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Biogeosciences Discussions

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

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I would like to respond some of the statements made in Alex's comments:

1. Alex makes the case that the air quality community does not use emission factor estimates as "constants", but rather as averages of a population of measurements. If true, then I think we would see projections from the air quality community made with bracketed uncertainties driven by the range of EF values used in the averages, including ranges driven by seasonality, drought, weather effects, etc. Most often, I see air quality (or atmospheric chemistry) projections modeled with average EFs treated as constants. The point we were trying to make in the paper is that there is a tendency to

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accept EF values for modeling purposes without proper consideration of the full range of potential variation. I think this issue could largely be resolved with more emphasis on the uncertainties in air quality projections framed by the variance in observed emission factors. The variance in emission factors is to a large extent dependent on species and environment – but often only variance among species is used in framing the averages used as constants in air quality modeling. The purpose of our paper was to issue a call for greater respect for variance in the values. It appears to have induced a stronger defense of existing modeling practices than we had hoped.

2. Alex, and to some extent, Thomas, argue that with improvements in our ability to make canopy-scale flux measurements, we will no longer need to rely so heavily on bottom-up modeling from fundamental EF algorithms. I wish to provide a different perspective to this conclusion. The responses of canopy emissions to environment can indeed be treated in empirical models using a correlational approach. However, the tension between empiricists and modelers (as described very elegantly in Peter Harley's comments) often boils down to a tension between 'correlational modeling' and 'mechanistic modeling'. If our aim is to understand responses of emissions to environmental variability not covered by a set of observations, then we need to understand the fundamental mechanisms driving the responses. Alex obviously recognizes this, as evidenced in his statement that the CO2 response of poplar species may not be applicable to all species. We can address this particular issue by understanding the mechanisms driving the CO2 response at the most fundamental level, and trying to design our models around those fundamental mechanisms. This is the point of the paper we wrote here.

An analogy is clear in the case of our understanding of C3 photosynthesis. The models we use for C3 photosynthesis were derived from observations on a few species of plants. However, we now recognize that those mechanisms are so fundamental that they exist in virtually all C3 species; thus, we can use a single model (with some species-specific parameterization) to predict CO2 assimilation rate across the domain

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of most C3 species. My point is that we could only take that step by focusing on the model mechanisms and their sensitivities to the environment. Had we decided at an early stage to 'step across' those mechanisms and focus only on canopy-scale measurements of CO2 exchange, we would not be able to make the predictions we currently make concerning the response of GPP to future global change. In fact, the canopy-scale approach favored in most big-leaf models has been shown to contain significant errors due to linear aggregation across non-linear functions. The mechanistic approach matters greatly, and that was the point of our paper. In fact, the sympathies expressed by Alex and Thomas (at least those detected by me) that the modeling of VOC emissions has outgrown a need for extensive insight into the causes of variance in the emission factor, is exactly the 'dangerous state' that we focused on in our (the biochemistry and physiology group) discussions at the Montpellier meetings, and which inspired our collaboration on this paper. The paper is intended to be a cautionary note, if you will allow it, to modelers to recognize the variance in EF (both caused by species and environment) and its underlying causes. I continue to think this is a valuable bit of caution to express as the community continues to model and assess observations at different scales.

It looks like we will have some lively discussions in Switzerland in May.

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