Comments from reviewer 2 (in red) and answers (in black)

"A strong link between variations in sea-ice extent and global atmospheric pressure" by Le Mouël et al applies the singular spectrum analysis (SSA) method to both Arctic and Antarctic sea ice and sea-level pressure (SLP) time series to identify and compare common sets of harmonics between the respective time series. Further, temporal comparisons are made between sub-annual to multidecadal harmonics, and those of longer periodicity in the ice cover and SLP data are related to astronomical and astrophysical forcing cycles. A researcher with expertise in such cycles would be better equipped to evaluate and offer an opinion on the validity of such arguments in the context of both past and modern cryospheric change. However, that said, it is not apparent what the key, novel findings are from the study. My additional comments herein mainly encompass data and methodology concerns.

Seasonal change has a clear impact on ice growth and melt, but how climate change and related oceanic and atmospheric warming of the last two plus decades factor into the interpretation of these results is unclear.

As can be seen in Figures 04 to 09, for instance Figure 05, the forced (annual) oscillations are quite stable, both in amplitude and phase, from the end of the '70s to the Present. This stability is expected (*cf.* Lambeck, 2005, chapter 7, page 146, "*Seasonal variations*") since motion of the rotation pole (governed by the Liouville-Euler equations) involves periodic climatic excitations functions (as far as the annual period and its harmonics are concerned). The re-organization of masses at the Earth's surface and the annual oscillation are linearly related by this system of equations. We show in Figures A01a and A01b (attached to this reply) the annual components of the two coordinates of the rotation pole since 1846 (from Lopes et al., 2021). We see that modulation of the amplitudes is quite small and so is the phase difference between the two coordinates. This in itself is not a new result (it can be found in Figure 5.13, page 97 of Lambeck, 2005). The reviewer is right in that, contrary to the trends of our Figure 03, the effect of climate warming on this oscillation is not clear. But we believe that some of these results are interesting, in that they allow us to advance on the physical nature of the oscillating excitation functions that are generally included in the Liouville-Euler system of equations (chapter 4, page 47, Lambeck, 2005). Our main findings have to do with understanding and explicitly formulating the excitation functions in the Liouville-Euler equations. At the same time, we use the data to suggest that a physical mechanism could be found in the Taylor-Couette flow on a sphere (evidenced here through the harmonic suite of SSA components at 1 year, 1/2, 1/3, 1/4 and 1/5 yr).

