

Biogeosciences Discuss., author comment AC1  
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## Reply on RC1

Håkan Pleijel et al.

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Author comment on "Mercury accumulation in leaves of different plant types – the significance of tissue age and specific leaf area" by Håkan Pleijel et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-117-AC1>, 2021

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### Response to Anonymous Reviewer 1

*We want to thank the reviewer for valuable comments and suggestions. Please find below our response to the reviewer point by point.*

In general, the authors seem less up to date with the research front and the choice of references may seem somewhat dated. This is especially true of the obvious achievements made with stable isotope analyzes and paradigm shifts that affect the view of the mercury cycle in terrestrial ecosystems.

**Response:** *We have now included the references suggested by the reviewer. See below for more details of how the references have been incorporated in the revised manuscript.*

L69: I recommend e.g. Yang et al. PLOS ONE 2018 instead of Fleck et al. (1999) for wood analysis.

**Response:** *We agree that the recent reference is more relevant and have replaced Fleck et al (1999) by Yang et al (2018).*

L74: Yuan et al. says that re-emission is only partially counteracting uptake. There is no contradiction between Yuan et al. and Lindberg et al. (the latter is an old reference that hardly belongs here. Perhaps Bishop et al. and / or Sommar et al. both STOTEN 2020 should be referenced here) and it is difficult to understand that "however" is used in the following sense. Revise L74 - L77.

**Response:** *We agree that there is no such contradiction. The reference to Lindberg et al has been removed here, together with some associated text. Bishop et al 2020 and Sommar et al 2020 have been included. "However" has been removed and the text has been changed to "There are several observations of Hg<sup>0</sup> emissions from leaves (Bishop et al., 2020; Sommar et al., 2020). Yuan et al. (2019), using stable Hg isotopes and a branch chamber system, provided direct evidence of foliar Hg<sup>0</sup> re-emission partly counteracting foliar uptake. Empirical evidence of the development of the Hg concentration over time in leaves suggests exposure of vegetation to elevated atmospheric levels of Hg<sup>0</sup> generally result in a net accumulation in leaves/needles, which is not in conflict with the dynamical bidirectional fluxes found using high time resolution of isotope techniques (e.g. Demers et al., 2013)."*

L77: The sentence is grossly misleading. It points out the need to quantitatively examine re-emissions without mentioning that the study totally ignores doing so.

**Response:** *This sentence has been removed.*

L255: "... significant... analysis.". Can this sentence be rewritten to be easier to understand?

**Response:** *We removed the last part of the sentence: ", but no significant interaction between leaf age and species was obtained in the statistical analysis". The piece of text does not add any important information and may be confusing.*

Fig. 5a Spelling "agee".

**Response:** *Thanks, corrected.*

L337 and on: The discussion is very long without the reader being informed that there are several studies that clearly report the global distribution of atmospheric Hg uptake into vegetation and that especially the subtropics / tropics are important (eg Wang et al. 2016 EST and others). Instead, the discussion tends to delve into individual studies with unnecessary verbosity instead of measurement data being inserted and discussed in the context of the current state of knowledge (for example reported in Obrist et al. 2018 Ambio). The manuscript benefits from a sharp revision of the discussion.

**Response:** *We agree that the Discussion is long and can be shortened. In response to the detailed comments by reviewer Lena Wohlgemuth the text of the discussion has both been revised and shortened. We have also shortened the Discussion further in several places and made use of the information in Wang et al (2016) for the comparison with data from Rwanda and Sweden in our study as well as of Obrist et al (2018) for defining the context at the start of the Discussion. Some of the comments by reviewer Lena Wohlgemuth, however, also resulted in addition of text, but the Discussion as a whole, has been substantially shortened.*

L365: Here the authors' opinion from L74- is repeated. It must be said that the manuscript's lack of air analyzes is not meritorious. In what appears to be an attempt to reverse the perspective that empirical data are not compatible with extensive bidirectional exchange, a number of studies are discredited here that elegantly use stable isotope analyzes (e.g. Demers GBC 2013, Zheng GBC 2016 and Yuan EST 2019) to clearly demonstrate the importance of re-emissions from foliage. It can be repeated that the isotope studies in no way contradict continuous net uptake of mercury over the long term, on the contrary, the actual (gross) uptake is greater than what bulk analyzes of leaf samples (this study) can show.

**Response:** *We do not believe that bidirectional fluxes are inconsistent with net accumulation over time, but our phrasing was unfortunate. The text has been changed to: "Further, Yuan et al. (2019) showed that part of the Hg<sup>0</sup> that has been absorbed and oxidised in the leaves becomes reduced and re-emitted. This process is in no way inconsistent with our results that there is a net accumulation of Hg in plant tissue over time horizons of months to years. Isotope studies that have been undertaken (e.g. Demers et al., 2013, Yuan et al., 2019) show that the gross uptake of Hg by leaves is larger than what bulk analysis of leaf samples (net uptake), such as in our study, show."*