

Interactive comment on “The multiphase physics of sea ice: a review” by E. C. Hunke et al.

Anonymous Referee #2

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First let me say that I am retired and no longer have ready access to many of the more recent papers referenced in the above paper. I stopped trying to keep up with the literature at the end of 2008 when I finished working on my book. Also I am basing my comments on the original version of the paper; not on a version responding to Ackley's review. For starters, my view of this paper is more charitable than was Ackley's.

1950-5 cannot be incorporated into lattice sites in the pure ice component of sea ice

1950-7 temperature, salinity and gas content)

1951-4 temperatures

1952-3 any effect must probably be very small.

1954-13 delete certain

1954-20 delete for example

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1955-22 what does order O mean????

1958 A listing of symbols + definitions+ units would be useful.

1960-5 Gravity is always present in the cases you are considering

1965 17-20 At present there are no good data available to check Wells' spacing predictions

1966-14 The comment re Petrich is true but so what???

-27 I don't recall Norbert considering changes in the size of brine pockets. Better check.

1967-26 If you use Schwarzacher's salinity profile its only for MY ice

1968-19 We only considered FY ice so there was no reason to consider flushing. You make it sound as if we screwed up.

1969-1-2 The Cox and Weeks results were not for the skeletal layer as we could not resolve that. I forget exactly but the values we reported were for ~3 cm into the ice (at the top of the skeletal layer). Although it appears that there is no compositional discontinuity at the sea ice –sea water interface, there definitely is a physical discontinuity. It definitely is a feathered interface. Someone needs to do some careful labworkala Harrison and Tiller (1963). Not me, I just play music.

1971-26 Hardly surprising as the water of the Southern Ocean is saltier.

1974-20 I do not believe this statement to be true. If something is lacking be specific as to what it is. You are just blowing hot air.

1975-24 This paper is supposed to relate to climate modeling, yet here you

are worrying about details with spacings between a mm to a few m??? I find this to be odd.

1976-1-2 This is not hard to do BUT it requires a MAJOR input of field time over a whole season.

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9-17 The techniques you are discussing here have a long way to go before you can relate them to the sort of internal structure changes you are discussing. Also on some of these techniques there is no available theory to use in interpreting the results.

1978 4-13 Delete this whole paragraph

General Comments

The paper is a reasonable review of the current status of mushy layer theory as applied to sea ice. Where this paper is weak in my view is when it tries to tie MLT to either climate modeling or to the biochemistry of sea ice. Perhaps the bio link can be advanced further once we have a lot more data, but I do not see how MLT is going to be of much help. As far as climate is concerned, using MLT strikes me as absurd overkill even if everything about it proved to be right on target. You are worrying about the 3 decimal place when you don't even know the number in front of the decimal point. If I were a climate modeler I would see if I could build a couple simple equations to use based on some of Kovacs studies using average salinity profiles. One simplification that is in process of occurring as we speak is that the amount of MY ice is getting to be less and less. If this trend continues we will only have to worry about the many thicknesses of FY ice that Make up the pack.

Interactive comment on The Cryosphere Discuss., 5, 1949, 2011.

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