Reviewer 1

General comments

The manuscript presents an impressive nine-year time series of water, carbon and light-use efficiencies measured with the eddy covariance technique in a hemiboreal forest in southern Estonia. The authors examined various control factors of the aforementioned resource-use efficiencies at daily and interannual scales along the hydroclimatic gradient. Resource-use efficiencies have been studied extensively in boreal and temperate forest ecosystems, but less so in hemiboreal forest ecosystems; therefore, this study provides valuable insight into this previously neglected sub-climatic zone. The manuscript gives mixed signals: it is well written at times, but sloppy at others. My main issues concern the methodology, which may at least in part be due to the ambiguous description of the data analysis methods.

We thank the reviewer for the careful and constructive assessment of our manuscript and for recognizing the value of the nine-year eddy-covariance dataset for hemiboreal forests. We agree that some parts of the Methods section were not described clearly enough, and we will revise them according to the reviewer's comments. A more detailed response is provided in the specific comments below.

The introduction is generally well-written. However, I would suggest opening up more about what the different resource-use efficiencies mean. You could move the sentences from discussion rows L476, L513, L546 to the introduction section and rephrase them.

We move the conceptual definitions of RUEs from the Discussion to the Introduction to better frame the study. A more detailed response is provided in the specific comments below.

I have several concerns about the methodology. The first issue concerns the maintenance of the eddy covariance system and how it affects the spectral attenuation of the H2O measurement and its time lag. I'm not convinced that the authors have adequately considered spectral attenuation and time lag in the H2O data.

We thank the reviewer for raising this important concern. The short (~1 m) LI-7200 intake tube was not actively heated, but its condition was continuously monitored throughout the measurement period. The absence of a RH-dependent time-lag assessment in our original data processing pipeline was indeed an oversight. However, the use of a sufficiently wide lag window is expected to have mitigated its impact to a large extent.

In the revised manuscript, we will assess the sensitivity of WUE estimates to the time-lag determination method as well as to spectral corrections applied to the H_2O fluxes. For a more detailed response, we refer to the corresponding section in the specific comments part of the review.

The second issue concerns the u*-threshold and its large variation across years.

Thank you for this comment. A sensitivity analysis for this site was previously performed using both variable annual u* thresholds and a fixed threshold of 0.2 m s^{-1} (Rogozin et al., 2026, Appendix 2). The resulting annual NEP and GEP estimates were similar and the differences between them remained within the uncertainty ranges. A more detailed response is provided in the specific comments below.

My third concern is related to the ambiguous description of filling the gaps in the CO2 and H2O data. The authors do not explain which drivers they used in the gap-filling. I'm worried that when the authors investigate the main environmental drivers of RUEs, they engage in circular reasoning. See specific comments below for more information about these issues.

Thank you for these important comments. We agree that the gap-filling procedure was not described clearly enough, and we will specify the predictors used, the fluxes gap-filled, and that gap-filling was performed separately for each year. We also acknowledge that using meteorological variables in both gap-filling and driver analysis can introduce dependence, although the effect differs among RUE components. Moreover, gap-filling was performed at the 30-min scale, whereas RUEs were analysed at daily and growing-season scales. A more detailed response is provided in the specific comments below.

Sec. 2.3 should be split into two sections: the first two paragraphs would have their own section, for example, "Eddy covariance data processing" and the rest would be about RUEs. Energy balance ratio (EBR) is interesting for this kind of time series, but not that relevant in terms of the objectives of this study. Also, you do not present any EBC results in the results section and there is only one sentence about EBR in the Discussion section. Therefore, the EBR methodology in Sect. 2.5 should be moved to Appendix C, where the table about EBR results is also presented.

Thank you for the suggestion. We agree that the Materials and Methods section would benefit from a clearer structure, and we will revise its organization accordingly. We will also move the EBR methodology from Sect. 2.5 to appendix, making the eddy-covariance data-processing description more transparent and better organized. A more detailed response is provided in the specific comments below.

The annual CO2 and H2O flux daytime and nighttime data coverage should be listed somewhere in a table. The discussion section is extensive and well-written. However, you should always clearly indicate if you are discussing annual or daily RUEs.

We will add a table in the revised manuscript showing how much daytime and nighttime CO_2 and H_2O flux data remained after filtering for each year. We will also revise the text so that every mention of RUE clearly states whether it refers to daily RUE or annual RUE.

