

Interactive comment on “A spurious jump in the satellite record: is Antarctic sea ice really expanding?” by I. Eisenman et al.

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We thank the reviewer for these thorough comments, which we have addressed as follows in the revised manuscript (reviewer comments in italics):

This paper points out an important undocumented change in the processing of satellite passive microwave data of sea ice that has a big effect on computed trends in sea-ice area and extent. This paper should be published and brought to the attention of the data-processing community so the problem can be properly addressed. The scientific community also needs to know about the discrepancy in Antarctic sea-ice trends revealed by this work.

In addition to the paper and the supplementary material, I have read four posted com-

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ments and one posted review of this paper. I agree with the central theme that runs through all five commentaries: the authors need to apply the v2-v1 offset (correction) to the v2 time series and calculate whether the resulting trend (1979-2013) is significant or not. Then they can decide whether the title of the paper is appropriate or not.

Indeed, this was a very helpful point raised in the online discussion. In the revised manuscript, we have included a discussion of the trends from the V2 ice extent time series with the V1-V2 offset removed (which we call “Bootstrap Version 1B”), and we have adjusted the title. See our response to Grant Foster for further details.

Page 276, first paragraph, “The IPCC AR5 reported...”. When giving numerical trends here, it would be helpful to say that the trends are with respect to anomalies from the mean annual cycle. When I first read this paragraph I was wondering whether the authors were discussing trends in September, or March, or some other season.

We have clarified this point based on this helpful suggestion. In the revised manuscript, we added to this sentence that the rate reported in the IPCC AR5 is for “monthly anomalies from the mean seasonal cycle”.

Page 278, line 8, “the trend in Version 2 often being more than 10x larger than in Version 1”. I think this is a misleading way to compare the trends. In Fig 1B, in about 2003 the trend in v1 drops to zero. So you could find end-points around this time when the trend in v2 is 1000x or a million x larger than the trend in v1. Similarly in 1999 when the v1 trend is slightly negative, the ratio of v2 trend to v1 trend approaches minus infinity. Neither of these are helpful characterizations of the relative trends. I would suggest deleting the phrase about 10x.

We have adopted this suggestion and deleted the phrase from the revised manuscript.

(1) Page 280, line 24: If v1 is correct then “Antarctic sea ice extent has remained nearly constant”. (2) Page 281, line 16: “the results of this analysis invite the speculation that the Antarctic sea ice cover is not expanding”. (3) Page 282, line 5: It’s possible that

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the apparent sea ice growth is spurious, “with the actual sea ice extent remaining approximately constant”. These three statements relate back to the central question raised by all commentators of this paper: if the v2 time series is adjusted according to the v2-v1 offset, is the trend significant or not? The authors are apparently saying “no”, but they have not presented the results of such a calculation.

All three statements have been adjusted in the revised manuscript to focus on the point that the trend may have been overestimated, rather than suggesting that the trend may be insignificant.

Section S1.1, end of first paragraph. Regarding the dates of the satellites and the dates when Bootstrap switches from one to the other: Bootstrap uses SSM/I data until 13 December 2007; Bootstrap uses SSMIS data starting on 1 August 2008. What about the time in between these dates?

We appreciate the reviewer taking the time to thoroughly examine the Supplement! This was a typo and has been corrected in the revised manuscript (SSM/I ends 31 Dec 2007; SSMIS starts 1 Jan 2008). We thank the reviewer for bringing this to our attention.

Section S2.1. What is the base period from which the mean annual cycle is calculated?

We assume this is referring to Fig. S2, which is referenced in Section S2.1. The figure caption mentioned “anomalies from the mean seasonal cycle” without specifying the base period. We appreciate the reviewer bringing this to our attention, and we have specified the base period, which is 1979-2004, in the Fig. S2 caption in the revised manuscript.

Section S2.1, end of second paragraph: “Figs. 2A,B,D,F” should be “Figs. S2A,B,D,F”.

We thank the reviewer for catching this typo, which we have corrected in the revised manuscript.

Comment about Arctic Sea Ice Extent and Trends

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This paper (actually the supplementary material) hints at two troubling aspects of the time series of Arctic sea ice derived from passive microwave data. First, look at Figure S3 B and D. The nature of the time series changes in 2007 (which is not a sensor transition). Before 2007, the anomalies are characterized by relatively high-frequency variability and small amplitude. After 2007, the anomalies have noticeably lower frequency and much larger amplitude. Look at Figure S6 A,C,D,F. Clearly something changed in 2007!

The onset of large-amplitude annual-frequency variability beginning in 2007 in Fig. S3B,D may be attributable to issues of coastline geometry causing the ice extent seasonal cycle amplitude to increase. We have added a brief discussion of this point to the revised manuscript supplement. The change in the trend for end periods before or after 2007 (Fig. S6 of original Discussion paper; Fig. S7 of revised manuscript) does not appear to be due to just this change in seasonal amplitude, however, as can be seen by considering the trend in annual-mean ice cover (Fig. S8 of revised manuscript).

Second, the authors point out in Section S2.2 (second-to-last paragraph) that Arctic sea ice anomaly trends computed from Bootstrap and NASA Team are not even close to one another. It has been well known for a long time that Bootstrap and NASA Team produce different values of sea ice concentration, but there has been perhaps an unstated assumption or hope that the trends should be similar. Now we see that they are not. These two aspects of the Arctic sea ice time series go beyond the scope of the present paper, and I am not suggesting that the authors need to expand upon what they have already done here. I just think that the Arctic sea ice time series needs a thorough investigation, as the authors have done for the Antarctic.

We appreciate this comment. We are currently looking into similar questions in the Arctic sea ice datasets, and we hope that the current paper may perhaps further encourage other groups to look into related questions as well.