

Interactive comment on “Impacts of prescribed burning on soil greenhouse gas fluxes in a suburban native forest of south-eastern Queensland, Australia” by Y. Zhao et al.

Anonymous Referee #1

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The presented study is not a before/after control/impact (BACI) study design. And the data are not presented in this way and have not been analysed in this way.

A BACI requires that control and treatment sites/plots have to be established before the treatment (in this case prescribed burn) and that these sites/plots are monitored for the variable of interest for some time before the treatment and after the treatment. For a BACI to work it is of highest importance that the replication of measurements is large enough to capture spatial variation of the variable of interest within the control sites and within the treatment sites because the interpretation of the study results purely relies on changes in the differences between the control and treatment sites/plots in the mean of the measured variable.

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A proper BACI would have been the appropriate way of looking into the question if prescribed burning has an impact on soil GHG fluxes and soil variables associated with soil GHG fluxes and their controlling soil variables. However, the experimental design of the presented manuscript cannot answer these questions as I have already outlined before:

a) Since temporal variation of soil GHG fluxes is large and also depended on weather conditions (time of day, time of year) the first ANOVA only tells us that there is temporal variation in soil GHG fluxes. There might be absolutely no causality between the burn and this temporal variation.

b) Since spatial variation in GHG is also large and there have only been 4 chambers used per 'treatment' (one chamber at 4 locations) in this study it is not possible to determine if the differences between the burned sites and the 'control' sites after the burn are at all causally linked to the fire or simply a result of spatial GHG flux variability.

As a result of this the interpretation and discussion of the data is severely limited and the current discussion goes far beyond what this limited dataset can offer.

I further believe that the manuscript does not clarify how the data were really analysed since it is completely unclear to me that 2 separate ANOVAS were performed. This shows that two reviewers have to speculate what has happened since the manuscript as presented does not allow a clear understanding of the experimental setup or the statistical approach.

Some other points to consider:

- At no stage is discussed if there might have been runoff mediated lateral flow of nutrients or ash from burnt to unburnt areas in the 1 year timeframe after the burn
- An coefficient of variance analyses that would indicate how spatially variable soil GHG fluxes and other variables for each measuring date are is missing
- The data have not been put into any climatic context – please provide weather data

C5456

(precipitation, air temp, RH) for the study period and put this in context with your soil moisture and temperature measurements

- Please provide soil bulk density and particle size analysis data for the study plots
- For anyone to make an informed decision if your spatial replication was sufficient please provide the raw data of your GHG measurements
- Please provide information what type of regression analysis you have used and provide adjusted R² values instead of R values. Also please reconsider if 3 data points are enough to do this type of analysis and if you meet the underlying assumptions of the test used
- Some of the references are not up to date and some of the newer literature on wildfire or prescribed burning effects on soil GHG fluxes in eucalypt fluxes is missing

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