Dear Prof. Mitchell,

This letter is intended to replace my e-mail correspondence of October 14, 2004.

I am requesting that my manuscript "The dependence of diffusive radiative transfer on grain-size, temperature, and pressure: implications for mantle processes" be published, despite the negative reviews for the following reasons:

1) I show in the attached rebuttal that the reviewers err on all their major points. The major reviewer errors are obvious (algebraic) which is easily confirmed.
2) Stevenson errs also on his minor points, and in addition, demonstrably misrepresents both astronomy and earth science literature, and calls on issues that have nothing to do with the contents of the manuscript.
3) It would be hard to find harsher reviewers than Stevenson and Brown. Although it is within Karato’s right as editor to contact who he feels most appropriate, the predisposition of these particular reviewers to rejection should be factored in to the decision. Let my detractors write a rebuttal. They won’t as they don’t have grounds.

Note that the anonymous reviewer recommended publication if the "errors" in my equations were fixed. The rebuttal demonstrates that the errors exist not in my equations, but in the equations of the reviews.

I added an analysis using cavity radiation to the attached revision. This and obtaining the correct asymptotic limits for $k_{\text{rad,dif}}$ are convincing and direct proof that emissivity is needed. I hope that you will find that I have addressed the comments in the slightly revised manuscript (attached), and will recommend publication.

The next 6 pages provide a detailed rebuttal of the reviews. Also enclosed are the reviews and supporting material (pages from Collin’s astrophysics text with the correct definition of $L_{\text{th}}$, and Shankland et al 1979).

Regards,

Anne Hofmeister
Research Professor
Detailed rebuttal of anonymous reviewer

I will first deal with the question of units: the relevant equations in the old manuscript are:

\[ k_{rad,dif}(T) = \frac{4}{3} \int_{0}^{\infty} \Lambda(T) \frac{c}{\gamma^2} \frac{\partial\{n(T)^2 S(T)\}}{\partial T} \, d\gamma. \quad (1) \]

Where \( S \), the source function is proportional to the black body function:

\[ I(\nu) = \frac{2\pi}{c} \frac{\hbar}{\gamma^5} \left[ \frac{1}{\exp(\hbar c / k_B T) - 1} \right] \quad (2) \]

Eq. 2 is from Brewster, and refers to isotropic blackbody radiation. Anonymous provided the following: for clarity I call this IBBmixedunits:

\[ IBBmixedunits(\nu) = \frac{2\hbar c}{\gamma^3} \left[ \frac{1}{\exp(\hbar c / k_B T) - 1} \right] \quad (3) = (eq 9 from anon). \]

Instead, the correct definition in consistent units of frequency in sec\(^{-1}\) is

\[ IBBConsistentUnits(\nu) = \frac{2\hbar}{\gamma^3} \left[ \frac{1}{\exp(\hbar c / k_B T) - 1} \right] \quad (4) = (eq 2 from shankland = also eq 7 from reviewer (anonymous made an algebra mistake in going from his eq. 7 to 9). \]

The conversion from IBBConsistentUnits to \( I \) is simply

\[ I(\nu) = \frac{\pi \nu^2}{c} \text{IBBCU} \quad (5) \]

Now substitute Eq. 5 for \( S \) in my Eq 1 and we obtain

\[ k_{rad,dif}(T) = \frac{4\pi}{3} \int_{0}^{\infty} \Lambda(T) \frac{c}{\gamma^2} \frac{\partial\{n(T)^2 \text{IBBCU}(T)\}}{\partial T} \, d\gamma \]

\[ = \frac{4\pi}{3} \int_{0}^{\infty} \Lambda(T) \frac{\partial\{n(T)^2 \text{IBBCU}(T)\}}{\partial T} \, d\gamma \quad (6) \]

Which is the same as eq. 10 in Anonymous and Eq. 1 of Shankland et al.. The reviewer made an algebra mistake in going from his Eq. 7 to 9 (factor of \( c \)). Nor did he notice the factor of \( \pi \) in my equation 2.
In revision, I moved the factor of $\pi$ from Eq 2 to Eq 1 to be consistent with the geophysics literature and point out that $\nu = 1/\lambda$. I mention the different convention used in my 2004 PEPI paper.

I have made no error of units in my equations, and they are consistent and equivalent to the previous formulation if emissivity in my result is set to unity. The reviewer, in contrast, made two algebraic errors in this part of his derivation.

I will now deal with errors in anonymous contention that emissivity is not needed in my equations.

