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Interactive comment on “Ice-driven CO_2 feedback on ice volume” *by* W. F. Ruddiman

V. Bol’shakov

vabolshakov@mail.ru

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I greet the author of the paper, W.F. Ruddiman, for his bold attempt to solve the fundamental and most difficult problems of the orbital theory of paleoclimate: the 100-kyr problem, the middle Pleistocene transition problem (the dominance of 41 kyr cycles in climate changes during the late Pliocene and early Pleistocene), and the stage-11 problem. I agree with the author that the main condition for solving these problems is careful analysis of different feedbacks transforming the orbital insolation signals into global climate changes. However, there are some basic drawbacks in this paper, which are common to majority of works have been investigating the theory of the Pleistocene climate evolution since the 19th century. Ruddiman assumes quantitative changes of insolation (monthly or half-year insolation at 65oN; Page 44, Line 5, 20-21; P.45, L.2-4; P.54, L.4-5; and others) as main driver of global climatic changes. However, in determining the paleoclimatic significance of rigorously quantified insolation oscillations, related to variations of all three orbital elements, Ruddiman, as many other

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authors, disregards the qualitative differences between these oscillations. It is meant here that the eccentricity is the alone orbital element which variations force changes in the annual mean heat inflow to the whole surface of the Earth. However, obliquity and precession variations virtually do not change the annual insolation received by the whole Earth, merely causing energy redistribution through latitudes and between the seasons, respectively. It is well known that precession variations don't change mean annual insolation at any latitude, i.e. the decreased winter insolation is compensated by increase in summer insolation and vice versa at any given latitude. For this reason long cold winter is accompanied by hot short summer in one Hemisphere and long cool summer is accompanied by mild short winter in another one. Besides, decrease in axial tilt (obliquity) leads to decrease of mean annual insolation in high latitudes and increase in insolation in low latitudes of both Hemispheres; increase of obliquity leads to opposite results. So, mean annual insolation of high and low latitudes changes in opposite phase with variations in axial tilt (phase reversal occurs between 44ž and 43ž latitudes of both Hemispheres). These significant qualitative differences of insolation changes, forced by different orbital elements, are ignored in Ruddiman's consideration, and it is not clear, why CO2 interects in different manner with obliquity and precession insolation signals. This circumstance alone suffices to discard the quantitative indices as being a realistic measure of global climatic impacts from variations of the respective orbital elements. The very technique of assessing the paleoclimatic significance of the variations in insolation incident on the upper boundary of the atmosphere, based on calculating these variations over particular caloric half-years (or, moreover, months), and geographic latitudes, does not stand up under scrutiny. Indeed, one cannot reasonably assume that for a half-year insolation influences global climate, and for another half-year it does not. Likewise, it makes no sense to believe that insolation at a single given latitude governs global climate changes within the entire continuous and interrelated Earth climate system, whose inertia is enormous. The viability of this approach is further challenged by the fact already mentioned that half-year precessional variations of insolation, as well as insolation variations at high and low latitudes due to changes of obliquity, occur in opposite phase. The main conclusions of my consideration are the following: 1. Insolation forcing represented by monthly (or half-year) insolation variations at single latitude should not be used for paleoclimatic modeling and interpretation, because such input signal (e.g. June 65°N insolation) does not fully reflect the changes in the solar radiation really influencing the Earth's climate. As far as input signal is not adequate, anyone can't determine true mechanism for transforming insolation variations into global climate changes, even if the output signal (paleoclimatic proxy records) is adequate. 2. Elaboration of the paleoclimatic model requires incorporation of insolation variations continuous in time (entire annual) and space (for the whole Earth), which are related to variations in all of the three orbital elements. However, if one calculates time-continuous (i.e., entire annual) and space-continuous insolation changes (i.e., those occurring at all the latitudes of the Earth), then the contribution to these changes from axial tilt and precession will prove to be zero. The attention to this "zero insolation problem" was attracted for the first time by J. Herschel, and A. von Humboldt, yet in the first part of the 19th century (Croll, 1875; Imbrie, Imbrie, 1986). As is known, the path of solving this problem was suggested by James Croll, who was the first to take into consideration terrestrial