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Interactive comment on "Use of absorption optical indices to assess seasonal variability of dissolved organic matter in amazon floodplain lakes" *by* Maria Paula da Silva et al.

Anonymous Referee #2

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Overall comments: The authors present a study of Amazon floodplain lakes during the rising and falling limbs of the hydrograph, using absorption spectra and simulated satellite remote sensing data to investigate the utility of remote sensing applications to the region. Overall, it's a very interesting dataset, with contrasting lakes (in terms of CDOM and hydrology). Remote sensing of inland waters is a rapidly growing field, and this is a valuable contribution, particularly calling out the need to explicitly address how to formulate models for complex waters with high CDOM and NAP.

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

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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?

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.

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. 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. 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.

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.

Specific comments:

Intro: 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 Line 27: Hastie et al 2019 on the Amazon carbon budget that incorporates aquatic cycling would be good to cite here and elsewhere Line 33: In what cases? 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. 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. 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. Line 47: describe what a412 is, and aCDOM more generally. 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. Line 62: I had a little trouble following this sentence. Consider revising. Line 64: commas are overused in this sentence 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 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 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? 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? Line 94: How long were samples stored before analysis? 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? 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. Line 114: Please also specify that this is a one-way ANOVA on ranks – more informative if someone happens to not know the name of the statistical test offhand Line 118: I had trouble understanding this sentence, and how spectral slope was treated in relation to the hydrography. Please revise. 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. 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.

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. Line 153: Are these averages across all sites? Please specify 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? Line 156: This sentence and the next I had some trouble following. Line 160: What were the differences? Specify 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 Line 170 – I realize that refers specifically to a254, but given that the figure 4 shows all spectra, it's a little confusing Line 180: Please describe Sr in the methods 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. Line 194: I found this sentence

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confusing. Please rephrase. Line 195: What model was selected??? Which data were included? Is this the model that was developed using Monte Carlo? Section 3.3: See major comment Figure 8: The equations are mislabeled. 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. Line 229: There are more recent studies on the Amazon about the role of DOC and incland waters on carbon cycling – Hastie et al 2018 (or maybe 2019) models that for the entire amazon, for instance.

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