Review ACP-2021-1068

Satellite soil moisture data assimilation impacts on modeling whether variables and ozone in the southeastern US - Part 2: Sensitivity to dry deposition parameters

1 general comments

`evaluating the overall quality of the discussion paper`

The manuscript presents a comprehensive comparison of various vegetation relevant variables (e.g. soil moisture, GPP, surface air temperature) modeled with and without soil moisture assimilation. The main feat is the implementation of a data assimilation of soil moisture in two widely used land surface models NOAH-MP and CLM within WRF-chem. The manuscript continues to explore the effect of assimilated soil moisture on the ozone dry deposition. The authors compare the dynamic dry deposition schemes of the NOAH-MP and CLM (Bell-Berry type stomatal resistance) with the Wesley scheme of NOAH-MP (Javis-type stomatal resistance) and NOAH (Javis-type stomatal resistance). Ultimately, they extrapolate their resulting ozone surface concentrations from 2 weeks (Aug 16–28) to vegetation ozone damage risk indices MDA8 and AOT40. All studies are validated against observational data.

- The manuscript is overall well written and
- addresses globally relevant issues.
- It remains, however, unclear what distinguish the *Wesely* and *dynamic* schemes (see specific comment)
- Some remarks need citations (see specific comments)
- Some figures / captions are hard to understand and might need a better explanation in the text

2 specific comments

`individual scientific questions/issues`

- P1L14: *"Realistically representing this process in models is important for accurately simulating* O_3 *concentrations and exceedances [...]"* In general, I agree with this opening statement. Though, the method of assimilating soil moisture observation into the model does make the results partly more realistic, no new approaches to ozone dry deposition different from the usual resistance analogous one are explored. How realistic this is from a micro-physical / micro-meteorological perspective is disputable.
  The authors may change the sentence slightly: "The representation of this process in models is [...]"

- Section 2
  - The authors discriminate "Wesely" and "dynamic" scheme as well as "Ball-Berry" and "Javis" stomatal resistance. In general, these only differ by including a coupling to the leaf area index (LAI) via a soil moisture and temperature dependent photosynthesis (V_{max}) which is also referred to as β schemes. The dynamic scheme also takes the canopy density (shaded vs sunlit leaves) into account. This could be made clearer in the beginning of the section. A coupling to soil moisture and dynamic LAI could be, in principle, also achieved with a Javis-type stomatal resistance model (see *ICP Mapping Manual - Chapter III: Mapping Critical Levels for Vegetation* – Mills et al. (2017)).
  - P4L126: The authors should include (and ideally list) the model resolutions used for this study as well.
  - P5L163: Is there a reason to distinguish small and capital letter stomatal resistances? Do they refer to leaf and canopy level or bulk resistances? The manuscript doesn't say so. Please indicate the meaning of this notation.
  - P6L179: $\mathrm{V}_{\mathrm{max}}$ vs V_{d} (P2L47). It is slightly unfortunate to use capital V for both dry deposition velocities and maximum carboxylation rate. The authors may consider using v_{d} for dry deposition velocities.
  - P5L216: Does the equation here refer to the Javis-type stomatal resistance used in the NOAH version?
  - P5L216: "$\sim 9999, > 40°\mathrm{C}\,\mathrm{or}\,T_s < 0°\mathrm{C}$" is not a good notation. Rather write "else". 9999 appears to be an arbitrarily large number and should be commented on.
  - P6L184–184: It is not clear, how (and if so) the β schemes of CLM and NOAH-MP differ in their mathematical formulation.
  - P6L184: Here and probably also in other occasions, it should be made clear which CLM version and configuration the authors compare to.
  - P6L191: Regarding the Monin-Obukhov similarity theory, different formulations for the universal functions exist. It is known, that the MO similarity theory is not applicable at turbulence resolving resolutions (e.g. large Eddy simulations) and is also challenged by mountainous terrain. This should be taken into consideration at this point. For a discussion see, e.g. Basu and Lacser (2017) and Emeis et al. (2018).
  - P8/9L252–L271: Including a comprehensive table summarizing the key information on the datasets used in Part I and II of this study would be beneficial. Key information should include resolution, temporal extend, observed variables, etc.
  - P9L274: *"MDA8* O_3 *fields over unban and nonurban regions were investigated [...]"* How do the authors account for and assess the effective ozone titration

in the proximity of urban conglomerates? Please elaborate on the skill of WRF-chem in this regard.

