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Interactive comment on "Response of Antarctic Ice Sheet Mass Balance to Climate Change" *by* Jingang Zhan et al.

Anonymous Referee #1

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The paper "Response of Antarctic Ice Sheet Mass Balance to Climate Change" by Zhan et al., deals with an interesting topic about the possible cause of the ice sheet mass changes and to some degree they show quite a interesting connection with the sea surface temperatures in the equatorial regions, i.e. global-scale climate change. In order to do that the authors employ the complex principal component analysis that includes the information about the phase, which I think is the key element in this new analysis.

However this paper is "incomplete". The title ambitiously claims to discuss the ice sheet changes in relation to climate changes, but the paper does not provide strong evidence on this. And the presentation is in many ways lacking and the authors dilute too much the actual new information with a lot of redundant and/or consolidated knowledge that

can be easily and abundantly found in the literature.

Part of the main analysis and related results looks interesting and original to me and it is worth publishing, but with substantial major revision.

The CPCA is a good idea to investigate not only "stationary" signal but propagation of disturbances. The method however has some potential drawbacks (mostly related at the not straightforward interpretation of the results, even worse than with conventional PCA because both the amplitude and phase relationships need to be considered), and no element about the reliability of this analysis is provided here. In my understanding the principal component extracted by the authors is different from the conventional principal component analysis (PCA), allowing in principle to identify also propagating signals, and therefore investigate both the space and time behavior. Conventional PCA have been employed in Antarctica in the past mostly to reveal the trends only. I believe that the additional information that Complex PCA can provide about the phase changes, can improve the power of analysis of the GRACE signal, and it would be very nice to see a discussion of the new insight that Complex PCA can provide in this specific case over Conventional PCA. However, and unfortunately, the authors do not show that.

In addition, the analysis of the other climate variables, like El Nino, ENSO, wind and others, is shown in terms of wavelets analysis and the correlation only, that is by its nature only a partial information, and too late in the discussion, while it should be done earlier (in the result session) and the principal component for those variable should be shown too (at least the temporal part). The claimed response of the ice sheet to the climate changes is only supported by correlation index between the climate forcings analyzed and the first 2 principal components of the GRACE-derived signal. The author themselves recognize that this is not a proof, and the last line of the conclusion is "To fully understand the causes of changes in ice sheet mass, other principal components need to be analyzed".

I believe, that analyizing more component is not the point, but rather taking into account other possible phenomena as forcing of the ice sheet changes. Therefore the claimed implication "climate forcing -> mass variation" is too strong and it is not supported. And considering that the Complex PCA are not common knowledge at least for some of the potentially interested readers, there should be more explanations on how to read the data, which are not easy understandable for everyone, otherwise. Referring to a previous paper is not helping to make the present paper more readable.

Another very weak point of this paper is that the analysis of the GRACE data leads the authors to unreasonable estimates of the mass balance in Antarctica. Not enough details are given to be able to point out what could be the reason for this, but the results are completely outside the realm of the possibility, given the strong consensus emerged in the recent years in the scientific community concerning GRACE (and not only GRACE) mass balance in Antarctica (see Shepherd et al. 2012 and 2018 for example).

The authors devote far too much space to discussing this mass balance (it is also put as the first item in the conclusion), comparing with works that are outdated, and not discussing any of the actual relevant work on the topic. A weak explanation about their results being quite "different" from the main literature just demote the whole work. Most importantly, it can be true that the CPCA analysis that the author perform on the GRACE data is not affected by this, but the fact that the GRACE-derived mass balance is so "off", raises the doubt that the processing could be correct, but the data to process could be wrong.

In the following I make more detailed comments.

L39-49: Too much. Shorten or remove it. It's not relevant for the paper.

L52-55: Here GIA, which is not a tectonic process, is missing from the picture.

L62-65: Here the Ocean interaction (which is the most important) is missing from the

picture.

L77: Why RL05 and not RL06? Since there's a new release, the reason of choice of the release should be mentioned.

L80-88: Since the RL06, the description of improvemenent in RL05 is outdated. I can understand the study has been performed on "old" release, but more words should be spent also on the new release RL06, so not to give the idea that RL05 is the last one or the most up to date.

L92: ICE-6G is inadequate for Antarctica. IJ05-r2 or W12 would be a much better choice. Since 2012 there are several papers about GIA in Antarctica, and several GIA models are available (Caron et al 2018, is one of the most recent and available). I understand that for this work trends like GIA do not matter, but I still don't understand why chosing one of the most inadequate GIA for Antarctica. Probably not using GIA would be a better option, and it would be a good idea to make a comparison with and without GIA correction: to show that makes no difference.

