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Reply on RC2

Minttu Havu et al.

Author comment on "Carbon sequestration potential of street tree plantings in Helsinki" by
Minttu Havu et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-242-AC2>,
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Response to the 2nd referee

We would like to thank the referee for his/her insightful comments and suggestions on our submitted manuscript, as they allowed us to improve it. Please find below the detailed responses. The reviewer comments are in italics whereas our response is plain text.

This paper reports on a modeling study of two urban street tree sites in Helsinki, Finland. It exploits a multi-year time series of field observations of tree sap flow, physiology, and soils for the two sites. Such measurements are rare in urban sites and using them to parameterize the models is a great strength of this project. Previous studies have measured and/or modeled urban tree net CO₂ exchange over one year to a few year's time. A significant new contribution of this study is that it couples an urban land-surface model (SUEWS), which is capable of representing photosynthetic CO₂ uptake and respiratory CO₂ release by the tree canopy in response to environmental drivers, with a soil carbon model (YASSO) which is capable to soil organic carbon fluxes and pools for the same conditions.

The modeling system and its parameterization are well documented and appropriately validated. The paper is well organized and generally well written (but see note below). The manuscript and its conclusions could be strengthened by moderate revisions, which are noted below.

We thank the referee for highlighting the novelty of the manuscript. Below, we discuss his/her suggestions to strengthen the manuscript in detail.

General comments:

1. Please explain more about how the study being on juvenile trees affects your overall conclusions. Plant relative growth rate will change as the tree size gets larger. What is the typical longevity of these tree species-- in "nature" and also what is typical maximum age

in the urban environment? Given the way the paper is framed around urban tree C sequestration potential and management for climate, it is likely that some readers could misunderstand or incorrectly extrapolate the findings to a mature urban forest or to the lifetime of the trees. It would help a lot if these points could be discussed when you are interpreting the main messages of your conclusions, including adding cautions or caveats where appropriate.

We agree with the referee's suggestion to add more cautions, as the studied street trees do not represent the whole variety of urban trees. In the revised MS, we will clarify that the expected lifespan of a street tree is approximately only 20-30 years, and we will also add a simplified estimation of carbon sequestration potential throughout this expected street tree lifespan by both models (see also the response to the comments by referee #1). Methods section will be modified to include description of the simplified estimation:

"A simplified estimation of carbon sequestration potential throughout the expected street tree lifespan was made using both models. The expected lifespan of a street tree is approximately 20–30 years (Roman and Scatena, 2011), and therefore, the estimation was made for 30 years (2002-2032) after the street tree planting. For SUEWS, both annual photosynthesis and plant respiration were averaged from pruned years (2008–2016) and assumed that the calculated average rate of photosynthesis and plant respiration will continue for 2017-2032. For Yasso, an additional model run using the average monthly mean air temperature and precipitation from the same years with stable root biomass was conducted."

Analysis concerning the new estimations will be added to the results section (L404). We will also revise the Discussion to include more insights on the carbon cycle of the street trees during their lifespan.

2. The aim of the modeling integrations to estimate the tree/soil C budget is clear, and it is a novel contribution. In fact, it is really something like site-based net C exchange, and perhaps it is not wrong to call it "sequestration potential". However, again for a general audience and for land managers who would read your work, it would be very helpful to explain how this relates to carbon sequestration as a climate concept. There, we normally think of carbon sequestered as being removed from the atmosphere and stored for a climatically relevant length of time (w.r.t. to fossil C emission reductions), such as 50-100 years or longer. How does a climatic concept of C sequestration relate to the urban system that you have modeled here? How long would the trees live or be allowed to grow on site before they grow too large (height interfering with wires, roots interfering with pavement, etc.), or before they die from insect outbreaks, urban heat, drought, road salting in winter, mechanical damage, etc.? What is the normal replacement interval for street trees like this? I am not asking that values be added for all of these factors; however, it would strengthen the paper if you can explain more specifically in what ways your results could relate to long-term carbon sequestration, and in which ways they do not.

