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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Solar cycle in current reanalyses: (non)linear attribution study" *by* A. Kuchar et al.

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We would like to thank the reviewer for his/her comments.

Author's response and changes in the manuscript are included below. In addition, you can find the revised manuscript as supplement where the particular changes are highlighted together with the comment referring to them.

Specific comments

1. The title does not really make sense. Consider changing to "The solar cycle in current reanalyses: linear verses non-linear attribution approaches" or something similar.

The title was change to "The 11-year solar cycle in current reanalyses: A (non)linear at-

tribution study of the middle atmosphere" based on the technical comment 3 of referee #4.

2. L41: insert 'most of' between 'where' and 'the ozone'.

"most of " was inserted between 'where' and 'the ozone'.

3. L66 missing citation, perhaps include Austin et al, 2008 here.

It was already corrected.

4. L72 include Matthes et al, 2004 and Matthes et al, 2010 here.

These citations were included.

5. L72-84 some discussion of the Kren et al, 2014 paper should be made here, as there look at this relationship in models and conclude that it could be chance.

The Kren et al, 2014 paper is discussed in that place. See in the text or sentence below.

However, fully coupled WACCM-4 model simulations by Kren et al, 2014 raised the possibility of occurrence of the observed solar-QBO response in the polar region.

6. L97 Next to (or instead of) Gray et al, please include Kuroda and Kodera, 2001.

Kuroda and Kodera, 2001 was included instead of Gray et al., 2010.

7. L108-109 Please include Gerber et al, 2009 in this list. Also, Mitchell et al, 2013 should be cited in place of (or as well as) the Baldwin and Dunkerton paper, as this was an update that dealt with timescales explicitly.

The paragraph included these citations were deleted according to the specific comment 2 of referee #4.

8. L122-135. Here reference should be made to the recent Chiodo papers (Chiodo et al, 2012; 2014).

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Chiodo et al., 2012 was included instead of Gray et al., 2010. Chiodo et al., 2014 was then discussed several times.

9. L129: Include Scaife et al, 2013 here.

Scaife et al, 2013 was included in that place.

10. L136-152 I think more needs to be made of the different types of MLR (see overall comment 1).

The paragraph related to the different types of MLR in terms of NAO and QBO3 terms was made in section Methodology. See paragraph and documenting figures 1,2,3 and 4.

The robustness of solar regression coefficient has been tested in terms of including or excluding particular regressors in the regression model, e.g. NAO term was removed from the model and resulting solar regression coefficient was compared with the solar regression coefficient from original regression model. The solar regression coefficient seems to be highly robust since either the amplitude or statistical significance was not changed when NAO or QBO3 or all of them were removed. However, cross-correlation analysis reveals that the correlation between NAO and TREND, SOLAR and SAOD regressors is statistically significant.

11. L175 My overall comment 1 is linked the Fujiwara et al, 2012.

The accordance and difference between the present study and Mitchell et al (2014) was discussed in section 1 and also mentioned further within the text. See added paragraph below.

Under this framework the paper by Mitchell et al. (2014a) has been published where 9 reanalysis datasets were examined in terms of 11-year SC, volcanic, ENSO and QBO variability. Complementing their study, we provide comparison with nonlinear regression techniques here, assessing robustness of the results obtained by Multiple Linear Regression 10 (MLR). Furthermore, EP-flux diagnostics are used to examine

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solar-induced response during winter season in both hemispheres, and solar-related variations of assimilated ozone are investigated.

12. L178 (and elsewhere) Consider changing 'on the last generation' to 'to the most recent generation at the time of writing'

" last generation" was only changed to "the most recent generation" within the text since it is kind of obvious that " the most recent" is related to the time of writing.

13. L202-215 See my overall point 2 here. But you should discuss papers that have calculated TEM diagnostics in reanalysis, as well as explaining any issues you may have had in doing so. For instance, Seviour et al, 2013 show how to do it for ERA-I, as well as issues that are faced. Mitchell et al, 2014b do the same for MERRA.

According to this specific comment and overall point 2 we have added these two paragraphs. Although the Eliassen–Palm (EP) flux diagnostics (described below) was computed on a 3-hourly basis the regression results have not changed significantly.

The Eliassen–Palm (EP) flux diagnostics (described below) was computed on a 3-hourly basis from MERRA reanalysis and subsequently monthly means of EP flux diagnostic variables were produced. Similar approach has been already used by Seviour et al, 2013 and Mitchell et al, 2014b. The former study proposed that even 6-hourly data are not only necessary but should also be sufficient to diagnose tropical upwelling in the lower stratosphere.

