Author response to formal review of
"A critical humidity threshold for monsoon transitions"
by J. Schewe, A. Levermann, H. Cheng

Response to Anonymous Referee #2

We wish to thank the referee very much for their help in evaluating our manuscript.

The referee suggests that the minimal model applied in our study "has two major flaws". We are convinced that this notion is due to a misunderstanding regarding the physical assumptions behind the model, and the scope of the model's applicability. We apologize for any shortcomings in the manuscript that may have led to this misunderstanding, and aim to clarify this in our response.

It is important to note that the aim of our study is to identify domains of existence for continental monsoon rainfall, in order to better understand how large-scale, abrupt monsoon transitions on paleo-timescales can come about. These transitions (as observed e.g. in the record presented in Wang et al., 2008 for the penultimate glacial) are a much larger signal than the variations in monsoon strength observed on shorter timescales; in fact, they can be interpreted as transitions between a "monsoon" and a "no monsoon" regime. Therefore, our approach is designed to capture only the most basic monsoon dynamics that define such domains of existence; it is not designed to reproduce smaller-amplitude monsoon changes on shorter timescales, for which a more detailed representation of monsoon dynamics would be required.

We apologize for not being clear enough on this in our original manuscript. We are however convinced that we can show below that the criticism is not fundamental as suggested by the referee and therefore does not hinder publication in Climate of the Past. We will be pleased to improve our explanation of the scope of the study in a revised manuscript, in order to avoid any misunderstanding about this important point in the future.

Please find below the referee's comments, quoted in *italic*, and our corresponding response.

Major concerns:
1. The authors did not derive Eq. (1) in this or the PNAS paper. It does not seem right. Where is the dry adiabatic term due to vertical temperature advection, which balances the diabatic terms to first order? By contrast, horizontal advection is secondary in the tropics. The landward flow is for 1000-850 hPa. How does it advect temperature in the entire atmospheric column?

As stated above, we believe that this is a misunderstanding. Vertical temperature transport within the troposphere is not being neglected. It transports heat from the (lower) inflow level to the (upper) outflow level and thereby maintains the column-average temperature difference ΔT (second term in eq. (1)). However, equation (1) represents a heat budget for the *entire* tropospheric column. Vertical temperature advection therefore does not enter the equation as an explicit term, because it does not directly change the average temperature within the column. Equation (1) is a closed budget, except for heat fluxes across the tropopause and to/from the land surface, which we neglect (see the discussion of the latter in the manuscript). Reanalysis data confirm that during the rainy season, the main heat sources and sinks for the tropospheric column over land are latent heating, radiation, and horizontal advection of temperature into and/or out of the column (see Fig. 2 in our manuscript).

Regarding the second question: as long as the net vertical air flux within the column is upward, there exists a height level such that the inflow of air masses into the column below that level is balanced by outflow above that level. Given a column-average temperature difference ΔT between the land region and its surroundings, temperature advection can therefore be represented by a single term $U \cdot \Delta T$, where U can be interpreted as the lower-level inflow or upper-level outflow, or simply a measure of the air mass exchange rate between the land region and the surroundings[1].

In our application of the minimal model, we make strong assumptions regarding the airflow geometry (namely, that the lower inflow of air comes mainly from the adjacent ocean region identified in the reanalysis data, and that the dividing height level is close to the level where the wind strength in the land-to-ocean direction changes sign, and remains approximately constant as horizontal wind strength varies). We are aware that these assumptions represent a major simplification of the actual physical situation, and remind the reader that this simplification is what the minimal model is all about: Reducing the complexity of monsoon dynamics to an extent that only the most basic mechanisms remain that induce the fundamental non-linearity.

We will strive to better explain the above points in a revised version of the manuscript, and we apologize for any confusion that may have arisen from the current manuscript.

[1]As requested by referee #1, we will replace the symbol ‘W’ by ‘U’ in a revised version of the manuscript.

2. Eq. (3) was never derived either. In the tropics including monsoon regions, moisture convergence, neglected in Eq. (3), dominates over moisture advection. Why was the dominant term neglected? The choice of vertical layers for qo (1000-600 hPa) and qL (1000-400 hPa) is mutually inconsistent. Why is the moist layer set thinner over ocean than over land? Is it to ensure qo-qL > 0?

The simplest concept of a monsoon circulation that is the core of our minimal model is that of an overturning circulation where moist air enters the land monsoon region at the lower level, converges and, after giving up moisture to precipitation, exits the region at the upper level, all with the same rate U (as sketched in our Fig. 1). In this picture, convergence is not neglected, but rather full convergence is assumed (only that there is no explicit convergence term because the equation is again a budget for the entire tropospheric column). Here, q_O would represent humidity over the ocean at the lower (inflow) level, and q_L would represent humidity over land at the *upper* (outflow) level.

In reality, there will not be full convergence, and some of the outflow will happen at the lower level. There will be a *mean* cross-column humidity q_L such that the total outflow of moisture is $U \cdot q_L$. That is why we consider humidity over land across a wider vertical range than humidity over ocean.

Equation (4) then states that the same q_L is also a measure of the precipitation rate P. Reanalysis data confirm this assumption to first order. In fact the correlation of P and q_L is relatively robust with respect to the exact vertical levels of q_L, which corresponds to the notion of a tropospheric column that is well mixed due to intense convection.

Again we apologize for any confusion that may have been caused by insufficient discussion of these points in the current manuscript, and offer to improve this in a revised manuscript.

Other comments
1. The authors appear unfamiliar with recent studies of South Asian monsoon dynamics. Some examples are Bordoni & Schneider (2008, Nature Geosci.), Prive & Plumb (2007, JC), Chou, Neelin & Su (2001, QJ).

We thank the referee for mentioning these interesting and important studies. We do not see any conflict between our results and these studies. If the referee disagrees, we would be grateful for further elaboration on the problem.

2. The East Asian monsoon is more complicated than the South Asian monsoon, nonlocal and affected by the westerly wind jet. The authors' approach needs substantial change for East Asia. See Sampe & Xie (2010, JC).

We agree with the referee that the East Asian monsoon has particularly complex dynamics, not least because it is located relatively far from the equator. We are however convinced that the moisture-advection feedback plays a fundamental role also in the EASM. Sampe and Xie (2010), while proposing a model that explains characteristics like location and shape of the EASM rain band by considering the westerly jet stream, highlight the importance of the low-level southerly inflow of moist air. Again, we do not see any conflict here. We offer a simple mechanism that could explain the abrupt transition behaviour found in paleo records. Our approach is not designed to explain detailed characteristics of individual monsoon systems; but we also believe that it is not inconsistent with those characteristics.

3. Line 26, page 1748. The notion of wind speed increase under global warming has been recently challenged (Wentz & Ricciardulli 2011, DOI:10.1126/science.1210317)

Young et al. have responded to the comment of Wentz & Ricciardulli on their paper (see Young et al. 2011, *DOI: 10.1126/science.1210548*). In essence, the debate is about the magnitude of the trend that has been observed so far (trends reported in different studies differ roughly by a factor of 2), not about the sign.

However, this reference is not essential for our results, and we are ready to remove it if the editor considers it advisable.

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