As mentioned in our previous comment, we will add a simplified estimation on how much carbon these street trees can potentially sequester in their expected lifespan (30 years). Even though old street trees exist, road and other constructions, and renovations take place usually before street trees reach maximum size and age. We acknowledge that this manuscript does not cover all possible species and life spans, nor the full life cycle of the trees including the time in nursery, transport and construction nor the end use of the wood, as the SUEWS model is only able to produce the net CO₂ flux on local scale and Yasso the local soil carbon storage. Full life cycle assessment is thus out of the scope of the current manuscript. However, we thank the reviewer for the idea, and we will add

more insights on this to the Discussion. The discussion will answer in what way the results relate to long-term carbon sequestration, and in which ways they do not, and what kind of restrictions there is in upscaling these results. In the end, the aim of the paper is to introduce and validate the models, and use the models to examine carbon cycling of two street tree species. Thus, an application on a larger spatial scale is out of the scope of this paper.

3. It's understood that this is a modeling study, but it would strengthen the paper if the discussion included some comparison of your results to other field or modeling based studies of urban annual net biogenic (tree/soil) C exchange. There are some from northern climates such as Vancouver, Minneapolis, London?, even Helsinki. Broadly, how do the conclusions here compare to those obtained for tree-covered landscapes in cities that have been obtained through flux measurements and/or model upscaling?

We acknowledge the referees suggestion to include more net biogenic C exchange studies to the manuscript. However, we would like to have the focus only on street trees, for which such studies to our knowledge do not exist. Therefore, we have compared the SUEWS model results with other street tree models in Europe, and the soil carbon model with respiration and soil carbon stock measurements (especially in section 4.1). In the revised MS, we will highlight that there are not comparable studies available.

4. Please check for consistence of verb tense throughout the paper. In places it switches back and forth between past and present tense.

We thank you for this suggestion. We will revisit the manuscript throughout. Particularly corrections will be made in sections 2.4.1, 2.4.2, 2.4.3.

Detailed comments:

Does the soil freeze to a significant depth in winter at these sites? How was frozen soil handled in the YASSO simulations (does Rsoil decline or even stop)?

Usually, the sites have snow cover that protects the soil from freezing in winter and it is quite common that even if there is some ice formation, notable share of the soil water is in liquid phase and the soil temperature stays close to zero. In such conditions, the decomposition of soil organic matter is slow but still existing. However, total freezing of soil is possible in low air temperature if there is no snow cover but there is no mechanism in Yasso that would consider it. Instead, the decomposition rate follows just the changes in air temperature also in frozen conditions. In any case, such episodes when soil is deeply frozen are rare in Helsinki and as the time step is only one month, such episodes are insignificant. We will describe the model behaviour in freezing circumstances in the revised manuscript.

Modified text in section 2.5 (starting L274):

“Air temperature goes below freezing during the studied period but typically the snow cover prevents soil from freezing. Even if there would be some ice formation in the soil, notable share of the soil water would still be in liquid phase and the soil temperature stays close to zero. Also, in Yasso there is no mechanism to account for completely frozen soil. Thus, in the model runs the decomposition rate follows the changes in air temperature also in frozen conditions.”

line 129: Do you know how high was the groundwater table at the two sites? Did the level vary by season and, if so, how would that have affected the results? And a related question: Was any irrigation used at the sites? (I am assuming not regularly because it was not mentioned in the manuscript.) However, was it used during the early years of the trees' growth--it is common for irrigation to be needed in the first 2-3 years after establishment, depending on the local precipitation regime.

At Tilia site, previously undetected high groundwater table was found during the street construction, and the level varied between 50-90 cm below street level. However, we did not have any information of the seasonality, therefore, we don't know how this would have affected the observations. The street trees were irrigated weekly for two years after transplanting, but we did not take this into account in our model runs. SUEWS has an irrigation model that can be turned on/off but it is designed for garden irrigation and not for this kind of weekly irrigation. Current version of Yasso cannot take irrigation into account at all. We will add a clarification to the study site description about the irrigation.