For this purpose the quasi-geostrophic approximation of Transformed Eulerian Mean (TEM) equations were used in the form employed by Edmon Jr et al. (1980), i.e. using their formula (3.1) for EP flux vectors, (3.2) for EP flux divergence and (3.4) for residual circulation. These variables were then interpolated to a regular vertical grid. For the visualization purposes the EP flux arrows were also scaled via the formula (3.13) in (Edmon Jr et al., 1980). The script was publicly released (Kuchar, 2015).

14. L232-235 This is not very clear until later in the analysis section. Rewrite so it is

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clear right away.

This part was rewritten to be clearer.

15. L296-299 Many studies just use AR1 for this, do the authors think that AR2 is better for some reason? Could they explain this.

Although the solar regression coefficient seems to be unchanged when using AR1 instead of AR2 we concluded the AR2 is more convenient because the autocorrelation of residuals is better removed according to the Durbin-Watson is mostly equal to 2 (i.e. indiciates no autocorrelation) throughout the whole zonal mean. See figures below documenting the D-W test based on the regression model with AR2 (Fig. 5) and AR1 (Fig. 6).

This part "{we have used an iterative algorithm to model the residuals as a second-order autoregressive process. The Durbin-Watson statistic has been used to detect the autocorrelation of the error terms from the regression model." was replaced by "we have used an iterative algorithm to model the residuals as a second-order autoregressive process (AR2). Durbin-Watson test confirmed that this setup was sufficient to model most of the residual autocorrelations in the data."

16. Section 4.1 see overall comment 1.

The results from the Mitchell et al. 2014 paper were discussed in section 4.1.1.

17. L410-415: Expand on what is meant by ‘using the model with EESC. . .’ (see comment 14. Also, use ‘regression model’ rather than ‘model’ otherwise readers may get confused with GCM models.

Those sentences were clarified in the way suggested above.

18. L476-578: I do not really follow this argument, and it is rather important because all your figures use it. Could you expand on this. Also, explain clearly how columns 2-4 should be interpreted from now on (e.g. on figures 1-3).

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This part was revised according to this comment.

19. Figure 3: I do not see the O3 response.

The structure of figures was revised – regression coefficient results through reanalyses are situated in one figure and relative impact results are situated in one figure for each reanalysis.

20. L525 I think the PJO needs to be discussed here. Please also cite the Kuroda and Kodera, 2001 paper.

The Kuroda and Kodera, 2001 paper was cited in this place.

21. L556-562 Again, not the PJO here.

The Kuroda and Kodera, 2001 paper was added here.

22. L628 This paragraph is a little confusing. What the authors say is only true in November, not really true for all of early winter. For instance, in December the vortex is weaker, and more easterly (between 80-90N).

“early Northern hemispheric (NH) winter (including November)” was replaced by “November”

23. L669 – Insert ‘probably’ between ‘This’ and ‘results’.

“probably” was inserted between “This” and “results”.

24. L752 Missing reference, I would add Austin et al, 2008 here.

This reference was already completed in the ACP discussion paper.

25. L788-802 To me this is the key conclusion of the paper. I think it should be right at the front of the conclusions.

This paragraph was moved at the front of the conclusions, i.e. on the second position.

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C12777/2015/acpd-14-C12777-2015-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 30879, 2014.

