Reviewer #1

I have only minor recommendations and comments:

p3 l66: please add the tidal acronyms

Done.

p3 l77: I think this is not mentioned in Pedatella, 2012

Leaving "electromagnetic" from the main text as Pedatella (2012) discusses the seasonal variations of tides in general.

Fig.2: Maybe show one year only, the two top panels look very similar

We have considered it and decided against this recommendation. An additional information provided by this Figure as it is, is the level of sparsity and gaps in the HAW3 data-set.

Fig.4-6: Please unify the y-axis ranges of the top panel

Done.

Fig.7: Where is the negative lag? (does it go down as well?)

Negative time lag is unphysical (and yes, it goes down). Following recommendation by the Reviewer #2, we have reverted to a simple correlation table, and time lags are not discussed anymore.

Reviewer #2

I have 5 substantive comments:

(1) I urge the authors to explicitly state that ocean state estimates like ECCO do not represent with 100source of error include atmospheric and magnetospheric noise which is not modelled, and might not be fully removed from the data. This is why I recommended earlier a perfect model experiment, to avoid such issues. Their chosen approach to present their results does stand on its own, but now the correlations should serve as a lower bound.

Agreed and reformulated.

I am not aware of published results that analyze how global ocean models compare with direct velocity measurements - mostly because negative results are not published. One recent article does discuss this in passing, however. See Szuts et al., 2019.

Reference added. We agree, that differences between the ECCO velocity model and actual velocity field represent a substantial source of error.

With minor effort they could add more insight into their ECCO-based predictions: calculate the correlation between the estimated cable voltage and the net transport across the cable. With entirely numerical results, there are no sources of noise to decrease this correlation, and so such a correlation could be considered an upper bound to what is achievable with measurements.

This was a very useful suggestion and we have implemented it and extended the discussion.

(2) line 50: Be careful: before making such a bold claim in an interdisciplinary field a thourough literature search is required. I found a half dozen articles that already cover this topic, most of which cite publications listed in their bibliography. I'm also including a number of references that treat short submarine cables or related topics, for further background.

Corrected.

(3) The discussion would be more complete and thourough by adding a few sentences that discuss how the middle and bottom subplots of Figures 4, 5, and 6 agree or disagree.

Extended discussion added.

(4) For an instantaneous process like motional induction, lagged correlation analysis does not hae any useful interpretation to my knowledge, which suggests removing figure 7. What would be useful, however, would be to provide confidence limits on the correlations (at zero lag). This would give insight into how significant such comparisons are, though my previous comment about the low fidelity of ECCO circulation to the real ocean means that these correlations are lower bounds. In doing these statistics, don't forget that the time-series are red and thus their degrees of freedom are much reduced than the number of data points in the time-series. So, divide the duration of the time-series by the separation of the splines's knot separation (e.g. 30.5 days).

Due to inclusion of induction coupling with the mantle and ocean self-induction, the process is not completely instantaneous, but we agree that the importance of these effects is not crucial here, and lagged correlations were replaced by a simple table with zero-lag correlation coefficients. Due to gaps in the cable data, we have not implemented the error analysis of the correlation coefficients. As expected, a quick analysis using the jackknife method predicted unrealistically small error bounds, and we have decided against showing it.

(5) In terms of the correlation values, I find it hard to believe that the HAW1S and HAW1N time-series, which are very similar, have correlations as different as 0.23 and 0.04. Have you de-trending all three time-series prior to calculating correlations? Unremoved trends could easily account for the difference in their correlations.

Thank you for pointing to this oversight. In the previous version of the paper, only the mean value was removed. De-trending now significantly affects the correlations, and also the presentation of voltages in the top panels of Figures 4–6. Best correlations are now obtained for HAW3 cable, and the difference between the N and S branches of the HAW1 cable are reduced.

Detailed comments:

line 6: ”numerical predictions of the electric field induced by ocean circulation” doesn’t make clear that the ocean circulation itself is an estimate.

Corrected.

line 7-8: ”correlation between cable voltage data and numerical predictions strongly depends on both the strength and coherence of the velocities flowing across the cable,” surely the correlation also depends on whether the numerical models accurately reproduce the true ocean signals (velocity and the resulting EM fields) sampled by the cable? Unless you can prove that the ocean models accurately predict ocean velocities - and there are far too few velocity observations to make such an estimate - then lack of correlation between cables voltages and numerical predictions is merely a representation issue that prevents answered the research question posed.

Reformulated.

line 19-20: To my knowledge, Faraday’s experiment was inconclusive and ”unsuccessful”; ”not very successful” suggests some level of success.

Corrected.

Figures 4, 5, and 6: These color contour plots really make my eyes hurt. Especially for the sake of color-blind individuals and black-and-white printouts, please use a simple 2-toned colormap like in Figure 3 instead. I find I can extend the dynamic range of colors by transitioning from red/blue to black at the end of the positive/negative range, which does not hinder the ability to visually separate positive regions from negative regions. See Light and Bartlein (2004), and updates to this (e.g. https://betterfigures.org/2014/11/18/end-of-the-rainbow/)

Color scales changed.

Figures 4, 5, and 6: How do you calculate transport? In oceanography, transport is volume per time, or m3/s. The legend for the bottom subplot says m2/s, however, which is transport per unit width. Please make consistent.

By $T_\perp$ we mean the vertically integrated horizontal velocities, perpendicular to the cable, thus the unit m^2/s. In the extended correlation analysis, we now use also the total transport across the cable $P_\perp$ in m^3/s.

Figure 4: Can you explain the relation between the voltage and transport plots in a little more detail? why there is strong transport from 250 km to 600 km, but instead the voltage induced in the cable is only positive around 150-250 km and 250-540 km? Why is the negative transport from 700-1200 km not reciprocated in voltage? Why does the weak positive transport from 0-150 km have a negative induced voltage?

We assign these differences to the complicated structure of electrical conductivity and ocean velocity in the vicinity of the Ryukyu arc. Large bathymetry changes have strong influence on the local EM fields.

Figure 4: For the top subplot, the caption refers to a brown line, but none is visible.

A reference to a run with simplified 2-D EM physics, that we decided not to show. Removed.

Figures 5 and 6: There is closer correspondence between the transport and voltage in these two examples compared with Figure 4. I would be interested to know why.

Because we are in the deep ocean with smaller variations in bathymetry and no resistive continents nearby.

Figure 7: Unless you discuss the lagged correlations, this figure is unnecessary.

Done. Replaced by Table 2.