Authors' response to Reviewer #2: Dr Fred Prata

This paper presents the hypothesis that an important climate forcing event in the 19th century and seen in the records of Greenland ice cores, was due to the eruption of an undersea volcano (Ferdinandea) off the coast of Sicily that occurred in 1831.

This is an innovative, interesting and scholarly research paper and it should be published. It is innovative in its use of "eye-witness" observations of certain atmospheric optical phenomena associated with volcanic aerosols to estimate size, composition, transport and even the amount of S in a little known volcanic eruption. It is interesting because it reveals new information on processes affecting the injection of aerosols into the stratosphere as well as perhaps solving the mystery of a sulphate signal in the Greenland ice core record thought to be from an 1831 tropical eruption. It is scholarly (there are > 120 references) as it involves collection and research of historical documents and records in several different languages, as well as covering topics in volcanology, atmospheric optical physics and dynamics and even some radiative transfer.

[R2,1] Thank you, Dr Prata. We are grateful for your helpful and detailed comments.

Specific Comments

The hypothesis presented relies on three key points:

1. That a small (VEI 3) volcanic eruption could inject aerosols into the stratosphere.
2. That the event injected sufficient SO2 to register a signal in the Greenland ice core.
3. That the aerosols were transported westwards in agreement with the (indirect) observations presented.

I cannot comment on point 2 as I am not sufficiently expert to assist the discussion, but I can provide some thoughts on points 1 and 3.

Point 1. The idea that an undersea, relatively minor eruption could produce stratospheric aerosols seems surprising. However, as the authors note, the eruption of Krakatau in December 2018 did just that. More recently, the 12 August 2021 eruption of the undersea volcano Fukutoku-Okanoba, off Japan generated a column that reached up to 18 km (in the stratosphere). These eruptions were monitored by modern satellite instruments and provide unambiguous evidence of stratospheric injection. The 1963 eruption of Surtsey was reported to have generated a column up >9000 m which is stratospheric at the high latitude of Surtsey. Thus there is evidence that near sea-surface volcanic eruptions can generate tall columns, even if the amount of solid-rock material is not large. The mechanism for this is thought to be due to a combination of tropospheric convective instability and the interaction of hot material with sea water, generating additional convective available potential energy (CAPE) which drives the buoyancy of the column. I think the authors should perhaps describe this in more detail as they must convince readers that a small VEI eruption can generate a stratospheric aerosol – crucial to their hypothesis.

[R2,2] It is indeed important to establish that relatively modest phreatomagmatic eruptions at sea-level can inject aerosol into the stratosphere but, as you say, the 2018 Anak Krakatau eruption and the 2021 Fukutoku-Okanoba eruption both provide 'unambiguous evidence' that they do. In this paper, our aim can therefore be narrowed to making a plausible case that the same could have happened in the different context of

the 1831 eruption. We trust that our discussion in manuscript lines 374 - 434 is sufficient to make this plausible case and that, in the ordinary way, our reference to Prata *et al.* (2018) is sufficient in terms of detailing at least key features of the CAPE mechanism. In terms of a more detailed application of the CAPE mechanism in the context of the 1831 eruption (*i.e.* to test our hypothesis on this point), however, that will involve a set of further considerations including choices of assumptions and data sets which we do think can only be satisfactorily addressed elsewhere.

Point 3. The authors estimate the zonal transport of the aerosol to be ~20 m/s westwards. This is quite fast. There are no detailed vertical wind profiles available for 1831 but there are modern climatologies and also good knowledge of the atmospheric circulation based on solid meteorological foundations. Eruptions from Mt Etna (a volcano close to Empedocles/Ferdinandea) generate volcanic plumes that predominantly move eastwards and sometimes northwards and southwards, but rarely westwards. This is because these eruptions are mostly tropospheric where the winds in summer are from the west. Modern climatologies of the vertical zonal winds suggest that up to the tropopause at 40 N during the NH summer winds are from the west. Above the tropopause the winds gradually shift to the east in accordance with thermal winds caused by the vertical gradient of temperature. As the sign of the gradient changes from negative to positive the winds change from eastwards to westwards. But, according to modern zonal wind climatologies for midlatitude NH summers, the magnitude of the westward winds is much less than 20 m/s. At 70 hPa (~18 km) it is generally <10 m/s and does not reach ~20 m/s until 10 hPa (~30 km). It is possible that in August 1831 the stratospheric winds were anomalously high, but that would be somewhat speculative and convenient. I wonder then whether the authors should re-evaluate the speed of transport or perhaps put an error range on their estimate to allow for this inconsistency.

