I have a couple broad questions related to the premise of this study – if you are trying to test potential remote sensing algorithms, using simulated MSI or Landsat data, how often are high quality images available that this could potentially be applied to? Clouds are a problem everywhere for these applications, but my impression is that they are an even more important factor in the Amazon and similar tropical regions. What is the satellite record like, particularly during the rising and falling limbs of the hydrograph when we would expect things to be most dynamic? Even in the absence of thick clouds, the high humidity, haze/thin clouds, and even smoke can be major barriers to reliable atmospheric corrections – since remote sensing of CDOM is so sensitive to atmospheric corrections, how do you think this would influence the usefulness of satellite applications for the region?
Referee#2 Comment 1

By the time we started our study, there was only one satellite in the Sentinel constellation, therefore the probability of having cloud-free images was not high during the rainy season. Nowadays, however, there are two satellites collecting data every 5 days, which increases the probability of, at least one partially cloud-free image. The present satellite missions are constellation oriented and there has been a huge effort in the signal processing algorithms oriented towards multisensor cross-calibration and harmonization (Claverie et al., 2018). Moreover, sensors have improved their sensitivity to the low signal having improved signal-to-noise ratios and higher radiometric resolution. Moreover, the best window for acquiring satellite images in the Amazon Basin (Martins et al., 2018) covers most of the period encompassing the rising and falling limbs of the Amazon River flood pulse.
We agree that using satellite remote sensing for monitoring CDOM is really challenging not only due to the low signal under a huge atmosphere interference but also due to glint effects and diffuse scatter of the surrounding forest. In the attempt of overcoming those limitations, we can push the knowledge ahead. The present study is just a little brick in this attempt.
Despite all the hard work in a very defying environment, having done numerous missions to get such a small amount of data, we think that without including remote sensing tools in the study of dissolved organic matter pathways from the forest to the water we will not fully understand the diversity and processes linking this complex environment.
In this first brick, we think we have proven that it is possible to model the relationship between UV slope and a_{CDOM} having a sensor with the spectral features of Sentinel/MSI. We recognize the weaknesses of the model mainly due to the sample size. But it is a first brick in the wall. This brick can be even replaced for a harder brick, but it is a brick.

Second, I'm not sure why it was needed to use S275-295 instead of a440, in these circumstances. The spectral slope has been used mostly in coastal ocean studies with higher spectral resolution sensors, in cases where there were specific questions about the source of DOM (usually terrestrial versus marine). The goal of this study seems to be to trace bulk DOM, largely – a440 or other specific wavelengths have been used extensively for that type of application, in freshwaters. I question whether its appropriate to use the spectral slope for this type of question, environmental system, and sensor type, at least without further justification. Field measurements of spectral slope do provide additional information, but if its simply being estimated from a440 without additional parameters, then I don't think you can make more conclusions than you could from just a440.
Referee#2 Comment 2

As there is an increased interest in assessing DOM quality and considering that literature have reported that there is a relationship between S value in the UV region and DOM properties (Helms et al., 2008; Peacock et al., 2014) (not only to its origin but also degradation state, for instance), we thought that if we could use optical data (e.g. absorption) derived from satellite, one could register changes in DOM quality patterns along time in response to the flood pulse. We have seen more and more studies focusing on the study of DOM quality using absorption data, especially using field Spectrolyzers. In our paper, we have tried to push the remote sensing data in this direction, taking into account the diversity and dimension of the Amazon basin, where it is hard to scaling up in situ data to broader regions. We know that the use of satellite information in the analysis of DOM quality is even more challenging once you have the influence of other parameters as the atmosphere, sensor and targets around. However, some studies (Vantrepotte et al., 2015; Fichot et al, 2013) have already shown that it is possible to estimate the spectral slope. Going in this direction, here we proposed a model to estimate $S_{275-295}$ from remote sensing data in a very complex system, regarding optical characteristics.

Regarding the different information that CDOM and S can tell, while a_{CDOM} gives a quantitative parameter (the intensity of a_{CDOM} signal is proportional to DOC concentration), S is related to properties of the DOM (Helms et al., 2008).

Section 3.3: I honestly found this entire section fairly confusing. There needs to be more detail in the statistical description of models, to start. For instance, saying "validation results were satisfactory" is not sufficient.
Referee#2 Comment 3

Authors agree with reviewer and have included more statistical information regarding model validation. The sentence will be changed to: "Validation results showed a good explanation of the model´s variance ($r^2=0.8$) and with values predicted by the model close to the observed values (%NRMSE=9.4), indicating the feasibility of estimating $S_{275-295}$ from a_{CDOM} (440) (Figure 8a)."