As positive evidence that emissivity is needed in the equations for diffusive radiative transfer in a grainy medium, please consider

1. the proof presented by Stevenson, once his algebraic errors are fixed. I believe this demonstrates that emissivity is needed.
2. Brewster fig. 11.1, p 369, 381, 385, also 228 as cited in the ms.
3. the remote sensing literature in which grain size is known to have an effect on radiative transfer. The monograph by Hapke provides this and discusses the role of emissivity.
4. the astronomy literature regarding radiative transfer in dust clouds, this is known at an undergraduate level (Kaufmann and Freedman) in monographs (Evans 1992) and peer-reviewed papers (Efstathiou and Rowan-Robinson, ref added, also Hofmeister et al in MNRAS, cited) that both grain size and emissivity play important roles.
5. The emissivity factor is needed to obtain reasonable values for $k_{rad}$ in the limits of small and large $A$. I rewrote this part of the text for clarity, and put in a few new eqns for emphasis.
6. figure 1 in the manuscript, and the discussion therein.
7. Cavity radiation, discussed below and added to the ms.

I will now dissect the reviewer’s proof.

His Eq. 1 contains the definition of the emission term = $I_{bb} a dS$, where $a$ is the absorption coefficient.
His Eq. 2 has $I_{bb}$ but not $a$ or $S$, it is not derived from Eq. 1
His Eq. 3 has $I_{bb}$ but not $a$, ditto
His Eq. 4 contains none of these, ditto
His Eq. 5 contains $I_{bb}$ but not $a$, ditto
His Eq. 6 contains $I_{bb}$ but not $a$, ditto. This is his final result.

In order to prove that emissivity was incorporated in Eq. 6, he needs to derive his Eq. 2 and Eq. 3 and show where Eq. 1 was used and how the emissivity factor is cancelled out.
He has not provided this. The review even states that he does not provide such a proof.

The most convincing way for me to deal with this, so that the reader can understand, is to include a discussion of cavity radiation in the ms (point 7), added to Section 2.2, after Eq. 8 in the ms:
That emissivity was not incorporated in previous geophysical models is confirmed with a thought experiment: Consider removing one single grain from the mantle, which leaves a cavity with radius \( r = d/2 \). The flux inside the cavity is \( \xi \sigma T^4 \), where \( \sigma = 5.67 \text{ W/m}^2 \text{K}^4 \) is the Stefan-Boltzmann constant (e.g., Halliday and Resnick 1966, p. 1175). From Carslaw and Jaeger (1960, p. 7),

\[
- k_{\text{rad}} \frac{\partial T}{\partial r} = \text{flux} = \xi \sigma T^4
\]

(9)

where I have utilized the fact that vibrational transport into the vacant cavity is nil. Irrespective of the particular temperature gradient in the cavity, Eq. 9 shows that \( k_{\text{rad}} \) must be proportional to the product \( \xi \sigma \). Using dimensional analysis, an approximate solution is:

\[
k_{\text{rad}} \sim 6\xi \sigma T^3 d.
\]

(10)

The result is emissivity multiplied essentially by Clark’s result \( k_{\text{rad}} = (16/3)\sigma T^4 \Lambda \), because the free path is \( \sim d \) for the non-absorbing cavity. Clark’s result is readily obtained from Eq. 1 by replacing \( S \) with \( I_{bb} \) and by treating \( \Lambda \) as a constant, see e.g., Shankland et al. (1979). This analysis shows that Eq. 1 must include the factor \( \xi I_{bb} \) to address the emission characteristics of a real material. Clark’s formula has erroneously been referred to as the graybody approximation (e.g., Ross, 1997), based on the assumption in his derivation that \( \Lambda = A \) is independent of frequency and temperature. Instead, it is a half-graybody approximation, as black emissions were assumed.

The second paragraph on Clark is needed to address errors in the literature and in the thinking of the reviewer.

How the other issues were addressed.

1) The physics presented in the paper is geared to level of knowledge of radiative transfer in geophysics. This background is needed.
2) Conditions under which the approximate Eq.11 is valid were added.
3) All equations were checked and are correct.
4) It is already clear in the manuscript that \( \Delta n \) at the interface was being calculated, as Snell’s law is prominent in that discussion.
5) Eq. 5 was referenced.
6) The sentence was rewritten and equation was fixed (I_{refl} not R)
7) Seigel and Howell is very difficult to read, and anonymous did not understand their assumptions. Please examine Brewster’s text if needed. But the beauty of science is that one does not have to defer to the authority: The figures and manuscript describe why emissivity is needed. Again, Stevenson’s review independently corroborates my formula, once his errors are fixed, but I think the above essay on cavity radiation is a better illustration.
Detailed rebuttal of Stevenson’s review.

