

Comments on Li Pan et al.:

- 1) This paper shows a great amount of work! I wish that the results hung together better as it is always distressing to find oneself being highly critical of such an effort. Reading this paper, and seeing how many highly experienced co-authors are listed, I have to wonder if these co-authors reviewed this paper before sending it out for review? I think not but I wish they had. (One of the authors is with NOAA/NESDIS and yet in the text only "NEDIS" shows up, twice.)
- 2) The title promises an evaluation of the 'fire smoke simulation algorithm' in the NAQFC, and yet it is unclear to me if the SYSTEM within which this algorithm is being evaluated is identical to the NAQFC, or differs from it in some meaningful way(s).
It isn't quite clear to me whether the fundamental simulations (*with fire*) evaluated in this paper are the results of NAQFC being run in true forecast mode, or in a retrospective set of runs with different inputs defining fires; for example does NAQFC run with raw HMS results while this work used HMS with QAQ/QC applied?
Either state that this system as exercised is configured identically to NAQFC or include a table clearly indicating any and all differences between the NAQFC system configuration and the system as tested with all simulations, both *with fire* runs and without fire runs, for this paper.
- 3) The abstract makes the point that the analysis reveals [lines 34-36] that the system does not obtain and use fire smoke external to the 12-km domain in its lateral boundary conditions. And this is restated in the conclusions in [lines 444-446]. If indeed this is true of the NAQFC, then this hardly qualifies as a discovery so much as a system design deficiency to be disclosed in the methodology where NAQFC is described and thus should be described out front, and hardly seems appropriate to results.
- 4) Throughout the paper, the results under review are variably attributed to either a) the smoke algorithm, b) the system that the fire smoke algorithm is part of (which may or may not be identical to the NAQFC), or the CMAQ model. I think this should be made clear in each and every occurrence.
As I see it, this work tests the CMAQ model *not at all* and so perhaps 'model' should be eschewed and 'system' made the clear object of inquiry. Then too, since the authors are comparing model results with observations, and the model results depend critically, both in SMOKE plume rise and in CMAQ dispersion/transport, upon the NAM meteorology, the absence of any analysis of the NAM performance for this period make statements about evaluation of the smoke algorithm rather weak.
- 5) HMS is also used by the SMARTFIRE system, but SMARTFIRE is apparently not part of the NAQFC system. Is there anything informative to say about

how this use of HMS results in different smoke emissions than use of SMARTFIRE would?

- 6) As modelers we use satellite data and we operate computationally on grids or various sorts. This work uses two domains defined on grids with horizontal grid cell spacing(s) of 4 and 12 km. I suggest consistently referring to these as 4-km and 12-km domains. PLEASE DO NOT say that a 4-km domain or grid is 4-km resolution! Because to reliably *resolve* (to detect) landscape features of a scale of 4 km in satellite imagery requires (an observing instrument with the equivalent of) a grid spacing of 1.333 km, or finer. Say *high resolution* or say *low resolution*, as you wish, but don't equate grid cell spacing and resolution, please. The fact that this is a common error in our literature doesn't make it correct.
- 7) Regarding using a number and units as an adjective indicating or qualifying something, use the form as in 4-km domain, in 6) above. When stating a value, separate the value and the units with a space, as done correctly in Line 199: "1.5 ppb".
- 8) Tables and figures are very poorly presented in that the Table title lines do not explain what they show in adequate detail, and column headings are not defined; in Table 1 NUMS is arrived at how? In plots axes commonly are unlabeled, units are missing, and where color scales are presented, sometimes two in a panel, the species' associations with these scales is left for the reader to infer. Altitude should indicate MAGL or MASL. A reader looking at a Table or Figure should see *everything* they need to interpret it correctly, and should not need to search the text for clues. The IMPROVE site labels should be given text backgrounds to make them legible against the vectors drawn behind them. And why does the label read MACA1 when it is MACA in the text?
- 9) These lines [163-168] confuse me:

CO was used as a fire tracer in the prediction and CO difference (ΔCO) between CMAQ simulation with and without fire emissions was used as the indicator of fire influence. For additional observations, we used potassium (K) collected at the IMPROVE (Interagency Monitoring of Protected Visual Environments) sites within the SENEX domain, acetonitrile (CH_3CN) measured from the SENEX campaign flights and fire plume shape detected by the HMS analysis as real fire signals. Temporal enhancement (Δ) in CO concentration due to fire denoted as ΔCO was directly compared with those signals.

ΔCO is defined first as the difference in [CO] between two runs, and then as the temporal change, meaning change over modeled time(?), during the simulation. Maybe use $d[\Delta\text{CO}]/dt$ if you want to talk about how rapidly the run-to-run difference changes over model time steps?

- 10) In many places the paper's clarity suffers from mechanical writing errors, poor word choice, confusing sentence syntax/construction and other inconsistencies. Here are more detailed comments with line references:
- a. Line 32-33 -- why both signatures and signals?
 - b. Line 41 – how about silvicultural prescribed burns?
 - c. Line 48 – NEDIS?
 - d. Line 77 – use comprises or comprised of, but not *comprises of*.
 - e. Line 82-86 – reword more clearly.
 - f. Line 88 – we're really more interested in the instruments than the platforms.
 - g. Line 106 – *modelling* is a non-US English spelling. It is used once in the text. Be consistent.
 - h. Plume rise formulas are from Gary Briggs and possessive form is Briggs', not Brigg's. Not every use requires possessive form. Line 135 would be “formulas by Briggs” and line 136 would be “The Briggs' algorithm”...
 - i. Line 141 – sentence is confusing.
 - j. Line 177 – scour > search.
 - k. Line 193 - space between 100 and ppb.
 - l. Line 203 – flux flowing > use either flux or flowing, not both.
 - m. Line 225 – suggest 24-h as adjectival form.
 - n. Line 227-228 – maybe label the part following 'but also' as a recognized confound.
 - o. Lines 233 -235 -- Seems to me that the filter for industrial sources is stated in a confusing way.
 - p. About excluding periods with high K as from dust. Can't severe wildfires entrain significant soil dust in their smoke plumes?
 - q. Line 257 -- suggest rationales > reasons.
 - r. Line 265-266 – is it is HMS-derived why not also CMAQ-predicted? Be consistent everywhere.
 - s. Line 270 – Why “In general”? Are there times/places where FMS does not scale with agreement?
 - t. Line 284 “... and our simulation system used a climatologically-based static boundary condition.” Again it is unclear whether ‘our simulation system is the same as NAQFC and *climatologically* is a misspelling.
 - u. Line 296-297 – “However, our model simulation in this study was from a retrospective module using current day HMS fire information.” So was your model run using the same fire information as available to the NAQFC forecast or not? Unclear still on this important point.
 - v. Line 321 – I suggest simulate in place of *regenerate*.
 - w. Line 351 – I suggest interpret for *characterize*.
 - x. Line 419 – *form* should be from
 - y. Line 448 – *kin* should be keen.

