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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 study aimed to investigate the impact of prescribed burning (a widely used management tool to reduce risks associated with wildfires in Australia) on soil CH4, CO2 and N2O exchange.

The impact of the risk management tool 'prescribed burning' on soil atmosphere greenhouse gas exchanged has only received little attention in the past and studies on this subject are therefore timely, important and fall within the general scope of BG.

The manuscript presents a study on the effect of one prescribed burning on soil CO2, CH4, N2O fluxes and some soil properties at four replicated sites in one forest system following a prescribed burn in May 2013. However, the presented study has a number

of shortcomings:

Most perplexing, since quite unusual in context of the large amount of publication on methods used to quantify soil greenhouse gas (GHG) exchange in forest ecosystems, in the presented manuscript one site is synonym with one soil flux chamber. In general at least 5-10 chambers per site are necessary to capture spatial variation in soil CO2, CH4 and to a lesser degree N2O exchange.

The authors claim to have measured 4 independent replicated sites whilst they potentially have not even measured enough chambers at the one site they investigated to capture the spatial variation in the measured soil fluxes sufficiently. The fact that they measured fluxes for each chamber on four consecutive days during each of the 2/3 field campaign does not make the result any more reliable. The four chambers/sites are located in the perimeter of one singular prescribed burn and are approximately 50 meters apart. The application and interpretation of the results of this study are therefore severely limited since we are dealing with no replication of the burn and pseudo-replication with regards to sites.

Another problem is the type of statistical analysis used. A repeated measurement ANOVA was used to analyse the data using measuring event as a factor. It is, however; well known that the measured soil fluxes vary temporally therefore presenting statistical results on the fact that the fluxes were different between the measuring events has very limited application.

Furthermore the authors opportunistically established additional non-burned controls in the burn perimeter after the burn (un-burnt patches) and compare these to the burned sites. Therefore the number of sites/chambers and the chamber location after the burn is not the same as before the burn. The authors are not clear about the statistical test used to analyse these results but it can be assumed that they again used the repeated measurement ANOVA. However, a repeated measurement ANOVA should not be used for this type of analysis because one of the assumptions is that the number and the individuals measured stay the same over time. In the presented manuscript the chamber numbers must at one stage have increased from 4 before the burn to 8 after the burn. Further it has to be assumed that the authors simply treat the 4 before burn locations as equivalent to the additional chamber locations. This is completely inadvisable and due to the large spatial variation in the measured soil fluxes and soil parameters will likely lead to incorrect result interpretation and overstated conclusions.

The opportunistically added un-burnt controls cannot be used in a comparison with the data gathered before the burn. Furthermore their value when compared to burned patches after the burn is questionable since differences between the un-burnt patches and burned patches could simply be attributed to spatial variation and might not at all be attributed to the impact of the fire.

As a whole the limitations presented by the experimental design of this study (one site, to little replication, opportunistic measurement regime) do not allow any of the interpretations and conclusions the authors make in the over-lengthy discussion of the manuscript. Observed effects are very likely based on temporal and spatial flux variability and cannot be attributed to the prescribed burning treatment.

It is further quite interesting that no data were collected in the first 2-3 month directly after the burn this would be the time-frame when fire effects could be expected to be most dominant.

Furthermore, there are no data presented to put the measuring campaigns into a climatic context => it is impossible to assess if the temporal variation observed in the flux data was simply attributed to differences in weather (soil moisture, soil temperature. . .). Soil moisture is only presented as gravimetric soil moisture content which is quite useless in the context of soil GHG exchange (volumetric moisture content is more appropriate).

The manuscript would also need to be edited by a native English speaker and will need to be completely rewritten in plain English. In its presented form it is unreadable in parts

and it is left to the reader to interpret sentences to make sense of large sections of text (especially in the result and the discussion section).

Interactive comment on Biogeosciences Discuss., 12, 10679, 2015.