Atmos. Chem. Phys. Discuss., 7, S5–S8, 2007
www.atmos-chem-phys-discuss.net/7/S5/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Towards a better representation of the solar cycle in general circulation models" *by* K. M. Nissen et al.

Anonymous Referee #2

Received and published: 17 January 2007

GENERAL COMMENTS

This paper could become a very useful contribution to the literature. However, in its current form I believe it to be somewhat misleading. It could lead to incorrect conclusions concerning the necessary spectral resolution that is required for modelling of the shortwave response to solar cycle variation.

The basic problem in the paper is that it compares the solar cycle response in a radiation scheme which has only one spectral interval (throughout the UV and visible) with results from a scheme with 49 spectral intervals. The reader is left to conclude that, because the 1 spectral interval scheme fails, it is therefore necessary to have 49 spectral intervals! I do not think this conclusion can be sustained. The Fouquart and Bonnel

(FB) scheme is UNUSUALLY crude in not even splitting the UV and visible - even very early GCM schemes such as the classic Lacis and Hansen (JAS, 1974) scheme did a more detailed job, and I suspect that it would do a much better job of modelling the response to solar cycle variability.

I think the authors need to be much clearer that (a) they have established that this unusually low spectral resolution model cannot (directly) be used for solar cycle studies but (b) it may be that other low spectral resolution schemes, which for example have one or two bands in the UV, may have adequate resolution.

Conclusion (a) is valuable in itself, given that the FB scheme is in quite widespread use, but extrapolating the conclusion to all low resolution schemes is unjustified. Ideally, it would be good if the authors could compare with other schemes, but I realise that this is a new research project in its own right.

Although I recommend major revisions, in a sense the paper could be fixed by quite simple but clear rewording, which makes clear the limitation of what they have shown.

It was very good that the authors examined the abilities of the FB scheme to model solar absorption in the absence of the solar cycle, and showed that it behaves well - this is a fair and useful result.

SPECIFIC COMMENTS

46-5: “4 bands” - this is a little misleading, as FB only have one band in the spectral region of interest.

46-11: “is dominant” - of course, Shibata and Kodera used modelled ozone responses to solar cycle variation, so it is safer to say the “S&K found the UV forcing to be dominant”.

48-12: “sufficient” - the use of this word is at the core of the problem I have with this paper. The paper does NOT examine what resolution is sufficient. It “merely” shows that a particular one-band scheme performs badly compared to a 49 band scheme.

ACPD
7, S5–S8, 2007

EGU

It has not established whether some resolution intermediate between 1 and 49 would be adequate - I bet it is, and I would bet a quite low number (2 or 3) would be quite adequate!

50-15: “line-by-line mode” - I am not quite sure what this means at these wavelengths - most codes use the absorption cross-sections in this spectral region, and do not resolve individual spectral lines, in many cases because the absorption is a true continua

50-25: “Herzberg” - some care needs to be taken in nomenclature, and indeed this is a problem with the earlier literature. On Figure 4, one curve is labelled “Herzberg”, but there is considerable absorption by ozone in this region from the tail of the Hartley band - this absorption is probably responsible for much of the “Herzberg” signal seen in Figure 4. Perhaps label it “Hartley/Herzberg” on the figure for clarity?

51-5: I’m sorry if I missed it, but the paper doesn’t appear to say what extraterrestrial irradiance is used at wavelengths other than the Lyman-alpha

51-6: What sources of scattering are there in the calculations - is it just Rayleigh scattering?

52-15: One thing that is striking about Figure 2 is that the FB model, below about 70 km, does quite a good job of simulating the shape of the heating rate perturbation. So multiplying the field by 20 could be a “poor man’s” way of including solar cycle variability using FB. The core of the problem with FB is that all the solar cycle irradiance variability has been smeared out to 680 nm, and there is little absorption by ozone across a lot of this range. If instead the solar cycle variability applied to the model was weighted by ozone absorption cross section (integral of absorption-cross-section multiplied by the percentage solar cycle variability) perhaps a more rationale solar cycle variability “fudge” could be applied to the model to get a better response. The paper does not make clear the physical source of the problem with FB, and if it did so, it would raise the question of whether even one or two extra bands in the UV could lead to a much better representation. The authors should mention this.

ACPD

7, S5–S8, 2007

53-21: I think it should be emphasized that these are (I think) fixed (sea?) surface temperature experiments.

54-19: I am not convinced that this feature in the lower stratosphere is a real physical feature - note particularly the opposite sign of response in the two model versions. 95% confidence level means that you would expect 5% of the domain to have significance by chance! Is there really a significant perturbation in the heating rates at these levels?

55-18: This sentence “a high resolution scheme is needed” is not justified by the material in the paper. All the authors can safely conclude is that the single-band FB scheme is not adequate.

55-19: Again ... “adequately” - the paper has not been able to address what is adequate.

56-1: “Lyman-alpha ... on climate” - I guess it depends on what you mean by climate. I think you have clearly illustrated that Lyman-alpha is vital for modelling processes in the upper mesosphere, but you have not yet demonstrated that it has an influence beyond this.

TECHNICAL COMMENTS 47-9: “solar forcing” of what?

54-15: I didn’t know what is meant by “old” here.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 45, 2007.