Climate of the Past Discussions

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Comment on cp-2022-38

Anonymous Referee #3

Referee comment on "Is it possible to estimate aerosol optical depth from historic colour paintings?" by Christian von Savigny et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-38-RC3, 2022

This study raises a number of important uncertainties that were not considered in the Zerefos et al., 2007 and 2014 methodology, in particular regarding the particle size distribution and stratospheric ozone concentration. It acts as an interesting and useful continuation of research on the question of whether historical paintings encode quantitative environmental information. However, I have four major comments that question the conclusion that "quantitative determination of the AOD from colours in paintings is generally not possible" — while agreeing that error quantification of the Zerefos methodology is an important next step.

Major comments

1) More justification for the choice of parameters used in the sensitivity studies should be provided. Having a sense of how these perturbations compare to values in a typical volcanic eruption would help contextualize the magnitude of the resulting errors.

As one example, regarding the claim that the particle size distribution is a strong confounding variable, it would be useful to provide more justification for the choice of 350 and 450 nm as perturbed parameters. A study is cited of observed values > 400 nm after Pinatubo. The authors do mention a number of other uncertainties regarding the particle size distribution and this sensitivity test, e.g., whether the distribution is log-normal, what are reasonable median values, whether the median scales with the strength of the volcanic eruption, how the distribution depends on the observation geometrics. Another issue appears to be how the median value varies as a function of altitude and latitude (cf. Figure 1b in Bingen et al., 2004). Given these various uncertainties, it is not clear to me whether median particle size can be considered a systematic source of error.

A related specific issue concerns that Pinatubo was a large eruption (VEI 6), and similarly

VEI 6 eruptions (between years 1500-1900) in the Zerefos study have AODs around 0.4-0.6, which is outside the range of AODs shown in Fig. 2 (going to 0.3). It would be useful to include the curves for an AOD value of 0.5 or 0.6 to show how the uncertainty introduced by particle size distribution compares with the full range spanned by AOD values considered in Zerefos et al., 2007 (e.g., their Fig 6).

Similarly, justification for the choice of ozone parameters would also be helpful. It seems that the influence of volcanic aerosols on stratospheric ozone is not yet fully understood. The different perturbations in this study could, moreover, co-vary, which has not been considered. For instance, it could, conceivably, be that a larger volcanic eruption does indeed increase the median particle size, but decreases the stratospheric ozone concentration, such that these two errors cancel to some extent. Some research indicates that stratospheric ozone could decrease if there is a natural (biogenic) source of bromine or hydrogen halides (e.g., Klobas et al., 2017, Ming et al., 2020). The large uncertainties, both in the individual processes and possible interactions/compensations among different factors also introduce substantial uncertainty in the sensitivity tests shown here that is not yet discussed.

2) As a continuation of the points raised in 1), it would be useful to have a synthesized error estimate – that is, jointly considering the influence of the various sensitivity studies. Such error bounds would allow for a like-for-like comparison with the Zerefos results (e.g., as a function of AOD and zenith angle, as in Zerefos et al., 2007 Tables 2 and 3). Such an overall error estimate would also give a sense of how the uncertainties involved in this methodology compare with uncertainties present in other methodologies to estimate historical volcanic AODs, such as from ice cores.

That said, an overall error bound seems difficult to estimate, given a lack of knowledge of how these error sources combine: additively? some compensation as discussed in 1)? It is not clear whether the error sources would combine to yield *iid* observational error, adding noise to the estimates of the paintings. In that case, that a signal in the R/G ratios can nevertheless be extracted, despite the noise, is surprising and would point to a strength, rather than a weakness, in the methodology.

In conclusion, it seems that overall error bounds are necessary before one can make the argument that a quantitative estimate of volcanic AOD from historical paintings is not possible.

