Biogeosciences Discussions, 2, S734–S738, 2005
www.biogeosciences.net/bgd/2/S734/
European Geosciences Union

Biogeosciences
Discussions

Interactive comment on “Causes and timing of future biosphere extinction” *by* S. Franck et al.

Anonymous Referee #2

Received and published: 29 November 2005

My comments are mainly general. As I have serious concerns on the way results are presented, I did not focus on minor remarks at that point. It should be noted that I am familiar with atmospheric science and numerical simulations in this particular field but much less with geochemistry. I am afraid that most of my comments originate from this biased viewpoint. A key quantity of the model is the mean surface temperature of the Earth, which is a (complicated) function of the composition of the atmosphere (including clouds), the surface albedo and the solar insolation. While the equations presented by the authors deal with biomass and CO2 exchange between reservoirs, the way climate is included is eluded. I have thus serious problems to understand the bases of the model.

1) The authors use a parametrization that couples several reservoirs. Although it is not much discussed, I assume that the values chosen for the parameter are inferred from measured fluxes at present day and/or tuned to fit with some of the chosen constraints (for instance Ts$>$70°C at 3.5 Ga, Ts= 15°C today, etc...). Uncertainties on the

parameters lead to uncertainties on the results. The sensitivity of the model to these parameters and their uncertainty (and thus its robustness to provide quantitive results) is not addressed in the paper and, as a consequence, the evolution simulated remains strictly qualitative and only general behavior of the system can be described. The reader will wonder what quantitative information should be derived from these runs, as results are given without uncertainty. For instance, when it is said that "The ultimate life span of the biosphere is defined by the extinction of procaryotes in about 1.6 Gyr", is it 1.6+/-0.3 Gyr or is it 1.6 Gyr +/- 1.6 Gyr ? From the mathematical form of the equations (1) to (6), I suspect a strong sensitivity to the individual parameters and a dramatic sensitivity to the whole set of parameters. I also guess that the tuning of the parameters in order to fit the geological constraints (that has to be done before any further extrapolation of the model into unconstrained periods and future) can itself be done within a wide region of the parameter space. When using such model, one of the thing that can be done is to compute the evolution for different sets of parameters randomly chosen within a realistic interval and to plot all the curves together. This is simple, not time-consuming with this kind of equation system, and would allow the user to estimate the required error bars. With such method or any other, 1) the uncertainties on the parameters and 2) their collective impact on the result have to be discussed. The authors mention a bistable solution found at some ages (which by the way illustrates the non-linearity of the model and the high sensitivity to the parameter that can be expected) but without explaining the origin of the bifurcation and the conditions at which it is found. Many parametrized models (or "box models") are published without sensitivity study but they are generally used only to identify general trends and properties of the possible solutions. Inversely, the authors insist on the quantitative aspect of their modelling. They really should not. I searched in earlier publications of the authors for a more detailled description of the model but I did not find a satisfying discussion of what can be realistically done with the model and what cannot. (Estimates of the edges of circumstellar habitable zones made by the authors and based on similar calculations are also lacking sensitivity and uncertainty discussions, although they are also claimed to be quantitative studies that question the results obtained by climate modelling.) Beyond the sensitivity study of the mathematical approach itself, the authors should explain in more details how the model is sensitive to purely geophysical assumptions, such as continental growth (as an example that comes to my mind), and of course climate, which is my second concern.

