

Dear reviewers,

We would like to thank you for your thoughtful and constructive comments on our manuscript. The feedback provided has been extremely helpful in improving the clarity, structure, and overall quality of the paper. All comments have been carefully considered and addressed in the revised version of the manuscript, with corresponding changes indicated throughout the text and detailed responses provided below.

Reviewer 1

The authors seek to determine the sensitivity to climate as well as the appropriate parameterisation for photosynthesis of tropical Andean trees. They use trees from different elevations of origin transplanted across 2 different elevational regimes (elevations, not temperatures!) to determine photosynthetic model parameters for their saplings for 8 species. They intend to simulate leaf net C uptake and then determine physiological states contributing to these C fluxes and the environmental limitations for this uptake. They consider three environmental drivers (light/PAR, VPD and temperature) for their model, fit some of the parameters with a subset of the data, and estimate the relative contribution of these drivers to simulated net carbon uptake. Advantages of the study include that it pertains to a unique biological system of Andean forests, the deep extent of physiological modelling, and the partitioning of key environmental variables. It is a very valid point that tropical forest is taken by models to signify both montane and lowland tropical forests, and an outcome of the study should help identify relevant model parameters for montane tropical forests in South America.

R/ We thank the reviewer for the positive and constructive summary of our study. We agree that our experimental design is based on altitudinal transplants, which naturally involve temperature differences between sites. We clarified this point in the revised version to avoid confusion between elevation and temperature treatments. We also appreciate the recognition of the relevance of our approach for improving the parameterisation of photosynthetic models for montane tropical forests.

There are a number of issues in the study for the authors to resolve, and I make 7 key points here about flaws in the manuscript, and raise others in the detailed comments. 1) Quite a few aspects of the study are glossed over and not presented in enough detail (see the many detailed comments below). Central to this is whether the temperature kinetics of V_{cmax} and J_{max} were determined or not, given that this is how acclimation to temperature is often defined. There don't appear to be any temperature kinetics in the modelling, or some default is assumed but not tested.

R/ We thank the reviewer for this insightful comment. We agree that clarifying how the temperature kinetics of V_{cmax} and J_{max} were determined is important, as these parameters are central to assessing thermal acclimation. For the Montane-Acclim project from which data were derived, Dusenge et al. (2025) explicitly derived the temperature dependencies of V_{cmax} and J_{max} from A–C_i curves measured at five to six leaf temperatures (15–40 °C) using the FvCB model implemented in the *plantecophys* R package (Duursma 2015). The temperature response functions were fitted using a modified Arrhenius model (Medlyn et al. 2002), from which we estimated

the activation energy (E_a) and optimum temperature (T_{opt}) for both V_{cmax} and J_{max} , while fixing the deactivation energy (H_d) at 200 kJ mol^{-1} to avoid over-parameterisation. Therefore, our analysis included full temperature kinetics for these parameters rather than assuming default values. We have clarified this in the Methods section (lines 174–178).

2) The target of assigning limitations is a set of diurnal measurements of gas exchange, which aren't actually shown in the manuscript nor is how well the modelling recovers the actual net C uptake evaluated. So the assignment of limitations assessed in Figures 4 and 5 cannot be evaluated. We also don't know anything about the environmental conditions on the particular days of measurements as this isn't shown, nor do we know if these days were the same with simultaneous measurements at the two sites, or whether they were done on different days, and we don't know if these measurements days were representative of the respective elevations. This seems like modelling on a blind target.

R/ We thank the reviewer for this helpful comment highlighting the need for more detail about the diurnal gas exchange measurements and model validation. In the revised version, we have expanded the Methods section to describe how diurnal cycles of leaf net CO_2 assimilation (A_n) were collected under ambient conditions on representative clear-sky days, within the same seasonal window at both sites. We now clarify that concurrent measurements of air temperature, VPD, and PAR were recorded to ensure that the days of measurement were representative of the typical climatic conditions at each elevation.

We showed that the modelled diurnal patterns of A_n closely matched observed data (mean $R^2 = 0.9$, Figure 2a). These additions demonstrate that the assignment of environmental limitations was based on realistic and validated simulations rather than unconstrained modelling. Details of these procedures and the validation results are now included in the revised Methods (lines 162–173).

3) The writing uses good English but incomplete descriptions and explanations.