Also, it might be better to separate out the remote sensing model from the results on the relationship between spectral slope and a440, both in the text and in the figures.
The questions being answered are completely separate: can field-measured a440 be used to predict S275-295 is a very different question than whether simulated remote sensing data can be used to predict a440.
Referee#2 Comment 4

We agree that those are two separated questions. But our premise is that if we can use field-measured a440 to predict $S_{275-295}$, we would only need to estimate a440 from satellite Rrs and apply the model to every water pixel to spatially map S. That is why they are not separated.

Also, if the ultimate goal is to derive s275- 295 from remote sensing data, than that needs to be presented, and for the propagation of error to be quantified somehow.
Finally, it seems like the remote sensing model is only for the rising limb, not the receding – is that right? Or at least that it excludes the two lakes during the receding limb? If so, I think you need to further justify that decision – I understand that you cannot estimate spectral slope as easily, but there's not a clear reason why you can't estimate a440, which is still a very useful parameter.
Referee#2 Comment 5

Yes, our ultimate goal was to assess the potential of estimating S using remote sensing data. The results, however, indicate that the model only works for the rising limb, which

is not a bad thing, because the largest differences among a_{CDOM} and S happen during the rising limb. During the receding limp and low water DOM pools become more homogeneous again due to both photodegradation and biodegradation as shown in Figure 3.

There are a few issues with grammar and clarity, throughout the manuscript. While this was not so much that I had trouble understanding, the authors might consider an additional round of copy-editing. Overuse of commas, for instance, is sometimes an issue. I've pointed out some of these cases in specific comments, but not all.
Referee#2 Comment 6

Authors apologize for the poor English version which was revised.

Specific comments:
Line 26: there's more recent papers on the size of the DOM carbon budget that might be more appropriate – the whole special issue of Limnology and Oceanography Letters on carbon cycling of inland waters would be a good resource
Referee#2 Comment 7

We would like to thank the reviewer for the suggestion. We include the following references in line 26: " DOM represents the largest pool of organic carbon in the aquatic environment (Cole et al., 2007; Seekell et al., 2018; Tranvik et al., 2009) and…"

And in line 232: "In the rising phase, the water coming mainly from the Solimões river undergoes overbank flooding (Figure 1c), overtopping its channel and flowing across the litter through the forest before reaching the lakes (Junk, 1989). The tree-DOM may be a n import source of organic matter carried to the lakes during this event (Van Stan and Stubbins, 2018) carrying a great amount of organic matter accumulated during the lower water season. "

Line 27: Hastie et al 2019 on the Amazon carbon budget that incorporates aquatic cycling would be good to cite here and elsewhere
Referee#2 Comment 8

Thank you for the suggestion. The study analyses the flux of C between river and floodplain in Amazon during wet and dry periods. The authors found that net ecosystem productivity (NEP) is higher in wet periods than in lowest discharge levels. Now it is incorporated in the text: "… playing a vital role in primary productivity of aquatic ecosystems and consequently fisheries and other food webs (Hastie et al., 2019; Maia and Volpato, 2013; Volpato et al., 2004)."

Line 33: In what cases?
Referee#2 Comment 9

Here we refer to cases in which only DOC concentration can limit the study of the seasonal variation in DOM composition and origin because it is only related to the bulk DOM. To clarify the sentence, it will be changed to: "However, simple measurement of DOC concentration can limit the study of the seasonal variation in the DOM composition and origin since it is related only to the bulk DOM (Jaffé et al., 2008). Quality parameters are needed to better understand DOM dynamics such as ultraviolet (UV) and visible absorption measurements, fluorescence, which are an alternative for high costly laboratory analysis (Li and Hur, 2017)."

Line 36: "being a relevant indicator: : :" that clause is probably not necessary – implicit in the rest of the sentence that it's a useful tool.

The sentence will be changed in the manuscript and it is now in the section 2.3.2 Spectral slope determination: "Helms et al., (2008) have shown that the spectral slope in the range of 275 and 295 nm ($S_{275-295}$) is an indicator of DOM molecular weight and a tracer of degradation processes."

