

Interactive comment on “A process-based model to estimate gas exchange and monoterpene emission rates in the mediterranean maquis – comparisons between modelled and measured fluxes at different scales” by M. Vitale et al.

Anonymous Referee #1

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Review of A process-based model to estimate gas exchange and monoterpene emission rates in the mediterranean maquis – comparisons between modelled and measured fluxes at different scales by Vitale, Matteucci, Fares and Davison Discussion paper in Biogeosciences Discussions

In the first reading of the MS I made it clear that before the publication of the discussion paper, the figures and language of the MS has to brought up to standards required for publication, while other issues can be corrected later. Now, for my big disappointment, essentially nothing has been changed. The figures are still of inferior quality with no

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clear axis labels. Also, the language is still not good. This is surprising given that a native speaker is on board. In addition to the other problems outlined below, I am finding myself in a difficult position to recommend against publication of a MS that I have previously encouraged for publication. However, the authors have failed a test, and not taken seriously my previous suggestions. Therefore, I have no trust in that the revision will be much improved.

Specific

P1748

L6. hypothetical, but realistic. In my reading, I do not see any inclusion of canopy architecture into the model at all. Whole canopy LAI is driving the flux in the big leaf model, no?

L16 vs. L17. The abstract contains statements that are overtly specific, and statements that are too general (first versus second). If you want to make a statement about the importance of structure, clearly "some" is too general. On the other hand, orientation of the site is not what the readers likely want to learn from the abstract.

L20. I am not sure that being 30-40% off is really appreciable. I could well take the point of using and developing a different model if we were able to get a better fit (as we normally get with empirical models like this). Yet, here the fit is inferior and overall, it remains unclear why the authors want to develop a new model. Why not to model the emissions with accepted standard models and if really needed, compare with the current model. Guenther et al. for instance or something like that. So, we would have a baseline. Here we have an inferior fit and overall, it is not clear why this exercise has been done.

1749 Introduction is too long. The review of big leaf models is lengthy and contains errors. Why not to more efficiently move towards the main aims of the paper?

L1 This is a perfect example of the lack of careful language editing. I guess that the

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sentence should read "research has been"; ... "community response to climatic change at";

L10 for increased accuracy

L22. This is not entirely true. These models can estimate the averages quite good, but prediction of such a stochastic variable as light is principally not possible (in high resolution), yet even be a daily time-course for a specific day.

1750

L4 scaling up

L17. vs. L19 Dai et al actually present an excellent 2-big-leaf model

Overall, the point is that due to non-linearity, single big-leaf model will always be incorrect as the arithmetic mean of two photosynthesis values estimated at two different PPFD-s will always be less than the photosynthesis value estimated at the average PPFD of these two light values. The thing is that big-leaf models use various fudge factors to cope with this problem, and these fudge factors have no theoretical founding. So, I see this also a problem in this paper that apparently a single big-leaf model is used that we know already is wrong, and then there is some attempt to include canopy traits, but it remains completely unclear how.

1751

L1 several studies?

L7-11. I do not think that such hybrids can be encouraged.

L16 address without "to";

L20-25. What was actually done? What are the profiles needed for?

L27. I guess you mean heterogeneous?

L29. "some";. This is hugely vague statement

S335

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1752

L1-2. Honestly, I do not see why you say “process-based”; All functions used are poorly empirical. It must be said “empirical model”;

L8 avoid use of common species names

L15 “functional functions”; is really awkward

1753

L5. its is not whole leaf but whole plant

Eq. 2. Why does respiration show up twice? This is a purely empirical function. Is there any experimental evidence that it fits data well?

It is also highly confusing to see symbols presented with and without subscript, in italics and in Roman, units presented as micromol and micromole etc. Such careless presentation is highly annoying and should have been corrected before publication of the Discussion paper as I have requested.

L14. Respiration rate is by no means a constant. This is too bold assumption that cannot be tolerated in a modelling study.

L16 dependent on

Eq. 3. We can do better today. At least it must be discussed that canopy is highly clumped in Mediterranean canopies.

Eq. 4. I do not understand this equation. Seems like a key component of the model. Yet, it is not internally consistent with Eq. 3 where k and LAI are independent.

1754 L1. Well coverage has nothing to do with k . I guess that the authors may want to rethink basic radiative transfer concepts at that point.

L9, well, then a second equation with “if” is needed

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L11 calculation of emission fluxes?

L12. Calculation of the monoterpene flux was estimated; Well, this is the perfect example of the language in this paper. In fact, I was tempted to stop my review at this point.

1755

L7. Do you mean several Mediterranean maquis plant species?

Eq 9. This equation contains five factors not defined before, and defined quite a bit later in the following pages. Presentation of an equation should be better rethought. All these empirical factors are highly speculative, and even with all these, it is hard to understand why the model fails to get a numerically coherent estimate.

Eq. 10. Another fudge factor. Why do we need this equation? This is based on a French site, how can such a factor be justified?

L19. What evidence is there that the emission factor is the same for these species?

L21. density is mass per volume. I guess that you have leaf dry mass per unit ground area, or what?

1756 L1. I am not sure that these two factors have solid theoretical founding. Better introduce as separate equations, or like with the escape efficiency; this is something just to get measured fluxes and simulated fluxes correct, but for air reactivity we need emissions without such corrections.

L11. What evidence is there that the emissions in Phillyrea and Arbutus depend on light?

L24. Why is p value in time units

A table with all parameters would be very useful

1757 Results: The number of significant digits provided for the numerical values is

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wrong in absolutely all cases. In the case of error estimate, no more than 2 significant digits are given (2 only then when the first number is 4 or less), and the number of significant digits in the measured value must match that in the error. It is no point to present that many digits, when error is big.

L20. what unit is soil?

L22. noteworthy? or worth of noting not “note to worthy”

1758

L20. Why not to show the actual correlation

L21. the correlation is bad

1761

L16. Again, why do you say “process-based”?”?

1762 L20-22. I have been there, and I simply do not believe that the proportion of old foliage is that small.

Discussion should be shortened, and it should be discussed in very simple manner why we do need an additional model and what are the advantages simulating the emissions this way.

Overall, the paper contains some good ideas, but it clearly fails to present the message in a way it should. It has been a frustrating reading for me, and as it is, I am sure that it will be dissatisfactory for the community. I am also very surprised that there is no editing effort by the journal staff involved during publication of the discussion paper.

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