

We would like to thank the referees for their thoughtful and useful comments on our paper. We have responded to the individual comments below, and modified the manuscript in several places to accommodate the improvements and clarifications requested by the referees.

In the text below, reviewer comments are in black, and our responses are in red.

In response to the comments on the assumption regarding the treatment of OC and BC, we have added detailed text and references explaining the use of this assumption, and the implications. We have also added a more detailed discussion of the scaling factor used for the emissions, supported by references to the literature. As both referees suggested, the section of the paper on the monsoon has been greatly shortened, as the lack of definitive conclusions here did not require a detailed description.

All responses are detailed below, and also in the accompanying revised manuscript.

#### Anonymous Referee #1

##### General comments:

I find the paper by Thornhill et al. interesting, clear and well-written, and it should fit into the scope of ACP. Biomass burning aerosol emissions are high in South America, and information on how these emissions may impact the regional climate is of value. However, I have some comments and questions that need to be addressed before I could recommend the paper for publication. My main comments concern methodological choices and model evaluation. Please see specific comments below.

We thank the reviewer for their comments. We have incorporated some responses to reviewer 2 into the responses below here for reviewer 1 since many of the comments were similar.

##### Specific comments:

Page 1, lines 15-18: It would be good to include uncertainties to the numbers given.

Uncertainties (standard error) have been added to the numbers.

Page 2, line 25: Feingold et al. (2005) is also a relevant reference here. Feingold, G., Jiang, H. L., and Harrington, J. Y.: On smoke suppression of clouds in Amazonia, *Geophys. Res. Lett.*, 32, 2, L02804, 10.1029/2004gl021369, 2005.

Thank you, interesting results in this reference have been referenced in the manuscript.

Page 5, line 9: What levels of CO<sub>2</sub> and other greenhouse gases were used? Representative of the 2000s?

Levels of greenhouse gases and other trace gases were fixed at levels representative of the year 2000 identical to Shaffrey et al. (2009). Ozone is a seasonally varying two-dimensional latitude-height field from Randel and Wu (1999) – this has been added to the manuscript.

Page 5, line 16: Isn't it a rather strong assumption to have BC and OC lumped into one species, given their very different properties (e.g., hygroscopic growth, radiative properties)?

Firstly, we would like to clarify that in CLASSIC, in addition to a biomass burning tracer, the species fossil-fuel BC and fossil-fuel OC are represented separately, by different tracers (in addition to the other aerosol species described in the paper) as stated on p5 lines 10-12.

In CLASSIC, the BBA aerosol tracer has two components: fresh and aged BBA, which were originally designed to correspond to different BC:OC ratios, as described in the paper. This assumption is justified by the fact that BC and OC are internally mixed in biomass burning aerosol particles. In addition, both remote sensing and in-situ aircraft observations characterize the ambient biomass burning aerosols, rather than their BC and OC components. Therefore although climate models which separate BC and OC may appear more sophisticated, we lack the observational constraint to support and validate their complexity, particularly in BBA source regions. Representing BBA as a separate aerosol tracer in fact allows its microphysical and optical properties to be specified using recent BBA observations newly available at the time of model development. Even more recent observational campaigns (such as SAMBBA) still for the most part provide information on the bulk BBA aerosol and so this formulation within the model allows adjustment and evaluation of the microphysics using the more recent SAMBBA field campaign results.

Section 2.1 has been re-written to provide more explanation of this approach.

We agree that our model does not represent spatial changes in vegetation type which may contribute to different aerosol regional composition and therefore different spatial optical properties. Most observational campaigns suggest that aerosol ages very rapidly following emission which would be expected to remove some of the variability. We are aware of a few SAMBBA measurements that have been made showing some differences in single scattering albedo between east and west Amazon area, but these measurements are few and far between. We did in fact run some additional simulations with SSA altered informed by these measurements, but the impacts were very small.

Text has been added to section 2.2 to better recognise this possible influence.

Page 5, lines 29-32: How are the high level emissions distributed among the different model levels 3 to 20? Is the vertical information given by GFED, or is it done online in the model, e.g., by a plume rise routine? I would expect your results to be very dependent on how the emissions are distributed vertically, e.g., to what fraction of emissions occur within or above the boundary layer.

