Anonymous Referee #1

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

Thank you for this comment. The final paragraph of the introduction will be accordingly revised to be explicit about these differences:

> This study aims to provide a 'first step' answer to the question can seafloor voltage cables be used to study large-scale circulation? To investigate whether it may eventually be feasible to use large-scale voltage cables for monitoring ocean flows, we evaluate the correlation between data from large-scale seafloor voltage cables and numerical predictions of the electric field induced by 3-D ocean circulation velocity fields. While this work builds off of studies using seafloor voltage cables to monitor flow velocity in ~100km wide passages, this study is the first to examine this application in basin-wide seafloor voltage cables.

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

Unfortunately, while I do agree that a band-pass filter would be more straight-forward (and perhaps less arbitrary) than using splines, because 1) the data is not continuous and 2) data gaps are often long enough that interpolating would cause more problems than it would solve, we avoid using frequency/spectral based methods. This is why we instead use splines.

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

Because we cannot use a bandpass filter, we remove tidal signals before the spline smoothing to ensure that they are not influencing the spline fit.

We will explicitly state in our paper that we remove tides of the following periods: 4 hr, 4.8 hr, 6 hr, 8 hr, 11.967236 hr, 12 hr, 12.421 hr, 12.6583 hr, 23.934472 hr, 24 hr, 24.066 hr, and 25.891 hr.

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?

We will be sure that the methods section will be revised to clearly state what signals are removed or left as a source of error. We do remove the direct current (DC) trend in the data and most magnetospheric signals are also removed by limiting the study to quiet times (Ap < 2 nT). Because we cannot separate secular variation in the cable data, we incorporate it into the numerical simulations by using time-dependent IGRF model values in the forcing term (and note that the IGRF model also does not separate the internal signal induced by secular variation).

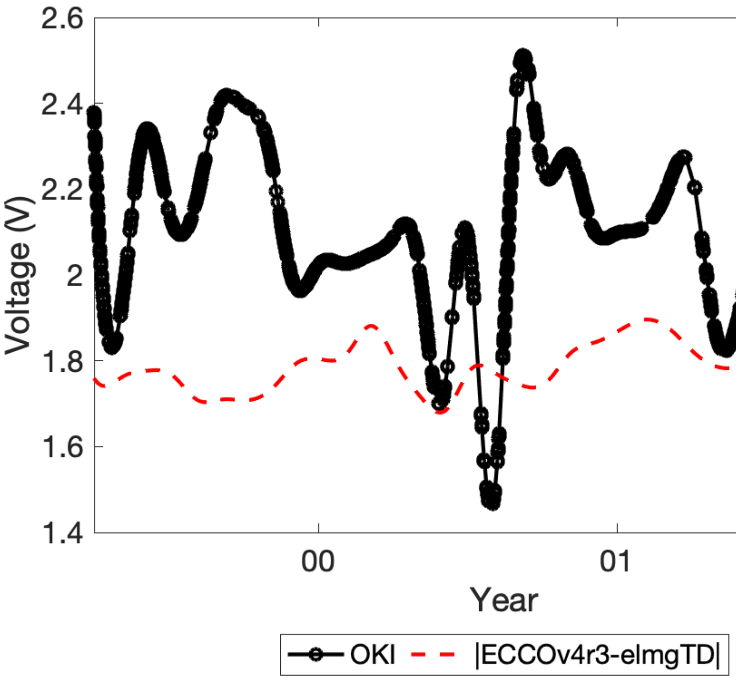
It is certainly possible that there is a direct seasonal influence from the ionosphere since ionospheric signals depends on sunlight and that varies seasonally. Indeed, there may be noise from other sources (eg. seasonal changes in the quiet magnetosphere) that is not removed by the methods we have undertaken. This would be the case even if we used bandpass methods: noise within the same frequency band as ocean circulation would still leak into our study.

As an alternative the study could focus on night side data alone.

