

Interactive comment on ““Earth-like” planetary magnetotails as non-linear oscillators” by Robert J. Burston

Robert Burston

arbieroo@gmail.com

Received and published: 24 May 2020

Response to the initial, general comment: “Authors argued that the dynamics of “Earth-like” magnetotail can be studied by analogy with the movement of mechanical spring, but the model parameters for the run are set artificially (see table 1- table 6), which have nothing to do with the planetary magnetosphere. So, I cannot see any application to the planetary magnetotail, and cannot recommend publication.” The connection between the spring model and “Earth-like” magnetotails is discussed at length in section 1.3. I am not the first to make this analogy, as referenced in the first sentence of section 1.3.1. In lines 190-193, I clearly state that the model is qualitative, not quantitative: ‘The new model. . .cannot offer quantitative results, the strong analogy with plasmoid formation and release allows for examination of the range of possible qualitative dynamics

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that can be expected in Earth-like magnetotails.’ Hence the reviewer is simply ignoring the entire argument set out in the Introduction, that the study of analogous systems is normal and fruitful in physics; has been used in the specific context of Earth’s magnetotail before; that qualitative dynamics are a useful area of study. The reviewer does not make an actual argument supporting their point of view, but instead disguises the lack of such an argument by using the word, “so. . .” There is no real logical connection between the first and second sentences quoted above. There is no attempt to refute the argument that there is an appropriate analogy between leaky taps, non-linear springs and Earth-like magnetotails, an analogy that, I re-iterate has been used in previous publications by respected authors (Hones, Baker.)

Comment 1: Section 1.3.2 deals with previous work on analogies to magnetotail dynamics and therefore, far from being “irrelevant” is extremely pertinent to the matter at hand. Section 1.3.3. explains why studying analogous models that are simpler than the real system is a worthwhile activity and is, again, crucial, not “irrelevant.” The section (1.4) on qualitative dynamics is included because the paper is about qualitative dynamics – a topic that, in my considerable experience of talking to space scientists about it, is not one that it is safe to assume is common knowledge at even an introductory level. Hence I explain the principles needed to understand the results of the paper, in the Introduction to the paper, so that readers have a reasonable chance of understanding my conclusions without resort to a text on dynamical systems theory. Again, essential, not “irrelevant.” My motivation for the study is covered extensively already, in sections 1.1 through 1.3.

Comment 2: “The analogy is a good way to insight the dynamic physics of magnetotail.” This contradicts what is said further down: “The simple or casual analogy makes no sense to understand the magnetotail dynamics?” Does the reviewer think the analogy is good or bad? No clear argument is presented either way. The physical reason for using the analogy is that the spring model presented is a relaxation oscillator and plasmoid release from Earth-like magnetotails qualitatively behaves like a relaxation

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oscillator. This is explained in Section 1.3.1. The extent to which the model can account for Earth-like magnetotail dynamics is this: Qualitatively, only. But at that level, very well. See sections 3 and 4. Additionally, the model is intentionally “simple”, see lines 69-74 and section 1.3.3. It is not, however, in any way “casual.” Comment 3: The author did not “just” do anything. The reviewer again disguises the lack of a real argument, this time using “therefore,” instead of, “so.” The reviewer makes absolutely no attempt to explain why these qualitative results are invalidated simply because they are not quantitative. I make no claim that they are quantitative; I claim that, because of the analogy that the reviewer makes no attempt to refute, the behaviours seen in the model can be expected in Earth-like magnetotails. This paper is entirely inappropriate to journals specifically about non-linear processes because it innovates in regard to planetary magnetotails, not analysis of non-linear systems.

Comment 4: The reviewer confuses inputs and outputs: The model has separate drivers for the Dungey and Vasyliunas Cycles, as explained in section 2.1. These are inputs. I do not claim or even imply anywhere in the paper that the model has separate outputs for each cycle. This is because it does not. I do have evidence that that the model can be driven through the same kind of limit-cycle-to-chaos sequences as shown in the logistic map orbit diagram, however a clear demonstration of this would extend an already long paper by a very considerable amount so the necessary results are not shown, beyond the fact that simply increasing $C_d + C_v$ can switch from limit-cycle to chaotic behaviour. This is recognised already in lines 429-434.

Comment 5: Line 45 does not even mention the Dungey cycle, let alone suggest that it is not present in Earth’s magnetosphere. My understanding is that substorms follow a flip to southward IMF. This can be short-lived compared to the timescale of a complete substorm and the IMF can have flipped back to Northward before the substorm is complete. Hence conditions of southward IMF are essential for substorms, but they do not have to last the entire duration of a substorm. I have modified lines 45-49 to reflect this.

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Interactive comment on Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2020-12>, 2020.

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