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Comment on “Quaternary glaciations: from observations to theories” by D. Paillard [Quat. Sci. Rev. 107 (2015), 11–24]

In the invited review by Paillard (2015), comprehensive information about both theoretical and empirical studies of Pleistocene glaciations and their causes are presented. D. Paillard's review concludes that Milankovitch's (1930, 1941) theory cannot explain the 100,000 yr periodicity of glaciations within the last one million years, and that an additional factor must be responsible for global climate fluctuations. According to Paillard (2015), changes in the carbon dioxide (CO_2) content of the atmosphere are responsible. This statement radically alters the conventional viewpoint about the leading role of orbital variations on global Pleistocene climate (e.g., Hays et al., 1976).

However, Paillard's (2015) conclusions cause serious objections. Furthermore, there are some inaccuracies in Paillard (2015) which lead to an incorrect representation of the development of orbital palaeoclimatic theory. This paradigm relates global climatic variations to orbital insolation variations, as put forward for example, by Adhémar (1842), Croll (1875), and Milankovitch (1930, 1941). In this Comment, we discuss the Paillard's (2015) viewpoints and their controversies.

1. Issues with palaeoclimatic theories by M. Milankovitch and J. Croll

We agree with Paillard (2015:11) that “… it is also a necessity to articulate our understanding of a scientific question.” First of all, it is necessary to express precisely what is the theory created by Milankovitch because there are disagreements on this issue (e.g., Roe, 2006; Bol'shakov, 2008). Perhaps this is why there are inaccuracies in Paillard's (2015) interpretation of it. In the Abstract (Paillard, 2015:11), the author notes that “… Milankovitch's theory predicts an ice age cyclicity of about 41,000 years, while the major periodicity found in the records is 100,000 yr.” In this statement two inaccuracies can be detected: 1) the 41,000 yr periodicity is common for the entire Quaternary, encompassing last ca 2,600,000 yr (e.g., Gibbard et al., 2010), whereas 100,000 yr periodicity prevails only for the last ca 1,000,000 yr; and 2) the contribution of 41,000 yr and 23,000–19,000 yr periodicities to changes of the caloric summer half-year insolation at 65° N, used by Milankovitch to determine the ages for glaciations, are almost equal; the former one prevails in the high latitudes, and the latter one – in the lower latitudes. We note also that Milankovitch's theory cannot explain the Middle Pleistocene transition (e.g., Clark et al., 2006) either.

Paillard (2015:19) states: “Concerning Milankovitch's theory, it is worth insisting on the point that the central object of interest is not “climate”, i.e. the temperature over different regions of the Earth, but the ice sheets …” This is incorrect because Milankovitch (1930) correlated directly (i.e., in linear fashion) changes in insolation, calculated for different latitudes and seasons, with variations in temperature – another words, *climate*. This approach resulted in the finding that changes in winter and summer temperatures for low and temperate latitudes were mirror opposites (see Milankovitch, 1930: diagrams III and IV). Only the deepest insolation minima (i.e., the largest decreases of temperature) for caloric summer half-year insolation at 65° N were interpreted by Milankovitch (1930) as glaciations. Later on, Milankovitch (1941) used the changes in insolation also for detection of snow line altitude; the title of Chapter XXII in this book is “Climatic consequences of the secular march of insolation”.

Another point is that Paillard (2015) inadequately presented the contribution of James Croll to palaeoclimatic studies. The discovery of positive feedbacks in climatic Earth's system is an outstanding achievement of Croll (1875), and these feedbacks are today the basis (and also a challenge) for palaeoclimatic modelling. It is worth noting that Croll's discovery was stimulated by comments made by J. Herschel and A. von Humboldt about the necessity to account for full annual (and not half-year) variations in orbital insolation. These comments stemmed from the observation that annual changes in ‘precession’ insolation are equal to zero for any latitude. This kind of insolation variation was termed ‘compensated’ by Croll. In the light of the fact that ‘precession’ and ‘obliquity’ insolation changes are globally compensated, it is worth citing the following from Croll (1875:13):

> “… Neither would excessively cold winters, followed by excessively hot summers, produce a glacial epoch. To assert, therefore, that the purely astronomical causes could produce such an effect would be simply absurd. … The important fact, however, was overlooked that, although the glacial epoch could not result directly from the increase of eccentricity, it might nevertheless do so indirectly. Although an increase of eccentricity could have no direct tendency to lower the temperature and cover our country with ice yet it might bring into operation physical agents which would produce this effect.”