High level emissions are distributed equally in mass between model levels 3-20. Vertical information is not available from GFED, and is performed internally in the model. No plume rise routine is included.

Although we agree that a more detailed representation of emissions in the vertical should give better results, recent efforts to examine whether varying the vertical distribution of emissions has a strong impact have found the reverse. E.g. Veira et al. (2015, ACP) examined this and found "simple plume height parametrizations provide sufficient representations of emission heights for global climate modeling." The modelled vertical BBA profile is affected by emissions, transport, and removal processes, and despite the simple vertical emission distribution, CLASSIC is able to reasonably capture the shape of the vertical profile as well as the column AOD when compared to observations (Johnson et al., 2016). Section 2.2 has been adjusted to include this extra information.

Page 6, line 1: Have the monthly, daily or 3-hourly GFED data been used?

Monthly GFED data have been used, now stated in Section 2.2.

Page 6, lines 4-7: Scaling the emissions by a factor of 2 is a rather extreme measure that needs to be justified. Is this factor solely based on the previous literature cited, or have you done any experiments in this study to reach a conclusion that a factor of 2 gives more realistic results?

We agree, this is an important point, which deserves further explanation.

Applying a global emission scaling factor is not a new method. Reddington et al. (2016) (their Table 2) show that multiple modelling studies have used scaling factors of up to values of 6 in the past; attempting good agreement between modelled and observed BBA AODs and particulate matter concentrations is a continuing problem. Johnson et al. (2016) also discuss the issue, noting that many studies have had to apply values  $>1$  in order to gain agreement between modelled and observed AODs and/or particulate matter measurements for BBA regions, such as Kaiser et al., 2012; Marlier et al., 2013; Petrenko et al., 2012; Tosca et al., 2013; Archer-Nicholls et al., 2016; Kolusu et al., 2015; Reddington et al., 2016.

Specific experiments have not been performed in this study to choose a factor of 2, but have been done as part of the wider SAMBBA project (Johnson et al., 2016; Reddington et al. 2016), which have also included the same climate model and aerosol scheme (Johnson et al., 2016). Johnson et al. (2016) use a scaling factor of 1.6 for CLASSIC, and here we increase this to 2, which is required to be higher as a consequence of decreasing the  $f(\text{RH})$  curve to be consistent with the SAMBBA aircraft observations.

Reddington et al. (2016) find that of many model properties, hygroscopic growth factors were most important in determining the required emission scaling factor. Thus we might expect to need to increase modelled hygroscopic growth in order to match models and observations of BBA; yet we find the reverse: the SAMBBA observations suggest that CLASSIC is already too hygroscopic (Johnson et al., 2016 and this work). Therefore we conclude that at this stage, we cannot solve the scaling factor problem within the scope of this paper, and urge further work in this area - both by future field work and modelling studies aiming to reduce these uncertainties.

It should also be noted that there are significant differences between different emission inventories, as well as between subsequent versions of the same emission dataset (van der Werf et al., 2010; Reddington et al., 2016), so they should by no means be seen as perfect or set in stone.

Section 2.2 has been expanded to include all these points.

We have also added some points about the scaling factor in the conclusion as we feel it is an important issue.

Page 6, lines 31-33: Any idea why the hygroscopic scattering growth is so different between southern African and Amazonian BBA? Is it likely due to some regional-specific properties (e.g., climatological differences), or is it more related to how the measurements were conducted?

It is difficult to reconcile the different  $f(\text{RH})$  curves between Magi and Hobbs (2003) for southern Africa and Kotchenruther & Hobbs (1998) for southern America. Both studies report airborne observations using a humidified nephelometer. MH2003 do not comment on why their 'regional air' is so hygroscopic, or contrast their observations to KH98. Johnson et al. (2016) hypothesize that the 'regional air' classed in MH2003 may have contained a substantial amount of hygroscopic industrial sulfate aerosol, which could have behaved differently. Davison et al. (2004) show some evidence that if combustion of peat swamps is involved, gas to particle conversion can produce high sulfate contributions to the BBA. We anticipate new state of the art observations from the recent CLARIFY project observations within the Southern African BBA plume to expand on this issue shortly. These points have been added to the manuscript.

Page 6, line 33: Are these values for fresh and aged BBA, respectively?

No, these values refer to the range presented in KH98, which are for regional haze from 4 different regions of Amazonia. This is now clarified in the manuscript.