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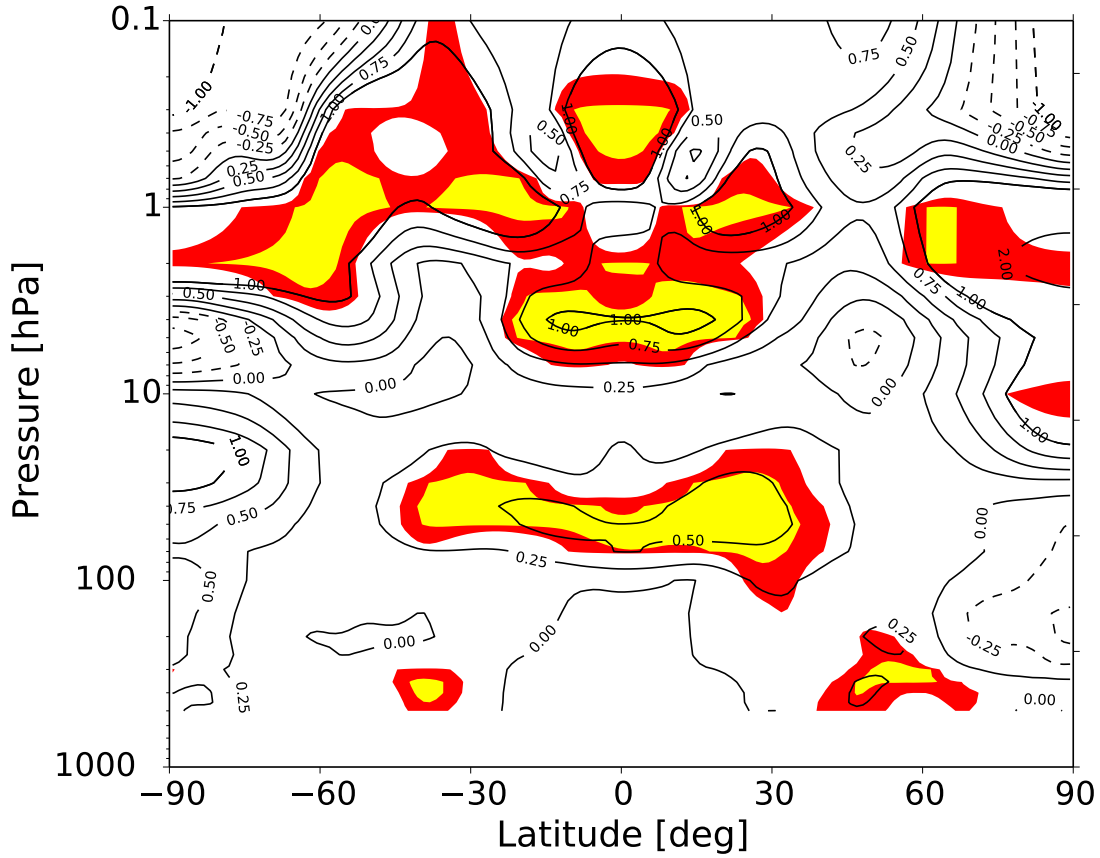

Fig. 1. Temperature response to SC for MERRA. All regressors included.

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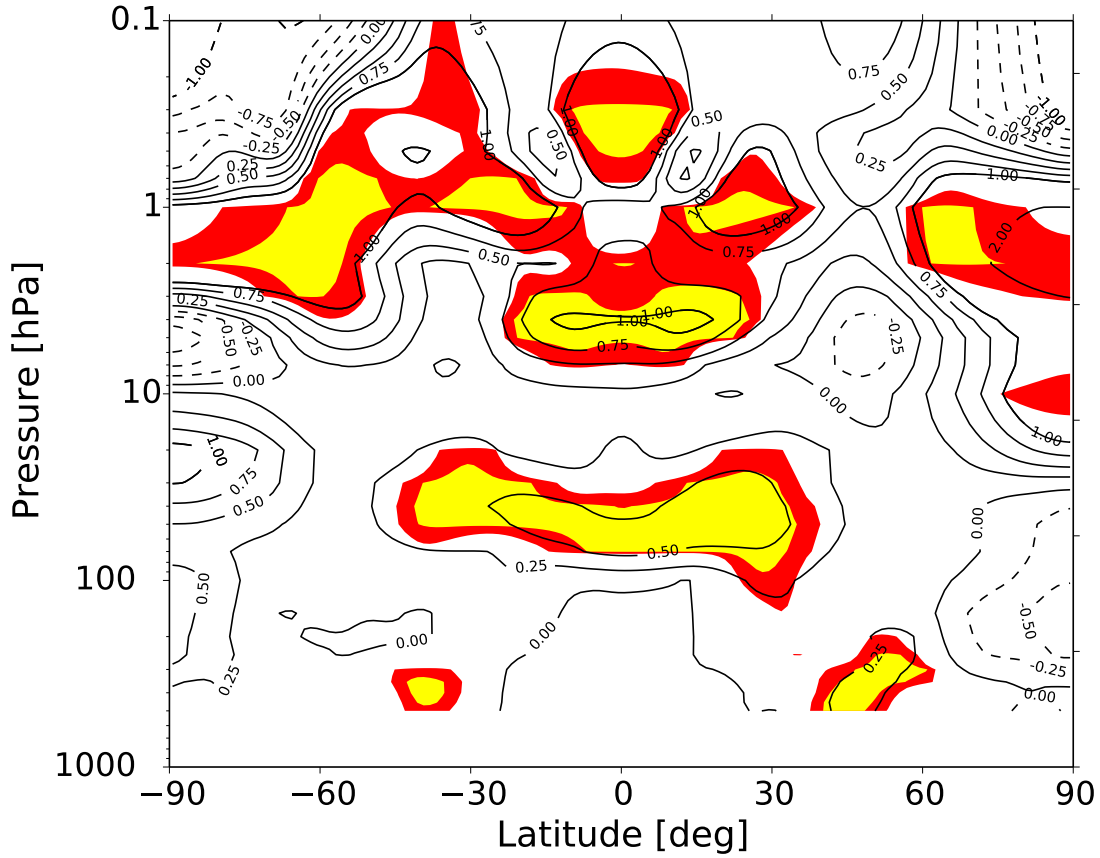

