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

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This is an important and well-written contribution to perhaps the most charismatic problem in deep-time paleoclimate study, the "Faint Young Sun Paradox" (FSYP). The authors provide a very useful comparison between the results of radiative transfer calculations carried out either with or without clouds. Analysis of the differences between the clear-sky and cloudy case (Section 4, Figs. 4–5) is clear and insightful, providing thorough explanations for these differences. Perhaps most useful is the parametric study of cloud radiative forcing sensitivity to cloud properties (Section 5, Figs. 11-13 and related text). Finally, the critical review of recent suggestions of the importance of clouds to solving the FYSP (Rondanelli & Lindzen, 2009; Rosing *et al.*, 2010) is important in that it quantitatively demonstrates that the suggested mechanisms may have helped, but not solved the FYSP on their own.

This said, whereas the case study (default cloud parameter combination) described in the paper is suitable as a basis for comparison with other cloud parameter combinations (as in the sensitivity study in Section 5), it may not be justifiable as a physical representation of the climate system any more than the clear-sky (+ increased surface albedo) parameterization (see General Comment 1).

General Comments:

1. I have concerns about the ability of a global average column to dependably simulate the effect of clouds over a wide range of conditions (that is, outside of its calibration to the modern energy budget). The tuning of cloud parameter choices to achieve energy balance and reproduce fluxes at the top of the atmosphere, whilst more subtle, is conceptually similar to what the authors dub whitewashing of the surface in the clear-sky approach. There is no physical meaning to globally averaged cloud fractions, vertical distribution, spatial overlap, water path, etc. These are just parameter values that yield modern-like atmospheric energy budgets—a greywashing of three atmospheric layers if you like. Moreover, there is no guarantee that any of these properties remain the same when $p\mathrm{CO}_2$ is increased. At $p\mathrm{CO}_2$ of 0.1 bar, for example, strong infrared cooling in the atmosphere could affect the altitude at which the clouds form and also the occurrence of water versus ice clouds. The ability to more closely reproduce a variety of fluxes in the atmosphere (Fig. 5) is simply an outcome of a greater number of tunable parameters in the cloudy model. Finally, being unidimensional, the model is incapable of capturing the effects of adding clouds on global climate dynamics, such as changes to the meridional temperature gradient and heat transport. 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.

All of these uncertainties aside, a very useful contribution of the present study is the distribution of possible radiative forcing under (variably) cloudy conditions (Fig. 4). This lends confidence that even if cloud properties were to change markedly (regardless of whether or not they represent the global average in any meaningful way), the radiative forcing due to an increase in $p\mathrm{CO}_2$ is bracketed within the distribution presented and different from the clear-sky forcing for the same $p\mathrm{CO}_2$ increase.

2. Using the present solar flux in these calculations results in an overestimate of the negative shortwave radiative forcing caused by addition of clouds. Whereas the effect of clouds in the IR is independent of incoming solar radiation, their shortwave effect depends on the incoming solar flux. As an extreme example, during polar night clouds provide only positive forcing because there is no incoming solar radiation to scatter or absorb at altitude. A decrease in the solar flux of 20–30% should correspond to a decrease in the negative shortwave forcing, a wider range of conditions under which clouds provide net positive forcing, and perhaps an overlap between the radiative forcing histogram with clouds and the forcing in the clear-sky case (Fig. 4). This doesn't make the cloud free methodology any more or less correct, but it does mean that the error due to its use is smaller in deep-time paleoclimate studies (compared to recent or modern climate studies) by virtue of a less luminous Sun. There is no need to redo most of the calculations presented in the paper, but Figs. 4, 7, 8 should be remade with a weaker solar flux.

3. I agree with the authors that in the absence of oxygen and ozone the difference in the sensitivity to increasing $p\mathrm{CO}_2$ between the clear-sky and cloudy model would be greater in the longwave. In the shortwave, however, exclusion of oxygen and ozone results in a more strongly scattering atmosphere (higher single scatter-

ing albedo). The effect that this has on radiative transfer with or without clouds depends on the absorption and scattering properties of the clouds themselves, but is worth examining. It would be useful to include a comparison between the effect of adding clouds to an oxygen- and ozone-free atmosphere and the effect of adding clouds to an atmosphere with these gases.

Specific Comments:

1. p 1166, line 13; p 1182, line 6; p 1185, line 7: The “iris” hypothesis is mentioned three times as being controversial. It may be useful to very briefly outline the hypothesis and the controversy.
2. p 1171, line 7–9: Collision-induced absorption (CIA) is not included in HITRAN, but a parameterization of the CO_2 continuum (CIA and sublorentzian absorption in the far tails of the absorption lines) is included in LBLRTM (Clough *et al.*, 2005). I am not completely up to date, but it may be included also in RRTM and this should be checked.
3. e.g. p 1183, line 24: For reasons mentioned in General Comment 1, I’m not sure that referring to the cloudy calculations carried out in this study as “real clouds” is justified. The contribution of this study to our understanding of the importance of clouds in deep-time paleoclimate calculations is substantial enough even without the implicit claim that the global average cloud parameterization represent reality.

Technical Comments:

1. p 1164, line 25: The first time “Ga” is used it should be defined.
2. p 1165, line 2: Consider adding “or both”, as both the greenhouse effect and the planetary albedo may have been different.

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3. p 1181, line 14: The sentence “The Archean is that the atmosphere. . . ” does not make sense.

4. p 1185, line 12: “warrant” instead of “warrent”.

5. Fig. 8 caption: Last sentence is internally inconsistent. If D_G = G(CF)-G(RC) is positive as in Fig. 8c, then the greenhouse effect is stronger for the cloud free case.

6. Fig. 12 caption: Last sentence should read “Marker ($\star$)”, not “Marker ($*$)”.

7. Fig. 13: Numbers on the bottom of the color bar are cut off.

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