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The Cryosphere Discussions

Interactive comment on "Thermal conductivity of anisotropic snow measured by three independent methods" *by* F. Riche and M. Schneebeli

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1 General comments

Riche and Schneebeli (2012) provide and discuss data from experiments and numerical computations related to the effective thermal conductivity of snow. Indeed, the existing spread between existing methods questions the validity of some of the data reported in the literature, and this topic deserves attention from the community. This publication fits very well with the scope of **The Cryosphere**. The text, however, needs considerable improvement before the article can be published in the peer-reviewed literature. In particular, the authors must better indicate what is the new information brought by this study, and what consists in a useful confirmation of results previously

reported. In addition, during the review process a discrepancy between the figures and the online supplement arose, preventing some of the review work and potentially spreading erroneous data in the literature.

2 Specific comments

As a member of a team involved in measurements of the effective thermal conductivity of snow using various means also used in the present study (Morin et al., 2010, Calonne et al., 2011, Domine et al., 2011, 2012), I am very happy to see independent assessments and comparisons of the various techniques used in this study, namely heated needle-probe (NP), heat-flux plate (HFP) and numerical computations based on micro-tomographic images (SIM). What is lacking the most, to my view, is a synthesis of the reported results compared to existing analogous data. By mistake, the data used for the recent publication by Calonne et al. (2011) were not reported in the original paper. We have the data available for use by anyone requesting them, and are providing them here in the review, so that the authors can use them to perform quantitative comparisons of their results with previously reported data. Making a stronger connection between existing and new datasets will help the community assess the relevance and the novelty of the present work.

3 Technical comments

3.1 Title

I found the title confusing and potentially misleading. As such, it seems to indicate that the snow samples were chosen anisotropic on purpose, i.e. that the study excludes potentially isotropic snow samples. The study confirms that many snow types are indeed anisotropic in terms of the effective thermal conductivity of snow, but some snow samples do not exhibit such an anisotropy. The title should be rephrased. In addition, I recommend the use of the "effective thermal conductivity" terminology because snow is intrinsically a multi-phase composite medium for which only effective thermal properties can be determined.

3.2 Abstract

Some of the sentences of the abstract are awkward:

- *However, parameterizations of thermal conductivity measured with the transient needle probe and the steady-state heat-flux plate show a bias.* Are parameterizations or measurements dealt with here ? The reader is left wondedring.
- *In addition, it is not clear to which degree thermal anisotropy is relevant.* This sentence is strange especially since several studies addressing the anisotropy of the effective thermal conductivity of snow have been published recently (Calonne et al., 2011, Shrtzer et al., 2011) and confirmed previous findings (Izumi and Huzioka; 1975).

In general, I found that the abstract does not accurately reflect the content of the paper and that it mixes new results and the confirmation of previously reported conclusions.

3.3 Introduction

In general the introduction needs editorial work to better provide the context of the study and outline the scientific relevance of the present work. The structure and the

flow of paragraphs should be improved. Specifically, a few items below require special attention (not exhaustive):

- Page 1840, line 23 : this sentence seems out of place ; in addition, it should be mentioned that the "heat budget" is that of the Earth's surface
- Page 1840, line 24 : I'm puzzled by this sentence. Not only the temperature gradient, but also the temperature itself and the liquid water content influence the rate of snow metamorphism. There is of course a strong coupling between the heat flux at the snowpack boundaries, the heat flux within the snowpack the vertical profile of the thermal conductivity and the vertical profile of temperature, but the current wording requires clarification.
- Page 1941, line 4 : the use of the greek letter κ for the effective thermal conductivity of snow is not consistent with the notations used in the rest of the paper (mostly k). This must be carefully checked to avoid inconsistencies.
- Page 1841, line 10 : the second half of the sentence is wrong. See e.g. Calonne et al. (2011), providing HFP, NP and SIM data for the same snow sample. However it remains correct that measurements on a large number of samples are lacking. This should be rephrased to be consistent with the current state of the literature.
- Page 1841, line 15 : "usually" : please provide references supporting this statement.
- Page 1841, line 25 : the "contact problem" applies mostly to the NP technique. This should be stated explicitly.
- Page 1842, line 15 : please also refer to recent studies on the anisotropy of the effective thermal conductivity of snow (Calonne et al., 2011, Shertzer and Adams, 2011, Holbrook, 2011).

