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Interactive comment on “A record of Antarctic sea ice extent in the Southern Indian Ocean for the past 300 yr and its relationship with global mean temperature” by C. Xiao et al.

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Xiao et al. examine MS- from an Antarctic ice core for years 1708-2000 and suggest that it is a useful proxy of sea ice extent (SIE) in the sector 70E-100E of the Southern Ocean. They find that their reconstructed SIE varies more or less in step with global temperature until recent decades, when the relationship reverses (i.e., SIE increases despite warming). The manuscript is clearly written and well presented, and the results are interesting overall. In my opinion, the previous 3 reviewers have provided excellent discussions of some of the most salient points that could be improved in the manuscript. My review is late, and I offer it only in the spirit of helpfulness. The authors

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may choose to address some of the points outlined below.

(1) A central argument in this manuscript feels almost circular. The authors argue that MS- is a proxy of SIE primarily because the two correlate during the period of overlap. The correlation isn't very strong ($R^2=0.16$), and one can see in Fig. 3a that the SIE increases slightly during the much of the overlap period whereas the MS-proxy implies an ice cover decrease. Then the authors compare with an estimate of global temperature and say the temperature and SIE are normally in phase except during the past couple decades when SIE increases despite warming. But didn't the MS- proxy actually show a sea ice decrease during this period (Fig. 3a)? So the MS-proxy appears to vary in phase with temperature throughout the record, whereas the instrumental SIE varies out of phase with temperature. It's a little hard to accept that something unprecedented happened near the start of the instrumental record when no change is indicated in the proxy record, i.e., hard to believe that ice and temperature really varied in phase during the pre-instrumental record when the trend doesn't agree between proxy and instrumental records during the period of overlap.

(2) It would be useful if the authors would discuss, even briefly, how the accuracy of this proxy compares with other sea ice extent proxies (e.g., Wolff et al., 2006, Nature 440:491-496; the two Abram et al. papers referenced in the manuscript). This would basically be extending the discussion in Sec. 4 to compare the accuracy of different proxies rather than just comparing how a proxy for a somewhat different region compares with this proxy. Being somewhat unfamiliar with this literature, a correlation with observations of just $R^2=0.16$ does not sound very good to me, but it would be interesting to hear how it stacks up against other sea ice cover proxies.

(3) It would be useful to say a little more about the observational data. Which NSIDC record was used for 1996-2000, Bootstrap or NASA Team? Why was the JIC record chosen rather than the NSIDC record for the earlier period (1973-1996), and would the results change much if the NSIDC record was instead used throughout? How reliable is the pre-SMMR JIC data (1973-1978), and what is it based on? Also, did the authors

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use gridded sea ice concentrations, apply a 15% threshold as is often done for sea ice extent, and then average only over 70-100E? Or did they follow a different procedure to get observed SIE? It would be useful to have this specified.

(4) I found it a little bit confusing that the term "sea ice extent" was used for the ice coverage in 70-100E, rather than for the entire hemisphere as in the more common usage of the term. This was exacerbated by the acronym "SIE" apparently being used sometimes for total SH sea ice cover (page 3612 lines 15 and 20-21) and other times for sea ice cover in the 70-100E sector. This comment could be addressed simply by using "SIE" only to refer to the sea ice cover in the 70-100E sector and adding a few words to this effect the first time the term is defined (the hemispheric ice extent could, for example, just be called "sea ice cover").

(5) The use of NH temperature records to estimate global temperature swings seems problematic to me. The manuscript mentions (page 3615 line 15) that "current average trends are quiet similar between the hemispheres", but I'd expect greenhouse-induced global warming trends to be more hemispherically uniform than natural variability.

(6) Presumably the yellow SIE curve in Fig. 3a is for the sector 70-100E. It'd be nice if this were indicated in the figure caption.

(7) A very minor suggestion, but I think Fig. 1 would be more clear if a couple lines of constant latitude & longitude were included.

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