This paper uses 17 years of CALIOP data to locate three "polluted dust" hotspots, describes the co-located composition with MERRA-2, and then runs an idealized core-shell radiative experiment for one case (May 2010, eastern China) to test how the dust-sulfate mixing state changes the direct radiative effect. The observational half is the stronger part, and I think it could nearly stand on its own. My comments stay on the composition and mixing state side of the work: how the mixture is characterized, what is assumed about it, and whether the observations actually pin down the microphysical states the radiative experiment depends on. I have left the details of the radiative transfer implementation to a referee closer to that area. I recommend major revision. None of the points below ask the authors to redo the study, but several concern assumptions that currently slip in without being labelled as assumptions, and they need sorting out before the numbers in the abstract and conclusions can stand.

**General comments**

1. MERRA-2 carries NO nitrate, and the strongest observation the paper cites points straight at nitrate. The GOCART module in MERRA-2 does not represent nitrate, which is a major PM component over both the north China plain and the Indo-Gangetic plain. Therefore, here is the awkward part. The observation the paper leans on for coated dust is Li and Shao (2009), who found ~90% of dust particles coated in northern China (Line 40), and they report those coatings as predominantly nitrate. But the study uses a dataset that cannot see nitrate to argue that the Capital Region is sulfate-dominated and then models a sulfate shell.

2. The external mixture reference rests on a specific sulfate size with no water uptake, and that one choice sets most of the external vs internal contrast. The externally mixed sulfate is given "*a uniform dry effective radius of 0.35 μm*" (Line 235), no distribution width, and no hygroscopic growth. I would suggest give the size distribution and either include hygroscopic growth or justify dropping it.

3. Section 4 models only sulfate over China, yet the paper's own composition result is that the other two hotspots are OC dominated. Line 165–168 finds West Central Africa OC-dominated and the Indian Subcontinent OC-dominated aloft. This is not a small omission. An absorbing OC or BC coating behaves quite differently from a scattering sulfate one, since lensing acts far more strongly on an absorbing core than on weakly absorbing dust. The sign and size of the mixing state effect could come out very different in those two hotspots. I would suggest adding at least one OC-coating case.

4. The composition story is built in one season, the radiation in another. The hotspots are defined from the DJF climatology (Line 116–118, boxes on Fig. 1h), the composition profiles in Fig. 3 are DJF, and the emission and flux maps in Fig. 5b,c are DJF. But the radiative case is May 2010, i.e. MAM. Fig. 2c shows the Capital Region dust AOD actually peaks in MAM and is much lower in DJF. So the claim that sulfate is the dominant coating species here is made in DJF and then used to justify a MAM calculation. I felt quite confused about this part.

5. The radiative case is a single month, because the effect is largest there (May 2010 case). I appreciate the authors are being honest about this, but the DRE numbers that follow are then written up as general conclusions. So please label every mixing state DRE value in the abstract, conclusions, and captions as an upper bound estimate, or extend the calculation to the multi-year seasonal mean, since the data are already in your hand.

6. The atmospheric profiles are described in two ways that contradict each other.
   a. L174–178: Section 3.1 says the 3-hourly data allow "direct coupling with ... instantaneous atmospheric profiles".
   b. L342: Section 4.2 says the optics are combined with "standard atmospheric profiles".

   Both cannot be true. If temperature, humidity, and ozone come from a standard atmosphere, which one is it, and how does the longwave DRE stay regionally and seasonally representative for the north China plain in May? Please state exactly which profiles were used.

7. A few mixing state references are missing. For a paper built around mixing state and composition, I would expect Riemer et al. (Rev. Geophys., 2019) on aerosol mixing state, Kok et al. (2017) and Adebiyi & Kok (2020) on coarse-mode dust and the SW/LW balance, and some of the work on nitrate-dust radiative effects (for example Karydis or Klingmüller et al.), which is directly relevant given the nitrate gap in comment 1.

**Specific comments**

8. Fig. 9, x-axis units: labeled "Extinction coefficient at 550 nm [$m^{-1}$]" with values up to 60. Please double check the unit.
9. Fig. 1 caption: says the hotspots are "white boxes in Figure 1e," but the boxes are on panel (h).

10. Fig. 4 shading color: the caption says "Blue shading," the text at Line178 says "green shading.".
11. Fig. 9 legend: check the typo "Full External Mixing."
12. Line164: "examine the DJF climatological vertical profiles ... (Fig. 2)" should point to Fig. 3.