Abbreviated review comments for previous versions, which have not been addressed.

(After the first revision)
They have not fully tested their data and filter. As I wrote in the last review, they should have evaluated the overall uncertainty of their method by a statistical approach and also separate the effects from (1) data precision and (2) data resolution and (3) filter performance, respectively, on the total uncertainty. This is important because the requirement for data quality (precision and resolution) actually depends on the filter performance.

They should conduct Monte-Carlo experiments (as in Kawamura et al., 2007) for evaluating (1). Noise level should be specified for creating pseudo data sets from the real Dome C data.

For evaluating (2), they should run several Monte-Carlo experiments through pseudo O_2/N_2 data sets created by scaled and resampled orbital curve at different mean resolution (with variability) added by a typical noise.

For evaluating (3), they should run Monte-Carlo experiments through a pseudo data sets created by scaling and resampling summer solstice insolation at the same resolutions and noise levels as done by Kawamura et al. for Vostok and Dome F. They can then compare the two-sigma variability for each peak with that of the Kawamura et al. which is available from the authors.

Second problem is on the filter. There are concerns that changes in signal amplitude from one precessional cycle to the next may displace O_2/N_2 peak with their filter (it may be enhanced with the presence of noise). A test of the filter for addressing this issue is to again running Monte-Carlo using resampled insolation curve around 300-500 kyr where precessional amplitude shows a lot of variability. The results (mean and s.d. of peak displacement) for each precessional peak should be discussed in the manuscript.

Third problem is that the choice of one orbital curve (mean insolation over Dec. 21 - Mar. 21) has absolutely no physical backing. It does not make any contribution to the stated objective "to test if whether or not, the Dome C O_2/N_2 depends on local 21st December insolation only". They should delete this orbital curve from the manuscript. Their finding of seasonal lag of temperature behind seasonal insolation is interesting, then it may be reasonable to bring up insolation (or mean insolation) around early January as a possible alternative tuning target. Note however that there is an underlying assumption to use such a curve that seasonal surface temperature dominates firn metamorphism related to O_2/N_2 fractionation. As pointed out by reviewer 3 (also see suppl. info. of Kawamura et al., 2007), sunshine also directly provides energy to surface snow grains, whose effect is largest at the solstice.

The results of the error analyses should be quantitatively presented and discussed in the manuscript.

Related to this issue, the manuscript suggests strongly without showing evidence that there could be fundamental mismatch in timing of O_2/N_2 relative to summer insolation by up to several 1000 years. The argument comes from Hutterli et al.'s theoretical modeling of vapor transport and the hypothesis that the total vapor transport in shallow firn driven by temperature gradient (tTGM) has the sole responsibility in determining the magnitude of O_2/N_2 fractionation. They indeed suggest significant mismatch between O_2/N_2 and local summer insolation. However, they actually found only one period in the last 340 kyr (around 130 kyr BP) where their modeled tTGM show significantly younger peak than that in summer insolation (differences for other periods are within O2/N2 tuning uncertainty).

The current Dome C record is of insufficient quality to add further information on the EDC3 dating error, so it is important to make it clear that there is currently no evidence for large phase shift in the published Antarctic records w.r.t. summer solstice insolation.

(For second-revised manuscript, which is the same as the manuscript appeared in CPD)
The authors failed again to respond to the review comments point by point. They should understand and react to all review comments if they wish to publish the data as a paper. Below are a few important points.

First in their reply, the authors claim that they followed the suggestions in the first review regarding evaluation of the data and filter, and then claim that the reviewer was instead inconsistent in the second round. This is wrong. They failed in the first revision to incorporate statistical approach (so-called Monte Carlo) requested in the first revision. The authors failed to read (or understand) the relevant literature, thus my detailed comment in the second review to have them understood. But even in the second revision, they did not take into account the variability of the sampling intervals (they only resampled at regular intervals). These were pointed out already in the first review by citing a paper, which method they could easily follow. The authors' claim that it is not important for this study to evaluate the effects of data and mathematical tool on age uncertainty is not acceptable. Regardless of the focus and conclusion of this manuscript, the method must be tested and clearly presented especially if it is the first appearance. It is important also partly because it may be reused in their future dating exercises.

In the error evaluation with noise-added pseudo data, they somehow made median filtered curve and 1 or 2 sigma limits. It is impossible to judge if this approach is reasonable because it is not explained at all, but I imagine that they re-sampled the 1000 filtered curves at fixed time intervals to take statistics at each time slice, and then connected the median O2/N2 values and 1 or 2 sigma percentiles (Fig 1 and 2 in the reply). However, such curves cannot be used for estimating age error. If these curves connect points along fixed percentiles with respect to variability in y-axis, all the curves should look similar (thus also similar in peak timings), so their claimed error is probably underestimation. They should indeed calculate the age distribution of each peak directly from the 1000 filtered curves. Again, authors failed to understand the literature and did a wrong calculation. The question about the effect of data quality on the total uncertainty has also been raised by other reviewers, but the authors are still not able to answer.

For fair discussion on the link and phase between insolation and O2/N2, they should also cite another paper on the topic (Fujita et al., 2009, JGR), which proposed a different mechanism to link summer insolation with gas fractionation and total gas content, which supports in-phase relationship between insolation and O2/N2 signal at Dome F. Also, while citing Hutterli et al.'s paper, the authors should also cite the criticism attached at the end of that paper by Fujita (a reviewer). They should also mention that the temperature-gradient-metamorphism hypothesis by Hutterli et al. actually produced only one instance in the last 340 kyr (at the penultimate glacial-interglacial transition) where the model result is inconsistent with the O2/N2 chronology by several thousand years, and that the phase variability vanishes if the accumulation rate is slightly reduced (within error) for their model calculation.