I will first deal with errors in Stevenson’s contention that emissivity is not needed in my equations.

On p. 2 and 3 he constructs a 1-d model for flux transfer. I show here that his analysis is algebraic trickery. His computation is inconsistent with his figure, and relies on misapplication of the quantum mechanical concept of thermalization length. The symbols are \( A = \) absorptivity, \( d = \) grain size, \( \sigma = \) Stephan-Boltzman constant, \( T = \) temperature, \( I_{th} = \) intensity of blackbody radiation, \( z = \) distance, and periods are used as multiplication symbols. \( L_{th} \) is the same longer length scale, he uses small \( l \) (which looks too much like capital “\( L \)”). He considers the limit of small \( Ad \), where \( Ad = \) emissivity. He begins with the statement “flux … from \( L_{th}/d \) layers… each layer contributes a net … amount of \(~ A d \sigma(T-L_{th} dT/dz)^4\).” Stevenson’s text (verbatim from his page 3) is:

“The net flux in the \( z \) direction is accordingly

\[
\frac{1}{2} \left( L_{th}/d \right) [Ad\sigma(T-L_{th} dT/dz)^4 - Ad\sigma(T+L_{th} dT/dz)^4] \sim 4\sigma T^3 dT/dz
\]

which is exactly what the standard formula (eq. 1) predicts for this limit. Notice that the result is independent of \( A \) and yet makes use of the low emissivity.”

The right hand side was not derived from the left hand side using the accepted rules of algebra. The rules of algebra require that since ALL terms on the left side are multiplied by \( A \), the term on right side MUST also be multiplied by the factor \( A \). This is easily verified.

Net flux \( \sim \frac{1}{2} (L_{th}/d)Ad\sigma [-2L_{th}T^3 dT/dz - 2L_{th}T^3 dT/dz] \sim -2 \) Emissivity \( \sigma T^3 (L_{th}^2/d) dT/dz \)

(i)

Stevenson “removed” \( A \) from the final result by absurd machinations involving \( L_{th} \). Using his definition = \((d/A)^{1/2}\) and large \( A \) provides \( L_{th} < d \), in violation of his assumptions.

Thermalization length is certainly not defined as Stevenson did. This is a concept from quantum mechanics used to describe processes not in thermal equilibrium (relevant pages are enclosed from http://ads.harvard.edu/books/1989fsa..book/AbookC15.pdf) It does not bear on the process of radiative diffusion, wherein local radiative equilibrium is assumed. It sure looks like that Stevenson came up with his particular definition of \( L_{th} \) just to prove me wrong.

Using either the correct definition, \( L_{th} > d/(1+dA) \), or a minimum \( L_{th} \sim 2d \), as shown in Stevenson’s figure, gives the result:

net flux \( \sim -2 \) Emissivity \( \sigma T^3 d dT/dz \) or \( -8 \) Emissivity \( \sigma T^3 d dT/dz \)

(ii)

Using the reviewer’s approach, but with \( L_{th} \) correctly defined, shows that radiative transfer is indeed proportional to the emissivity of the medium, which corroborates the
correctness of the heart of my theory: that emissivity must be included in modeling
diffusive radiative transfer.

In my manuscript, I added discussion of cavity radiation (same as the red section
above). This is a 3-d model that uses 19th century physics to show emissivity must be
incorporated. It is shorter, clearer, valid for any values of $A$ and/or $d$, and returns Clark’s
result.

Now, I discuss the problematic statements in each paragraph:

Paragraph 1 contends there are fundamental errors re emissivity. I show above that
Stevenson has derived this conclusion using algebraic trickery and inconsistent
definitions. My manuscript shows that a significant number of Stevenson’s previous
papers are without basis: he has a vested interest in stopping publication of my work.

Paragraph 2 agrees that grain size matters. The rest of pp 2 is a smoke screen.

Let us start at the bottom of pp 2. What he says is that $A >> d/L^2$. The only
definition of $L$ that he provided is $L >> L_{th} \sim (d/A)^{1/2}$. I’ve already shown that his
definition of thermalization length has no basis. Furthermore, using this absurdity
provides the preposterous result:

$$A >> d/(d/A) = A.$$ (iii)

No restrictions exist on the size of $A$. So, $A$ can be really, really small, and thus
the limit $A \to 0$ is valid as discussed in the old manuscript. Thus, Eq. (iii)
invalidates the conclusions of this paragraph. This error is related to the mistakes
made on p.2-3 of his review.

