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Ocean Science Discussions

Interactive comment on "Can seafloor voltage cables be used to study large-scale circulation? An investigation in the Pacific Ocean" *by* Neesha R. Schnepf et al.

Anonymous Referee #1

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The paper reports on the use of deep sea telecommunication cables for ocean monitoring. The paper is well ordered, well worded and easy to follow. The topic itself is highly relevant, given the lack of data for the ocean and the major part of oceanic processes in the (changing) climate system. Overall the paper could do more to motivate its relevance and to separate itself from similar previous studies. I see flaws in the papers conclusions or at least in the discussion of the conclusions.

Scientific remarks:

1 - Introduction: The introduction is good and short. However, the relevance of the study could be stressed more. It should be a stated clearly that the paper focuses on

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oceanic velocities only. The differences to the well cited previous studies (e.g., Larsen et al.) should be made very clear.

2 - Data and processing: The applied methods seem a bit arbitrary. I would recommend using band-pass filters in stead of splines. The splines' impact on the spectrum is not straight forward.

Daily (and sub-daily?) tides are removed before the spline smoothing. Why is this necessary? Again, better use a band pass filter.

Some signals are not removed or discussed: trends, secular variation (accounted for in sec.3), solar cycle, ionospheric and magnetospheric effects. Some of these are mentioned in the introduction but should in addition be discussed here. The ionosphere is only mentioned with a seasonal influence on the tides. But surely it can have a direct seasonal influence? As an alternative the study could focus on night side data alone.

Longer signals are not removed (band pass filter?). It would be a good idea to at least remove the trends before calculating correlations with ECCO. ECCO may have very different trends for different reasons. Furthermore, the use of climatological conductivity in elmgTD may falsify the trends of the ECCO EM results.

3 - EM Prediction: Why are only the horizontal ECCO velocities used? Can the influence of the vertical velocities and their changes be quantified?

To use climatological conductivity should at least discussed in sec. 4. It may be better to use the conductivity from ECCO. Or booth, to demonstrate the effect of sigma on the results.

Are sediments considered in elmgTD?

Line 107: The elmgTD was forced with monthly velocities, monthly core fields, monthly conductivity but produces daily estimates? If so, do they contain any information on shorter than monthly time scales?

Why are the ECCO data not spline smoothed as well?

Line 110: What is meant by layer most closely corresponding to the sea floor The models bottom layer? Do elmgTD and ECCO use a different bathymetry or does elmgTD bathymetry does not fit well to the reality? Maybe I just don't understand this sentence.

4 - Results and discussion: In my opinion, some conclusions lack a solid base. To amend this I would strongly recommend some recalculations or additional analyses. At the very least, the discussion should be deepened. The sections main arguments base on a mismatch between the ECCO results and the cable estimates. In short, two very different data sets are compared and if the observations do not fit to the model based estimates, then the observations are said to have a low "signal-to-noise-ratio".

The ECCO based results are not questioned or discussed at all

I have several questions here: How reliable are the ECCO results? What are the errors of the used velocities? There probably exist model inter comparison studies. I would advise to repeat the elmgTD calculations with at least two other ocean models.

What is the influence of the used climatological conductivity? It would be a good idea to recalculate the ECCO based results with ECCO based conductivity which shows real intra-annual variability and discuss the differences.

Is the quality of the modeled data globally uniform? If not (probably not), then the "signal-to-noise-ration" mentioned in the paper depends not on cable length or the strength and uniformity of ocean currents but may just depend on location. ECCO is an assimilative ocean model: that means, that the errors of the results depend on the amount and quality of available data. This is not globally uniform, too. For example, if satellite altimetry is assimilated (major source of ECCO's information), then a big current like the Kuroshio (OKI cable) has a strong sea surface gradient and will be much better represented by the assimilated model than the more or less "flat" oceanic areas (HAW). But the HAW measurements might still be not worse than the OKI measure-

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ments. As long as it is not clear if the found low correlations are caused by the model or the observations or the principal differences in the data, one should not call them low signal-to-noise ratio.

The differences between modeled data and observed data are not discussed enough Please discuss the effects in the cable data that are not in the modeled data: trends, ionosphere, solar cycle etc. see remarks to Sec. 2

Please discuss the representation error/issue: Grid box averages are compared to a very local cable path. By looking at Fig. 5C, one can see that even very similar cable paths can already produce very different results. Please discuss this. In addition, from the differences between HAW1N and HAW1S some real signal-to-noise ratio could be derived. An error bar probably could be produced that sets the model to observation comparison into relation. Is there any explanation, why these two cables produce different time series (surely, the effects mentioned in the previous paragraph should affect both cables equally)?

Again, for the model side an error bar should be generated or estimated, too.

Spline smoothed observations are compared to temporal averages from the model?

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