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Interactive comment on "Soil CO_2 efflux in an old-growth southern conifer forests (*Agathis australis*) — magnitude, components, and controls" *by* L. Schwendenmann and C. Macinnis-Ng

Anonymous Referee #2

Received and published: 17 May 2016

General remarks: The interesting manuscript (ms) represents the investigations within a native forest in New Zealand with the aim to characterize dependencies between forest structure, soil properties, meteorological conditions and soil respiration processes. The ms has clear objectives and represents a good contribution to scientific progress in the interdisciplinary field of abiotic and biotic soil respiration influences. In order to describe temporal variability the data interpretation is based on times series over 18 month of CO2 efflux measurements. Furthermore, the authors considered a huge number of relevant references, giving a comprehensive insight and the chance to compare the approach with the results from other researchers. The overall quality of the

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Discussion paper

manuscript is high. It offers interesting insights into the soil CO2 efflux within an old-growth kauri forest and the main controlling factors for such a forest site. The entire results are discussed based on sound statistical analysis. However, in my opinion there are some issues which need to be discussed more in detail to close some minor gaps improving the ms.

Specific comments:

Line 36: Janssens et al., 2001 – two times in reference list – which one is meant here? → 2001a, b

Line 138 and Figure1: The figure 1 is not easy to understand concerning the experimental set up. There are some items, which are not explained in the legend: filled grey circles, filled grey stars, grey lines (I supposed it is the topography in m a.s.l.?) Where are the trenched plots? There is only text at the upper edge.

In the context topography: you mention a potential dependency between topography and organic layer thickness (line 448-450). I would strongly recommend analyzing a functional trend between soil moisture and topography and hence, maybe some influences on soil respiration. From your data compilation it is not clear to see, but maybe the soil moisture differences you mentioned in Table 2 for the trenched plots could be superimposed by topographic driven soil moisture differences. You can find some discussions in recent papers, e.g.,

Masamichi Takahashi , Keizo Hirai , Pitayakon Limtong , Chaveevan Leaungvutivirog , Samreong Panuthai , Songtam Suksawang , Somchai Anusontpornperm & Dokrak Marod (2011) Topographic variation in heterotrophic and autotrophic soil respiration in a tropical seasonal forest in Thailand, Soil Science and Plant Nutrition, 57:3, 452-465, DOI: 10.1080/00380768.2011.589363

Wang, B., Zha, T.S., Jia, X., Gong, J.N., Wu, B., Bourque, C.P.A., Zhang, Y., Qin, S.G., Chen, G.P., Peltola, H., 2015. Microtopographic variation in soil respiration and its

controlling factors vary with plant phenophases in a desert–shrub ecosystem. Biogeosciences 12, 5705-5714.

Line 221: . . . plant material (45% C, 25 2.3% N) . . . → 25 ??

Line 312 and Figure 2: You mentioned a relation between high CO2 efflux and heavy rain events (as described and shown in paper Macinnis-Ng & Schwendenmann, 2015). Why do not show precipitation information in figure 2? I suppose, that graph would visually support very well your interpretation!

Line 333: . . .Outside_Trench_Insered . . . t is missing

Line 537: reference Epron et al., 2001 is not in reference list

Line 564: . . .de Jong and Schappert. . .

Line 957: January

Line 958: The different letters .. indicates a significant difference . . . between what? Mean and Median? What means a, b, x, y, z?

Line 965a: The determined regression coefficients are in all cases very weak – hence, it is not really a convincing correlation! As an example, you could include a figure to show the different modelled approaches.

Line 965b: Table 3 subscription . . . adjusted R2 = coefficient. . . RMSE = root mean aquare

Line 798: reference Metcalfe et al., 2011 is not mentioned in text

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