The manuscript contained a discussion of the tradeoffs due to the appearance of
the product $dA$ in the equations for $k_{rad}$. In revising the paper, I added formulae
providing $k_{rad}$ for the limiting cases of $dA >> 1$ and $dA << 1$ in order to drive this
point home.

Paragraph 3 says that the new theory is wrong because it does not provide the same
results as the old theory. Yet, according to Stevenson in pp 2, grain-size effects radiative
transfer, and thus Shankland’s model is incorrect, as it did not include grain-size! That
Stevenson insists my model must reproduce the erroneous results of an incorrect model is
ludicrous.

Moreover, the three orders of magnitude difference claimed by Stevenson cannot
be substantiated. Shankland et al’s results (Fig. 6, attached) are similar to my
results for mantle grain sizes of 1 cm to 0.1 cm. What is new is the dependence on
$T$ and $d$. 
The last part of paragraph 3 misquotes the astronomy literature. Stevenson is really confused here, and is thinking about stellar interiors, not dust clouds. I’ve cited several papers in my manuscript from relevant astronomy literature. The chapters he quotes do not contain Eq. 1 as he states and are not relevant to the present ms. He has misrepresented the contents of the astronomy and physics literatures (e.g., the thermalization length), as well as misrepresenting Shankland et al 1979’s results and equations, and the current manuscript. I believe that I do know the astronomy literature concerning dust: I am a co-author on 6 peer-reviewed publications on this subject and serve as a reviewer in this field. This is a new venture for me that has been a lot of fun and even helped with the present manuscript.

Paragraph 4 was discussed above. His analysis is inconsistent with his assumptions.

Paragraph 5 misquotes my manuscript, distorting what was said in each case.

As an example, consider photon-hoping, the type of radiative transfer occurring within the grains. The manuscript says that this case can be dismissed because I assume that the grains are isothermal. Stevenson says that I should also dismiss phonons (vibrational transport) on this basis. Apparently he does not understand how an assumption made in order to solve the problem differs from the real physical processes.

Paragraph 6 provides the underlying reason for the rejection: that is “too many papers are published on this topic.” He is not judging my manuscript on its contents.

Paragraph 7 demonstrates that Stevenson lacks objectivity. He could not get a chapter from his phd thesis published (it was obviously wrong, as Guillot later showed in Science and Icarus) and so I should not be allowed to publish my paper. He alludes to the importance of trace amounts of Cs in gas giants to blocking radiative transfer, without attribution. Nothing resembling his contention exists in the astronomy, physics or Earth science literatures on this topic. I think that the cesium arguments are from a preprint of Stevenson’s. The present manuscript shows that the cesium arguments are wrong. I really thing this is what bother’s Stevenson underneath it all. He worked so hard to show that his thesis was correct, and with my work, he is shown wrong again.

In summary, Stevenson’s claims are unsupported, and his review is highly prejudicial.

As things can always be improved, I’ve made a few changes in the ms for clarity, added equations for $k_{\text{rad}}$ in the limits of small and large $d$ and $A$, and the derivation based on cavity radiation. I think these additions will help the general reader. I also updated the reference list.
October 1, 2004

Anne,

My apology for an extremely long time that has taken since we received your manuscript. This is due to the difficulty in obtaining reports from the reviewers. I understand how frustrated you have been and appreciate your patience.

I have finally obtained two reports, one from an anonymous physicist, and another from Dave Stevenson. Both are very familiar with the physics of radiative heat transfer and I consider their evaluation seriously. Obviously I respect your suggestion of potential reviewers and in fact I have asked more than 6 of them to help me review your article, but they did not agree to do this for one reason or another. In any case, after a long wait, I now have two reviews, which as you will see are not favorable for publishing your paper. Both of them pointed out some fundamental problems particularly the way in which you included emissivity. This is a heart of your theory, and therefore based on the evaluation of your paper by these reviewers, I regret to say that I cannot recommend the publication of your paper.

