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Hydrology and Earth System Sciences Discussions

Interactive comment on "Biological catalysis of the hydrological cycle: life's thermodynamic function" *by* K. Michaelian

K. Michaelian

karo@fisica.unam.mx

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Complete response to Prof. Makarieva:

1 Basic equations

In a previous response (Michaelian, 2011a), I have addressed Prof. Makarieva's criticism (Makarieva, 2011) of Ulanowicz's (1987) formula for the global entropy production of Earth used in my paper (Michaelian, 2011b). What Prof. Makarieva overlooked when using the Ulanowicz formula to find a spurious value for Earth's entropy production is

that the energy flux radiated by a black-body given by the Planck formula

$$I(\nu) = \frac{2h\nu^3}{c^2}\frac{1}{e^{h\nu/kT}-1}. \quad (1)$$

is, as was stated in my manuscript, the energy radiated *per unit area of the radiating body, per unit solid angle* [subtended by the source at the observer], and per unit frequency. The emphasis is on the information that Prof. Makarieva has overlooked when performing her calculation with the Ulanowicz formula. If one does not take the normalizing solid angles and areas into account, then one is effectively calculating the energy flux on the surface of the Sun, as realized by Prof. Makarieva. Of course, one cannot compare apples with oranges; both the incoming and outgoing entropy fluxes must be calculated and compared *at the Earth's surface*.

Prof. Gorshkov's equation, defended by Makarieva, for the entropy production per unit surface area of Earth is of the heat flow type

$$p = I_S\left(\frac{1}{T_E}-\frac{1}{T_S}\right)(1-A), \quad (2)$$

where I_S is the average solar radiation incident on Earth, and A its albedo. There are two crucial points to note about this equation; 1) it is a heat flow type equation which incorrectly treats photons as if they were heat, 2) there is absolutely no dependence on the frequency or wavelength of the incident or emitted radiation. Point (1) implies that this type of analysis suffers from a number of deficiencies related to the distinct nature of photons from heat. Heat flow analysis neglects the increase in entropy that the photon flux experiences due to its expansion in space on leaving the Sun, which is a wavelength dependent effect (Wu et al., 2011). Radiation pressure is also not taken into account. Furthermore, not all of the high energy photons from the Sun are absorbed and dissipated; a portion, corresponding to the planets *wavelength dependent* albedo, are reflected directly back into space with no change in wavelength. However, this scattering from the atmosphere or surface leads to a more isotropic distribution of photons than that of the incident beam, and thus this reflection by the planet also contributes to its entropy production, which is also wavelength dependent, and which is also not included in heat flow calculations.

Not having any dependence on the frequency or wavelength (point (2) above) is perhaps the most serious problem with heat flow type calculations. This means that this equation can only be used under the very special conditions in which the particular frequency or wavelength distribution of the radiation is not important. This is the case only when the system is in thermodynamic equilibrium and the radiated spectra are black-body. However, neither the Earth nor the Sun are in thermodynamic equilibrium (e.g. the existence of life on Earth and convection on the Sun). Heat flow calculations have no choice other than to assume black-body spectra for the incident and emitted radiations in order to obtain equilibrium temperatures at which the flux occurs. However, the true incident and emitted spectra are significantly different from those of a black-body (Wu et al., 2011). Contrary to what both Profs. Gorshkov and Makarieva claim, albedo is *not* the only variable that life can affect in contributing to the entropy production of Earth. It is the wavelength dependence of Earth's emitted (radiated) spectra that life affects and this affects most the entropy production (see table 1 of revised text).

Wu and Liu (2010) have demonstrated that the net entropy flux of Earth calculated through heat flow type equations is about 30% to 40% lower than that obtained through an accurate photon analysis. Indeed, using the albedo of Earth $A = 0.3$ in the Gorshkov equation (I find it curious that Makarieva left out Earth's albedo in her calculation), and values for the Sun's temperature $T_S = 6073$ K, the Earth's temperature $T_E = 278$ K, and the incident energy flux , $I_s = 340$ W m^{-2} as given by Makarieva (2011) gives for Eq. (2)

$$p = 0.817 W m^{-2} K^{-1}, \qquad (3)$$

about 36% lower than the more accurate photon analysis calculations of Wu and Liu (2010).

