

Interactive comment on “Climate dependent contrast in surface mass balance in East Antarctica over the past 216 kyr” by F. Parrenin et al.

Anonymous Referee #2

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Review of Fujita et al. and Parrenin et al.

Two papers by Fujita et al. and Parrenin et al. were submitted as companions. They both use volcanic matches between the Dome C and Dome Fuji ice cores to synchronize the timescales. The result is that the relative depth-age scales show considerable disagreement in certain periods likely driven by variations in accumulation rate. The two papers have slightly different foci, with Fujita emphasizing the timescale differences and Parrenin et al. exploring the accumulation relationship. They are closely related so I have written a single review for both papers.

The two papers had a lot of overlap and I think they would work better as a single

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manuscript. I think the Parrenin et al. paper could fit nicely as a section or two in the Fujita et al paper. Alternatively, one paper could focus on the volcanic match (see below) and one on the timescale and SMB implications.

The new and fundamental contribution of this paper is the volcanic match synchronization between Dome Fuji and Dome C. Evaluating the robustness of the synchronization is critical to the work. Relatively little is written about the matching and only a single example of the matches is shown (Figure 2, Fujita). I will detail my concerns about the volcanic matching first and then move on to the remainder of the two manuscripts.

Event Matching The first thing I noticed is that the previous interglacial period (i.e. 120-130 ka) has about double the match points as the Holocene (i.e. 0-10 ka). This surprised me because the previous interglacial has been thinned to less than half of its original thickness which typically makes identification of volcanic events more difficult. Some of the MIS5e peaks may also become less distinct due to diffusion. I did not see any note or discussion of this interesting feature. I do not think the volcanic activity of the previous interglacial was twice as great as during the Holocene.

I am also confused by the process. The authors first found “major tie points” but do not describe what that means. Typically, it is the sequence of events, and not the magnitude of a single event, which best determine the tie points. The authors need to describe their method in more detail, and provide multiple examples of what constitutes a “major tie point”.

The other thing that struck me was that there were no other data sets used to test the matching. What about Be10 at the Laschamp event? What about geochemical fingerprinting of tephra layers? These outside data sets would provide an enormous boost in confidence to the matching of non-specific bumps in electrical conductance and sulfate.

The statement on F412,L5 “We note that there are no uncertainties associated with the use of different proxy records (ECM, DEP, ACECM and FIC) for the identification of

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volcanic events” is wrong. DC-ECM, DEP/AC-ECM, and FIC measure different things and are not always the same. The 18 ka event (Hammer et al., 1997) is the best example of this: if you had the ECM from one core and the Sulfate from a different core, the events would look completely different. The analysis needs to be more thoughtful and describe why these different measurements record the same volcanic events often enough that it is not a major problem.

The appendix focuses on the semi-automated method for selecting “minor tie points”. I have many questions about this method and think it might be finding lots of incorrect tie points 1) Why is the acceptable match tolerance set as a fixed distance of 0.1m when the average annual layer thickness differs down the core (by a factor of ~5 from the surface to the depth at 216 ka for Dome Fuji)? It would seem to make more sense for the acceptable window to be scaled to the approximate annual layer thickness. 2) On line F422,L25, they write “volcanic events as rare as every ~154 years (in average)” but in fact the 154 years is only the average occurrence of volcanic events that can be matched. In high resolution Antarctic cores for the past couple thousand years, the occurrence of volcanic events is about ten times that (every ~15 years, e.g. Sigl et al., 2013). In fact, matches of multiple cores around all of Antarctica reveal that upwards of 80 events in the past 2000 years (up to every 25 years) can be matched (Sigl et al., 2014). A discussion of the number of events that are identified but not matched would be very useful. 3) When my concerns in 1) and 2) are combined, it seems like there is a high probability of finding incorrect links. A 0.1m tolerance, which is a 0.2m window, is a time span of about 20 years during the previous interglacial (and more deeper in the core or at colder periods). This could lead to a very high probability of mismatching.

As a last point, it is unclear to me what the plans are for making the data publicly available. This is ESSENTIAL so that others can evaluate the quality of the matches themselves. I could not find the Dome Fuji data which may be because this is the first publication with it. The Dome C ECM and DEP available through NOAA Paleoclimate data archive were not of the same resolution as presented here. I did not check the

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EDC sulfate data. The recent paper on the NEEM timescale (Svensson et al., 2013) which was dated by matching the ECM and DEP records to NGRIP has set the standard for releasing the underlying data. This is critical because anyone can make their own determination of where the matches are robust.

