

Interactive comment on “Plant responses to volcanically-elevated CO₂ in two Costa Rican forests” by Robert R. Bogue et al.

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Review response to SC1: We thank the reviewer for highlighting the utility and great potential of using elevated CO₂ from volcanoes to understand long-term changes on ecosystems. We note that the primary criticism in this study raised by the reviewer is how we analysed the wood core isotopes with respect to the time dimension. We agree that this aspect needed further work. The exact growth chronology was not central to our primary results, so we now reference a range of growth rates from the literature for comparison.

"In this paper, the authors investigate the potential use of tree wood carbon stable isotopes as proxies for elevated CO₂ exposure in a volcanic environment. The approach

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has a great potential for understanding the effect of elevated CO₂ on plant's physiology. This study, however, suffers from high uncertainty in the methods applied to analyze the 13C from the tree core, There is no clear chronological constrain for tree growth of the time frame (presented here as the 3 growth years) and thereby on the resolution discussed in the analyses. Below are detailed comments. It is never clear how the tree 13C data match the resolution for the soil CO₂ flux (as presented in Figure). The methods and results need more details in order to contextualize the implications of the documented elevated CO₂ signature."

Establishing quantitative growth rates for the trees studied was outside the scope of this preliminary study but would be very helpful for future studies attempting to use our methods. For comparison, we provide references to a range of analogous growth rates. We will clarify the methods for measurements of CO₂ flux and 13C in the revised manuscript. Soil CO₂ flux was measured with an accumulation chamber near the base of the tree (generally within 5 meters, terrain permitting) at three different points and then averaged to provide a single CO₂ flux value to compare to the 13C measurement of the corresponding tree sample. This technique is intended to provide a simple relative way to compare the CO₂ exposure of different trees, as a tree with high CO₂ flux near its base should experience consistently higher CO₂ concentrations than a tree with lower CO₂ flux.

"Line 50: you introduce rubisco without defining what it is."

We will define rubisco in this sentence in the revised manuscript.

"Line 73. "It is" instead of "it was" unclear"

This wording will be edited in the revised manuscript.

"In the Investigated locations and sampling strategy section: there is no reference to figure 1 where sampling transects are described."

A reference to Figure 1 will be added.

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"section 2.2 title: "Species studied" replace by "studied tree species""

This change will be made in the revised manuscript.

"The authors describe that the sampling for isotopes was conducted using a drill and drilling holes in the outermost 5 cm. Was this at any specific resolution? or just aimed at generating wood powder for analyses?"

This was only intended to generate wood powder for ^{13}C analyses. We will clarify this in the revised manuscript.

"The authors estimated that the outermost 5 cm correspond to the last 2-3 years? what is this assumption based on?"

This assumption is based on the existing literature of analogous growth rates. We will clarify the text that this was not measured by us, and this is explicitly unknown in our study.

"Lines 232-233: Are there any information about the canopy height, it seems that the 0-100 m would be the most appropriate level. Does the model capture finer vertical differences in the CO_2 concentration within the canopy for e.g. between 10m and 40 m?"

The modelled atmosphere up to 500 m agl may see a CO_2 concentration influence from, or have an influence on, the air in the tree canopy. The broad scale of the modelled domain was designed to assess if an external influence of CO_2 from San José is a significant measurable component of the air above the measurement sites, but not to model the under-canopy or within-canopy air flows. The model results shown in Fig. 2 represent only the 0-100 m agl air. We will clarify this in the wording in the revised manuscript. We do not have precise canopy height measurements, but all trees measured ranged from ~5-30 m and we estimate canopy heights at about 20-30 m on average at our measured sites. We do have DBH measurements, which are related to canopy height, but future studies should measure canopy height. We did not measure

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or model vertical differences in CO_2 concentrations; but, we agree that this would be important for future studies. As such, we acknowledge that limitation to our study, and frame our results as suggestive rather than definitive, pointing to this type of design for future studies.

