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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assimilation of stratospheric and mesospheric temperatures from MLS and SABER into a global NWP model" *by* K. W. Hoppel et al.

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First, we thank Dr. Manney for her generous and constructive review comments. She is correct in speculating that we do not have a analogous simulation without MLS and SABER data assimilated and, for a number of practical reasons, it is infeasible to generate one now. We do have operational analysis fields from NOGAPS, but these are generated with a low-altitude system extending to 1 hPa only, with the 20-1 hPa region heavily diffused. Thus perhaps the nearest analogue we have are the NOGAPS-ALPHA forecast runs of Siskind et al. (2007), who performed forecast simulations initialized with a combination of NOGAPS/GMAO analysis extending to ˜0.4 hPa (relaxed to climatology above that level). Those forecasts were able to reproduce the disturbed elevated stratopause of 2006 and more normal stratopause of 2005 through upward propagation of information from the initial fields via directly simulated planetary wave and parameterized orographic gravity wave propagation and dissipation. Some perspectives on this come from Point #5 below, in which free running models without MLS/SABER data predict a deep "generic"; mesospheric cooling response. In fact observations here and earlier, discussed in point #5 below, show a very different temperature response. This alone seems to argue for assimilations containing MLS and SABER being superior to state-of-the-art model simulations which yield a different kind of dynamical temperature response at present. In other words, model responses at these altitudes are generally biased with respect to the observations. We will try to bring out these perspectives a little more in revision. Longer term, we hope to conduct systematic data denial experiments of the type envisaged in the comment.

1. We modified "SABER" to read "SABER and MLS"

2. A citation for the GEOS-4 analysis (Bloom et al, 2005) has been added to the introduction in addition to the ERA-40 citation.

3. The lower limit of 32 hPa was chosen for MLS and SABER so that the tropospheric and lowermost stratospheric analysis would be close to the operational analysis, with minimal overlap between the AMSU-A radiances and limb profiles. The focus of this study is on the extension of data insertion into the upper stratospheric and mesospheric rather than the optimal combination of limb profiles and radiance data in the lower stratosphere.

4. Changed as suggested.

5. The Ren et al. (2008) paper was not cited because it had not appeared in print when we wrote this paper. It has been added to the citations in the introduction list of CMAM studies which incorporate a full mesosphere, but only assimilate observations in the stratosphere and below. While certainly a very interesting GCM/DA study of the remarkable 2002 Southern Hemisphere major warming, their results require some discussion in the context of some earlier observational and modeling work we have

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done. Ren et al. (2008) argue that their modeled deep mesospheric cooling response is both "predictable" and "generic." They reach this conclusion not by comparing with independent mesospheric temperature measurements, but by comparing with previous modeling studies that have also suggested a deep mesospheric cooling response to stratospheric warmings (Holton, 1983; Liu and Roble, 2002). It is difficult to reconcile generic deep MLT cooling with the SABER and MLS observations reported here, which show the polar winter stratopause essentially reforming at higher altitudes in response to the 2006 warming, which seems to imply a cooling response only in the lower mesosphere followed by more of a warming response in the mid-upper mesosphere. A few years back we performed our own detailed forecasting simulations of the 2002 SH warming using NOGAPS-ALPHA (Allen et al., Mon. Wea. Rev., 2006). Those runs yielded mesospheric forecasts constrained by tropospheric and stratospheric analysis fields, somewhat similar to the Ren et al. (2008) simulations. However, we chose not to analyze those mesospheric model fields because we had no temperature data for objective validation of the forecast quality since SABER's yaw cycle had it viewing high northern latitudes at the time. Instead we first studied SABER temperatures for three other warmings when SABER was yawed to observe these events (Siskind et al., Geophys. Res. Lett. GRL, 2005). The combined SABER temperature anomalies showed a much shallower mesospheric cooling response to the warming that was confined to ˜0.7-0.01 hPa, followed by a transition to a null response or weak shallow upper mesospheric or lower thermospheric warming. These observed responses appeared to explain the null effects in OH airglow temperatures reported by ground observers during previous stratospheric warmings. See Siskind et al. (2005) for further details. We then modeled one of these SABER-observed warming events with NOGAPS-ALPHA; the August 2002 SH minor warming that preceded the major warming (Coy et al., Geophys. Res. Lett., 2005) when SABER was still yawed to view southern polar latitudes. Despite the lack of an explicit nonorographic gravity wave drag parameterization in these runs, the forecasts initialized with tropospheric and stratospheric analysis fields nonetheless reproduced a shallow lower mesospheric cooling response much like the one observed by SABER, and in contrast to the deep MLT cooling responses modeled by Liu and Roble (2002) and Ren et al. (2008). See Coy et al. (2005) for further details. The Ren et al. (2008) study cites none of this earlier work and argues for a generic deep mesospheric cooling response driven by nonorographic gravity wave drag that seems to be at odds with accumulated SABER observations of mesospheric temperature responses to stratospheric warmings. Its interesting, and probably significant, that both the 2006 event (Siskind et al., 2007,; this work) and the August 2002 event studied by Coy et al. (2005) and Siskind et al., although very different in length and intensity, fail to show the uniform deep cooling proposed as generic and predictable by Ren et al.. Thus we are not at all sure that Ren et al. (2008) is an appropriate citation in the portions of the text highlighted by Dr. Manney's comment, since doing so argues for dominant physical coupling mechanisms and mesospheric temperature responses proposed by Ren et al. (2008) that do not seem consistent with SABER observations reported both here and elsewhere. Nonetheless, we don't want to appear too negative about their paper. Their study is extremely interesting from the point of view of mesospheric modeling and data assimilation generally, and certainly illustrates the tremendous importance (and challenges) of accurately tuning the model's gravity wave drag parameterizations at these altitudes. In summary we will certainly cite this paper in parts of the revised paper, but with the caveats cited above in mind in choosing where and how to cite it.

