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

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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.

Although the interpretation is a decrease in the strength of the trade-winds over the mid-late Holocene, the primary record is that of 15N and 13C. We would prefer to, in this instance, keep closer to the primary data. If required by the editor, we could make an accommodation along the lines of: Mid-late Holocene North Pacific trade winds as inferred from deep-sea coral 15N and 13C.

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

trend?

Authors' response:

In general and over the industrial period there has been an overall increase in dustiness (as reflected in mid-latitude North American ice cores with back trajectories to east asia). Such a source would have most likely resulted in an increase in δ15N not the decrease that we observe.

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?

Authors' response:

If we understand the question correctly, the disequilibrium flux is, within the context of CO2 concentrations, "rapidly" ameliorated over the few month window that equilibration takes place. This is in contrast to the time required to isotopically equilibrate ($\sim$10 years). As the reviewer notes, there are a number of controls on the (steady state) 13C content of surface DIC including air-sea exchange, temperature, etc. We conclude that equilibration, temperature, etc., are not sufficient to explain the range or the direction of change that we see in our record. We are happy to clarify this section with less muddled prose.

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.

Authors' response:

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There are very few detailed studies that explicitly describe the potential impact of changes in trophic structure on sinking POC (either sedimentary or in proteinaceous corals) when interpreting stable isotope (13C and 15N) results. The assumption has been that trophic changes (and diagnosis) are minimal. Given the shallowness of our sample suite (in general $\sim$ 400m or less) we can probably neglect diagenesis of sinking POC. We currently assume that the trophic structure remains constant through time and acknowledge that this is a weakness in all bulk isotope measurements, including our own. One study that explicitly determines trophic structure is an elegant study by Sherwood et al., where they use compound specific stable isotopic analyses of Gulf of Maine (Atlantic Ocean) primnoids to elucidate source water changes. In their study, the "trophic level" CSI-AA 15N did not indicate trophic level changes. We are actively pursuing the CSI-AA approach to a subset of samples in our collection.

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.

Authors' response:

We agree with the reviewer and will change the text to better articulate the time-span of this coral. We did not mean to imply that the specimen spanned the end of the deglaciation, but at the end of the deglaciation and as the reviewer notes, post Younger-Dryas (within calibration uncertainties).

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We would be hesitant to attempt to compare discrete ‘spot’ results from our initial curosory sampling with that of a continuously sampled speleothem. One possibility is to sequentially sample the coral so that we gain fidelity (individual sample resolution) and a continuous time-series over the age-range of the specimen.

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.

Authors’ response:

The paper by Cobb et al., was published subsequent to our submittal and we were unaware of its existence. We will, and in accordance with both your and reviewer 2’s request, modify the section in question to include the Cobb et al., results.

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

Authors’ response:

Holocene changes are smaller in amplitude (inferred absolute flux) than G/I changes. We will clarify the text to avoid future misunderstandings.

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!

Authors’ response:

It is possibly a coincidence, and it is difficult to see a common singular source for an advective signal. That being said, the coupling of the ocean and atmosphere on a variety of timescales does not preclude a change in the overlying large-scale wind field

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yielding a response in shoaling of the mixed layer and/or an expansion of the NPSG that simultaneously has an impact on the nutrient dynamics in the western equatorial Pacific. This is something that we will explore further.

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