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Review Report Form

	High	Average	Low	No Answer
* Originality / Novelty	( )	( )	(x)	( )
* Significance of Content	( )	( )	(x)	( )
* Quality of Presentation	( )	( )	(x)	( )
* Scientific Soundness	( )	( )	(x)	( )
* Interest to the readers	( )	( )	(x)	( )
* Overall Merit	( )	( )	(x)	( )

*** Overall Recommendation**

( ) Accept in present form
( ) Accept after minor revision, I do not need to see the revised version
( ) Reconsider after major revision, I want to see the revised version
(x) Reject

*** English Language and Style**

( ) English language and style are fine
( ) Minor spell check required
(x) Extensive editing of English language and style required

Comments and Suggestions for Authors

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This manuscript is gibberish and should be rejected. There is no conceivable revision that could render it publishable.

It repeats and builds on a foundation of gibberish that the author has previously managed to get published in fourth tier journals such as E&E with no effective peer review, and builds on it with more gibberish. I am not going to take time to give a complete account of all the nonsense in this paper, but will only confine myself to a few key points.

First, I will take up some of the author's previous claims, that lay at the root of what is attempted in the present MS. The key claim is that the greenhouse effect of CO2 is saturated so that further increases of CO2 cannot warm the Earth. This argument rests on a supposed proof by the author that the optical thickness of a planet's atmosphere has a maximum possible value that cannot be broached by making CO2 higher. This claim is prima facia invalidated by Venus, whose atmosphere is nearly pure CO2 and has an optical thickness of around a hundred, and as a result has a surface temperature exceeding 700K, despite the fact that its highly reflective clouds mean that the planet is heated by less absorbed solar radiation than Earth. This would have been immediately clear if the author had been honest enough to include Venus in his Figure 1. The arguments are internally inconsistent, since there is nothing in

them that distinguishes CO2 from other greenhouse agents, including water vapor and clouds; yet the author shows observations which he interprets as yielding optical thickness variations in response to water vapor, and of course every satellite IR cloud image shows that clouds can drastically reduce radiating temperature. But at the very foundations of the authors claim lie the most egregious blunders of all: two errors on basic physics so elementary that just about any undergraduate physics student can spot them.

The first of these is a complete failure to understand Kirchoff's laws. The issue is not (as the author claims) a dispute over whether Kirchoff's laws of radiation apply to the atmosphere; they clearly do, apart from the extreme upper reaches where local thermodynamic equilibrium breaks down. The issue is whether Miskolczi understand their statement, and how to apply them properly. He does not. Kirchoff's law states that the emissivity equals the absorptivity at any given wavelength. It does not even imply that the emissivity equals absorptivity averaged over wavelengths, since that breaks down when the spectrum illuminating the substance is different from the emission spectrum. But Miskolczi's error is far more elementary than this: he seems to think that the laws say that emission itself equals the absorption. Clearly nonsense, since the emission (regardless of emissivity) rises with whatever temperature a body happens to have, regardless of what it is absorbing. In certain very limited circumstances Kirchoff's law can imply as a consequence that emission itself equals absorption, but these are not nearly general enough to encompass Miskolczi's claims.

The second elementary physics error regards the virial theorem, which yields another of the constraints leading to the author's claim of a maximum possible optical thickness. The virial theorem relates mean kinetic energy to mean potential energy, and applied to a gravitationally bound atmosphere is tautologically satisfied in hydrostatic equilibrium. But the author mystically and magically extends the virial theorem to apply to the radiation field. There is no justification whatever in physics for doing so.

The author may point to scatter plots (Fig. 6) which he will say confirm the correlation implied by his result, but these plots do not imply causation; they do not tell you what would happen if you took the present atmosphere and increased CO2 (or any other greenhouse gas). There are any number of other explanations for why the data lines up as it does, but the author has failed to consider them. The fact that the author has to change his supposedly universal "rules" in an ad hoc way to accomodate the Martian results (line 409ff) in itself should ring alarm bells.

