

Clim. Past Discuss., referee comment RC1  
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## Comment on cp-2021-10

Anonymous Referee #1

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Referee comment on "A Modified Milankovitch theory that reconciles contradictions with the paleoclimate record" by Robert E. Wong, *Clim. Past Discuss.*,  
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This paper has the ambitious goal of rewriting Milankovitch theory in order to explain the apparent 100 kyr cycles seen in global climate records. It claims that it resolves some issues that exist in reconciling current theory with data.

It is quite hard to review this paper because much of the discussion is conceptual and broad statements are made that seem to be correct at a rhetorical level but that have no basis when pulled apart. The basic idea of the paper is that while the annual insolation received by the Earth varies little during an eccentricity cycle, the daily energy received by Earth when it is at aphelion is less than when it is at perihelion; and that this contrast is large when eccentricity is large and zero when eccentricity is zero. Both these statements are obvious and well-known.

The author then says that this means that at aphelion and with large eccentricity, Earth will experience cool winters in one hemisphere and cool summers in the other – this is also true. The logical leap he then makes is to say that this will promote glaciation in both hemispheres. As a statement taken on its own (and not considering other aspects of orbital theory) this is flawed in several ways:

Firstly while it is well understood (and the basis of Milankovitch theory) that cold summers at high latitude allow glaciation because they turn seasonal snowcover into permanent ice cover, there is no basis for the statement about winter. Although it is a gross simplification, to first order cold winters are likely to lead to less snowfall. However the author seems to be referring to something else, which is the proportion of rain to snow. This is a red herring because at the latitudes where an ice sheet would nucleate it never rains in winter. Finally it is also the case that at the latitudes that ice sheets form there is almost no winter insolation (above 67 degrees none at all in midwinter), so changes in insolation through eccentricity cycles are very small. Thus there is no basis for the assertion that the winter hemisphere would accumulate ice at aphelion more when eccentricity is large.

Even more fundamentally, I could equally plausibly make the opposite argument. At perihelion and with high eccentricity, Earth will experience warm winters in one hemisphere and warm summers in the other. This would therefore promote deglaciation in both hemispheres. This obviously suffers the same flaw for winter as the statement by the author. However it illustrates the fallacy of making a bold statement based on only a part of the story: using just the position at aphelion or perihelion, one could argue that high eccentricity promotes both glaciation and deglaciation. There is actually no substitute for looking at the system in the detail required to tease apart the actual energy received in geographical locations (such as 65N and 65S), as is done in traditional Milankovitch analysis.

These two issues come together when one thinks about what happens to a high latitude point through a year at high eccentricity. When it was NH summer at aphelion, the NH ice sheets persist and grow because of low summer insolation (we agree). But the author would claim that the SH ice sheet grows because of low winter insolation (we disagree because the small change in winter insolation shouldn't affect growth), but he ignores the fact that the very high summer insolation at aphelion would melt any excess ice and promote SH ice sheet loss. After 10 kyr (half a precession cycle) everything would be reversed.

Although the paper briefly discusses it the author fails to engage with precession which determines which hemisphere is at aphelion in summer. It is of course through its control on the amplitude of the energy variability during a precession cycle that eccentricity does exert an influence on climate. By ignoring this nuance, the author creates an illusion of simplicity that is not justified. The real hemispheres, at times of high eccentricity, experience both low insolation in a particular season (at aphelion) and, 10 kyr later (half a precession cycle) high insolation (at perihelion). The author chooses only to be interested in the former.

These fundamental problems mean that the paper is not making any breakthrough and is flawed. I will not therefore go into detail on other parts, however a few comments are worthwhile.

I am mystified by Fig 2a: what are the numbers on the curve. They seem somehow to refer to the cycle lengths but they don't seem to be either the distance between adjacent peaks or the result of spectral analysis of the same curve.

An obvious deficit in the paper is that it never shows the climate curves it claims to explain. This is strange.

While the author correctly describes some of the difficulties with Milankovitch theory he does not acknowledge that many of them are satisfactorily explained. For example the change in greenhouse gases (especially CO<sub>2</sub>) over the glacial cycles offers a very satisfactory way of globalising what should be a hemispheric climate response. There are

now several ideas that explain why multiple precession cycles are needed to initiate deglaciation, and these seem plausible (a new idea is welcome but not needed). I'd agree that the strength of MIS 11 is not yet well understood, but the current paper does not address this.