6. Changed as suggested.

7. There appears to be a substantial latitude dependence of the MLS/SABER biases as evident in Fig. 7. In the future, we will consider using a more sophisticated bias correction scheme that can be developed and tested over a longer time period than the 2 months analyzed here. We agree that we could have modified MLS temperatures rather than SABER. From a practical standpoint, SABER latitude coverage during this 2-month analysis is less than MLS, so it is easier to construct a more representative correction to SABER using MLS rather than the reverse.

8. The first sentence of section 4.1 cites a list of references all of which describe the sudden stratospheric warming (SSW). We have added the Manney et al (2008 ACP) and Hoffman et al (2007) to this list, so that it now contains all the suggested references. A few of the subsequent citations of Coy et al. and Siskind et al. have been removed because the features being described can be found in most of the references cited in the list.

9. We presume the first sentence refers at the end to Page 8468 rather than 8458. Our forecasting runs to date suggest parameterized orographic, rather than nonorographic, gravity wave drag is relevant during the 2006 warming (Siskind et al., 2007), although that study did not activate a nonorographic gravity wave drag parameterization. We presume Dr. Manney is referring to the earlier modeling studies of Liu and Roble (2002) and Ren et al. (2008) that have stressed the nonorographic parameterization: here the earlier comments under point #5 pertain. Studying the relative roles of both orographic and nonorographic gravity wave drag parameterizations is the subject of ongoing research and we will discuss this more in revision. On point (1), without a completely analogous control assimilation lacking MLS and SABER inputs, we cannot answer this question definitively. Our focus here is on the mesospheric extension, rather than improving the stratospheric assimilation. On point (2), since the focus of this work was the mesospheric extension, the system was run at a much lower horizontal resolution (T79) than operational systems like ECMWF (T512 or T799), and there are substantial differences too in the types and ranges of data assimilated than just the MLS and SABER addition here. Given the importance of horizontal resolution in forecasting small-scale tropospheric blocking and ridging events that potentially seed SSWs (e.g., Allen et al., 2006), we think it is safe to assume that our low-resolution T79 assimilation runs will not compare as well with operational systems run at far higher resolution, notwithstanding SABER and MLS inputs. Given these myriad differences it is unlikely that we can say much definitive about any differences among the respective stratospheric analyses that may exist.

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10. We have not performed identical analysis of the SSW on other data sets such as GEOS-5 or ECMWF, although we find the same basic features of the SSW in the middle stratosphere in the NOGAPS-ALPHA analysis and GEOS-4, which are both used in the SSW study of Coy et al., (submitted 2008). We expect some differences in the middle stratosphere because the NOGAPS-ALPHA analysis uses MLS and SABER data above the 32 hPa level rather than AMSU-A radiances that are used in the other analysis, and because the model is run at lower horizontal resolution (T79). The focus of this paper is on using MLS and SABER to extend n DA system through the mesosphere for the first time, and not in using MLS and SABER to improve stratospheric analyses which, as Dr. Manney notes, appear to be of high quality already without input of MLS or SABER data. Only with an entirely analogous control experiment with MLS and SABER removed could we comment definitively on this question here.

11. The results near the pole are similar to those shown in Fig. 7 for 50-70 degrees latitude. We choose to show 3 latitude bins instead of 9 for brevity. Wider latitude bands are not used because we want the geophysical variability in each bin to be dominated by zonal variations rather than meridional variations, so that O-F correlations reflect skill at forecasting synoptic features rather than the background meridional gradient.

12. Changed as requested.

13. The white line in Fig 9 is an analysis error estimate based on MLS and SABER O-F values, which are only calculated at the levels the data are assimilated (above the 32 hPa level). Below this level, we expect the analysis accuracy to be similar to operational systems. By comparison with other meteorological analyses such as GEOS-4, we estimate that the RMS analysis error below the 32 hPa level is less than 2 K.

14. We agree that the symmetry and quiescence of the flow are important because they can be expected to diminish the impact of data assimilation beyond simply constraining the zonal mean temperature. Additional text has been added to make this point.

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