Sincerely,

S. Karato

Shun-ichiro Karato
Editor of PAGEOPH
Comments on "The dependence of diffusive radiative transfer on grain-size, temperature, and Fe-content: implications for mantle process" by A.M. Hofmeister

The Hofmeister manuscript (denoted as AMH) attempted to introduce new physics to previous theory on the effective conductivity of diffusive radiation transfer, to incorporate spectroscopic data and to find geophysical implications. The claimed new physics includes adding emissivity $\xi$ term to black body term, estimating the scattering-dependence on grain size $d$, and correcting for back-reflections at the interfaces. Unfortunately, adding emissivity $\xi$ was due to a simple-minded thought (by the author) that black-body was assumed in derivation of $k_{\text{rad,dif}}$. But such adding was wrong physically as the derivation never assumed so (see next for details). Clark, 1957 never assumed blackbody either in his calculation. (A gray-body example was shown for illustrative purpose.) Shankland et al. 1979 and Clark, 1959 and other literature, essentially employed the same theory based on similar derivation and assumptions. Also black-body assumption was inherently excluded in the derivation - otherwise it would have been a simple case of no practical use. From physics point of view, AMH did not supply anything new. Rather it was wrong or at least inconsistent with the theory she cited.

But considering scattering effect of grain-size is interesting. (Shankland et al., 1979. neglected such effects for their own reasons.) I also presume that the spectroscopic data presented in AMH are better or at least independent of Shankland et al, 1979. Thus compared to Shankland et al, 1979 and Clark, 1959, the AMH deserves eventual publication even the same theory is employed. AMH should be publishable after major revision.

In general, the physics and mathematics were not well justified. I will only focus on the major issues as it is difficult to exhaust all the details. I will present some important details as follows and conclude with suggestions.

1 Derivation of $k_{\text{rad,dif}}$

The author cited the formulation for $k_{\text{rad,dif}}$ (Eqs. 1-2 in AMH) from Clark, 1957 and Shankland et al., 1979. (Eqs. 1-2 in AMH were not correctly spelled.) The author had thought that blackbody assumption was in the derivation. (This thought was the key point of the paper.) Although she cited Brewster, 1992 to justify adding an emissivity term $[\xi(\nu,T)]$ to black-body radiation (Eq. 8), it was physically wrong as shown later. As I could
not locate Brewster, 1992, I shall not comment on the legitimacy of the support from this reference. Eqs. 1-2 in AMH were cited from Clark, 1957 and Shankland et al., 1979. Clark, 1957 derived Eqs. 1-2 without rigorous justification. Yet Siegel and Howell, 1972 (cited by Shankland et al., 1979 and AMH) derived such equations with extreme details and rigorousness. (The book referred to by myself is Siegel and Howell, 2002, Taylor & Francis.) I will show in the following that Eqs. 1-2 are not for diffusion of blackbody radiation, instead they are for optically thick translucent materials. The appearance of blackbody term in Eq. 1 is due to the contribution of emission of the concerned medium. The arguments presented here closely follow Siegel and Howell, 2002 (denoted as SH). I also follow their notations for convenience.

The spatial variation of spectral intensity \( i_\lambda \) of light traveling in the concerned medium is composed of four contributions: loss by absorption and scattering, and gain by spontaneous emission and scattering (Eq. 13-9, p. 561 in SH). Consider a small distance \( dS \), the emission term is

\[
 i_\lambda (1 - e^{-a_\lambda dS}) \approx i_{\lambda b}[1 - (1 - a_\lambda dS)] = i_{\lambda b}a_\lambda dS 
\]  

(1)

from Taylor expansion of \( e^{-a_\lambda dS} \) in emissivity \( (1 - e^{-a_\lambda dS}) \) as \( dS \) is small. (Compared to Eq. 11-22b in the case of \( n \neq 1 \); p.430 in SH.) \( i_{\lambda b} \) is \( I(\nu, T) \) in the manuscript. As long as emission is concerned, it is referenced to the blackbody radiation of local temperature to quantify the fraction emitted. \( i_\lambda \) and \( i_{\lambda b} \) are not necessarily the same, as the local medium, say, might have much lower temperature than that corresponding to \( i_\lambda \). (But for absorption attenuation, \( i_\lambda \) should be used.) Obviously, no blackbody assumption was made here. (The fictitious blackbody in Fig. 11-7, p.430 in SH representing the source of temperature \( T \) is used for illustrating the idea of local thermodynamic equilibrium for the non-black medium \( dV \).) This is why \( i_{\lambda b} \) appears in Eq. 1. It would be helpful to go through all the intermediate derivations in SH but I will present the last few.

