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Earth System Dynamics
Discussions

Interactive comment on "Quantifying interdecadal changes in large-scale patterns of surface air temperature variability" *by* Dario A. Zappalà et al.

Anonymous Referee #1

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Comments on the manuscript entitled "Quantifying interdecadal changes in large-scale patterns of surface air temperature variability" by Zappalà et al.

The authors try to quantify interdecadal changes in large-scale patterns of surface air temperature (SAT) variability using the Hilbert transform. The authors obtain instantaneous amplitude and frequency of a time series by applying Hilbert transform to the daily SAT data. Afterwards the instantaneous amplitude and frequency are averaged and compared to quantify the differences in SAT amplitude and frequency between two decades (1979-1988 VS 2007-2016). This methodology has some fatal flaws (please see the following major comments). I did not find the merit of the proposed methodology compared to other commonly used methods. The study is also contradicting the title of the manuscript. Given these reasons, the manuscript does not meet the stan-

dard of an international journal in its current form. I have to vote for rejection of the manuscript at this time.

Major comments: The methodology proposed in the manuscript has some fatal flaws: (1) The period of decadal- or interdecadal-scale climate variability is not necessarily equal to ten years in nature. It may vary in a very large range. The phase may be different at different geographic locations. For example, Pacific Decadal Oscillation (PDO) and Atlantic Multidecadal Oscillation (AMO) are very significant decadal and multidecadal variabilities in climate system. They have very different periods and geographic locations. Therefore, it is inappropriate that the authors compare the data between two fixed time periods, i.e. 1979-1988 and 2007-2016, when quantifying the interdecadal changes of SAT. In terms of the extraction of climate variability, a successful signal processing method, e.g. FFT, wavelet transform, should be able to automatically detect the amplitude, period, and phase of a time series at various time scales. The author proposed method includes signals at all time scales and does not filter out the interdecadal variability. In this respect, I do not see the merit of the method proposed in the manuscript. I noticed that the authors cited some literatures with regard to Hilbert-Huang transform (HHT). HHT, consisting of empirical mode decomposition (EMD) and Hilbert spectral analysis, can provide a time-frequency-energy description of a time series, which has been used extensively in geophysical research. Why the authors only use Hilbert transform here? In terms of the detection of interdecadal climate variability, are there any merits of the proposed methodology compared to the HHT? I personally do not think the authors present an effective method in identifying interdecadal climate variabilities. If the authors think they did, they have to clearly elucidate the merits of their approach beyond other data analysis methods.

The introduction was not well written. There are many general sentences followed by a number of literature citations without going into details. For example, in P1 L16-19, "Data analysis tools commonly used for the study of complex system are successfully being used for climate data analysis (Huang and Wu, 2008; Ghil etal., 2011; Palus,

2014; Tsonis and Swanson, 2008; Donges et al., 2009; Fountailis et al., 2015; Tantet and Dijkstra, 2014)". What data analysis methods or tools do the authors refer to here? What is the strength and shortcoming of various methods? These general sentences barely provided any useful information to readers.

The significance test proposed by the authors lacks mathematical basis. What is the significance level? The determination of significance threshold seems arbitrary. Why 2σs is chosen?

The title of manuscript includes two key words, "interdecadal changes" and "larges-scale" patterns". However, as I previously explained, it is inappropriate to measure the interdecadal changes in SAT with the difference between two fixed periods. The interdecadal changes of SAT presented in the study are misleading due to the methodology. On the other hand, the manuscript focuses on some specific spots, e.g. the spots located in Arctic and Amazonia. This is not large-scale pattern of SAT. Therefore, the research contradicts the title of the manuscript. The authors could consider focusing on the large-scale patterns of SAT, e.g. the PDO- or AMO-related SST pattern. However, I do not see very clear PDO- or AMO-like SAT pattern from the figures, which is probably due to the inappropriate approach employed, i.e. using the difference between two fixed periods. For example, no clear difference in PDO-like SAT pattern can be found if both periods (1979-1988 and 2007-2016) were in the positive phase of PDO. Under such circumstance, the conclusion of no PDO-like decadal variability would be clearly wrong.

The explanations on the reasons of interdecadal changes in SAT daily time series are vague and hand waving. I'm not saying the explanations are wrong but lack of in-depth analysis and evidence. Some explanations do not even match the results shown in the figures. For example, figure 1 shows a clear increase in the amplitude of SAT time series. The authors argue that the increase in the amplitude of SAT series is linked to the decrease in precipitation. The decrease in precipitation in turn leads to an increase in SAT due to changes in energy partition between latent heat and sensible heat (P4,

L12-16). Assume this is correct, but it only explains the increase in SAT rather than the increase in the "amplitude" of SAT.

The conclusion section did not well summarize the main findings. In my point of view, the key point of the manuscript is the method proposed to identify climate variability using Hilbert transform. However the short conclusion section barely summarizes the method.

What does the "confirm this migration in the XX century" mean in P8, L2?

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