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Climate of the Past Discussions

Interactive comment on "Changing climatic response: a conceptual model for glacial cycles and the Mid-Pleistocene Transition" *by* I. Daruka and P. D. Ditlevsen

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Since the Referees raise a number of points and Referee # 2 raise a series of critical points, we will discuss these in much detail below. For readability, we have repeated the reviews in italics, while our response is in regular text.

Referee 1, M. Crucifix: Received and published: 8 April 2014

1. What are the specific roles of multiplicative forcing vs non-linear climate poten- tial in the dynamical properties of this model (100-ka oscillation, and greatest Lyapunov exponent)?

This is a very good question, which for a complete answer warrants a thorough analysis, partly beyond the scope of this paper, especially with regard to the Lyapunov exponent. In the noise-free case, if the multiplicative forcing is substituted by a constant amplitude sinusoidal forcing we obtain the Duffing oscillator. In that case the non-linear (actually non-harmonic) climate potential has the effect that the internal frequency of the system depends on the amplitude of variations. Since the system is open (forced and damped), the system can resonate at (integer multiples of) the frequency of the forcing. The role of the multiplicative forcing is different.

We have added a paragraph:

The non-linearity in the model occurs in two terms, the non-harmonic potential and the multiplicative forcing. In the noise free case with the multiplicative forcing substituted by a constant amplitude harmonic forcing, the model reduces to the Duffing oscillator: $\ddot{x} = \alpha - x + x^3 - \kappa\dot{x} + A\sin\omega t$. In this case the natural frequency of the system depends on the amplitude of oscillations, which makes the response to the external forcing a resonance phenomenon. In case of multiplicative forcing but with a harmonic potential, we can think of the dynamics as oscillations in a time varying potential $\ddot{x} = (-\omega_0^2 + A\sin\omega t)x - \kappa\dot{x}$. In the case $\omega_0 \gg \omega$, the potential is quasi-stationary, and the system oscillates with a time varying frequency $\sqrt{\omega_0^2 - A\sin\omega t}$. In the case $\omega_0 \ll \omega$ the system will experience a time-mean harmonic potential, and an unaltered natural frequency $\sqrt{\omega_0^2 - \langle A\sin\omega t\rangle} = \omega_0$. The case $\omega_0 \approx \omega$, which is relevant for this study is much harder to analyse. By numerical investigation, we see that the model combining both types of non-linearity gives the best match to the observed record.

2. What could be the physical interpretation given to variations of the damping factor?

We have added:

Kappa has dimension of 1/time, it can be interpreted as an (inverse) time-scale for a Newtonian relaxation back to the mean climatic state.

3. What is the fundamental dynamical difference between Ditlevsen (2009), Huybers (2009), and the present Daruka-Ditlevsen model? How would their signature on the climate record be different, and thus distinguishable? (MPT, amplitude and frequency modulation patterns, spectral signature, phasing with eccentricity ...).

The fundamental dynamical differences are: The Ditlevsen (2009) model is not an oscillator model, it can be seen as an extension of the stochastic resonance model, offering a dynamical explanation for the empirical rule-based model by Pillard (1998). The Huybers model is a discrete map model, based on the classical Imbrie and Imbrie ice volume model. Like the logistic map it can show the scenario of period doubling route to chaos. It thus cannot be directly compared with the data record. The presented model is an oscillator model, where we speculate that the change at the MPT can be seen as a resonance phenomenon (different from the period doubling scenario proposed by Huybers). The question of discriminating between the different proposed dynamical mechanisms proposed by the different conceptual models is very important and interesting. We believe that this should be seriously pursued, but it is beyond the scope of this paper.

4. Same question as above, but with respect to oscillators ?

See answer above.

5. If the signatures are in fact similar, could the authors think of a decisive physical argument?

See above.

6. What is the specific effect of the quasi-periodic nature of the forcing in equation (5) in what the authors have identified as a 'butterfly effect'? What is then the physical implication?

We have added an explanation:

As is seen from the existence of positive Lyapunov exponents (Fig. **??**) for some parameter values in the case of of a simple sinusoidal forcing shows that critical dependence on initial conditions also exist in this case, however, for the system to be chaotic in the mathematical sense it also has to be topologically mixing (any open set in attractor will evolve to cover the attractor densely) and the exists a series of periodic orbits which will converge to any trajectory on the attractor. The question of if and under which conditions true chaos exist in the model is beyond our scope here, We shall only be interested in what is relevant in the context of climate dynamics, namely predictability reflected in critical dependence on climate state and parameters.

