Novel methods to study sea ice deformation, Linear Kinematic Features and Coherent Dynamic Clusters from imaging remote sensing data by Polona Itkin

General comments
This revised version of the paper is much easier to follow and the novelty of the CDC's is evident. I appreciate the author taking the time to explain things more clearly. I still think the validation/comparison could be simplified more for casual readers but perhaps that is just my unfamiliarity with that approach. I did notice many type-o's throughout that require attention. I noted several of them but a more thorough proof-read is required to catch them all – this is very minor. Overall, this is a strong contribution to the sea ice dynamics field.

Stephen Howell, ECCC

Specific Comments (mostly type-o's and I am sure there are more).
Line 20
Remove "does"
change increase to increases
provide to "providing a"
obstruct to obstructing

Line 33
Replace deep with better

Line 149
Replace Figure ?? with the correction Figure reference.

Line 156
LKF has already been defined.

Line 169
LKF has already been defined.

Lines 196-197
Move "were" to after "study"

Editor
This paper has some good elements. The CDE framework is novel but poorly defined and this needs to be revised. The power law is useful but I found it a challenge to follow. I see no reason why simple buoy comparison statistics cannot be used as well. I think readers will struggle with many sections of these paper in terms of readability. Overall, this paper has new elements but the presentation needs to be improved.

Novel methods to study sea ice deformation, linear kinematic features and coherent dynamic elements from imaging remote sensing data by Polona Itkin

Summary
Automatically identifying sea ice dynamic features is challenging. In this paper the author presents several new methods to estimate several dynamic features from SAR imagery using the N-ICE2015 study period as a test case. I like the ideas and methods presented in this paper and they certainly add the understanding of sea ice dynamics. This paper has new elements however, I found some sections and items challenging to fully grasp. I think this paper can be published it just requires some revisions to improve its presentation (readability, clarity, etc.). I hope my comments help the author improve this work.

General Comments (major)
1. I really like the idea of CDE's but their definition is a bit confusing. It seems to me CDE's are an architecture or framework or terms (not term singular) that certain variables can be used to collectively describe winter pack ice. Am I right? However, you first define Coherent Dynamic Elements (CDE) as the boundary of rigid ice plates (Line 58 and 59). OK. In the Abstract you say CDE describes the behaviour of the winter pack but nothing in the paper including your Conclusion relates the winter pack behaviour during N-ICE2015 in that context. I thought I was missing something. Further, if a new term is introduced, then the definition must be consistent. Your definition and usage of CDE needs revision throughout the text otherwise readers will be scratching their heads as its meaning and usage. I suggest defining the CDE framework (with associated variables) earlier in the paper and explicitly describe how these terms can be used collectively to understand winter pack behaviour with evidence from N-ICE2015.

2. I understand why the power law was employed for accuracy/quality assessment but it is not the easiest section to comprehend. Perhaps it is my ignorance. Nevertheless, I think this section needs to be revised as casual readers will struggle – I did. I see is no reason why a simple buoy to SAR deformation comparison cannot be performed. The buoy data is available from the lead

author (Itkin et al., 2015). Further, the two-way comparison is far more useful anyways and what casual readers will be look for. I think the power law quality check metrics can still be included but the author needs to add some additional "bread and butter" comparison statistics for casual readers.

3. On Line 10 you state, "Our results revealed a cyclically changing winter sea ice cover, marked by synoptic events and transitions from pack ice to the marginal ice zone." However, this really was not investigated in the paper. There is no synoptic data in the paper. Again, casual statements like these will leave readers confused because this analysis is nowhere to be found in the paper. Why not add some supporting synoptic data (spatially) to make the manuscript more comprehensive?

4. There are so many acronyms and notation that the reader often forgets or has to refer back to what the definition is. There is nothing wrong with spelling things out in full and in fact it makes your paper more accessible to casual readers. Considering removing some of the notation for text.

Specific Comments (minor)
Line 19
What implications? A good to idea to state what they are i.e. For example, …

Lines 22-25
How can increased deformation erode the long-term memory of ice thickness? As I read Mitch's paper he and co-authors state predictability is lost with the onset of melt. Or are you suggesting winter-time deformation will complicate winter ice thickness retrievals? You need to be explicit about the link between deformation and seasonal prediction.

Line 49
Those are not really references related to RADARSAT-1 and RADARSAT-2. I suggest the following:

Mahmood, A., Crawford, J.P., Michaud, R., and Jezek, K.C. 1998. "Mapping the world with remote sensing." Eos, Transactions, American Geophysical Union, Vol. 79(No. 2): pp. 17, 23

Z. Ali, I. Barnard, P. Fox, P. Duggan, R. Gray, Peter Allan, Andre Brand & R. Ste-Mari (2004) Description of RADARSAT-2 synthetic aperture radar design, Canadian Journal of Remote Sensing, 30:3, 246-257, DOI: 10.5589/m03-078

Line 53
I think the RGPS has some done a lot more than derive scaling laws and intersection angles with respect to understanding sea ice dynamics.

Line 54-55
The spatial resolution of "deformation estimates from SAR" has been…

Line 62-65

Redundant. You just stated most of this information in the previous paragraph.

Line 70
You already defined SAR.

Line 78
As with previous comment

Line 99
How where the SAR images pre-processed? Were they calibrated? I think some details on this is required.
Line 189
See General Comment #1.

Line 378
The Conclusions do not really match (are missing) some of the items presented in the Introduction.

Line 400
Can something be said as to the applicability of these techniques to summertime conditions? Or are these strictly limited to the winter time?

Figure 2 and 3:
Probably a good idea to note in the Figure Caption the artifacts or bad data presented (Line 145)