R /We thank the reviewer for noting that some parts of the manuscript required fuller descriptions and explanations. In response, we carefully revised the text throughout to improve clarity and completeness. In particular, we expanded the Methods section to include additional details on the estimation of photosynthetic parameters (V_{cmax} and J_{max}), the diurnal gas exchange measurements, and model evaluation. We also clarified aspects of the Results and Discussion to better explain how photosynthetic acclimation was quantified and interpreted. We believe that these revisions substantially enhance the readability and transparency of the manuscript.

4) The sites and species are not described much if at all. The sites are improperly described by temperatures rather than the physical elevations where they occurred.

R/ We included elevational reference for each species and then their mean annual temperatures.

5) Much of the text in the Intro and Discussion is given over to modelling these ecosystems, yet all the data is from young trees. How in the Intro and Discussion are we to make inferences about mature trees from this? I know that mature trees cannot be transplanted, but did anyone look at differences from the juvenile (sapling) to mature phase? If not, then please do not include text about mature forest biomass and about modelling net CO₂ uptake of Andean forests (most of which are mature, not planted).

R/ We appreciate the reviewer's comment regarding the extrapolation of our results to mature forests. We agree that our measurements were conducted on juvenile trees; however, several lines of evidence indicate that the physiological and compositional responses of juvenile and mature trees to warming can be comparable. For instance, Duque et al. (2015) reported that both juvenile and adult trees show similar thermophilization trends across Andean forests, with an even stronger response among juveniles, which are directly exposed to novel thermal environments and thus provide early signals of forest-level change. Moreover, Cox et al. (2023) reviewed the limited available data and found that photosynthetic rates of tropical trees are generally consistent across ontogenetic stages and comparable to those reported for seedlings (e.g., Bahr et al., 2017). Indeed, juveniles often exhibit higher photosynthetic capacity than adults, reflecting their role as the most dynamic contributors to forest regeneration and future carbon uptake.

Although our experimental trees were isolated individuals and not subject to competitive effects typical of mature stands, they are representative of average physiological performance and growth potential under field conditions. Importantly, the studied species are dominant canopy-forming taxa, representing roughly 20–30% of individuals in Andean montane forests. Therefore, understanding their thermal and photosynthetic responses provides meaningful insight into the likely physiological responses of Andean forests as a whole under future warming.

We have clarified these points and adjusted the Discussion to explicitly state that our results pertain to juvenile trees, while emphasizing their ecological relevance for predicting future forest responses (lines 458–462).

6) there are no hypotheses grounding the study

R/ Thank you for the suggestion. We have rewritten the last paragraph of the Introduction (lines 117–128) to present the study objectives as explicit research questions and hypotheses.

7) the Intro section does not describe past efforts involving limitations to net CO₂ uptake and modelling them, even though this is a major feature of the manuscript.

R/ We thank the reviewer for this suggestion. We have now included additional context in the Introduction summarising previous modelling efforts that addressed environmental limitations to net CO₂ uptake. This new information, added in lines 99–110, clarifies how our study builds upon and extends these approaches to tropical montane species.

The Intro contains a set of disjunct arguments around tree adaptations to climate focussed on photosynthesis. The arguments are not explained well and often not grounded in reality, so the basis for the work isn't established very well. For example, the authors set up a strange disconnect between biomass per unit land area and photosynthesis per unit leaf area, without elaborating on the large conceptual distance between these two things and the number of processes that intervene between the two.

R/ We thank the reviewer for this valuable observation. We have revised the Introduction to improve the conceptual connection between ecosystem-scale biomass patterns and leaf-level photosynthetic processes. The revised text now clarifies that biomass per unit land area integrates many ecological processes but is ultimately constrained by carbon assimilation at the leaf level. We also explain that our study focuses on identifying the physiological mechanisms—particularly photosynthetic thermal acclimation—that can help explain how tropical montane forests maintain high carbon stocks despite cooler climates (lines 38–55).

The authors variously seem to argue that species that are temperate or tropical do not show local variation in photosynthetic characteristics with climate, or that local species photosynthetic traits are very important. In the Intro, the Cuni-Sanchez paper that is cited isn't about biomass at all. And in Duque et al. (2021) Table 1, there is a clear monotonic decline in biomass (Mg C ha⁻¹) from 89 at 500-1200 metres to 59 at 2800-3600 metres, not what the authors say in terms of a lack of biomass differences with elevation. Please explain these properly.

