

## ***Interactive comment on “Results from the DAMOCLES ice-buoy campaigns in the transpolar drift stream 2007–2009” by M. Haller et al.***

**Anonymous Referee #2**

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My main comment concerns the false interpretation (in my opinion) about tidal oscillation triggered indirectly by wind storms. First of all wind stress is the main driving force generating inertial oscillations (not tides).

Second the argument presented here in favor of tidal oscillations is very weak. It is said page 3760 (lines 23 to 25) " it cannot be distinguished whether the oscillation if of inertial or tidal origin since both have almost identical periods". That is correct. "The weeks-long duration of the oscillations speaks more for a tidal oscillation" Why?

I suggest the authors should read and refer to two recent papers published by Gimbert et al (a group working at LGGE in Grenoble). The first paper was published in JGR in 2012 and deals with " Recent mechanical weakening of the Arctic Sea Ice cover as revealed from larger inertial oscillations" Vol 117.

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The second paper is actually part of the DSI and deals with "Sea Ice inertial oscillation magnitudes in the Arctic Ocean". Both papers are published by Gimbert as a first author.

I am quite convinced that what Haller et al are describing is more likely relevant to inertial oscillations rather than tidal oscillations.

Anyway there is no real argument presented in this paper in favor of tidal oscillations rather than inertial oscillations due to wind forcing and this should not be presented as a concrete result both in the abstract and in the conclusion of this paper. It is much too vague and controversial at this stage.

There is a lack of important references in this paper.

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Interactive comment on The Cryosphere Discuss., 7, 3749, 2013.

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