

## ***Interactive comment on “Responses of soil physico-chemical properties to combustion: a space for time substitution study to infer how changes in climate are likely to affect response of topsoil to fires” by S. N. Araya et al.***

**G. M. Davies (Referee)**

davies.411@osu.edu

Received and published: 3 May 2016

Overall I thought this paper was a useful contribution to our understanding of how variation in fire severity induces changes in soils across heterogeneous landscapes. The manuscript was well-structured and easy to follow and very clearly written. The lab methods were generally presented in detail and evaluated a wide range of metrics potentially influenced by fire. The figures and tables were generally very clear and nicely drawn. There was a small number of typos throughout and I've drawn attention to these in the annotated manuscript provided below. I think the paper is definitely

C1

suitable for publication but some major changes are required:

1) I have to admit that I wasn't particularly convinced by the climate change "story" the paper currently seeks to build itself on. First I didn't see how the design can really be considered a space for time substitution (indeed that's never really justified in detail in the introduction or methods). My guess was the authors are suggesting that the lower elevation sites are meant to emulate higher future temperatures. I would argue that's a pretty broad simplification and I'm not sure I can go with it - future higher temperatures will be super-imposed upon existing soil types (changing them) creating novel edaphic-climate combinations and new ecosite types. The present study is more of a sensitivity analysis examining differences in the response of soils to varying (simulated) fire intensity. That's certainly not uninteresting in itself and should be more than enough justification.

I think this is the strongest link the authors have to argue climate change implications of their work can be found right towards the end of the discussion (page 18). I'm not convinced about the climate series idea - soils are likely to change only slowly to climate change with very significant lag. The soils represent the results of underlying geological conditions and millenia of differing biological activity - surely that won't be erased overnight by climate change alone? What might happen is that alteration to disturbance regimes will alter vegetation and microbial communities and, in the process, alter soil properties and soil forming processes. I would urge the authors to recast their paper on more reasonable grounds

2) I thought the lab methods could do with greater justification and a greater consideration needs to be given as to whether the methods really emulate what happens during a wildfire in any useful way. For instance what might the implications of working with dried soils be? How does the presence of water in the soil affect physical and chemical processes during the passage of the fire front - why not study how moisture content and heating temperature affect changes? I would also like to see more thought about whether the heating times are appropriate. I would have preferred to see some study of

C2

the effects of heating duration. 30-40 minutes is a long time for a fire to be resident at a site. I warrant that it might approximate conditions under a smouldering log but then to what extent are you actually simulating changes more generally associated with a fire - logs occupy a small proportion of the soil surface.

3) There is room for improvement of the statistical analysis. Specifically: - Mixed models would be more appropriate (no need to average cores) - Stats (main test at least) need to be reported in full (even if in tables in supplementary material) - Data appears ripe for analysis with multivariate methods. Constrained ordination (e.g. Redundancy Analysis) would be particularly interesting as would allow you to test how changes in properties occur across temperatures and sites - have the authors considered such approaches? - New analyses are introduced in the Discussion section that were never described in the Methods, some of the results appear incomplete - the authors refer to doing regression analyses but only present a table of correlation coefficients.

The Discussion section generally very good with nice links made to previous similar or related studies. There did appear to be some confusion between the concepts of fire intensity and fire severity though. I recommend following the usage defined by Keeley which has been widely adopted: [http://www.fs.fed.us/postfirevegcondition/documents/publications/keeley\\_ijwf\\_2009.pdf](http://www.fs.fed.us/postfirevegcondition/documents/publications/keeley_ijwf_2009.pdf)

Specific comments, corrections and requests for clarification can be found in the annotated manuscript attached.

Please also note the supplement to this comment:

<http://www.soil-discuss.net/soil-2016-4/soil-2016-4-RC3-supplement.pdf>

---

Interactive comment on SOIL Discuss., doi:10.5194/soil-2016-4, 2016.