Comments on paper angeo-2018-82 "Analysis of Juno perijove 1 magnetic field data using the Jovian paraboloid magnetospheric model" by Pensionerov *et al.*

1 General comments

In this paper the authors present Jovian magnetic field measurements from the middle magnetosphere collected during Juno perijove 1 pass. The data are analysed in order to determine optimal parameters for the magnetodisc described by the semi empirical global paraboloid Jovian magnetic field model by Alexeev and Belenkaya (2005). This model consists of six components contributing to the total magnetospheric magnetic field (internal field, IMF and different current systems contributions).

In their analysis, the magnetic field data are kept untouched, and the principal contributions to the magnetic field in the observed region (middle magnetosphere) are assumed to be the internal field and the magnetodisc. Only two parameters of the four parameters to describe the magnetodisc are 'fitted' (while there are a total of nine parameters for the global magnetic field). These parameters are the radius of the inner edge of the disc R_{DC2} and the magnetic field at the outer edge of the magnetodisc B_{DC}, the other two parameters consist of Jupiter's dipole ψ (and is calculated as function of time), and the radius of the outer edge of the disc R_{DC1} (fixed to the value given by Alexeev and Belenkaya (2005) with data from the inbound trajectory of Ulysses).

Similar studies to estimate the magnetodisc's parameters according to a model have been carried for Jupiter (as well as Saturn) with empirical models such as the CAN disc (Connerney, Acuna and Ness, 1983) using magnetic data from various missions (Voyager, Pioneer, Galileo, Ulysses, Cassini). There are also detailed physical models such as Caudal (1986), and Achilleos, Guio and Arridge (2010) for Saturn to which magnetic data have been compared. This study is carried using magnetic data collected from the on-going mission to Jupiter, Juno. This could potentially contribute and add to the existing knowledge from previous work but I believe that the article in its present form is not acceptable for publication in Annales Geophysicae. But I would encourage the authors to resubmit their paper after implementing the revisions as proposed hereafter.

2 Specific comments

In an age where advanced nonlinear fitting programs and methods have never been so easy to access, I find it somehow not acceptable to 'characterise' the best fit of a multi-parameter fit model with a contour plot of the residuals for the two parameters B_{DC} and R_{DC2} (Fig. 3). I would recommend to try and use a standard nonlinear fitting program implementing a Levenberg Marquardt method or similar, that provides as well meaningful statistics like error estimates for the parameters. You might be want as well to try and fit R_{DC1} with such method.

Eq. 3 does not make sense in its present form. The numerator under the summation over measurement points is homogeneous to the square of a vector while a scalar is meant: the Euclidean vector norm. It is not clear what is actually fitted, the components of the vector (in what coordinate system?). Figures 4, 5 and 6 all show the amplitude of the magnetic field. It would be more meaningful to present the radial, meridional, azimuthal components and the amplitude of the residual magnetic field in order to identify the component that 'best' fit (the radial component?) and 'worst' fit (the azimuthal component due to the poloidal nature of the field/lack of bend-back model?).

In Figures 4 and 5 the observations at large radial distance exhibit large fluctuations. Do you have any explanation?. Does it make sense at all to include these data in the fit? Wouldn't it be better to smooth the data first? Also does Figure 4 (inbound) not suggest that R_{DC2} could be larger than $92R_J$ while in Figure 5 (outbound) R_{DC2} could be smaller than $92R_J$?

In Section 4, I would recommend to carry out a valid and fair comparison with the CAN disc by actually fitting the four parameters of the CAN disc, otherwise the comparison seems arbitrary.

Figures could be made slightly bigger in general. In Figure 1 it might be useful to add panels for cylindrical and spherical distance as well as local time.

3 Technical corrections

- l. 27, p. 2: it would be nice to elaborate on why the IMF better off neglected rather than considering a typical value.
- paragraph starting l. 12, p. 4: the discussion on the sensibility of S to the range of measurements considered for the fit is of prime importance. It needs some clarification and also expanded to justify the choice of range.
- l. 7, p. 5: what does the value $S = 0.2$ correspond to concretely in terms of statistics? As it stands it seems to be an arbitrary choice.
- l. 14, p. 5: I am not sure to follow the argument. What is it meant by 'acceptable pairs of parameters are aligned with the line to some extent'? Some clarification needed.
- l. 1-5, p. 6: the discussion about the discrepancies observed in the internal field is too vague and lack content.

- paragraph starting l. 2, p. 7: as mentioned before does it really make sense to take arbitrary values for the CAN disc parameters. Wouldn't it make more sense to carry a proper fit?

- l. 7, p. 8: what do you mean by 'magnetodisc models with azimuthal current dependencies different from r^{-2} should also be investigated'? The CAN disc model just used in that Section varies as r^{-1}. Do you have any suggestion? In Achilleos, Guio and Arridge (2010), it is suggested that the dependency is steeper than r^{-1}.