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# *Interactive comment on* "Version 2 of the EUMETSAT OSI SAF and ESA CCI Sea Ice Concentration Climate Data Records" *by* Thomas Lavergne et al.

**Anonymous Referee #1**

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Summary

This paper describes a new version of the OSISAF sea ice concentration product and the ESA sea ice CDR. The products are derived from passive microwave data. The new version includes several enhancements from the Version 1 OSISAF product. Comparisons with independent estimates show good agreement. The new version provides a consistent record of sea ice concentrations for the scientific community.

General Comment

The manuscript provides a thorough introduction of the new versions. The description of the algorithm and processing, including enhancements from Version is clear and



detailed. The initial evaluation results look reasonable and given that it builds on the previous version and thorough earlier validation, they are quite sufficient to provide high confidence in the quality of the product. The Level 4 filtered product is particularly beneficial for users who wish to have a "clean" concentration estimate and this is an excellent improvement from Version. I have only a few minor comments that the authors should address before publication.

(One further general comment: it would be helpful for readability to either indent new paragraphs and/or skip a line between paragraphs.)

Specific Comments (by page and line number):

P2, L9: while the albedo specifically depends on concentration, it is not only concentration: snow melt state and particularly melt ponds substantially affect albedo even for 100% concentration.

P4, L24-25: I find the (resp. XXXX) style awkward to read and somewhat confusing. I would just write out each in sequence rather than using parenthesis, but this may just be a preference by me.

P6, L1: "daily composited fields of SIC" – how is the compositing done? Is it simply drop-in-the-bucket?

P10, L12-20: What are the uncertainties in the NWP fields and the RTM? While the dynamical tiepoints and the double-difference approach may negate much of the influence, I do wonder how effective the correction is if the NWP data and/or the RTM have high uncertainties? This feeds into my next comment below.

P10, L28: the use of the NWP fields is novel and I like the physical approach. However, L is not reliable from NWP. Isn't L one of the largest if not generally the largest source of emission, at least over open water. So not being able to correct for that really limits the effectiveness of the NWP correction, doesn't it? The use of weather filters in the Level 4 fields eliminates this, which is good, but the quality of the Level 3 fields must

be limited, right?

P13, L17: It might be worth considering showing an example of the “ice curve”. I can generally visualize, but a figure would perhaps better illustrate it.

P13, L20-26: I’m not sure I understand Figure 5. It appears to show an increase in open water concentration near the ice edge due to the correction (e.g., in Barents Sea and Davis Strait regions). Is that correct? Wouldn’t that reduce the quality if the correction essentially added ice to open water regions?

P16, L3: This should be discussed further – why is the gridded land-spillover correction still needed after the swath correction? How much coastal contamination remains after the swath correction. If the swath correction is not sufficient on its own, is it worth doing – i.e., would the Cavalieri correction work just as well without the swath correction? I guess the basic question is whether there is a benefit to doing both corrections or is the Cavalieri correction just as good? If so, then why do the swath correction?

P16, L31: “basic isotropic schemes” is not very specific. Is it a bi-linear interpolation?

P19, L17-23: I can understand that the ERA-Interim fields are not as good earlier in the record and thus the correction for SMMR is not as good. However, there is a noticeable step-change between SMMR and SSMI in Figure 8. Did ERA-Interim undergo a step change in terms of data sources or other processing quality at the same time? If not, then it seems like it’s not ERA-Interim (or at least not only), but rather something else causing the step change. Perhaps it’s related to the change in frequency from 18 GHz for SMMR to 19.3 GHz for SSMI?

P21, L4: One thing not discussed is the potential impact of satellite crossing times on the retrievals. I assume the dynamic tiepoints should handle these discrepancies, but it might be worth mentioning.

P25, L12: Why not produce a 12.5 km or 10 km resolution AMSR-E and/or AMSR2 product, i.e., using the same channels (19, 37 GHz) as for SMMR-SSMI-SSMIS, but



obtaining a higher spatial resolution for the period of 2002-present? It seems like this would be more beneficial than at least the 25 km SICCI. I can see a benefit of using the 6V channel for the 50 km product, but that isn’t in the 25 km SICCI.

Minor Comments (by page and line number):

P3, L17: use “in” instead of “entering”

P3, L26: use “share” or “provide” instead of “keep”

P16, L26: use “contrasts with” instead of “is conversely to”

P24, L3: “aiming at most complete” to “aiming to produce the most complete daily maps possible”

P25, L11: use “allowed, e.g., consistent processing of SIC CDRs. . .”

P26, L18: use “on the order of. . .”

P27, L1-2: use “the impact that melting and melt-ponds have. . .”

P27, L10: use “could be investigated if selecting. . .”

P28, L24: use “aim to have the best temporal consistency. . .”

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