[R2,3] As yet, our conclusion as to the 20 ms^{-1} aerosol transport velocity is admittedly based on relatively sparse data (sect. 3.4, Figure 7, sect. 4.2 and Figure 11). Assuming even observational errors of ± 12 hours (*i.e.* morning *vs* evening observation or *vice versa),* will not materially change it. We certainly take the point, though, so to flag the potential for an apparent inconsistency or that the aerosol could have been injected at a greater altitude, we have revised the manuscript at line 441 to include the following paragraph:

> **"Zonal mean wind fields derived from twentieth century data suggest that easterly wind velocity (40º N, July) would not be expected to exceed about 10 ms^{-1} below an altitude of approximately 20 km (30 hPa) in the low stratosphere and that a velocity of about 20 ms^{-1} would only be expected to be reached at an altitude of approximately 35 km (8 hPa) in the mid-stratosphere (Randel, 2003). A particular focus for this hypothesis testing will therefore examine whether the aerosol is likely to have reached only the low stratosphere, in which case the easterly aerosol transport velocity estimated in section 3.4 (20 ms^{-1}) would be inconsistent with the easterly wind velocity suggested for the lower stratosphere by the zonal mean wind field data (10 ms^{-1}) by a factor of two, or whether it could have reached the mid-stratosphere."**

None of these points are serious enough for me to suggest a revision is needed and I am happy to recommend publication subject to technical corrections. Indeed I found the paper so interesting that I wondered whether the authors had exhausted all observations, such as reports from ships logs (perhaps too scant?),

[R2,4] Thank you. In fact, the literature search presented in this paper already extended over several years. There are no doubt many further observations that remain to be found, though, and we certainly encourage the testing and refinement of our reconstruction through searches for further such observations (manuscript lines 560 – 561). Yes, although we have not yet had the time to review them in earnest in this case (although see, for example, source B1), ships logs are potentially an excellent further source of high-quality observations, as demonstrated by, for example, the RECLAIM project (Wilkinson et al.).

were pumice rafts observed?

[R2,5] Although they were not a primary focus of the literature search, yes, pumice rafts were reported in the vicinity of the eruption.

or could the possibility that the aerosols reached the upper stratosphere be sustained (where there are stronger winds), in which case one might expect observations of PSCs during the NH winter. I believe the earliest documented evidence for PSCs dates back only to the 1870s.

[R2,6] Please see [R2, 3]. We have certainly been interested in this possibility. To that end, one of us (CG) has also collated a body of observations of unusual twilight phenomena which took place from August 1831 for reporting and analysis elsewhere. To take the point more immediately, though, we have revised the manuscript at line 570 to raise the issue for future work in the following way:

> **"Analysis of reported observations of unusual atmospheric optical phenomena both in 1831 and in 1883 may support further investigation in a number of additional directions…Further, the duration of reported twilight glow observations in 1883 were used to constrain the altitude of the aerosol responsible (Symons et al., 1888; Meinel & Meinel, 1991). In the context of testing hypothesis H2, an analysis of the duration of the reported twilight glow observations mentioned in sect. 4.2, as well as of a supplementary collected body of contemporary observations of twilight glows (and other unusual twilight phenomena) should provide independent evidence as to the altitude reached by the aerosol responsible in 1831."**

Minor comments

These are really quite minor.

1. I don't think it is necessary to have a full-stop after the longitude/latitude directions (e.g. N. should be N)

[R2,7] We have removed full-stops from the cardinal directions in the revised manuscript.

2. Extinction coefficient and extinction efficiency factor are both acceptable but just use one. I think extinction efficiency is most commonly used.

[R2,8] The term 'extinction efficiency' is used in the revised manuscript. Please see [R1, 13].

3. I suppose that sometimes observations were hampered by cloudiness. One cannot expect a particularly complete set of observations of the Sun so I think some inconsistencies could be explained by lack of visibility.

[R2, 9] Source [A19] reports that the overcast sky was of a 'threatening' dark bluish colour but, yes, it is not clear whether thicker cloud layers could elsewhere have made observation of the phenomenon impossible.

4. Were there any lunar observations? (The Moon was 43% illuminated and 23 days old on 14 August 1831).

[R2,10] Yes, there were several observations of a blue(+) moon reported in the sources collected in Appendix A but in the interests of brevity we did not include them.

5. I noticed in Table 1 A22 that there was mention of a "black dot" observed on the Sun. Could this be a sunspot? If so,it adds weight to the drastic diminution of light from the Sun, as observing sunspots requires considerable rejection of sunlight. There are > 200 observations of sunspots made in 1831 (see: https://onlinelibrary.wiley.com/doi/abs/10.1002/asna.201111601) some with notes. These records are held at the archives of the Royal Astronomical Society in London, so it might be possible for one of the authors to view them and investigate whether anything unusual was noted on solar observations during August 1831.

[R2,11] Yes, there were several observations of a sunspot (or, depending on the visual acuity of the observer, a group of sunspots) reported in the sources collected in Appendix A but in the interests of brevity we did not include them. The presence of a large sunspot tends to increase the likelihood that observers will notice the reduced magnitude (manuscript section 3.6: between $\Delta M = 3.4$ and $\Delta M = 12$) of a naked eye sun. Given, perhaps, a similar interest in (naked eye) sunspots as the reviewer, one of us (CG) has collated these and other contemporary observations of the naked eye sunspots in August 1831 for reporting and analysis elsewhere (and had indeed paid an initial visit to the RAS (!) to look over Schwabe's observations with a view to using them as a baseline set against which to judge the naked eye observations).