3) The art historical analysis in Sec. 4.1 focuses only on a single painter, Caspar David Friedrich, who was known to be more spiritual and less empirical than many of his contemporaries, such as John Constable, JMW Turner, or Johan Christian Dahl. As one example, the German thinker Johann Wolfgang von Goethe became aware of Luke Howard's cloud studies and expounded them in intellectual circles in Germany. As described in Richard Hamblyn's *The Invention of Clouds: How an Amateur Meteorologist Forged the Language of the Skies*, whereas many painters, such as Constable and Dahl actively engaged with these scientific ideas, Friedrich was an exception in categorically dismissing scientific ideas as incompatible with his artistic practice (e.g., refusing Goethe's

request to provide illustrations for Goethe's 1817 essay on Howard, saying the project would 'undermine the whole foundation of landscape painting, *Hamblyn, p. 221*).

It is understood that this section is not an exhaustive art historical analysis. Yet the balance of this section needs improvement. It could note the general spirit of empiricism and greater overlap between the arts and sciences that was present in the period considered from 1500-1900 – rather than simply selecting one painter to make the point that artwork does not contain quantitative environmental information. Environment is, of course, not the only factor that influences artwork, but there is evidence that painters do depict the environment with some sort of fidelity. This evidence can be qualitative (e.g., the Turner quote at the end of Zerefos et al., 2007, Paul Cézanne's critique of Monet that he was "only an eye…But my God!... what an eye!"), but also more quantitative (e.g., Olson et al., 2003, Aragon et al., 2006, Fikke et al., 2017).

4) My fourth major comment relates to the 'tone' of the manuscript, which gives the impression of dismissing any quantitative skill of the Zerefos et al. methodology (e.g., that the Zerefos results "have to be considered highly questionable"). The uncertainties mentioned in points 1) and 2), however, imply that the ultimate magnitude of the error is far from certain. It therefore appears somewhat misleading and unfortunate to entirely disregard this methodology, especially given the paucity of records of historical volcanic AOD.

Perhaps the total errors will someday be shown to be too large such that the Zerefos methodology does not have any quantitative skill, but it is premature to make this conclusion given uncertainties in 1) the perturbed parameters in the sensitivity studies and 2) potential compensations among the different factors considered here. Instead, the authors could consider changing the language to say that uncertainties considered are avenues for further research. Reducing uncertainty about these important issues would allow for better constraining volcanic AOD from historical images.

Minor comments

1) The authors might consider including a schematic illustrating the key radiative processes and quantities, e.g., diffuse downward radiation vs. near-horizon radiance, solar zenith angle, to increase readability for a broader audience. In such a schematic, it could also be worthwhile to show a sunset with bluer sky towards the zenith and increasingly red colors towards the horizon to illustrate the issue of irradiance vs. near-horizon radiance.

2) Zerefos et al., 2007 say that there are large errors when the zenith angle exceeds 90 degrees: "In our calculations we used the diffuse irradiance over the whole hemisphere at the given wavelengths. However if we calculate the R/G ratios shown in Figs. 1 and 2 using the integral of the calculated radiances within 20 degree azimuth, around the setting sun and for zenith angles 70–90deg, then we would be able to simulate with the model

the paintings' ratios even better. We note here that the radiative transfer solver included in libRadtran is only a pseudo-spherical and not a fully spherical code, and therefore its accuracy for radiance calculations is limited at high SZAs.". The current manuscript says, "One issue is that they did not calculate the radiance ratio for a near-horizon viewing geometry, but calculated the ratio of the diffuse downward fluxes at the two wavelengths, although the near-horizon radiance would be the correct quantity in this context." I found this discrepancy hard to understand. Could the authors please clarify why Zerefos et al., 2007 wrote that the accuracy of radiance calculations is limited at high solar zenith angles, but it is no longer a problem – have the models improved since 2007 to eliminate this issue? They also seem to address the concern of diffuse downward fluxes vs. near-horizon radiance in their manuscript and find that it little influences results (as cited above), so I was surprised to see this difference so prominently highlighted.

3) Typo 'Krakatoa' line 9 in abstract

4) Lines around 85, a reference could perhaps be given for the CIE colour matching functions?

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