

Interactive
Comment

Interactive comment on “The emission factor of volatile isoprenoids: caveats, model algorithms, response shapes and scaling” by Ü. Niinemets et al.

R. Grote (Referee)

ruediger.grote@imk.fzk.de

Received and published: 24 February 2010

General The authors have taken the challenge to evaluate the widely used concept of emission factor, which I think is a valuable asset and comes timely to provide a basis for an uprising discussion about the future direction of model improvements. The authors demonstrate a deep knowledge of the subject and a (more or less) good overview about the respective literature. The drawbacks are that the manuscript is sometimes quite lengthy, while concrete estimations of sensitivity to particular impacts are often not given (but I like Figure 7). In addition, particularly the introduction and conclusion part needs restructuring. Overall, I think that it should not be too hard mending these

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



problems.

More Specific

The introduction should be shortened. Many single references can be replaced by recent reviews. Other examples for possible shortenings are the scaling question and the particular discussion of CO₂ effects. I do not doubt that scaling is an important question/task but it may be better addressed in another review, which does have an emphasis on this problem. Also it is not needed to discuss details about emission factor impacts because this will only be repeated later on.

It is very worth noticing that “significant variation in the shape of the light response curve” of isoprene emission exists (Fig1). Unfortunately, this variation is demonstrated only by the results from Harley (1996, 1997). For a review, this is a bit unsatisfying. Please also note more recent results from Lerda and Throop 2000, Funk et al. 2006, and Fuentes et al. 2007. A possibility of process-based estimation of Es in dependence on (stratified) light and temperature is given in Grote et al. 2010. You can also find brief discussions about the need for dynamic emission factor descriptions for different canopy layers there.

The review discusses the possible uncertainty related to the parameters of emission estimation, i.e. T_m and C_{l1}, but does not provide a simulation estimate about the impact that an assumed or measured uncertainty might actually have. Such an estimate would not only illustrate the effect but strengthen the argumentation that it really needs improvement. Similar, it is noted that the Q₁₀ temperature parameter for emission from storages is subject to variation. However, despite extensive literature given, neither the actual range observed is given, nor the different impact of alternative formulation of pure storage vs. combined storage/production simulation is explored. Furthermore, a discussion about the impact of storage filling on seasonal shifts between these two emissions procedures is missing (see also Schurgers et al. 2009).

The chapter about CO₂ dependencies (2.1.3) and specific storage emission (2.2.1)

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



should be overworked. On the one hand they are quite extensive, which is in contrast to the impression that the overall effect seems to be quite small. It is also a little bit puzzling that the only alternatives for the common approaches mentioned is the one suggested earlier by the authors, neglecting the possibilities that arise through direct simulation of product concentrations (for instance Grote et al. 2009 describes a model based on 'dynamic pools' as suggested on page 1256/L7). The end of chapter 2.5.1 brings us back to the discussion about the impacts of canopy stratification. This is certainly important but double discussions should be avoided. Furthermore, it should be considered that a whatsoever 'optimum' criterion for emission is in any case critical to be applied because the 'purpose' of many kinds of VOC emission is still unclear and under discussion.

The authors have distributed some conclusions across the text. However, they hardly summarize any of these in the 'conclusion' section. A part of this chapter is only a summary while the end seems to be more appropriate for the introduction. Try again (no offence meant).

Quite Specific (mostly technical)

P1258, L20ff: Similar as mentioned earlier with respect to the review of other parameters, I suggest that the review of induced emissions is summarized in a table. Note that induced emissions might need to be divided into those arising from storage damages and induced production.

P1266, L8: As far as I know, MEGAN runs with hourly specific climate data, which I do not think is correctly adapted by the authors.

I have noticed some problems with the references. First of all, an 'accompanying paper' (Niinemets et al. 2010) is indicated several times which should probably read 'Biogeoscience Discussion' (not Biogeosciences) but I could not find it in the internet. Further problems I noticed are:

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Blande et al. 2005 should be better replaced by the peer reviewed paper given later by the same authors (Blande et al. 2007). Similarly Pare and Tumlinson 1998 is probably better replaced by Pare and Tumlinson 1999.

Spellcheck Eder et al. 1993

Keenan et al. 2009 is out of the Discussion and can be cited now as peer-reviewed paper (see below)

Keenan et al. 2010 is (unfortunately) not in press (!) but has been rejected by FE. It can thus not be cited.

The given Tenhunen et al. reference is not the relevant one for Smith function (see better on below)

There is extensive but not a consistence use of doi and http references. Generally, I would skip these if print references are available.

Niinemets 2010 and Penuelas and Staudt 2010 that are currently available online in 'Trends Plant Science' should both be given with doi numbers instead of 'in press' or nothing. Please note that the same issue holds at least two other reviews (Arneth and Niinemets, Holopainen and Gershenson) that are probably useful to cite here.

Additional literature mentioned:

Arneth A, Niinemets Ü (2010) Induced BVOCs: how to bug our models? Trends Plant Sci. doi:10.1016/j.tplants.2009.12.004

Holopainen JK, Gershenson J (2010) Multiple stress factors and the emission of plant VOCs. Trends Plant Sci. doi:10.1016/j.tplants.2010.01.006

Blande JD, Tiiva P, Oksanen E, Holopainen JK (2007) Emission of herbivore-induced volatile terpenoids from two hybrid aspen (*Populus tremula* x *tremuloides*) clones under ambient and elevated ozone concentrations in the field. Global Change Biol. 13:2538-2550

BGD

7, C108–C112, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Fuentes J, Wang D, Bowling D, Potosnak M, Monson R, Goliff W, Stockwell W (2007) Biogenic hydrocarbon chemistry within and above a mixed deciduous forest. *J. Atmos. Chem.* 56:165-185

Funk JL, Giardina CP, Knohl A, Lerdau MT (2006) Influence of nutrient availability, stand age, and canopy structure on isoprene flux in a *Eucalyptus saligna* experimental forest. *J. Geophys. Res.* 111:doi:10.1029/2005JG000085

Grote R, Lavoie AV, Rambal S, Staudt M, Zimmer I, Schnitzler J-P (2009) Modelling the drought impact on monoterpene fluxes from an evergreen Mediterranean forest canopy. *Oecologia* 160:213-223

Grote R, Keenan T, Lavoie A-V, Staudt M (2010) Process-based modelling of seasonality and drought stress in isoprenoid emission models. *Biogeosciences* 7:257-274

Keenan T, Niinemets Ü, Sabaté S, Gracia C, Peñuelas J (2009) Process based inventory of isoprenoid emissions from European forests: model comparisons, current knowledge and uncertainties. *Atmos. Chem. Phys.* 9:4053-4076

Pare PW, Tumlinson JH (1999) Plant volatiles as a defense against insect herbivores. *Plant Physiol.* 121:325-332

Lerdau M, Throop HL (2000) Sources of Variability in Isoprene Emission and Photosynthesis in Two Species of Tropical Wet Forest Trees. *Biotropica* 32:670-676

Tenhunen JD, Yocum CS, Gates DM (1976) Development of a photosynthesis model with an emphasis on ecological applications. I. Theory. *Oecologia* 26:89-100

Interactive comment on *Biogeosciences Discuss.*, 7, 1233, 2010.

BGD

7, C108–C112, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