Specific comments:

Figure 1: Are the footprint contours shown here for the whole dataset or some specific period? Clarify that in the caption.

The footprint contours shown in Fig. 1 were calculated using the full nine-year eddy-covariance dataset from 2016 to 2024. We will clarify this in the revised figure caption.

L99-100: Have you made any measurements of tree stand characteristics at the site (trees per hectare, height, basal area, stem volume)? It would be nice to have that information here.

Thank you for the suggestion. We will add more detailed the stand characteristics to the revised manuscript. We did not conduct a full forest inventory at the site; however, we will look for available stand-characteristic data, and we will add the information. This may include stand age, canopy/tree height, and, if available, stand density, basal area, or stem volume.

L113: I was expecting to see more information about the EC system, such as the length and size of the heated (?) inlet tubing and the flow rate. If this is mentioned in some previous paper, please reference that. Also, the exact measurement height is not mentioned; it is unclear whether it is the same as the tower's height of 39 meters. Please rephrase.

Thank you for the comment. Since we use a widespread LI-7200 gas analyser, we considered such technical details negligible. We will add a more detailed description of the instrument setup to the instrumentation section. The system was mounted at the top of a 39 m tower, so the measurement height equals the tower height. We will rephrase in the original manuscript.

L120: "… in two different locations per site." What does "per site" mean? Did you have multiple sites? How far apart were the SWC measurement locations, and how far were they from the EC tower?

Thank you for pointing this out. The phrase "per site" was imprecise, as this study included only one EC site. We meant two locations within the tower footprint area. We will rephrase this sentence.

L126: What do you mean by "unreliable"? Did you use some thresholds to filter some absurd values or something else? Please, clarify.

Thank you for the comment. By "unreliable values" we referred to obvious sensor or logging artefacts, i.e. values that were clearly outside any physically meaningful range for the measured variable, such as unrealistically large negative or positive numbers caused by sensor malfunction, communication errors, or data-logger problems. We agree that the wording was too vague. In the revised manuscript, we will replace "unreliable values" with "obvious sensor or logging artefacts" and clarify that these records were removed during the initial quality control of the meteorological dataset.

L133: Add the EddyPro version number.

Thank you for pointing this out. We will add the EddyPro version 7.0.9 to the revised manuscript.

L134: I suppose all this flux calculation methodology applies to H2O as well. Mention that here if so.

Yes, the same EddyPro processing workflow was applied to both CO_2 and H_2O fluxes. We will clarify this in the revised manuscript.

L137: Did you consider the RH dependence of the H2O time lag? H2O time lag depends strongly on RH and it should be taken into account. See also my next comment below.

We thank the reviewer for this important comment! We acknowledge that we did not explicitly account for the RH dependence of the H_2O time lag in the original processing workflow. However, the intake tube in our setup was short (~1 m), and its condition was continuously monitored. The intake tube and filter assembly were inspected regularly and replaced when necessary to minimize contamination and flow restriction. Time-lag estimation followed the covariance maximization approach with a sufficiently wide search window to ensure reliable lag detection (range is -5 till 5 sec and the average lag is 0.9+-0.12). We will acknowledge this limitation in the revised manuscript.

L139: Is there a specific reason why you did not use in-situ methods for spectral corrections as they are relatively simple to apply in EddyPro? In-situ methods (Ibrom et al., 2007 and Fratini et al., 2012) are generally preferred over older analytical methods, also in LI-7200 systems. Did you check the H2O spectra/cospectra? H2O spectra and time lags depend strongly on the dirtiness of the inlet line, even when the inlet line is shorter than 1 m, while CO2 is much more forgiving. How often did you change the sample line? Did you check the spectra annually? Especially if the sample line is not changed annually, you should check the H2O spectra every year, as they can change substantially from year to year and affect the spectral correction factors

We thank the reviewer for these important methodological comments. Flux processing was performed using the standard analytical spectral correction implemented in EddyPro software. While in-situ approaches (Fratini et al., 2012; Ibrom et al., 2007) can provide improved characterization of frequency losses for H_2O fluxes, particularly in systems with substantial tube attenuation, we considered the analytical approach appropriate for our setup because the intake tube was short (<1 m) and routinely maintained. Signal strength and flow diagnostics did not indicate persistent degradation of analyser performance during the study period. In addition, the use of a consistent analytical correction approach ensured methodological uniformity across the full nine-year dataset.