Further comments:

p. 1103, l. 14: In Milankovitch compared two solutions: those of Pilgrim, based on Stockwell's integrals, and those of his colleague Miskovic, based on the Leverrier inte-

grals with corrections on the masses. He did not compute astronomical elements himself [see Milankovitch's Canon of Insolation, English edition by the Serbian Academy of Sci- ences and Arts (1998), p. 371]. Milankovitch's contribution lies essentially in (a) the elimination of other effects, such as polar wandering, as explanation of ice ages and (b) the modelling of the climate response to insolation changes, with an explicit account of radiative feedbacks.

Thank you for this historic clarification, which we have adapted into the introduction:

Milutin Milankovitch's contribution to ice age theory was to revise James Croll's original idea (1864) of changes in Earth's orbit to waning and waxing of ice sheets. Based on more accurate calculations of orbital parameters, than those available to Croll, he proposed (together with climatologist Wladimir Köppen) modelling the climate response to insolation changes being dominated by the summer melt of the ice sheets, with an explicit account of radiative feedbacks.

p. 1104 , lines 1-5 : how would this discussion accommodate the observations by Lisiecki, Nature Geosciences (2010)?

Our discussion is largely supported by the analysis by Lisiecki. However, we are a little skeptical with respect to the statistical robustness of the Lisiecki analysis, so we have chosen not to discuss this paper, as our paper is far from containing a complete review of the relevant literature.

p. 1105, l. 3 : it needs to be clarified whether the point being discussed is the forcing or the internal dynamics.

Has been added: (such as the 65N ss insolation)

p 1106, l. 24 : Admittedly, Paillard and Parrenin (2004) does a pretty good job in simulating the MPT. The model features both additive and multiplicative forcing terms, but additive forcing alone may be enough to explain the MPT. This is achieved by gradually increasing the length and amplitude of the limit cycle, causing different mode locking regimes to be scanned (Crucifix et al. 2011)

We have added a short paragraph in the section on existing models:

Yet another conceptual relaxation oscillator model **?** has been proposed, pointing to the mechanism of atmospheric CO_2 being governed by the slow ventilation of the deep ocean, which in turn is governed by the salination of the deep ocean from sea ice brine rejection and deposition of freshwater in the ice sheets. In this model the atmospheric CO_2 is governed by a threshold function of the ocean ventilation, which by introducing a slow millennial scale drift in the ventilation can reproduce the MPT as a response to the 65N summer solstice insolation.

p. 1115. l. 8 : The term 'butterfly' effect is very generic and informal. It could be confused with the more restrictive meaning of sensitive dependence to initial conditions. Here the authors describe a sensitive dependence to the parameter. This is a distinction that we are only beginning to realise in climate science and time is adequate to chose words carefully.

We agree, and have explicitly stated in the text:

constituting a critical dependence on parameters. (instead of butterfly effect)

p. 1113 and figures : IMPORTANT: all numerical values of parameters must be checked since they are generally inconsistent between text and figures or accross figures (). This has hampered verification and result replication during this review.

We have done our best to check and correct for consistency.

The source of confusion could have been that we used time units of 100kyr, and in these units the omega for the 41kyr cycles amounts to be 15.331; We have explicitly made that clear now.

Anonymous Referee #2 Received and published: 18 April 2014

The manuscript presents a new conceptual model for glacial-interglacial dynamics. The authors are discussing thoroughly the structure of their model's solutions in comparison with some known qualitative features of glacial-interglacial cycles, like the MPT. This might be of interest for specialists in dynamical systems or chaos theory, but I don't think this is relevant to climate sciences, at least it its current state. Indeed, the model's physics is not explained and appears to be very unrealistic. Furthermore, the results are quite far from what is known from paleoclimatology. I am not sure that the model can be modified in order to answer my comments below.

