We thank the anonymous referee for their review, and address the points raised below.

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 submitted 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.

As has been the case previously, the Law Dome seasonal sulphate data will be made available with acceptance of this paper at http://data.aad.gov.au. The NGRIP data will be made available also (see appendix A for details).

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.

In the Talos Dome record, the date for Kuwae was set to 1452 CE and used as a dating reference horizon (Stenni et al, 2002; Gao et al., 2006). Law Dome is independently dated, with no external reference horizons, only layer counting. We believe the most accurately dated records are layer counted records that do not "adjust" or "tune" their records using volcanic reference horizons. If the chosen dates are wrong, which is highly possible prior to the well dated historically documented eruptions; this leads to errors in the timescale. If dating needs adjustment during the relatively short time period of known volcanic activity, there is a clear issue with the ice core timescale or dating methods. Ice cores dated through non independent methods still hold valuable chemistry information, and the ability Law Dome has to

resolve dating uncertainties with older eruptions may allow better correlations between records to make the fullest use possible of this information.

There is a 10-month gap in our volcanic sulphate record between 1452.3 and 1453.1 CE, therefore we cannot conclusively rule out a volcanic event during that period. As we stated in our paper, we cannot be sure which of those two possible eruptions is that of Kuwae. We suggest the later eruption (1458 CE) is Kuwae because of its reported magnitude and location. The clear signature of the large event (often one of the largest events in the past two millennia) is evident across multiple cores, so irrespective of whether this signature is a result of the Kuwae caldera, we are confident this is the same eruption in all cores, and the one most likely to have a significant global temperature effect. The NGRIP data, together with tree ring responses (e.g. Briffa et al., 1998) and historical evidence (e.g. Pang, 1993) suggests a NH eruption took place around 1453 CE, 5 years prior to the large eruption signature attributed to Kuwae. The majority of Southern Hemisphere cores do not show this second, earlier signature. It is possible a coincidental local (Antarctic) eruption may have taken place,

3) For the record, I think I believe the authors' revised date estimate for Kuwae.
We thank the reviewer for their comment.

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.

We have revised our abstract to state that we are able to better correlate records.

*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/km**2 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).*

There are two candidate eruptions in NGRIP that may be aligned with the Law Dome 531 CE eruption (dated 529 and 533 CE respectively in NGRIP) and we have aligned our records to the 533 event, which is the smaller of the two in NGRIP.

The referee suggests possible issues with the GISP2 timescale, but also worth noting is the lower resolution (bi-annual) sulphate record (compared to the sub-seasonal NGRIP record). Comparing volcanic event depositional values between cores can be a difficult prospect, as variability site properties (e.g. atmospheric transport pathways, amount and frequency of snowfall, post-depositional processes and sampling techniques) can have an effect on the preservation volcanic signatures. Previous studies (e.g. Cole-Dai et al., 1997; Palmer et al., 2002; Castellano et al., 2005) have normalized flux values relative to Tambora, which should be less dependent on site-specific glaciological. Such detail is outside the scope of this work, but a logical progression from this study.

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.

As the reviewer mentioned in point 8, not all eruptions are recorded in all ice cores. The possibility for contemporaneous eruptions always exists, and further investigation may prove this to be the case, however, the absence of a signal in GISP2 is not evidence of this.

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.

As discussed in point 5, there are reasons why the two ice cores could have different flux values, even when just a short distance apart. In the NGRIP record, there is no other volcanic signal in the vicinity of the 258 event.

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.

We are not in a position to know all the details in establishing the DML and South Pole timescales, however, the drift or "noise" in both the DML and South Pole ice cores is within the reported error bounds of the respective cores (Traufetter et al., 2004; Ferris et al., 2011). The 531 CE eruption is a distinctive horizon due to its large size and duration, therefore we believe this match to be correct across the cores highlighted in this study. In the case of South Pole, the date for that particular event is 531±15 yrs, and the events matched either side of this 531 CE event is close to 15 yrs different from our Law Dome dates.

*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 modest – would certainly doubt fluxes much greater than 10 kg/km**2 as VEI = 5.*

The volcanic affiliations were determined by Palmer et al., 2001. The volcanic explosivity index (VEI) is determined by a number of variables, and there may not be a linear correlation between VEI and sulphate deposition in an ice core. Event deposition depends on several factors, including the sulphur content of and transport of eruption gases, distance from eruption centre and post-depositional factors affecting the preservation of the signal at the ice core site. We make no claims about the ability of VEI 4 or 5 eruptions to affect the climate, and we consider such a study interesting, but outside the scope of this work.

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