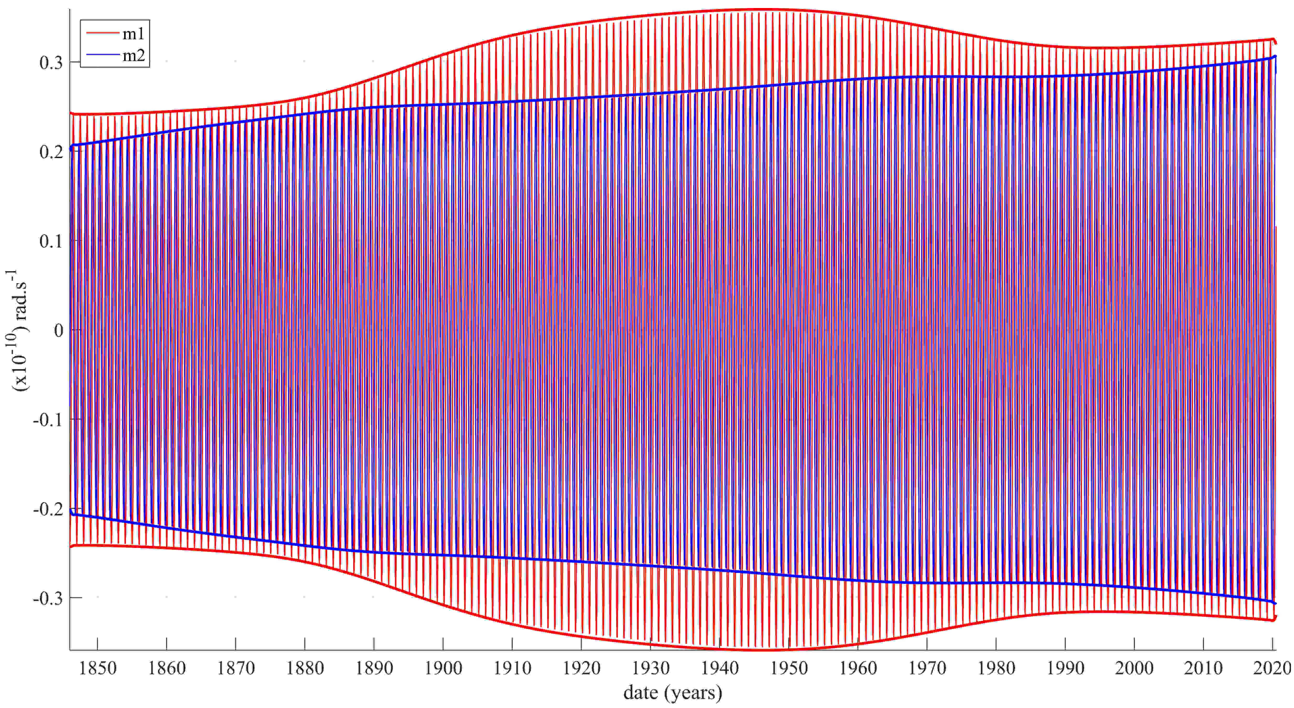

Figure A01a: Annual oscillations of the coordinates of the rotation pole (from Lopes et al., 2021)

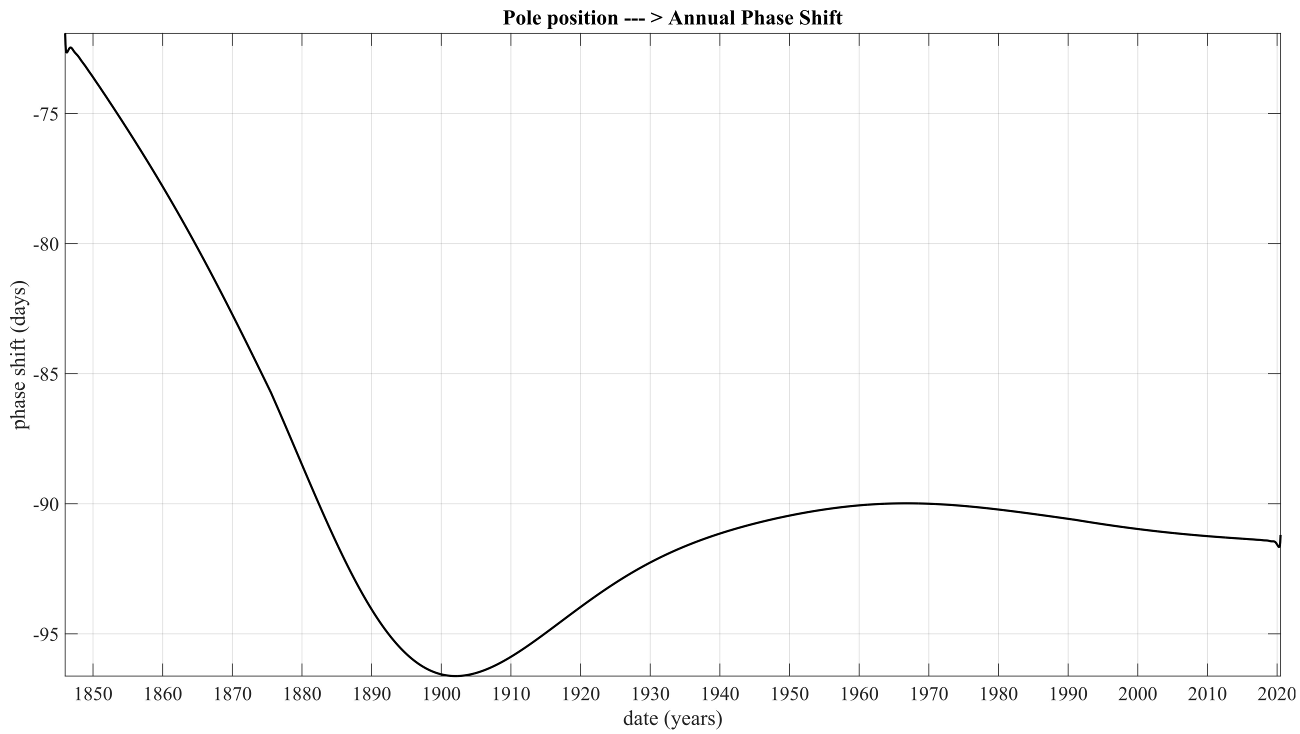

Figure A01b: Phase difference between the annual components of the two coordinates of the rotation pole (from Lopes et al. 2021)

Further, to provide longer-term context to the results, the conclusions attempt to offer some insights between ice cover and SLP beyond the satellite era. This is problematic due to sparse data over the polar oceans (especially Southern Ocean) until the 1950s and thus likely impacts confidence in the sea ice and SLP periodicities calculated over that period, though no error estimates are provided accounting for this shortcoming. Below I outline more detailed concerns along these and editorial lines.

