

## ***Interactive comment on “Variability of light transmission through Arctic land-fast sea ice during spring” by M. Nicolaus et al.***

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*Made on a cloudy December morning.*

Dear editor and authors,

Sorry for being slightly late, it was not on purpose.

This is a nice little and concise paper on original under-ice light transmittance measured using a very smart method. Hence, it has to be published. Besides, I enjoyed reading it!

I have a few comments to improve the paper, which together constitute a minor revision.

Best wishes

PS: I haven't read the other reviewer's comments before posting my own.

## Main comments

Here are the key points that have to be addressed to improve the paper

1. I would have liked a table synthesizing measurements of Ed, Et, T, for each of the three measurement sessions, declined wrt mean, max, min, mean, mod and std. Presently, in order to get the synthetic information, the reader has to sneak through the text, it is not comfortable. Such a table would make sections 3.3 and 3.4 easier to read.

2. In order to have a better view on the seasonal changes in light transmission, the authors should discuss the role of the increasing length of the diurnal cycle. Presently, one could believe that the under-ice light climate is not that far from March to May. But the diurnal cycle should contrast things a little bit more. There are several solutions to address this comment: 1) discuss with one or two sentences what could be the impact of the diurnal cycle; 2) make an estimate of daily mean under-ice downwelling radiation flux for each session of measurements, using a diurnal cycle distribution. Choose the one that best fits with what is feasible.

Besides, the time of the day at which the radiation measurements was not clear either, I would have liked to see it. Maybe I missed it.

3. There is a problem in the reporting of your chlorophyll measurements. Sometimes you use "mg/m<sup>2</sup>", sometimes "μg/m<sup>2</sup>". There is a 3 order of magnitude difference in your reported values.

I saw 0.5, 2, 3 mg chl-a / m<sup>2</sup> p. 4370, line 10-11

But the range is 0.33-3.82 in Table 1 page 4380 in μg chl-a / m<sup>2</sup>

Then, page 4375, again you switch back to μg chl-a / m<sup>2</sup>. Please make sure what are your values, and consistently use mg chl-a/m<sup>2</sup>.

4. To complement my comment number 3, I would refine your chlorophyll terminology.

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You could follow Meiners et al GRL2012.

→ chlorophyll concentration for a value in in mg/m<sup>3</sup>

→ chlorophyll content for vertically integrated values in mg/m<sup>2</sup>

If you find that the method to compute chl-a content is problematic in your paper, the method for computing integrated chl-a is also explicited in Meiners et al. Try not to use chlorophyll concentration for values in mg/m<sup>2</sup>, it is confusing.

### Specific and picky comments

Please check what the order of references should be in EGU journals, either chronological or alphabetical, it is presently inconsistent through the paper.

The order of figures does not follow their order of appearance in the text, please correct.

#### p. 4365

I. 17 "This is, because" -> "This is because".

I. 25 you could add a few words on under-ice blooms (Arrigo et al, Science 2012; Mundy et al., GRL 2009). I think that is well correlated with the focus of your paper!

#### p. 4366

I.1 Add "for photosynthesis" after "primary energy source"

I. 15 "with variability of a factour four around the mean" -> please be more precise, using "standard deviation": variability is ill-defined.

I. 17 "using divers" sounds weird to me, or at least somewhat dictatorial. Use "performed by divers"?

I. 24 "presented" -> "present"

#### p. 4367

I. 20 tell a few words on how you do that, it is still ambiguous. Which depth. Be more precise.

I. 23 I don't know what is an avalanche transmitter - can you briefly say how that works; and what is the advantage to use such a transmitter.

§starting line 22 i think this method is very smart and would deserve a little drawing to explain it. The buoyant frame is the key nice thing and is not stressed enough as the key advantage of your method.

**p. 4368**

I. 5-7 Sentence is kryptic, I don't get it. What do you mean by "tagged". How do you then mark points at the snow surface being sure that what the position you sample is the same as the under ice measurement. Does it occur after each point measurement or do you do that 1.5h later, once the full measurement session is over?

**p. 4370**

I. 8 what is "less pronounced lamellae depth"

I. 17 Please check the order of magnitude comparison with the values of Mundy et al 2007. It could be only one order of magnitude difference.

**p. 4372**

I. 15 even IF

**p. 4373**

I. 9 "expected not to increase" ?

**Conclusions**

I would have liked to see a few more things in your conclusions.

1. How far are we from the quantification of a seasonal cycle of under-ice light climate

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? Is it already clear from previous publications? Does this paper adds a contribution or not? Is the sampling of data presented from that paper sufficient or not ?

2. Elaborate if you can on the role of seasonally changing cloud cover and diurnal cycle length. If you cannot say anything, say how those two factors would contribute to variability and seasonal variations in Et.

3. Draw clear conclusions on the consequences of your study for observing systems. There are a few things, but I would have liked a little bit more. Do we systematically need under-ice surveys to complement autonomous stations ? Or Do we need only snow depth distributions estimates ? Or can we know a priori what is the variability on Et, just by knowing the time of the season ?

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Interactive comment on The Cryosphere Discuss., 6, 4363, 2012.

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