In the revised version of the manuscript I have dedicated a paragraph to describing why heat flow type equations give very poor estimates for the entropy production of planets. In order to claim rigor myself, I have also removed reference to the Ulanowicz equation which, although taking into account at least some wavelength dependent characteristics of the dissipation process, and therefore providing a better approximation than heat-flow type calculations (of course once taking correct normalizing areas and solid angles for the incident photon flux into accout), it is still only an approximation. Instead, in the revised version, I have taken the advice of the anonymous referee and followed Wu and Liu (2010) and used the correct Planck formula for the photon entropy flux. Since actual incident and emitted spectra for the planets are not available, I have used a gray-body approach to obtain the incident and emitted spectra. Although this too is an approximation, it is still a wavelength dependent approach and therefore avoids most of the deficiencies associated with the heat flow analysis. In any case, this formalism is ready for precisely determined spectra once the data become available from satellites.

Finally, although Prof. Makarieva stresses the importance of "quantitative calculations", the quantitative values of the entropy production of the planets is really of only minor relevance to the point being made in my manuscript. This point is that Earth's entropy production per unit area is significantly greater (almost double) than that of its neighbors (no matter what formalism is used to calculate it), and this may perhaps be attributable to life (see revised text for arguments in support of this suggestion).

2 Literature analysis

Prof. Makarieva suggests that my reference to Zotin's (1984) paper is incorrect since Zotin's analysis is "misleading" and has "been refuted by recent research", citing three works, including one of her own (Makarieva, 2008). However, the very first point to be made here is that nowhere in the original manuscript did I make reference to Zotin's data (although I did in my preliminary response to Prof. Schymanski; correctly acknowledged by Makarieva).

Having made that clear, I would now like to defend both Zotin and my use of Zotin's data employed in my response to Prof. Schymanski concerning evidence for the idea that irreversible biotic and abiotic processes have coupled over the history of life on Earth and that this has led to an increase in Earth's global entropy production.

Controversy in science is normal; indeed it is part of a healthy scientific process. Almost all works in science are eventually supplanted by new analysis, but the pioneering work serves greatly as a first approximation or first model, and here Zotin should be recognized and commended for being the pioneer that he was in an astounding number of areas. Zotin's work has been criticized but it has also been amply defended and employed (Kleimonov, 2002; and hundreds of more citations) and even generalized (Chaisson, 2001). Moreover, the fact that one has written an article reporting to refute another's, does not automatically disqualify the original work, and much less *all* of the work of that particular author. In my view, that part of Zotin's work that I employ, regarding increases in the metabolic rates of organisms over the history of life on Earth, has not been generally invalidated by the scientific community. The more stringent suggestion that the metabolic rates of organisms *per unit biomass* has increased over the history of life on Earth is more controversial and, as I understand it, is what Prof. Makarieva is criticizing. Her criticism of Zotin on this aspect may, or may not (see below), be valid, but nowhere in my article, or even in my response to the other referees, do I employ this more stringent proposition. Prof. Makarieva's criticism of my citing Zotin, is thus completely misplaced since it has nothing to do with that part of Zotin's data I was referring to in my response to Prof. Schymanski.

Zotin's data concerning the increase in the overall metabolism of organisms over the evolutive history of life on Earth is supported by a number of distinct lines of inquirey;

1. Whenever irreversible processes couple, the net entropy production increases (Onsager, 1931; Morel and Fleck, 1989). There is undeniable evidence that the number of the domains, kingdoms, species, and individuals, has grown over time, implying new coupling (eg. symbiosis and endosymbiosis, mutualism and comensualism). All of these imply new and coupled irreversible processes, and hence, by Onsager's principle, increased entropy prodcution.

2. The origin of life some 3.8 billion years ago must have been a humble event, contributing very little to the overall entropy production of Earth. Most would be in agreement that todays ecosystems are considerably more complex and dissipative than that at the very beginings of life. Of course, one must also consider the coupling of biotic processes with abiotic processes, however, Onsagers principle suggests that the spontaneous coupling of irreversible processes is such that entropy production *increases*, unless external conditions are changed. Human society is another prime example of this (Zotin, 1990).

I have now decided that it is imperative to include reference to Zotin in the revised manuscript.

