Interactive comment on “Diurnal variations of aerosol optical properties in the North China Plain and their influences on the estimates of direct aerosol radiative forcing” by Y. Kuang et al.

Anonymous Referee #2
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Review of “Diurnal variations of aerosol optical properties in the North China Plain and their influences on the estimates of direct aerosol radiative forcing” by Kuang et al., submitted to Atmos. Chem. Phys.

In this study, the authors use in-situ aerosol measurements combined with Mie and radiative transfer calculations to quantify the impact of the diurnal cycle of aerosol optical properties on the direct radiative effect. They find that although diurnal variations can be sizable, their impact is generally more modest. This result has positive implications for studies based on satellite or AERONET retrievals, which are limited to one measurement per day or to a narrow range of solar zenith angles.

The paper is interesting and well written. The demonstration of cancellations of the effects of single-scattering albedo and asymmetry factor is of particular interest. I recommend publication after the following comments are addressed. Because comments involve additional analyses, they may amount to major revisions. In particular, the authors should clearly specify the implications of their work, and clarify their assumptions on the refractive indices used in their calculations. I am also concerned that the authors focus on diurnal variability without saying that day-to-day variability is much larger. Finally, the authors should make a better case on the applicability of their findings to other regions.

1 Main comments

• In the introduction, the authors motivate their study by saying that satellites only provide one or two measurements per day, and AERONET inversions are limited to a narrow range of solar zenith angles (page 341, lines 8 to 24). The authors should be fair, and their abstract and conclusion (after line 27 on page 355) should clearly state that according to their findings, observing an incomplete diurnal cycle has only second-order consequences on direct radiative effect estimates.

• The authors use a fixed refractive index for the core and shell of their aerosol particles (page 346, lines 10 and 19). To what chemical compositions do those refractive indices correspond? Are refractive indices constant with wavelength? If so, wouldn’t that be a larger source of error than diurnal variations in optical properties?

• Throughout the paper, day-to-day variability of aerosol optical properties appears to be a major factor, but it is never quantified. According to Figures 1 and 2, day-to-day variability is much larger than diurnal variability. It would be useful to
compare the two in terms of direct radiative effect. In addition, when discussing
variability in direct radiative effect (from page 353, line 22), variability for case
1 (compared to its 17-day average) should be shown, and the high variability of
cases 5 and 8 deserves to be discussed (especially since the authors mention it
on page 354, line 16).

• The authors claim that their findings can be applied beyond the North China
Plain (page 354, lines 16–19), but this is not obvious and needs a more specific
discussion. Which regions share the same diurnal pattern of relative humidity?
In which seasons? Are aerosols in those regions similarly hygroscopic? I believe
that the results are not as universal as claimed by the authors.

2 Other comments

• Page 340, line 6 and page 355, line 2: Saying that diurnal variations are “evident”
is not useful. It would be better to say what they are.

• Throughout the paper, I recommend the use of direct radiative effect rather than
forcing. The latter term is nowadays commonly used for the radiative effects of
anthropogenic aerosols only (see IPCC 2013 chapter 7, or Myhre et al., 2013).

• Page 341, lines 4 and 20: constant in time? They also vary with wavelength.

• Page 341, lines 5–7: The authors seem to give the conclusion of the study a little
early.

• Page 341, near line 17: The study by Kaufmann et al. (2000) is relevant here.

• Page 342, line 3: “rather small” Be more quantitative: how small?

• Page 343, lines 21-22: The current sentence makes it sound like the temporal
resolution depends on relative humidity. Please rephrase.

• Page 344, line 13: Rigorously speaking, $g$ is an approximation of the phase func-
tion. Using that approximation introduces errors, see Boucher (1998).

• Page 344, line 13: Say here that the AERONET site is not located in the same
place as the HaChi measurements.

• Page 345, equation 3: This assumes that $r_{\text{ext-LAC}}$ does not depend on $D_p$. Is
that assumption reasonable?

• Page 345, equation 4 and page 347, equation 11: I recommend to place brackets
to clearly indicate what terms are under the summation signs.

• Page 345, line 11: A reference is required for the assumed density.

• Page 345, line 18: Again, a supporting reference is needed for that assumption.
Is it a strong assumption?

• Page 345, equation 6: Is it really RH on the left-hand side of the equation?

• Page 346, line 1: Please give the value of $s/a$.

• Page 346, line 4: Please give a short summary of the size-resolved $\kappa$ method, so
the paper stands on its own.

• Page 346, line 5: Note that using Mie theory assumes that particles are spherical.

• Page 348, line 4: Are calculations for the shortwave spectrum only? Are diurnal
variations in solar zenith angle fully accounted for?

• Page 348, line 16: Please give a typical value for surface albedo at the measure-
ment site.
3 Technical comments

- Page 341, line 12: “limited for” -> “limited to”
- Page 342, line 15: “are therefore important” -> “is therefore important”
- Page 347, line 7: “according to the definition” -> “its definition”.
- Page 353, line 20, also captions of Figures 4, 5 and 6: typo: compare -> compared
- Page 353, line 27: exactly -> exact
