

Interactive comment on “Comment on “Invariability of relationship between the polar cap magnetic activity and geoeffective interplanetary electric field” by Troshichev et al. (2011)” by Peter Stauning

Anonymous Referee #1

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This manuscript comment [Stauning 2020] identifies a defect in a paper by [Troshichev et al 2006]. Specifically, it is shown that the original analysis appears to have mistakenly used interplanetary magnetic field (IMF) vector components in the GSE reference frame instead of GSM as intended. It is not entirely clear how this error was discovered ("New analyses has disclosed ...") and whether it has been confirmed by review of the original code. Figure 1 presents an interval of solar wind data B_y and B_z magnetic field components in GSE and GSM coordinates compared with a similar plot taken from the original paper. Although the magnetic field components in GSM and GSM are very

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similar for much of this interval, they are distinctly different for several hours, during which it is clear that the the [TJS2006] solar wind plot is not of GSM but GSE instead.

[Stauning2020] notes that "The mistake had no strong impact on the remaining presentation of the PC index concept in [TJS2006]. Usually, such a mistake would not attract attention after the many years that have passed since the publishing in 2006. However, the incorrect feature drags a trail of erroneous relations and invalid statements presented in publications on polar cap indices issued since 2006 extending up to present (2020)."

This comment is primarily concerned with a study by [TPJ2011] which was thought to have inherited the GSE/GSM error from [TJS2006]. The goal of [TPJ2011] was to explore whether a single set of model coefficients could be utilized for all levels of solar activity. [Stauning2020] expected that any effect due to GSE/GSM errors might be evident as changes in the rotation angle of the local horizontal geomagnetic field (ϕ).

However, [Stauning2020] notes A problem for the analysis of possible effects of the invalid PCS scaling parameters derived in TJS2006 by using IMF components in their GSE representation is the unavailability of numerical files of the parameters. Instead, the colour-coded diagrams have been "manually" read-off to be converted to numerical files.

[Stauning2020] asserts that the results obtained by [TPJ2011] are in error and "It has not been possible to deduce the origin of the scaling parameters actually used for two PCS versions being compared in Figs. 6, 7, and 8 of TPJ2011". and "Furthermore, the "without QDC" curves are not derived from calculations of scaling parameters from the "with QDC" version just without using QDCs but are of indefinable origin".

[Stauning2020] presents a detailed comparison of several different parameter sets, concluding that the corresponding results in [TPJ2011] were inconsistent with the stated set of coefficients. Subsequent analysis leads to the conclusion that key re-

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sults are partially due to erroneous quiet day curves (QDC).

Finally, [Stauning2020] returns to the GSE/GSM discrepancy and concludes that it could result in quasi-random error of 1-2 mV/m under quiet to disturbed conditions and 10 mV/m or more during storm times.

In my opinion, this manuscript contains several noteworthy results and should be published. As a comment it should serve as a useful caution regarding the conclusions of [TPJ2011]. It also provides an opportunity for the authors of [TPJ2011] to present a detailed reply about their methods and conclusions.

That still leaves the question of how to provide a persistent warning regarding [TJS2006]. Ideally, the authors of [TJS2006] could write a correction or erratum in *Annales Geophysicae* which would appear in a citation search. Alternatively [Stauning2020] could be used as the basis for a second comment very similar to this one.

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