By the term ‘physical agents’ Croll meant positive feedbacks. He considered two primary mechanisms in this category: 1) feedback between temperature and snow and ice cover (albedo); and 2) feedback between global temperature and displacement of the ocean currents. According to Croll, the impact of feedbacks during long cold winters caused annual cooling despite the fact that such winters are accompanied by short hot summers. Besides this, Croll was the first scholar who identified the correct mechanism for the influence of obliquity variations on climate in high latitudes. During a decline in obliquity the annual insolation in high

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latitudes also decreases, leading to cooling and an increase in ice and snow volume in polar regions of both hemispheres. Vice versa, during an increase in obliquity, insolation will rise, and this will cause melting of polar ice. Using the albedo feedback, it is easy to determine the mechanism for the influence of obliquity variations on global climate (Croll, 1875; Bol'shakov, 2003; Bol'shakov and Kuzmin, 2014).

Obviously, there are some flaws in Croll's theory. The main one is the consideration of precession variations of insolation modulated by eccentricity as the major factor which determines the fluctuations in global climate (see Bol'shakov et al., 2012). Paillard (2015:16) pointed out the shortcomings of Croll: “In particular, once again, the expectations from Croll are entirely reversed: if ice ages are indeed linked to eccentricity changes, they are associated with low values, not high ones.” At the same time, Paillard (2015) did not mention the flaws in Milankovitch's theory. For example, according to Milankovitch glaciations are statistically associated with high (not low!) eccentricity values. This is determined by the fact that precession changes modulated by eccentricity contribute to fluctuations of summer insolation at 65° N along with obliquity variations. Due to this, the lowest insolation values which Milankovitch interpreted as glaciations correspond statistically to higher eccentricity values, and this contradicts to empirical data.

The main deficiency of Milankovitch's theory is obvious: contra to opinions by Herschel and von Humboldt, Milankovitch used for palaeoclimatic conclusions variations in half-year insolation at one latitude only, and this has been pointed out repeatedly (e.g., Imbrie, 1982:413). Thus, we agree with Paillard (2015) that additions to Milankovitch's theory are necessary — but which ones? Below is our opinion.

2. Issues with geochemical theory by D. Paillard

According to the geochemical theory of Paillard (2015:19), fluctuations in the amount of CO_2 in the atmosphere, together with orbitally-determined variations of insolation, are the major factor responsible for Pleistocene glaciations. Changes in CO_2 content influence global climate via the greenhouse effect. Therefore, according to Paillard (2015) CO_2 variations are not the *consequence* of, but a *cause* of global climatic changes in the Pleistocene. Because CO_2 fluctuations, detected in Antarctic ice cores (e.g., Petit et al., 1999; Jouzel, 2013), are similar to temperature changes in the Antarctic and $\delta^{18}O$ variations in deep sea cores, the changes in CO_2 records have the same orbital periodicities as in the above-mentioned palaeoclimatic archives. In this case, one can easily explain the existence of orbital periodicities in ice and deep sea core records by using a simple linear mechanism for transformation of the CO_2 signal into climatic changes. This approach could solve the problem of 100,000 yr periodicity of glaciations. But if so, it is no longer necessary to employ Milankovitch's theory as the explanation for glaciations. This statement seems to be logical when one takes into account the flaws in Milankovitch's theory and their contradiction with observational data.

The geochemical theory also makes the more common orbital theory of palaeoclimate unnecessary. In light of this situation, we reference a new concept of orbital palaeoclimatic theory (Bol'shakov 2003, 2008) which is free of the contradictions and drawbacks of Milankovitch's theory. In this concept, mechanisms of climatic influence of particular orbital elements are presented with the provision that insolation variations for the full annual period and all latitudes must be considered. CO_2 changes are regarded as a positive feedback which result from climatic fluctuations controlled by orbitally-driven variations in insolation.

The mechanisms of climatic influence of orbital elements, used in the new concept, fit well to empirical data by Hays et al. (1976): 1) the glaciations coincide with eccentricity's minima which correspond to decreases in global insolation; 2) coolings in the 40,000 yr harmonics of palaeoclimatic changes correlate with the lowering of the Earth's axis tilt; and 3) insolation changes in the northern hemisphere, determined by precession, coincides with cooling phases in the 23,000 yr harmonics of palaeoclimatic records. Because of this, it seems logical not to begin the development of a new theory when the possibilities within the previous one (i.e., orbital theory of palaeoclimate) still exist. What causes confusion is that Paillard (2015) does not address papers where a solution to the 100,000 yr periodicity problem without control by CO_2 is addressed (e.g., Bol'shakov, 2003; Ganopolski and Calov, 2011).

