Biogeosciences

Discussions

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Interactive comment on "Simulating precipitation decline under a Mediterranean deciduous Oak forest: effects on isoprene seasonal emissions and predictions under climatic scenarios" *by* Anne-Cyrielle Genard-Zielinski et al.

Anonymous Referee #2

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General comment: The subject of this paper is important to address. Water stress on plant BVOC emissions is generally lacking or under-represented in models. Field observations in the regions where plants suffer from drought and have high BVOC emission are important for understanding processes beneath. The paper has strengths, but also needs major improvements before considering publication. I am a modeller, so my main focus will be on the modelling part in this paper. The collected data have clearly shown the impacts of AD treatment on isoprene emission through the season. However, the application of MEGAN in the paper is less satisfying. For the key parameters to model isoprene emission in this region, instead of using site-observed data,

the authors picked values from literature. And then there is long discussion about how unsuitability of MEGAN for this site. I would strongly suggest the authors to use site-specific data before evaluating the MEGAN model. And then, the future runs with the trained G14 model are interesting, but the discussion has put too much focus on the number coming from the runs, instead of really discuss potential uncertainties from the applied G14 model as well as relating these numbers to other predictions, like process-based model to address limitations of using ANN method. At last, this paper has no conclusion. The key messages to readers are not clear at this moment.

Abstract: It clearly described what results this paper has gotten, however, the conclusion is currently missing. There are quite many abbreviations in abstract as well as in the main paper, which makes the reading less smooth. Furthermore, the readers could also like to have a few lines at the beginning of the abstract to know why we need this type of study.

Method: First, There is no description about MEGAN model at all in the method section. Second, the purpose of running ORCHIDEE model is to get the predicted SW and ST for using in G14, right? How this modelled data have been used is not described. Then, for the AD treatment, is the deployable roof blocking all precipitation during certain period? If not, the authors need to clarify how this AD treatment has been done.

Results: When applying MEGAN model for this study site, why not adjust the emission capacity and wilting point based on data form this study site? There is large section in the discussion about the unsuitability of wilting point used in MEGAN for this study site. But the model is not designed for this specific site, I think it is logical to use the site-specific data before discuss potential uncertainties in MEGAN.

Discussion: The authors have put a lot of text on discussing the simulated ER in future scenarios, which I don't think it is necessary. The trained G14 model, with only influences from environmental factors, has no considerations of C source for isoprene synthesis, vegetation dynamics as well as adaption. The training period for G14 is too

short to be able to see the full picture of potential future dynamics of ER in this region. I would rather see the runs on the future scenarios as sensitivity testing, not prediction. There is no conclusion in the main paper.

Detailed comments:

Change "gaz" to "gas".

P2, L7, spell out species full name when it is first time mention.

P3, L5, need references for empirical-based model.

P3, L31, take away comma before and after "both".

P3, L32, use abbreviation for species name after the 1st time.

P4, L3-8, the aims seem to list what this manuscript is going to do, not really to any level of scientific understandings. I would suggest the authors to promote the real scientific questions this manuscript is aiming to answer.

P4, L26, "during one week, once a month" suggest to change to "one week per month"

P4, l27, "except from" suggest to change to "except for the period from"

P5, L22, have the biases between the Forcalquier station precipitation data and the site data been adjusted before using?

P6, L18, what is "AF"? Too many abbreviations in this paper! And the abbreviations are not consistent sometimes, e.g., both SW and Sw have appeared in the paper.

P6, L23, wrong order of Cout and Cin.

P8, L1, how does Krusal-Wallis test use for detecting seasonal variation?? Please clarify it.

P9, L31, what do you mean "the amplification of the ND" at the beginning of this sentence. In the same sentence, how can we see the ND changes reach maximum in July

and continued till Nov? Please clarify it.

P10, L2-5, why there is an increase of Pcum in the AD plots after Sep, 2012? so the rain is not totally blocked?

P10, l11, On the P5, L17-19, it mentioned that the same PAR was used for AD and ND plots. Why there is difference existed? Please clarify it.

P10, L22-23, suggest to delete it, it is for discussion and has been mentioned as well.

P11, L2, what "P = 3.9" mean? statistic p-value?

P11, L5-9, "the general high variability observed . . ." it is hard to see high variability for the AD-ND site in April, what do you mean or refer to here?

P11, L13-14, why not adjust the emission capacity values and wilting points based on the site-specific data?

P11, L18, not accurate to say "correctly assessed".

P12, L6-7, "were more sensitive to lower frequencies. . ." is not correct. From the figure, for this period April and June, ER is still more sensitive to the 0-7 days changes, but just with relatively more contributions from 14-21 days, compared to the situation in July. Right?

P12, L9, I could suggest the authors to convert the contribution from each factor into ratio (%), instead of absolute values in ER. After conversion to ratio, the authors could compare the changes of explanation ability of each factor through the season.

P12, L30, wrong order of the numbers for corresponding RCPs

P12, L31, should be dERG14

P13, L17, "during all summer" change to "during the summer"

P14, L5, both "μgC gDM-1 h-1" and "μg gDM-1 h-1" have appeared in the paper. Please correct them and be consistent.

P14, L28-31, "other additional parameters. . ." : I would suggest the authors to change the conclusion made here, before adjusting the MEGAN model to the local conditions.

P15, L21-24, the sentence starting with "Note that MEGAN performed better on our experimental data . . .", since the authors already know that the wilting point used in the MEGAN is not suitable for this site, the impacts from SW is far-off at this moment. As mentioned above, it is better to adjust the parameters used in MEGAN first and then discuss potential uncertainties existing in the model about stress enhancement on isoprene emissions.

P16, L2-3, change " Ìr̀Ìř" to the standard used in BG. And the degree day is not the first time mentioning, put the abbreviation at the place where it shows up the first time. P16, L12, ". . . representation of soil moisture in . . . is currently poor". I don't think it is relevant here to mention the quality of soil moisture estimation at large scale. It may be not the case for the study site. Also, there is no soil moisture validation for models in this paper. It may be more relevant to discuss about the response function of soil moisture (the one multiplying with emission capacity).

Section 4.3. In general, it is unnecessary long section, with many parts of discussion is describing results (e.g.,

P17, L4-6, P17, L13-16). Since the trained model G14 has so many uncertainties in terms of predicting future emissions, like, short training period, the ignorance of C source for isoprene synthesis, spending long text to describe how much changes predicted from this model is not necessary. As mentioned at the beginning, I would take this exercise as a sensitivity testing, not prediction. It could be more relevant to compare the testing result from this paper to other literature for the same or similar area and then further suggest what could be further improved in this ANN method.

Figures:

Figure 1: it is difficult to link data to the xtick/xlabels.

Figure 2: what the letter "c" mean?

Figure 3: Why not do the same log-transform for the comparison between the modelled and measured ER? There are many months' data which cannot be shown on the Fig. 3a. Suggest to change them.

Figure 5: check if all abbreviations have been explained in the caption.