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Biogeosciences Discussions

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 #2

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General comments:

This paper addresses the question of BVOC emission under field conditions in the Mediterranean basin. Since biogenic emissions play an important role in air chemistry and air pollution in this region, and considering expected climate and vegetation changes, modelling BVOC and especially monoterpene release from the biosphere is currently a rather 'hot' topic. It has recently been addressed by modellers that used evaluation data from Spainish and French sites and simulation results from the Castelporziano site in Italy could represent an interesting complementation, very suitable for

this Journal.

The focus of the manuscript is clearly on model description. A previously published carbon assimilation model has been complemented with the Guenther algorithm to estimate monoterpene emission. The connection between assimilation and emission being that assimilation drives leaf biomass development (on a daily basis) and monoterpene emission is scaled with the amount of foliage. Some evaluations are given for gross primary production (GPP) and monoterpene emission of the shrubland site based on eddy-covariance flux measurements.

I have to admit, I have serious problems with the modelling approach presented. Number one is that the only difference between a standard simulation of GPP and a standard simulation of monoterpene emission (with Guenther approach from 1995) is that the leaf area index (or foliage biomass) is considered dynamic. And that there is no evaluation about leaf area development or even absolute size! In Vitale et al. 2003 LAI is assumed (modelled) to vary between 2.5 and 3.5 with a maximum in August. Gratani et al. 1997 indicated for Castelporziano forests a LAI of 4.5. Cutini 2002 indicates 4.0 but I don't know if this is really a comparable site. Information about the seasonal course is even scarcer but from general biological knowledge I doubt very much that the maximum is actually in Mid-Summer. Grote 2007 assumes from indirect literature indication that it is in spring after bud-break and has also shown that the assumptions about LAI and foliage development matter for emission estimate. I have also serious doubts that an LAI simulation approach that assumes leaf growth proportional to assimilation of the previous day without consideration of inherent phenology or drought stress impact can be suitable for representing Mediterranean vegetation dynamics. Overall, it remains to be demonstrated if the model assumptions are a) realistic and b) suitable to improve the simulations compared with simpler assumptions (e.g. LAI is constant).

Secondly, although the simulation assumes that LAI dynamics are important to consider, vertical LAI distribution has been neglected. The authors have spent some effort

BGD

6, S269–S273, 2009

into discussion the importance of stratified modelling in the literature and ended up that this should not be applied here because 'the morpho-structural properties of the vegetation are well characterized' (which I would like to see) and 'a multi-layer approach is extremely difficult to apply' (which should not be the case if condition one is true). In any case, only that the application (means: initialization) is difficult does not mean that a simpler approach is automatically justified. It should be noticed that the 'big-leaf-approach' is by far not as common as stated (except on the global scale perhaps). Numerous papers use canopy stratifications to estimate layer-specific micrometeorological conditions from that emission is calculated (some are also mentioned in the introduction). It is also known that Holm oaks concentrate their foliage very much on the top of the canopy (e.g .Sala et al. 1994) and that lower and upper leaves show different emission potentials (e.g. e.g. Staudt et al. 2003). Both facts imply that stratification is indeed necessary - except if the LAI at this site is exceptionally small (e.g. $<$1-2), which is not indicated. Thus, I assume that even when detailed information is not available, more realistic assumptions would improve the simulation study.

Finally, I am of the opinion that a model that does not consider drought stress or at least inherent seasonal dynamics (other than a 20% reduction in winter as considered by factor CD) on emission potential is not applicable at drought-exposed Mediterranean sites such as Castelporziano. Numerous reports are available that indicate such an seasonality (e.g. Monson et al. 1994, Sharkey et al. 1999, Geron et al. 2000, Petron et al. 2001, Staudt et al. 2003). Evidence for drought stress impact on monoterpene emission might not yet be conclusive (but see Grote et al. 2009) but neglecting it even for photosynthesis seems very strange. It is thus not much surprising that the evaluation is quite bad even with respect to gross primary production. Given the large uncertainties in any direction it might be a little bit of a surprise that the annually simulated emission amount is exactly the one estimated from measurements. However, with no evaluation presented for daily or seasonal emission I am inclined to assume this to be an artefact.

Besides, English language should be improved. Tempi, comma settings, and gram-

BGD

6, S269–S273, 2009

mar are strongly heterogenic and often it is difficult to follow the authors thoughts. A separation of text into more paragraphs could help too.

Some specific comments

Introduction:

- The introduction only deals with canopy scaling (without conclusive outcome as mentioned above) but fails to give an overview about the importance and state of the art of BVOC (monoterpene) modelling. Shouldn't that be considered when presenting a 'new model to estimate monoterpene emission'? What is different to other modelling approaches, particularly those carried out in comparable environments? Why is the MOCA model used as basis?

Methods:

- The whole of section 2.2.2 is basically the same as in Simon et al. 2006. In large paragraphs it has obviously been a 1:1 copy and paste exercise! However, the authors seem to have missed that that equation 6 is applied to emission from storages only, while the temperature dependence factor CT from equation 7 is for emissions that are produced as a direct response to temperature and light. If the 'pool' factor is later on set to 0, half of equation 9 and equation 6 gets irrelevant.

- The assumed emission potential of 24.9 is the value derived by Simon et al. 2006 as average from numerous publication that vary from 6-58. From the (falsely indicate) three references only Owen et al. 1997 was included in this list.

Results

- There might be some terrible misunderstanding but in Figure 5 GPP measured seems to be at app. 5 gC m-2 d-1 whereas modelled values are around 13. I would have indicated this as app. 200-300% higher while the authors arrive at only 30%. What have I missed here??

BGD

6, S269–S273, 2009

Discussion

- There are really good reasons to discuss the limitations of the MOCA model here. There are some more as mentioned in the general statements. In my opinion these short-comings actually do not justify the application of the model at all.

References - Please note that you can not cite a manuscript accepted in Biogeoscience-Discussions as accepted in Biogeoscience (Davison et al. 2009). This is a major difference.

Interactive comment on Biogeosciences Discuss., 6, 1747, 2009.