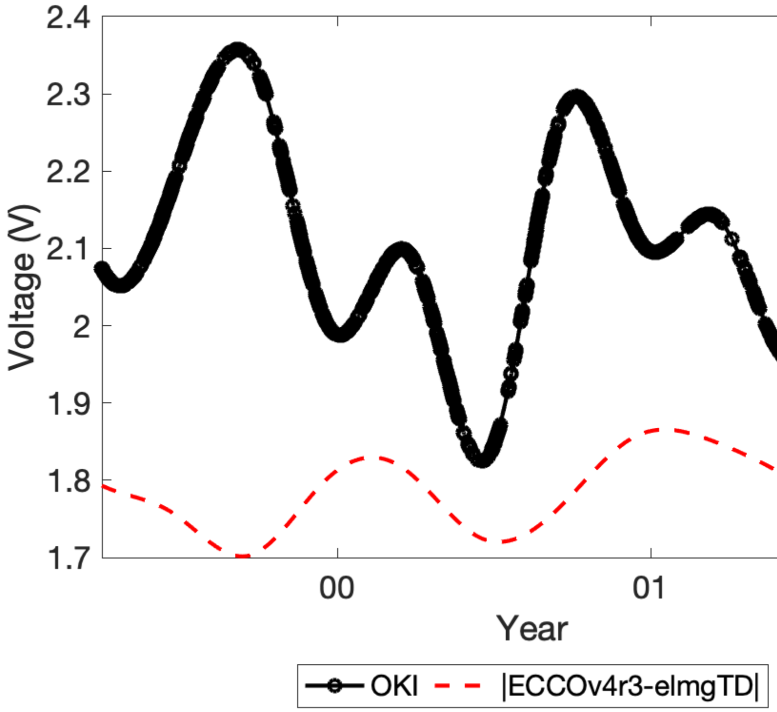
This is only possible for one of the cable's in our study: the OKI cable. The other cables span across too many time zones, so limiting the data to times when it is night across the entire cable limits the data too much. Because the OKI cable mostly varies with latitude rather than longitude, we were able to utilize night-time only data and perform our analysis on that data. For this analysis, local night-time was determined as the time between local sunset and local sunrise at the mean latitude and mean longitude coordinate for the OKI cable.

Comparing OKI's nighttime data to our numerical simulations is not ideal because by only using night-time data, we are decreasing the sample size of our dataset and increasing the variance (the variance of all OKI data is 1.8798 V, whereas for night-only data it is 2.1250 V).

Cable	**30.5 spline fit**		**90 spline fit**	
	R	**R^2**	**R**	**R^2**
OKI	0.5920	0.5920	0.6526	0.4259
OKI night-only	-0.1437	-3.9602	-0.1819	-7.4372



Left: plot of the OKI cable's nighttime processed data using a spline fit of 30.5 day knots.



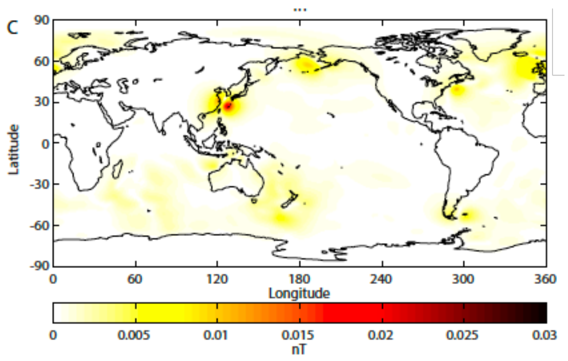
Left: plot of the OKI cable's nighttime processed data using a spline fit of 90 day knots.

As shown in in the above figures and table, the data and numerical simulations now have voltages of more similar magnitude. However, the simulation still has a much narrower range of voltage compared to the processed data, and using only night-time data in fact dramatically worsen the correlation with the simulation results.

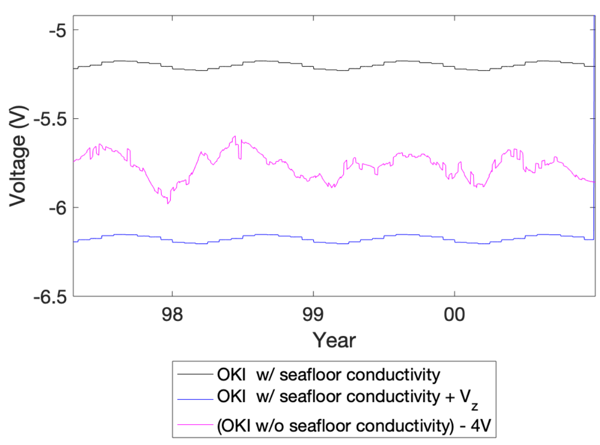
Longer signals are not removed (band pass filter?). It would be a good idea to at least remove the trends before calculating correlations with ECCO. ECCO may have very different trends for different reasons. Furthermore, the use of climatological conductivity in elmgTD may falsify the trends of the ECCO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

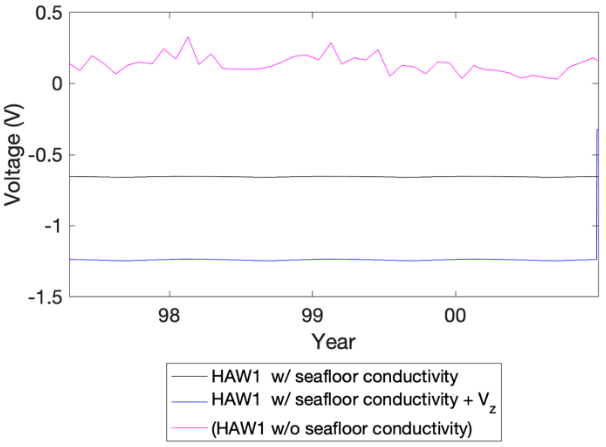
As previously explained, we do not use frequency methods because of the prevalence of gaps in the data. We believe that it is useful to retain all the signals remaining after our subtraction of tides and the linear trend.