  - P9L281-L282: *"Our 13-days WRF-chem model results were linearly-extrapolated to ∼three months to derive the* $\mathrm{POD_y}$ *and* AOT40 *fields."* For a mere comparison concerning the effect of soil moisture assimilation and dry deposition schemes, this extrapolation may not be necessary. This extrapolation from 2 weeks in August to three months appear not to be robust especially in a dynamic scheme. The authors may consider dropping parts of section 3.3 and focus on the core study of their manuscript.

- Section 3
  - P10L313/L215: *"hydrological regime"* and *slightly drier* Has CLM been run with hydraulic stress module (Kennedy et al., 2019)? This would affect LAI and stomatal resistance.
  - P11L223/334: *"Referring to the satellite-derived GVF fields which are also subject to large uncertainty, [...]"* How large is "large" with respect to the magnitude of the differences between models and schemes discussed?
  - P12L353/354: *"[...] been reported in previous studies."* Can you cite these studies?
  - P17L519: How has $\mathrm{F_s}$ been estimated or is it the same as $\mathrm{F_t}$? It is not clear from the manuscript whether this is a direct output from the model or has been obtained from other output variables, e.g. $\mathrm{F_t}$ which was derived from $\mathrm{V_d}$.
  - P13L400 and Fig.7: *"Figure 7 presents the period-mean, daily averaged* $\mathrm{V_d}$ *and dry deposition flux* $\mathrm{F_t}$ *[...]"* Taking the results presented in Figure 10 into consideration, does it make sense to present $\mathrm{V_d}$ and $\mathrm{F_t}$ as daily averages? Effectively, all model integrations show very similar nighttime dry deposition. It could be more informative to show and discuss their day time or noon ($12 \pm 2\,\mathrm{h}$) averages.
  - P13L405: *"[...] many existing model- and measurement-based studies [...]"* Citations?
  - P14L422–423: Would it be possible to indicate the r values in Figure 9? How large are the uncertainties associated with the slopes?
  - P15L558–460: Regarding the bias of the MLM derived dry deposition velocities as mentioned on P9L67, would it be possible to correct for this? Or at least make a more comprehensive statement about the nature of this bias as it affects your model performance evaluation.
  - P15L464: *"responded least significantly"* Consider rephrasing as no statistical test seems to back up the use of the term "significant". Regarding Figure 10, there are no standard deviations or other indicators of uncertainty or variability displayed to help assess the improvement of model skill. If possible

please include such. It would be, in general, interesting to have a look at the relative change of different contributions to V_d, e.g. R_s, R_a. Such assessment has already been suggested by Hardacre et al. (2015).

  – P17L539–541: *"Based on the known seasonal variability of surface* O_3 *and* V_d *in the study region, the linearly scaled* POD_y *and* AOT40 *values may have been overall underestimated."* This is a strong statement, and you should gve some references regarding "the known seasonal variability". E.g. Fig. 2 in Strode at al. (2015) does not quite support this conclusion for all regions of the US. The RMSE in Figure 11m points to a difference between observation and modelled MDA8 of the order of 10–16 %! Which you confirm to be "positively biased" (P16L503). This in conjunction with the very short model integration of about 2 weeks does not support such a strong claim of "underestimation". You should rephrase this conclusion or make a more robust assessment.

- Section 4 should more clearly address the order of magnitude of the different uncertainties (model and observation) relevant to the presented analyses.
- Figure 11 is quite busy. You may consider moving panel m to a Figure of its own.
- Figure 10, the difference between observation and model results should be more thoroughly discussed. E.g. where does the large difference in the temporal extent and timing of V_d come from?

3 technical corrections

`purely technical corrections`

- P1L18: I'm missing the acronym WRF-chem.
- P5L136/L142/L148: *"lass access"* Typo "last"?
- P5L157: GVF should read GVF.
- P7L204/L209: The "[ ]" are not necessary.
- P7L211: *")surface"* missing whitespace.
- P9L274: *unban* Typo "urban"
- P9L282 and others: ∼*three months* use of ∼ in text body should be avoided. Maybe use "approximately" or "roughly" or "about" instead.
- P15L554: *"[...] dramatically higher [...]"* The authors may consider using a more neutral formulation.
- P16L515: *"aggressive efforts"* The authors may consider using a more neutral formulation.

- Figure 3/4: '-' should probably read '$-$'?