- Page 1848, line 20 : simplify the sentences by " The design of the HFP is based on the work of Köchle (2009), as summarized in the following points: ..."

3.4 Section “Theory”

To me the first part of this section belongs to the introduction, while the sections 2.1 and 2.2 should belong to the section Methods. I suggest deleting this section and move its content appropriately towards the Introduction and Methods section. Specifically, a few items below require special attention (not exhaustive):

- Page 1843, line 5 (first sentence). Why not simply write “The thermal conductivity of porous media is often anisotropic” ? Note that “the thermal conductivity *in* porous media” is meaningless (either write “thermal conduction *in* porous media”, or “thermal conductivity *of* porous media”).
- Page 1843, line 9 : please use a special font or symbols for tensors.
- Page 1843, line 10 : this sentence assumes that the tensor **k** has only diagonal terms. This should be stated explicitly.
- Page 1843, line 12 : replace “set” by “assume”.
- Page 1843, line 15 : referring to an AGU poster does not seem appropriate here, given that the content of the AGU poster is very likely to match more or less the content of the present article. I recommend deleting the reference to this AGU poster. Furthermore, this sentence does not indicate the method used to measure the two components of the tensor **k**.
- Page 1843, line 16 : replace “simulated” by “computed”
- Page 1844, line 9 : replace “earth” by “Earth”

- Page 1844, line 19 : it may be useful to the reader to know that the definition adopted for the anisotropy factor is the same as in Calonne et al. (2011) (referred there as the “anisotropy coefficient”).
- Page 1845, line 20 : I strongly doubt that the heat flux plates used in this study incorporate a measurement of the latent heat-flux. In practice, such devices are designed to measure the conductive component of heat transfer (they actually measure a temperature difference between the top and bottom of the heat-flux plate, and convert it to a flux following a calibration equivalent to determining the effective thermal conductivity of the medium there are made of). The description of the latent heat flux component is extremely unclear and needs attention, because latent heat effects operate at the microstructure scale and should thus be considerer as a volume source term at the macroscopic scale rather than a heat flux parallel to the temperature gradient. I strongly suggest that this part should be discussed with greater care and details in terms of the physics involved.

3.5 Section “Methods”

- Page 1846, line 5 : what is the criterion employed to determine whether the sample is homogeneous or not from SMP measurements ?
- Page 1846, line 18 : the sentence should be rephrased, for instance: “The effective thermal conductivity of snow ranges from 0 to 0.8 $\mathrm{W\,m^{-1}\,K^{-1}}$”. A reference should be given to support this statement (of introductory nature, btw) and the value “0” is actually out of the range given that the effective thermal conductivity of snow cannot be lower than that of air (which is above 0).
- Page 1846, line 23 : what is meant by “absolute” here ? Maybe the appropriate term is “reference value” ?

- Page 1847, line 2 : I suggest replacing "that do not freeze" by "that do not undergo phase change in the temperature range encountered in nature".
- Page 1847, line 7 : add a space between "for" and "thermal".
- Page 1847, line 14 : It would be appropriate to state here that the needles and the parameters used differ from the study by Riche and Schneebeli (2010). Given that the same two authors have written this previous study, confusion may arise whether the conclusions of the previous study remain valid with the setup employed here. Along the same line, I recommend that the authors state that the needles employed are exactly the same as in other recent studies dealing with needle-probe measurements of the effective thermal conductivity of snow (Morin et al., 2010, Calonne et al., 2011, Domine et al., 2011, 2012).
- Page 1847, line 19 : It would be appropriate to refer to the peer-review literature to support this statement. The fact that the first 30s have to be discarded is explicitly referred to in Morin et al. (2010), and was also discussed in previous studies such as Sturm et al. (1997) and references therein.
- Page 1847, line 22 : equation (8) is not linear, so the statement referring to a "linear part" of the curve is inconsistent. It must be stated that equation (8) can be approximated by a linear curve from which k can be determined (Sturm et al., 1997 ; Morin et al., 2010).
- Page 1849, line 4 : "by the inherent heat capacity" of what ?
- Page 1849, line 7 : is it really "error" that is referred to here, or rather the variability around the mean ?
- Page 1850, line 15 : delete "receive an" and "of"