Page 7, line 2: Looks like you have taken the lower value (1.05 for 80% RH) from Kotchenruther and Hobbs (1998) when drawing the green dashed line in Fig. 3. What is the reason for that? Also, did Kotchenruther and Hobbs (1998) find any difference in the optical properties between fresh and aged BBA, or did they conclude that these were similar as in SAMBBA?

This is correct, we have taken the lower value curve (1.05 at 80% RH) - this represents the KH98 Porto Velho data, which was the same location as the SAMBBA aircraft observations. There may not be a strong reason for selecting this lower curve on a global scale in a climate model, and therefore we acknowledge that our AODs should actually be viewed as a lower limit - they could be higher in the model if we had adopted, say,  $f(\text{RH})$  curves with values as high as 1.29 at 80% RH, which could still be realistic. This is now added to the manuscript.

Page 9, lines 4-7: Doesn't this show that BB BC and OC should be treated separately?

Please see response to BC/OC BBA representation above.

Page 10, line 13-14: "see figures" is very unspecific. I suggest referring to Fig. 5 (i.e., "see Fig. 5"). Similarly for Table 1 caption ("which is outlined in each figure").

This has been corrected by referring to the figure explicitly.

Figure 5 caption: Suggest changing to "used to calculate the mean values in Table 1".  
Figure 5 caption: "Stippling represents 95% confidence limit". I presume this is based on the interannual variability, i.e., a Student's t-test on the yearly time series? This comment also applies to the other map figures.

The caption has been changed as suggested.

The significance was calculated using the Student T-test over the time series to distinguish between changes due to the emissions differences, and changes due to inter-annual variability. Text has been added to the manuscript to describe this in more detail.

Table 1: It would be good to include some uncertainties here, to be able to assess whether or not the changes are significant. E.g., I expect the AOD change to be significant, but I am not convinced that this is the case for the change in cloud fraction. Changes that are significant (e.g., based on a Student's t-test on the yearly time series) could be indicated in bold.

Uncertainties have been added in to the table.

Table 1: I would show the % change either as  $(H-L)/L$  or  $(L-H)/H$ .

There are several ways to define and express these changes, but we have decided to retain the percentages as expressed, with the clear description of how they were calculated.

Page 12, line 4: Table 1 shows 70.6% reduction.

Figures in text and table have been corrected.

Figure 6: I find the colour scale a bit confusing because I normally associate brown colour with drying. I suggest using blue colour for high cloud fraction and brown for low cloud fraction.

Colour schemes can be very much a matter of personal taste and style. We prefer to keep consistency across the entire paper, where brown/red indicates an increase in a parameter and blue indicates a decrease, for all variables except precipitation, where bluer colours indicate more precipitation. We prefer to leave Figure 6 as it is, clearly labelled with a colour bar, under the concept that browner colours show an increase.

Figures 10-11: Stippling is not mentioned in the captions.

The caption has been corrected to include description of stippling.

Page 17, lines 1-2: Any reason why it is negative?

The difference here is very small, so the change here is not significant.

Page 19, lines 3-4: Do you mean "we can see the semi-direct effect through cloud burn-off, but the ..."?

Text corrected.

Page 20, line 2: As mentioned earlier, it would be good to include uncertainties to the mean numbers. E.g.,  $0.14 \pm X.XX$  degrees C.

Uncertainties have been added in the text as suggested.

Page 20, lines 3-4: Should mention that the increase is not significant.

Page 21, line 3: The precipitation increase is also not significant.

For both these have added text to indicate significance or lack thereof.

Page 21, line 4: Table 1 shows 15.2%.

Discrepancies between the table and text have been corrected.

Figure 16: The caption has a lot of unnecessary repetition. Please rewrite (e.g.,

"September mean wind circulation at 850 mb for (a) high emissions case, (b) low emissions case, and (c) the difference between high and low emission cases...."). Also, the common unit for atmospheric pressure is Pascal and not millibar. I would replace "mb" with "hPa".

The caption has been rewritten to remove the repetition.  
The units have been changed to hPa.

Page 21, line 16: Please correct Figure numbers.

Figure numbers have been corrected (refers to Supplement figures).

