

Interactive comment on “A novel source of atmospheric H₂: abiotic degradation of organic material” by H. Lee et al.

H. Lee et al.

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We appreciate the reviewer’s careful and constructive suggestions to improve the quality of this manuscript. We have carefully considered all of the suggestions in the revised version of the manuscript. Below are our line by line responses to the specific comments.

Specific comments:

Abstract: The abstract wording is a bit awkward.

- We revised the abstract to make it sound more fluent.

Abstract: The following statement in the abstract is not necessarily supported by this

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work: "Our results suggest that abiotic release of H₂ during organic matter is ubiquitous in terrestrial ecosystems". Please revise to reflect that the biomass and temperatures tested suggest the process is ubiquitous in arid ecosystems (such as cited in lines 17-19), and may also occur in other ecosystems/climatic zones.

- We appreciate this suggestion and have made changes to the statement.

Page 1 Line 8 - Delete "a" in "through a microbially-mediated".

- Deleted.

Page 1 Line 13 - Replace "in" with "at the" in "Among the known source-sink dynamics in soil-atmosphere interface"

- Replaced.

Page 2 Line 21 - Do you have an example of typical H₂:CO mole fraction ratios from your samples that you could report to provide evidence for this mechanism?

- Typical H₂:CO mole fraction ratios were 0.14-0.59 during thermal degradation and 0.02-0.07 during photodegradation. This information is now included in the text.

Page 5 Lines 20-23 - It does seem that the probability for microbial activity is low, but one might assume these plants and associated microbes are desiccation resistant, and microbial activity can most likely persist after two days of drying. It is not reasonable to expect microbial activity to be assayed given the scope and methods of this study. I would just convey that the likelihood of microbial activity is minimized, but not cannot be excluded as a possibility. It is not clear that the radiation supplied (UVB near natural solar conditions) would halt microbial activity, as Johnson, 2003 only reports that levels considerably greater than ambient are be damaging.

- The reviewer is correct that we can not rule out the possibility of some microbial activity, although the abiotic conditions suggest that it would be extremely low. We have clarified in the text that microbial activity was not specifically assayed, and thus that

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some limited microbial activity is theoretically possible even under abiotic conditions that are not conducive to microbes.

Page 6 Lines 2-3 - What is the basis for this statement? "since it is unlikely that any microbial activity could respond with such rapidity in reaching near steady state (mole fractions)" Temperature-dependent enzymatic processes can respond quite rapidly (see Figure 2 in Smith-Downey, N. V., Randerson, J. T., & Eiler, J. M. (2006). Temperature and moisture dependence of soil H₂ uptake measured in the laboratory. Geophysical Research Letters, 33(14), 1–5. doi:10.1029/2006GL026749). Also adjust Page 12 Lines 16-17 accordingly.

- We appreciate the suggestion. We revised the text based on this suggestion.

Page 9 Line 1 - The use of zero air or N₂ tanks is worrying in this study because H₂ mole fractions are about an order of magnitude lower than ambient levels (e.g., your Fig 1). Apparent H₂ production could be due (partially or entirely) to outgassing of H₂ from the substrate, which as a diffusive process, could be enhanced by temperature. Did you conduct any of these experiments in natural or synthetic air with H₂ mole fractions around 500 ppb? I feel this is an important test to validate these experimental results.

- We used zero air with little or no H₂, so the reviewer has a good point that outgassing of H₂ could be a concern. However, because a subset of experiments were performed where temperature was ramped up and then down with no change in H₂ emissions, we conclude that the observed emissions were due to thermal and/or photodegradation. If emissions were due to outgassing, they would be expected to diminish over time.

Page 9 Line 7 Instead of "on an aerial basis" use "area" or "per unit area".

- Revised to per unit area.

Page 10 Line 3 - Do you have an estimated detection limit for your calculated H₂ production rates? If so, please state in methods and report rates accordingly. Should be repeated on Page 14 line 14.

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- The detection limit for calculated H₂ production rate was ± 0.01 nmol m⁻² hr⁻¹. We added this information to the text as suggested.

Page 10 Line 15 - units - also in Table 1. Also page 12 line 21.

- Units were added as suggested.

Page 10 Line - Activation energy instead of reactive energy

- Corrected as suggested.

Page 11 Line 16 - delete "the" in "The H₂ production"

- Deleted.

Page 13 Line 4 - Please make sure you are clear about whether you are discussing thermal or photolytic degradation when comparing to Derendrop 2011c here.

- Revised as suggested.

Page 13 Line 10 - Please specify whether you are just referring to the two pathways of degradation you have proposed (thermal or photo), or whether additional pathways are suggested.

- The text was revised to reflect the reviewer's suggestions.

Page 14 Line 10 - list Derendrop species again.

- Added.

Page 14 Line 16-19 - Sentence is awkward.

- Revised.

Page 15 Line 19-20 - Under represented is not correct terminology here. Not represented?

- Revised to 'unrecognized'.

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Discussion: Please mention again that microbial activity cannot be completely excluded. H₂ production may occur anaerobically via fermenting bacteria and aerobically by fungi.

- This idea was added to the Discussion as suggested.

Table 1 – units for E_a in table and caption. Space in thermaldegradation. Table 2 - define SE Figure 1 - define ppb and use mole fraction instead of concentration. No need for MM:SS units/label in graph, the time should start from zero when experiment began. Is date relevant? Supporting Information - Define SE.

- Above were revised as suggested.

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