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Interactive comment on "Ecosystem responses to elevated CO_2 using airborne remote sensing at Mammoth Mountain, California" *by* Kerry Cawse-Nicholson et al.

Kerry Cawse-Nicholson et al.

kcawseni@jpl.nasa.gov

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This is an interesting study using a purported natural CO2 enhancement gradient to understand ecosystem scale responses to elevated CO2. The authors use a linear regression model to control for a couple of covariates to discern the effect of eCO2 on structure and process.

We thank the reviewer for noting the interest of our study using a well-documented natural CO2 enhancement to understand ecosystem scale responses to elevated CO2.

Overall, the empirical model results in confusing results, which the authors try to explain

by referring to similar studies in other naturally enhanced CO2 systems.

Some of the results of this natural long-term exposure experiment may seem confusing at first because they contradict shorter-term experiments. But, this is exactly why we did the study—if we knew what the results were going to be, there would be no reason for the study. Moreover, the results highlight numerous points made throughout the literature with respect to the FACE experiments—their short-term nature has been unable to uncover long-term results, which is exactly the unique strength and a primary purpose of our study. We edited the manuscript to make these points more clear (and to make understanding the results less confusing).

I find the discussion quite speculative and have two concerns on the study and the usefulness of volcanic-CO2 seepage as an experimental setting.

We agree that the Discussion is structured more as a Discussion, less as Results. We tried to make clear that this study was exploratory, rather than definitive, and that this study was meant to identify both potential signals as well as design elements for further study.

1) The authors argue that the Mammoth Mt region is very well studied and that variability in CO2 over time and space is minimal, and that the ecosystems in the area are in some equilibrium with the seepage. But even ignoring variability before measurements began, the Figure 1 shows very high variability since measurements first started. I don't think we can say with any confidence what the CO2 exposure has been over time and space, and whether the current study reflects the equilibrium conditions to eCO2.

It is a fundamental principle of volcanology that all active volcanoes emit CO2 continuously during their entire life cycle. The CO2 emissions at Mammoth Mountain have been well known since at least 1989, and their variability well documented by repeated CO2 efflux mapping between at least 1995 (ongoing), by the USGS (Werner et al. 2014). Multiple sites have since at least 1995 been continuously monitored for CO2 by the USGS (McGee Gerlach, 1998), showing highly invariant continuous excess CO2 emissions at these sites (e.g., Rogie et al 2001; Werner et al., 2014). The CO2 seeps at the site have been known for even longer (Varekamp Buseck 1984, and geothermal assessment reports from the 1970s at least). All these studies show that the active Mammoth Mountain volcanic system has experienced a replenishment of the magmatic CO2 source in the deep subsurface in about 1989, possibly already an earlier one in 1978, though no systematic CO2 measurements were conducted in that earlier time period (Hill, 1996). Werner et al 2014) show remarkable spatial consistency for 9 years of systematic measurements at the CO2 gas seeps on Mammoth Mountain.

2) The authors focus only on eCO2 as a driver of variability in structure and processes. Soil conditions (physical and chemical) are overlooked and it is quite possible that some sort of chemical toxicity is interacting with plant growth and causing the unusual 'eCO2 responses' that the team finds.

The reviewer is correct in that soil chemistry is altered at the points of CO2 emission (e.g., McGee Gerlach, 1998). However, we excluded those areas, instead focusing on the fertilization zone, which is away from those emission points, with unaffected soils, where tree canopies are exposed to the CO2, which has diffused in the atmosphere away from the emission points.

Minor comments: - Define MASTER and ASO when first used - Effect of canopy height model (selecting tallest pixel in each 1 m2 grid cell) will likely bias the biomass estimate to outliers, why not use percentiles, i.e. 90th, to avoid this artefact? - Please discuss a bit more the sample size used to develop the plant traits models with AVIRIS.

We will define MASTER and ASO at first use. Outliers have already been removed as part of the preprocessing of the biomass estimate. We will clarify this point. We will include information on the foliar trait model development in the next version of the manuscript.

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