Line 37: define CDOM. Also, CDOM is a concept of a pool of organic matter – the portion that absorbs light. It encompasses most of the various optical absorbance parameters, but it is usually better to specify what exact proxy is meant by CDOM. So, if you're referring to a specific proxy – Sr or a440 – its usually better to use that term than the broader "CDOM" category.

The sentence will be changed to: "A study carried out on Mississippi and Atchafalaya River revealed that the rates of water exchange between river and floodplain increase DOC concentrations and alter DOM composition (Shen et al., 2012). The authors have reached to this conclusion based on measurements of the absorption coefficient of colored dissolved organic matter (CDOM) at 350 nm (a_{CDOM} (350)), a_{CDOM} spectral slope ($S_{275-295}$) and DOC concentration. DOM optical properties (Spencer et al. 2008) such as a_{CDOM} (350) presented higher values during the spring flush at Yukon River basin due to the remarkably high content of aromatic vascular plant material derived from surface soil and litter layers."

Also, the sentence about Helms et al 2008 is a bit out of place – move to methods? Or wrap in a broader discussion of what the different CDOM/absorbance parameters mean and can tell us about the environment.

The sentence was changed to the section "2.3.2 Spectral slope determination" as follow:" Helms et al., (2008) have shown that the spectral slope calculated in the range of 275 and 295 nm ($S_{275-295}$) is an indicator of DOM molecular weight and a tracer of degradation processes. In the present study, the spectral slope in the interval between 275 and 295 nm ($S_{275-295}$) was computed according to the Equation 3 (Bricaud et al., 1981), using non-linear fit (Helms et al., 2008). This function describes the a_{CDOM} (λ) behavior along the electromagnetic spectrum and is expressed as:"

Line 47: describe what a412 is, and aCDOM more generally.

The meaning of a_{CDOM} (λ) – absorption coefficient (a) of colored dissolved organic matter at the wavelength λ are now described above (Please see comment 8).

Line 50: Relatively few studies have looked at spectral slope remote sensing of inland waters, but there are MANY out there that look at a440, a412, a350, others. It'd be worthwhile to explain that this is commonly used. Mostly for Landsat, but there's a few out there using Sentinel, and laying out for a non-remote sensing audience, as might be expected for Biogeosciences, that this is an established field would be useful.

The text will be changed to: "In order to study DOM dynamics in wide spatial-temporal scale satellite images have been assessed as a source of optical information about CDOM. Many studies (Kutser et al., 2005; Brezonik et al., 2015) have used Landsat images for studying aCDOM at the different wavelengths, more commonly at 440 and 420. In recent years the availability of Multi-Spectral Instrument (MSI) images, on board of the Sentinel-2A (June/2015) and Sentinel-2B (March/2017), has expanded the potential of remote sensing application for DOM monitoring. According to Toming et al. (2016) MSI-Sentinel suitability for studying DOM. However, only a few studies have looked at the spectral slope of DOM. In a Pan-Arctic study, Fichot et al. (2013) showed that $S_{275-295}$ can be directly estimated from satellite images using a multi-linear parameterization of MODIS marine reflectance. However, the relationship proposed by Fichot et al. (2013) between Remote Sensing Reflectance (Rrs) and $S_{275-295}$ is based on two assumptions: i) CDOM optical dominance in 45 water bodies; ii) co-variation between CDOM and other particulate matter. To circumvent these assumptions, Vantrepotte et al. (2015) investigating the relationship between CDOM and DOC showed that S275-295 can be estimated from MODIS data using aCDOM (412) as proxy. Nonetheless, both studies (Fichot et al., 2013; Vantrepotte et al., 2015) used MODIS data whose spatial resolution (250-1000 m) restricts the application to inland water studies."

Line 62: I had a little trouble following this sentence. Consider revising.
Referee#2 Comment 15

The sentence will be changed to: "The seasonal flood is caused by both the rainfalls (in upper Amazon basin and locally - from December to May, with an average of 300 mm/month) and by the annual melt of the Andean cordillera during the austral summer (Junk, 1989)."

Line 64: commas are overused in this sentence
Referee#2 Comment 16

The sentence will be changed to: "The yearly MSDR flood pulse causes (Queiroz, 2007), in average, 12 meters amplitude in the water level between the dry (September to November) and the flood season (May and mid-July)."