Paillard (2015:19) also presents other reasons to modify significantly orbital theory of palaeoclimate: “… the astronomical theory certainly needs a geochemical component to account for observed paleoclimatic changes. But this adds complexity to the problem, since a theory of ice ages now needs to account not only for ice sheet changes, but also for atmospheric CO_2 variations.” However, geochemical theory not only “adds complexity to the problem” — their very existence in empirical CO_2 records in terms of orbital periodicities and influence on CO_2 content changes are still remain. In other words, one still needs a solution to the problems of 100,000 yr glacial periodicity and the Middle Pleistocene transition. In this case, why it is necessary to create a new theory at all? Perhaps, Paillard (2015) considers that it is easier to solve these problems for CO_2 records than for $\delta^{18}O$ archives? According to our reasoning, this seems unlikely. For example, the problem of the Middle Pleistocene transition is an intractable task for geochemical theory, because in order to explain it one must assume climatic feedback dependencies on external influences from general climatic conditions on the Earth (Bol'shakov, 2003; Bol'shakov and Kuzmin, 2014). We note that in geochemical theory, changes in CO_2 content do not depend on climatic fluctuations.

We conclude that because the new concept of the orbital palaeoclimatic theory offers solutions to the problems of 100,000 yr periodicity and Middle Pleistocene transition (Bol'shakov, 2008, 2014; Bol'shakov and Kuzmin, 2014), it is not necessary to develop a new geochemical theory. It would be more worthwhile, in our opinion, to continue to enhance the new concept of the orbital theory of palaeoclimate by building its quantitative, mathematically rigorous justification.

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On Quaternary glaciations, observations and theories

1. Introduction

In a recent paper, Paillard (2015) presents a rapid overview of both major theoretical and empirical studies of Pleistocene glaciations. In particular, it is explained how, over the last 150 years, astronomical theories were confronted to observational constraints and why the “100-kyr problem” is still the major unsolved issue of Quaternary ice ages. This paper also discusses the main alternative theory, which involves changes in atmospheric carbon dioxide concentration. It is then argued that a synthesis of both theories would better account for empirical evidences, as well as for our current knowledge of climate physics. Indeed, if there is no doubt that ice ages are “paced” by the astronomy as evidenced in Hays et al. (1976), the cause of terminations, and therefore the dynamics of the 100-kyr cycles, appears to be closely linked to Southern Ocean climate and atmospheric pCO_2.

However, Bol'shakov and Kuzmin (2015) argue that Paillard (2015) is an incorrect representation of the development of the orbital paleoclimatic theory, and that pCO_2 changes have no role in the glacial 100-kyr cyclicity, which is caused by “*eccentricity minima which correspond to decrease in global insolation*”. They unfortunately misunderstood the main points in Paillard (2015), namely that if the pacing is obviously astronomical, terminations appear to have a specific dynamics that can be explained in a consistent way through pCO_2 changes, as evidenced by the clear lead of pCO_2 over ice sheet changes during the last deglaciation (e.g. Shakun et al., 2012). In contrast to their statements, the astronomical and geochemical theories are not exclusive. They are both required to account for ice ages, their pacing and their dynamics.

2. Issues with paleoclimatic theories by M. Milankovitch and J. Croll

The Paillard (2015) paper was not meant to be a detailed and exhaustive account of all the historical development of theories and observations on ice ages. Many books and review papers have been written with this specific objective (eg. Berger, 2012; Krüger, 2013; Woodward, 2014). Still, most of the criticisms raised by Bol'shakov and Kuzmin (2015) are either inaccurate or not quite relevant.

On the statement that Milankovitch predicts a 41 ka cyclicity and not the observed 100-kyr one, they raised two objections. The first one is that the 41 ka cyclicity is indeed observed between 1 and 2.6 Ma BP, which is correct (eg. Lisiecki and Raymo, 2005). But our notion of “Quaternary” is not exactly the same today as it was at the time of Milankovitch. He was concerned with the most recent glaciations and was unaware of earlier ones, in particular the ones before the Middle Pleistocene transition (or MPT at about 1 Ma BP). More precisely, his model was attempting to explain the four glaciations identified in the Alps by Penk and Brückner (1909): Würm, Riss, Mindel, Günz. The comment raised by Bol'shakov and Kuzmin (2015) is therefore technically correct, but inappropriate in an historical context. On a more positive side, we can indeed say that Milankovitch did predict the obliquity-driven glaciations, as observed today before the MPT, a historical fact, which is certainly worth emphasizing.

A second objection by Bol'shakov and Kuzmin (2015) is that “*the contribution of 41 ka and 23-19 ka periodicities to changes of the caloric summer half-year insolation at 65° N used by Milankovitch to determine the ages for glaciations* are almost equal; the former one prevails in the high latitudes, and the latter one – in the lower latitudes”. But since 65°N, or the location where northern hemisphere ice-sheets are expected to grow, is certainly a rather high

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