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Interactive comment on "On the variability of the semidiurnal solar and lunar tides of the equatorial electrojet during sudden stratospheric warmings" *by* Tarique A. Siddiqui et al.

Anonymous Referee #2

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General comments

The paper presents observations of equatorial electrojet (EEJ) during four sudden stratospheric warming events and discusses variations in solar and lunar tides in the EEJ in relation to SSW timing. This is an important contribution, as relative variations in solar and lunar tides and mechanisms behind these variations remain a matter of an active discussion in literature. The paper also uses numerical simulations for 3 out of four SSW events and attempts to interpret results in the context of simulations. The topic of the paper is significant and appropriate for the journal. However, I think the paper needs more clarifications, in particular about interpretation of simulations, and

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recommend a moderate revision. I have several comments that I hope will be helpful to the authors.

Specific comments

1. The authors accounted for dependence of EEJ strength on solar flux values by normalizing to a fixed solar flux of F10.7 = 150. I wonder why this level is so high, as three out of 4 SSW cases used in the study occurred during much lower solar activity. Is there a good evidence that 'corrected' EEJ strength does not depend on the value of solar flux used for normalization?

2. p. 5, 'We assume constant amplitude and phase of the tidal components within the 21-day window' – as amplitudes change on a shorter time scale, it is important to discuss the influence of this assumption on final results. Also, is there a justification for using a 21-day window instead of 15-day window?

3. It is not clear from the description if the authors used simultaneous fit to S and L components.

4. I am concerned about panels with stratospheric data in figures 2-5. The temperature at 10hPa seems to be very different from figures in previously published papers. For example, in case of SSW 2009, that was extensively studied by different authors, there is a dramatic variation in temperature from below ∼200K in December to ∼265K during the peak of SSW in the NCEP data, and the temperature is below multi-year average from mid-February to the end of April (see figure). Please check your NCEP data and plotting routine. I have loaded attached figure from https://acd-ext.gsfc.nasa.gov/Data_services/met/ann_data.html

5. P. 9, 'To a certain degree, there is a similarity in timing between the enhancements of the SW2 and the S2 over Huancayo' – I am not sure about this, they seem to be pretty different to me.

6. Observations and simulations are given using different temporal scales – why? It

makes it more difficult to compare. Was model output available only after Jan 1 and only for 50 days? If model output is limited to a shorter period, how does the use of 21-day window affect tidal results?

7. The presented simulations are difficult to interpret. Besides different temporal periods, the authors use different parameters, EEJ strength in data and temperature in the model. Is it possible to process simulations to calculate EEJ strength from the model output, and compare observed and simulated EEJ? At the very least, it would be useful to add a discussion on how temperature at middle latitude is related to EEJ at the equator. Brief description is given on page 10, lines 23-24 – I suggest to extend it and move earlier, before discussing simulations.

8. Simulations are presented essentially for three different models, and there are major differences between simulated SW2 and M2 in the magnitude of tidal modes, temporal evolution, and latitudinal structure of tidal modes, especially for the M2 mode. The differences exist between different simulations, but as they are also used for different SSW cases, it makes it difficult to assess what models are getting correctly and what they are not getting correctly. What is the justification for using three different models?

9. As the authors choose to present tides in neutral temperature in simulations, they could compare simulations results with SABER results presented by Zhang and Forbes, 2014 (Zhang, X., and J. M. Forbes (2014), Lunar tide in the thermosphere and weakening of the northern polar vortex, Geophys. Res. Lett., 41, 8201–8207, doi:10.1002/2014GL062103. I am particularly concerned about the latitudinal structure of lunar tide and the timing of the amplifications in lunar tide. There are significant differences between Zhang and Forbes observations and simulations presented in this paper. I am concerned that the authors overstate the levels of success in simulations.

10. Overall, I think the modeling portion of the paper needs more work. It does not provide a solid understanding of the level of agreement or disagreement with observations, and what models can or cannot simulate successfully.

Minor comments:

p. 2, ‘have reported about the lower thermospheric warming’ - have reported the lower thermospheric warming?

p.2, line 30 – comma after SSW?

p. 4, ‘which mostly result due to the lunar semidiurnal’ – ‘which mostly result from the lunar semidiurnal’? Or ‘which mostly are due to the lunar semidiurnal’?

p.4, ‘t denotes the solar in hours’ – it is not clear; please clarify – do you mean solar time?

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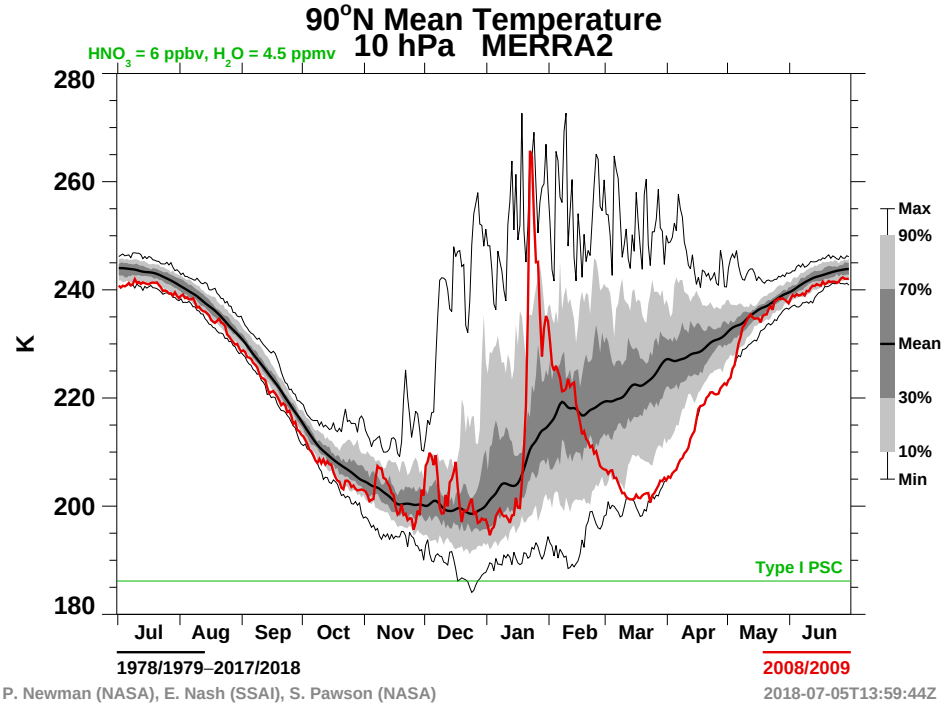

Fig. 1. Stratospheric temperature at 90N and 10hPa from the NASA data

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