Regarding Makarieva's criticism of Zotin's mass-specific trends in the evolutionary history of life on Earth (although this has nothing to do with my article, nor with my response to the referees), Zotin's general outlook on evolutionary trends is more thermodynamic (as I see it), emphasizing "entropy production" (Zotin, 1990), while that of Makarieva (2008) emphasizes "energy use" which has little thermodynamic justification for being a varible describing evolutionary trends, except, perhaps, under very restrictive conditions.

Prof. Makarieva's comment, that I should have been "more cautious" when citing works to "avoid misleading the reader", is unfair and I ask her to consider whether she, herself, has not breached a similar criterium in writing her comment. Finally, I find it curious that Prof. Makarieva did not, more correctly, object to my citing in the manuscript one of her works (Makarieva and Gorshkov, 2006), even though this work has been strongly criticized by another (Meesters et al., 2007).

All said, I sincerely thank Prof. Makarieva for her comment. It has led to considerable reflection that has certainly improved the manuscript.

References

Chaisson, E. J.: Cosmic evolution: rise of complexity in nature. Harvard University Press, Cambridge, 2001.

Kleimenov, S. Y., and Alekseeva, T. A.: Test for Bioenergetic Progress and Specific Energy Metabolism in Isopod Crustaceans (Isopoda) of Various Ecology, Biology Bulletin 29, 473–476, 2002. DOI: 10.1023/A:1020413826516

Makarieva, A. M. and Gorshkov, V. G.: Biotic pump of atmospheric moisture as driver of the hydrological cycle on land, Hydrol. Earth Syst. Sci., 11, 1013-1033, doi:10.5194/hess-11-1013-2007, 2007.

Makarieva, A. M. Gorshkov, V. G., Li, B. L., Chown, S. L., Reich, P. B., and Gavrilov, V. M.: Mean mass-specific metabolic rates are strikingly similar across life's major domains: Evidence for life's metabolic optimum, PNAS 1005, 16994–16999, 2008.

Makarieva, A. M.: Interactive comment on "Biological catalysis of the hydrological cycle: life's thermodynamic function" by K. Michaelian, Hydrol. Earth Syst. Sci. Discuss., 8, C549–C554, 2011 www.hydrol-earth-syst-sci-discuss.net/8/C549/2011/.

Meesters, A. G. C. A., Dolman, A. J., and Bruijnzeel, L. A.: Comment on "Biotic pump of atmospheric moisture as driver of the hydrological cycle on land" by A. M. Makarieva and V. G. Gorshkov, Hydrol. Earth Syst. Sci., 11, 1013–1033, 2007, Hydrol. Earth Syst. Sci., 13, 1299-1305, doi:10.5194/hess-13-1299-2009, 2009.

Michaelian, K., Preliminary reply to Prof. Schymanski, Author comment, Hydrol. Earth. Syst. Sci. Discuss. 2011a. http://www.hydrol-earth-syst-sci-discuss.net/8/C543/2011/.

Michaelian, K.: Biological catalysis of the hydrological cycle: life's thermodynamic function, Hydrol. Earth Syst. Sci. Discuss., 8, 1093–1123, 2011b. doi:10.5194/hessd-8-1093-2011.

Morel, R. E., Fleck, G: Onsager's Principle: A Unifying Bio-theme. J. theor. Biol., 136, 171–175, 1989.

Onsager, L.: Reciprocal Relations in Irreversible Processes. I., Phys. Rev., 37, 405–426, 1931.

Ulanowicz, R. E., Hannon, B.M.: Life and the production of entropy. Proc. R. Soc. Lond. B 232, 181–192, 1987.

Wu, W. and Liu, Y.:Radiation entropy flux and entropy production of the Earth system. Reviews of Geophysics, 48, RG2003, 2010.

Wu, W., Liu, Y. and Wen,G.: Earth Syst. Dynam. Discuss., 2, 45–70, 2011 www.earth-syst-dynam-discuss.net/2/45/2011/ doi:10.5194/esdd-2-45-2011.

Zotin, A.I., Thermodynamic Bases of Biological Processes: Physiological Reactions and Adaptations, de Gruyeter, New York, 1990, ISBN 3-1 -0111401-1.