We agree that a simple conceptual model obviously cannot describe all the physical processes involved in glacial dynamics, it will thus necessarily appear less realistic in some aspects. However, we do not agree on the antagonism between 'specialists in dynamical systems or chaos theory' and 'climate scientists'. The paleoclimatic records

show that the climate system does behave in a non-linear way, which warrants study of the dynamical characteristics, which must be understood before any detailed climate model can be constructed. As an example, the phenomenon of stochastic resonance (SR) was first proposed as a dynamical explanation for ice ages. Even though there is very little physics involved in SR, and it is not the final theory for glacial cycles, it offers a dynamical explanation of the climatic response to a weak forcing. SR has had a profound influence on our thinking about the glacial dynamics. Our model does by no means aspire to a comparison, it is only a small step in progressing the thinking along those lines. However, we do believe that this approach is fruitful and necessary in the climate sciences. Multi-step/hierarchical approaches to solving the prevailing large-scale puzzle regarding the glacial variability of the Pleistocene epoch. Needless to say that state-of-the-art physics based earth system models are very far from being able to reproduce what is seen in the Pleistocene climate record. As mentioned before, simple conceptual models by their nature cannot not address all details. We do, however, attempt to indicate a possible physical interpretation, which Referee # 2 is strongly critical about. Parts of the critics we admit to, and have revised the manuscript accordingly, while part of the critics we will refute in the following. Before proceeding we thank the referee for a thorough and constructive review.

Main points. The authors are apparently not distinguishing "temperature" and "ice volume" in their text (see comments below) and they often refer to the results as "climate". From a physical point of view, these two variables have little in common, and in order to build a physically relevant model, it is important to decide which physics is involved. More precisely, I do not understand the background assumptions. x(t) is defined as a "global ice mass anomaly", and equation (1) says that it increases with temperature. Do the authors assume that the ice sheet is growing when it is warmer ? Or may be a minus sign is missing somewhere ? Or may be tau(t) is the ice volume (see comments below) and x(t) is something else representing the "effective climatic memory" of the system ? But which physical component could it be, if not the ice volume ?

The comment above clearly shows that we have not been able to communicate our interpretation of the model clearly enough. Hopefully the following paragraph, which we have added to the manuscript, clarifies (Substituting p 1108 line 17- p 1109 line 2):

The climate system is obviously of very high dimensionality, with dynamics governed by a multitude of processes operating on many different time-scales. The reason that a low dimensional representation can be relevant relies on the assumption that the astronomical forcing is decisive in the dynamics. The astronomical forcing is indeed of low dimensionality governed by only three parameters: precession, obliquity and eccentricity. A second motivation for the conceptual modeling approach is the observation that the (stacked) benthic foraminifera isotope record from ocean sediment cores seems to be a robust climatic signal more-or-less reproduced in all paleoclimatic proxies, indicating a synchronous global climate response to the orbital forcing. In the following we shall refer to this record simply as the climate record. As a minimal modeling approach we explore a two variable non-linear oscillator model. The first variable tau is targeting the climate record. This proxy reflects both the global ice volume, through the increase in heavy isotope water concentration in the ocean when ice masses built up, and the deep ocean temperature, through temperature dependent fractionation in foraminiferal growth. Thus tau can be thought of an external observable, as a global temperature anomaly, proportional to (minus) the global ice volume. The other variable x represents an (unobserved, internal) variable, which determines the state of the climate and thus holds the long term memory, such as total heat content in the deep ocean. This aligns with the approach of Saltzman and Maasch (1991) and that of Tziperman and Gildor(2003) arguing that besides the ice volume, also the deep sea temperature and possible other factors play a decisive role in determining the internal climate dynamics.

Later in the text, the corresponding time-scale (1/lambda) is taken to be 10 kyr. Do the authors believe that ice volume is lagging temperature by such a long time ?

Hopefully, the above clarifies that we are not proposing that the ice volume is lagging temperature. Just to clarify, in general a 1/lambda timescale (from dx/dt=lambda tau) is a response time, not a time lag (from an equation like dx(t)/dt=tau(t-1/lambda)). The typical time-scale could have it's origin in physical quantities such as the heat capacity of the deep ocean or the buildup time for glaciers from an accumulation rate of, say, 1 meter snow/year.

Actually, the standard Milankovitch theory acts mostly the other way around (the "global mean Earth temper- ature", if this means anything, is a consequence of ice volume changes, not a cause).

We agree, but this is consistent with our approach.

Things are even more strange in equation (2). Clearly, x(t) can have both positive and negative values (eg. see figures). The forcing term A(t) is always positive (eg. see equation (5)).