Figure 1: Could you locate on the map where the discharge gauges are on each river? Also, since you don't refer to the roman numeral elsewhere in figures/tables, I would just put the name of each lake next to it, instead of the numerals
Referee#2 Comment 17

The discharge gauges are outside the image showed in the manuscript. The Solimões´s station is upstream from the Solimões river (2.49° S, 66.06° W) and the Japurá station upstream from Japurá river (1.86° S, 65.60° W). Regarding the lakes´ number, we appreciate the suggestion but we think that the image stays cleaner showing the lakes´ name in the legend.

Line 68: Briefly describe these criteria 69: This is the first time you've mentioned Landsat. If you're also including Landsat in your criteria and research, it should be discussed some in the intro, particularly since there's quite a bit of literature out there on remote sensing of CDOM using Landsat.
Referee#2 Comment 18

The Landsat was included in the criteria because the study cited (Jorge et al., 2017a) analyzed Landsat data. Since here we only focused on Sentinel, we decided to remove

it. The sentence will be changed to:" The lakes were selected according to criteria defined in Jorge et al. (2017a) to guarantee access to them throughout the hydrological year and sizes compatible to the spatial resolution of satellite sensors (MSI/ Sentinel-2A)."

Line 73: So, these are connected all year long? There's not a time period when they aren't connected? At their lowest, do they consistently meet the criteria you mention above?
Referee#2 Comment 19

Mamirauá, Pirarara and Pantaeão are connected to the river during the whole year, even during the dry season. Exceptions may occur during extreme dry years, when the lakes´ level is extreme low. Buabuá tends to lose connection in the dry month (September).

O Pirará e o Pantaleão são conectados e tem acesso o ano todo. Em anos de secas extremas, eles podem perde conexão durante as cotas mínimas mas a norma é que haja conexão. O Mamirauá e o Buá-Bua tendem a perder conexão no mês de mínima.

Line 78: It looks like there are 24 stations, total, but 87 samples. Not 87 stations. Section 2.3.1:
I am admittedly not as familiar with in situ Rrs methods as the other tools used here, but is there any information on quality control/error for this data set?
Referee#2 Comment 20

You are right. The sentence will be changed to:" in total 87 samples were collected among the lakes and seasons"

Regarding quality control, all measurements were taken following the IOCCG protocols and previous experience on Amazon Floodplain lakes reported in Sander de Carvalho (2015). Unfortunately due to the lack of resources no replicas were taken for CDOM absorption measurements. For radiometric measurements, errors are within the 30 % expected for *in situ* remote sensing sampling.

Line 94: How long were samples stored before analysis?
Referee#2 Comment 21

Each field mission lasted around 12 days, with 8 days of sampling and the remaining days in transit. Considering that all samples were processed up to 4 days after returning to the lab, the samples were kept in the refrigerator for up to 14 days (8 samplings days + 2 days in transit + up to 4 days to be processed).

Line 95: blank corrected? Is this a single or dual beam spec? Given that the a440 was fairly low in some of these samples, what was the limit of detection with a 1 cm cuvette?
Referee#2 Comment 22

The samples were corrected by blank measurements and dual beam spec. was used.

Line 111: absorption at 440 nm in not that high – in fact, its not uncommon for it to be near detection limits in low-cdom lakes, when using a 1 cm cuvette, on many specs.
Referee#2 Comment 23

We chose this wavelength as a compromise between absorption signal from the spectrometer and wavelength available in Sentinel as one should expected better correlation between information in the same wavelength. Also, as describe, it is a common to choose this wavelength in remote sensing studies.

Line 114: Please also specify that this is a oneway ANOVA on ranks – more informative if someone happens to not know the name of the statistical test offhand.
Referee#2 Comment 24

The sentence will be changed to:" Kruskal Wallis test (oneway ANOVA on rank) with a significance level of 95% was applied to test the existence of significant…"

Line 118: I had trouble understanding this sentence, and how spectral slope was treated in relation to the hydrography. Please revise.
Referee#2 Comment 25

We changed the sentence in the manuscript to: "The mean $S_{275-295}$ among months of the same hydrograph phases (e.g. July and August for receding; March and April for rising) was computed for each sampling point to analyze their variability within each lake and phase. "

Section 2.3.2: Generally, just describing that you made plots is not necessary in a description of statistical analyses. However, I would like to know what programs (or packages, if using R or python or the like) were used.
Referee#2 Comment 26

We changed this part in the manuscript: "…phase. The statistical analysis were performed using the software Matlab (Mathworks, Natick, MA, USA)."