- Page 1850, line 17 : the numerical method used to determine the effective thermal conductivity of snow from microstructure 3D images must be given with more details. Not only the numerical methods but also the boundary conditions should be provided. The two publications referred to (Kaempfer and Plapp, 2009; Pinzer, 2009) do not explicitly mention the numerical computation of the effctive thermal conductivity of snow, so more details are needed to explain how the computation was done. The principles of the method employed should be compared to other methods used so far to determine the effective thermal conductivity of snow samples from 3D images, so that potential differences can be discussed later on in the manuscript (Kaempfer et al., 2005; Shertzer and Adams, 2011; Calonne et al., 2011). It is also unclear what is the value reported in the Tables and in the Figures. Indeed, computations were carried out on different subvolumes, and it is not clear what is the value finally reported. If it is the mean, then I suggest that the anisotropy coefficient shall not be compited as the ratio between the average vertical and horizontal component, but rather as the average of the ratio between individual horizontal and vertical components. Regardless what is reported, greater clarity if needed so that other scientists can use the results in their own research.

3.6 Section "Results"

This section is somewhat frustrating, since the results are never quantitatively compared to data already reported in the literature. One striking example pertains to the existing parameterizations of the effective thermal conductivity of snow vs. density. Calonne et al. (2011) have recently shown that the results from numerical computations of k_{eff} and density were extremely close to the parameterization of Yen (1981), and inconsistent with the parameterization reported by Sturm et al. (1997). The reported data also indicated that the scatter around the regression curve was much smaller in the case of the numerical computations of Calonne et al. (2011) than the experimental data of Sturm et al. (1997), meaning that density alone could be a good indicator of the average value of the tensorial components of k_{eff} (of course, anisotropy plays a role for some snow types). I think it is a pity that the authors do not use here their data obtained by numerical computation to add potentially new information to this discussion, and I strongly encourage them to add a section on this matter. Plots attached to this comment provide a first comparison between numerically computed values of k_{eff} by Riche and Schneebeli (2012) and Calonne et al. (2011), showing similarities and differences which shall be discussed in the present publication. The same applies to the anisotropy of snow: the new data obtained by the authors should be compared to existing data, to see whether the newly reported data support previous assessments or contradict them. Again, figures attached to this comment provide a preliminary comparison between the results of Riche and Schneebeli (2012) and Calonne et al. (2011) and Izumi and Huzioka, which I think the authors should incorporate in their work. In particular, the authors have measured MF samples with a significant and positive anisotropy coefficient, up to 1.5, which appears to be extremely high (as high as depth hoar samples, and close to the theoretical limit for the anisotropy of a porous medium). I think the authors should provide more details on the microstructure of these samples. Our own experience is that MF samples often feature a representative elementary volume (for the effective thermal conductivity) as high or higher than DH samples (see supplement of Calonne et al. (2011)) which leads to lower confidence in the results obtained for these snow types (in terms of anisotropy) than snow types with smaller REV. I think the authors should comment on this here.

