

## ***Interactive comment on “Clouds and the Faint Young Sun Paradox” by C. Goldblatt and K. J. Zahnle***

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We thank Itay Halevy for his detailed review of our manuscript. We provide a response to each of his comments below

General comment 1:

The fundamental issue raised in this comment is whether 1-D models of the climate system are appropriate to model changes in climate. Our position is that 1-D models are very useful in addressing first-order questions of climate change, backed by a long history of their successful application. Two features of 1-D models make them useful for a study of this type. First, they can be run very quickly, facilitating the large exploration of phase space that we do here. We ran millions of profiles, which simply

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could not have been done in 3-D. Second, the assumptions put into 1-D models can be (and are here) simple and transparent. This means the reader can make his or her own informed judgment about how to interpret the results. By contrast, complicated 3-D models are far from transparent in the parametrisations used and there are rarely well stated in publications: detailed results are often presented, but it is generally impossible to tell what they mean. In particular, it must be borne in mind that even in high resolution numerical weather prediction models, clouds are sub-grid scale and parameterised rather than resolved. These parameterisations may not perform well under highly different climates. Cloud resolving models are a long way away. Thus fully exploring the parameter space in a simple model, as we do here, can help us learn a great deal about the system.

In response to specific issues included in this point:

“There is no physical meaning to globally averaged [cloud properties]... These are just parameter values”. We disagree. We demonstrated with figures 2 and 3 that clouds are not randomly located in altitude-latitude space. There is distinct structure. Thus reducing the problem with averages is physically justified. Moreover, whilst we allow cloud parameters to vary freely in the sensitivity test, we constrain them as tightly as possible to the available data for our case study. This is described in section 3.4 of the manuscript (some extra emphasis added in revised version).

“no guarantee that any of these properties will remain the same when pCO<sub>2</sub> is increased”. Of course, nothing is guaranteed. However, at first approximation, the cloud structure of the atmosphere will depend on the vertical structure of the atmosphere. By using a fixed T-p structure, we avoid entering into a regime where major changes should be expected. Changing atmospheric composition will likely cause some cloud changes. Resolving them is beyond the scope of this study and should be tackled with a cloud resolving GCM, at such a time that one exists.

“Finally, being unidimensional, the model is incapable of capturing the effects of adding

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clouds on global climate dynamics, such as changes to the meridional temperature gradient and heat transport.” Agreed. We make no such claims. This applies to all 1-D models. We discuss only global mean temperatures and not temperature distribution, or anything which would depend on temperature distribution.

“All of this means that Figs. 7 and 8 (and the related text) simply present a forcing sensitivity different from the clear-sky case, but not necessarily one that is any closer to being true. This should be made clearer in the paper.” We disagree strongly. By including clouds which are treated in a physically correct way, we think we are likely to be much closer to ‘true’ than with a non-physical representation of clouds. The key relevant processes are absorption of long-wave radiation and scattering of short-wave radiation by cloud particles. We represent these, whereas a clear sky model does not. The relative strengths of these processes may change over the conditions which we explore, but we are confident that the ‘error’ here will be much smaller than the ‘error’ from not representing these processes at all. Of course a model is only a model, and never ‘true’, but we strongly believe that incorporating the correct physics will make ours a better model. Nonetheless, we have followed the suggestion of making this clearer, emphasising in the discussion of figure 7 that the clear sky model is compared to our ‘real cloud case study’, not to the (unknown) ‘truth’.

General comment 2:

We broadly agree with this. In presenting results, there was the choice between using a lower solar flux, more representative of the Archean, or using the present solar flux, which could be done consistently through the paper and was fully consistent with our model validation. After some deliberation, the latter won.

Figures 4, 7 and 8 show the radiative forcing from increasing CO<sub>2</sub>. CO<sub>2</sub> is a strong long-wave absorber, but a weak shortwave absorber. Hence the radiative forcing in the longwave region is an order of magnitude stronger. Repeating the sensitivity test in figure 4 with both reference and high CO<sub>2</sub> cases using 80% of the present solar flux

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yields radiative forcings from increasing CO<sub>2</sub> which are different by 0.2 to 0.3 Wm<sup>-2</sup>. This would not cause any noticeable change in the histogram, as suggested by the review. Thus re-plotting figures 4 or 7 does not seem to be called for. Figure 8 shows long-wave fluxes only.

A related issue – and perhaps the one which this comment is aimed at – is what the forcing from changing the solar constant would be in the cloudy versus cloud free models. This can be computed directly from TOA fluxes in figure 5. As a higher proportion of incoming solar radiation is absorbed in the cloud free model (planetary albedo of 0.236 compared to 0.310) the the reduction in energy supply with a cut in solar forcing is larger. For an 20% decrease in solar flux, representative of the late Archean, the decrease in absorbed solar flux is 52.3 Wm<sup>-2</sup> for the cloud-free model compared top the cloudy model. This is a different error in the cloud-free model. Proponents of the cloud free model will no doubt be pleased that it is of opposing sign. We have added a paragraph discussing this.

General comment 3:

These are interesting questions, but beyond the scope of this paper. RRTM is designed and tested for a modern atmospheric composition, and attempting to modify it risks introducing error. We see the strength of the approach here as making the minimum changes from the modern profile. If we did remove ozone, we would have to use a different temperature profile to be self-consistent, and this would introduce uncertainty.

There are larger changes which we omit too: with no oxygen, our base assumption should be that surface pressure was 0.8 bar, though the N<sub>2</sub> content likely varied too. These changes are beyond the scope of this paper too. For Rayleigh scattering, the effect of including up to 0.1 bar of CO<sub>2</sub> will be larger than removing O<sub>2</sub>.

We intend to address these issues in a future paper, using a line-by-line model which is highly modifiable for different compositions.

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Specific comment 1:

We have made our first mention of this in section 1 clearer to address this comment. We do not think that this is the appropriate forum to discuss the criticism of the hypothesis in detail.

Specific comment 2:

Clarified in the text. We meant collision-induced absorption (absorption due to forbidden transitions) which Jim Kasting tells us becomes important at  $p\text{CO}_2 \sim 0.1\text{bar}$ . These transitions are not in HITRAN. This is a different issue, as we understand it, from continuum. Of course, the reviewer has the best (and unpublished still?) data on this.

Specific comment 3:

We have changed this instance to “relative to when clouds are included in a physically based manner”. See our response to general comment 1 for general issues.

Technical comments

1. Done.

2. Done.

3. Fixed.

4. Fixed.

5. This sentence was erroneous and has been removed. Discussion of this figure in the text enhanced.

6. This error was introduced by the sub-editor. We gave very clear instructions not to change our original marker, but these were ignored. We ask that the editor addresses this issue with the sub-editor.

7. They look fine from here... (?)

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Interactive comment on *Clim. Past Discuss.*, 6, 1163, 2010.

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