R/ We thank the reviewer for this clarification. We have revised the *Introduction* to ensure conceptual consistency and accurate citation of previous studies. We now acknowledge that Duque et al. (2021) reported a monotonic decline in aboveground carbon stocks with elevation (from 89 to 59 Mg C ha⁻¹), but we emphasize that these values remain high relative to the cooler conditions under which they occur. We also corrected the reference to Cuni-Sánchez et al. (2021), which concerns *carbon stocks* rather than biomass. The revised text clarifies that our intention was not to argue for a lack of biomass variation with elevation, but rather to highlight that tropical montane forests maintain substantial carbon storage and that physiological acclimation of photosynthesis may help explain this resilience (lines 38–55).

Such logical disconnects throughout the Intro need to be reconciled in a uniform, streamlined text. I would suggest eliminating l. 36 up to line 43 and stick to photosynthesis in the Intro, which after all is the actual topic of the manuscript.

R/ We thank the reviewer for this constructive suggestion. Following this advice, we have completely rewritten the corresponding paragraph in the Introduction to maintain a clear and streamlined focus on photosynthesis as the central topic of the study. The revised version now provides a concise and logical transition to the study hypotheses.

Detailed comments

1. 50 'overall species performance under extreme warming'?

R/ We thank the reviewer for pointing out this ambiguity. We have revised the sentence to clarify what is meant by “overall species performance under extreme warming.” The text now explicitly refers to the growth and photosynthetic functioning of montane species under their warmest experimental conditions, and this adjustment is included in lines 58–60 of the revised manuscript.

2. 52 'likely due to increased mortality at lower elevations' Mortality of what? Clearly not all the species, but the authors mean that this is something selective of species. If so, say so, as the statement otherwise seems silly and nonsensical.
3. 54 'lower -hottest limits'. You mean "lower elevation"? Hot is not associated with low temperature, but clearly this is not explained well.
4. 55 'This observed mortality' see questions 1. 52 and improve this statement.

R/ We thank the reviewer for this clarifications. We have revised the text to specify and answer to detailed comments 2-4 that the increased mortality refers to montane species at their lower elevational limits, where temperatures exceed their native thermal range (lines 61–66).

55 '... mortality may stem from thermal limitations to photosynthesis under extreme warming for species at the hottest limit of their distributions'. This is a core concept of the paper but it's not explained well or fully. Imagine that photosynthesis is reduced by high temperatures. Is this a 'thermal limitations to photosynthesis'? What is 'extreme warming' and why does it is extreme when climate is hotter? Isn't climate just hotter on average?

R/ We thank the reviewer for highlighting this key point. We have expanded the explanation to clarify how thermal limitations to photosynthesis can lead to higher mortality of montane species at the warmest limits of their distributions. The revised text (lines 65–70) now explains that reduced photosynthetic capacity and carbon imbalance under high temperature stress can constrain growth and survival, linking physiological acclimation directly to species persistence under warming.

I think I know the answers but my point is that these things are poorly if even slightly explained, yet underpin most of the study and don't seem well developed in this manuscript as yet.

1. 57 'rapidly changing conditions', where was this documented? Why are conditions changing? Again, I know the answer but please say why and bring the reader along! We should not have to guess about such things.

R/ We thank the reviewer for this useful comment. We have clarified what is meant by “rapidly changing conditions” by explaining that Andean montane forests are experiencing accelerated warming, changes in cloud cover, and shifts in moisture regimes over recent decades, which together alter local microclimates and species distributions. This clarification and the corresponding references have been added in lines 70–73 of the revised manuscript.

2. 60 'Most global vegetation, land surface, and Earth system models ...'. Only one is cited. Please include references to others. Self-citing is fine but broader literature is also encouraged.

R/ Thank you for the comment. We have added additional references in lines 75-76

3. 60 '... use a common representation of tropical forest for both montane and lowland tropical forests'. You mean a single value for the plant functional type? Please clarify.

R/ We thank the reviewer for this clarification. Yes, we refer to the fact that most vegetation models use a single Plant Functional Type (PFT) to represent both montane and lowland tropical forests. We have revised the sentence to make this explicit in the text (lines 74–76).

4. 62 "these traits are plastic in response to warming ..." I think that kumarathunge et al. is most compelling in this respect. Self-citations are also OK, but please cite broader too.

