

Interactive comment on “Interplay between linear, dissipative and permanently critical mechanical processes in Arctic sea ice” by A. Chmel et al.

Anonymous Referee #1

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I found this manuscript very strange and disappointing. The subject concerns, as far as I can understand, the critical character of sea ice fracturing, discussed from a very limited (see below) dataset of tiltmeter signal. Section 2, which represents in length about one half of the manuscript, is a sort of introduction about (i) Coulombic failure of sea ice and (ii) some words about the self-similar character of sea ice fracturing patterns as well as a vague, obscure section about “non-equilibrium systems”, “criticality”, or “self-organized criticality”. This section adds nothing to the current state of the art, and misses a vast amount of recent literature about these subjects (e.g. about Coulombic faulting, the recent book of Schulson and Duval is a good starting point). Section 3 starts with some generalities about elastic waves related to sea ice fracturing. Once again, a vast amount of literature, going back to the 60’s, is omitted. Then, the authors present on fig. 3 a potentially interesting result about the relationship between in-situ

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stress data and tiltmeter data. However, this figure is extracted from an old PhD thesis (in Russian), and gives only one particular example. A much more thorough quantitative analysis would be necessary (and interesting) to interpret the tiltmeter records in terms of fracturing. The “core” of the paper focuses on one 15-days tiltmeter record shown on fig. 4. From this particular record, “events” are defined, using a thresholding procedure that is briefly mentioned. However, if one can understand the procedure used to define the beginning of an “event”, nothing is said about the definition of the end of this event. This can have an impact when looking at the intermittency of the record (section 4). From this filtered record, the authors obtain a power law distribution of energies, fig. 5. The main drawback here is that, as the data come from only one tiltmeter, without localization of the source, the effect of wave attenuation with distance from the source on the events amplitudes is not taken into account. Therefore, the distribution shown on fig. 5 could simply reflect a spatial distribution of potential sources, some being close to the tiltmeter (so, with high amplitude), some being far. In addition, a much larger dataset would be needed to draw any kind of generic conclusion. The very short section 4 considers “time invariance”. Intermittency is obvious on fig. 4. However, the distribution shown on fig. 6 could be disturbed by the way the “end” of the events are defined (or not, see above), especially for time scales below 100 s. But the main problem here concerns the possible origin of the intermittency of the signal: the authors argue it is related (only) to the fracturing process itself. However, how can we discriminate this from the effect of the intermittent character of the forcings (and especially wind forcing) ? The limited time series of fig. 4 suggests sequences of high activity about every week, i.e. corresponding roughly to the synoptic time scale. The much longer section 5 is a vague discussion about ill-defined (in this manuscript !) concepts such as “SOC”, “profitable (!!) attractor”, chaos, “tuning parameter”, ect. . . The use of “order parameters” in L25 of p1440 seems particularly misleading (an order parameter does not “control” an evolution).

The manuscript is full of such vague, ill-defined sentences, e.g.: L1-3, p1435: where is “thermodynamics” in the data discussed here ? L13, p1435: what is “semi-brittle”

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? L2-3, p1436: what is “cohesion flow” ? L24, p1436: what is the exponent β ? L4-8, p1437, and the entire following section down to section 3 L7, p1441: “parametric interactions” ? L15, p1441: “profitable attractor” ?

For all these reasons, I don’t think this manuscript is suitable for publication in “The Cryosphere” (or in any serious journal, at this stage).

Interactive comment on The Cryosphere Discuss., 4, 1433, 2010.

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