Fig. 2. Temperature response to SC for MERRA. All regressors included except NAO.

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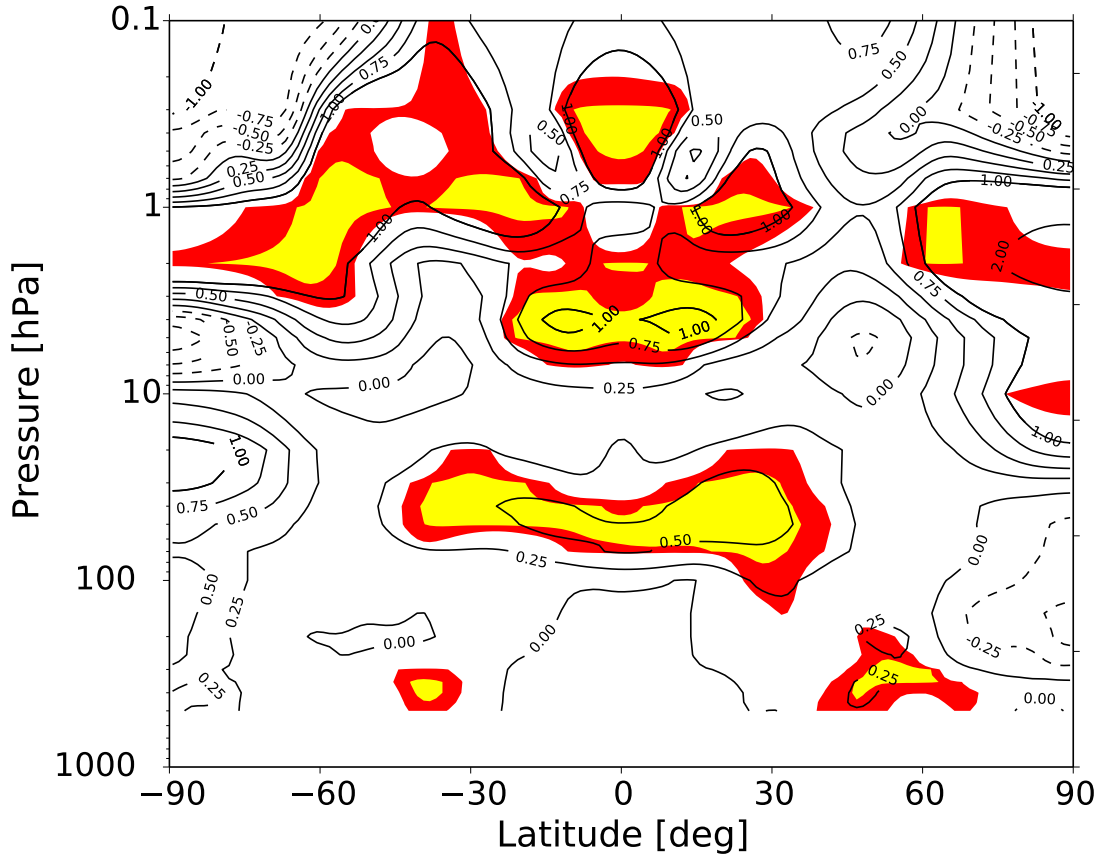

Fig. 3. Temperature response to SC for MERRA. All regressors included except QBO3.