Fujita et al. My comments on the remainder of the Fujita et al. paper are rather brief. I found the writing to be rather confusing to follow. Overall, the analysis of the causes of the age discrepancy is solid (if challenging to keep track of). I think a section that reviews the basics of the timescales and the methods and age markers used to construct them would be very helpful. Those readers already familiar with the timescale construction could simply skip over the section, while a review would likely benefit the majority of readers who haven't kept up on the details of the timescale construction.

The final paragraph of the conclusion makes a strong case for large uncertainties in the age markers, and hence the underlying timescales. I wish there had been a discussion of whether the age uncertainties given for the two ice core timescales (DFO2006 and AICC2012) are compatible with the work here.

I also wonder about the use of the term “age gaps”. This makes it sound like ice of certain ages is missing, which isn't the case.

Parrenin et al. The Parrenin et al. paper focuses on the ratio of surface mass balance at the two sites. Overall, this paper seems more like an extended outline than a final manuscript. The methods are not well described and the discussion items are mostly lists of possibilities. There are only two figures that are not repeats of figures in Fujita et al. I think this work would fit well into the Fujita et al. paper since much of the start of the paper is a repeat of what's described there and the ideas in this paper could be condensed into a nice discussion section in that paper.

The SMB has to be reconstructed from the depth-age relationship using a thinning function. This is the synchro-based SMB ratio. They also find an ocean-corrected and a source-corrected SMB ratio based on scaling to water isotopes. These synchro-

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based and isotope-based SMB ratios mostly agree, except between 102 and 112 ka. I wonder if the sharp jump at ~111 ka is indicative of the wrong volcanic match.

Section 3.1 – the comparison of the SMB ratio and the dD values is undeveloped. The authors write that Figure 4 suggests a “correlation” but I don’t think the authors actually performed a statistical correlation. It should be straightforward to do.

Section 4.1 – This section is quite interesting. But the theme of this section – that isotopic values, accumulation rate, and site temperature have a very complex relationship, seems to be forgotten in the following sections.

Section 4.2 – The reliability of the thinning corrections should be evaluated in more detail. Fuller descriptions would be very helpful. Explain why a negligible decreasing trend toward the past supports the thinning corrections. Also, describe the mass conservation and the relationship to the radar data more fully. How large were the radar surveys? What spatial scales need to be considered? I would also like there to be a description of how time-varying vertical strain profiles would impact the inferred SMB. For example, both of these sites are drilled near divides, where the vertical strain rate is different than at flank sites. What is the influence of allowing different vertical strain patterns through time. Also, the basal topography is rough at EDC (I’m not sure about Dome Fuji). If the dome had migrated over higher bedrock at different times, how much might this affect the inferred SMB? If this remains a stand-alone paper, there is plenty of space for additional text and figures.

Section 4.3 – After reading Section 4.1, I’m not sure these simple scaling arguments to get a quantitative temperature change are worthwhile. Why should a constant temperature-accumulation relationship be trusted?

Section 4.4 – The last paragraph should be expanded. Be specific about how much the flux calculations, chronologies, and firn and ice sheet modeling will be affected. Are there conclusions in the published literature that are now challenged by the findings in this paper?

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Section 4.5 – The inference of possible impact on site temperature reconstructions is too simplified. First, this section should be integrated with the following section where the complexities of ice elevation change are discussed. But is a 50m or 0.5C change even significant given the other uncertainties in the calculations of past temperature change – i.e. isotope/temperature scaling (since borehole thermometry cannot be used).

Section 4.6 – It is an interesting list of possible atmospheric reasons, but is there any way to test them? Are there measurements already completed, or that could be made, to distinguish between possibilities? The paragraph on the dome position is also too simplified. What is the spatial pattern of SMB across the domes? What is the spatial relationship between SMB and isotopes across the domes? I don’t follow the logic why a migrating dome could not have an impact.

One last comment, why is the companion paper referred to as “Fujita, Parrenin, et al.” when all the other references use only the last name of the first author (i.e. Church et al.)?

Interactive comment on *Clim. Past Discuss.*, 11, 377, 2015.

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