Modified text (L98):

"The trees were irrigated weekly for two years after street construction. Irrigation was however neglected in the model simulations as Yasso cannot currently include irrigation and the irrigation model in SUEWS is designed for typical garden irrigation. This is expected to have a minor impact on the results. "

line 282-3: Aboveground litter was ignored in the simulations on the basis that the autumn leaf fall of these deciduous species is normally removed. This is a reasonable approach for modeling the C exchange of the "tree site" itself. However, I think you should take this issue further in the discussion and conclusions of the paper because you have "framed" the paper around urban tree plantings and sequestration. What would be the consequence of leaf litter fall for your annual carbon budget and how would this affect your overall conclusions and implications for how urban tree plantings affect the urban C budget?

We will add more insights to Discussion on the carbon content of the leaves as there are previous articles from these street trees that have already estimated some values. Between 2002 and 2011, the leaves would contribute cumulatively 12.5 kg C per tree of which approximately 7.3 kg C per tree still remained in 2011. The total C in pruning was 0.7 kg C per tree of which 0.6 kg C still remained in 2011 as the trees were pruned only after 2008 (Riikonen et al., 2017). As mentioned previously, we will add a simplified estimation of carbon sequestration potential throughout expected street tree lifespan and include the soil respiration estimate arising from the decomposition of leaves and pruned wooden parts.

line 321: Canopy densification was ignored (stopped) in the model after a certain year, based on the fact that these trees were pruned annually after they had reached that age. However, the biomass of leaves and branches removed by pruning would presumably be used to create mulch or compost or biofuel, etc, thereby all being released to the atmosphere. So, similarly as in the comment immediately above, how would the exclusion of the pruned biomass affect your annual carbon budget and what are the implications for your overall conclusions? If it is possible to make a quick quantitative estimate of these two carbon losses (collected litter and pruned biomass), that would be a nice addition that would strengthen the paper. If it is not possible to have a quantitative value, then it would

at least be good to add these points to the discussion and the explanation of conclusions about the total C budget from urban trees.

As mentioned previously, we will add more discussion on this to the revised manuscript, although the effect of pruning seemed small for these young trees (cumulatively 0.7 kg C per tree in 2003-2011) compared to the carbon in leaves and even our estimates. However, the trees were pruned only after 2008, which would affect the future estimations as the trees grow. To support the discussion, we will calculate soil respiration estimate arising from the decomposition of leaves and pruned wooden parts. As these are removed from the area of interest, the decomposition is taking place somewhere else but is naturally linked to the carbon sequestration of these tree of interest.

line 398: "climate neutral" is not quite correct, in my view. First, there are, of course, other effects of trees on climate besides net CO2 exchange. Second, there is the point about the model simulations being focused on the tree/soil system. I'd suggest writing "carbon neutral", and with the caveat that it's from the point of view of the tree/soil system (without the exports of leaf litter and pruned biomass).

We agree with the reviewer, that carbon neutral is more suitable term, and this will be changed to the revised manuscript.

line 426: When you discuss year-to-year variability, can you also say something about how important was the effect of annual differences in growing-season length, or timing? Especially in cold climates, these two factors can be important for annual C exchange, beyond only the variation in Tair.

We thank the reviewer for pointing this out. In the result section we noted on L386 that "Leaf onset begun at different times in different years depending on the simulated growing degree days, leading to a difference of up to 20 days in the model simulations." We extended our analysis for the whole growing season, and based on simulated LAI, the active season varied up to 26 days in the model simulations. We will add these details in the revised MS and extend the discussion also in section 4.1 (L426). However, as the weather conditions vary during the growing season despite its length so much it is not possible to derive conclusions on the exact impact of the growing season length on carbon sequestration. However, weather conditions vary throughout the season so with these model runs utilizing the measured weather, it is not easy to determine the effect of growing season length to the photosynthetic production of the whole year. We agree that it is an interesting point and we are happy to discover that in further studies where we or some other group run the model for other applications but here, we would like to keep current focus on model testing and simulating the initial development of a street plantation.