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Comment

Interactive comment on “VOC emissions from dry leaf litter and their dependence on temperature” by L. Derendorp et al.

Anonymous Referee #1

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General

There are yet many problems with that manuscript. The data itself are worth to be reported but the manuscript is lacking a clear thread and conclusive target to report what the title is stating.

Mainly, there are reported temperature driven emissions of some hydrocarbons and methyl chloride from porous media, which are either the intact or ground leaves as collected from leaf litter. To describe that system, one have to take the lipid peroxidation reaction, which requires oxygen, and the diffusion through the medium into account. Both "rates" are by the way depending on the Arrhenius equation linking them to temperature. In case of methyl chloride, pectin hydrolysis might be the main source, depending on temperature, pH, grade of methylation, and also on the diffusion out of

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the medium. These things are neither introduced nor discussed. Instead, there is a not further explained precursor to VOC reaction, assumed to be pseudo first order, to motivate the use of Arrhenius plots in further discussions. The reaction leading from pectin to methyl chloride is also not explicitly named. Is it one only or are there several? Given the fact, that there have to be at least two activation energies and proportionality constants (the pre-exponential) which are different for both processes (chemical reaction and physical diffusion) the following discussion about the possible reason for the deviation of the results from the theoretical curves are in question. If desired, one can state, that the combined process of building up VOCs and their diffusion out of the medium, has an apparent, temperature influenced reaction rate that obeys Arrhenius law. But then, still it has to be proofed, that this combined reaction is of the assumed pseudo-first order as the precursor does not stay in excess. The depletion of pools after heating might be an additional reason to skip the the pseudo-first order assumption.

The discussion about a possible deposition is just a speculation at that point. Introduced, after there was not a good agreement found for the ground material emissions. Without any measure about the possible deposition from ambient air it is quite dangerous to state and further use that in the latter discussions as a fact. In my opinion, the ground material is just exposed better to oxygen and the diffusion is, as positive correlated to the surface area, higher. That causes the higher emission rates at low temperatures.

Skip the part about the global budget. (1) The discussion above tells that the emission rates obtained from your lab experiments are yet in question and (2) a 30°C annual mean temperature is way to high. Even in the Mediterranean area the annual air temperature means will hardly be above 20°C, but here the soil temperature, which is usually lower than air temperature, is the driving factor.

Technical

The manuscript has a very short and quite incomplete Material and Methods part,

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called "Experimental methods". As a fact, many parts which should be given in that section appear later on within the manuscript mixed into the Results and Discussions section.

Many procedures are not conclusively given, as an example, it is stated that leaves have been air dried at 25°C, for how long? As later it is referred to possible losses due to that handling, with the given information is no honest statement possible on such losses. Or the statement about the storage in nitrogen and artificial air is not stated in that section at all and appears out of the blue in the Results and Discussions part.

I suggest to reorganize it, extend the methodology, focus to the temperature dependencies and describe the processes properly.

Interactive comment on Biogeosciences Discuss., 7, 823, 2010.

BGD

7, C105–C107, 2010

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