Ocean Sci. Discuss., 6, C335–C338, 2009
www.ocean-sci-discuss.net/6/C335/2009/

Ocean Science
Discussions

Interactive comment on "Variability of scaling time series in the sea ice drift dynamics in the Arctic Ocean" *by* A. Chmel et al.

Anonymous Referee #2

Received and published: 12 August 2009

This paper presents analysis of a time-series of the position of the NP-35 ice floe during Spring 2008. The work follows on from previous GPS analysis published by the same authors from previous NP camps.

First I must mention that I found the paper hard to read and understand because of the language style. Many sentences in the paper could be rearranged and shortened to get their message across in a more straight forward manner. I also found that the paper lacks details for full understanding of the authors methodology. In particular the concept of 'waiting time' was not properly introduced. I know that the authors have introduced this in previous papers, however this was not even referenced in a way that would help the reader.

Aside from the language problems, that can be fixed, I found this to be an interesting

paper. However, the paper requires much clarification. I also have some concerns about your error estimate and methodology.

Specific comments:

This paper is about "the motion of an individual ice field". Did you mean to say "ice floe"? You can not define the motion of an ice field with a point measurement.

Abstract: "These data were put in comparison with the "waiting times" statistics that is with the distributions of lengths of time intervals between subsequent important local accelerations of the ice field"

What do you mean by "important"? On reading the paper, I remain confused as to how you decide to threshold accelerations, and so how you determine what an important event is.

Page 1597, "the great pack fragmentation occurred in the region of NP 32 activity". This event needs to be outlined in more detail. Only those who follow ice camp news will remember this event.

Figure 1. I am glad to see you have made an honest attempt to estimate your GPS position error at the unique location of the ice camp. It is very good to do to estimate error in-situ as multi-path, satellite configuration and local climate all play a role in this. My criticism of your method is that you do not account for ice floe rotation. The two components of distance between your master and slave GPS should not be considered separately if you do not account for floe rotation. I wonder if this rotation can cause the elongation (ellipse) that you observe in the scatter of x/y distances. Your method does provide an estimate of error for the absolute position (distance between antenna). Remember that you are estimating twice the error - this is not clear in the text.

Figure 1. Why do you plot two ellipses and what is the meaning of the grey shaded area?

Error estimate: How does the position error propagate through your velocity and accel-

eration estimates? Does this affect your choice of acceleration cutoff?

Page 1599. You do not describe how you estimate velocity or acceleration from the time series of positions. Do you use up-wind or central differencing for example. Equation 1 is not sufficient by itself.

You do not define "waiting time" in a clear understandable way.

Page 1599, "Only the values V that exceed the standard error of velocity (table 1) 5 times or more where taken into account in the subsequent data analysis". Okay, so does this mean you remove quiescent periods from you analysis? Why do this? I understand that during these stationary periods the GPS noise is greater than the ice motion. Does this result in spurious accelerations in the time series, which would affect your analysis of waiting time.

I think my misunderstand stems from the unclear language in this manuscript. Clarification on your quality control in a paragraph that is separate from the methodology might help.

You do not define "waiting time" in a clear understandable way. My understanding is that this is the time between acceleration values that pass your A_cut-off filter. How do you define A_cut-off? Are you results robust to the choose of A_cut-off? I would expect there would a lower value of A_cut-off that will be affected by GPS noise, but above this value, are the results robust?

To understand your interpretation of the results, and choice of 5 time intervals (A-E), it would be helpful to include time series of acceleration and MSLP in figure 3. The MSLP time series will help the reader orient to the changing synoptic conditions between intervals A-E.

Regarding the "time multi-fractality", I would like to mention that another hypothesis that could explain this is that the ice pack has two preferred directions of cracks (conjugate pair). The forcing on the ice pack or anisotropic ice strength (which is related to the

position of the cracks) might result is differencing wait time distributions.

I find it interesting that interval B has the largest value of delta. This is during a time of "excitement". I think further comment is warranted here. Was the ice closing at the time. What was the proximity of active leads and ridges to the GPS (closer than during other times?)? Was a particularly energetic storm system involved? Or was there a large fetch resulting in much larger ice stress at the camp?

It is quite interesting that the distribution of wait times changes between the 5 intervals. The analogy with the slider-block model is interesting in providing a hypothesis for why these differences are observed. I believe this observation is worthy of being published.

Looking forward to your response.

Interactive comment on Ocean Sci. Discuss., 6, 1595, 2009.