

Ann. Geophys. Discuss., author comment AC1
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Reply on RC1

Shin'ya Nakano and Ryuho Kataoka

Author comment on "Echo state network model for analyzing solar-wind effects on the AU and AL indices" by Shin'ya Nakano and Ryuho Kataoka, Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2021-54-AC1>, 2021

We are grateful to the referee for the helpful comments. The followings are our responses to the comments.

> *Lines 102-103: "We can then identify properties of the auroral electrojets by analyzing the synthetic indices obtained from various artificial inputs"*

> *This can be very useful, but it is surprising how it was done in practice. The construction of the "synthetic solar wind" was done by fixing one of the input parameters to zero or other fixed level in order to determine the effects on the output. While that process produces some useful results, I expected something like use of a step function for velocity or IMF values. For example, using a steady value of one parameter such as Bz for a period of time, such as a few hours, then stepping up to another value, and repeating. The results should show how the AU/AL indices respond to that parameter.*

According to the suggestion, we conducted some experiments which give a variation of a solar wind parameter by as rectangular waves with various periods. The results of the experiments are shown in the supplement attached and will be included in the revised version. Temporal variations are smoothed out when the period of the input was set at 20 minutes. However, it is not guaranteed that the ESN output reflects on the actual response time scale because the ESN output tends to be smoother than the observed variation as shown in Figures 7 and 8. Other characteristics of the experiment seem to be consistent with the results which are already shown in the current version.

> *Line 94 and elsewhere: "ESN meets a satisfactorily high accuracy". I think the accuracy is overstated, as the model output seems to miss the amplitudes of a lot of AU/AL variations.*

We agree with the referee that the model output tends to underestimate the amplitudes of impulsive variations. In Line 94 and so on, we intended to just point out that the ESN achieve a comparable accuracy to existing models. We will modify the expression.

> *Table 1 and elsewhere in the text: It would be helpful to include correlations, as another measure of model performance.*

The correlation coefficients will be added in the revised version.

> *The paper needs to have more details in the Discussion or Summary regarding the relationship of the results with the dynamic pressure ($nV^2/2$). There is no mention of pressure, although it appears in several previous publications, such as Newell et al. [2007 and 2008], as related to the effects of the density. Interestingly, the results in Figure 9 seem to follow a V^2 curve.*

> *I think that two of the references cited by Newell et al. [2008] had indicated that sudden increases in dynamic pressure only produced on temporary response, in the magnetosphere [Boudouridis et al, 2005, Ober et al., 2007]. For example, the polar cap electric potential may increase for a while, then go back down to near the pre-impulse level. This temporary behavior complicates any search for a consistent relationship between the solar wind density and ionospheric response.*

We already mention that the dynamic pressure effect possibly explains Figure 9 (Lines 182-186). As we discussed in Lines 186-187, however, our opinion is that the dynamic pressure effect does not completely explain the compound effect between the solar wind density and velocity. As shown in the lower panel of Figure 9, the density effect on AL becomes zero on average when the solar wind velocity is around 300 km/s. If the density effect was totally due to the dynamic pressure effect, the density effect would be visible even under the low-speed solar wind conditions.

However, the upper panel of Figure 9 suggests that the density effect on AU is non-zero even if the solar wind speed is small. This result may be explained as the dynamic pressure effect. It would be possible that AU is related with the polar cap potential which is affected by the solar wind dynamic pressure as suggested by the referee. We will add the discussion on the possible relationship between AU and the dynamic pressure in the revised version.

> *In Figure 1, the graph showing the three IMF components is not clear. These should be put into three separate rows.*

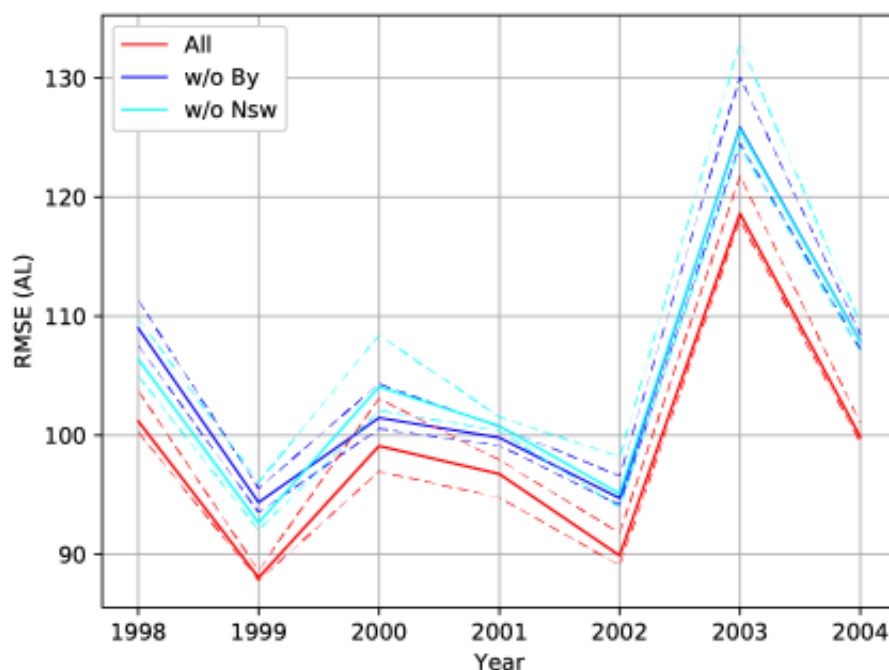
Our opinion is that the differences among the three IMF components are not very important in this figure. We just intend to show the correspondence between IMF fluctuations and auroral activities in this figure. It would be enough if the three IMF components can be distinguished in Figure 7 and 8.

> *Lines 98-99: This sentence is not clear.*

This sentence will be removed to avoid the confusion.

> *Figure 3 needs to be taller in order to help show the differences between some of the lines.*

We appreciate for the comment. We have prepared another figure to clearly show the effects of By and Nsw as below. This figure will be added in the revised version.



> I don't agree with the use of the word "sounding," and a different terminology would be preferred as the title sounds a little pretentious. In my opinion, this use doesn't agree with any of the multiple, dictionary meanings of the word "sound" or "sounding." Whether or not a change is made is entirely up to the discretion of the authors.

We will modify the title in the revised version.

> The web link for WDC for Geomagnetism, Kyoto isn't working, due to the line break. A different Latex package for URL references might work, or try putting the link all on one line without a break. This is a common problem encountered with URLs in Latex.

We appreciate the referee for pointing out the problem. We think that this problem can be fixed when typesetting the final version.

Please also note the supplement to this comment:
<https://angeo.copernicus.org/preprints/angeo-2021-54/angeo-2021-54-AC1-supplement.pdf>