The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-51-AC2, 2017

The Cryosphere
Discussions

Interactive comment on "Brief communication: Changing mid-twentieth century Antarctic sea ice variability linked to tropical forcing" *by* Chris S. M. Turney et al.

Chris S. M. Turney et al.

c.turney@unsw.edu.au

Received and published: 19 July 2017

Anonymous Referee #1

General comments: The precise goal of this manuscript is not clear. In particular it is not clear what are the time scales of interest to the author. The title says that there is a link between tropical forcing and Antarctic sea ice variability. In the introduction and in the conclusion multi-decadal to centenial variability seem to be the interest because trends of sea ice extent over multiple decades are discussed. On the other hand most of the manuscript seem to be about inter-annual variability using seasonally averaged detrended ERA-interim surface pressure and HadSST Nino3.4 to show linear correla-

Printer-friendly version

Discussion paper

tion between different regions. The physics of short and long time scales is not the same for example Ferreira et al. 2015 show that the same wind anomaly produces opposite effect on sea ice extent at short and long time scales. Therefore my recommendation is that the authors make clear what they are trying to show and align their demonstration accordingly. If the focus is the inter-annual variability of sea ice then the conclusions cannot be used to explain the multi-decadal trend observed during the satellite period. If the focus is on the multi-decadal trend of sea ice extend then a detrended Nino3.4 index doesn't seem to be a good index to use because the time scale of El-Nino is 2 to 7 years. Maybe the Interdecadal Pacific Oscillation would be a better index (see Meehl et al. 2015). >We do agree the introduction requires some clarification given the range of timescales and the potential opposing mechanisms operating across the region. The complex nature of the processes is summarised by Ferreira et al who describe the surface expression of relatively warm deep water after some 20–25 years of enhanced westerly airflow, overriding the chilling effect of surface waters associated with enhanced northward Ekman drift. Importantly, the potential contribution of sub-surface melting described by Bintanja et al. suggests that sub-surface melting may be further contributing to sea ice expansion off the George V coast, delaying the surface expression of warmer waters postulated by Ferreira et al. Whilst we cannot definitively resolve these issues our study aims to provide a long-term perspective from the mid-nineteenth century assuming the short-term Ekman processes dominate. We have therefore reframed the Introduction to make this more explicit. Whilst we sympathise with the referee's comment regarding multidecadal variability, our recent work on climate changes in the southwest Pacific (Turney et al., 2017, Climate of the Past) suggests that interannual variability is the most significant periodicity over the past 140 years.

Specific comments: Whatever the time scale chosen by the authors for this work, the choice to correlate first Nino3.4 to SWP and then SWP to sea ice extent is not sufficient. If the hypothesis is that there is a relation between the tropical Pacific and sea ice then these two variables should be correlated directly. >The reviewer is correct and we have now undertaken direct correlations. Our analyses have refined the timing and processes involved. From more detailed study we find the highest correlation between the region of marked sea ice expansion off the George V coast and tropical Pacific is centred on during the late spring-early summer during the periodin October-December (rather than September-November as we originally reported). The table below summarises the correlations for both Nino 3.4 and Nino 3 (using HadISST) using detrended and deseasonalised data, taking year-on-year differences (p values in parentheses; significant values p<0.1 is marked by *).

Nino 3 September-November 0.12 (0.47) October-December 0.31 (0.06)* November-January 0.31 (0.05)* December-February 0.18 (0.27)*

Nino 3.4 September-November 0.14 (0.42) October-December 0.30 (0.07)* November-January 0.28 (0.09)* December-February 0.15 (0.39)

Regardless of the parameter used, central tropical sea surface temperatures appear to have a direct relationship with sea ice off George V Land coast, through modulating the pressure anomaly in the southwest Pacific. As a result, the figures have been revised to incorporate the October-December period, although the overall pattern of responses is broadly the same as September-November used in the previous submission. For instance, the revised Figure 2 is shown below (with modified caption). Our analyses support an increasing body of literature that the central tropical Pacific modulates atmospheric circulation across large parts of the high latitudes and plays a direct role on sea ice off the George V coast across October-December (e.g. Wilson et al., 2014). Our work is supported by further analyses described below that suggests this teleconnection is indeed via Rossby wave penetration of the high latitudes.