L100: Section 3.1 is useless, we already know all this. Use only formulas that are new or serve some purpose in the paper.

L109/L113: I think that the correct terminology is "linearly independent variables" not "uncorrelated", which has a different mathematical/statistical meaning.

L135: I don't understand how there could be missing points? Are the missing point in time?

L145: The accuracy of RL05 is lower than RL06. A reader might wonder again why not using RL06.

L147: The GIA component is the most inaccurate. Did you take that into account? How? With only one GIA model without errors as the ICE-6G is impossible to have the correct idea of the uncertainty on GIA.

L147-149: All this stuff if it were correct would be redundand with all the previous work, so it's rather useless to discuss it here. It should at least put in comparison with previous works and with the ESA CCI AMB for example, which is based on published work (http://esa-icesheets-antarctica-cci.org/index.php?q=GMB and https://data1.geo.tu-dresden.de/ais_gmb/).

The pattern found by the authors is quite the same as fig.1 (from the esa cci amb) but the ampliture are not. B19-B27 mass balance should be about -180 Gt/yr and not 258.5 Gt/yr:

AIS19 basin 19 0.7 $\pm$ 3.1

AIS20 basin 20 -35.9 $\pm$ 5.6

AIS21 basin 21 -55.0 $\pm$ 8.1

AIS22 basin 22 -50.8 $\pm$ 9.8

AIS23 basin 23 -9.4 $\pm$ 4.9

AIS24 basin 24 -10.7 $\pm$ 4.7

AIS27 basin 27 1.0 $\pm$ 3.5

AIS28 NAP(b25-26) -18.9 $\pm$ 5.9

TOT -179.0 $\pm$ 17.2

L160-169: In this paragraph the use of esperssion "climate change" gives the strong impression that the authors have already drawn their conclusion. But they have yet to demonstrate a clear relationship between climate changes and the ice sheet changes they extract in their analysis. So here and in the rest of the paragraph, I wouldn't call it "climate change" of the fist component. Here just call it "the behavior" of the first component.

L166: I'd like to see also the 3rd component. (instead of figure 1, which is useless)

L167-168: So far, the authors have not shown a correlation of the climate change with the behavior of the principal component, so that statement is not true.

L172-179: Totally redundant (with text above) and wrong.

L184: This work should be compared with GRACE based work (not with altimetry-based work). Altimetry and other techniques have much lower accuracy when it comes to mass trend. The most robust results obtained with GRACE are not in agreement with the mass changes obtained here. See also Shepherd et al. 2018. So the statement is false.

L184: Velicogna and Wahr (2006)?? This is absolutely outdated. They used a very early release of GRACE, a too short time series and a totally unsuitable ensamble of GIA models. It shouldn't be used as reference, for studies that use later release and much longer time series.

L184-189: Totally useless discussion (see also my reason in the above introduction).

L197-198: Exactly! And you didn't mention the most relevant papers anyway.

L198: "this difference may be due...". Since Shepherd et al 2012 it is clear that all the GRACE derived data agree very well when all the input ingredients are the same and even if the methods are different. So no more excuses. If the numbers doesn't match it means that the authors used something quite different in their processing and it's most likely wrong. GRACE derived estimated are well consolidated, so at this point there's no much room left for discussion here. So there is only one thing to do, find the error and fix it.

L208-210: I agree on this but it's not a good excuse for getting the numbers wrong.

L212: And here again, the authors put their conclusion before showing solid prof of that. As I said before at this point the authors hve not shown the correlation with the global climate change pattern yet.

L213: Section 5.2, This part is important and actually show some correlation with the principal component and the global climate dynamic. This must be moved before the discussion as part of the result! Before claiming that the principal component follows a climate change pattern. Note that I am expert in ice sheet changes and not strictly an expert in global climatology, so this section 5.2 is rather difficult to read for me (and more than half of your readers would be like me). So I will point out where it would be nice to have little more information in the text rather than go on internet digging them out from the literature. Here I also note that the previous section about ice mass changes were exaggerately detailed (and I criticised that), if a reader is not expert in ice sheet changes I admit that some more details could be useful, yet it should be done taking the state of the art into account correctly, which has not been done by the authors.

L219: ... rephrase please. Maybe you mean: reflects changes in the low frequency with 5 year or longer periodicity?

