

# ***Interactive comment on* “Characterization of active and total fungal communities in the atmosphere over the Amazon rainforest” by A. M. Womack et al.**

## **Anonymous Referee #1**

Received and published: 21 August 2015

### General comments:

This manuscript addresses a topical idea in land-atmosphere connectivity. In the introduction, the authors establish an important distinction between the role of active versus dormant states of fungal hyphae and spores in the atmosphere in the context of ice nucleation capacity, but ice nucleation capacity is not explicitly studied or reported in this survey. Ultimately, this is a survey of active versus total fungal sequence counts, and an introduction focusing on biogeography is more appropriate - with the ice nucleation capacity restricted to the discussion. The questions addressed in the manuscript are straightforward and not hypothesis-driven, but at the same time answer fundamental unknowns regarding the biogeography of fungi over the Brazilian Amazon.

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The conclusion drawn is that spores from Basidiomycota dominate the total fungal composition of air but that in terms of the metabolically active "community" of fungi in air, hyphal fragments of fungi within the Ascomycota dominate.

One important limitation of the study is the sampling extent, which was restricted to four days at the end of the dry season. Tropical fieldwork is difficult, but how representative is this four-day period? How does seasonality influence the ratio of viable to dormant fungal tissue? How are the natural histories of the dominant taxa driving the total versus active patterns influenced by seasonality? Could one make the argument that decomposition rates are higher during the rainy season and therefore one might expect higher numbers of active Polyporales cells? The authors should address this sampling limitation - or at least address the potential role of seasonality in the introduction/discussion.

Another question that cannot be addressed with this sampling scheme is the diurnal versus nocturnal shift in fungal composition - both in terms of OTUs but also active/dormant state. What cues do fungi use to release spores and how would this influence the active/dormant ratio in the atmosphere? This provides another opportunity to flesh out the discussion, which is currently limited in scope.

Specific comments:

The cDNA library prep and primers used are appropriate. Presumably control filters were used? If so please include results.

This reviewer is not qualified to review the specifics of the metatranscriptome protocols.

The fact that the samples are dominated by Ascomycota and Basidiomycota is not informative. There are three fungal Phyla if one does not include the Imperfect Fungi (which are typically thought to be ascomycetes that that lost sexual state). Some information at taxonomic resolution that is informative is provided (e.g., Polyporales) - the authors should provide more of this context by listing the other families - beyond what

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is provided in Fig 1.

All of the supplementary tables are useful.

P9: I do not follow the logic of the sentence starting on line 7. Just because these are wood decay fungi, it does not necessarily follow that they are from a local source given that there is much evidence (some of which should be cited in this manuscript) for extremely long distance "dispersal" through the lower atmosphere.

In order to provide more support for this idea that "inputs of fungi to the atmosphere are from local, rather than distant, sources" could the authors compare sequence similarity of some of the more common OTUs in both their study and the reference tropical soil database they are using? Greater sequence similarity - and not simply community composition, would provide evidence for the supposition that resident fungal communities were in fact the source of atmospheric spores. Without this evidence, the supposition should be removed.

Fig 3 (and supplemental 3) - given the error bars associated with the active atmospheric fungal community, it does not appear that it differs from grassland or tundra soil and therefore is not more similar to tropical soil. Please address.

The mass balance approach is a useful contribution to this manuscript and while some very broad assumptions are made given that comparisons are being made between two Phyla (!), it is a useful exercise.

Technical corrections:

The figure legends are all lacking. Please include relevant statistics, error bar details, etc.

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Interactive comment on Biogeosciences Discuss., 12, 7177, 2015.

**BGD**

12, C4601–C4603, 2015

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