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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Enhancements of gravity wave amplitudes at midlatitudes during sudden stratospheric warmings in 2008" *by* T. Flury et al.

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1 Summary

This paper studies stratospheric gravity waves (GWs) resolved in radiosonde and satellite data in and around Switzerland during the stratospheric sudden warmings (SSWs) of January-February 2008. The authors infer periods and wavelengths of GW-induced fluctuations in radiosonde data from Payerne and compare with GW-induced radiance anomalies in satellite nadir radiances. Correlations with stratospheric potential vorticity, wind speed and ascent rate are attempted. The authors draw conclusions as to the role of enhanced tropospheric and stratospheric winds near the jet edge in spontaneously emitting enhanced GWs in the stratosphere (or perhaps troposphere), and speculate further as to their role in SSW dynamics generally.

The manuscript is (in my opinion) not up to the minimum acceptable standards required for a peer-reviewed scientific journal paper (ACP). I therefore recommend rejection.

My overarching concerns are articulated in a series of major comments below. In summary, the introduction omits any discussion of our first-order understanding of GW-SSW interactions and dynamics, indicating that the authors are not sufficiently informed about the GW-SSW science they are attempting to contribute to in this paper. The GW parameter analysis of radiosonde data in section 3.1 contains fundamentally flawed assumptions that invalidate the final results, while the satellite data in section 3.2 are poorly explained and improperly interpreted. The scientific analysis and interpretations contained in sections 3.2, 3.3, 3.5 and 3.6 are all particularly weak, consisting in each case of one short paragraph that inadequately explains an accompanying figure, then concluding prematurely with a poorly articulated physical interpretation that amounts to little more than speculation. Section 3.4 contains a confusingly errant motivation for correlating GW activity with stratospheric potential vorticity through mutual dependence on Brunt-Vaisala frequency. The paper concludes with a series of speculations as to the wider possible ramifications of GW dynamics on SSWs that are not supported by any of the preceding observational evidence.

The manuscript text is replete with odd grammar and awkward sentence constuctions that hinder clear articulation of the science. The paper would have benefitted from careful proofreading by colleagues prior to submission and I would strongly recommend this be done should the authors contemplate submitting any revised version of this manuscript.

2 Major Comments

2.1 Scientific Context

2.1.1 Current State of Knowledge

The introduction (section 1) betrays a near-total lack of familiarity with standard textbook knowledge of GW influences on stratospheric sudden warming (SSW) dynamics and vice versa. The impression conveyed to readers is that GW influences on SSWs remain all but unknown, but might be important (e.g., P29973 L4-5): the review of previous research focuses exclusively on a few purely observational studies of gravity-wave transmission during SSWs. I'd recommend the authors read the introduction and discussion sections of Siskind et al. (2010), which provide a reasonable review of latest state of knowledge in this area as well as providing some recent modeling results on GW effects on SSW dynamics.

Briefly, it has been known for nearly 30 years that the net hemispheric impact of SSWs is to inhibit the propagation of stratospheric orographic gravity waves (and perhaps nonorographic gravity waves) into the middle atmosphere, thereby substantially reducing the semicontinuous source of gravity-wave drag and diabatic descent for the winter polar upper stratosphere and mesosphere (Holton, 1983). Thus the mesosphere relaxes towards its colder radiatively-determined state, yielding the well-known "mesospheric sudden cooling" phenomenon that, until recently, was thought to accompany all SSWs (Labitzke, 1972; Holton, 1983). Recent observations and modeling have shown much richer and more variable thermal response of the stratosphere-mesosphere system during SSWs, due in part to varying impacts of orographic and nonorographic gravity waves during various stages of particular events.

The current observations need to be placed within this scientific context rather than

the current false picture of almost total scientific uncertainty. As one possible suggestion, apparent transient zonally-asymmetric enhancements of gravity-wave transmission at the displaced vortex edge during SSWs might be potentially interesting within the context of vortex-wide net reductions in gravity-wave activity entering the middle atmosphere during SSWs. For example, a wave-1 (vortex displacement) SSW would imply that the displaced vortex edge at these longitudes is accompanied by weaker winds at longitudes $\pm 180^\circ$ away from this same latitude, implying zonally asymmetric nonorographic gravity wave transmission into the middle atmosphere during SSWs, as has been recognized for many years (Dunkerton and Butchart, 1984). The resulting zonally asymmetric nonorographic gravity-wave breaking in the mesosphere may force *in situ* wave-1 planetary wave structures and high-level diabatic descent that cause the stratopause to reform above 80 km just after the primary SSW (see Siskind et al., 2010, and references therein).