Page 21, lines 16-17: Good to see that the model compares relatively well with ERAInterim for surface pressure and wind. How does the model compare for other important variables, such as cloud cover and precipitation, in the South America region? Has this been published before, or could you extract more variables from ERA-Interim to expand the comparison?

We have extended the comparison to clouds cover and precipitation. However we include these figures in the supplement in order not to lengthen the main paper. They show the model broadly reproduces the general patterns of cloud cover and precipitation for September.

Figure 17: The figure quality is not good. The titles and axis labels are small and not readable. Please improve. The same comment applies to Figs. 18-19.

Figures have been changed or removed as part of the re-write of the monsoon section, and the quality of the new figures has been improved.

Page 25, line 5: Insert "(Fig. 19)" after "S. America".

Corrected.

Page 27, lines 18-22: In general I find the discussion in Section 4 a bit too detailed and inconclusive. Given that no conclusion on the BB impacts on the monsoon can be reached, due to missing daily output in the experiments, I think the paper would benefit from a substantial shortening of Section 4. For instance, you could keep Fig. 19 and part of the Fig. 19 discussion, and move Figs. 17-18 and most of the associated discussion to the Supplementary Material.

We acknowledge that there is too much weight given to this section and thank the reviewer for their suggestion. We have moved the figures showing similarities to previous work to the supplement, and include only a figure showing the effect of emissions in October and November in the main paper, alongside a much shortened discussion.

Page 31-34: The reference list needs a lot of cleaning up to comply with the format used in the ACP journal.

We were surprised to read this comment as we used the ACP template and reference format. We cannot see any inconsistencies or errors, so refer this point to the editorial team. We will be happy to correct any specific errors.

Technical corrections:

Page 9, line 7: Fig 4 -> Fig. 4  
Figure 7: Please remove or change "Cloud Amt" -> "Cloud Amount" in legend.  
Figure 7 caption: "mass mixing ratio" -> "mass mixing ratio (MMR)"  
Figure 8 caption: "Deep" -> "deep"  
Page 14, line 26: "Fig.9" -> "Fig. 9"  
Page 15, line 1: "reduces" -> "reduce"?  
Page 15, line 13: "CS" -> "clear sky"  
Page 18, line 7: ", illustrates" -> "illustrate"  
Page 20, line 4: "Fig.15(a)" -> "Fig. 15(a)"  
Page 22, line 3: "Fig. 15(c)" -> "Fig. 16(c)"  
Page 22, line 11: "patters" -> "patterns"  
Page 22, line 14: "suggestuing" -> "suggesting"  
Page 23, line 28: "(18(a-d-g))" -> "(Fig. 18(a-d-g))"  
Page 25, line 6 and 7: "dryer" -> "drier"  
Page 25, line 31: "andout" -> "and out"  
Page 27, line 22: "a results" -> "a result"  
**These corrections have been done, Fig. 7 legend fixed.**

Anonymous Referee #2

I have reviewed the manuscript titled "The Effect of South American Biomass Burning Aerosol Emissions on the Regional Climate." The manuscript uses observations from the SAMBBA campaign to constrain the aerosol scheme in the HadGEM3 model. Multiyear experiments are run using representative low and high biomass burning years and the resulting impact to the regional climate is assessed.

While the study fits within the scope of ACP, after careful consideration I cannot support publication as written. I believe that significant revisions are required prior to consideration in ACP. The manuscript is well written to some extent, but does contain several typos and one section needs to be made more concise. More importantly, I believe there are some flaws in the assumptions that must be addressed by the authors, and the novelty of the study in my opinion (running several years of experiments using a low and high case of biomass burning) is not sufficiently explored. Below are my major comments, followed by more specific comments.

**We thank the reviewer for their comments. Since many of the comments overlap with those of reviewer 1, we refer the reviewer to the responses to reviewer 1 in many cases.**

Major Comment 1: The manuscript does not include any model comparisons with observations.

First, I would expect that some of the key assumptions made in the study would be verified through comparisons to observations, such as the assumption that the GFEDv3.1 emissions data should be scaled by a factor of 2. Is this because of issues with the GFEDv3.1 database or because the hygroscopic growth in the model was lowered? Second, I would expect that the model results be compared with in situ and remotely-sensed observations to ensure it is appropriately representing the

regional dynamics, microphysics, and thermodynamics of the atmosphere prior to running comparison studies between low and high biomass burning years.

