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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Isoprene and monoterpene emissions in Australia: comparison of a multi-layer canopy model with MEGAN and with atmospheric concentration observations" *by* Kathryn M. Emmerson et al.

Anonymous Referee #2

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As the authors rightly point out, biogenic emissions play a critical role in the atmosphere and indeed the Earth system as a whole. It is therefore important that the modelling community evaluate, validate and constrain estimates of these emissions. Australia is an understudied region and one in which previous studies, including a recent one by these authors, have shown biogenic emissions models to perform poorly. The authors present the findings of a study comparing emissions estimates from two models, one developed specifically for Australian vegetation canopies, the other the widely used global MEGAN model, against observations of atmospheric concentrations of isoprene

and monoterpenes made during 5 short duration field campaigns. Such a study is much needed in order to gain critical insight into the mechanisms driving biogenic emissions and subsequent oxidation in this world region.

However I find a number of shortcomings in the present work that limit its usefulness to the atmospheric chemistry community. Chief among these are:

i. The authors are attempting to validate a model developed 15 years ago specifically for biogenic emissions from Australian ecosystems and that has clearly not been updated to reflect the current state of the art.

The ABCGEM model contains only 2 plant functional types: native trees and grasses. It is hard to believe that the rich biodiversity of Australian ecosystems can be credibly captured by such simplicity. Further, this also limits the usefulness of the model to the wider emissions modelling community.

Monoterpene emissions in the ABCGEM model are assumed purely temperature dependent while those in MEGAN are assumed partially light-dependent. The authors spend quite some time in a theoretical discussion of how this results in different activity functions in the two models and later conclude that the better performance of the ABCGEM model suggests that monoterpene emissions from native trees in Australia are less light dependent than other world regions. Given the highly complex, highly non-linear relationship between primary emissions and instantaneous atmospheric concentrations many kms away and given the ease with which this could have been tested I do not understand why the authors have not performed an additional simulation with the light-dependency switched off in MEGAN. This is incredibly straightforward and I would like to see this done before the paper is accepted.

ii. The main conclusion of the paper appears to be that biogenic emission estimates are critically dependent on the landcover maps used to model them. I feel that this is far from novel (see e.g. Guenther et al., 2006, Arneth et al., 2011 and Huang et al., 2015), and does little to help us improve emissions estimates in global models. This to my

mind is the main weakness of this study. The authors do not present any suggestions as to how we can overcome current deficiencies in a model that can be used to model estimates in any or all world regions. The authors would be well advised to consider the work presented here as a starting point. What can we learn from the apparent skill of the ABCGEM model that we can apply elsewhere?

iii. I was pleased to see the authors have explicitly included some consideration of the uncertainties associated with the emissions estimates. However, although the authors have carefully followed the error propagation methodology this considers only errors associated with measurements of a subset of the driving variables. It does not include other systemic and potentially substantial errors associated with the model parameterisations themselves (either in form or in the values of the empirical constants). As such it is rather misleading.

iv. As it stands, this is neither a rigorous evaluation of the performance of the canopy model and estimated emission rates nor is it an in-depth analysis of the atmospheric chemistry in the region. In fact, I find it hard to understand what the authors intend. It seems mostly to be a lengthy appraisal of model treatment of leaf area index (LAI) and the deficiencies of the various available landcover maps. As such, it does not to my mind fit within the remit of ACP but would be far better placed in sister journal GMD, although with the above caveats regarding the need for better evaluation of the canopy model itself.

In its present form, I do not consider this work to be suitable for publication in ACP. I would suggest that as a bare minimum, the authors need to address my concerns above and to reverse their current approach and concentrate on spatial and temporal distributions of modelled isoprene and monoterpene emissions, and modelled and measured atmospheric concentrations which are of far greater interest to the wider atmospheric science community (the primary audience of ACP) and which offer the possibility of real advances in the field. Further, I would also like to see how modelled concentrations of primary oxidants and oxidation products compare with those mea-

sured at the various sites. Given the highly complex and highly non-linear chemistry of bVOCs comparison of concentrations cannot necessarily be used to deduce skill in modelling emissions. Further validation would be useful.

At present this paper is being used as a vehicle for a description of the ABCGEM model which seems to have little applicability outside of this region. If that is the intention of the authors I would recommend seeking publication in GMD with substantially more consideration given to how the comparison (with MEGAN output and observations) can be applied to improve model performance. AND to take steps to do just that with further sensitivity tests, e.g. MEGAN without light-dependent monoterpene emissions.

Specific comments: Overall I feel that much of the discussion and presentation of data comparing the functional form of the activity factors in ABCGEM and MEGAN would be better placed in the SI. Likewise the exhaustive coverage of LAI, which seems to be over-accounted for in ABCGEM.

I would like to see far greater detail of the measurement sites, their footprint and dominant air mass origin during the period of the campaign(s), vegetation / ecosystem type, etc included in the main paper.

The authors use the term “emission factor” to mean several different things at various times during the analysis, in particular in Section 4 where they continue to use the same phrase to describe both a basal emission factor (i.e. emission rate at standard conditions) and a landscape emission factor (i.e. some kind of average gridcell BEF which accounts for contributions for all vegetation types within a model gridcell). I think it is this phrasing that makes this section so hard to follow when the authors introduce the effect of differences in LAI between MEGAN and ABCGEM.