2.1.2 Excessive Conjecture and Speculation

This lack of a proper scientific context for the present research based on unfamiliarity with the existing body of GW-SSW literature leads the authors into a lot of poorly informed speculation later in the paper. For example

- P29981 L12-15: "*One could imagine the vortex as a rigid rotor which is pushed out of equilibrium and then the axis of rotation begins to precess. The edge of the vortex shows the strongest shear flow, which is balanced by spontaneous emission of gravity waves (Williams et al., 2003).*" I have no idea what the authors are talking about to be perfectly honest. To the extent I do, the GW amplitude increases at the vortex edge have little to do with spontaneous GW emission from stratospheric shear zones, and pretty much everything to do with increased transmission of waves from tropospheric sources into the stratosphere and mesosphere due to strong uniform mean westerlies (no critical levels). To further confuse the reader, the authors later claim (P29983 L6) that the correlation is driven by GW radiation from tropospheric jets, which invalidates the stratospheric vortex radiation model above. None of it is supported by observational evidence in this paper, and is thus merely speculation (and contradictory speculation at that).

- P29981 L25 - P29982 L2: "*The presence of a curved tropopause jet is always linked with the presence of gravity waves, while the curvature of the tropopause jet can be caused by a blocking or a planetary wave. Thus gravity waves are a possible link between tropospheric blockings and SSWs. Martius et al. (2009) found a strong correlation between blockings and SSWs*." The first statement is simply false: it is only the small fraction of unstable curved jets that spontaneously emit GWs. The last statements confuse the dynamics totally. The blocking connection to SSWs is a long-known textbook correlation rather than something just discovered by Martius et al. (see section 6.2.5 of Andrews et al., 1987), and is related to *Rossby-wave* (not GW) generation that drives the SSW. Any GWs that are also produced are essentially irrelevant to the wave momentum driving of the SSW. There is more but I'll leave it at that. Suffice it to say, the statements above confuse rather than illuminate.

2.2 AMSU-A Radiances

The AMSU-A observations in Figure 5 are poorly explained (see section 2.1.1) and their scientific relevance to the radiosonde analysis and the theme of GW transmission during SSWs is tenous as best (see section 2.1.2).

2.2.1 Technical Details

It is claimed (P299974 L19) that the data in Fig. 5 were acquired by the AMSU-A instrument on Aqua. However, the (unexplained) titles on the AMSU-A plots in Figure

5 show "N18" and "N15," strongly suggesting to me that these observations were not acquired from Aqua, but instead came from the AMSU-A instruments on the NOAA-18 and NOAA-15 satellites.

Indeed, myriad important details of the data plotted in Figure 5 are left undiscussed. For example, it is claimed (P29979 L5) that the plotted fields are "AMSU radiances." In fact they are small-scale anomalies in the radiances, expressed as a brightness temperature (K), that were somehow isolated from the raw radiance fields. How exactly? What form of radiance data are these (Level 1b)? What is the time of each overpass (ascending or descending)?

It also appears that pushbroom imagery from several successive overpasses are overplotted in Figure 5. Are the footprint data at overlapping swaths averaged? If not, then what criterion was used to decide which swath data to obscure? Where is the color bar in Figure 5 and the associated physical units and dynamic range? Is the color scale linear or logarithmic? And so on.....

Later, the authors discuss GWs in radiosonde data that are "not visible for (*sic*) the satellite" (P29977 L5). Yet absolutely no details are provided about how the AMSU-A instrument acquires data and resolves GW structures, making such statement meaningless to all but the tiny subset of readers who know something already about how AMSU-A can detect GWs in its swath-scanned nadir radiances and which GWs the instrument can and cannot see.

2.2.2 Scientific Interpretation

Radiance anomaly maps in Fig. 5 are analyzed in section 3.2 as evidence of nonorographic inertia GWs, as opposed to the short fast orographic GWs supposedly inferred from section 3.1, despite the fact that no evidence whatsover is presented to convince the reader that the intrinsic periods of these waves are near the local inertial period.

The “analysis” (if one can call it that) of the GW in the left panel of Figure 5 consists of 2-3 brief sentences, unsubstantiated physically in any way, that the imaged GW propagates (quote, P29979 L7) “from France to Scandinavia.” While this statement is typically vague, the logical assumption is that the authors are arguing that this GW was generated by a nonorographic source somewhere over France and then propagated obliquely away from this source to lie over southern Scandinavia by the time the GW propagated to 5 hPa altitude.

Yet Eckermann et al. (2007) saw a remarkably similar GW-induced radiance anomaly south of southern Scandinavia in January 2003 at 5 hPa, and presented a detailed and convincing case that this wave was orographic (not nonorographic) and originated from flow over the mountains of southern Scandinvia (not from France). Contrasting the detailed forward-modeled analysis and 3D GW modeling in that paper with the 2 sentence speculations provided here, I’m more inclined at this stage to believe the Eckermann et al. (2007) interpretation: see also their 3D model simulations of the event in Eckermann et al. (2006) and their validation against radiosonde and aircraft data.