Emissions scaling factors - please see response to reviewer 1.

We have added additional comparisons to observations in the Supplemental materials, specifically for cloud cover and precipitation, which show that the model is reproducing the main features of the climate in the region.

Major Comment 2: Similar to a comment made by Reviewer 1, isn't it a rather strong (and incorrect) assumption that BC and OC are lumped into one species, given their different properties? GFED3.1 emissions are provided in terms of vegetation type but it appears that an average SSA is used for all emissions. As a result, the model isn't adequately representing the varied aerosol composition that occurs due to the different vegetation types from the central Amazon down to the Cerrado. The AERONET inversions that you show in Figure 4d support that a single aerosol composition doesn't make sense if the area of study is represented by the relatively large black square in Figure 5. I believe that Ten Hoeve et al. 2016 is relevant to reference here (<http://onlinelibrary.wiley.com/doi/10.1002/2015GL066873/full>). Figure 2 in that paper shows that changes in the aerosol composition will lead to changes to the impact of aerosols on cloud fraction. If the aerosol composition was allowed to vary, the resulting impact to cloud properties may be different.

Please see response to reviewer 1.

Major Comment 3: The study a few times refers to variability in deforestation. The abstract specifically states, "This study therefore provides an insight into how variability in deforestation and biomass burning emissions may contribute to the South American climate, . . . ." The way that sentence (and others that mention deforestation in the manuscript) reads makes it sound like the study performs experiments on the effect of deforestation on land-atmosphere interactions through changes to the land surface in the model. Another example is Page 3 Line 2. I believe this is misleading.

Reddington et al. (2015) show that there is a clear positive relationship between deforestation, BBA emissions, and AOD. Therefore we consider the impact of varying emissions to be reasonably related to the impact of varied deforestation rates. However, we have reworded several instances of the description of this relationship to make it clearer that we are not actually representing deforestation in the model, e.g. the abstract now reads, "This study therefore provides an insight into how variability in deforestation, realized through variability in biomass burning emissions, may contribute to the South American climate. . . ."

Changed in the manuscript. Wording has been changed at two points in the introduction, abstract, and conclusion.

Major Comment 4: The "impacts of the monsoon" section is long and can be significantly shortened, especially considering that "within the constraints of the experiments performed so far, it is difficult to suggest how much, and what kind, of an effect there may be (typo) on the monsoon as a result (typo) of the biomass burning." Because there is not a strong signal, I would keep the discussion of this to a minimum. For instance, I got lost between Page 23, Line 9-24 when Figure 17 was discussed, and then again discussed on Page 25, Lines 14-26 after Figures 18 and 19 were discussed. It



seems like this entire section could be consolidated. Furthermore, I would have expected to see some comparisons of your results to other studies that have already modeled the effect of biomass burning on the monsoon, such as Zhang et al. 2009.

We have responded to a similar comment from Reviewer 1 on the monsoon discussion, and have made the alterations to significantly reduce and improve this section as both reviewers have suggested.

(From Rev. 1 response: “We acknowledge that there is too much weight given to this section and thank the reviewer for their suggestion. We have moved the figures showing similarities to previous work to the supplement, and include only a figure showing the effect of emissions in October and November in the main paper, alongside a much shorter discussion.”)

Major Comment 5: Several studies have looked at aerosol cloud interactions over the Amazon, using aerosol and microphysics schemes that are more sophisticated than the bulk scheme used here. I would suggest comparing your results briefly to theirs (e.g. Wu et al., 2011, Ten Hoeve et al., 2012). However, I believe the most novel aspect of this study is that multi-year simulations are run, in turn reducing the effect of meteorology on the results. You can run the same high BB emissions over dry and wet years, warm and cool years, etc. and compare to low BB emissions over those same years. I think it would strengthen the paper to include a discussion on how the effect of different years with different meteorology either affect the H BB – L BB results, or don't.

We have added some additional comments in the manuscript on the comparisons with other publications suggested here.

