Atmospheric Chemistry and Physics
Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2 https://doi.org/10.5194/acp-2022-152-RC2, 2022

Comment on acp-2022-152

Anonymous Referee #2

We thank the reviewer for their comments. The comments have been constructive, informative and will improve the next version of the submission. Please see responses marked in blue text below.

Referee comment on "Sensitivity analysis of an aerosol aware microphysics scheme in WRF during case studies of fog in Namibia" by Michael Weston et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-152-RC2, 2022

This paper presents an analysis of two well-observed fog cases over Namibia with multiple versions of an aerosol-aware microphysics parametrization in WRF. The result show some interesting issues with the microphysical parametrization and its representation of fog, which are certainly worth reporting, although the manuscript could be clearer in explaining what these issues are and identifying possible further work to address them. I suggest the paper could be suitable for publication with some revision to improve this aspect.

To address the point on how the microphysics scheme affect fog formation, we will incorporate the following text into the introduction of the paper.

Motivation for the study
Microphysics schemes in mesoscale models are designed with cloud formation in mind, and not necessarily fog formation. Droplet activation is based on a parcel that is lifted and cooled adiabatically to reach saturation, and is therefore most sensitive to the updraft speed. However, fog often occurs under stable conditions where updrafts are low in speed or even negative. Thus, droplet activation is dependent on other processes like non-adiabatic cooling. This point is highlighted by Boutle *et al* (2018) who observe a cooling rate prior to fog formation of 1Khr^{-1} which is equivalent to an updraft speed of 0.04 ms^{-1} assuming a wet adiabatic lapse rate of 6.5Kkm^{-1}. In their case, the minimum updraft speed in the microphysics scheme was 0.1ms^{-1}, and thus would over estimate fog drop activation. To address this issue, Poku et al. (2021) expanded an existing microphysics scheme to allow for non-adiabatic cooling, which allowed for more realistic cloud droplet number concentration in the simulation.

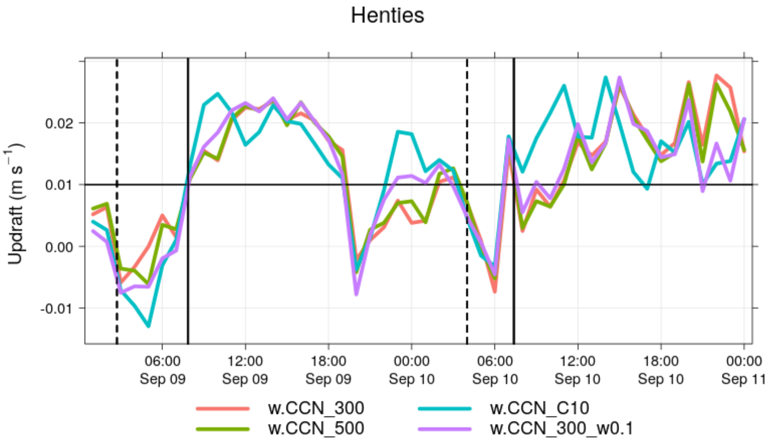
Scientific value
In this paper, we assess an aerosol aware microphysics scheme that uses a minimum updraft speed of 0.01 ms^{-1}, which equates to a cooling rate of 0.23 Khr^{-1}. This minimum updraft speed should solve some of the over activation issues highlighted by Boutle *et al* (2018) and Poku *et al* (2021). The main aim is to see how this scheme performs, before applying major changes in the code to account for non-adiabatic cooling rates or similar. Furthermore, our study site is located in the tropics which we see this as a benefit to the community at large as most fog modelling studies are focused on mid- to high-latitude sites.

Major points:

The authors appear to view the minimum updraft speed used for CCN activation as a tuning parameter. It is not. Whilst it is fine to adjust this parameter as part of a sensitivity analysis, the authors need to be clearer on the reasons for doing this, i.e. it is highlighting deficiencies elsewhere in the model? If insufficient activation is achieved for the physically based default setup (obtained from parcel model analysis), then one of 2 things must be happening:

1. The model updrafts themselves are underestimated. This point is not mentioned at all in the paper, and should be. Whilst I suspect (as usually happens in fog), the model updrafts are correctly small, it would be worth discussing - especially if you have observations of the near surface vertical velocity variance available.

