Review of cp-2020-4

Dear Leticia Luz,

It has been a pleasure reading your manuscript. I think it is a very interesting comparison between two datasets form two closely located cores. I think we can learn a lot form these kinds of studies including the one presented here. That being said, I have difficulties following the text. I have the feeling there is a lot of duplication in the description of the currents, for one and I think it would be really good if you would check the writing thoroughly again. On top of that there are some weird things I would like to mention, I doubt if anyone co-injected water with a known isotopic composition into a GC setup for alkenone analysis. I am guessing that the nC27 n-alkane was not for quantification, you already describe quantification in the Uk section, but actually was used as isotope standard to be co-injected with your samples. The nC27 from Arndt (not Arna) Schimmelmann has a pre-determined isotopic composition. Hydrogen isotopes are expressed in ‰ relative to VSMOW (0‰). This complete mash-up of this methods section makes me wonder about the knowledge of the authors and the quality of measurements and/or the involvement or interest of the person that did the actual measurements?

My slightly negative feelings are further strengthened by the ice volume free oxygen isotope record. According to the manuscript this was obtained by correcting for the Uk temperatures. So it is a temperature corrected δ^{18}O record, not and ice volume free δ^{18}O record? δ^{18}O of forams and I will ignore diagenetic overprinting, is determined by (calcification) temperature and the δ^{18}O of seawater. The latter is correlated with salinity and affected by ice volume especially in these glacial/interglacial records. To get to salinity the forma record has to be corrected for temperature and ice volume by subtracting a benthic foram record, for instance. If you did what you said, the IVF record does not only reflect changes in salinity? Be careful there.

Your actual measured δ^{18}O records are not so different from each other, except maybe for the bump in the coastal record during the deglaciation. The temperature records are different and that basically determines the difference between the temperature corrected δ^{18}O records. Again, be careful with what you are looking at. In this case the temperature comes from different organisms than the δ^{18}O, which will result in additional uncertainties. The mismatch between the δ^{2}H of the alkenones and the δ^{18}O of the forams suggests that these organisms reflect different growth conditions, water masses and/or seasons which does not make it any easier. A Mg/Ca based temperature correction might be better. Of course, other people have also used Uk temperatures to correct δ^{18}O to get at water isotopic composition and with that salinity. So it is not necessarily wrong, just be careful and discuss this potential problem. Especially since your whole story is based on the temperature corrected δ^{18}O records and not the actual measured data.

The last thing that makes me wonder a little what is going on with this manuscript is the $\Delta\delta$ SST from figure 6, big delta as difference fine, little delta is for isotopes not Uk based SSTs. Very strange.

All in all, I think that this is an interesting study, but I think the data needs a bit more work and I am not entirely sure the authors know exactly what they are doing or some of them have not seen the actual submitted version. As is it can not be published.