2) It was not clear at all from the equations given in section 2 how the mean surface temperature was computed. Where does Fig 1 (a) come from ? In all the equations given, Ts is a parameter and fig 1 (a) seems to present it as a solution of the model. If the model includes a parametrization or tabulated values for Ts as a function of atmospheric pressure, greenhouse gases abundance (CO2 in this case), and solar luminosity then this information should be given in the paper. For instance Fig 1(a) shows values of Ts above 80°C for $t>3$Ga. Even the more detailled climate models would have problems to find a solution for such high temperature. In fact the choice of $Ts>80$°C at $t>3$ Ga (from Knauth and Lowe, 2003) seems unrealistic and terribly difficult to reproduce with a climate models. Surface temperature between 80°C and 100°C would imply a partial pressure of H2O between 0.5 and 1 bar and a very high level of CO2, assuming that CO2 can sustain such high values of Ts, which I doubt. Indeed, several groups working on climate modelling are presently trying to obtain the mean surface temperatures claimed by Knauth and Lowe, by playing with CO2 and CH4 but they remain unsuccessful yet. Modelling the radiative transfer in an atmosphere where the greenhouse gases reach partial pressure of more than 1 bar is extremely challenging, and in this case we would have more than 1 bar of CO2 and about 1 bar of H2O in addition to N2. Greenhouse would be dominated by pressure broadening for which we are lacking data. Moreover, these surface partial pressures of H2O are associated with high level of stratospheric water vapor, resulting in an efficient escape of H to space. Kasting (1989) estimated that 1 terrestrial ocean can be lost in less than 0.5 Gyr in such configuration. Although I do not fully agree with this result from Kasting [because it assumes that the loss to space would be limited by the photolysis of H2O while I think that it would be limited to lower values by the temperature of the upper

atmosphere], huge amounts of H would still be lost. Consequences of that would be a dramatic increase of the D/H ratio and considerable leftovers of O2 (˜1 bar of O2 for each ten meters of H2O lost). Confused by the problem of this high temperature at 3.5 Ga, I asked their opinion to some geochemists. Their answer was that the cherts used to infer a temperature (from oxygen isotopes) all seem to have formed in hydrothermal environments and do not reflect the surface temperature. This point is debatted in the paper by Knauth and Lowe but it seems that the way they rule out this possibility does not meet a general agreement in the community. I do not have the expertise to take position on this specific question, though. Also, if the model presented by the authors do agree with this high value for Ts without tuning, it should be explained explain in more details what are the processes that prevent the "classic" carbonate-silicate regulation of Ts to work: at 3.5 Ga, about 100 mbar of CO2 are required to warm the surface above 0°C, so this is what one would expect if the carbonate-silicate stabilization of Ts is at work. Sleep and Zahnle (2001) claimed that such high values would be unstable due to carbonitization of seafloors and they concluded that the Earth was frozen. What is found in the present paper goes in the opposite direction and is thus a very important result. Such a discrepancy should be explained to the non-geochemist that I am. I suspect that tuning the parameters to fit these very high values of Ts has major consequences on the further evolution. I suggest to check the evolution for a much cooler surface temperature at $t>3$ Ga (let's say $T<30$°C)

3) I would expect some discussion on the importance of biological evolution and selection on the maintenance of life. The limits for life used in the model are the one inferred from presently known organisms. Life might be able to adapt to some of the dramatic changes predicted in the paper or able to counter them by providing climate feedbacks. For instance, the main problem is warming: the solar luminosity increases, inducing a decrease of CO2 until a point where the temperature increases despite very low levels of CO2. However, biogenic feedbacks inducing a cooling might be plausible. An increase of the albedo or the emission of anti-greenhouse gases could delay the sad ending... For instance, the emission of gases that would absorb the direct solar

luminosity in the middle atmosphere would produce a strong stratospheric warming but could lower the surface temperature. An atmospheric composition favouring the formation of clouds could also result in a strong cooling. These hypotheses could seem a little bit too "Gaia-like" but I do think that simulating the evolution of Earth with a biosphere that evolve only by transferring biomass from one group of known organisms to another is unlikely to describe the real future and past of our planet. (We could even imagine that if mankind eventually survive its present uncontrolled effects on the climate system, the human civilization could "engineer" the atmosphere to maintain habitability longer than the biosphere could do.)

4) I believe that the introduction of a biogenic flux of CH4 from the methanogenic pool of the prokaryotes, that can produce high surface temperature before the rise of O2, would significantly change the story, right ?

Interactive comment on Biogeosciences Discussions, 2, 1665, 2005.