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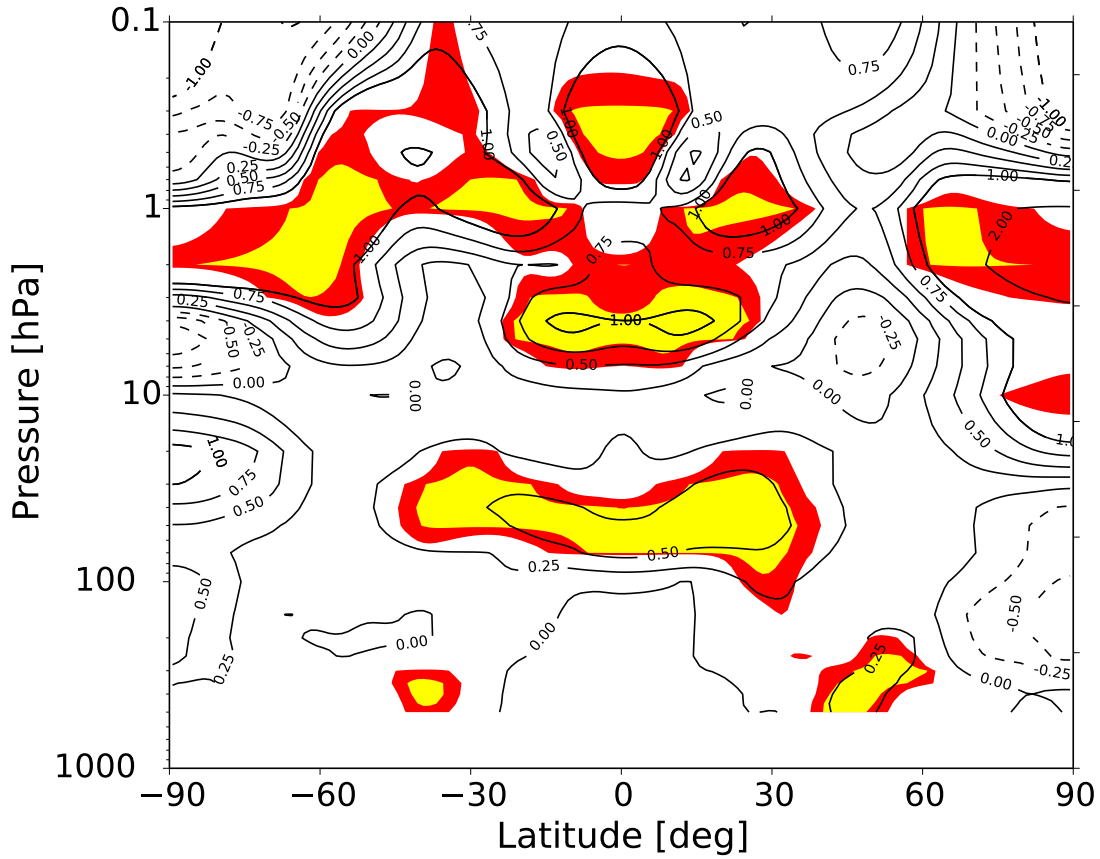

Fig. 4. Temperature response to SC for MERRA. All regressors included except NAO and QBO3.

