

Interactive comment on “An independently dated 2000-yr volcanic record from Law Dome, East Antarctica, including a new perspective on the dating of the c. 1450s eruption of Kuwae, Vanuatu” by C. T. Plummer et al.

Anonymous Referee #4

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Plummer et al. review

This is an interesting and important paper that should be a valuable contribution to sorting out the difficult task of southern hemisphere volcano chronologies for the last 2000 years. The paper should be published but there are some points that in my opinion need addressing:

1) first and foremost – the authors MUST agree to publishing the seasonal sulphate data from both Law Dome and NGRIP at the time of formal acceptance of this sub-

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mitted paper. In order to make the data as widely disseminated as possible, the data should be included as both a supplement to the paper and also contributed to two key data centers – the NGDC paleoclimate data centre and the ice core data centre. Not all potential users are equally familiar with all sites so posting the data in different locations maximizes the chance of the data being put to widespread use. I emphasize the term MUST because the ice core community has sometimes been very slow if not outright reluctant to publish some of their data – especially for some reason the sulphate data, which some investigators seem to think is their own personal property. In a time of widespread debate and controversy about data availability – especially with respect to data used in climate studies – this attitude of the ice-core community can no longer be tolerated. Note that I am not singling out the authors of this paper for particular criticism, just commenting as a “user” of such data that the problem exists in general but must now be addressed, head on, in particular with respect to every new case that arises.

2) while the authors make an interesting case for an alternate date to Kuwae, I am still confused because I was under the impression that the chronology of the, for example, Talos Dome record was also based on layer counting and the date for the large 15th century sulphate peak was 1452 – very close to the “standard” date of 1453. In fact even the Law Dome record seems to have a little bump in the raw data about 1453 – could you be also looking at two eruptions in the Antarctic record as well, and if so which is Kuwae? The Taylor Dome record, whose chronology can of course be disputed, also shows a peak in 1453 and since that core seems to be dominated almost entirely by local eruptions, is it possible the 1453 sulphate layer is from a local eruption and 1458 is the more distal Kuwae peak? If so, Sulphur-34 analyses might be very helpful in sorting this out eventually. As it now stands I am happy to let the authors stand by their opinion – nothing is so well known about the timing that we can exclude definitively one date or the other, but perhaps the authors might want to contemplate this possibility a little more and, at the very least, explain why their chronology is superior to Stenni et al’s Talos Dome record.

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- 3) For the record, I think I believe the authors' revised date estimate for Kuwae. . .
- 4) The opportunity to better correlate first-millennium volcano records is at least as important as Kuwae age – text reads that way but not title and abstract
- 5) I am surprised the GISP2 sulphate data has not been used to check more against the NGRIP and Law Dome records. I think some people are wary of the GISP2 chronology in the first millennium but it does have the right date for Vesuvius (within one year) and there is another peak around 472 that could well be another Vesuvius eruption. Examining the excess sulphur in the 531 interval of G2 suggests only about 10 kg/km² for the peak – much less than NGRIP and possibly indicating that NGRIP is recording a high latitude eruption (I might add that the Tambora level for GISP2 is comparable to NGRIP, as one would expect for a low-latitude eruption in which the stratospheric layer might be less patchy than for a high-latitude eruption).
- 6) On a similar note I cannot find any support for inferring a low latitude source for NGRIP year 674 when there is no support in G2 for such an eruption – it could be coincidental eruptions in high latitudes.
- 7) On a similar note I am sceptical about attribution of v large NGRIP fluxes 258 to a low latitude eruption – again G2 records only about 5 kg/km².
- 8) The above point is relevant to the fact that the authors have not seem to consider that not all volcanic eruptions are recorded in all ice cores and that some of the patchiness between, say, LD, DML, and SP may be due to that rather than errant chronology. I admit that the argument for a drift in DML chronology could be happening but its seems ad hoc to infer a drift in SP and then reverting back to being synchronous with LD at 532. Some readers, myself included might just think the offsets between SP and LD may simply reflect the patchiness I refer to.
- 9) Table of volcano affiliations – from my experience I consider it very unlikely that any VEI = 4 event has a significant climate impact, and VEI = 5 events even seem to be

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modest – would certainly doubt fluxes much greater than 10 kg/km^{**2} as $\text{VEI} = 5$.

10) To drive the point home for a final time – SUBANNUAL DATA MUST BE PUBLISHED!!

Some of the above points are quibbles rather than direct challenges. Nevertheless I think the authors need to think a little more about their paper. I recommend acceptance after modest revision.

End of review

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