Indeed, as recalled above, there is a link between the re-organization of masses at the Earth surface and the mean pole of rotation. This link being a system of linear differential equations, it is legitimate to try (in the absence of data for sea ice) to move back in time using existing data. Moreover, since here we are interested in oscillations with periods that are short compared to the time range of data, it is legitimate to predict (forward and backward) mechanisms that link them. In summary, the Liouville-Euler equations link the motion of the rotation pole and the mass re-organizations at the Earth surface. These linear partial differential equations (functions of time) have been used extensively (e.g. Nakiboglu and Lambeck, 1980; Zotov et al., 2017). They provide a widely accepted physical model, which allows one to propose more general conclusions even with incomplete data (see below), given that they be of short period compared to the data interval.

Regarding the impact on the components with annual and sub-annual periods that we extract: The SSA method (*eg.* Vautard et Ghil, 1989; Golyandina & Zhigljavsky, 2013) allows one to extract components with annual and sub-annual periods; we have checked and estimated more precisely these periods with Fourier transforms. They are always accompanied by an associated uncertainty (*cf.* Table 01). Since we discuss periodicities that are at least 42 times shorter than the time range of the data (42 years) we are certain to comply with Shannon's criterion.

Besides, the sea ice time series appears to be stationary (stricto sensu) which is a necessary condition to obtain a good Fourier transform (a bit more than two cycles would have been sufficient to determine the annual oscillation). Following Claerbout (1976), 8 to 10 cycles would have been sufficient in the case of a noisy stationary (senso latu) signal. We have 42!

The reviewer writes that we do not provide uncertainties or error estimates. As can be seen in Table 01 such is not the case. It would certainly have been even better if the data themselves (time series) had error estimates but such is unfortunately not the case.

Specific Comments

1) To reiterate, seasonal temperature and pressure changes due to annual earth-sun relations certainly impact the annual cycle of ice growth and melt and presence and strength of climatological pressure features. From SSA applied to sea ice and pressure time series, we would expect related "cycles" to emerge at seasonal and annual scales through time. The rates of Earth's air/ocean temperature changes, however, are not nearly as consistent through time.

We agree with this point of principle. But on this time scale our analysis does not show any such variation. Let us recall that, regardless of the origins of the annual forcing, any modification (in phase or amplitude) in the forcing must have an effect on polar motion (*cf.* Liouville-Euler). Yet, as shown in Figures A01a and A01b, such does not seem to be the case. Ice does not form or melt instantaneously, but integrates environmental conditions (including geophysical phenomena) over time. The published literature reflects an ongoing debate regarding the climate and global warming, in particular SLP: the rise in temperature must either affect the great atmospheric convection cells (Hadley, Ferrel, etc …) or increase the number of extreme events (*e.g.* Chang , 1995; Dima and Wallace, 2003; Frierson et al., 2007; Hu and Fu, 2007; Kharin et al., 2007; Lu et al., 2007; Tandon et al., 2013; Shepherd, 2014; Tao et al., 2016; Grise and Davis, 2020, Schaeffer et al., 2005; Stott et al., 2010; Rahmstorf and Coumou, 2011; Rummukainen , 2012; Trenberth et al., 2015). As far as we are concerned, only the (SSA) trends seem to undergo a strong amplitude modulation. This is more in line with an impact on the longer periods rather than on periodicities as short as annual (*cf.* Nakiboglu & Lambeck, 1980). However, this is not the purpose of the present study.

Is SSA an appropriate methodology to measure such evolving and covarying sea ice and SLP change?

In principle, none of the methods of spectral analysis (be they Fourier transforms (FT), wavelet transforms (WT) or SSA) is better than another one in an absolute sense. Again in principle, the FT or WT can both be pushed to high orders to match the results we obtain with SSA. As an example from our own group, results on polar motion obtained with wavelets by Gibert et al. (1998) are identical with those of Lopes et al. (2021) using SSA. The SSA method is particularly well explained in Golyandina & Zhigljavsky (2013); it originates from the scientific fields of climate and paleoclimate (before being put on a complete mathematical basis, note that the Cooley-Tuckey method for Fourier transforms or the wavelet transform were "invented" by geophysicists). Here is an incomplete list of (productive) applications of SSA: Vautard and Ghil (1989), Vautard et al. (1992), Zhaomin & Shisong (1996), Jevrejeva and Moore (2001), Kravtsov and Ghil (2004), Darby et al. (2017), Pepelyshev & Zhigljavsky (2017). All of these papers deal with climate, sea ice, and/or temperature: it is because of these successful studies that we have selected to apply SSA in this paper.

