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Interactive comment on "A novel paleo-bleaching proxy using boron isotopes and high-resolution laser ablation to reconstruct coral bleaching events" *by* G. Dishon et al.

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This is a very interesting study that further explores the use of boron isotopes as potential novel paleo-bleaching proxies. This is clearly an important area of research and while a previous study failed to find a link between short-term coral bleaching and changes in boron isotopes, the authors provide evidence here that longer bleaching events may be recorded in the skeleton. The experimental approach and the use of massive (?) Porites coral are key strengths of the study. While reading the paper, several questions arose and I think it could be helpful for the reader to address these during the revision process.

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Discussion Paper

I agree with reviewer 2 that it is not clear how the authors mechanistically link coral bleaching to decreases in d11B. Do they suggest that decreases in pH in the diffuse boundary layer are reflected by d11B? If so, how would that be possible given that d11B does not record seawater pH directly, but rather the increased internal pH of the calcifying fluid? What about the hypothesis that pH-upregulation may be compromised by coral bleaching?

In this context, it seems unfortunate that measurements of Fv/Fm were only conducted until bleaching was seen (defined as what kind of Fv/Fm values?). Since the authors also measured d11B during the recovery phase, continuous Fv/Fm measurements (or other physiological measurements such as chlorophyll a, symbiont density and calcification) would have helped to link the various observed bleaching states (i.e., the severity of bleaching) to a certain decrease in d11B.

The comparison with a naturally bleached, mesophotic coral is very interesting, especially since physiological data are available for this coral, and seems to corroborate the findings observed in the experiment. On the other hand, bleaching in this mesophotic coral is highly unusual in that the coral paradoxically seems to perform better in a bleached state. Can the authors elaborate on a potential mechanistic link for this coral and the implications for the Porites coral?

One major concern is that the one control coral shown in Fig. 1a had unusually high d11B values (up to 28 permil). What about d11B values of the other measured corals from the control treatment? Were they also that high? Clearly, such high values for the control corals would make it easier to define decreases in d11B as a "bleaching footprint". Further, the authors state that decreases in d11B of as little as 1.5 permil may be interpreted as a bleaching footprint, yet offsets between analytical sessions required a correction of 2 permil for some of their data.

The authors state that in contrast to short-term coral bleaching (Schoepf et al. 2014b), longer bleaching events such as the one simulated in their experiment will be recorded

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by d11B. Although the heat stress phase in Schoepf et al. 2014b only lasted 2.5 weeks, the corals were nevertheless physiologically bleached for much longer, yet changes in d11B were not observed even 1.5 months after the heat stress phase. Can the authors elaborate on this discrepancy between the two studies, especially since two of the study species in Schoepf et al. 2014b were also Porites corals?

Could the authors clarify the sample size for each treatment? The numbers differ between the Supplement and the main text and are also not consistent with the number of tracks shown in Figure 1. Did the authors keep track of which fragments originated from which of the two, original parent colonies? Also, how robust do the authors think that their results are given that only two adjacent colonies were used for collection? Is this enough to establish a novel proxy?

Were pH and seawater carbonate chemistry monitored throughout the experiment? These data would be useful to demonstrate that d11B could not have been affected by changes in seawater pH.

Can the authors provide more information regarding the conditions under which the pH microsensor measurements were made. It is not clear whether they were made using treatment seawater in terms of temperature, nor what the flow conditions were which can significantly affect the diffuse boundary layer. How reproducible were the microprofiles? At what time point during the experiment were these measurements made? Since light levels were more than twice as high than during the experiment, wouldn't they corals have been light stressed?

Interactive comment on Biogeosciences Discuss., 12, 8131, 2015.