Line 127: I agree that Monte Carlo is likely a good way to get around the limited number of samples, but was there any bias in how the calibration/validation data were split? I could imagine that might also influence results, if certain sites or seasons were over or under-represented in either dataset.
Referee#2 Comment 27

We tried to select the most representative model out of 100,000 runs by applying the methodology to select the model described in Augusto-Silva et al. (2014). Note that we didn't select the model with best performance, but the one that was in the range of most representative mean square error and coefficients. Also, the Monte Carlo was only run in the model selection and not validation. In this way, we tried to minimize the bias.

Line 140: The work done in Arctic rivers and estuaries are sometimes impacted by high sediment – see Matsuoka et al (multiple years – especially look at Mackenzie River and beaufort sea papers), Herrault et al 2017 (although this is the Yenisey, which has low sediment), and Griffin et al 2018. Brezonik et al 2015 also includes data from the St Louis River Estuary, that has both high CDOM and high sediment. While these papers do not propose the same model formation as you do, I believe most of them also mention the issue of sediment, and some include band ratios that incorporate NIR bands. Olmanson et al 2016 also uses the NIR landsat band. I think the rationale you present here is sound, and I have no problem with your model formulation, but I don't think you can claim others have not tried to develop models for similarly high NAP and high CDOM environments. I would also say that these sites are not necessarily

"high" CDOM environments – that is, first, a somewhat subjective term. In addition, in my experience, CDOM is visible to the naked eye around 3 m-1 a440 – and only a portion of your samples reach that threshold. That's not necessarily the only way to claim that something is "high" CDOM, but it's an easy rule of thumb. Indeed, you acknowledge at the end of the discussion that this study only includes a small range of CDOM.
Referee#2 Comment 28

We appreciate the suggestion and we agree. The text will be change to:" Once the relationship between S_{275-29} and a_{CDOM} (440) was modeled, another algorithm was calibrated and validated to estimate a_{CDOM} (440) using the simulated MSI Rrs. There is some effort in the literature to propose models to estimate a_{CDOM} in complex environments, regarding high CDOM and Non-Algal Particle (NAP) contribution to the Rrs (Matsuoka et al., 2009; Matsuoka et al., 2012). In this study, we propose a new model for estimating a_{CDOM} (440) introducing a ratio between near infrared bands to remove NAP contribution from its inorganic fraction. The rational for introducing this ratio is the null signal of CDOM and the dominance of NAP in this wavelength range (Kirk, 2011)"

Line 153: Are these averages across all sites? Please specify
Referee#2 Comment 29

Yes, the averages are across all sites. The text will be changed to:" The highest amplitude of a_{CDOM} (440) in the entire data set (e.g. across all sites) occurred in March (1.22 to 5.46 m^{-1}) and April (1.60 to 5.97 m^{-1}), with averages of 2.56 and 3.01 m^{-1}, respectively"

Line 155: Looking at Figure 3, at least some of the more highly colored lakes in Mar/Apr look like there's spatial variability within the lake. What did you do to conclude there was not spatial variability within lakes?
Referee#2 Comment 30

We based our conclusion in the Kruskal Wallis results, that showed no difference between lakes when all the data set was analyzed.

Line 156: This sentence and the next I had some trouble following.
Referee#2 Comment 31

The sentences will be changed to: "The water level during the sampling campaign in the rising and receding phase was almost the same (mean:30.04 ± 1.38 m). However, at the rising phase, high variability (CV: 52.45%) of a_{CDOM} (440) occurred, while in the receding a_{CDOM} (440) variability (CV: 14.74%) was much smaller."

Line 160: What were the differences? Specify
Referee#2 Comment 32

The sentence was rewritten:" Kruskal Wallis results using samples from all lakes and dates indicated that there are significant differences (p<0.001) in aCDOM (440) between lakes and hydrograph phases. After the removal of Buabuá and Mamirauá samples acquired in March and April (rising), Kruskal Wallis results showed no significant differences in a_{CDOM} (440) values (p=0.51). The two runs indicate that DOM

Figure 5: Was there a statistical test or grouping analysis done to draw these circles, or just by sight? Also,it looks like the Buabua and Mamiraua do separate from the other lakes, even during the receding phase (and you point this out in the text). That contradicts the claim in
Referee#2 Comment 33

The circles were draw by sight. This data is related to the mean $S_{275-295}$ between the two months of the same hydrograph phase (please see comment 21). Indeed $S_{275-295}$ varies between hydrograph phases, but this shifts are not follow by a_{CDOM}. In Figure 6 you can see how this two variables correlate.

