Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Effectiveness of short term air quality emission controls: A high-resolution model study of Beijing during the APEC period" *by* Tabish Umar Ansari et al.

Anonymous Referee #1

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Ansari et al. report numerical model simulations of air quality over China, focusing on Beijing, during the APEC summit in 2014. They investigate the benefit of short-term emission reduction measurements on near-surface pollutant concentrations and investigate uncertainty in model parameters. They conclude that choices of meteorological input data, model resolution and physical parameterisations are central to model performance, that emission controls were valuable in reducing pollutant levels but resulted in meeting air quality standards only because of favourable meteorological conditions.

General comments:

The authors present their research and results in a reasonably well-written manner,

and I could follow their reasoning with ease. There are, however a number of major concerns that I like to see addressed before this is published.

1) Their manuscript is too long, especially the sections on evaluating meteorological input datasets can surely be shortened and superfluous text, tables and figures moved to the supplementary material. This will improve readability and avoid loosing readers before the interesting stuff happens.

Particulate matter pollution is a intricate combination of source and sink processes which are individual for each chemical component, and their reaction to emission changes is as well. Hence all major components need to be represented (well) by the model to make believable predictions. The lack of secondary organic aerosol (up to 25% mass) and dust aerosol, as well as a strong overestimation of $NO_3(-)$ and underestimation of $SO_4(2-)$ are serious, yet total PM 2.5 mass miraculously works well. This can only be for the wrong reasons, which then has strong implications for the reliability of the results of sensitivity studies. The modelling system is in parts outdated and incomplete for an investigation of air quality in such a complex context. Hence:

2) Missing model components (SOA, dust) are readily available, especially for the WRF-Chem model used here, so they should be used

3) The $SO_2(g)$ to $SO_4(2-)(p)$ seems to be wrong and needs to be fixed

4) The emission inventory needs updating, and it should be done in a consistent manner rather than reducing SO_2 by 60% and not touching the rest.

Detailed comments:

P1L19: this relationship is considered to be non-linear (e.g. Apte et al., 2015, Conibear et al., 2018a) according to recent findings - especially for high PM, benefits are much smaller. I suggest avoiding to give an exact number if this is merely the tangent at an (arbitrary?) point of a (now known-to-be) non-linear relation.

P2l5: comma missing after "Independent observational (. . .), modeling"

P2L20: this is not a thorough evaluation of met conditions

P3L1: this sounds like an arbitrary selection of processes to investigate - reason?

P3L15: This contradicts the manuscript by Sun et al. (2016b), cited here as reference. They state in their 'Implications' section: 'We demonstrate the response of aerosol composition, size distributions, and source contributions in Beijing to emission controls during APEC based on comprehensive measurements at both ground level and at a height of 260âĂĽm in urban Beijing. We observed large reductions of secondary aerosols during APEC, of 61–67% and 51–57% for SIA, and of 55% and 37% for SOA at 260âĂĽm and the ground site, respectively, whereas primary aerosols at ground level did not change in the same way. This large reduction of secondary aerosol is closely linked to the corresponding reduction of precursors over a regional scale, which suppresses the formation and growth of secondary aerosol by a factor of 2–3. Our results demonstrate that the achievement of "APEC Blue" is largely a result of significant reductions of secondary aerosol due to emission controls, although the mountain-valley breeze circulation also played a role.' (Sun et al., 2016b) How do you reconcile these seemingly contradictory statements? (Especially given that several of the authors of that publication are also co-authors here) Just because your "emphasis is largely on these components here" (p3l17) does not excuse missing the rest.

P3l30: only reducing SO2 emissions to account for the fact that the inventory is for 2010, whilst you are simulation 2014, is arbitrary - there are projections of Asian emissions available that allow to consistently project the whole dataset.

P3L33-34: again an indication that your model might be insufficient for the task at hand!

P4L2: MEGAN has been developed for North American conditions - can you be confident that it is applicable in China?