R/ We have now included the reference to Kumarathunge et al., which provides an important broader context for our study. In the original version, we had primarily cited publications derived from our project to explicitly contextualize the case study, but we appreciate the reviewer's suggestion to include more general references and have adjusted the text accordingly.

5. 69 'This can be problematic if there are large differences in species' key physiological parameters' makes a valid point, yet for species-rich forests like these tropical ones, how far do we take this? I don't believe that the authors have demonstrated that montane species are truly different in these aspects from lowland species, but we could also think that continents might be different from one another and so on and so on. So the argument falls flat because there are always species differences, yet the authors haven't established that such differences are really important nor influential, and they haven't established that every pixel needs to parameterise its characteristic or most dominant species or sets of species. I suppose I would like some of the nuances of the decisions that modellers need to make to be portrayed here rather than the simplistic 'we need to know the key physiological parameters of everything'.

R/ We thank the reviewer for this insightful comment. We have revised the paragraph to clarify that, while it is not feasible to parameterize every species in species-rich tropical forests, identifying consistent physiological contrasts among major functional groups can improve model realism without excessive complexity. This clarification has been incorporated in lines 84–91 of the revised manuscript.

6. 75 'large biodiversity' or 'high biodiversity'?

R/ We agree with the reviewer that tropical forests have high biodiversity. Thank you for the comment.

7. 77-81 makes a null argument that the authors don't believe. If they contend that species plasticity is important (l. 47), then whether a species has a temperate origin or not may not matter, but their plasticity does matter. So then the statement 'that some species have similar photosynthetic traits to those of temperate species, while others have similar traits to tropical ... species' is irrelevant. I wonder whether this argument is necessary?

R/ We thank the reviewer for this helpful suggestion. We have simplified the paragraph to focus on interspecific differences in photosynthetic traits, removing the unnecessary reference to temperate and tropical species origins. This revision has been incorporated in lines 92–96 of the revised manuscript.

8. 79 'The overall effect is expected to increase the interspecific variance of photosynthetic traits at any one location'. This is only true if there is a mix of species with these origins, but again, this would only be true if Andean species proper weren't preferred in certain locations. I doubt this is true so the authors could give some caveats around this.

R/ We thank the reviewer for this valuable observation. We have added a caveat acknowledging that the expected interspecific variance in photosynthetic traits depends on the degree of species mixing at a given site, and may be lower in communities dominated by Andean species adapted to similar thermal conditions. This clarification has been added in lines 92–96 of the revised manuscript.

9. 97 'parametrization' please spell this correctly.

R/ We fix it throughout the text.

10. 100 'impact' on what? One doesn't just investigate the impact, because impact has to affect something meaningful.

R/ We have rewritten the paragraph in lines 117–128 to make the research questions and hypotheses more explicit.

11. 130 Do we care that the experimental involved a site at 26°C but then there is no further mention of this site? I find this confusing and though part of a larger experiment I think it should be removed as it isn't relevant to this study.

R/ We have removed this part from the text as suggested.

12. 141 'we used data from six montane and two lowland species from two experimental sites (14°C and 22°C)'. I have a few comments here: it seems odd to call the sites '14 °C site' and '22 °C site' in text and figures. The temperatures are from a description of the sites, but the sites are actually physically located at particular elevations. In other words, the sites are *always* at an elevation of 2500m and 1500m (are these rounded?), whereas the temperatures they correspond with may be different

according to the duration of monitoring. So these should really be referred to by their elevations, with temperature descriptors as a secondary feature.

R/ These comments have been addressed throughout the manuscript as part of the revisions made in response to the main comments.

The species are never stated in the text (they are listed in Table 1), and this seems like a must. There is no description of the species either, they could be herbs or trees, this is totally unclear.

R/ We have added the word tree in several parts of the text to clarify this point, which we had previously overlooked. Thank you for the comment.

1. 145 A-Ci curves were 'measured under light-saturated conditions (~1800 PPFD)'. Given that modern portable photosynthesis systems control light levels during measurements, why is this not stated as an exact value that was used? Did the authors in fact control light level?

R/ We thank the reviewer for this clarification. We confirm that light levels were actively controlled during A-Ci measurements using the LI-6800 LED light source and set to 1800 $\mu\text{mol m}^{-2} \text{s}^{-1}$. This has been clarified in the revised text (line 169).

