

Interactive comment on “Linkages between land initialization of the NASA-Unified WRF v7 and biogenic isoprene emission estimates during the SEAC⁴RS and DISCOVER-AQ airborne campaigns” by Min Huang et al.

Anonymous Referee #1

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GENERAL COMMENTS

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The main goal of the manuscript by Huang et al. is to quantify the effect of different initial/boundary conditions on meteorological variables that drive isoprene emissions. The authors show convincingly how choice of land initial conditions (and in particular the time used for initialisation) have a much larger impact on atmospheric temperatures, and therefore isoprene emissions, the atmospheric conditions.

This is important work that should be of interest to both the meteorological and atmo-

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spheric chemistry modelling communities. The work is well within the scope of GMD. My main concerns are around presentation – I find the text very hard to read, the title uninformative. As a result, the important and interesting messages get lost amongst the details. Substantial editing is required before publication in GMD. More details below.

Title – The title is long and detailed, but to me it doesn't accurately represent what is to come in the text. As someone who doesn't use the NASA-Unified WRF model, I wouldn't read this paper, but having now read it I realise it is relevant to my work after all! I encourage the authors to revise it to inform the reader that the focus is on the impacts of different choices of land and atmosphere initial conditions on ability to simulate biogenic emissions. I don't think the choice of SEAC⁴RS and DISCOVER-AQ is relevant enough to include in the title – these just happened to be the most relevant evaluation observations available.

Readability – The text throughout is dense, often hard to follow, and full of grammatical and spelling errors. I highlight some but not all of these below. I suggest a very careful read-through and edit (if not by the authors, then perhaps by a professional editor). There are a very large number of acronyms used throughout, and since this work should appeal to multiple modelling communities, it may be useful where possible to use a description rather than an acronym. An appendix listing all acronyms (and what they refer to, since in some cases model names are not intuitive) would also be helpful/

Abstract – There are too many acronyms in the abstract, making it hard to follow. Most of these are not needed until the main text. For example, lines 19-20, at this stage the reader doesn't need to know the specific land surface model or reanalysis data set used. Just stating “. . . demonstrating that initialising the input land surface model using a coarser resolution dataset led to significant positive biases. . .” would be much clearer. Same applies elsewhere. Also the sentence “This study emphasizes. . . chemical data assimilation” (lines 29-32) is very long and could be split into two for clarity.

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Introduction – Much of the introduction doesn't seem well suited to the work presented in this particular manuscript. For example, the entire first paragraph that discusses isoprene impacts on ozone seems irrelevant as there is no further mention (or simulation) of ozone. As this is a GMD paper, the focus on O₃ seems irrelevant (and in any case, the references seem spotty and cherry-picked to only discuss those that have shown large responses to isoprene – some others off the top of my head include Wu et al. 2008 (doi: 10.1029/2007JD008917), Millet et al., 2016 (doi:10.1021/acs.est.5b06367)). In the second paragraph, there is too much detail about MEGAN that can wait until the model description (for example, “on flexible scales”, “MEGAN computes emissions based on...”). I suggest reorganising to start with the current paragraph 3, then following with some of paragraph 2 (with emphasis on uncertainties/errors in MEGAN), then paragraph 4.

“Usual” and “Control” – the choice of language to describe the simulations is very confusing. What is described here as “usual” is what most authors mean when they say “control” (i.e. the control simulation is the normal or base method, without modifications. So using the word “control” to describe the simulations where something new/different is done is very counterintuitive. It took me until the 2nd reading of the paper to actually work out which simulations used something new. Suggest “usual” becomes “control” and “control” becomes “sensitivity”.

3.3 Uncertainty discussions – while I appreciate the attempt to discuss remaining uncertainties, I find the current version in Section 3.3 doesn't add much value. It needs to include some discussion (i.e. literature based) of what the expected impacts of each of these things would be (I'm mainly referring to the NUWRF-MEGAN section here). I also find (d) in this section completely meaningless (“other limitations”) – if they aren't going to be stated or explained, then why bother mentioning them?

Fig. 1 – I like the idea of an overview figure, but find this one hard to understand. Suggest adding resolution to the figure, and separating (b) to show the different types of sensitivity simulations that were done (perhaps including names from the table).

