention efforts^{75–77}. As such, although lifespan malleability is not synonymous with intentional changeability, there seems to be a link between the two, with natural malleability setting a potential foundation for self-directed change⁷⁸. Second, perceived feasibility and attainability of change are associated with the success of intentional personality change efforts^{15,63,79,80}. Thus, highlighting personality traits’ changeability by demonstrating its natural malleability may empower people to pursue desired changes and encourage intervention development to support these efforts.”

We completely agree that developmental malleability is not equivalent to intentional changeability, and do not want to give readers the impression that they are. We hope our clear delineation of these types of change, and examples illustrating why one likely matters for the other, resolves this concern that was present in our prior submission.

We appreciate the time taken by the statistics advisor to review and comment on our manuscript in light of reviewer unavailability—thank you!