Responses to the comments and suggestions of an anonymous reviewer

Reviewer 2	
96: is this annual production?	Yes, we are talking about annual production. Necessary clarification made
115: can it be said with certainty what is and is not common in this case?	wording changed on lines 114-116
121 and elsewhere: the dot to signify multiplication is not needed in my opinion	corrected here and throughout the manuscript
159: tropical forests should be mentioned as well, if not only for completeness e.g. https://www.nature.com/articles/s41467-018-04658-y	an explanation is given as to why the review is limited to boreal and mid-latitude ecosystems (lines 159-162). The work cited by the reviewer is interesting, but it reports on laboratory studies of VOC emission by tropical soils, their microbiota, that is, it is not directly related to the topic of our review.
167: this paragraph is unnecessary. There's enough justification of litter mass and perhaps these points can be integrated elsewhere.	we still cannot agree that this paragraph (lines 169-173) is unnecessary. Not everyone is aware of the differences in litter biomass in forest and meadow ecosystems.
205: is Zimmer et al. the relevant reference here?	yes, this paragraph (lines 203-213) reports on the indirect contribution of the microbiota to litter degradation and is based on data published in 2003 by Zimmer et al.
211: Trowbridge et al. covered this topic for the case of soil fungi: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019JG005479	Thank you, we've added a link to this overlooked work (line 215).
239: does photodegredation result in VOC flux from litter? The photodegredation wection was a bit long and speculative and distracted from the main theme. It would be better shortened.	This is a legitimate question that should be answered in the future. And that is why we cannot agree that the discussion of the problem of photodecomposition distracts from the main topic. It seems important to us to draw the attention of researchers to the problem of VOC emission from litter under the action of solar radiation. Therefore, we abandoned our original intention to delete the paragraph starting on line 239, but changed it somewhat (lines 245-259).
for section II, subsections IIa and IIb for abiotic then biotic controls could help the reader navigate all of this material.	We agree with this remark. This section is divided into two parts
445 and a number of paragraphs afterward focus mostly on decomposition over time, which is interesting of course but it is unclear how this entire section contributes to a review	Here we draw attention and attach importance to the succession of micro-organisms precisely because it affects the composition of VOCs and the rate of their

of VOC emissions which remain largely uninvestigated as noted on line 485. Shortening this section to focus briefly on microbial changes during the decomposition process would help focus on the topic of the review.	emission from the litter decomposed by these microbes. In our opinion, this is directly related to the topic of the review. For greater clarity, we have added an additional clarification (lines 467-469).
Section V is great and makes key points about global representativeness.	Thanks a lot
Reviewer 3	
L18: Repetitive with L11, consider deleting.	Changes applied
L40: Delete "to" after the comma	It is done
L40-44: Consider making this one topic sentence by combining and streamlining. E.g. Terrestrial living vegetation is the main source of atmospheric VOCs that significantly affect chemical processes in the boundary layer. These emissions have garnered considerable attention due to the majority of them being highly reactive, and thus, impacting atmospheric chemistry…"	The reviewer's proposal was accepted, the corresponding change was made (lines 40-47).
L80: It's unclear why methyl chloride is emphasized here, instead, you could use the Bahlmann et al. citation to support the statement that significant biological sources remain elusive.	This passage is excluded.
L84: What does the acronym TOHRE stand for?	The abbreviation means what precedes it in this sentence: the Total OH REactivity
L87: identify and quantitatively characterize is repetitive, could simply say "identify and quantify." Also, it's unclear how we can characterize unknown reactive VOCs…this is a point that could be elaborated upon, and other well-known processes and effects (like the production and destruction of ozone, etc.) could be more streamlined and concise.	The proposed simplification is done. For clarity, the sentence has been reformulated.
L108: This sentence is unclear; the concentration of many secondary metabolites is significantly reduced compared to what? Fresh and living leaves?	Of course, in comparison with living intact leaves and in the order in which the foliage is mentioned in lines 107 and 108. For clarity, the sentence has been reformulated.
L134: While interesting, the text in this paragraph can be significantly reduced and streamlined. Also, it is unclear how the natural source of reactive VOCs has remained unaccounted for and unquantified if the Zimmerman et al. (1978) paper reported the magnitude of VOCs from leaf litter. Is it that this has remained relatively unexplored	The reviewer writes here: «*While interesting, the text in this paragraph can be significantly reduced and streamlined»* and then writes*: «Please clarify»*. So what to do: "significantly reduce" or "refine", that is, significantly expand? This phenomenon can be explained as follows: the 1980s and 1990s and the