Please see full response to point 2 below. However, to confirm, yes the model updraft speed is correctly small, below 0.02 ms^{-1} prior to fog formation and either negative or below 0.01 ms^{-1} during fog.



2. The updraft is not the process causing the aerosol activation. Whilst this point is mentioned briefly in the paper, it needs to be made clearer, and could be further discussed, e.g. what cooling rate does the change in minimum updraft velocity they try imply, and is this realistic?

Both reviewers have made this point that the non-adiabatic cooling rate may be responsible for the cloud droplet activation and we should present the equivalent cooling rate for our applied minimum updraft speed. We thank the reviewers for highlighting this point. Our response is below and will be incorporated into the next version of the submission.

An updraft speed of 0.1 ms^{-1} equates to a cooling rate of 3.51 Khr^{-1} (2.34 Khr^{-1}) at the dry (wet) adiabatic lapse rate. Our observed cooling rate at 2 m air temperature is below 1 Khr^{-1} prior to the fog formation (Fig. 3). A cooling rate of 1 Khr^{-1} equates to an updraft speed between 0.028 to 0.04 ms^{-1} at the dry and wet adiabat respectively. The modelled updraft speed is below 0.02 ms^{-1} prior to fog formation and therefore in line with the observed cooling rate. Therefore, the use of a minimum updraft speed of 0.1 ms^{-1} does not have a physical basis in our simulation and is used only as part of the sensitivity analysis. It is also clear that if we use a physical basis for the minimum updraft speed that the model will not activate enough cloud droplets and that another physical process must be manifesting the droplet activation.

In our case, we assume that the observed fog events are due to cloud base lowering (CBL). The satellite images in figures 4 and 5 indicate cloud is present over the site before fog is observed in the surface observations (Fig 3). The surface observations indicate that fog starts at 02h43 and 04h00 UTC on the 9th and 10th of September respectively (Fig 3). Cloud is visible from the satellite from 01h00 on 9 September and even 16h00 UTC on 9 September prior to the fog on 10 September. From the literature, cloud base lowering fog events do not show the same cooling rate at the surface as radiation fog, as the overhead cloud inhibits cooling (e.g. Roman-Cascon 2019). Furthermore, the influence of the maritime environment at the site will dampen cooling from the desert surface. Thus, our observed cooling rates are low which is to be expected. The observed droplet number may then be due to the descent of a mature cloud to the surface, which was initially formed under conditions different to the surface observations.

We note that the model does not show this mechanism of CBL. Instead, droplets form in the lowest model level first and the fog layer thickens over time (Fig 22). Thus, the model is missing the initial stratus cloud formation (or advection) and subsequent cloud base lowering. As indicated in Fig 7, the model has a cold bias at the surface and as a result over estimates relative humidity in the lowest model levels (Fig 9). This highlights the complexity of the study site which is at the intersection of contrasting land cover and air mass types (ocean and desert) and that the planetary boundary layer scheme has struggled to simulate this land-sea interface. Adequate modelling of this interface is perhaps an ambitious task and was not the focus of this study from the outset. As a result the microphysics scheme has activated at a lower altitude that the observations.

Some specific (but not exhaustive) examples of text that needs addressing in this regard:

L260-267 - this could be clearer in explaining the motivation, i.e. you're picking the minimum updraft to achieve the observed activation, but this doesn't imply that the updraft is the reason for the activation, i.e. it achieves the right answer but for the wrong reason

We believe that our response to the point above will address this point.

L450-457 - again, need to be clearer here that the right answer is being achieved for the wrong reasons, and add some discussion on what the right way to achieve the desired result could be.

We believe that our response to the point above will address this point.

Minor points:

L14 - I'd say "is used" rather than "is parametrized".

