

Biogeosciences Discuss., referee comment RC3
<https://doi.org/10.5194/bg-2021-239-RC3>, 2021
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Comment on bg-2021-239

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Referee comment on "Physiological and climate controls on foliar mercury uptake by European tree species" by Lena Wohlgemuth et al., Biogeosciences Discuss.,
<https://doi.org/10.5194/bg-2021-239-RC3>, 2021

Comments on: Wohlgemuth et al (2021) Physiological and climate controls on foliar mercury uptake by European tree species

The authors of this paper have analyzed how meteorological/climate variables as well as leaf traits affect the uptake rate of mercury (Hg) of leaves/needles in a range of tree species over a substantial part of Europe. The data base is large, which is an important asset in this type of analysis, formed by observations made using the ICP Forest level II plots and in addition data from a dense sampling network in Austria. Important results include the relationship of foliar Hg uptake rate with leaf nitrogen concentration and leaf mass per area (LMA) as well as links with soil moisture, water vapour pressure deficit (VPD) and other meteorological factors, suggesting a close link of Hg uptake rate with stomatal conductance and physiological activity. In general, this is a well-written and valuable piece of work which should be published. However, improvements are possible, and a number of mostly relatively minor changes and amendments should be considered.

Specific comments:

Line 41: "simulated start of the growing season" – some more details about how the simulation was made should be included.

Line 98: consider removing "balanced", it does not become clear what this refers to in this context, and it is unnecessary to include this word.

Lines 112-118. Information about which needle age classes were harvested should be added as well as which needle age classes were used in different analyses.

Line 117: "typically"? Please be more specific.

Line 173: typo, "factors" should be "factor".

Line 204 "leaf unfolding" seems not to be the appropriate expression here!? Rather: "emergence of the new flush of needles"? A similar comment applies to the text under 3.1 of the supplementary information. Here it is stated (line 5 and line 22 on page 3) that it is the beginning of the "growing season" that is determined from the PEP725 database. But isn't it again the emergence of the new flush of needles which is treated here? The growing season of the older needles and thus "the beginning of the growing season of coniferous tree species" (line 5) – with stomatal gas exchange - starts much earlier.

Lines 275-278 and other places: there is a strong focus on current year needles in the study, although not completely. The authors should explain why this is the case. Most conifers retain their needles ~4-10 years and Hg accumulation will continue over several years. In the years after the first, evergreen conifer needles will have a longer period of physiological activity and uptake of Hg, starting earlier in spring and ending later in autumn, compared to broadleaved trees. Thus, even if the uptake rate of Hg per day (mostly used in the paper) is smaller for conifers, this will partly be offset by the longer duration over the year of gas exchange in needles older than current. This is significant when analyzing biogeochemical fluxes on a (multi)annual scale. To focus only on the uptake rate per day obscures the importance of the variability in the duration of Hg uptake over the year in different types of trees.

Lines 287-288: as the authors state, the otherwise very informative paper Zhou et al (2021) does not distinguish between different needle age classes, which makes it hard to compare with needle age specific Hg concentration data. Some other reference should be used to support the statement.

Line 338: should be "Leaf Mass per Area".

Line 345: should be "Figure 4" (not 44).

Figure 4: This figure contains very interesting information, especially the relationship between daily Hg uptake rate and foliar N concentration (representing physiological activity). Such a relationship could become very useful for large-scale modelling of Hg fluxes if supported also by further data. However, it is confusing that the daily Hg uptake rates, especially of the conifers, in Figure 4 are substantially higher than those reported for the same species in Table 1 based on a larger number of observations. Also, the N concentration values differ between Figure 4 and Table 1 for the same species. The authors should discuss these discrepancies, their causes and implications.

Figure 5 (this applies also to figures in the supplementary): While the relationship for pine has a reasonable distribution of data, permitting the authors to derive a quite clear linear relationship, this is not the case for spruce. For the latter species there are two clusters of data separated by a large empty space with respect to the x-variable, which contains no data. Linear regression is not to recommend for such a data set. An option would be to compare and test the difference between the two clusters with a t-test.

Line 371: It is not fully explained why the specific water VPD thresholds were used for different species. From where were these thresholds taken?

Line 389: replace "had" by "has".

Lines 393-394: "... the high degree of stomatal closure under drought stress of spruce". I do not understand this statement in relation to the data presented. The average daily Hg uptake rate, suggested by authors to be a possible proxy for stomatal conductance, does not differ very much between high and low VPD (Figure 5b), indicating a small response in stomatal conductance by high water VPD in spruce!?

Figure 7: Although there is a general trend for higher Hg concentrations in older needles in Figure 7, also seen in other studies, the pattern in the figure is not completely clear. This may partly be caused by the heterogeneity of the data, with a strongly varying number of observations for the different needle age classes. It may also be the case that the larger number of needle age classes included in the figure compared to most other studies, indicate a levelling off in the rate of annual increase in Hg concentration of the oldest needles. The authors should discuss the data presented in Figure 7 in further depth including the possible consequences of the heterogeneity of the data. The quite strong levelling off in annual Hg concentration increase for y4-y6 in Figure 7 is not in complete agreement with the statement on lines 432-436.

Lines 450-453 on stomatal conductance modelling: the phrase "model in a multiplicative way" is not appropriate and hard to understand for people not strongly involved in stomatal conductance modelling. Also, there exist other types (e.g., photosynthesis based) stomatal conductance models than multiplicative which could be considered. It would be appropriate to use a more recent reference than Emberson et al (2000), since a lot of development has taken place in stomatal conductance modelling over the last two decades. The authors may consider Emberson et al (2018) Ozone effects on crops and consideration in crop models. *European Journal of Agronomy* 100, 19–34, although it is on crops.

Line 480-481: The suggestion by the authors to assume that foliar Hg can represent the stomatal conductance (integrated over longer time scales) is interesting and thought-provoking. This statement must be based on the assumption that the atmospheric concentration of Hg is essentially constant, from year to year and from place to place. To what extent is this assumption valid? This should be discussed.

In the Conclusion part it would be appropriate to discuss the relationship between the net accumulation in leaves/needles over time periods of weeks to years, which is the focus in the paper, and the dynamic bi-directional fluxes of Hg to/from vegetation observed in many studies using highly time resolved measurements.

Essentially, this is a very interesting and valuable paper.