

Interactive comment on “Mean winds, SAO and QBO in the stratosphere, mesosphere and lower thermosphere over Ascension Island (8deg; S 14deg; W)” by K. A. Day and N. J. Mitchell

Anonymous Referee #1

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"Mean winds, temperatures and the SAO in the mesosphere and lower thermosphere over Ascension Island (8S, 14W)"

by K.A. Day and N.J. Mitchell

This is a revised version of the original paper, which was not acceptable for publication because of sloppy and confusing writing, and because it included much material that was irrelevant to the main findings. Those findings, which are interesting and could be suitable for eventual publication, are (1) that the radar observations made by the authors at Ascension Island show the presence of a strong semiannual oscillation (SAO), and (2) that the westward phase of this SAO appears to be modulated at certain times

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by the stratospheric quasi-biennial oscillation (QBO).

The present revision does eliminate all of the irrelevant material present in the original, which obscured the main story. However, it most definitely does not remove the very sloppy writing, which remains rife in the present version. Specific comments are offered below. Many of the comments pertain to the clarity of the presentation rather than to technical issues, but they must be addressed before this paper is considered for publication. It is up to the authors to do this conscientiously and thoroughly themselves; serving as a proofreader is not a good use of a reviewer's time.

Specific Comments (page number, line):

(6780,18) "the HWM-07 model" You need a reference here—you cannot assume that readers will universally know what this model is. Further, you might want to add the qualifier "empirical" before "model".

(6781,8) "Mesospheric quasi-biennial oscillation" Is there such a thing? What is discussed in the present paper is an apparent modulation of the amplitude of the mesospheric SAO by the stratospheric QBO, which is not the same as a QBO in the mesosphere. If the authors believe there is a separate mesospheric QBO, they should provide references. But note that, in any case, the existence of a mesospheric QBO is not particularly germane to the main results presented in this paper.

(6781,24) "Baldwin et al (2001)" The title of this paper is wrong as given in the references.

(6782,2) "QBO signature appears in the mesosphere" Please give a reference.

(6782,12) "it has been suggested that the first westward phase" Who has suggested this? Why only the first westward phase?

(6782,17) "investigated the correlation" Garcia and Sassi (1999) did not look at correlations, as theirs was not a statistical study. They instead proposed a physical mechanism to explain the apparent relationship between the stratospheric QBO and the

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amplitude of the mesospheric SAO. Note also that they did not suggest that the mechanism applied only to the first westward phase.

(6783,3) "the HWM-07 model" Here, in addition to adding a reference, you need to describe in a sentence or two what this model is.

(6784,12) "the MQBO" I am having trouble seeing much power at 28 months (~ 852 days) in either zonal or meridional winds, so I am not sure what it is you are talking about here. Note that the figure lacks any indication of the statistical significance of any of the spectral peaks with respect to, for example, a background red spectrum. Annual and semiannual peaks are obvious enough, so perhaps statistical testing is not needed in that case, but any claims about a mesospheric QBO are tenuous without proper statistical evaluation.

(6784,14) "appears as a broadening" OK, but you need to make a better case that this is what the spectrum shows. I presume you are referring to the peaks on either side of the semiannual peak seen in the U spectrum. If these side lobes are indeed an indication of quasi-biennial modulation of the SAO, then it must be true that $1/\omega_1 - 1/\omega_2 \sim 1/\omega_{qb}$, where ω_1 and ω_2 are the peaks on either side of the SAO peak, and ω_{qb} is the quasi-biennial frequency. The reader cannot check whether this is the case because the abscissa of Fig. 2 cannot be read precisely enough.

(6785,3) "Here we observe..." This paragraph is a mess and must be thoroughly rewritten such that it is comprehensible. Please avoid confusing terms like "southern hemisphere summer" and stick to months of the year. Note that "Southern hemispheric summer-time flow" is both confusing and wrong: you do not know what the flow is in the southern hemisphere—only at Ascension Island, which is really a deep Tropics location that happens to be a few degrees south of the equator.

(6783,23) "late autumn and early winter" Please get rid of these confusing terms: use instead the month(s) of the year when the phenomena you want to highlight occur.

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Ditto at the end of this paragraph (line 28).