No, the referee must have misread eqn. (5). A(t) is more-or-less a sin-function, with both positive and negative contributions.

The multiplicative coupling term - x(t)A(t) in equation (2) is therefore sometimes negative and sometimes positive. In other words, a large insolation forcing does sometimes warm the climate (or melts the ice, or whatever the interpretation of tau) but at other times does the exact opposite (ie. cools the climate or grows the ice sheet). This point appears very crucial for the results, since it explains quite directly the locking to 2x or 3x the forcing periodicity (see eg. figure 5). But I cannot think of a (simple) physical mechanism that could act this way, with the insolation forcing having an opposite effect *on "climate", depending on the state of the system. This only looks like a mathematical trick. I personnaly believe that conceptual models are very useful, but they should be based on simple and reasonnable physical assumptions.*

Yes, that is right! And we agree, non-linear dynamics can be very contra-intuitive! In this case it is insolation forcing in conjunction with the climate state, represented by x, which can act in both directions. In terms of ice sheet dynamics this is well known: In some circumstances the mass balance is determined by the ablation, thus larger insolation, more melt and retreat of ice, in other the mass balance is determined by the accumulation, thus larger insolation more precipitation and advance of ice.

In this paper, these assumptions are not stated, and they appear quite unrealistic.

We tried our best to explicity state these assumptions in the revised manuscript and we think that our assumptions fit well into the existing line/family of conceptual modeling. As stated above, being contra-intuitive is not necessarily means unrealistic.

But more generally, I don't think the authors are raising the right questions. Please read Hays et al (1976). It has NEVER been suggested that eccentricity was "directly" driving the climatic 100-kyr cycles, but only that it was acting through the modulation of precession. This idea goes back to Croll's theory in the 19th century. This is probably THE main reason why precessional forcing is assumed to be important besides obliquity: it can easily generate 100-kyr oscillations through, for instance, signal rectification or any other simple (or less simple) non-linear process. This is strongly suggested by the phasing in the data : glacial maxima correspond systematically to eccentricity minima. This cannot be pure chance (and this also appears to be true in pre-Quaternary glaciation contexts). Prior to the discovery of these 100-kyr cycles (locked to eccentricity)

in the 1970's, it was clear (eg. see Milankovitch) that obliquity is indeed the dominant signal at high latitude. The key question behind the "100-kyr cycle" is not so much its origin, but much more its mechanisms: how can we get a phase locking of the ice volume 100-kyr cycles to eccentricity ? (again, please read Hays et al 1976). This is unlikely to be achieved by "removing" precession altogether.

Indeed, the Hays, Imbrie and Shackleton (1976) paper is one of our classics! Obviously, progress has been made since then, especially on the chronology in the paleorecords. We have expanded on the discussion of precession-eccentricity and added a reference to the Hays et al. paper:

The effect of the change in eccentricity is mainly through the coupling to the precessional cycle; when eccentricity is high the effect of precession is high, and when the eccentricity is small, the effect of precession (distance to the Sun as a function of season) vanishes. The lack of spectral power in the 100-kyr band in insolation is referred to as the 100-kyr problem of the Milankovich theory (**??**).

Detailed comments. p1103 line 6: benthic foram: "single core deviations .. can be interpreted as local climate". NO, please look at textbooks on paleoceanography. In benthic foraminifera, the isotopic signal is dominated by the (global) change in the ocean volume, linked to the size of the ice sheet. This cannot be described as a "local" climatic effect (though there is also a local temperature contribution to the signal which is often small for deep- dwelling animals). This is precisely the reason why benthic forams are used: the signal is mainly a global one.

We agree, our formulation was imprecise. We have revised accordingly:

The bottom living foraminiferal are used from the rational, that the deep ocean temperature is globally similar between locations. Single core deviations from the stack is due to bioturbation and a lesser extent local temperature variations, showing a rather poor signal-to-noise ratio for individual cores.

p1103 line 10: "strongly correlates with Antarctic ice core... which thus provides limited additional information" I STRONGLY disagree. The two records are not similar at all. They are furthermore phase-shifted during deglaciations. The 18O stack is linked to global ice volume, the Antarctic isotopes are linked to Antarctic temperatures. These two physical variables have nothing in common. The leads, lags and other differences between them carries extremely relevant dynamical information. Atmospheric CO2 is obviously also another extremely relevant variable for climate over this time period that should be used.