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SPECIFIC COMMENTS

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Pg 2, line 21-22: Also Zeng et al., 2015 (doi:10.5194/acp-15-7217-2015); Emmerson et al., 2016 (doi:10.5194/acp-16-6997-2016)

Pg 2, line 28-29: “Much less has been done...” – see Zheng et al., 2015 (doi:10.5194/acp-15-8559-2015), Bauwens et al., 2016 (doi:10.5194/acp-16-10133-2016)

Pg 3, line 18: “key variables” – state them here

Pg 5, line 29 – Pg 6, line 18: I got very lost in this paragraph – which is an important one for understanding what was actually done! I think it needs to be rewritten to first highlight what the different types of simulations are (in simplified form), followed by one paragraph to describe the basic simulation and another (or two) to describe the modifications in the other simulations.

Pg 7, first paragraph: reference Toon et al. (doi: 10.1002/2015JD024297)

Pg 7, line 25 and elsewhere: I think Figs. A1-A3 belong in a Supplement rather than an Appendix as there is no associated text and they are not related to one another.

Pg 8, line 14: “Close to the estimated OH concentrations... 2006” – what are these? Please provide the value here to allow the reader to make the comparison

Pg 10, line 22: DIAL-HSRL derived PBLH is mentioned here but not show or discussed, besides saying the model is “closer to the reality” of it. More discussion or plots needed to justify that statement.

Pg 11, line 32: 30% seems to me like a big bias, seems like this requires some discussion

Pg 13-14, Section 3.2.2: I don't get anything out of this section and am unclear what I

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am meant to take away. I suggest moving it to a Supplement and referencing briefly in the main text.

Pg 14, line 27: “random” – is it really a random error? It seems like if there were even reasonably consistent wind directions in a given location, this would be a systematic error. . .

Pg 15, line 9: “result in Wolfe et al. (2015) of 8+/-1 mg/m²/h”. This comparison value is given in a completely different unit from the one shown in the figure (and it is also hard to compare when the figure and text are not co-located). Please provide the value in the text and make sure the units of the calculated value and the Wolfe et al. value are the same (doesn't matter which).

Pg 15, line 16: “LIS simulation” – explain what type of model LIS is for the reader that skips straight to the conclusion

Pg 16, line 8: “self spin-up method” – what is this? Needs some explanation

Fig 3: Not obvious why (b) and (c) show different coverage in the observations, and the choice of >=80% is not justified. Either justify or (preferable) just show all data in (c)

Fig 8: Would be easier to understand/interpret if bars were grouped by AM, noon, and PM (then colored by different dates/campaigns)

TECHNICAL COMMENTS

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As noted previously, there are many errors of grammar, spelling, or poor word choice. I point out some but not all of them here.

Pg 1 line 25: “modify” -> “reduce”

Pg 1, line 29: “resulted” -> “resulting”

Pg 2, line 5: “50% of reduction” -> “50% reduction”

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Pg 2, line 14: “over the NA” -> “over NA”

Pg 3, line 17: “experimented” not right word, perhaps “tested”

Pg 4, line 6: should “LAI” actually be “gamma_LAI” (with symbol)?

Pg 4, lines 7-8: “which needs to be better understood” – irrelevant to this work, delete

Pg 4, line 16: reference the actual data rather than a blog post

Pg 4, line 27-28: this sentence references Fig 2 before Fig 1 – either need to rearrange the figure or (preferable) remove this sentence and put it in the section where Fig 2 is described

Pg 5, line 13: “Same as in. . .” -> “As in. . .”

Pg 5, line 14: “four-soil layer” - should this be “four-layer soil”?

Pg 5, line 15: “widely used” – any references?

Pg 5, line 26: “full clocks” – what does this mean? I've never seen this term.

Pg 6, line 2: “Same as in. . .” -> “As in. . .”

Pg 6, line 5: “representing” -> “represent:

Pg 6, line 7: “focused” -> “focus”

Pg 6, line 26: “rate coefficients with OH” – does this mean “rate coefficient of isoprene with OH”?

Pg 8, lines 27, 28: “were” “was” (twice)

Pg 9, line 6-7: “Long-term soil moisture changes. . . p72-73” – this sentence is irrelevant to the work presented here, delete.

Pg 9, line 12: “at where” -> “where”

Pg 10, lines 10-13: “The magnitudes. . . daily minima” – long and confusing sentence,

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suggest splitting into two

Pg 10, lines 20-22: "RMSEs ... uncertain) – long and confusing sentence, suggest splitting into two

Pg 11, line 10: Fig 5 referenced before Fig 4 – reorder these figures

Pg 12, line 3: "activate" -> "active"

Pg 12, line 26: "weakest" -> "weaker"

Pg 15, line 21: "resulted" -> "resulting"

Pg 16, line 10: "Experimenting simulations" -> "Experiments using simulations"

Fig 2a: colors are hard to see; how about plotting the color on top of the black line rather than below?

Fig 2d: colorbar missing label

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2017-13, 2017.