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Biogeosciences Discussions

Interactive comment on "Late Holocene variations in Pacific surface circulation and biogeochemistry inferred from proteinaceous deep-sea corals" *by* T. P. Guilderson et al.

Anonymous Referee #1

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This paper presents a very nice new dataset of stable carbon and nitrogen isotopic measurements from deep sea corals. These measurements provide insight on the long-term evolution of carbon and nitrogen cycling near both Hawaii and the Line Islands, over the late Holocene, as well as during a brief window just prior to the Holocene. The authors do quite a thorough job of covering background and describing the results, and I think they come to a well-reasoned conclusion that the main transient signal is due to a weakening of trade winds over the Holocene.

I think the paper should certainly be published. The following comments are aimed at strengthening some parts of the argument, correcting some details, and bringing the

conclusions across more emphatically.

1. I think the title could be changed to better reflect the main conclusion, that trade winds decreased in strength over the mid-late Holocene.

2. Has the possibility of changes in N deposition been ruled out for the HOT-ALOHA trend?

3. The 13C explanation (p 3933) strikes me as being indirect, in that it discusses 13C as being dependent on nutrient cycling. I find this confusing, since 13C is a stable isotope of carbon, and therefore can be significantly decoupled from nutrient cycling by air-sea exchange. Would it not be better to discuss the d13C variability as a combined consequence of the equilibrium values, determined mostly by temperature and salinity, in comparison with the degree of disequilibrium due to upwelling of respired DIC?

4. There is little discussion of the dependence of d15N on trophic level. Is there good evidence that the trophic level of these corals is invariant over time? If not, could it be a significant component of the observed variability? I think this needs to be mentioned, at least.

5. Page 3932, line 17: I don't think it's sufficiently precise to refer to this sample as spanning 'the end of the last deglaciation'.. If the age model is correct, the coral is actually from entirely after the Younger Dryas, when the most dramatic climate shifts were pretty much finished. What I think would be more interesting is if the coral actually spanned the end of the Younger Dryas, which was around 11,500 years ago. This would be particularly intriguing since speleothems (e.g. Wang, Science 2001) show shifts in tropical hydrology that would be qualitatively consistent with the coral records, with a rapid shift at the end of the Younger Dryas and a gradual return to Younger-Dryas-like conditions from the mid- to late-Holocene. I would suspect this is a possibility, within the uncertainty of the reservoir age - which should be on the order of a few centuries, at least.

6. A recent paper by Cobb et al. (Science 2013) made a strong argument that there was no discernible change in ENSO over the Holocene. It seems to me that this may be sufficient evidence to discount the possibility of ENSO having any role in relationship to the data presented here - which would allow the ENSO-related arguments (pp 3935-3936) to be removed.

7. Page 3937. I think the Holocene dust deposition changes would have been negligible - they are tiny, compared to the glacial-interglacial changes.

Finally, I wonder if there may be some useful connection with the Kienast et al. (GRL, 2008) sediment d15N changes in the western tropical Pacific, which show a similar decline - though I suppose it could be coincidence!

Interactive comment on Biogeosciences Discuss., 10, 3925, 2013.