As I said, the old gibberish above has been repeated in the present MS, but now on the the new gibberish the author has introduced. In large measure, this new gibberish amounts to the assertion that the author's use of a line-by-line code (HARTCODE) invalidates everything done in radiation codes in GCM modelling. This is nonsense, because the band-averaged GCM radiation codes (which are used for reasons of computational efficiency) have been extensively tested against several different line-by-line codes which are not in any important regard different in construction from HARTCODE (see notably W. D. Collins et al, J. Geophys. Res. 111, D14317 (2006)). These confirm the radiative forcing due to increase of CO2, and invalidate the author's claim that there is an absolute cap to the greenhouse effect. If the author has gotten a different result from HARTCODE, it is because he has not used it correctly.

In fact, the root of the reason the author has gotten so confused in the new attempts in this paper is the focus on average optical thickness as an indicator of the greenhouse effect. The change in OLR due to increase in CO2) is a much more direct and sensitive indication of the effect. Because new absorption is added only in the wings of the CO2 absorption feature, the change in averaged optical thickness corresponding to a significant radiative forcing is hardly detectable. Further,

Date & Signature

optical thickness in itself does not directly translate into
a corresponding greenhouse effect, since the greenhouse effect
relies on the vertical profile of atmospheric temperature, so
the effect on OLR cannot be deduced from tau alone, but requires
consideration of the altitude from which radiation to space
originates. This relative disconnect between radiative
forcing and tau is what invalidates the conclusion the author
tries to draw from Fig.21. He never addresses the issue of how
much change in tau one expects to see over this time period
from the increase in CO2 (as diagnosed directly from a radiation
model), and whether the observational error and natural variability
over this time period would allow the expected change to be
detected. Actually, detecting the changes over the observational
record are difficult enough even if one uses a more sensitive
and useful measure, like spectral radiance; for what it's worth,
the observational analysis of Harries et al (Nature 410, 2001),
which is much more carefully done, claims to directly detect
the signature of CO2 radiative forcing. I have my doubts as
to whether the data is really up to the task of demonstrating
that with confidence over such a short time period (which
says more about observing systems than our understanding
of the physics, which is amply illustrated by the excellent
comparison between observed and simulated present day spectra).
But Harries et al -- which uses a line-by-line model -- unambiguously
demonstrates that increasing CO2 leads to an increasing greenhouse
effect.

I'll note also that the inference about water vapor feedback
near line 102 is incorrect, as line-by-line models amply confirm
the reduction of OLR with increasing water vapor content. For that matter,
the supposed result is even inconsistent with the authors claims
of a water vapor effect in Fig. 23. Perhaps the source of the
error lies in the fact that the author is comparing results from
different temperature profiles, but there are other possibilities
for where he might have gone wrong.

There is a lot of other nonsense in this paper (e.g line 209 regarding
the demand that IPCC declare and accept a standard atmosphere), and moreover a
lot of generally insulting language that has no place in any scientific
publication. But I think the above examples will suffice.

In closing, let me say also that I find it peculiar that
Miskolczi is submitting this to a journal with a focus
on public health. I do understand that this special
issue has as a goal exposing the environment and public
health community to some areas of climate science outside
their normal areas of inquiry, but wouldn't it be reasonable
to expect such extraordinary claims as the author's (which
are inconsistent with most of the radiative transfer work done
in the past half century or more) to first past muster in a journal
such as JGR-Atmospheres or JSQRT, which engages the physical science
community? It is the author's misfortune that despite the unconventional
venue he has chosen, the Editorial staff was adept enough to route the
paper to somebody who in fact understands the subject matter. Despite
having been frequently informed of what is wrong with his argument,
Miskolczi continues to shop this work around, presumably in the hope
of eventually finding reviewers gullible or lazy enough to let it through.
I do not really know the author's motivations, but this is certainly a
shameless abuse of the peer review system.

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* denotes required fields.