Unfortunately, it appears that our description of the model setup has led to some confusion for this reviewer. The model has been run with prescribed sea surface temperatures, repeating annually, Thus we do not have the full annual variability represented as influences of ENSO and other modes of SST variability, which have been shown to have an influence on S. American regional climate etc are not captured (e.g. Marengo et al 2012). We are therefore not able to compare the wet/dry years and warm/cool years in the way suggested here. The multi-year runs are instead necessary to improve the statistics for understanding the effect of the biomass burning changes specifically which in fact necessitates removing the effects of variability in SST, rather than as a representation of the real-world meteorology over the 30 year time span. Despite having removed the SST influence, other short time scale variability remains present - this is minimised by using 30 year repeated simulations. This has been clarified in the description of the model set up (Section 2.1).

Specific Comments:

1. Line 24, Page 2: Missing comma

This has been corrected.

2. The sentence starting with “Changes in surface fluxes” on Page 3 Line 17 should have a reference

Zhang et al 2009 - reference added.

3. Please describe how the aerosol cloud interactions are parameterized in the model. There is some discussion of the aerosol scheme but not much discussion of how these

interactions are parameterized.

Fresh BBA is not considered hydrophilic. Aged BBA is considered hydrophilic and acts as a cloud condensation nuclei (CCN). Cloud droplet number concentration (CDNC) is calculated from the number concentration of CCN in the accumulation mode of BBA according to Jones et al. (1994; 2001) using a relationship based on multiple aircraft observations and assuming externally mixed aerosols. The BBA indirect effect parameterization is analogous to that of the sulfate scheme CLASSIC. CDNC is then used to calculate cloud droplet effective radius in the radiation scheme and the autoconversion rate of cloud water to rainwater in the large scale precipitation scheme (Bellouin et al., 2011). This information has been added to section 2.1.

4. Please describe in more detail how you determined the statistical significance.  
(Page 10, Line 10)

The significance was calculated using the Student T-test over the time series to distinguish between changes due to the emissions differences, and changes due to inter-annual variability. Text has been added to the manuscript to describe this.

5. Why do you suspect surface temperature has not changed appreciably given other surface flux changes? The percent change in Table 1 is 0.0%.

The reduction in the downwelling surface SW radiation flux is largely compensated for by the changes in the sensible and latent heat fluxes, which mean there is little change to the surface temperature. A sentence to this effect has been added to the manuscript.

6. The sentence beginning with “The difference (high-low). . .” on Page 12, Line 1 seems obvious and could be removed.

I have left the sentence in as it also clarifies that African BBA does not change between the two simulations.

7. Page 13, Line 3: What area outlined? In one of the figures?

It is the area outlined in all the plots by the black square. I have amended the manuscript text to make this clear.

8. Page 14 discusses how biomass burning reduces deep convection caused by the stabilization of the atmosphere, reduction of surface fluxes, etc. However, many studies have shown a destabilization of the atmosphere above the aerosol layer due to heating at the aerosol layer, leading to increased convection above the aerosol layer. If you look at a cross-section of cloud water, do you see increases above the aerosol layer?

The mass fraction for the cloud liquid water shows a reduction above the aerosol layer, so we are not seeing increased moisture above the aerosol layer.

9. I believe there is not enough evidence to make the statement on Page 14:

“. . .suggesting that the reduction in cloud fraction may be due predominantly to this mechanism.” How do you know if the reduction of cloud fraction is due to the reduced deep convection and in turn reduced cloud top anvils, or simply due to cloud burn-off of cumulus at the aerosol layer? Each effect impacts a different type of cloud. I believe

further investigation is required to ascertain the source of the cloud fraction changes. The deep convection difference plot implies that there is a reduction in the deeper convection in the model in the high emissions case, and we are suggesting that this mechanism applies specifically to the reduction in the cloud fraction at heights of 9-12 km. This has been clarified, so that this is the implication for high cloud, and that the burn-off at the aerosol layer is likely to be responsible for the reduced cloud fraction where cloud and aerosol are co-located.

10. There are no units included in the caption of Figure 8.

This has been corrected for BL height, the other quantities shown are dimensionless.

11. Please reword the sentence on Page 19, Line 3 starting with “Within”. Confusingly worded as written.

The sentence has been re-written to clarify the meaning.

12. Similar to Major Comment 4 above, Figure 18 a,b,d,e,g,h don't add much value because you can't see the difference between these two simulations easily. Perhaps some of these figures could be moved to the supplemental. Same with Figure 19.

We agree with this suggestion and have moved some figures to the Supplement, and re-organised others to make the differences clearer, as part of the changes to Section 4.

### References for Comments

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