Again, we are sorry that we have not explained ourselves clearly enough, especially that our phrasing "limited additional information" can be misunderstood. We do not want to enter silly arguments: All we are stating is that the marine isotope record and the Antarctic isotope records to first order records the same climate signal (the curves A and E in the figure below are indeed strongly correlated). We agree, one records global ice volume, the other Antarctic temperatures. These two variables are thus -like it or not- strongly correlated. So is atmospheric CO2 (not shown below) to first order, and we do understand why. We completely agree that leads and lags carry extremely important climatic information. And we are grateful that our colleges in paleoclimatology takes on the tremendous effort it is to establish accurate chronologies, pinning tephra tie-points etc. which is necessary for establishing these leads and lags. Unfortunately, at present it is our understanding that the marine isotope chronologies on the Milankovich time-scales is only accurate to within the millennial scale, which is not accurate enough to really tie to, say, the phase of the precessional cycle. A clarification

of this point has been added.

p1104 line 1: "The 100-kyr problem". see main comments above: I believe the authors are mis-understanding the "100-kyr problem".

See above.

p1105 line 1: "it is unlikely that a single time series...". We need only three time series (eccentricity, obliquity and precessional angle) to get the full richness of the spatio-temporal astronomical forcing, ie. the insolation at different latitudes and months. This is not such a large number... Reducing these three time-series to just one (usually the summer insolation at 65 N) may possibly be a problem, but it is not such a "drastic" loss of information (idem p1110 line 15)

This has been clarified with the discussion above.

p1105 line 6: "to which extent the global stack marine isotope... is sufficient to discriminate between the suggested ... conceptual models". Indeed !! This is precisely why I strongly disagree with the over-simplification (see comments above) of looking at only one paleoclimatic data series, while thousands of them are available. When looking for "globally relevant" variables, a natural choice would be to represent of course ice volume (benthic 18O data) but also atmospheric CO2, which is available over almost the last million years. Many other dynamically relevant records are also available, for instance concerning the deep ocean state (temperature, ventilation, etc...). Again, the variations of these different variables, are most of the time, very different in terms of timing, amplitude, frequency content, etc... Looking at only one variable is indeed not sufficient. But a first step is to differentiate them (... and not mixing "temperature" and *"ice volume" into a single vague concept called "climate").*

We do agree on this "declaration of intent". Ultimately we could aim at modeling the full natural history of our planet. However, to be blunt, we think we are here facing a "clash of cultures": There is a very big gap between what a climate model is capable of doing and what qualitative interpretations can be done from proxy-records. Even if the program could be realized, it is not known to what extent the one realized climate history is reproducible, and to what extent it is one of many possible realizations of Earth's climate. This is another argument why it is relevant to explore possible dynamical mechanisms in simplified and conceptual models.

p1105 line 22: "energy balance models ... more realistically to describe the Snowball Earth" It is not clear what the authors mean by "energy balance model". If the question is about ice-sheets, obviously the model needs an ice-sheet (which is not the case in EBMs). The ice-albedo-temperature feedback involved in the Snowball or the simple EBMs mentionned is mainly the snow or the sea-ice albedo (which depends mostly on temperature). When considering ice-sheets, the situation is very different, since ice sheets do not depend solely on temperature, but also ice dynamics, continental topography, etc... It is more than likely (eg. Calov and Ganopolski., Geophys. Res. Lett., 2005) that multiple equilibria exist in the coupled atmosphere - ice sheet problem, mainly because of the albedo feedback (ie. a coupled EBM - ice sheet model, could probably be sufficient). Again glacial-interglacial cycles are about ice sheets, not about snow or sea ice, and even less about temperature (idem p1109 line 25).

We do not agree. Ice-albedo feedback is relevant for ice-sheets as well as sea ice, and the role of sea ice in glacial-interglacial cycles is not settled. (cf. the Gildor-Tziperman (200) sea-ice switch).

p1108 line 20: choice of model variables. I am surprised by the choice of "temperature" tau(t) as a model variable. This should require a bit more physical discussion of what the authors are actually meaning by this term. Atmospheric temperature has no memory (on the time-scale of interest here). Ocean temperature may have some long memory in its deeper levels, but I am not sure that this is what the authors are meaning here. Anyway, there is nothing like a paleoclimatic record of "global temperature", only local ones, which are not discussed here. The discussion afterwards in the paper is based on a comparison between this "temperature" and the record of ice volume. But the variable x(t) is presented as the one corresponding to the "global ice mass anomaly". I would therefore expect a comparison between x(t) and ice volume records. But clearly, on Figure 5, the x(t) variable does not have the same shape and does not show the "asymmetry" of tau(t), which might therefore completely change the conclusions of the paper... More probably the authors should change their physical interpretations and call tau(t) the ice volume, and x(t) something else...