Although we did not perform systematic annual evaluations of H_2O spectra and cospectra, the EC data were processed separately for each year, allowing us to monitor overall data quality and instrument performance throughout the measurement period. We acknowledge that additional in-situ spectral analyses could further improve the characterization of H_2O attenuation effects and have added this point as a methodological limitation in the revised manuscript.

Furthermore, the primary focus of this study was on long-term variability in ecosystem resource use efficiencies, with H_2O fluxes used only for the derivation of evapotranspiration and water use efficiency metrics. Therefore, while improved in-situ spectral characterization would refine LE estimates, we do not expect the use of analytical spectral corrections to substantially affect the main conclusions of the study.

Therefore, in the revised manuscript, we will reprocess one year using in-situ spectral and cospectral corrections and compare the resulting annual LE/ET and WUE estimates with those obtained using the analytical correction. This will allow us to assess whether the choice of spectral correction method affects WUE values.

L139: "Periods with technical issues or interruptions were excluded." Do you mean the whole dataset or the spectral analysis? Did you filter by steady-state and ITC flags? If not, you should do it and add the description of that here! Which flagging system did you use for those tests? Did you apply footprint filtering?

Thank you for the comment. This sentence referred to filtering the entire NEP dataset. By "periods with technical issues or interruptions", we meant periods when NEP data were removed because of maintenance work, instrument malfunction, power interruptions, or other clearly identifiable technical problems.

We used the EddyPro quality-flagging scheme based on Foken et al. 2004 (the "1-9" system), which combines steady-state and ITC flags. All records with quality flags higher than 5 were excluded from further analysis. We did not apply an additional footprint-based exclusion filter because the flux footprint was dominated by the target old coniferous forest stand and no major non-forest land-cover types occurred within the main footprint area. We will update the flux processing section of the methods in the revised manuscript.

L141: You are measuring in a forest at 39 m height(?) and you estimate the storage flux by the tower-top method. Don't you have a concentration profile measurement on your site to get a more accurate estimate of storage flux?

Thank you for the comment. Unfortunately, Soontaga site is not yet equipped with CO_2 concentration profile measurement system. Therefore, storage flux was estimated using the tower-top method.

L146-147: The annual u*-threshold varied quite a lot during the study period. No management activities or changing the measurement height were mentioned in the site description, so the u*-threshold should remain relatively stable over the years and within a year, as the forest does not contain deciduous tree species. Can you think of any reason why the u*-threshold varied so much? If there was a lot of nighttime data missing in some

years, determining the u*-threshold may be difficult and you may get an anomalous u*-threshold for that year compared to others. In that case, using a "bad" threshold for that year is not the best way to go. I suggest estimating a single u*-threshold from periods of good data quality (ignoring winters if they cause issues) for the entire study period, or using another approach to handle anomalous annual u*-thresholds.

Thank you for this comment. A sensitivity analysis of the u*-threshold selection was already performed for this site in our previous study (Rogozin et al., 2026), see Appendix 2, in which annual values derived using variable u* thresholds were compared with those obtained using a fixed threshold of 0.2 m s^{-1}. The resulting annual NEP and GEP estimates were similar and remained within the associated uncertainty ranges. We will refer to this analysis in the revised manuscript and briefly clarify the robustness of the u*-filtering approach used here.

L147-148: This was done only for the 30-min NEP or also for LE? This whole paragraph basically describes the NEP data processing, but nothing is written about H2O. I'm sure that most of them apply for H2O as well. Please clarify what was done for the H2O data.

Thank you for pointing this out. The same quality-control and filtering steps were applied to both 30-min NEP and LE fluxes, although this was not clearly stated in the original manuscript. We will revise the Methods section to explicitly clarify the processing and filtering applied to LE prior to ET calculation.

L152/Figure A1: Why make this plot for NEE when in Sect. 2.3 you write that in this manuscript, you use NEP and the ecological sign convention?

Thank you for pointing this out. We agree that using NEE in Fig. A1 while reporting NEP throughout the manuscript may create unnecessary confusion. In the revised manuscript, we will use NEP consistently throughout the text, tables, and figures. We will therefore revise Fig. A1 so that the gap-filling diagnostic plot is shown for NEP. The figure caption and Sect. 2.3 will be updated accordingly.