Throughout: “inline” - do the authors mean “online”?

Abstract:

L23: surely “simpler” rather than “simplified” as there is no suggestion that the authors

have reduced ABCGEM in any way for this work.

Introduction:

Throughout: there are far too many unsubstantiated statements made without reference to supporting literature, in particular:

L15: Please supply a reference for the C-CTM when first introduced.

L21-22: Do the authors have evidence that these forests have a substantial impact?

L31-32: How is this relevant in a region where isoprene:monoterpene is unity?

Methods: 2.1 As stated previously I would like to see far greater detail of the campaigns, the sites and meteorological conditions, the measurements available, etc. I consider this essential for the Brinsgelly and Randwick data which have not been previously published.

2.2 As this model has not previously been described further detail is required in the main text. It would be far easier to follow if the authors presented the parameterisations here rather than attempt to describe in words.

2.2.1 Why set B to a value of 400 g m-2? This does not seem consistent with the description in Section 1 of the SI.

Also why devote so much of this section to a description of the grass PFT when it is promptly left out of the model?

2.2.2 This section is a discussion not a method. Further, as the authors devote so much time discussing the light-dependence of monoterpene emissions it would be useful to learn whether previous experiments on native Australian vegetation have shown evidence either way.

Section 3: Why is the C-CTM not described as part of the Methods section like ABCGEM is?

L30: I suggest showing the model domains on Fig. 1.

3.1 I find this description of the seeming over dependence of ABCGEM on LAI extremely difficult to follow. However, if it is driven with 1980s LAI as a default it is good to see that the authors have conducted the AML sensitivity test.

L25 Typo: “ccalculated”

Section 4 Results and discussion

4.1: As stated above I found this particularly hard to follow in part because of the use of “emission factor” to describe several things.

L14-23: I don’t think that the “emission factors” being compared here are directly comparable. . .

4.2 L8: See point below regarding Figure 4.

L10-11: There would be no reason for anyone to expect the emissions to scale linearly with LAI . . . This comes back again to the confusing and inconsistent terminology used with regard to emission factors and emission rates.

L20-21: The issue of whether or not monoterpene emissions should be treated as light-dependent or not is really quite an important one as it is a fundamental mechanism rather than a model “artefact” such as LAI. Yet the authors seem to have made no attempt to investigate this further. To my mind this is where the real novelty could lie, and where the authors could offer something to the modelling community as a whole. Is it the case that MEGAN is incorrect to assume that all monoterpene emissions have some light-dependence? Should these factors be PFT-specific? etc.

L25-26: Again, surely this is to be expected . . .

Section 4.3 appears to be missing

4.4 L3-10: It would be helpful for the authors to include % differences to put their

absolute changes into context.

4.5 Overall I find the analyses of the output of the two model with respect to observations well done. However, for this paper to be suitable for ACP I feel that it is this section that should form the focus of the paper rather than the preceding consideration of LAI.

p9, L3-6: Agreed. If updating a model developed 15 years ago to use a more up-to-date input dataset causes the model to "fail" the model clearly needs further development which is one of my key concerns with this work as it is presented. Why use this model rather than using the wealth of observations to improve the skill of MEGAN for this region? The two models take essentially the same approach to estimating emissions (i.e. empirical rather than mechanistic) so it is not evident what we gain from going back to the older model.

L14-15: I feel the wind roses should be in the main paper as this consideration of emissions vs meteorology / chemistry is important.

L20-25: MEGAN appears to have a good fit to the profile of monoterpene concentrations at Randwick and (to a lesser extent) SPS1 whereas at Bringelly all 3 simulations vastly over predict night-time concentrations. It seems far from clear that the light-dependence of monoterpene emissions is the only issue here. I suggest the authors need to investigate more fully.

p10, L1-5: I'm not sure that the q-q plots add much to the discussion and would suggest they be moved to the Supplementary.

L19-24: See previous comments regarding the estimated uncertainty.

5 Conclusions L25: I wouldn't consider the two models to have independent approaches to estimating emissions; both are based on the Guenther empirical algorithms developed in the 1990s.

p11, L21-23: I would argue that in spite of using the same chemistry scheme some

of the difference in concentrations between the two models will still be due to chemical processing as it is highly non-linear and strongly dependent on VOC-NOx ratios which will differ if VOC emissions change. I agree that the predominant cause of the differences are differences in emissions.

L31: As noted previously I feel the authors need to give far more detail of these two sets of measurements as they have not been previously published.

p12, L7-8: Agreed, but that doesn't preclude the authors from investigating further at this stage.

Table 2. What about PAR and Temperature for each campaign? (average and some measure of range)

Figures 2 -4 would be better presented in the SI (but see below)

Figures 3 & 4 I feel the way the data is presented is fundamentally flawed. LAI is not a discrete variable but rather a weighted average for a grid cell based on proportional land cover. It therefore makes no sense to plot the data showing ranges for emissions but not LAI. While I understand that the authors have binned the data by a range of LAI it is still not appropriate to plot the emission rate against the mid-point of the LAI bin. At the very least, it should be a weighted average of the LAIs of the grid cells within that bin but even then I would question its appropriateness.

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