2. 50 'the temperature response of apparent V_{cmax} and J_{max} '. Were these temperature responses actually measured? Where are these data presented? How were the temperature kinetics for V_{cmax} and J_{max} derived?

R/ This point has been addressed and the corresponding clarification has been added in lines 174–178 of the revised manuscript.

3. 165 'Diurnal cycles of A_n were measured in four individuals per species'. How many diurnals, one or several? There are few details given, what were the conditions on the day, etc.? Were they cloudy, warm, seasonable temperatures? Were they done on different days for different elevations?

Then these data, which are the target of the modelling, are never shown. So in the context of the assessment of the various limitations, one cannot evaluate what conditions the measurements were done at, whether they contrast between the two sites or not, nor how they encompass specific conditions or a range of conditions.

R/ We have added more details about the diurnal measurements in lines 187–192 and included a corresponding figure in the supplementary material.

1. 182 'If model performance did not improve.... If model performance improved'. What were the criteria for 'improvement'?

R/ The text has been revised to address this comment, and the clarification has been added in lines 212–213 of the manuscript.

2. 300 'total photosynthesis' What is total photosynthesis here? Why not average, averaging the species that occur? A far better explanation of this aspect of the study is needed.

R/ We agree with the reviewer and have replaced “total” with “average.”

The authors are gaming that all 1000 individuals involve different compositions of lowland and mid-elevation species as far as I can tell, but this needs a great deal of further explanation. There is an underlying (and unwritten) assumption that only mid-elevation species are best-performing at their middle elevation, whereas actual species composition at the elevations they simulated are never stated. In fact, it isn't even clear if the species they planted comprise common species at the different elevations they studied, so the target for the simulations is unclear.

R/ We have clarified this point by explaining the basis of the simulated species composition and the objective of the simulations. These revisions have been incorporated in lines 257–270 of the revised manuscript.

For the Photosynthetic parameters in Table 2, the max and min here are from an absolute max and min of the fitted parameters to each set of observations, but this should be clear in the Table caption.

R/ The table legend has been revised to address this comment.

Fig. 3 shows 'simulated relative contribution of physiological parameters to simulated net carbon uptake'. But did the model actually recover measured net CO₂ uptake? Is this leaf-level?

R/ The figure legend has been revised to address this comment.

Fig. 6 caption could be more clearly written. The part 'weighing their contribution accordingly to their abundance in a forest of 1000 individuals' isn't entirely clear, and if the Anet values are weighed then why is the Y-axis labelled 'Anet x 1000'?

R/ We have clarified this point by explaining the basis of the simulated species composition and the objective of the simulations. These revisions have been incorporated in lines 257–270 of the revised manuscript.

Reviewer 2

In this manuscript the authors aim to better understand tropical montane forests' thermal acclimation capacity and evaluate the key factors affecting photosynthetic rate in different thermal regimes (at different elevations). The authors highlight the importance of customised parametrisation of montane tropical species in models to decrease uncertainty in carbon sequestration predictions.

The paper is within the scope of the journal and addresses a relevant topic that is of interest for both the ecologists/ecophysiologicals, as well as the modelling community. I find that the study findings are valuable and promote further investigation into the performance

differences between tropical lowland and montane species. However, the manuscript would benefit from a clearer presentation of its key findings, stronger integration with existing literature, and some structural and stylistic refinements.

Note: Supplementary material: I can't seem to be able to find the referenced supplementary figures and tables in the preprint document or elsewhere

R/ We sincerely thank the reviewer for the constructive and detailed comments. All suggestions have been carefully addressed and incorporated throughout the manuscript, as detailed in the specific responses to each point below.

Specific comments

The current title is somewhat generic and does not reflect the key features of the study. Please consider alternative titles that reflect either the key results or methods applied (which are nicely summarised in Fig. 1).

R/ In accordance with the reviewer's suggestion, the manuscript title has been revised to "Sensitivity of a mechanistic photosynthesis model to tropical Andean species and environments."

Introduction: I would suggest reformulating the last paragraph where the study aims are defined – it seems like these can be into two sub-groups: an empirical and a modelling focused one.

