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Interactive comment on "Tree proximity affects soil respiration dynamics in a coastal temperate deciduous forest" *by* Stephanie C. Pennington et al.

Anonymous Referee #1

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The manuscript entitled "Tree proximity affects soil respiration dynamics in a coastal temperate deciduous forest" is in fact addressing three different questions: (1) tree proximity and soil respiration, (2) temperature sensitivity, and (3) required sampling effort. Only the first one is clearly reflected in the title. These three questions are relevant and within the scope of BG, but they are not novel and there is no novel concept, idea or tool that emerged for this study. This is an additional set of data (a case study). (1) The approach of linearly connecting the basal area of trees to a fixed distance (5 m) and ground respiration is simplistic. The distance at which an individual tree influenced soil respiration is probably dependent on the size of this tree. In other words, biggest trees are expected to have a stronger influence than smaller trees. There are several

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(many papers) relating addressing the effect of tree size and proximity on soil respiration that are not cited in this manuscript. Among them: Fang C, Moncrieff JB, Gholz HL, Clark KL (1998) Soil CO2 efflux and its spatial variation in a Florida slash pine plantation. Plant Soil 205:135–146. doi:10.1023/A:1004304309827 Metcalfe DB, Meir P, Aragão LEOC, Malhi Y, da Costa ACL, Braga A, Gonçalves PHL, de Athaydes J, de Almeida SS, Williams M (2007) Factors controlling spatio-temporal variation in carbon dioxide efflux from surface litter, roots, and soil organic matter at four rain forest sites in the eastern Amazon: PARTITIONING AMAZON SOIL RESPIRATION. J Geophys Res 112. doi:10.1029/2007JG000443 Katayama A, Kume T, Komatsu H, Ohashi M, Nakagawa M, Yamashita M, Otsuki K, Suzuki M, Kumagai T (2009) Effect of forest structure on the spatial variation in soil respiration in a Bornean tropical rainforest. Agric For Meteorol 149:1666–1673. doi :10.1016/j.agrformet.2009.05.007 Bréchet L, Ponton S, Alméras T, Bonal D, Epron D (2011) Does spatial distribution of tree size account for spatial variation in soil respiration in a tropical forest? Plant and Soil 347:293–303. doi: 10.1007/s11104-011-0848-1 Schwendenmann L, Macinnis-Ng C (2016) Soil CO2 efflux in an old-growth southern conifer forest (Agathis australis) – magnitude, components and controls. SOIL 2:403–419. doi: 10.5194/soil-2-403-2016 Reading these papers (but the list is not limitative) would have given way to analyze more finely the results, especially the last two.

(2) The observation that autotrophic respiration is more sensitive to temperature than heterotrophic respiration is also confirmative of many studies. Note that the paper Aguilos et al 2011 that is cited when discussing this point has not been accepted for publication in Biogeoscience, so the citation is wrong. Note that the citation Wei et al is incorrect: should be Wei et al (doi: 10.1016/j.soilbio.2010.04.013). The discussion of this fact is rather poor and miss one of the most important drivers of the apparent temperature sensitivity of RA: phenology. This may be important in the present study since soil respiration was measured over a full year and species are deciduous. Among many other sources, this has been discussed in: Epron D, Le Dantec V, Dufrêne E, Granier A (2001) Seasonal dynamics of soil carbon dioxide efflux and

simulated rhizosphere respiration in a beech forest. Tree Physiology 21:145–152. doi: 10.1093/treephys/21.2-3.145 Ruehr NK, Buchmann N (2010) Soil respiration fluxes in a temperate mixed forest: seasonality and temperature sensitivities differ among microbial and root-rhizosphere respiration. Tree Physiol 30:165–176.

(3) The third point deals with estimate the number of samples required for a robust estimate of the Rs. This has also been done plenty of time so there are two options: use it as a description of the site in the materials and methods section or do not only compare with other estimates but discuss more the reason why the number of samples required is higher in this study than in many others, thus why spatial variability is higher. Four lines is not enough. The discussion now is poor.

In conclusion, while the manuscript is based on an interesting data set obtained with valid methods, the discussion is not strong enough to reach substantial conclusions. A little more time would have been needed, maybe. One may expect the last sentence of the abstract to be the core of the discussion. The state of the art in the introduction should also be reinforced by looking more in details in the huge relevant literature. The argument that no study has examined the influences of trees on spatial variation of Rs in the Chesapeake Bay watershed can be used for millions of watersheds in the world. This sentence should be removed.

Specific comments:

Line 21 (and 47): remove “in time” there is no evidence that a better knowledge of spatial variation will improve scaling soil respiration “in time”

Line 35: need a clear definition of what is stand. In the description, there are 3 sites and 3 plots within site, but no stand.

Lines 64-66: This sentence is very speculative, probably wrong and not needed. At similar age, tropical forests are at least as productive as temperate forest, and evergreen forests in a given climate are at least as productive as deciduous forests on an

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annual basis.

Line 107: linear or exponential regression. . . which one is reported?

Line 122: why not try to consider the size of the tree when increasing the radial distance. I mean include only big trees when far from the collar and all trees when close to the collar. Why not testing all distances from 1 to 15 m and use the one that give the best correlation with soil respiration?

Lines 125-139: provide the model. According to Table 3, soil moisture has a specific equation. Which non-significant terms were eliminated using the forward-and-back stepwise algorithm. This is not so clear that terms have been removed when looking at Table 3

Lines 140-145: it will be better to add the type of season as an additional factor in your model rather than running the model on a split dataset. Same comment for the dryness splitting.

Line 173: 40% is not almost half. But maybe it is 49%, not 40 (table 3)

Lines 185-187: not need to recall hypothesis in the result section.

Lines 195-201: the analysis will be greatly improved by considering not only the distance but also the size of the neighbour trees that interact with the distance (bigger trees have influence on longer distance)

Lines 293: the data does not support the idea that the high spatial variation is related to stand structure. First, only BA is considered for characterising stand structure, which probably is not enough. And second, this BA does not explain so much the variation.

Table 1: add the altitude of the three sites so that the risk of submersion can be evaluated by the readers

Table 2: do not use +/- for SD, this is statistically incorrect (OK with SE or CI only). Instead use parentheses.

Table 3: Improve the presentation (less digit, SE in the same column than value with +/-). Check df values, there is a problem. And show, in addition to the global model, the all the sub models you use (dormant versus growing). And the three dryness thirds as well. But see previous comments on the model.

Figure 1 is not very informative (not useful)

Figure 2: why mean flux? It is called individual observation in Fig 4 which seems better

Figure 3: hard to see what happens at short distance, especially at 5 m that is the selected distance. Can a log scale improve readability?

Figure 4: check x-axis labelled

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