L153: Did you use filled NEP to model Reco?

Thank you for the comment. R_{eco} was estimated using the nighttime partitioning method of Reichstein et al. (2005), implemented in the REddyProc package 1.3.4. In this approach, daytime R_{eco} is derived from a model based on measured nighttime NEP values. GEP was subsequently calculated from NEP and R_{eco}.

L154: Add the version number of the ReddyProc package.

Thank you for pointing this out. We will add the ReddyProc package version number, which was 1.3.4.

L175: Figure D1 is referenced in the text before Table C1. Order the appendices in the order of reference.

We agree that the appendices were not ordered according to their first appearance in the text. We will reorder the appendices accordingly in the revised manuscript.

L205: Clarify that you used filled values.

Thank you for the comment. We agree and will clarify that daily and growing-season fluxes and derived RUEs were calculated using gap-filled flux values.

L219 and L362 and L485

- **L219: What is this 20% threshold based on? Currently, it seems arbitrarily selected, but it is an important threshold because it affects how you draw conclusions from your data. Therefore, the threshold should be well justified.**
- **L362: As you do not mention it, I assume that WUE in 2019 did not differ by over 20% from the median. I can't make out the exact numbers from Fig. 3, but it has to be quite close to the 20% deviation limit. So, it might be worth mentioning in the text, especially when you do not justify the 20% deviation limit in the methods in any way, which makes it seem arbitrary.**
- **L485: I have commented on this 20% deviation a couple of times before. I would consider adding a brief discussion of the 2019 WUE.**

Thank you for the comment. The threshold was selected as a pragmatic compromise: a lower threshold would classify many small fluctuations as anomalies, whereas a higher threshold could miss functionally relevant annual shifts. Importantly, the 20% criterion was not used alone. Because daily RUE values provide many observations, statistical significance alone may identify small but ecologically minor shifts. We therefore combined the Wilcoxon signed-rank test with a conservative effect-size filter: a year was classified as anomalous only when the growing-season metric deviated by more than ±20% from the multi-year median and the daily anomalies were statistically significant.

We will also clarify the near-threshold case of WUE in 2019. WUE in 2019 was close to, but did not exceed, the 20% deviation threshold and was therefore not classified as anomalous. If a stricter 25% threshold were used, 2019 would also remain non-anomalous, while the main WUE anomaly in 2022 would still be retained. For CUE, a 25% threshold would remove the smaller positive deviation in 2017, but the strong negative deviations in 2020 and 2024 would remain. Thus, the main conclusions are not driven by the exact choice of the 20% threshold. We will revise the Methods accordingly and add a short paragraph discussing the 2019 WUE case to make this classification transparent.

L221: I'm confused, were the annual and growing season flux sums for RUEs the same thing in this manuscript? If yes, choose either the annual or the growing season flux sum and use it throughout the manuscript. Do not use both terms if they mean the same thing here, as it is confusing.

Thank you for pointing this out. In this manuscript, RUEs were calculated from growing-season flux sums, not full calendar-year sums. We will clarify the terminology used throughout the manuscript: "seasonal" refers to growing-season values within a given year, "multi-year" refers to statistics calculated across the 2016–2024 period, and "annual/interannual" refers to comparisons among individual study years. We will revise the wording to avoid confusion between full-year and growing-season estimates.

L149, L226, and L266–267

- **L149: Clarify which drivers you used to gapfill CO2 and H2O fluxes. Also, did you do the gap-filling annually or for the whole dataset as one?**
- **L226: Which drivers did you use to fill NEP and ET, and model Reco? If I understand correctly, you are creating a dependence between the predictor and the response. You can't use the same variables in this analysis as those you used (or variables that strongly correlate with them) to fill NEP and ET. For example, it does not make sense to model NEP using Tair, PAR, and VPD to fill data gaps, and then investigate the importance of Tair, PAR, and VPD in CUE when NEP is one of the two parameters used to calculate CUE. The same applies to GEP, as that is calculated based on NEP. This is circular reasoning. This is especially important if a significant portion of your EC flux data is gap-filled, which is almost always the case.**

- **L266-267: So, are these the drivers used to fill the gaps in NEP? They also should be mentioned earlier when you describe the gap-filling methodology in Sect. 2.3.**

Thank you for these important comments. We agree that the gap-filling procedure was not described clearly enough in Sect. 2.3. In the revised manuscript, we will specify which predictors were used for gap-filling, which fluxes were gap-filled, and that gap-filling was performed separately for each year. This was done to preserve year-to-year differences in flux–environment relationships and to avoid applying one common model to years with different meteorological conditions and data coverage.