Will edit as suggested

L73 - spelling of "Boutle"

Will edit as suggested

L112 - is 34m really low enough for the lowest model level in fog? Some more discussion on this might be useful - did you do any sensitivity studies to this? It implies that the fog must be at least 34m deep before it is present in the model, which could have significant effects on its early development. I'd suggest the authors look at https://acp.copernicus.org/articles/22/319/2022/ to see what effect the lowest level height can have on model development, e.g. the FV3 results with a lowest level at 21m are quite poor.

Thank you for this interesting reference.

Yes we agree with your assessment and can list this as a limitation in the simulation. Although, we also note that Spirig *et al* (2019) report fog depth of over 200m at Gobabeb in Namibia, where the stratus cloud intersects with the land.

Fig 3, 6 etc - it's usually helpful to plot visibility on a logarithmic axis, due to its highly nonlinear nature, to better show the low visibility events.

Will edit as suggested

L300-315 - I don't really understand here how the CCN is evolved in time in the different experiments, so it would be useful to explain this further. My assumption was that it was a prognostic variable, advected by the flow and processed by the physical parametrizations? But the text seems to suggest that it is somehow diagnosed from the boundary layer depth over land - why? And why is this not applied in the simulations where the CCN is initialised from a climatology - What happens to the CCN when the BL depth adjustment is not used? If it's important enough to discuss, why not process the CCN in the same way in all experiments (with or without this BL depth adjustment) for consistency? I think this is just complicating the results for no good reason, and would be better to be consistent.

We agree that the description of the CCN vertical profile should be included in the methodology before being discussed in the results. We will introduce these concepts in the methodology in the next submission. We will also expand the description of the various scenarios and the motivation for them. Basically, excluding CCN_C10, the treatment of the vertical distribution is the same in all scenarios. CCN_10 does not require any assumption of the vertical distribution as it makes use of a 3-D data set as input.

With regards to the vertical profiles, Fonseca *et al* 2021 (doi: 10.3390/atmos12121687) have documented the equations from the microphysics fortran routine.

$$N(z) = N_1 + N_0\, EXP\left[-\left(\frac{h(z) - h(1)}{1000}\right) N_3\right] \quad (1)$$

with

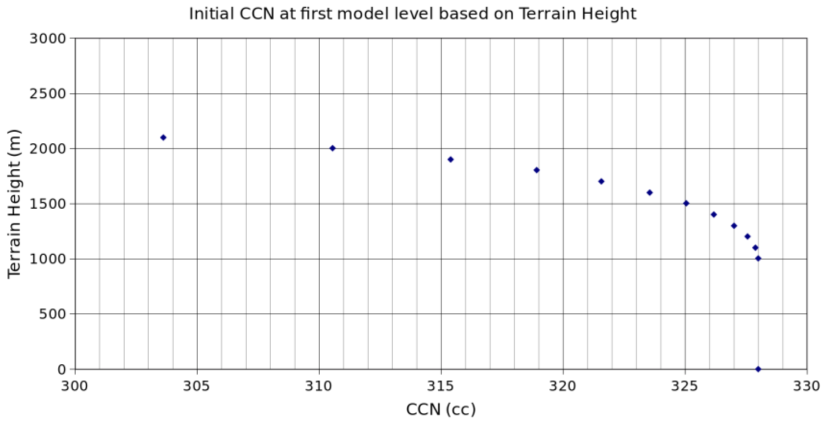
$$N_3 = -\frac{1}{0.8} LOG\left(\frac{N_1}{N_0}\right)\ if\ h(1)\ \leq 1000\ \text{m} \quad (2)$$

$$N_3 = -\frac{1}{0.01} LOG\left(\frac{N_1}{N_0}\right)\ if\ h(1)\ \geq 2500\ \text{m} \quad (3)$$

