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Interactive comment

# *Interactive comment on* "Nonlinear response of tropical lower stratospheric temperature and water vapor to ENSO" *by* Chaim I. Garfinkel et al.

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Garfinkel et al. study the effect of ENSO on temperature around the tropical tropopause, and related to that on water entering the stratosphere. They study the problem using observations of stratospheric water vapor (the SWOOSH data set), temperatures from the MERRA reanalysis, and climate model simulations in a range of configurations. The paper's main point is that the response in tropopause temperature and water entering the stratosphere is non-linearly related to ENSO (as represented by some index) - such that both strong El Nino and La Nina lead to a temperature increase (and correspondingly moistening of the stratosphere). Using this result, they argue that the sequence of strong El Nino followed by strong La Ninas in the late 1990's led to elevated temperatures (and moister air) for a few years, which contributes to the



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Discussion paper

’drop’ observed around September/October 2000. The hypothesis put forward is very interesting - but I have a number of concerns.

I could not help the impression that this paper was written up slightly careless. At times, the text reads more like a story than a scientific paper; similarly, the paper has problems finding the right tone. Consider the abstract. There, it is first written that: "The impact ... is nonlinear." which leaves no room for doubt. However, the next sentence does not provide the hard evidence expected by the reader, but uses the rather weak word "appear" twice. Also, considering the seemingly straightforward hypothesis - the non-linearity of ENSO - I expected to be shown a plot that shows the non-linearity beyond doubt. Instead, the paper presents a full 16 figures that show a lot of information - most of it only qualitatively discussed. The paper would be much stronger if the authors were able to support the main point of their paper in one or two clearly drafted figures. Figure 1 presumably presents the model data that best supports the argument for non-lineariy - however the points are so small, and cyan has very little contrast, such that it is easy to overlook the datapoints supporting the hypothesis. Simply tweaking colors and symbol size would probably help. Also, I’d like to see a more quantitative treatment of the non-linearity (i.e. it would be simply to compare the linear regression with a non-linear regression). Also, it would be fair to show the statistical uncertainty in the "observational" data shown in Figure 3; we should be honest that the observational timeseries is really (too?) short to make statistically robust statements.

The paper then applies the argument of non-linearity to explain the sudden and persistent drop of stratospheric water vapor around October 2000. However, there is a major conundrum pointed out in Fueglistaler et al. (J. Geophys Res., 2013) that needs to be addressed here: The arguably best representation of true temperatures in reanalysis data fails to properly produce a drop as observed in HALOE data. That is, the mechanism discussed in this paper applies to the large-scale effect of temperature and circulation, but the problem is that even if the free-running GCM would recover the reanalysis temperature perfectly, it would not be able to produce a drop as prominent as observed by HALOE. Correspondingly, it is not surprising that the drop diagnosed by the authors is only 23% of the HALOE drop. While there is plenty of good reason to have trust in HALOE data, it is crucial to note here that the stratospheric water vapor time series as observed by SAGEII agrees very well with the reanalysis-based model estimates (Fueglistaler et al., 2013). This needs to be discussed; and I would strongly encourage you to also consider quantifying the importance not against HALOE, but against the AMIP-mode model generated data (this helps your paper). However, the analysis of the drop as presented in Figure 11 is close to cherry-picking: anyone can see that what is labelled here as "decadal drop" is anything but a decadal drop. I also suspect that this time series does not compare favourably against SWOOSH at all - should this not be reason for serious concern given that this is an AMIP run?

To summarize, the paper by Garfinkel et al. touches many interesting points, and makes use of interesting numerical model runs. The paper needs, however, a major overhaul; the main points need to be worked out clearer in the data, and the discussion of the "drop" needs to be more careful. Given the recommendation for major revisions, I do not go further into the details of the current manuscript.

Signed review, S. Fueglistaler

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