EGUsphere, referee comment RC1
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Comment on egusphere-2022-232

Anonymous Referee #1

Referee comment on "Impulse-driven oscillations of the near-Earth's magnetosphere" by Hiroatsu Sato et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-232-RC1, 2022

The paper builds on and analyzes the performance of a simple model of solar wind – Earth's magnetic field interaction. Consequences of a sudden pressure pulse in the solar wind for the dynamics of the system are then discussed and the respective variations are qualitatively evaluated. These are, in turn, compared with ground-based magnetometer and Van Allen Probes measurements. The claimed reasonable agreement is interpreted in terms of this simple model being, to the lowest order, sufficient to model the near-Earth magnetosphere.

I find the paper rather interesting, as such simple-model approach is quite rarely seen nowadays. On the other hand (or perhaps because of that), I have some doubts/questions concerning the model formulation and its comparison with the measurements.

Detailed comments

- Static limit in equation (1) and around: I feel this argumentation based on the pressure balance is well known. It would be more usual to have the solar wind dynamic pressure units (Figure 3) in nPa and to have the equation (1) in SI units. Also, it is worth noting that the -1/6 scaling resulting from this simple picture is often slightly violated in empirical magnetopause models, so I have some doubts about that "generally accepted" formulation.
- Equation (2) governing the assumed magnetopause oscillations: I believe that this is quite essential for the model formulation and should be better discussed and justified. First, what is the source of the inertia here? What typical values are found/considered? Do the typical speeds of magnetopause obtained here correspond to the observations? (these can be determined experimentally using multi-spacecraft measurements, Cluster

was used for that as far as I know). Second, the damping coefficient should be discussed better. It is said that it does not correspond to the dissipation, but is rather a result of the phase-lag in the mathematical formulation. Ok; but I would be hesitant to call this a "physical mechanism" – and the energy should perhaps still go somewhere (?)

- 125-135: People typically consider ExB drift to be negligible for the radiation belts particles, as for high energies grad-B and curvature drifts dominate. I have thus some doubts about the calculation here. How was Figure 10 obtained? For what energies? What pitch angles? The asymmetry of magnetic field should result in some drift-shell splitting. None of this is discussed/described (and considered?).
- 160-165: what are the assumed values of the density here? The relative densities of high-energetic particles will be comparatively very low. Also, the energization of the radiation belt particles is typically due to (inward) radial diffusion, which, in turns, decreases the azimuthal drift velocity.
- Comparison with observations: it remains quite unclear what the model can or cannot predict and how this match or does not match the observations. The sudden change of the magnetic field measured due to the increase of the Chapman-Ferraro current (and magnetopause moving to lower distances) at the time of the pressure pulse is well known. The model might be in principle able to predict the subsequent oscillation period (?) and attenuation of the magnetic field pulsations (?), but these are difficult to see in the data and some more elaborated comparison with the model output is missing. Instead, the shock parameters (not really too relevant for the model evaluation (?)) are described.
- There was recently quite a large number of papers dealing with the shock effects on radiation belts / magnetospheric plasma waves which seem to be quite ignored in the present manuscript (e.g., Sun et al. (2015), doi: 12014JA020754; Foster et al. (2015), doi: doi:10.1002/2014JA020642; Tsuji et al. (2017), doi: 10.1002/2016JA023704; Blum et al. (2021), doi: 10.1029/2021GL092700 – and most likely many others they cite/are cited by).
- 295: This configuration of the three dipoles should be better described already in the beginning, not just here in the conclusions. The claimed "good agreement" between the model and observations is not really demonstrated.