Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2020-47-AC2, 2020

Annales Geophysicae Discussions
Open Access
EGU

Interactive comment on "Migrating tide climatologies measured by a high-latitude array of SuperDARN HF-radars" *by* Willem E. van Caspel et al.

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Received and published: 29 September 2020

The authors would like to thank the reviewer for their comments on our work. We address each comment individually by stating the original Referee Comment (RC) followed by the Author's Response (AR).

RC1: Page 1, line 14: Perhaps replace the term "Hough" (which will be unfamiliar to most readers) with "spherical harmonic"?

AR1: The authors agree with RC1 in that latitudinal spherical harmonic structures (associated Legendre polynomials) will be more familiar to most readers. However,

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since Hough modes form such an integral part of tidal theory (as described in detail in the work of Chapman and Lindzen (2012) cited on line 13), and because Hough modes are also referred to in the discussion, we would still like to make reference to Hough modes in our introduction of the tides. Since both spherical harmonics and Hough modes can be expressed as a combination of either one, as both form a complete set of basis functions, we think the following change to line 14 in the revised manuscript may offer more clarity:

"The tides have a latitudinal spherical harmonic structure (termed Hough modes) and longitudinal zonal wavenumber (S) structure,..."

RC2: Page 1, line 23: "yaw cycle intermittency" sounds wordy and opaque. Replace with "slow local time precession".

AR2: Line 23 has been rewritten to include slow local time precession as a drawback of satellite measurements, while still including yaw cycle maneuvers (e.g., the changing latitudinal coverage of SABER/TIMED every 60 days, and every 36 days for MLS/UARS) as a separate drawback. Line 23 now reads:

"Typical drawbacks associated with satellite measurements arise due to constraints imposed by asynoptic sampling (Salby, 1982), including slow local time precession and yaw cycle maneuvers."

RC3: Page 4, lines 76-77: This sentence is incomprehensible. Are you trying to say that "If measurements are not available for both stations at any given time, measurements are excluded in a manner so as to optimize the equidistant longitudinal spread of measurements?"

AR3: It was our intention to say that, if measurements are available for two closely-spaced (in longitude) stations at any given time, we leave out one of the station's measurements in the fit to Eq. 1, such that the longitudinal spread of measurements becomes more equally spaced over the fitting domain.

For example, if at a certain time there are measurements available from Han, Pyk, Sto, Gbr, Sas and Kod (as shown in Fig. 1), the measurement of Pyk is excluded in the fit to Eq. 1. If we didn't exclude Pyk, the winds measured at the longitude corresponding to the (Sto, Pyk) 'longitude pair' would effectively have a double weight in the least-squares fitting routine, which is undesirable. The measurement of Pyk is therefore excluded in the fit, since the fit is then applied only to measurements that are spaced roughly 50 degrees longitude apart. In the revised manuscript, lines 76-77 have been rewritten to,

"To that end, for the radar pairs closely spaced in longitude, (Ksr, Kod), (Rnk, Kap), and (Sto,Pyk), only one of each of the pairs measurements is used in the fit to Eq. 1, even if data are available for both.".

RC4: Figures 2-5 need to be enlarged.

AR4: Figures 2-5 have been enlarged as well as modified in accordance with the comments of Reviewer 1.

RC5: I suggest showing the climatology first, then the year to year variability.

AR5: We followed the plan of similar climatological papers, where the raw data, with its variability, is presented first. The climatology is then shown to emphasize that only the seasonal variations present in the data have been isolated in the climatology. We then go on to discuss only those features observed in the climatology and draw conclusions from them. If we reverse the order, the flow jumps from climatology, to raw data, to discussing the climatology. While we could reverse the order without prejudicing the results, we would prefer to keep the present order to reflect other climatology papers and to provide a better "readability" factor.

RC6: Page 7, line 151: replace "tidal modes" with "tides".

AR6: "tidal modes" has been replaced with "tides" throughout the text in accordance with RC11.

RC7: Page 8, lines 155-156: Simplify to: "...not lead to significant cross-contamination errors between the migrating tides."

AR7: Lines 155-156 have been simplified in accordance with RC7.

RC8: Figure 6: Any idea why the RMS difference for SW2 is so much higher than the others?

AR8: We are unable to identify any particular reason why SW2 has a RMSE roughly 50% greater than that of DW1 and TW3. For example, no one-to-one relation exists between the yearly mean tidal amplitudes in NAVGEM-360 (9.8, 10.1, and 2.74 ms-1 for DW1, SW2, and TW3, respectively) and the yearly mean RMSE values shown in Figure 6.

RC9: Page 10, line 187: "Should read "Whether conditions are favourable..."

AR9: Line 187 has been updated in accordance with RC9.

RC10: Page 10. The nomenclature is confusing. (1,1) is the first symmetric propagating Hough mode. (1,2) is the first antisymmetric propagating mode.

AR10: For consistency, we now refer to the diurnal (1,1) mode in the same manner as Dhadly et al. 2018, which is the work cited in our discussion on the DW1 tide. The mode is now simply referred to as the Diurnal (1,1) Hough-mode.

RC11: The term "mode" refers to the latitudinal structures, or Hough modes. It should not be used to describe the longitudinal wavenumber or frequency. Thus, DW1, SW2, etc. are tides. (1,1) is a mode.

AR11: "tidal modes" has been replaced with "tides" throughout the text to correctly reflect the distinction between the latitudinal Hough mode structure and longitudinal wavenumber structure of atmospheric tides.

RC12: Page 10, lines 195- 208. Lots of speculation here about the diurnal winds and how they may be distorted by the SuperDARN "observational filter". Is it feasible to quantify these effects by forward modeling DW1 winds into meteor echoes?

AR12: The various factors that might impact the representation of DW1 in the SuperDARN meteor winds have been further examined in the interim, and the vertical wavelength of DW1 being near to the vertical average represented by SuperDARN winds is likely to be the most impactful. Hence, to avoid unnecessary speculation, the following lines have been removed from the text:

“In addition, DW1 is the tide most susceptible to contamination by non-migrating tides, given that the 180 degrees of longitude aliases DW1 and the diurnal oscillation in the mean wind (D0). Although no evidence of such cross-contamination is found in the NAVGEM-HA sampling experiments, with D0 amplitudes in NAVGEM-360 never reaching above 4.0 ms-1. Lastly, DW1 is likely to be the tide that is most strongly affected by the diurnal cycle of meteor echoes (Hussey et al., 2000;Tsutsumi et al., 2009).“

The quantify the effect of the vertical average “observational filter” of SuperDARN would require the model top of the NAVGEM-HA meteorological analysis system to be extended at least up to ∼125 km altitude. But as discussed in AR1 to the comments of reviewer 1, this is beyond the scope of the current work.

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