Specifically, a few things in the section deserve improvement:

- Page 1851, line 9 : “long heating time” : please refer explicitly here to the duration of the heating time
- Page 1851, line 14 : here the statistical robustness of the linear regression is given through a R value without noting the number of pairs used ; later, (page

1852, line 11), it is an r^2 value which is given, again without stating the number of samples. The statistical significance is given using a p-value. This is confusing and statistical methods should be homogenized throughout the paper.
- Page 1851, line 22 : figures do not seem to be called in order of appearance ; this must be checked.
- Page 1852, line 4 : “obvious” : to be entirely convinced, I think the reader needs to see the images in question.
- Page 1852, line 24 : (Fig. 6) should be written after the first sentence as “kSIM z,y was calculated for 35 samples in total (Fig. 6).”
- Page 1854, line 5 : Delete "be"
- Page 1853, line 5 : data from Izumi and Huzioka should be included in the figures, so that the comparison could be carried out in a more quantitative manner (see attached figures ; digitized data from the original publication are available to the authors upon request to the reviewer).

3.7 Section “Discussion”

- Page 1854, line 5 : the argument relative to the heterogeneity of the medium and the temperature field around the needle is interesting but is not entirely new. Although stated a little differently using here an analogy to electromagnetic measurements, that the medium around the needle cannot be considered physically homogeneous was already pointed out by Calonne et al. (2011).
- Page 1854, line 17 : the beginning of the sentence needs rephrasing.
- Page 1854, line 23 to 26 : as writtent above, it is obvious that the argument of the heterogeneity of the microstructure (Calonne et al., 2011) and the thermal

field are simply too sides of the same coin. There is no need here to advocate that these two effects are independent. It would be interesting if the authors could elaborate a little more on the potential sources of discrepancy between the different methods. How their treatment of latent heat release could inform on potential latent heat effects in the vicinity of the needle probe during the transient heating (e.g., heat used for phase change may lead to an apparent reduction of the temperature rise, hence leading to underestimated k values).

- Page 1855, line 10 : it is needed here to state that this is consistent with previous independent findings (Calonne et al., 2011 ; Shertzer and Adams, 2011).
- Page 1855, line 27 : “within a factor of two” : a reference or mode details are needed to support this statement.
- Page 1856, line 4 : “complete difference” : this needs rephrasing.

3.8 Section “Conclusions”

- Page 1856, line 11 : here again, the greek letter κ was used instead of k to refer the effective thermal conductivity of snow. This must be checked.
- Page 1856, line 13 : please replace “found” with “confirm”. That the effective thermal conductivity of snow is anisotropic has be reported before (e.g., Izumi and Huzioka, 1975; Shertzer and Adams, 2011, Calonne et al., 2011)
- Page 1856, line 17 :“weakly” : please provide a quantitative assessment of this statement. This has proven to be impossible to do during the review because of the apparent error in the Table 1 of the online supplement. Nevertheless, based on Calonne et al. (2011) it appeared that the anisotropy of the effective thermal conductivity of snow was able to discriminate between different snow types. The

authors should delve deeper into this question in the present article, in light of the other papers on the topic.

- Page 1857, line 4 : not only a microtomograph, but also a non-trivial physically-based model operating at the microstructure scale is needed to infer the effective thermal conductivity of snow from 3D images. This should be added to the conclusion.

3.9 Tables and figures

Table 1 : Explicit reference to Fierz et al. (2009) should be made. The values of SSA, i.th and i.sp are apparently neither used nor discussed in the paper. The caption should explicitly mention that this is a list of the snow samples whose effective thermal conductivity was measured using the three methods employed in the study.

Table 2 : same comment as above regarding i.th, i.sp and SSA.

Figure 2 : caption, third line, replace “to high” by “too high”

Figure 3, 4 and 6 : Snow types should be reported on these curves using the fonts representing snow types which has been released together with the ICSSG. The corresponding colors should be used (color plots are free in **The Cryosphere**).

Figure 5 : what is “HPA” ? The term is also present in the caption of Figure 6.

3.10 Supplementary data

There is apparently at least one error in the supplementary data. Indeed, on the first line of Table 1, the depth hoar sample exhibits a k_x^{SIM} value larger than the k_z^{SIM}, although none of the plots indicate that a depth hoar sample can exhibit such a behavior. An electronic discussion with the authors has indicated that k_x^{SIM} and k_z^{SIM}

were swapped in the first line of the Table 1. This discussion has also revealed that it is not the same density value which is reported in this Table and in Figure 6 (numerical computation vs. gravimetric measurement). This should be clarified in a revised manuscript.

