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Comment on egusphere-2022-356

Anonymous Referee #1

Referee comment on "Evaluation of the NAQFC driven by the NOAA Global Forecast System (version 16): comparison with the WRF-CMAQ during the summer 2019 FIREX-AQ campaign" by Youhua Tang et al., EGUsphere,
https://doi.org/10.5194/egusphere-2022-356-RC1, 2022

Title: Evaluation of the NAQFC Driven by the NOAA Global Forecast System Version 16: Comparison with the WRF-CMAQ Downscaling Method During the Summer 2019 FIREX-AQ Campaign
Authors: Tang et al

Summary:
This article provides an incremental step forward compared to the Campbell et al 2022 paper. Campbell et al provides a huge step forward for the regional air quality modeling field. A major limitation for regional models has been coupling to existing available meteorology. The Campbell et al 2022 is a great addition that includes a similar NACC-CMAQ and GFS-CMAQ comparison, and this paper helps to strengthen evidence that the resulting modeling is credible. If I am correct, the previous Campbell et al. comparison did not isolate meteorology differences. This paper uses the NEIC 2016v1 emission inventory for both models (as well as GBBEPx and BEIS), which allows for a more clear isolation of meteorology. The meteorology still includes both physics-parameterization, scale, input and interpolation differences. The isolation of met is definitely a strength. The paper uses validation against FIREX-AQ and one month against surface observations. The weakness in this comparison is the focus on only summer months, which highlights ozone performance more than PM2.5.

Response:
Overall this is a good paper that characterizes model performance of an important configuration (GFS-CMAQ) and compares it to a more common application (WRF-CMAQ). Perhaps my one disappointment was that the time period for AirNow evaluation was very short and may not highlight issues under a variety of conditions where the model will be applied. I support the publication of this manuscript. Hopefully, minor notes below can be incorporated.

Line-by-line:

Table 2 is awesome!

Pg 7, Ln 4: The 127 layers in GFS are not comparable to the 35 in CMAQ. As noted in your Table 1, GFS uses 127 to reach 80km where CMAQ reaches 100 hPa, which is more like 17km in the international standard atmosphere. It would be more interesting to note the number of layers that are likely to affect the near-surface winds.

Pg 7, Ln 37: for[ ]10

Figure 2: Ideally, these would be based on local times. The 6UTC is about 1am on the east coast while the west coast is more like 10pm. And 18UTC is about 1pm on the east coast and 10am on the west. I know the authors are aware and likely have already considered the implications based on the PBL rise, but the reader won't be fully aware of the differences in model PBL development rates. The sharp rise and collapse noted by the authors raises questions. For example, the geographic differences could have something to do with the rates of rise and drop rather than the ultimate depths.

Figure 3 is very interesting!

Pg 8, ln 24: same as pg 7, ln 4

Pg 10, ln 24-26: How does the resolution you are showing here compare to the resolution used in the CMAQ modeling? Was the NACC output done at hourly resolution?

Pg 11, ln 2-4: Can you expand a bit here? Is the CMAQ-diagnosed W just as appropriate for the NACC interpolated values?

Pg 11, ln 15-17: Is the ozone here VOC-limited? Or, why do you think this when the NOx is underestimated from 20:30Z to 23Z and ethene (a highly reactive VOC) is overestimated? Underestimated CO can actually increase the yield of HO2 per OH reaction.

Pg 12, ln 12-14: Could the high altitude underpredictions be due to instrument interference from CH3O2NO2? (e.g., Browne doi:10.5194/acp-11-4209-2011)

Figure 11: The black outlines of the dots makes it impossible to see the values when at a constant altitude. Can you remove the outline or widen the plots to provide more visibility?

Pg 13, ln 29: nitpicky, but 25UTC should be 01UTC Aug 7

Pg 14, ln 31: I am interpreting non-fire events as all non-fire times. So, these aren't really events. In that way, I would avoid calling these "2019 events" since you are talking about overall statistics. Similarly, the concentrations of SO2 you are seeing are quite low -- the mean biases are fractions of a microgram or ppb. Based on these magnitudes indicating ambient values, it seems plausible that the this could indicate chemical lifetimes or deposition errors too? BTW, most power plants have continuous emission monitoring, which leads to more certain emissions.

pg 15, ln 1-23: The observed NOz is about 1.63 ppb while the sum of measured HNO3 and PAN is 0.69 ppb. True the PAN bias is very low (-0.22 or -0.25 ppb), but clearly this is disproportionate to the NOz low-bias (-0.96 ppb). I would have liked to see a more clear discussion of the role of nitrate in NOy and therefore in NOz. At least some fraction of particulate nitrate is usually included in the NOy measurement (doi: 10.1021/es501896w). The model NO3 is low biased by about 1 microgram/m3, which when converted to std-ppbv is a non trivial fraction of the NOz bias. It would be nice to have a more clear discussion of what contributes to the NOz underestimation.

Pg 15, ln 25-30: Can you discuss where the errors may originate like you did with NOz and NH4, and nitrate?