since? Please clarify.	beginning of the new millennium passed under the sign of an intensive study of volatile emissions of LIVING plant foliage, and until this direction began to approach exhaustion, there were no people willing to switch to DEAD leaf litter. The first publications on this topic appeared in the early 2000s (Isidorov et al. 2002, 2003, 2005).
L154: Specify “these important components” as leaf litter and soil cover and consider moving the definitions of these terms (L172-186) up after L154 and end the paragraph with L154-158.	"these important components" are indicated (lines 152-154). However, the rearrangement proposed by the reviewer does not seem urgently necessary to us.
L187-189. This sentence is quite long and the clause “available information regarding the emission rates of separate VOCs” seems out of place and not well integrated. Consider revising for brevity/clarity.	From this not-too-long sentence, the reader can learn what will be discussed in the following sections of the review, and decide whether to read further. We don't understand why this seems out of place and "not well integrated".
L217: Change “they” to “that”	We replaced "they" with "that"
L233: Please provide a citation for this statement.	Literary reference added by us ((line 240)
L226: The relative proportion or importance. It would make sense that a lot or most of these processes are occurring all the time, but perhaps at different rates and interacting/overlapping in various ways depending on prevailing environmental conditions. This makes it sounds like processes like leaching appear and disappear, where in fact there is always that potential, but it becomes more and less important during certain times of the year and under specific conditions. Also, this topic sentence is inconsistent with the content. The biotic processes are mentioned, but this paragraph only focuses on abiotic factors. 236 is vague and not effective in supporting the rest of the text in this section.	Both. This notion is completely inconsistent with reality. Often there are periods of drought, and of course, there can be no talk of leaching anything out of the litter. In fact, "leaching appear and disappear" (moreover, disappears completely), depending on the appearance or disappearance of the precipitation required for this process. Yes, this fragment (as well as the entire section 2c) is devoted to abiotic factors and biotic factors are only mentioned as competing ones. Why can't they be mentioned? We do not understand the remark regarding line 236: which is called "vague and inefficient". We tried to give an explanation, but we are not sure that it corresponds to the comment of the reviewer.
L239: More pronounced distinction of what? Please specify and construct a more robust topic sentence.	We have made changes in lines 244-258

Also, L257-261 seem irrelevant considering the initial speculation was made on very limited data.	This is exactly what is written on the following lines of the same paragraph.
L283-286: Please provide a citation for this and it also seems like a bit of a no sequitur between paragraphs.	What is said in this paragraph is nothing more than an assumption, but it is based on common sense, according to which the role of photo/thermochemical processes is more significant in a completely or predominantly open area than under a forest canopy. Also, why does the reviewer say there is «no sequitur» between paragraphs? In the previous paragraph, it was just about the role of thermochemical processes.
L331: Please write out LSC in words to remind reader of the acronym.	The change was made, although the abbreviation LSC was given earlier (line 175).
L408: This entire paragraph is about one study, and instead, could be briefly summarized to follow up on what was stated on L405, emphasizing that VOCs after sufficient decomposition by microbes were likely the products of their metabolism and point out these specific classes et.g., C3-C8 carbonyl compounds, lower alcohols, etc. L445: the microbial succession section can be simplified and some information from the preceding paragraph incorporated within (instead of it standing alone with lots of extraneous information). Also, L459 is vague and uninformative as no other details are provided. The following sentence is equally confusing in terms of whether these patterns are generalizable or are site/ecotype specific.	The assertion that the paragraph beginning on line 408 is devoted to only one study is not true, and it is easy to verify this. We also do not understand why the phrase on line 459 (line 470 in the new edition of the article) seems vague and uninformative, namely: "The successional nature of fungal decomposition of the herbaceous litter of the Longleaf Pine savanna ecosystem was noted by Lodato et al. (2021)". We do not quite understand the meaning of the remark, but nevertheless make some changes (lines 475-476)
L470: I think this sentence is missing something at the end (a verb?).	Yes, for some unknown reason part of the sentence disappeared in the pdf file and it looks devoid of a verb. It is corrected (lines 482-483).
L630: There are studies on soil cover and below canopy VOCs from deciduous US	The work of these authors is cited in our

forests that might be relevant here (See Stoy et al. and Trowbridge et al. 2021).	review (line 360, 1209, 1239)
I'm not quite sure I see the relevance of Table 1.	We would be interested to know why Tab. 1 is in doubt? Unlike the reviewer, we have no such doubts, since this table clearly demonstrates the different participation of biological and abiological factors in the processes under discussion.
The inclusion of Tables 7 and 8 are also confusing: Why list all the compound emission presented from one paper? Might this be a copyright issue?	We are compelled to draw the attention of the reviewer to the fact that the data given in the above tables are of a unique nature. To date, this is the first published information on the composition and (especially) on the rates of VOC emission from the litter of deciduous trees. We consider it important to keep these tables in the review.
The same can be said for Figures 1 and 2 (reproduced data from the author's 2010 paper). Why are these being specifically highlighted when the review should be synthesizing and compiling data from many sources.	The problem lies precisely in the absence of "many sources". We have to make do with those that are published.
Response to first reviewer's remarks	
We have nothing to add to Dr. Praplan's response to Biogeosciences at the end of March 2022.	