"Lines 250-252. The measurements in this study reveal that CO_2 levels are high based on CO_2 fluxes data. The ecosystem growing in this environment are certainly exposed to those high CO_2 concentration levels, however, the results described in this section do not show the link to stomatal conductance and chlorophyll concentrations."

We thank the reviewer for noting the elevated CO_2 concentrations that ecosystems are exposed to in this environment. For clarity, we will move the mention of the specific measurements (i.e. stomatal conductance and chlorophyll concentrations) to the appropriate section (3.3).

"The CO_2 concentration level are measured at soil level, do you expect the concentration to remain similar at higher canopy level, say 15-20 m?"

We note that we present soil CO_2 fluxes, not concentrations. As mentioned above, vertical changes in volcanic CO_2 concentration were outside the scope of our study but would definitely be important for future studies. We will add lines to the discussion to clarify this.

"Lines 265-266: the ^{13}C values for *A. acuminata* and *O. xalapensis* are identical (statistically not different). Was the difference between the two species and *B. nitidia* tested statistically?"

No, because the current study did not focus on quantifying interspecific differences. Such testing would be important for future studies.

"The following sentences are confusing. As CO_2 flux increased, the wood cores contained progressively higher amounts of ^{13}C for two of the three species. Tree core $\delta^{13}\text{C}$ showed no relationship with stomatal conductance for any species. As CO_2

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fluxes increased over time or spatially? how was that tested in the case of time? There is no mention of stomatal conductance data or else and here a brief description of the relationship between $\delta^{13}C$ and stomatal conductance? "

We will rework this section to improve flow and add clarity. All changes in CO₂ flux are spatial, and the temporal variability of the higher, geologically dominated CO₂ fluxes (>12 g m⁻² d⁻¹, see Fig. 3) is known to be a very dampened, long-term signal related principally to very slow (decadal to centennial) changes in deep geological CO₂ supply, and changes in top soil permeability (we conducted all measurements in the dry season). With regard to stomatal conductance, our data were preliminary and instantaneous, and were not meant to be compared with values of $\delta^{13}C$. Again, such a comparison would be informative in future work.

"In the discussion, the authors argue that the existing significant correlation between the $\delta^{13}C$ in the trees and soil does not imply that trees were incorporating the heavy volcanic CO₂. The values of tree's $\delta^{13}C$ should reflect the incorporation of eCO₂"

We apologize for confusion in our wording. The reviewer is correct in that the values of the tree $\delta^{13}C$ reflects the incorporation of eCO₂. We will edit this statement in the paper for added clarity.

"Line 312, Why is it assumed to be 400 ppm? if it is the atmospheric value is 400 ppm for the particular period: several years from tree rings then it should be referenced to from atmospheric measurements (Mauna Loa for example)."

400 ppm was chosen as a mean value, and a few ppm in either direction makes little difference to the final result (the Mauna Loa values vary by a mean of 2 ppm year-to-year, which is much less than the seasonal variability in CO₂ in air at 12-15 ppm range). We did not intend for these calculations to represent perfectly accurate measurements of volcanic CO₂ exposure, but merely to give an idea of the magnitude of CO₂ enhancement. We will edit this section to better reflect this intent.

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"the sections 4.3 and 4.4 are very speculative as the results presented and discussed in the paper are exploratory and bear some weaknesses in the sample processing, particularly the tree cores age and investigated period. These two sections can be shortened and merged."

We agree with the reviewer and will shorten and merge these two sections. We put these as separate sections because 4.3 is intended as a theoretical framework for the implications of trees recording volcanic CO₂ emissions, whereas 4.4 is more directly intended to aid potential future studies that intend to study the same areas and questions that we explored.

"Line 422. The statement of identifying areas of dense old-growth forest is not based on any chronological framework. They are potentially old but the methods used in this study did not provide any dating of the trees."

We agree that this statement was more for discussion, as opposed to a detailed dendrochronology. We will remove this statement to avoid confusion.

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