Aim 1 (empirical): data from a transplant experiment:

- o Intraspecific variation of physiological traits
- o Their relative contribution to Anet
- o Relative contribution of environmental drivers to Anet in 2 or 3 thermal regimes

Aim 2 (modelling): evaluate how well mean parameters of lowland species perform in modelling Anet of tropical montane forest species

R/ Thank you for the suggestion. We have rewritten the last paragraph of the Introduction (lines 117–128) to present the study objectives as explicit research questions and hypotheses.

Materials in methods: Please consider re-ordering the method sections so they follow the presented aims' order (e.g. transplant experiment, photosynthesis model, parametrisation, evaluation, sensitivity analysis)

R/ In accordance with the reviewer's suggestion, the Methods subsections have been reorganised to enhance clarity and coherence.

L107: models in plural? Do you mean the same model different parametrisation?

R/ We have clarified this point in line 132.

2.5. Model application: This analysis would benefit from referring to observed photosynthetic rates if available from a site with similar species composition.

R/ We have clarified this point by explaining the basis of the simulated species composition and the objective of the simulations. These revisions have been incorporated in lines 257–270 of the revised manuscript.

Limitations: How does the differences between juvenile and mature trees' characteristics affect the interpretation of your results?

R/ We appreciate the reviewer's comment regarding the extrapolation of our results to mature forests. We agree that our measurements were conducted on juvenile trees; however, several lines of evidence indicate that the physiological and compositional responses of juvenile and mature trees to warming can be comparable. For instance, Duque et al. (2015) reported that both juvenile and adult trees show similar thermophilization trends across Andean forests, with an even stronger response among juveniles, which are directly exposed to novel thermal environments and thus provide early signals of forest-level change. Moreover, Cox et al. (2023) reviewed the limited available data and found that photosynthetic rates of tropical trees are generally consistent across ontogenetic stages and comparable to those reported for seedlings (e.g., Bahr et al., 2017). Indeed, juveniles often exhibit higher photosynthetic capacity than adults, reflecting their role as the most dynamic contributors to forest regeneration and future carbon uptake.

Although our experimental trees were isolated individuals and not subject to competitive effects typical of mature stands, they are representative of average physiological performance and growth potential under field conditions. Importantly, the studied species are dominant canopy-forming taxa, representing roughly 20–30% of individuals in Andean montane forests. Therefore, understanding their thermal and photosynthetic responses provides meaningful insight into the likely physiological responses of Andean forests as a whole under future warming.

We have clarified these points and adjusted the Discussion to explicitly state that our results pertain to juvenile trees, while emphasizing their ecological relevance for predicting future forest responses (lines 458–462).

Results: Table 1 with the species list should be presented earlier in the text.

R/ We agree with the reviewer and have included it in line 139.

Discussion: L435: Section 4.4. would benefit from the inclusion of reference studies when stating models would benefit from customised lowland/montane parametrisation.

R/ In accordance with the reviewer's suggestion, this information has been added in line 583-584 of the revised manuscript.

Technical corrections

L55: edit reference formatting

R/ The text has been adjusted accordingly in the revised manuscript.

L65-66: "Photosynthesis is one..." Please rewrite this sentence so it flows better with the rest of the paragraph. (Even though photosynthesis... traits employed...)

R/ The text has been adjusted accordingly in the revised manuscript.

L80: "real response" Please reformulate this sentence (simulations with average trait values may not yield realistic response to environmental change?).

R/ We have revised and partially rewritten the paragraph to incorporate the reviewer's suggestions and improve clarity.

L120: Eq. 2 formatting

R/ It has been adjusted

L130: The 26 °C group is not shown in your results, is there a reason you focus on the 22 and 14 °C groups? L139: kg

R/ We have removed all references to the 26 °C site throughout the manuscript.

L150: "We evaluate ..." Please edit/reformulate this long sentence.

R/ It has been adjusted

L169: extra parenthesis

R/ It has been adjusted

L234: For clarity, I would suggest using either the elevation or temperature groupings (assuming temperature classes are the proxy for elevation), rather than both.

R/ In accordance with Reviewer 1's suggestion, we have clarified throughout the text that the study includes two elevational sites with different temperature regimes.

Results

L249: spelling ("there")

R/ It has been adjusted

L251: language use ("best")

R/ It has been adjusted

L253: language use ("very good")

R/ It has been adjusted

L301: consider changing "discrepancies" to differences

R/ It has been adjusted