The referee's comments are reproduced below, with authors response (in italics) to the separate points. Corrections in the manuscript will be marked in red. Deletions are not specified. The revised manuscript, with correction in colour, will be made open when both sets of referee comments have been taken into account.

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.

Authors response: yes, the basic scaling is well known as also stated in the paper. It can be derived from dimensional reasoning (see Appendix B) but the numerical coefficient is found empirically in other studies. The present simple model gives it analytically, we believe this is new, although this is not a major new result. The referee's remarks that the 1/6 scaling is violated by observations. Such a result will be dimensionally wrong and sounds strange if the deviations are significant. We would appreciate more information from the referee. As shown in Appendix B, the 1/6 result is the only one that is dimensionally correct with the given parameters, so if this law is violated there must be parameters missing: this would be an important observation even if we can not specify what is missing. Comments on this are inserted in the manuscript. We remade figure 3 to have the horizontal axis in nPa. Equation (1) is actually in SI-unuts and this is made more clear now.

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 (?)

Authors response: The damping is caused by the asymmetry in the solar wind pressure: when the magnetopause is approaching (i.e., moving away from the Earth) the magnetopause is doing work on the solar wind, while in the receding phase it is opposite. The two cases are not symmetric since the ram solar wind pressure depends on the relative velocity between the solar wind and the magnetopause. In the approaching phase this force is large, while it is smaller in the receding phase. The work done in the two oscillation phases is different. The net result is a loss of energy from the oscillation. The initial transient time interval is different: here the solar wind pulse or shock arrives at *an interface at rest, and the oscillations are initiated to reach full amplitude. This explanation is now inserted in the text. The magnetopause inertia is discussed in more detail and references to observation and simulations are given.*

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?).

Authors response: we included the ExB drift of magnetic field lines (in an MHD sense) in the radial direction: Yes, perpendicular to B (evidently), the gradient B and curvature drifts are the important ones. We had a short discussion in Appendix C. In terms of MHD plasma dynamics we have the magnetic field lines moving with the ExB-drifts, the particles move with their respective dynamics on these magnetic field lines. The text is clarified a little more on this.

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.

Authors response: the mentioned decrease in azimuthal velocity was discussed in some detail in Appendix C, but is now emphasized in the text. Due to the compression of the sunward part of the Earth's magnetic field, the estimate of charged particle velocities based on a magnetic dipolar field will only serve as a guideline, but in principle we agree with the referee. The text is improved concerning the density of heated particles, but all we can state here is that the fraction is low, as argued by the referee.

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.

Authors response: we believe it is important to quantify the two shocks (the differences are significant), their relative strengths in particular, so some effort was made to derive and present these data. The Conclusion section contains a summary of features not *covered by the present model.*

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).

Authors response: unfortunately, the referee is quite right here: these references were missing and we have no objections to including them, in particular also because they are quite recent. On the other hand, they are not essential for our analysis. The DOI-number for Sun et al (2015) was incomplete, but hopefully we found the correct reference. The amount of literature on the subject is vast and the main contribution of our work may be the simplicity, yet usefulness, of our model.

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.

Authors response: in principle the referee is of course right, but we should emphasize that in reality there are no objective measures for good agreement, only for no agreement. Given the simplicity of our model we find that it is able to predict surprisingly many features of the geomagnetic disturbances induced by solar-wind shocks.