Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC4
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# Comment on bg-2022-47

Anonymous Referee #4

Referee comment on "Recent significant decline of strong carbon peat accumulation rates in tropical Andes related to climate change and glacier retreat" by Romina Llanos et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-47-RC4, 2022

The authors use 4 short peat cores from 2 high-elevation Peruvian peatlands to discuss recent changes in carbon accumulation rates (CARs). They found very high apparent rates of carbon accumulation at these sites. They document a decrease in CAR after 1980 that could be caused by an increase in annual temperature; they corroborate this hypothesis with d13C values from the peat cores. The authors assume that d13C values are proxies for temperature (this assumption comes from a different article). Overall, this is an interesting study that could be appropriate for CofP readership, but I am afraid it is not ready for publication. I have numerous methodological issues (some may be flaws), which are described below. In particular, the CAR calculations may not represent true recent changes in carbon dynamics. Likewise, I have serious doubt about the application of the temperature transfer function to the d13C of bulk (?) peat. I hope the comments below are of use to the authors, who should definitely review their study and submit at a later time.

Idea: It would be interesting to read about why you think these young peat deposits started developing less than 100 years ago -- could make for a good discussion of the article.

GENERAL COMMENTS:

Study area: I would have liked to read more about the hydrology of the area, its vegetation, whether it is pristine or impacted by local communities (and animals), etc.

Why did you choose those 2 sites should also be covered, as well as brief descriptions of those 2 sites (including the coring sites themselves). As of now, this section lacks important information.

Methods: lots of important information is missing (see the list below).

Results: (1) the trends in CAR that "slow down" in the early 1980s might be due to an autogenic process: the young peat has not decomposed and compacted yet, making TOC values smaller than the older peat. This would potentially yield lower CARs... (2) the changes in d13C are not considering the Suess effect. Other factors impacting d13C should also be tested/discussed, including hydrological changes.

SPECIFIC COMMENTS:

Abstract:

lines 14-15: "...since glaciers have been recognized as one of their vital water sources" -- this is true for some Andean peatlands, but certainly not for MANY of them. This statement is therefore too general and misleading.

line 23: a "new" proxy... the reference you are using is over 10 years old! not so "new" (and they were not the first ones to use it either...)

Introduction:

line 35: This statement about "all carbon in the atmosphere" is incorrect!! "The amount of C stored in peatlands is similar to the total C stocks in all living biomass or in the atmosphere"

line 68: what are you referring to here? "between 500 and 700m in length"

line 72: this is not a "new" method!!

Study Area

line 77 / Figure 1: I'm a bit surprised by your delienation of the watersheds; I'm not familiar with this region, but why is watershed 1 so large and watershed 2 so small? Is watershed 2 in fact part of watershed 1? I don't understand why you are reporting the size of the watersheds...

Methods

line 105: why are the cores shorter than the PVC tubes? Are the peat deposits only 29 to 35 deep? If so, you need to mention this important "detail".

line 110-113: please add references to the methods you describe.

line 120: why not use a Bayesian approach? It seems like the standard in paleoecological studies these days. Bacon can accomodate for your postbomb dates.

line 129: by convention, you must report against which international standard your d13C values were calibrated against! (VPDB?). Also, and perhaps more importantly, how did you sample and prepare for d13C measurements? Did you measure the bulk peat, the Distichia leaves, or something else? Did you extract the cellulose or not? What weight did you use in the lab? A lot of information missing here... that would hinder replication of your study.

line 131: you should use organic matter density (rather than dry bulk density) to truly estimate CAR... You can do it since you have OM% from the LOI measurements...

lines 134-135: you need to explain the mechanism that links d13C with temperature... If I remember the Skrzypek study, they used an elevational gradient to build their relationship, which means that temperature may NOT be the main factor, but rather changes in pressure...

lines 135-136: this sentence does not make sense to me; what do you mean? "This value was similar to the previously reported range for other species (included Sphagnum peat: -0.5 to -0.6‰/°C)"

line 139: why did you use 600 mb in this case? Are there known limitations/issues with using the NCEP/NCAR reanalysis in the high Andes that should be documented?

Results:

Table 1: you say that you report "2 sigmas", but clearly you do not. Instead, you only report the calibrated age - it's unclear if this is the mean, median, or most probable age provided by Clam. Since those are post-bomb dates, it would be useful to know the most plausible age ranges (on either sides of the postbomb calibration curve).

Figure 2: I cannot tell which dates (and error bars) belong to which cores! would it be possible to have 4 panels (one for each core)? It could go in the supplementary file...

lines 163-165: you say that "there was a general upward trend in TOC content from the peat basal depth of the cores from both studied peatlands to approximately 13 cm (the early 1980s) and then the TOC values decreased to the top of the cores (2015 CE)". This is likely because the uppermost samples are "fresher", being that they have not undergone decomposition and compaction. This is likely why your recent CARs are lower than your older CARS... In other words, this could all be an autogenic signal that has nothing to do with a temperature change.

line 174: mean CAR were higher at APA1 than at APA2 - probably because APA1 has high bulk density?! It would be worth to calculate organic matter density for a fairer comparison of those sites.

line 189: did you consider the Suess effect at your sites? It is expected that d13C become more negative over time because of fossil fuels mixing in the global atmosphere... "At

both peatlands, there was a general trend to more negative δ13C values from the basal depth to the top of the cores". Getting rid of the Suess effect would be very useful. Then, I see that your 2 cores tell different stories: one of them (the red line on Fig 5) would show increasing d13C values vs. the blue curve would show a decreasing d13C trend. As mentioned in my intro, I am not convinced that these are temperature records. These could relate to hydrological changes: could it be that one site is becoming wetter )blue line) vs the other one is becoming drier (red line)? Please look into the literature that discusses stomatal closure.

Discussion:

lines ~ 200: you should read the paper by Benfield and Yu, Distichia deposits from Columbia were analyzed... You'll see that they also document very high recent CARs.

lines 195-205: you cannot compare your core tops with Holocene-aged cores and say that your cores have greater CARs! This is obvious: short peat hasn't decomposed much, especially compared to old sites... Figure 6 is a misrepresentation and flawed way to compare these data. For a fairer discussion, only look at recent CARs from around the world... There are plenty of data to play with!

I did not comment on the rest of the discussion, as I question the validity of the results.