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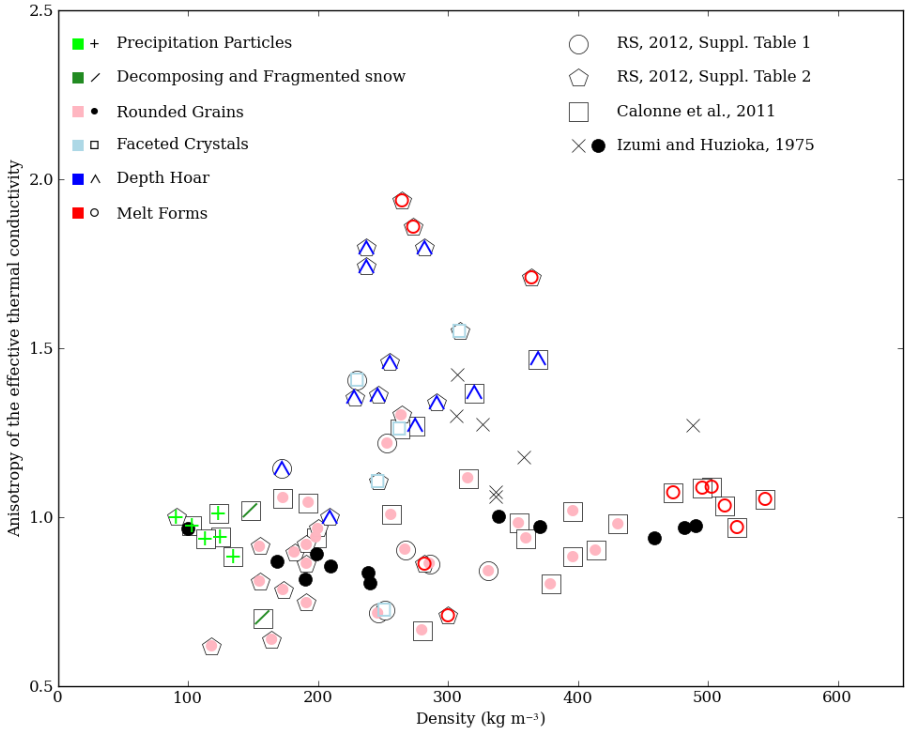

Fig. 1. Anisotropy of the effective thermal conductivity of snow using data from Calonne et al. (2011) (symbols surrounded by squares), Riche and Schneebeli (2012) and Izumi and Huzioka (1975).