Additionally, we do not believe using climatological conductivity data is a source of significant error. As shown in Grayver et al (2019), using an annual average seawater electric conductivity value versus using a monthly climatological value yields a difference of less than 0.005 nT in the induced oceanic electromagnetic signals for most of the globe (the plot of this is shown to the right). Thus, using the NOAA WOA seawater climatological values versus ECCO's seawater electric conductivity values is not likely to significantly alter our results.



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





These figures show how the results vary when seafloor conductivity and vertical ocean flow are included. There is a great difference between using seafloor conductivity and omitting it. Meanwhile, including vertical ocean flow simply shifts the results by a small factor, and thus is less important for correlation studies. We will a discussion of this direct current shift in our future manuscript.

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.

"To compare numerical predictions with the processed seafloor cable observations, the seafloor electric field was isolated by determining the electric field values of the depth layer most closely corresponding to the seafloor. These seafloor electric field values were then integrated along the path of a given cable, excluding the cable's continental endpoints. The results of this are shown and discussed in the next section."

We will revise that sentence and section so that the following information is clearer:

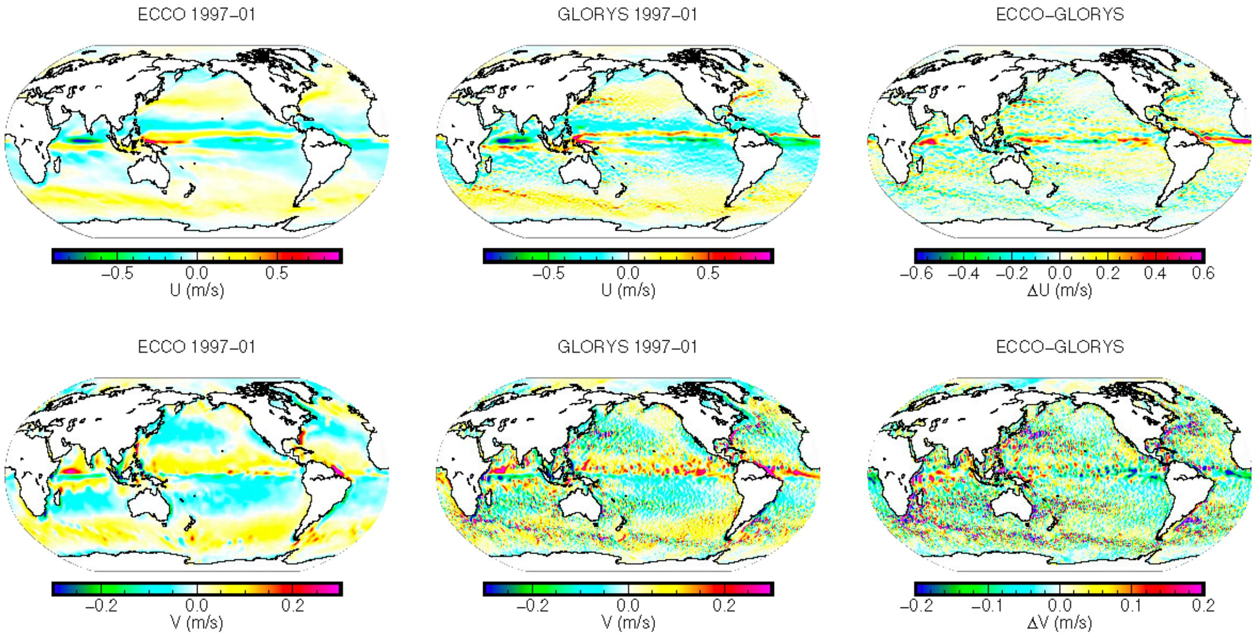
- The elmgTD solver uses the same vertical layers as ECCO (or whatever other ocean flow input model we use)
- ECCO uses 50 vertical layers at every grid point and each layer represents the same depth. However, the seafloor occurs at different depths across the ocean, so for each grid point the depth corresponding to the seafloor must be determined. This determined as the depth layer where the ocean velocities become zero.

