

Geosci. Model Dev. Discuss., referee comment RC1  
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## Comment on gmd-2021-202

Christopher Cleal (Referee)

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Referee comment on "A dynamic local-scale vegetation model for lycopsids (LYCOm v1.0)"  
by Suman Halder et al., Geosci. Model Dev. Discuss.,  
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This paper is an attempt to develop a local dynamic vegetation model to show that lycopsids may have had a significant impact on weathering rates and atmospheric CO<sub>2</sub> during Silurian times. I cannot comment on the validity of the model itself, as this is outside my area of competence; I assume that someone more familiar with such models will also be looking at this manuscript. The following comments are mainly restricted to the general context (mainly palaeobotanical) of the results.

Throughout: "Lycophytes" implies a plant division (or phylum), which is debatable and open to confusion. Better to call these plants lycopsids (i.e., class Lycopsida), which I think few would dispute.

Line 2. Lycopsids never really evolved into woody plants as such. The late Palaeozoic arborescent lycopsids had some secondary wood in their trunk, but the majority of its thickness consisted of periderm. The term woody plants is usually used for those gymnosperms and angiosperms whose trunks and stems consisted almost entirely of secondary wood.

Line 34. Why is Thomas & Watson (1976) being recorded here? This is a record of a large lycopsid trunk found in much younger strata, in the late Bashkirian. If you are talking about the earliest lycopsids, surely reference to late Silurian Baragwanathia would be more relevant.

Line 37. Neither Rubinstein et al. (2010) nor Steemans et al. (2009) are reporting lycopsids from the Ordovician. They are reporting cryptospore-bearing Eoembryophyta. The lycopsids probably had their origins in the eoembryophytes, but there is no way you could really call these eoembryophytes lycopsids. The earliest lycopsid remains (and lycophyte remains, namely the Zosterophylloids) remain late Silurian in age.

Line 37. It is here that Thomas & Watson (1976) should be mentioned as documenting these arborescent lycopsids, rather than the Gensel & Berry paper.

Lines 127-131. I might be mis-interpreting this, but why is it thought that transpiration during the Silurian was into saturated air?

Lines 134-135. I don't think these Silurian lycopsids had much in the way of underground roots.

Line 510. Bryophytes were not the immediate precursors of the lycopsids - these were the Zosterophylloids (a sister group to the lycopsids) and the various eotracheophytic / eoembryophytic "rhyniophytoid" groups. Lichens are of course fungi (albeit with algal symbionts) and it is difficult to see how they would have contributed significantly to early Palaeozoic levels of photosynthesis. Moreover, most lichens favour hardground-type substrates, whereas the model seems to be dealing with plants on soft substrates, and so it is difficult to see the relevance of comparing the effect of lichens against that of lycopsids.

The evidence does seem to suggest that lycopsids had the potential to increase local levels of photosynthesis and maybe substrate stability. But aerial cover of lycopsids during the Silurian is likely to have been very limited - probably mainly restricted to areas of wet substrates. With such a limited aerial spread, what global impact are they likely to have had in Silurian times on atmospheric composition? On the other hand, it has been suggested that the early eotracheophytes were already influencing atmospheric CO<sub>2</sub> and weathering rates by the Ordovician (e.g., Servais et al. 2019. *Palaeogeography, Palaeoclimatology, Palaeoecology*, 534: 109280) so it is maybe to be expected that the much larger lycopsids could have been having a larger effect in the late Silurian.