Figure 2: (a) Spatial correlation between deseasonalised and detrended HadISST Nino 3.4 region sea surface temperatures (SSTs) and mean sea level pressure (October-December) derived from ERA-Interim (Dee et al., 2011) (1979-2016). Note, the schematic black arrowheads show the Rossby wave train that extends over the south-

west Pacific (SWP; 50ËŽ-60ËŽS, 160-180ËŽE) into the Amundsen Sea (AS; 65ËŽ-55ËŽS, 95-135ËŽW). (b-d) Spatial correlations between SWP pressure anomaly, and zonal (b) and meridional (c) wind stress at 10 m, and sea ice concentration (National Snow and Ice Data Center; https://nsidc.org/data) (d). Hatched areas denote significant at 95% confidence level.

To investigate the relation between equatorial Pacific and sea ice I would strongly recommend to use a climate model without data assimilation in which the temperature is changed in the equatorial Pacific and the response is analysed. Using a climate model in which surface temperature is assimilated and showing that the behaviour is qualitatively similar to geopotential height from reanalysis and satellite observations of sea ice extent seems useless. >We appreciate the referee's comments but this really does depend on what you are trying to test. We have now clarified the Introduction so that we describe the need to develop long-term records that extend beyond the satellite era (post-1979). Here we wished to explore whether a climate model forced by observations matches the changes in sea ice inferred from the recently reported climate reconstruction from the subantarctic southwest Pacific (Turney et al., 2017, Climate of the Past). We hope this sounds alright.

The abstract says the results of this manuscript provide new insight in Antarctic ocean circulation but this is not the case. There is no discussion about ocean circulation in the manuscript. >We have clarified the text to make it clear we are referring to Ekman processes. We apologise for the confusion.

Details: Fig.2a: The unit of "yr-1" is not very readable, maybe % per decade would be clearer >This is a fair point. Referee #2 also raised this issue but for all figures. We have increased the font size as requested.

Additional Reference Wilson, A.B., Bromwich, D.H., Hines, K.M., Wang, S.-h., 2014. El Niño flavors and their simulated impacts on atmospheric circulation in the high southern latitudes. Journal of Climate 27, 8934-8955.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-51, 2017.

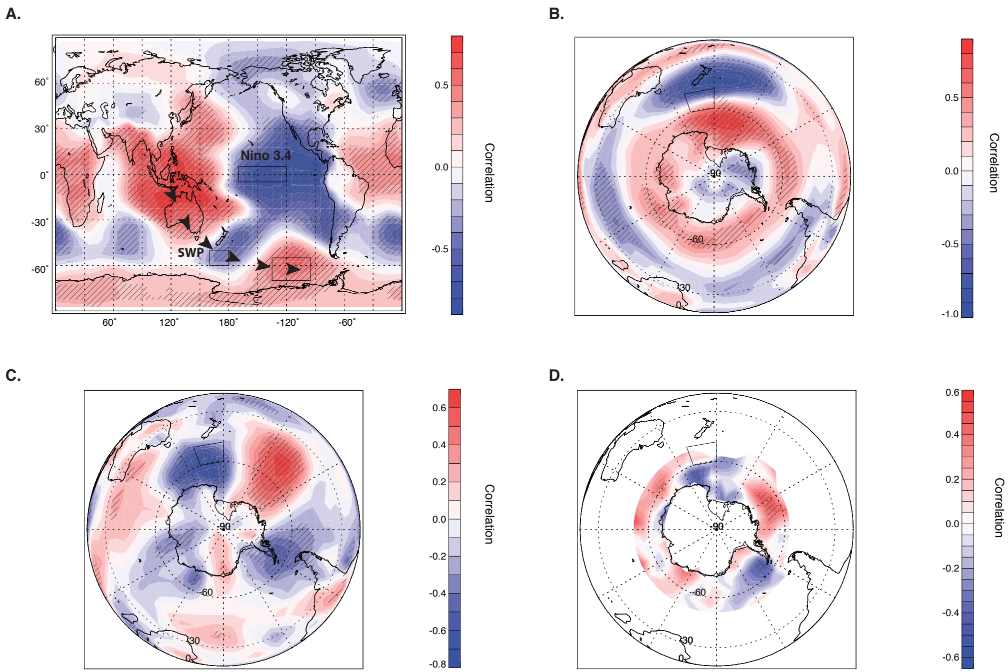
A.
Nino 3.4
SWP
Correlation
B.
Correlation
C.
Correlation
D.
Correlation

Fig. 1.