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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 #1

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Review of manuscript soil-2016-21: Soil CO2 efflux in an old-growth southern conifer forests (Agathis australis) – magnitude, components, and controls

=== General comments

The manuscript describes a study of soil respiration in a native forest of New Zealand. It is well written, meaning correct and fluent language, a clear introduction and presentation of the methods and results. It manages to describe well the characteristics of soil CO2 efflux in this type of forest and has the advantage of being the first such study in this particular ecosystem. The originality of the study is mostly if not entirely the

result of this last point. While it presents correlation analyses and finds temperature and root biomass as the most important factors explaining the CO2 fluxes, it remains otherwise mostly descriptive. Some results, interpretations and conclusions are not entirely convincing. In particular, I would question the correctness of the model fitting section.

=== Specific comments

Introduction and Methods are well written. Here I find nothing to question. When trenching or inserting deep collars, severed roots can add to the decomposing pool and change the estimate of heterotrophic respiration. How were decomposing roots accounted for in this study?

In section 3.3 you describe fitting models for the T response but fail to mention the most common used i.e. Q10 or LT, etc.

The Q10-function is usually equivalent to an exponential function and has only 2 parameters, i.e. a * Q10ˆ((T-Tref)/10). Why do you have 3 parameters for the Q10 function? Is one a constant? Please check your functions in Table 3. Everything in the exponent should be closed by parenthesis. Also, the fact that you improved your R2 with a bivariate model but have much larger RMSE is not consistent. Check that your calculations are correct. Adding explanatory variables should only reduce the RMSE if you are using the same data.

It would be good to have plots showing the response to T and M.

In the discussion you calculate an average and compare with other ecosystems. Using the average of your measurements is incorrect. Since there is a T and root effect you should account for these when getting yearly estimates. At least use the T relationship, since T at night is probably lower, so the yearly average is lower than that of your measurements.

You discuss how the vegetation may control the amount of CO2 efflux. The question of

whether your system is near equilibrium is important here. If a forest is near equilibrium, the quality of the litter is important only in determining the stock sizes, not the CO2 fluxes. The latter will be equal to the amounts of input.

When discussing the effect of T, make clear that your T range is small, which does not mean there is little T effect, just that you cannot see it. In terms of the average yearly T, this will probably have a larger effect in how it affects the productivity fo the vegetation, so indirectly through litter input.

In conclusions, you state that the study has found that the vegetation type exerts a strong influence on soil carbon related processes. This is an effect of all land vegetation and is no finding by itself, thus making for a very weak conclusion. An insight on the vegetation effect on the soil C stocks or some other more specific observation should come here. Also, you mention that species effects were should in the study, however no species comparison was made, so mentioning species effects is incorrect, here and in the abstract.

Lines 317-319 This line is not clear to me

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