Line 170 – I realize that refers specifically to a254, but given that the figure 4 shows all spectra, it's a little confusing
Referee#2 Comment 34

We opted to show the entire spectra to also discuss it shape. A254 was also chosen to explain the differences in the magnitude of the spectra because it is not close to the limits of instrument detection and it is a common parameter analyzed for aromaticity. Even though aromaticity was outside the scope of the study, one could use it to compare with other sites.

Line 180: Please describe Sr in the methods
Referee#2 Comment 35

It was described at "2.3.2 Spectral slope determination": "The spectral slope ratio (SR) between the wavelength intervals of 275-295 nm on 350-400 nm was also computed in the same away describe in Equation 3". And the sentence on line 180 was changed to:" S_R was applied to trace DOM sources in a tropical river-wetland system (Dalmagro et al., 2017)."

Figure 6: Vantrepotte et al (2015), not 2005, in the caption. Also, what adjustment is being referred to? Also, please provide more detailed description of the statistics and model formulation (coefficients, etc), perhaps in a table.
Referee#2 Comment 36

It has been changed in the caption: "Figure 6 - Adjustment of the model proposed by Vantrepotte et al. (2015, Equation 7) (in red) and adjustment of the proposed power-law model described in Equation 6 (in yellow)." The coefficients of the equation were add on line 188 in the section "3.3 Seasonal relationship between a_{CDOM} and $S_{275-295}$" as follow, since the information regarding model fitting is already expressed in the figure:" The model proposed by Vantrepotte et al. (2015) was tested using the entire data set (coefficients of equation 0.05, 0.10, 3.06 and 0.0), but a power-law function provided a better fit (Figure 6).

Line 194: I found this sentence confusing. Please rephrase.
Referee#2 Comment 37

The sentence was re written: "As in the receding phase a_{CDOM} values are very similar between lakes and no correlation was found between a_{CDOM} (440) and $S_{275-295}$ the model was developed just with the samples acquired in the rising phase.

Line 195: What model was selected??? Which data were included? Is this the model that was developed using Monte Carlo?
Referee#2 Comment 38

The model selected was to estimate $S_{275-295}$ from $a_{CDOM}(440)$. The model selection in the sentence states for the coefficients chosen based on the Monte Carlo simulation. For this model, only data from the rising phase were selected. The sentence has been changes in the manuscript: "The selected model to estimate $S_{275-295}$ from a_{CDOM} (440), developed using Monte Carlo and data from rising phase, shows a satisfactory fit (MSE<0.0001) and is described in Equation 6:"

Section 3.3: See major comment
Referee#2 Comment 39

Please see reply to comment 3

Figure 8: The equations are mislabeled.
Referee#2 Comment 40

Now it has been changes: "Figure 8 – Measured versus estimated scatterplot of (left) $S_{275-295}$ (nm^{-1}) and (right) a_{CDOM} (440) (m^{-1}). Equation 4 was used to estimate $S_{275-295}$ (Y axis). Equation 5 was used to estimate $a_{CDOM}(440)$ (Y axis). The red solid line indicates the regression line between measured and estimated values; the red double dotted lines are the 95% confidence interval; and the black dashed line is the 1:1 line."

Line 213: But the previous paragraph just stated that hydrography was a controlling factor of CDOM! Is the flood pulse not a controlling factor of water level? If not, that needs to be explained more fully.
Referee#2 Comment 41

What we wanted to state here is that the water level is not the major factor controlling a_{CDOM} since at the same water level we have different values of a_{CDOM} (440). Thus the phase of hydrograph (related to the flood pulse) is a key point in the a_{CDOM} dynamic. The sentence will be change to:" The water level in the floodplain is quite similar between the rising and receding seasons, suggesting that the flood pulse is the major factor explaining the variability of those optical variables"

Line 229: There are more recent studies on the Amazon about the role of DOC and inland waters on carbon cycling – Hastie et al 2018 (or maybe 2019) models that for the entire amazon, for instance.
Referee#2 Comment 42

We appreciate the suggestion, but we decide for another reference, more recent than before and carried out during seven years (1994–2000). "Considering that in the Amazon basin, DOC accounts for 70% of total organic matter and that floodplain areas are relevant sources of DOC to the Solimões/Amazon River (Morreira-Turq et al., 2003)"

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