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Interactive comment on "The SP19 chronology for the South Pole Ice Core - Part 2: gas chronology, ∆age, and smoothing of atmospheric records" *by* Jenna A. Epifanio et al.

Anonymous Referee #2

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General comments:
This short preprint presents a gas chronology for the South Pole 2014 ice core covering 54,000 years. It is based on a high resolution discrete methane record synchronized with the WAIS Divide methane record. In my understanding, both the methane record and the new chronology are new and important carefully built datasets that deserve to be published fast. The minor comments below intend to improve the manuscript mostly by providing more details on some aspects.

Specific comments:

p2 l8-10 - the study by Lee et al. (2020) about the impact of high/low impurity levels on methane records could be cited here.
p2 l14-16 and p6 l20-21 - this presentation is somewhat too simplistic, for example LIDIE estimates in AICC2012 use constraints from δ^{15}N data (Veres et al., 2013; Bazin et al., 2013), and direct constraints on ∆age exist at least in Greenland (e.g. Severinghaus et al., Nature, 1998)
p2 l26-28 - it would be useful to summarize how the WD gas chronology was estimated
p3 l6-8 - the intercalibration results and replicate measurements could be provided in the Supplement, illustrated by a figure and commented with more details
p3 l8 (poor sample quality) sample quality issues could be commented, with for example the brittle zone extent and ice quality.
p3 l36 - p4 l1 - an important blank correction (35ppb) due to CH_4 outgassing from stainless steel flasks is applied to PSU data, potentially affecting the measurement precision. I believe that the OSU and PSU data series should be provided individually in the Supplement and the intercomparison better described (see also comment on p3 l6-8).
p5 l19-27 - could the model parameters (500-year window, one by one tie point testing) influence the results? The glacial period results seem robust on Figure 1, the possibility of matching the wrong event seems less easy to exclude for the Holocene small events. This could be commented.
p6 l23-24 - some densification models use other input parameters such as dust (Freitag et al., 2013, Bréant et al., 2017) or wind (Keenan et al., 2020), this could be mentioned
p7 l5-8 - the ∆age results are interesting and should be commented more extensively in order to better complement the δ^{15}N based discussion in Winski et al. (2019). I think that the constraints provided by δ^{15}N and the results before 8kBP in Fig. 13 of Winski et al. (2019) should be further discussed here.
p7 l26-28 and Table 2 - the CH_4 peak near 1500 AD, well documented in Rhodes et al. (2016) in several ice cores, also constitutes good target for smoothing evaluation, it

seems to be recorded near 153m in the South Pole data, it could be included in Table 2 and Section 3.2

p8 l3-27 - the method used to evaluate the gas age distribution characteristics leaves the reader confused about the relevance of basing the evaluation on a Dome C distribution and how it is modified by the alpha parameter. A simpler approach based on tuning a lognormal distribution could be used (e.g. Köhler et al., 2011, Fourteau et al., 2020) and would have the advantage to allow for providing the age distribution parameters (2 values) and compare them with previous estimates at other sites. Moreover, Fourteau et al. (2020) provide a high resolution EDC record of the DO6 to DO9 events, and it appears that DO9 and the CH_4 peak between DO9 and DO8 would provide stronger constraints to evaluate the smoothing rate of the South Pole signal and its comparison with EDC.

p9 l4-6 - this brief mention of the centennial CH_4 variations throughout the Holocene is interesting and could be further illustrated and commented

Figure 4 - the results before 8kaBP where the predicted δ^{15}N differs from the data in Figure 13 of Winski et al. (2019) should also be shown and commented.

Technical Corrections:

p2 l2 - Souney et al. (2020) is not in the list of references

p6 l29-31 - the reason why WD has small Δage could be provided for non ice core specialist readers

p7 l1 - I did not understand what is meant by “is the first of its kind” as it derived from a stratigraphic matching to WD

p7 l4 - the densification model used should be introduced

p7 l17 - the diffusion of trace gases in air does not always stop at the LID (Buizert et al., 2012) and some low accumulation sites do not show δ^{15}N plateau (Witrant et al., 2012)

p7 l27 - SP14and -> SP14 and

p9 l21-25 - I suggest to also provide a more detailed dataset including the laboratory

where the measurement originates from, corrections applied and replicate measurements

Figure 7 - the SPC14 record is difficult to distinguish, using a brighter colour would likely help

Table 2 - providing the depth or gas age interval would help; some Holocene events could be analysed (for example the CH_4 peak near 1500 AD or the 8.2 kaBP minimum studied by Spahni et al., 2003)

References not cited in the manuscript:

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Witrant et al., A new multi-gas constrained model of trace gas non-homogeneous transport in firn: evaluation and behaviour at eleven polar sites, Atmos. Chem. Phys., 12, 11465–11483, https://doi.org/10.5194/acp-12-11465-2012, 2012

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