How does global climate change and related oceanic and atmospheric warming exacerbated at both poles during at least the last two decades (i.e., Arctic amplification) factor into the interpretation of your results and the purported astronomical and astrophysical forcings linked with non-stationary sea ice and perhaps SLP behaviors?

As explained in our section 5.3 on mechanisms, our aim is to try to understand and to explicitly formulate the excitation functions in the Liouville-Euler equations. At the same time, we use the data to suggest that a physical mechanism could be found in the Taylor-Couette flow on a sphere, which we believe is an important problem; moreover, it is evidenced here through the harmonic suite of SSA components at 1 year, 1/2, 1/3, 1/4 and 1/5 yr. As stated above, we do not know whether global warming impacts the annual oscillations; there is more information in the trends of Figure 03 that vary a lot. We have confirmed a result that has been obtained previously by others, clearly seen in the raw data of Figure 01: the annual variations have remained stable for 42 years and have not been affected by global warming on this time scale. If they had been, we would not have found the harmonic suite of components.

2) For satellite-era comparisons against sea ice variability, why use the coarse resolution HadSLP2 and not a newer, higher spatiotemporal resolution product such ERA5? There is quite a difference in spatial resolution between these two products and ERA5 assimilates lots of new data sources. At minimum, more justification for HadSLP2 over a newer product like ERA5 needs to be provided. The data quality/quantity issue further plays into the longer-term interpretation of results mentioned in the following comment.

For two reasons. We are interested in understanding global scale phenomena such as polar motion. Once again, there is a link (through the Liouville-Euler equations) between re-organization of surface masses and polar rotation. The latter is a scalar, available with a rough sampling of 1 month since 1840. For coherency, all our analyses are performed on data/time series with the same (rough) sampling. The second reason is that the data set HadSLP2 is to our knowledge the most widely used, studied and published. We were not aware of the ERA5 data set and thank the reviewer for pointing it out to us. We certainly intend to use it in a future study.

3) In providing long-term context to the core study results, the conclusions need to be modified. Meteorological data including surface pressure is very sparse for the Southern Ocean and Antarctica, especially prior to the IGY (~1957-1958). This data quantity issue is recognized in the conclusions of Allan and Ansell (2006), which provides an overview of the HadSLP2 dataset used in the paper. Further, many gridded products have questionable data quality with a scarce number of observations included before the first half of the twentieth century (Fogt et al., 2018 J. Climate). How sparse are Southern Ocean data observations comprising the HadSLP product pre-dating the satellite era, let alone during this era from which the main results are built? Much like the passive microwave ice cover record, a description of the HadSLP dataset construction and available observations through the pressure record need to be discussed and results emphasized for periods when the data quality/quantity are most robust. These dataset issues need to be kept in perspective when interpreting the results back beyond the IGY and to the 1840s.

We agree with the reviewer and remove Figure 11 and all comments linked to it.

Technical Corrections

Some editorial remarks and clarifications are listed by line number (L):

L17: "It fits topographic forcing." – what does? Please clarify.

This anomaly is seen in maps of correlations of variations in sea-ice extent with atmospheric pressure, surface temperature and winds. It fits the regional topography.

L44: Change "identifications" to "identification"

OK done

L48: Spell out the climate indices (e.g., AO, AAO) where first introduced.

Arctic Oscillation, Antarctic Oscillation.

L51-52: The AO is commonly a statistical solution based on a univariate geopotential height field (e.g., NOAA CPC uses the 1000 hPa GPH field). Please clarify this description.

Aren't AO and AAO EOFs rather than statistical solutions? Do we need to restate the definition of AO and AAO?

L63: Remove "lod"

OK done

L67: This sentence is confusing. Please re-write to clarify its intent.

We have applied SSA to study the variations and oscillatory components of a number of solar, climatological and geophysical phenomena, parameters and proxies (see the above references), but not yet atmospheric pressure (AP).

L84: "quoting Cavalieri et al" with what? Methods? Please clarify what is meant here.

After 2010 and up to 2018, we refer to Parkinson (2019).

L116: The Allan and Ansell paper was published in 2006 not 2004.

OK corrected.

L120: In addition to specific comments above, references to previous studies that have used the data for polar research would help support the statement.

We found it very difficult to check all aspects of this complex procedure and prefer, as in most of our previous studies of similar series, to trust the available data.

Figure 2 (and others): Y axis labels referencing surface pressure should consistently list “hPa.” Check that these pressure units are consistently referenced through the paper.

hP changed to hPa on the Oy axis of Figures 2,3,4,6,7,8.

L297: Reference and description of the earth-sun distance data should be provided in the data section.

OK done

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