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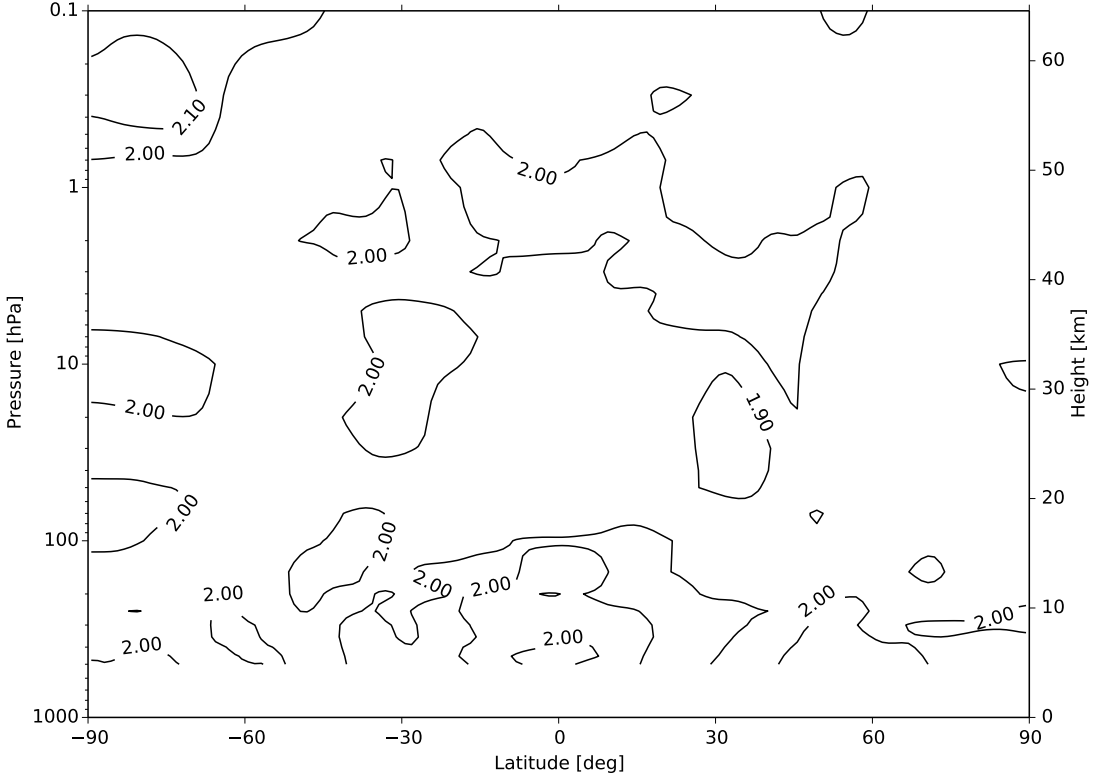

Fig. 5. D-W test of temperature regression coefficient for MERRA. AR2

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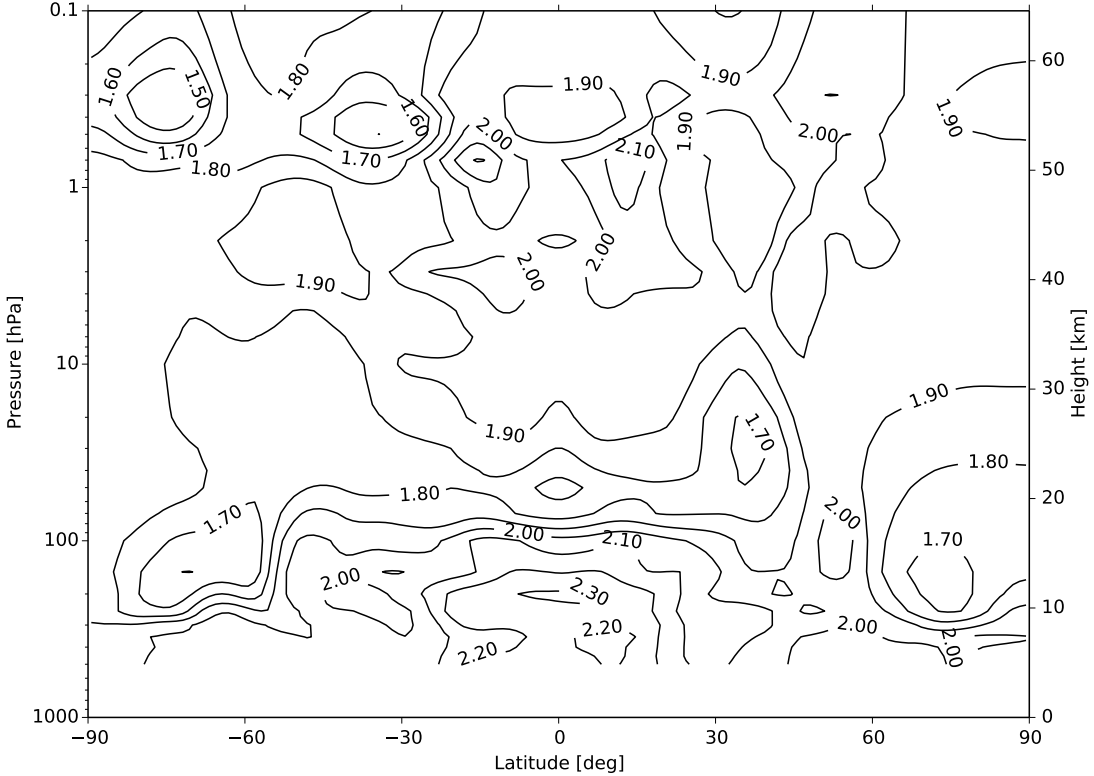

Fig. 6. D-W test of temperature regression coefficient for MERRA. AR1

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