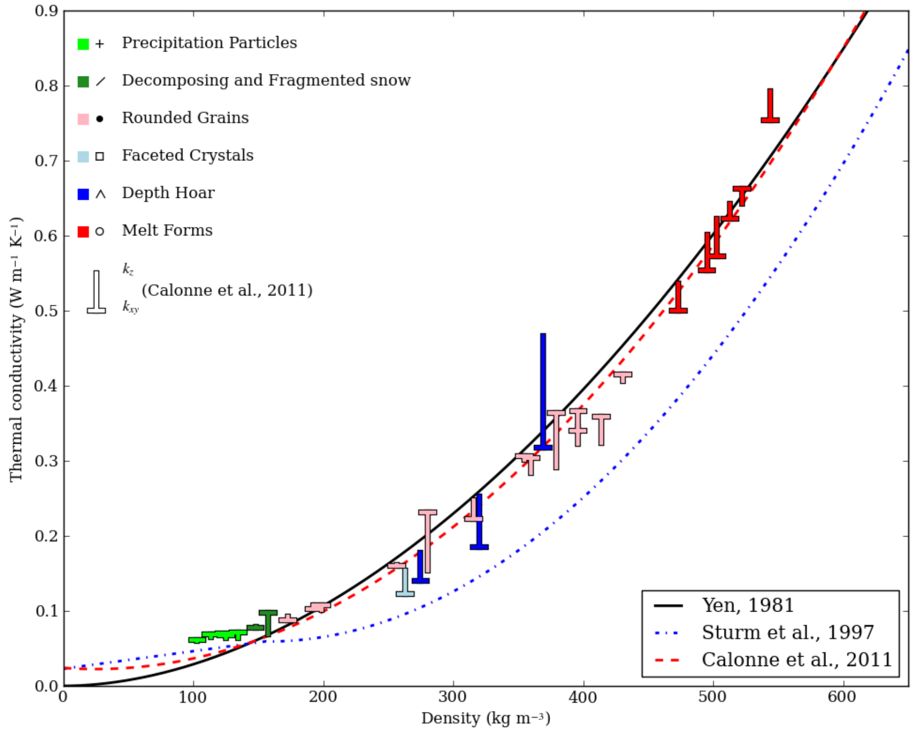

Fig. 2. Effective thermal conductivity of snow vs. snow density from Calonne et al. (2011). "T"-shape symbols represent the anisotropy of the samples, with a color coding from the ICSSG (Fierz et al. 2009)

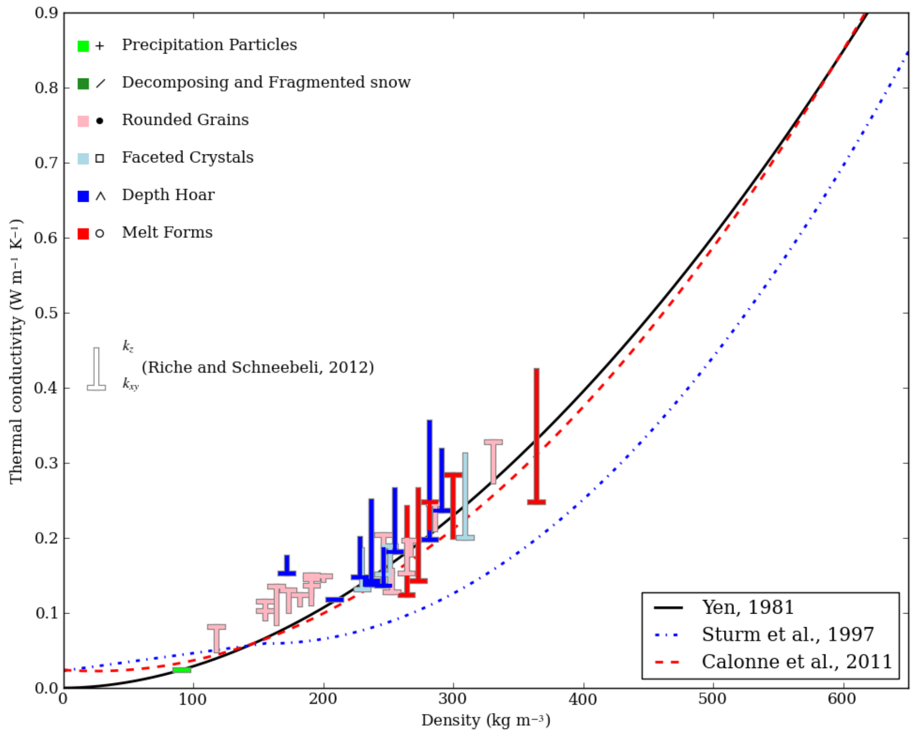

Fig. 3. Effective thermal conductivity of snow vs. snow density from Riche and Schneebeli (2012) using data from the electronic supplement.