Consider 1-D radiative diffusion for an optically thick medium, to the first-order approximation,

\[
 i_{\lambda b} = i_{\lambda b} - \frac{\cos \theta}{K_\lambda} \frac{di_{\lambda b}}{dx} 
\]  

(2)

(Eq. 15-27, p.634 in SH). Integration yields

\[
 q_\lambda(x) = -\frac{4\pi}{3K_\lambda(x)} \frac{di_{\lambda b}}{dx} 
\]  

(3)
(Eq. 15-29, p.634 in SH).

If we consider only thermal conduction and radiative diffusion (Eq. 13-44, p.574 in SH), the simplified energy equation is

$$\rho c_p \frac{dT}{dt} = \nabla \cdot (k \nabla T - \mathbf{q}_r).$$  \hfill (4)

So in 1-D,

$$-\nabla \cdot \mathbf{q}_r = -\frac{dq}{dx} = \frac{4\pi}{3K_\lambda(x)} \frac{di_{\lambda b}}{dT} \frac{dT}{dx}$$  \hfill (5)

assuming $K_\lambda(x)$ varies slowly with $x$. By comparing thermal conduction term $k \frac{dT}{dx}$ to Eq. 5, the effective conductivity due to radiation-diffusion is obtained by integration over the whole wavelength (or frequency) as

$$k_{rad,dif} = \frac{4\pi}{3} \int_0^\infty \Lambda(\lambda, T) \frac{\partial(n^2 I_{\lambda b})}{\partial T} d\lambda.$$

Here we shifted to AMH notations. Note that second-order approximation and 3-D geometry can be derived (p.634-635). A closely related formulation using Rosseland approximation is widely used in astrophysics.

In frequency $\nu \equiv c/\lambda$ (unit is 1/second),

$$I_{\nu b} = \frac{2h\nu^3}{c^2} \frac{1}{e^{h\nu/kT} - 1}$$  \hfill (7)

with unit of Wm$^{-2}$Hz$^{-1}$sr$^{-1}$, i.e. power per unit area per unit frequency interval per solid angle. Thus

$$k_{rad,dif} = \frac{4\pi}{3} \int_0^\infty \Lambda(\nu, T) \frac{\partial(n^2 I_{\nu b})}{\partial T} d\nu$$  \hfill (8)

with unit of WK$^{-1}$m$^{-1}$ for thermal conductivity. (Solid angle integration was implicitly done in Eq. 3.)

If wavenumber $\nu \equiv 1/\lambda$ (m$^{-1}$) is used as in AMH, we have

$$I_{\nu b} = \frac{2hc^2\nu^3}{e^{hc\nu/kT} - 1}$$  \hfill (9)

with the unit of Wm$^{-1}$sr$^{-1}$ or Wm$^{-2}(1/m)^{-1}$sr$^{-1}$, i.e. power per unit area per unit wavenumber interval per solid angle.

$$k_{rad,dif} = \frac{4\pi}{3} \int_0^\infty \Lambda(\nu, T) \frac{\partial(n^2 I_{\nu b})}{\partial T} d\nu$$  \hfill (10)
with the unit of WK$^{-1}$m$^{-1}$ for thermal conductivity.

Eqs. 1-2 in AMH are not consistent with the theory they were based on. $S(\nu, T)$ in AMH has unit of Jm$^{-3}$m$^{-1}$, i.e. the energy density per wavelength as the author claimed, compared to Wm$^{-2}$(1/m)$^{-1}$sr$^{-1}$ according to the theory. Even if $S(\nu, T)$ were energy density per wavelength, Eq. 2 in AMH should have had the same unit to be consistent. But Eq. 2 in AMH has unit of Jm$^{-8}$s$^4$ thus is in error. The correct equations should be Eqs. 9-10 in this comment if $\nu \equiv 1/\lambda$.

In summary, the author appears to have misunderstood the physics and misspelled Eqs. 1-2 (in AMH) and Eq. 8 (in AMH) is wrong, at least if compared to the derivations of the theory cited by the author.

2 Other issues

- The physics discussed should be limited to the theory or calculation used. I suggest the author discuss only the physics and assumptions behind the theory.

- Note that Eq. 11 in AMH is valid only at reference pressure and temperature, say $T_0$ and $P_0$. The value given is only for ambient conditions. This should be pointed out to readers to avoid wrong impression.

- Check all the equations for correct units and consistency.

- For calculation of $R$, the $n$ should be the relative $n$ at the interface, not necessarily the same $n$ in Eq. 1 of AMH. (It is the same for vacuum-sample case.)

- Give reference to Eq. 5, AMH.