P5L20ff It is confusing to conflate comparisons over different spatial areas (domains D1, 2, 3) with different sets of observations given that you do 2-way nesting in your

model. In D1, meteorology and chemistry over the area covered by D2 are calculated on the D2 grid and then averaged back onto D1. Same for the region D3 in D2. So basically you are evaluating observations against model results where low pass filters of different strength were applied - no new information. At the same time you add new stations in the area not covered by inner domains. Weird.

P5L20ff Section 3.1 is overly detailed for a manuscript submitted to ACP, as it provides no further scientific insight beyond showing that meteorological variables can be simulated with good quality (known since 20 years), near-surface observations are difficult to match with a coarse grid (still quite some averaging to do at 3 km!) model, and that ECMWF IFS data seems to be a bit better than NCEP FNL (seemingly the case for 5+ years now). Hence I suggest: Shorten this paragraph to ~10 lines, move Figure 2 and Table 2 and the rest of the paragraph into Supplementary material.

P8L12: so you do overestimate PM 2.5, but you don't have SOA and dust - what makes up for this missing component, so PM2.5 mass matches observations? Why? Does the replacement have the same formation pathways as SOA and dust? How can you pretend your model will react realistically to a change in precursor emissions given you are using different species (and formation pathways) to make PM 2.5 mass?

P8L20: This is roughly the reduction in NO2 expected from APEC emission cuts, no? So your error is roughly the magnitude of your signal, leading to quite a low signal-to-noise ratio that needs to be discussed.

P8L22: As you still have this large overestimation of SO2 in Beijing, how did you come up with the 60% reduction in emissions on P3L30? Why not more? There should be more up-to-date emission estimates for Beijing than MEIC 2010. . .

P8L25: O3 surface obs are notoriously difficult to interpret against model simulations due to the strong titration effects during the night, especially over urban areas. How does the maximum 8 hour O3 look like?

P8L27: by chance any ammonia measurements? HNO3?

P10L20ff: so here you go. PM2.5 and PM10 mass is right for the wrong reasons. Will a scenario simulation give the right answers, then?

P11L3: given that you underestimate SO3, NH3 will happily bind to NO3 to neutralize and form NH4NO3.

P11L6: Could technically be reasoned due to the fact that SO2 -> SO4âĂŤ conversion takes some time, so most of your local SO4âĂŤ might be imported. Given that you are underestimating SO2 outside of Beijing, this would make sense. But: it happens during stagnant conditions, so I would suggest that something seems seriously wrong with your model for seconary inorganic aerosols / SO2-SO4âĂŤ conversion.

P12L3ff: see my previous comments on lacking model SOA.

P14Table4: a candidate to be put in the Supplemental Material

P15L1ff: it is unclear to me what you have done here - how could you mix modelled PM 2.5 up to simulated and observed PBL heights? Did you do additional simulations assimilating PBLH? Explain better!

P16L12: It should be made clear that 3 km average simulations over densely urbanized areas (think high-rise buildings) cannot realistically be expected to match an observation within that area due to the strong local topographical effects.

P15L21ff: SO4 is mainly formed through liquid-phase oxidation of SO2 in cloud droplets to H2SO4 and subsequent salt formation with NH3. Hence SO2->SO4âĂŤ formation is typically not limited by aerosol surface area.

P15L21ff: NH3 preferably combines with SO4âĂŤ to form (NH4)2SO4, only after most SO4âĂŤ is depleted the remaining NH3 forms NH4NO3 (e.g. Seinfeld and Pandis, Atmospheric Chemistry and Physics, Wiley Interscience, 2012). Your SO4âĂŤ is too low from the beginning, this "sensitivity study" hence does not take place in the right

chemical regime. How can you expect your results to be meaningful?

P15L30: We finally come to the topic of this manuscript. After 15 pages. This is too long. See my previous suggestions on how to reduce the extent of this work.

P19L1: I think it is an oversimplification that dust episodes only affect PM10, but not PM2.5. Apparently the APEC summit took place right in a slight dust episode, but you also do not simulate that component. WRF-Chem has multiple, easy to use dust schemes - why don't you just use them?