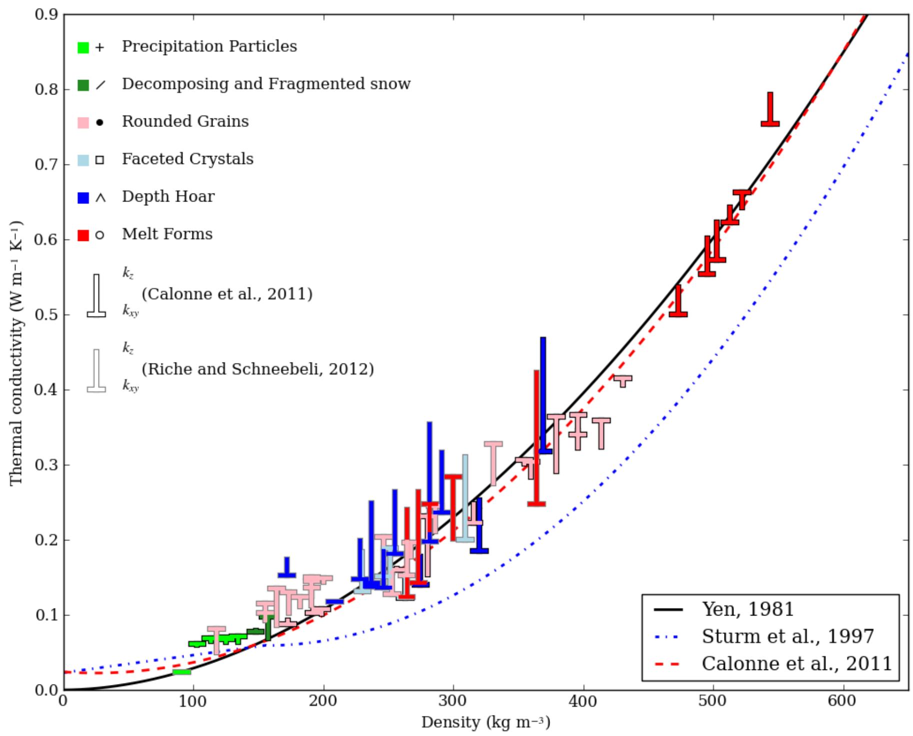

Fig. 4. Effective thermal conductivity of snow vs. snow density from Calonne et al. (2011) (black surrounding) and Riche and Schneebeli (2012) (grey surrounding)

Type	Density	SSA	k_x	k_y	k_z
	kg m^{-3}	m^2 kg^{-1}	$Wm^{-1}K^{-1}$		
PP	102,90	55,79	0,060	0,063	0,060
PP	113,44	42,48	0,071	0,067	0,065
PP	123,31	41,37	0,068	0,072	0,070
PP	124,88	55,30	0,067	0,067	0,063
PP	134,60	50,91	0,072	0,072	0,063
DF	147,71	29,32	0,078	0,076	0,078
DF	157,58	25,36	0,095	0,100	0,068
RG	172,74	23,32	0,088	0,089	0,093
RG	192,47	19,90	0,096	0,109	0,107
RG	198,64	19,23	0,106	0,109	0,101
RG	256,28	17,24	0,171	0,148	0,161
RG	280,07	17,15	0,234	0,229	0,154
RG	314,83	27,68	0,223	0,234	0,205
RG	354,51	21,59	0,302	0,310	0,301
RG	359,84	18,13	0,306	0,300	0,284
RG	378,96	14,57	0,363	0,363	0,291
RG	396,07	10,46	0,347	0,332	0,345
RG	396,13	12,29	0,373	0,359	0,323
RG	413,71	20,76	0,363	0,356	0,324
RG	430,59	17,34	0,419	0,411	0,407
FC	262,74	15,43	0,123	0,121	0,154
DH	274,79	18,18	0,139	0,140	0,178
DH	315,31	15,19	0,189	0,181	0,253
DH	369,20	21,84	0,322	0,314	0,466
MF	472,83	3,78	0,501	0,499	0,536
MF	495,11	5,25	0,532	0,576	0,601
MF	502,60	6,18	0,574	0,571	0,623
MF	512,89	7,69	0,623	0,621	0,643
MF	522,31	8,49	0,668	0,658	0,642
MF	544,08	6,99	0,762	0,745	0,793

Fig. 5. Data from the numerical computations of Calonne et al. (2011).