

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

Anonymous Referee #2

Received and published: 12 September 2020

Recommendation: Publish with minor revisions

This paper presents observations of migrating diurnal (DW1), semidiurnal (SW2) and terdiurnal (TW3) tides in 16 years of high-latitude horizontal winds measured by the SuperDARN network. These are the only direct measurements of mesospheric and lower thermospheric winds from which migrating tides can be defined globally on time scales shorter than satellite 24-hour precession periods (1-2 months). The authors have analyzed the data very carefully, and make a strong case for the fidelity of their tidal retrievals. Aside from a few curious features (e. g., reversal of the polarization of DW1 winds during summer, a TW3 amplitude maximum in October, an SW2 maximum

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in September), the tides do not exhibit any particularly interesting behavior. The main strength, and take-home message of the paper is the validity of the analysis. Although straightforward, the robustness of this method was not a foregone conclusion when applied to SuperDARN winds, since the network only spans about 200 degrees longitude. I therefore consider the material worthy of publication, because of the promise of this method for identifying short-term tidal variability, a topic that is highly pertinent to vertical coupling and whole-atmosphere modeling.

This paper is for the most part clearly written and organized. I recommend publication after the authors respond to the following, mostly minor comments.

1. Page 1, line 14: Perhaps replace the term “Hough” (which will be unfamiliar to most readers) with “spherical harmonic”?
2. Page 1, line 23: “yaw cycle intermittency” sounds wordy and opaque. Replace with “slow local time precession”.
3. 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?”
4. Figures 2-5 need to be enlarged.
5. I suggest showing the climatology first, then the year to year variability.
6. Page 7, line 151: replace “tidal modes” with “tides”.
7. Page 8, lines 155-156: Simplify to: “...not lead to significant cross-contamination errors between the migrating tides.”
8. Figure 6: Any idea why the RMS difference for SW2 is so much higher than the others?
9. Page 10, line 187: “Should read “Whether conditions are favourable...”

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

11. 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.

12. 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?

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