- $I_{\text{tra}} + R = I_0$ is wrong. (p.11 in AMH).

- Although I was not able to locate Brewster, 1992, I believe the derivation and physics of Siegel and Howell, 2000 are sound. Thus doubly involving emissivity (once in the theory and second time by AMH) is wrong. Simply applying the original theory by incorporating the grain-size and new data is still interesting and deserves publication.
The radiative conductivity of the mantle is a poorly known and possibly important factor in understanding heat transport within Earth. This paper has three aspects: a general discussion of processes, a proposed new formula for the conductivity (eq. 10), and some new data that (together with other data) are then inserted into the formula to provide predicted estimates of the conductivity. I think that the introductory discussion is confused and that the proposed formula is in error. I document this in detail below. I am also skeptical about the use of the data to assess the radiative conductivity inside Earth, though it could be that there is still some useful result that could emerge from this part (once the error is rectified).

I agree with the general principle that grain size matters and I will focus on that case. There are five important length scales in this problem. They are: the macroscopic length $L$ over which $T$ varies significantly (typically 100km or more), the characteristic absorption length $A^{-1}$, the scattering length d (taken to be proportional to the grain size), the thermalization distance $l_{th}$ (the typical distance that a photon goes from its source before being absorbed), and the wavelength of the photon. (I will ignore the issues related to how the latter compares with $d$, as does the author, but there are cases where this matters). In the case $Ad<<1$ (where the proposed new formula, eq. 10, is so different from the standard literature), $l_{th} \sim (d/A)^{1/2}$. This follows from the realization that the photon must go a total distance $A^{-1} = d/(Ad)$, but since it is a random walk, the net distance from the source is d times the square-root of the number of scattering steps $(Ad)^{-1}$, i.e. $d/(Ad)^{1/2}$. Now the diffusion approximation requires that $L >> l_{th}$ and nobody (including the author) doubts that this is so for Earth's mantle. Notice, however, that this requires $A >> d/L^2$. This means that you cannot take the limit $A \rightarrow 0$; the conductivity does not go to zero in this limit (as the author claims) but it is undefined. This may seem like a pedantic concern, but it isn't because I think it lies at the heart of some conceptual confusion.

At this point, it is worth making clear what the stakes are in this issue. (I'm always assuming $Ad<<1$ for what I say here). According to eq.(10), the conductivity $k \propto Ad^2$ whereas the standard formula (equations 1, 3 and 7) would give $k \propto d$ in the same limit. All else being equal, the author is claiming that a medium in which $d\sim100cm$, $A^{-1} \sim1km$ ($Ad^2 \sim 0.1 \text{ cm}$) would have about an order of magnitude lower radiative conductivity than a medium in which $d\sim A^{-1} \sim1cm$ ($Ad^2 \sim 1cm$), even though the first medium has much longer mean free paths for both scattering and absorption! Yet the first medium is still clearly in the diffusive regime and will certainly therefore satisfy the usual criteria for reaching equilibrium ($l_{th} << L$). The standard formula ($k \propto d$) would give a larger conductivity for the first case by two orders of magnitude. In other words, the standard result and Hofmeister's new result differ by three orders of magnitude in this example. It's admittedly an extreme example but it helps to clarify the issues. The origin for Hofmeister's claim is that the radiation field that provides the differential flux is much smaller than the black body field (smaller by the emissivity $\sim Ad$). In other words, the claim is that emissivity function must be included in the source function (eq. 8). This is a
fallacy, and directly contradicts standard literature, including the astrophysical literature\(^1\) where a great deal of attention is paid to the problems of emissivity. (I do not have access to the cited text, Brewster; someone else has our library copy but I suspect he doesn’t really do what Hofmeister claims.\(^2\)) The emissivity is very real of course. But so is steady state (what gets emitted must be absorbed near by, as already discussed). In this limit, the source function does not have the emissivity but is black body–like. In words, the reason is this: At those frequencies where the emissivity is low, the absorptivity is also low so there is a greater volume of material emitting photons that pass through a given element (or grain or whatever). In other words, the photons come from further away. The beauty of Kirchoff’s laws is that this exactly compensates, and you recover the Planck function. This is all standard stuff, e.g., my cited references. Those references give a formula like eq.(1) and not the new result, eq.(10).

