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

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Received and published: 10 December 2012

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.
This is indeed a good suggestion. We have included such a table.

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.
Thanks for that comment. We agree that the aspect of diurnal cycles was not well represented in the original version. We have put more emphasis on it in the results, discussion and conclusions.

3. There is a problem in the reporting of your chlorophyll measurements. Sometimes you use "mg/m2", sometimes "_g/m2". There is a 3 order of magnitude difference in your reported values. I saw 0.5, 2, 3 mg chl-a / m2 p. 4370, line 10-11
But the range is 0.33-3.82 in Table 1 page 4380 in _g chl-a / m2 Then, page 4375, again you switch back to _g chl-a /m2. Please make sure what are your values, and consistently use mg chl-a/m2.
We modified that, see comments below.

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

could follow Meiners et al GRL2012.
–> chlorophyll concentration for a value in in mg/m3
–> chlorophyll content for vertically integrated values in mg/m2
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/m2, it is confusing.
Thanks for highlighting this. We adapted terminology and units through the entire manuscript accordingly.

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
l. 17 "This is, because" -> "This is because".
Corrected

l. 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!
We added a comment including under-ice blooms

p.4366
l.1 Add "for photosynthesis" after "primary energy source"

l. 15 "with variability of a factor four around the mean" -> please be more precise, using "standard deviation": variability is ill-defined.
Perovich et al. (1998) state that transmittance ranged from 0.2 to 0.7%. However, referring to their Figure 4, the range is more from 0.05 to 0.8% with a mean around 0.2%. This is how we derived a “factor of 4 around the mean”. We now decided to simply use their range, but read from the figure, as we do it for our data, too:
“Perovich et al (1998) show total transmittances between 0.05 and 0.8% during April, showing a high variability.”

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

l. 24 "presented" -> "present"
We stick to “presented” as this ms presents it.

p. 4367
l. 20 tell a few words on how you do that, it is still ambiguous. Which depth. Be more precise.
We edited this passage to be more precise

l. 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.
We edited this passage to be more precise

§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.
We have added some more details on this methodological paragraph and also included the new approach in the discussions.

p. 4368
l. 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?
We edited this passage to be more precise

p. 4370
l. 8 what is "less pronounced lamellae depth"
We edited this passage to be more precise

l. 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.
The Mundy et al (2007) values range up to 100 mg m-2 with most values between 20 and 60 mg m-2. We changed to "more than one order of magnitude"

p. 4372
l. 15 even IF
Ok

p. 4373
l. 9 "expected not to increase" ?
We edited this passage to be more precise

Conclusions
I would have liked to see a few more things in your conclusions.
Thanks for all these constructive comments and questions. We appreciate them very much and included all aspects into the conclusions.

1. How far are we from the quantification of a seasonal cycle of under-ice light climate? 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 ?
With this study we give a contribution to increase our understanding of spatial variability of the under-ice light climate, at least for spring. The study shows that the spatial variability is a key issue, as it is large. But also large, may be even larger impacts are expected later in the season when melt ponds form over large areas of the Arctic Ocean. There are some time-series data from MYI regimes, SHEBA and Tara, but much more data from different ice regimes are needed. In particular there is a need for more studies that combine the aspects

of seasonality and spatial variability. This could be done in future through repeated transects during drifting stations (currently there are several initiatives discussed) or monitoring work. Those measurements should be complemented by more autonomous stations, where each single one give a time series for specific conditions, but combining them will allow to include aspects of spatial variability.

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.
Clouds are known to be one of the greatest uncertainties in all seasonal and large-scale (mostly numerical) studies, and we can not elaborate and answer this. Diurnal variability is more important for biology than for physics. We have added some comments on the increasing length of the day into discussion and conclusions.

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?
It would already be a great achievement if we would have good snow depth (better even some more snow properties like density, temperature, grain size) estimates for the Arctic, including its spatial distribution and seasonality. Because as long as snow is optically thick, it controls the light regime in and under the ice. But once ponds are formed these make a greater impact on energy that is transmitted. It would be useful to extend studies like in Nicolaus et al. (2012, GRL) to get towards larger pictures. In particular, data during different seasons (April to July) are missing, data from thin ice classes, and seasonal ice regime in the Arctic basin (Perovich et al., 2012). One other aspect is to get more affordable systems for larger observational networks (drifting observatories) and connect them to derive spatial variability, too (see also above).
Knowing incident solar irradiance at the surface is much more challenging as it might seem to be. Again, clouds are a major issue here (see also comment above). However, using estimates for surface irradiance and paramterizations of light transmission will help to generate better estimates.