Clim. Past Discuss.,
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Climate of the Past Discussions

Interactive comment on "Can we reconstruct the formation of large open ocean polynyas in the Southern Ocean using ice core records?" *by* Hugues Goosse et al.

Anonymous Referee #1

Received and published: 14 September 2020

This paper is an interesting and relevant contribution to the discussion on whether large open-ocean polynyas in the Weddell Sea have occurred before the mid 1970s (the first and only such event in the instrumental (satellite-derived) record), and if yes, how regularly and with what intensity (size). According to the authors, sediment cores were so far useless for this purpose, while records from ice cores and Antarctic weather stations provide apparently more insight on this topic. With the further aid of atmosphere models that are being constrained at their surface by sea-ice concentration and thus sea-surface temperature, the authors make an attempt to connect phases of higher snow accumulation in ice cores "downwind of polynyas" with warm anomalies over the Weddell Sea, thereby projecting possible polynya occurrences over the past millen-

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nium. The presented strategy and results are subject to substantial uncertainties. This has been clearly expressed throughout the paper. While the results thus need to be taken with caution, the methods and implications are nevertheless sound and worth publishing. Before doing so the authors may want to consider the following.

Main points:

⇒ In general, I think the text is too long for what is being presented. As an example, the Introduction, while providing a nice overview over the literature on Weddell Sea polynya formation, appears too long considering that the main thrust of this paper is polynya reconstruction from ice cores, and not the mechanism of polynya formation.

⇒ Section 2.3 is very technical and rather confusing (at least to me). I think a reader would get more out of it if the main steps of the procedure were displayed in a diagram.

⇒ At several occasions the authors mention (anomalous snow accumulation) "downwind of the polynya". Weijer et al. (2017; their Fig. 6) come up with an estimate of precipitation actually "downwind of a polynya" based on a high-resolution (0.10 degree sea and ocean; 0.25 degree atmosphere and land) CESM simulation (Small et al., 2014). While "just" a model result, if at all, land sees higher precipitation rates only when winds blow from northerly directions (NE or NW, N not shown). While your statement is thus supported by these simulation results, you make apparently no attempt to relate snow accumulation on land to wind direction. Is there any reason for why you do not take into account wind direction from ECHAM5-wiso or SPEAR in your reconstruction, or did I miss something?

⇒ The ice cores located around the Greenwich meridian at about 75S at altitudes higher than 2600 m (Fig.3a) do not seem to be impacted by any of the anomalies and regressions you are showing. There is also no physical explanation on how snow accumulation at such high altitudes and some 500 km inland could be affected by open-ocean polynyas. Including these ice cores in your reconstruction need a more convincing justification than just being in the (relative) geographic vicinity of potential

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polynyas.

More detailed, line-by-line comments:

Line 23: Add “snow” before “accumulation.

Line 29: Coastal polynyas are additionally surrounded by land or ice shelves.

Lines 109 and 116: The two “Stössel et al.” citations should be swapped.

Line 123: It seems more appropriate to replace “suggested” by “speculated”. BTW: see also last paragraph of Kurtakoti et al. (2018) on this topic.

Line 127: “longer that” -> “that is longer than”.

Lines 131-133: Awkward and too long a sentence. Polynyas may have an influence on the continent regardless of whether there is paleoclimate data for the specific ocean region available or not.

Line 157 and later: “associated to” -> “associated with”.

Line 168: “dating error . . . maximum of a few years” doesn’t sound very promising for reconstructing polynyas that last for only 2-3 years.

Lines 179-183: “A part of the trend could be due to a recent shift in polynya activity”; you could check that by reducing the time series to 1850-1980. How does the trend in general look like? Is there no trend before 1850? Why should removing the trend change the frequency of polynya occurrence?

Line 201: “They have constant forcing”; what forcing? Atmospheric CO2? Pre-industrial?

Line 202: “provide” -> “simulate”; insert “polynya” before “events”.

Line 204: What does “model prior” mean? What does “their” refer to?

Line 209: The status of “Zhang et al., 2020” is submitted, so not accessible. So the

“differences between the two simulations” need to be described.

Fig.2: Why do you show annual-mean values rather than winter-mean or winter half-year values? In this region, polynyas do not exist in summer, and they exert a significant impact on the atmosphere only in winter. Wouldn’t the explanation given in lines 269-270 be a good reason to just consider winter months?

Lines 277-278: “as for temperature . . . than the one in 1995”; what is this referring to? Fig.3b shows SMB, not temperature, and in Fig.2b, the temperature in the late 1970s is clearly the warmest of the shown record.

Line 301: Insert “-ocean” before “polynya”.

Some suggested rewrite: Line 317: Insert “and defining” behind “calculating”. Line 318: Insert “index” behind “S”. Insert “-ocean” behind “Open”. Line 321: “with this index” -> “onto the above specified mixed-layer depth index”. Line 324: Insert “mixed-layer depth” before “index”. Line 326: Insert “mixed-layer depth” before “index”, and remove “based on the mixed layer depth”.

Lines 333-334: “large warming and precipitation changes”; none occur at the high elevation ice cores along 75S around the Greenwich meridian shown in Fig.3a.

Lines 361-362: “a large fraction . . . higher than the mean”; Fig.3a shows 7 core sites, 4 of which with SMB values lower than the mean.

Line 390: “have preferred” -> “decided”.

Lines 394-395: This sentence raises the concern that the uncertainties may make your conclusions obsolete.

Line 404: “show a clear maximum in 1975”; they also show a maximum in 1983 when there was no polynya.

Line 412: “downwind from the polynya”; why is this variable (wind) not considered in your reconstruction?

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Line 447: Insert “atmosphere” before “model”.

Line 450: “ice cores can be used” -> “it is tempting to use”.

Line 453: “downwind”: this has not been shown.

Line 458: “simple average” of what?; “data assimilation”: what data has been assimilated?

Line 462: “of the index”: what index?

Line 464: Add “Criscitiello et al., 2013”; see reference list in Ethan Campbell’s comments.

Line 465: What does “these” refer to?

Lines 470-471: Or much larger polynyas, or indeed ice embayments in the Weddell Sea, as often simulated (see e.g. Cheon et al., 2014; Kurtakoti et al., 2018).

Line 473: What does “few” mean? 2-3 times?

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