

Interactive comment on “Leaf phenology as one important driver of seasonal changes in isoprene emission in central Amazonia” by Eliane G. Alves et al.

Anonymous Referee #1

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The study by Alves et al. provides a context for phenological control over isoprene emissions in an Amazonian tropical forest. I think that the results are interesting, but only because this is a tropical forest. The potential for phenology and leaf age to control isoprene emission rates has been recognized in past studies going back 25 years. Studies by Fall, Monson, Harley, Litvak, Sharkey, Loreto and many others have clearly shown these relationships in temperate forest trees. The Alves et al. study is most interesting because it deals with a tropical forest, for which this type of insight is missing.

The main critique I level against the study is that it is written to largely ignore this

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past body of work, and the broader context of phenological influences over isoprene emission, and instead makes it sound like this is a new relationship discovered since 2013.

I recommend a major revision of the work that honestly takes into account the historical context of the phenology-emission relationship and its relevance to the observations made in the tropical forest. In this revision I recommend making it clear that the novel aspects of the current work are that it (1) is among the first to show the importance of the phenology-emission relationship in a tropical forest, and (2) that it allows for the MEGAN model to be modified to better predict emissions in tropical forests.

Lines 66-68. The phrase "as BVOC emissions are regarded as highly significant for ecosystem productivity (Kesselmeier et al., 2002) with isoprene being the most emitted hydrocarbon, it thereby plays an important role in carbon balance", is worded strangely. What does "highly significant for ecosystem productivity" mean? Why is that part of the phrase supported with a reference, but the next part of the phrase, "it thereby plays an important role in carbon balance", is not supported by a reference? Does the Kesselmeier reference cover both parts of the phrase? If not, it seems that a second reference is needed for the second part of the phrase. I am especially interested in what is meant by "important role", as my understanding is that isoprene emission occurs as a small absolute flux compared to overall carbon fluxes (e.g., approximately 1000 times lower).

Lines 402-406. The phrase "and as isoprene emissions are strongly dependent on leaf age and mainly emitted by mature leaves (Alves et al., 2014), seasonal changes in the forest leaf-age fractions may also influence the seasonality of isoprene emissions, suggesting higher emissions in the presence of more mature leaves and during high ecosystem photosynthetic capacity efficiency." I found this to be a bit of an egregious claim by the authors. The implication is that the dependence of isoprene emissions on leaf age and phenology was only discovered by the authors in 2014 ignores a rich literature that has shown the effects of leaf age and phenology on isoprene emissions

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going back at least two decades. There are many past studies showing, in explicit terms, the effects of leaf age and phenology on isoprene emissions. The authors seem gracious in citing the rich literature connecting photosynthate to isoprene emissions, but then take sole credit for discovering the connection between phenology and isoprene emissions. This should be corrected so that the true scope of the problem and related past research is brought honestly into this paper.

Lines 428-432. The phrase "This is consistent with previous studies that provide evidence that alternative non-photosynthetic pathways may contribute to isoprene synthesis under stress (Loreto and Delfine, 2000), which may then lead to a decoupling of isoprene emission from 432 photosynthesis at high temperatures (Foster et al., 2014)." This seems to be a rather large and speculative jump in logic. There is no reason to suspect that the seasonal offset between GPP and isoprene emission rate is due to the use of stored carbon sources. In fact, the past literature (going back into the 1990s) shows that the leaf age effect is likely due to developmental (ontogenetic) patterns of isoprene synthase activity. Thus, the phenological constraint over isoprene emission (1) has the potential to override the correlation between photosynthesis rate and isoprene emission rate, and (2) this is due to an enzymatic limitation, not a limitation of carbohydrate availability. The authors seem to be unaware of this past literature as it is not mentioned in their paper. This should be corrected.

Lines 512-513. The phrase "However, less notable factors might also influence ecosystem isoprene emission." Once again, this phrase makes it seem like very few past studies have considered factors like phenology or leaf age as an important control over isoprene emissions. Actually, these factors have been recognized as just as important as temperature and light for over 25 years. The authors need to present their results in a way that embeds them honestly within the rich past tradition of isoprene emissions research.

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