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## ***Interactive comment on* “Seasonality in a boreal forest ecosystem affects the use of soil temperature and moisture as predictors of soil CO<sub>2</sub> efflux” by S. M. Niinistö et al.**

### **Anonymous Referee #2**

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The authors address the important question of seasonality in soil CO<sub>2</sub> efflux in forest ecosystems and try to identify factors controlling the temporal variation through both measurements and modeling. The study took place more than ten years ago but the topic is still highly relevant and challenging. Seasonality and interannual variability of carbon fluxes in forests are in general poorly understood. The results support more recent findings on the importance of seasonality in carbon inputs to soils and highlight the importance of wintertime measurements of soil respiration.

It was a pleasure to read the manuscript. It is well written and easy to read and follow. The content of the paper is reflected in the title. The results are presented in a clear

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and concise way and the discussion part is excellent. The conclusions are supported by the results.

General comments:

My only concerns are related to the methodologies used. In Introduction, it is stated that the authors expect the soil CO<sub>2</sub> efflux to be controlled by both environmental factors and other factors that vary seasonally (page 2814, lines 11-14) and yet no measurements of such other factors were made. Not even soil moisture was measured from the beginning of the project. I do understand that many of the findings presented in the Introduction (e.g. Högberg 2001; Wallander et al., 2007, 2001) were not available at the time of the study and that there were technological limitations. Nonetheless, wouldn't it have been possible to monitor e.g. phenology (both canopy and at the surface layer), incoming PAR at the ground level, root growth, distance to trees (as a proxy for root exudations of photosynthates), substrate quality, microbial biomass etc?

The soil temperature measurements are problematic, I think. Why was the temperatures measured at such a shallow depth (1-2 cm in the organic layer) in a study aiming at investigating temporal variations in soil CO<sub>2</sub> efflux? Couldn't it be expected that the efflux at a given temperature would be lower in the early season since deeper soil, where most of the CO<sub>2</sub> is produced, would still be colder than later in the season? How about the spatial variation in soil temperature? How representative was the single measurement point used for the modeling for the different plots? If you were using just one measurement point, wouldn't it have been good to measure a soil temperature profile at that point?

The soil moisture measurements are also problematic in my opinion. Again, what about the spatial variability? How representative are a few measurement points outside the plots? Do you see any problems with using different approaches at different plots? Where were the gravimetric measurements done?

How were the wintertime measurements of CO<sub>2</sub> efflux made? Were the chambers

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placed on pre-installed collars or placed directly on the ground? If placed directly on the ground, were the measurements taken at different locations each time? Do you expect that measurements of soil CO<sub>2</sub> efflux with different types of chambers/methods can lead to biased results? Why wasn't the same equipment used for both wintertime and snow-free periods? In the results (page 2819, lines 15 and 24) you show that the minimum values are basically the same, suggesting that it would have been ok to use the same equipment. Why didn't you measure throughout the whole snow-covered season? Do you think that these few measurement occasions are sufficient to capture the temporal variation in CO<sub>2</sub> efflux? How do you think the annual estimates would have been affected if you had included measurements during all snow-covered months?

Specific comments and technical corrections:

Page 2815, line 8: Is this LAI value valid for all three plots, despite the large differences in tree densities (see lines 23-24)?

Page 2816, line 13: What were the characteristics of this plot? I think you need to add a description of soil properties and stand characteristics (tree density, LAI etc. here).

Page 2816, line 23: How was the CO<sub>2</sub> concentration analyzed? How was the CO<sub>2</sub> trapped in the metal straw?

Page 2817, line 11: Do you count the moss layer as a soil layer?

Page 2818, lines 20-25: Can you describe the reasons for the logarithmic transformation of CO<sub>2</sub> efflux? I also think it would be good to add a description of the linear/quibic models tested (more than what can be found in Table 2).

Page 2819, lines 13-14 and Fig. 1: It appears as if the soil CO<sub>2</sub> efflux drops before the decline in soil water content and not as a consequence of that decline. However, the tick marks on the x-axis of Fig. 1b are not the same as on the rest of the panels in Fig. 1, which makes it difficult to judge.

Page 2822, line 4: 0.8 m<sup>3</sup>m<sup>-3</sup> should be 0.08 m<sup>3</sup>m<sup>-3</sup>, I think.

Page 2825, lines 15-20: I think you should add a description in the Methods section to why you measured the water content of moss. This was the point where I understood why those measurements had been done.

Page 2826, lines 5-6: How could differences in snowpack structure contribute to higher efflux values in 1999-2000 as compared to 1998-1999 when you removed the snowpack in order to do the measurements?

Page 2842, Table 2: You mention that you used Ts at 5 cm depth but in the manuscript you write that the soil temperature used for modeling was measured at an equivalent depth, i.e. 1-2 cm depth?

Page 2843, Fig.1b: Marks on the x-axis are not the same as on the rest of the panels (see earlier comment).

Page 2845, Fig. 3c,d: You present the results in mg CO<sub>2</sub>m<sup>-2</sup>h<sup>-1</sup> here but in g CO<sub>2</sub>m<sup>-2</sup>h<sup>-1</sup> elsewhere. I think it is good to be consistent so perhaps you should use one or the other but not both.

Page 2846, Fig. 4: See previous comment.

Page 2847, Fig. 5a,b: See previous comment.

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