We also agree that using the same meteorological variables for gap-filling and later driver analysis can create dependence between predictors and response variables. However, this issue differs among the RUE components. GEP was not directly gap-filled or modelled with these predictors. It was derived from NEP and R_{eco} after flux partitioning. ET was gap-filled, but the proportion of modelled ET values was small (5.6% in nine years). In addition, gap-filling was done at the 30-min scale, whereas RUEs were calculated using daily and growing-season aggregates.

Nevertheless, we acknowledge that gap-filling could bias the driver analysis to some extent. We will make this limitation clearer in the Materials and Methods section, report the proportion of gap-filled CO_2 and ET data, and describe the gap-filling predictors transparently.

L281: What about u*-uncertainty described in the two previous sentences? Is it included here? If so, please clarify. If not, what was the purpose of u*-threshold scenarios?

Thank you for pointing this out. We agree that the original wording was misleading and mixed two different types of uncertainty. In the revised manuscript, we will estimate uncertainty for both MDS and XGBoost in the same way, using a Monte Carlo approach. We will revise Sect. 2.5 and the Fig. B1 caption to make this clear.

L292: The variables were defined just before equation #5. No need to do it here again.

Thank you for noting this. We agree and will remove the repeated definitions in the revised manuscript to improve readability.

L338: What was the multi-year median of ET? Add it here.

Thank you for pointing this out. It was 355 kg H_2O m $^{-1}$ season^{-1}, we will add the value to the revised manuscript.

L338: Replace the left parenthesis "(" with a full stop ".".

Thank you, we will fix it in the revised version of the manuscript.

L339: "Seasonal NEP was higher than the multi-year median…"? Add also the value for the multi-year median. Aren't these based on the numbers in Table 2? If that is the case, add reference(s) to Table 2 in the paragraph.

Thank you for the suggestion. The reported deviations are based on the seasonal NEP values presented in Table 2. We will refer to Table 2 in the revised text.

L346: Table G1 referenced before Figures E1 and F1. Order the appendices in the order of reference.

Thank you for pointing this out. We agree that the appendices should follow the order in which they are first cited in the manuscript. We will reorder the appendices accordingly.

L348-356: There is something wrong here. This is pretty much a duplicate of the previous paragraph.

Thank you for noticing this. We agree, and we will carefully revise the text to avoid duplication.

L363: Instead of using the words "increased" and "declining", I suggest using "higher" and "lower" than the median.

Thank you for the suggestion. We agree that "higher" and "lower than the median" more accurately describe these year-specific deviations, whereas "increased" and "declined" could imply a temporal trend. We will revise the wording accordingly.

Figure 4: Add titles to the subplots (WUE, CUE, LUE).

Thank you for the suggestion. We agree and will add subplot titles indicating WUE, CUE, and LUE to Fig. 4.

L388: "Figure 5 illustrates…" This sentence is redundant, as figure captions are meant to tell what the figure contains. Replace this sentence with a general sentence about the results in this paragraph.

Thank you for the suggestion. We agree that this sentence is redundant. We will replace it with a general result statement.

L389-392: Were all the variables with the highest variance explained statistically significantly? Clarify.

Thank you for this comment. We agree that the text should more clearly distinguish between the explanatory power of individual GAM fits and the statistical support for the fitted smooth terms. However, we do not interpret GAM relationships solely on the basis of p-values, because smooth-term significance tests in GAMs are approximate and do not by themselves indicate ecological relevance or predictor dominance. In the revised manuscript, we will clarify that the variables highlighted in this paragraph are those with the strongest individual fitted relationships, based on R^2, response shape, confidence intervals, and ecological interpretability. We will also check the approximate significance of the corresponding smooth terms and avoid mechanistic interpretation where the fitted relationship is weak or uncertain.