I don’t know whether this will help, but here is a very crude 1D model that reproduces the standard result and contradicts the claimed new result. Consider a set of parallel plane layers, each of thickness \(d\). There is a temperature gradient from left to right. At the middle of the set of layers shown below, where the temperature is \(T\), we are getting flux from the left from each of \(l_0/d\) layers and each contributes a net (positive \(z\)-direction)

\[
\text{amount } \sim \text{Ad.} \sigma(T-l_0 \text{ dT/dz})^4. \quad \text{(The more distant layers do not contribute because they are beyond the absorption distance). Likewise, each layer on the right of the middle}
\]

\(^1\) My sources are *The Physics of Astrophysics*, F. Shu, vol. 1, chapter 2; and *Radiative Processes in Astrophysics*, Rybicki and Lightman, chapter 1.

\(^2\) I should make it clear that I don’t think you can just simply take the standard astrophysical results and apply them to the mantle; there really are complications that arise from granularity. But the issue of emissivity is well covered in the standard literature.
contributes $Ad \cdot \sigma(T + l_{th} \cdot dT/dz)^4$ in the other direction. The net flux in the $z$ direction is accordingly

$$\frac{1}{2}(l_{th}/d) \cdot [Ad \cdot \sigma(T - l_{th} \cdot dT/dz)^4 - Ad \cdot \sigma(T + l_{th} \cdot dT/dz)^4] \sim -4 \sigma T^3 \cdot d(\cdot dT/dz)$$

which is exactly what the standard formula (eq.1) predicts for this limit. Notice that the result is independent of $A$ and yet makes explicit use of the low emissivity! In other words, the emissivity is most definitely taken into account but doesn’t matter in the final correctly computed result. Standard texts derive this very straightforwardly without even introducing the thermalization length but it is comforting to know that one can get the right result by an intuitive approach.

There are many problems in this paper with the discussion section that precedes the theory and calculations. Sometimes it is just lack of clarity but in some cases I think it is fundamental (conceptual). On p3, we are told that “although the phonon contribution is always diffusive, the photon contribution is not.” Not correct, at least in the context in which this sentence is placed (i.e., a discussion of Earth’s mantle). They are both always diffusive in this context. On p4, the casual reader might think that the author is saying that radiative transport from the core into the mantle is negligible. Not true, if radiative conduction is at all important in the mantle. (The smallness of the mean free path of phonons should likewise not lead one to argue that the phononic conduction cannot carry heat from core to mantle.) I think the author’s intent was merely to say that $l_{th}$ is small. At the bottom of p5, photon hopping is dismissed on the basis that mantle grains are essentially isothermal. Why not also dismiss phononic conduction on the same basis?! This is a non-argument. (Near isothermality of individual grains is often referred to, and nearly always irrelevant to the issues at hand). Absorption (and sometimes scattering) is quantum-mechanical, despite what is claimed on p6. I could go on.

I am not sure what to make of the use of actual data. Clearly the issue of reflectivity at grain boundaries is a tricky one and requires more work. But let me offer a couple of general comments to explain why I have some general skepticism about the recent revival of enthusiasm for this subject of radiative conductivity. First, the issue (p3) about this being “profoundly” important for convection and Earth heat transport (because of its $T$-dependence). If you make a list of all the things that are important for mantle convection, there are ~10 items on the list (maybe more). If you keep nine of these fixed and then vary the remaining one, then you will usually find a large change in the character of the convection. It doesn’t matter which one you choose. (The Yuen Mantra). But does it help to then say that each of these items is “profoundly” important? I think this is overuse of the word “profoundly”.

As to the actual amplitude of the radiative conductivity, let me offer you a case history. Thirty years ago, I attempted (in an unpublished chapter of my Ph. D. thesis) to establish that radiative conduction is unimportant at levels deep beneath Jupiter’s atmosphere, where $T \sim 1000$ to 2000K. I failed. Subsequently, Tristan Guillot came along (in his Ph.D. thesis, around 1990) and showed that hydrogen, water, methane and ammonia are all rather transparent (i.e., have windows) for photons corresponding to these temperatures,
even for the rather high pressures involved. As a consequence, he predicted that giant planets should have deep-seated radiative layers. Now, some years later, we realize that the windows are closed off by... cesium! Cesium has an abundance of about $10^{-11}$ relative to hydrogen in Jupiter. Do I need to belabor the point? Radiative transport is peculiarly sensitive to “dirt”. I can cite many other case histories. I am skeptical of claims that an understanding of end-member major phases will tell us the radiative conductivity of Earth’s mantle. We don’t even know all the relevant phases in the lower mantle. Major phases will only give you the correct answer if they predict low conductivity. If they predict high conductivity then you should be instinctively skeptical!