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 section's 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".

Thank you for your comments. We agree that we should strengthen this discussion for the reasons you point out.

I would advise to repeat the elmgTD calculations with at least two other ocean models.

We investigated also using the GLORYS model. This model is higher resolution than ECCO and has significant differences in its velocities (see below figures).



Unsurprisingly, the predicted voltages varied dramatically between these two models, with no meaningful correlation between the models predicted cable voltages. These differences are largely due to differences in the top oceanic layer: the induced voltages depend on the depth-integrated velocities and the largest ocean currents are at the top layers. GLORYS is also a higher resolution, eddy-resolving model.

Thus, we have decided to continue by using the approach recommended by Reviewer #2 (see below): rather than try to compare the voltage predictions to one model and have a discussion that treats both the one model and the cable data as solidly reliable sources, we will discuss the limitations in this method and discuss how future cable studies may be better conducted by performing a principle component analysis (PCA) on synthetic data using the higher resolution GLORYS model. The goal of the PCA will be to determine the relationship between the induced cable voltage and the ocean flow’s transport across the cable as it depends on both time and cable length.

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…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 measurements. 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.

Balmaseda et al (2015) compares different ocean reanalyses. ECCO and GLORYS are both composed of data from satellites and in-situ measurements, and the assimilations are forced to also satisfy the laws of physics and thermodynamics. ECCO operates on a 1-degree global grid, whereas GLORYS uses a 0.25 degree grid and includes resolving eddies.

You bring up a very important point; ECCO uses a variety of satellite data to determine sea surface height and ocean currents. As you state, this better represents areas with stronger signals such as the Kuroshio Current and underrepresents “flatter” regions like the Eastern Pacific. The GLORYS model also has similar limitations. Indeed, any data-based model will.

We will revise the manuscript so it does not describe the cables as having a certain type of signal-to-noise ratio, but instead explicitly states the different sources of uncertainty entering the analysis from both the numerical work and the observational work.

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

We addressed this in your Section 2 comment and will be sure a similar discussion is also incorporated at this point of the paper.

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.

Thank you for raising this point. In our updated simulations, along with calculating the results on a 1 degree grid, we also calculated them on transects of the cables’ paths. This is the best way we can ensure error is not being introduced because of using numerical results from the wrong location. Of course, this method still is not perfect since there are no guarantees that the cables are perfect lines on the seafloor between their starting points.

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)?

Thank you for asking about this. The two cables are quite similar:

HAW1N				HAW1S			
Range (V)		Median (V)	| mean | (V)	Range (V)		Median (V)	| mean | (V)
-27.845	15.7686	-0.33576	1.9746	-27.0786	16.6138	0.45492	2.0437

(HAW1N - HAW1S) residuals
standard deviation (V)
0.2117

We did a few more calculations on this since our analysis uses 90-day and 30.5-day knotted spline fits of the day—a process that in smoothing/averaging the data inherently lowers the observational error.

HAW1N 90.5 day spline fit				HAW1S 90.5 day spline fit			
Range (V)		Median (V)	| mean | (V)	Range (V)		Median (V)	| mean | (V)
-0.3189	0.5203	-0.0211	0.0271	-0.3966	0.4989	-0.0179	0.0255

(HAW1N - HAW1S) 90.5 day spline fit residuals
standard deviation (V)
0.0627

HAW1N 30.5 day spline fit				HAW1S 30.5 day spline fit			
Range (V)		Median (V)	| mean | (V)	Range (V)		Median (V)	| mean | (V)
-0.5158	1.0839	0.0033	0.0271	-0.4974	0.6909	0.0174	0.0255

(HAW1N - HAW1S) 30.5 day spline fit residuals
standard deviation (V)
0.0816

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

The dependence of the induced voltages on the flow velocities is linear on the global scale: i.e., increasing the flow velocities everywhere and at all times by 10% will give a 10% increase of predicted voltages. Although it does not hold locally in space or time, we can use this as a crude estimate of error: relative error of the predicted voltages is the same as the relative error of flows. There is no clear number of the estimated velocity error from the ECCO model; doing an intensive ensemble simulation could provide such an error estimate, however, this is beyond the scope of this study.

For the conductivity, the dependence is non-linear. On page 3, we show a figure illustrating the difference between using an annual average seawater electric conductivity value versus using a monthly climatological value.

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

We'll add a sentence to the end of the Numerical Predictions section that explicitly states how the numerical results are compared to the simulations:

> For comparison to the spline smoothed observations, the numerical simulations' seafloor cable voltage predictions underwent the same 30.5-day knotted and 90-day knotted spline fits. These are shown in the results of Figure 5.