Figure 5: Grey shading of the GAM fit is barely visible. Some R2 values are bolded (significance?) while others are not, which is not explained in the figure caption. In the legend, add a colon after the word "Season" or move it to the top of the legend.

Thank you for pointing this out. We agree that Fig. 5 needs clearer formatting. We will increase the visibility of the GAM confidence shading, explain the meaning of bold R^2 values in the caption, and revise the legend.

L403 and L407:

- **L403: I doubt that the "slight increase" was significant. If it was not significant, then there was no increase.**
- **L407: "…after which a slight decline was observed." I had to zoom quite a lot to see that slight decline, which is probably not significant. Consider removing this part or rephrasing.**

Thank you for these comments. We agree that these small edge patterns should not be overinterpreted. In the revised manuscript, we will remove wording such as "slight increase" and "slight decline" where these patterns are weak or not central to the interpretation. We will focus the text on the main GAM response patterns and avoid describing minor edge tendencies as meaningful changes.

L404: "it" -> "WUE"

Thank you for noting this. We agree that replacing "it" with "WUE" improves clarity.

L476, L513, L546 and L577

- **L476, L513, L546: The first sentences in these paragraphs would be a better fit in the introduction than here. See my general comment about the introduction section.**
- **L577: I don't think this paragraph is needed here. Consider removing or moving to, e.g., the introduction section.**

Thank you for this suggestion. Our intention in placing these sentences at the beginning of the Discussion paragraphs was to remind readers how each RUE should be interpreted before discussing the results, and thereby to make the section easier to follow. However, we agree that the full conceptual definitions of WUE, CUE, and LUE are better placed in the Introduction, where they can frame the study more clearly. We will therefore move shortened versions of these definitions to the Introduction and remove the redundant definitional openings from the Discussion. In the Discussion, we will retain only brief interpretive transitions where needed to preserve readability and flow

Regarding the opening paragraph of Sect. 4.3, we agree that it can be shortened. However, we prefer to retain a brief transition there because it separates the interannual interpretation from the daily-scale driver analysis. We will revise it to make it more concise and better integrated with the following VPD paragraph.

L499: But what could be the reason behind the lower EBR in 2022, and also in 2020 and 2021? Can you rule out possible problems in the measurement system? Were the filter and the inlet sample line replaced regularly? If not, they could cause changes in H2O spectra that may not be accounted for in Moncrieff's method, resulting in lower spectral correction factors and lower EBR.

Thank you for raising this important point. At this stage, we do not have a definitive explanation for why EBR was lower in 2020–2022, particularly in 2022. We agree that this requires more careful treatment in the manuscript. However, we do not interpret lower EBR alone as evidence of measurement-system failure, since incomplete energy balance closure is common at forest EC sites and can arise from multiple causes, including storage terms, footprint heterogeneity, advection, atmospheric stability, and uncertainty in ground heat flux estimation. Nevertheless, we acknowledge that H_2O spectral attenuation related to inlet-line or filter conditions could have contributed to lower LE and therefore lower EBR. We will revise the manuscript to make this uncertainty explicit. Specifically, we will add available information on EC system maintenance and H_2O quality control, and we will state that possible underestimation of LE in years with lower EBR cannot be fully excluded.

L549: "Despite age of…" Add "the".

Thank you for noting this. We will fix it in the revised manuscript.

L582: Why is there a paragraph break here?

Thank you for pointing this out. The paragraph break was unnecessary and did not indicate a conceptual shift. We will remove it and merge the sentences to improve the flow of Section 4.3.

L597: According to whom or is this a result of your study? Clarify.

Thank you for pointing this out. This statement was intended as an interpretation of our results, supported by the RF analysis showing VPD as the dominant driver of daily WUE and LUE. We will rephrase the sentence to make clear that this conclusion is based on our analysis.

L655: The comma here is unnecessary.

Thank you for noting this. We agree and will remove the unnecessary comma.

L653-L658: This is a long and hard-to-follow sentence. Consider rephrasing.

Thank you for pointing this out. We agree that the sentence was overloaded and could be difficult to follow. We will split it into shorter sentences.

Figure B1: The word "Methods" in the label at the bottom of the figures is weirdly positioned. Add a colon after "Methods" or move it to the top of the label.

We will revise Fig. B1 by improving the position of the "Methods" label, either by adding a colon or moving it to the top of the legend.

References

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