This has been clarified by our definition of tau and x.

p1109 line 7: "The paleoclimatic record strongly indicates that the climate can be in one or more possible states" Well, this needs a bit of substantiation. Which records ? For Dansgaard-Oeschger events, I agree that it might be obvious. But for glacial-interglacial cycles, this does not appear so "clear" from the record of Figure 1 for instance... This assumption may indeed help to produce the "right results", but this is then a (strong) assumption of a model (which one?), not an observation from the record.

This, we thought, was evident, but it is not crucial, so we have omitted "strongly". Still we think most climatologists would subscribe to the notion that glacials and interglacials are two possible climate states, without making this a rigorous mathematical statement, we do not add a lengthy discussion here.

p1112 line 13: "of of chaos"

ok

p1114 line 1: "maximum temperature lagged 9 kyr to.." Do you mean temperature or ice volume ? The data is about ice volume (and Huybers' paper also).

This has been chained to:

As a note, we observe that the maximum for the variable τ lagged 9 kyr to the maximum amplitude of the external drive, throughout the whole Pleistocene epoch (Fig. **??**). This result is in a good agreement with the findings of Huybers (**?**).

See also comment below.

p1114 line : "rapid warnings" -> warmings ? though again, I don't think this is appropriate. The real world physics is about ice sheet growth and decay. Warmings and coolings are based on a different dynamics.

We do not understand this remark: Obviously waxing and waning of ice sheets is closely related to climatic cooling and warming.

Again, we thank the Referees' for their effort, and hope that we have answered all questions and critics in a satisfactory manner and therefore the Editor will consider it for publication in Climate of the Past.

Interactive comment on Clim. Past Discuss., 10, 1101, 2014.

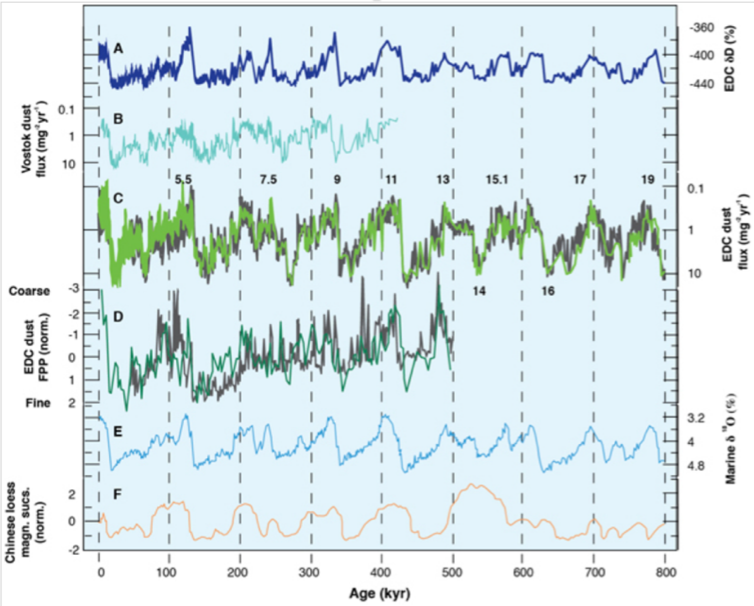

Figure 1: EPICA Dome C (EDC, Antarctica) data in comparison with other climatic indicators.
a, Stable isotope (δD) record from EDC. b, Vostok dust flux record. c, EDC dust flux records (numbers indicate Marine Isotope Stages). d, EDC dust size data expressed as fine particle percentage. e, Marine sediment $\delta^{18}O$ stack (proxy for global ice volume). f, Magnetic susceptibility stack record for Chinese loess. Peaks in most records depicted and odd MIS numbers indicate interglacial phases while troughs and even MIS numbers indicate glacial phases.

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Fig. 1.