(6786,3) "southern hemispheric winter months" Please rewrite as "May-August" This is both more precise and easier on the ear.

(6786,15) "considered outliers" As I remarked in the review of the original paper, no good reasons are offered for considering the winds in 2002 as "outliers", which seems to imply that the observations for this year are somehow suspect.

(6786,17) "changed the structure of the wave" What wave?

(6787,10) "northward southern hemispheric summer-time winds ... maximize in December-January" A lot of unnecessary verbiage here. How about "northward winds ... maximize in December-January"?

(6787,12) "our observations" I think "our observational composite" (Fig. 3, lower right-hand panel) would be more accurate, as individual years differ from this composite behavior.

(6787,27) "the southern hemisphere winter time" More confusion. "May-August" says is more precisely and clearly.

(6787,9) "in its first westward phase" Why only the first westward phase?

(6787,25) "the suggestion by Baldwin et al (2001) (as described above)" What suggestion? Nothing of the sort is "described above".

(6789,1) "Figure 7" The caption of this figure refers to a double-headed arrow that "shows the small difference between the mesospheric and stratospheric winds", but the difference indicated appears to be between maximum eastward and maximum westward winds in the stratosphere.

(6789,18) Fig. 8 shows the relationship" In fact, this figure appears to show no relationship at all! But the next figure (Fig. 9) is more suggestive of a relationship.

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(6789,25) "the difference" Is it the difference between eastward and westward winds that matters? Or just the presence of weak westward winds? (which would allow all but the slowest waves with westward phase velocities to propagate upward without encountering critical levels).

(6790,1) "Figure 9..." The data for years before 2001 are not part of the authors' Ascension Island observations. Please tell the reader where these data came from!. And give some motivation why you are examining this longer dataset: As far as I can see, the point is that years when the eastward winds of the stratospheric QBO are weak and coincide with one of the westward phases of the mesospheric SAO are not very common. But when such a coincidence occurs (1993, 1995, 2002), then the westward phase of the mesospheric SAO is very strong.

(6790,8) "Figure 10..." The legend on the right hand side panel states that "Strong westward winds propagate" I have no idea what this means. Perhaps you meant to say that waves with $c_x < -5$ m/s can propagate vertically without encountering critical levels? Similarly, the left hand panel of the figure reads "westward winds absorbed"-- this does not make any sense.

(6790,10) "Considering all the years..." This statement is logically part of the first paragraph of page 6790, which discusses Fig. 9, and should not appear after you discuss Fig. 10!

(6791,5) "they reported a clear SAO..." This sentence is incomprehensible. Please fix it.

(6791,14) "stronger ... weaker..." stronger and weaker than what?

(6791,21) "We observed ... the MSAO to be modulated..." I think this is too strong, as clearcut cases of very strong westward phases of the mesospheric SAO are few and far between. The most that can be said, in my opinion, is that Figure 9 suggests that very strong MSAO westward phases appear to occur at times when westward winds in

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the stratosphere are especially weak.

(6792,11) "Garcia and Sassi (1999) suggested..." I found this paragraph unintelligible. Please rewrite so that readers do not have to spend inordinate amounts of time trying to decode your meaning.

(6792,18) "In contrast" Why is this "in contrast"? As far as I can tell, both Antonita et al. and Garcia and Sassi propose that the state of the stratospheric QBO conditions the eastward spectrum of waves that propagate into the mesosphere, which then alters the forcing of the westward phase of the MSAO. A et al. did this with (I believe) mesoscale gravity waves, while G and S proposed that medium-scale inertia-gravity waves are involved.

(6793,1) "They found..." I could not make any sense of this. Please rewrite.

(6793,17) "during the first westward phase when the SQBO winds ... met the criteria and ... were not filtered" Why only the first westward phase? And what criteria are you referring to? And what was it that was not filtered? Please rewrite; you can do better than this!

(6793,18) "allows winds to propagate" This makes no sense. Please think it through and rewrite.

(6793,20) "observed by Garcia et al (1997)" These authors did not "observe" anything; they showed and discussed observations made by others.

(6793,22) "in the first phase" It remains unclear to me why this mechanism is supposed to work only in the first phase of the MSAO. Please explain, or remove.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 13, 6779, 2013.

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