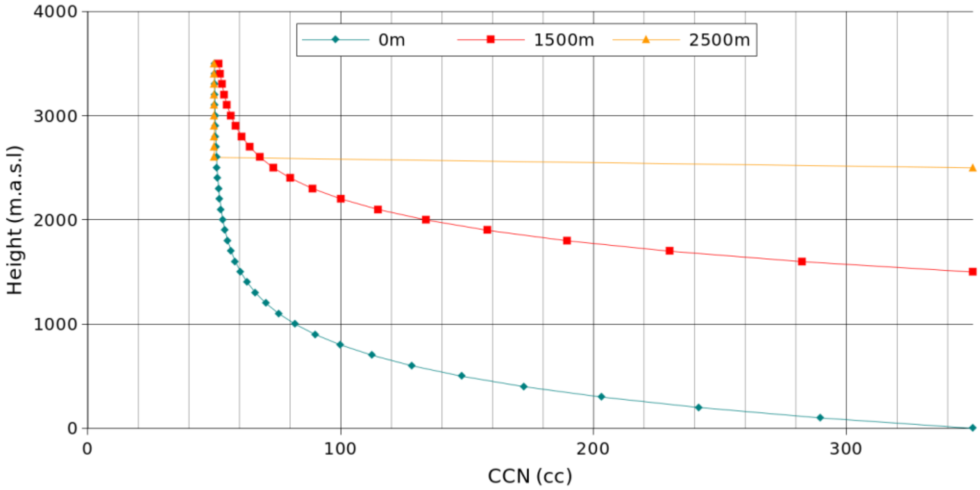
$$N_3 = -\frac{1}{0.8\, COS\,[h(1) \times 0.001 - 1]} LOG\left(\frac{N_1}{N_0}\right)\ if\ 1000\ \text{m} < h(1) < 2500\ \text{m} \quad (4)$$

Fonseca *et al* (2021). *"In the equations above, h(z) is the height of the model level z in meters, with h(1) being the height of the first model level. The constants N1 and N0 are set to 50 × 106 m3 and 300 × 106 m3 for water-friendly aerosols, and 0.5 × 106 m3 and 1.5 × 106 m3 for ice-friendly aerosols, respectively. This definition is based on the premise that aerosols are mostly concentrated in the lowest part of the atmosphere, with a faster decrease with height over the higher terrain, and a profile tailored for the continental United States."*

If we consider different terrain heights (z) and a first model level at z+34 we get the following initial CCN using these equations. Terrain heights of 0 and 1000 m have the same initial CCN, but after 1000 m it decreases. As Namibia has a coastal plain before the escarpment extends beyond 1000 m, the sea and coastal plain has the same initial CCN near the surface, while he terrain above 1000 m has a lower initial CCN concentration. This explains the two classes for CCN_300 initial CCN in Fig 10a.



If we plot these equations for different terrain heights (z) we get the following curves (Thompson and Eidhammer 2014 also show this curve but for much higher initial CCN concentration). Height intervals (dots) are 100 m. The decrease in CCN concentration between level z and z+1 is larger if the starting terrain height is 1500 m than 0 m. Thus, as vertical mixing takes place over time, we expect the surface concentration over a higher terrain point to decrease more than a lower terrain point. This partly explains why the CCN over land in Fig 10b can decrease more than the CCN over the ocean when compared to the initial CCN.



We hope this explanation is helpful.

Fig 10 - would be helpful to use the same scale for panels a-d, rather than varying the left and right columns

I think we tried this at first but lost detail and meaning in the plot. We will try as suggested and decide if we should update these scales.

Fig 10 - it would be worth discussing somewhere why there is such a large discrepancy between the simple initialisations and the analysis - are the simple setups just really bad for this area of the world, so the analysis is the correct thing to use, or how much do we trust the analysis for fog initialisation?

I think we are trying to see which setup is closest to the observations. As part of the process we came to realise that this was an ambitious task due to the complexity of the fog formation described in our response under the major comments. We came to realise some challenges like the cloud over the ocean, the land-sea interface, the cloud formation and lowering were not modelled well. These issues can most likely be addressed in the model through sound physical parameterisation, but were beyond the scope of our initial aims. Nevertheless, these results are informative and could help guide future work in the region.