feedbacks transforming the orbital insolation signals into global climatic changes. In my opinion, his discovery is an outstanding achievement of theoretical paleoclimatology. Let us use a concrete case study to demonstrate how important it is to take feedbacks into account. In validating the mechanism of the climate impact from variations of the Earth's tilt e (a decrease in e causes a decrease in annual insolation at high latitudes, thus resulting in cooling), Croll, and then Milankovitch used a positive feedback mechanism, controlled by a change in albedo due to an area change in the ice and snow cover, mainly at high latitudes. However, because during e variations, insolation changes at high and low latitudes occur in opposite phase, the existence at low latitudes of a positive feedback that could be comparable to the high-latitude albedo feedback, would strongly obstruct the unraveling of how variations in the tilt angle influence global climate. This example shows that accounting for the character of feedbacks and orbital signals may help solve the problem of the obliquity- and, possibly, precession-related insolation variations having a zero annual mean for the whole Earth, and to determine the global climatic significance of variations in all of the three orbital elements. So, it may be done the third conclusion: 3. Elaboration of the paleoclimatic model requires incorporation of various terrestrial feedback factors. A rigorous account for a variety of feedbacks is thus among the pivotal prerequisites for elaboration of the orbital theory of paleoclimate. It is precisely by taking into account how these feedbacks operate through time and space that one can solve the problem of zero annual mean insolation changes (related to variations in the axial tilt and precession) over the entire Globe. This would require developing a realistic and accurate mechanism for climatic influences from variations in each of orbital elements, primarily precession. It is common knowledge that there exist different terrestrial feedbacks, some of these being not only positive (due to albedo changes following changes in the volume of ice and snow and vegetation cover of the planet or changes in the atmospheric content of greenhouse gases), but also negative, e.g., due to strengthening of circulation in the atmosphere, tending to reduce the longitudinal temperature gradients, which increase during glaciation events. Contrary to the last remark, I should notice here that Ruddiman, after Raymo and Nisancioglu (2003), regards the enhanced atmospheric circulation connected with increase of temperature gradients due to decreasing of tilt angle as positive feedback (P.54, L.18-21). There are good reasons to assume that different types of global feedbacks exert different relative influences on orbital signals, caused by variations in individual orbital elements. Thus, the positive feedback controlled by albedo changes (primarily at high latitudes of the Earth) will most likely provide the strongest amplification for the insolation signal due to variations in the axial tilt, whose largest relative changes are also related to high latitudes. Changes in the rate of atmospheric circulation, involving changes in temperature gradients between high and low latitudes, would likely exert the most influence on the same insolation signal. The feedback induced by oscillations of the atmospheric contents of greenhouse gases is most likely to exert the strongest influence (relative to its influence on the obliquity signal) on the direct (not connected with precessional modulation) eccentricity signal, changing the annual mean insolation of the whole Earth. The less clear mechanism of precessional influence on climate, associated, in particular, with paleomonsoons (Barron, et al., 1985; Clemens, Prell, 1991) may have a regional rather than a global significance, and, hence, it may exert its specific influence through terrestrial feedbacks on the precessional signal. Contrary to the opinion of Ruddiman (Part 3) I believe, after Budyko, (1977) that the strongest feedback influenced the obliquity and eccentricity signals, is connected with albedo of snow and ice cover, but not with greenhouse gases, in the Pleistocene. This point of view is supported by the fact that characterized by the orbital periodicities climate changes during the Pleistocene were much more strong than during the Mesozoic when ice sheets were absent. The consideration of qualitative peculiarities of individual insolation signals and different terrestrial feedbacks allows also to come to the following conclusions (Bol'shakov, 2001-2005): (a) there might be only a non-linear response to each orbital signal (connected with variations in eccentricity, obliquity, and precession); (b) during the Pleistocene, variations in precession insolation should have less climatic impact than the two other orbital elements. Thus, from the above, a simple qualitative explanation of the problems of 100-kyr period, middle Pleistocene transition, and of the Phanerozoic climatic dynamics which is characterized by orbital periodicities could be put forward. In my opinion, my suggestions (Bol'shakov, 2001-2005), concerning the Pleistocene climate evolution is more realistic than that of W.F. Ruddiman in his paper.

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