L222: The 8.5 and 6.5 years periodic signal in El Nino makes an indeed interesting correlation, but it's not a proof. Since it's not the main energy, do the authors know where this energy is coming from? What are the phenomena that affects that periodicity in El Nino? Is is possible that it is actually a feedback from the ice sheet itself?

L223: "Lag correlation analysis", what does it mean?

L225: what the data in the link are meant to be used by the reader?

L226-227: "...which is much greater..." I understand the meaning but this can be rephrased better.

L228-229: This is interesting! Not robust at all but very interesting. And could it be the opposite, i.e. the ice changes affecting El Nino? I remember previous studies (mostly posters) about this in the early stages of GRACE, but they have never been published because (with short time series) the correlation was weak. Here it's clearly

more visible, but it is not presented in a very convincing way. The study and especially the presentation can be improved.

L230-236: Now you can say this (not before as you did prematurely).

L259: "researchers" -> studies

L269/L271: The are at least two "was" that should be "is".

L277: which signals are you referring to?

L282: Section 5.3. This discussion is about the second component, which has annual periodicity as many other phenomena. It's rather meaningless to find a correlation with an annual signal when you could find a good correlation with any other annual signal in the world. So which one is the most relevant in this case? I think a correlation analysis is insufficient here to determine that. So this is much less interesting and even less solid than the discussion about the first component. L300: For the same reason above, the inference here is extremely weak.

L337-339: This sentence is weird and if the meaning is correct it is really weak. The second component by definition cannot reflect "the overall increase in the mass of ice sheets in Antarctica". Overall increase that is not happening by the way.

L342-347: Same as above. The GRACE derived mass loss are too strong. And even if they were right they are not worth mentioning in the conclusion, since the mass balance is not the aim of the paper. They could be barely used as validation tool... validation that failed in this case.

L356: Could the correlation of annual signal be due to other factors? As I said, there are many phenomena in the world with annual periodicity, each of them would correlate very well or even better. So this is not a solid result.

L365: Rather than other components I'd say that other phenomena should be included in the analysis and eventually excluded from the list of possible cause (or effects).

The figures are of very poor resolution and not easy to read.

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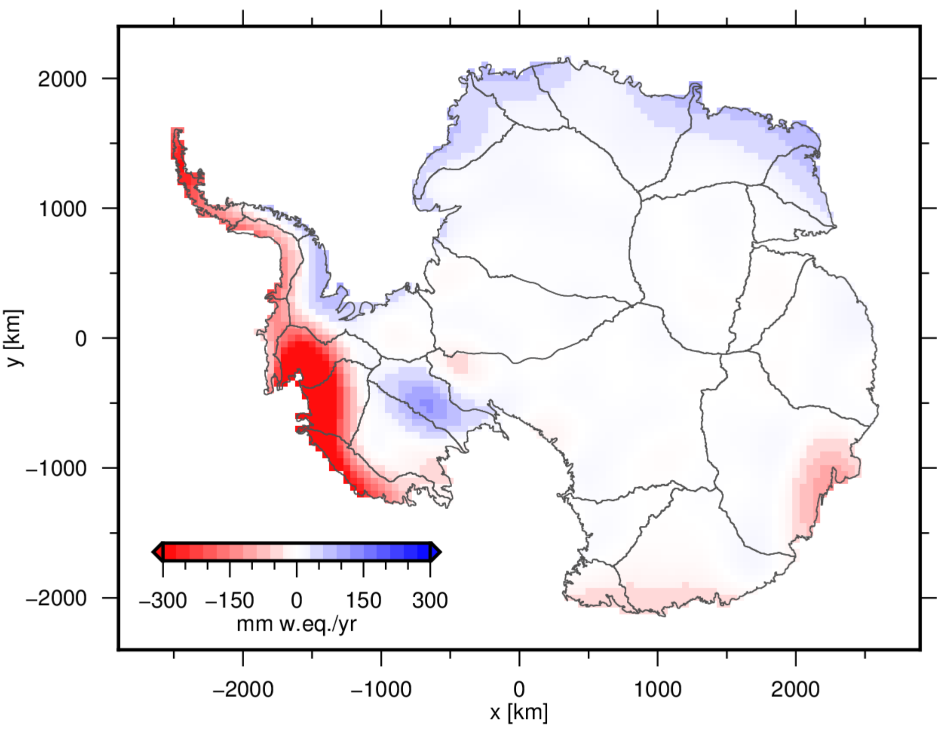

Fig. 1.