Likewise, the GW in the right panel of Figure 5 is simply asserted to be a nonorographic inertia GW propagating north-south across the Swiss Alps. Wouldn’t it be at least possible that this wave was orographic in origin? Without detailed modeling that takes into account the GWs that the instrument can and cannot see (see Eckermann et al., 2007), it is impossible to simply eyeball anomaly maps like Fig. 5 and make snap physical judgements as to the nature, source and geographical origin of these kinds of events.

Finally, the banding in both panels of Fig. 5 looks to be suspiciously parallel to the scan track and orthogonal to the satellite motion. It suggests that some of this banding could be instrumental, due to radiance glitches along track rather than any geophysical GW signal. This should be investigated. Lacking any details on how these anomalies were isolated, it is impossible to discount this at present.

In summary, it is not clear that the AMSU data in Fig. 5 offer anything at all currently to the study of GW dynamics during SSWs, and do not relate in any obvious way to the preceding radiosonde analysis in section 3.1.

2.3 GW Periods and Wavelengths

The discussion of GW periods and wavelengths inferred from the radiosonde perturbations is both confusing and misleading.

The problems start early in section 2, where the GW period T_p is defined as an absolute quantity (P29975 L3-9 – for simplicity, we’ll ignore the immediate confusion caused by the earlier definition of T a few lines earlier [L1] to represent temperature). Of course the Doppler effect means that wave periods are not absolutes, but frame dependent: in this case, a balloon-based frame that ascends obliquely through the atmosphere at an ascent velocity v_{asc}. This assumption then affects the discussion and analysis on pages 29977-29978, where wave period is now redefined as T rather than T_p. The authors discuss inferred values of $T \sim 5$ min as (quote, P29978 L18) “close to the Brunt-Vaisala period” which is “typical for a high-frequency gravity wave.”

While it is impossible to know for sure, since precise definitions are never given, these latter statements indicate to me that the authors believe that periods measured from the balloon frame are intrinsic periods (i.e. those measured from a frame moving with the background flow). The authors perhaps believe this based on their assumption (again, never stated explicitly) that the balloon is advected horizontally by the background horizontal winds. However, defining ω_b and T_b was wave frequency and period measured from the balloon frame, and ω and T as the wave’s intrinsic frequency and period, respectively, these parameter are related by the standard Doppler relation

$$\omega_b \quad = \quad \omega - m v_{asc}, \tag{1}$$

$$T_b = \left[T^{-1} \pm \frac{v_{asc}}{\lambda_z}\right]^{-1}, \quad (2)$$

where $|m| = 2\pi/\lambda_z$ is the wave's vertical wavenumber, $m < 0$ imples downward (upward) phase (group) propagation, and mv_{asc} is the Doppler-shift in frequency caused by the balloon's ascent.

Back-of-the-envelope calculations using $v_{asc} = 6.5$ m s^{-1} quickly show that the Doppler shift term mv_{asc} is large, except when $|m| \to 0$, and thus cannot be ignored in a GW analysis. In short T_b is **not** the intrinsic period, and thus the results in Fig. 9, for example, are essentially meaningless as an intrinsic wave property: they are instead a Doppler shifted period, which cannot be backed out to yield intrinsic parameters due to an inability to measure m values directly.

This in turn segues to the authors' discussion about measured vertical wavelengths $\lambda_z = 2\pi/|m|$. Figure 4 presents a schematic of the radiosonde GW measurement based on a GW with an assumed $m = 0$, which implies a ducted mode. The authors provide no physical motivation or justification for this choice (see P29978 L9): presumably, they view such choices as self-evidently realistic. They are not. While trapped orographic GW modes can be common in the troposphere, there is very little evidence that lower stratospheric GW fields are ever dominated by trapped modes. Rather, almost all previous studies show that GWs above the tropopause are dominated by free-propagating waves ($|m| > 0$). This in turn reinforces the invalidity of the $T_b = T$ hypothesis according to eqs. (1)-(2) above.

Figure 4 and the calculations in eq. (2) on P29978 include yet another critical but unstated assumption: that the horizontal GW phase speed is assumed to be stationary with respect to the ground, so that the GW structure in Figure 4 does not move. While this assumption is probably justifiable for an orographic GW, it needs to be stated clearly and explicitly. It is clearly unjustified for the nonorographic inertia GW interpretation later in the paper.

Ths confusion culminates in section 3.6 where the authors combine two GW-based assumptions for the radiosonde analysis: (1) a ducted orographic GW of $\lambda_z = \infty$ and $T \sim 5$ min, and (2) an inertia gravity wave, the latter yielding $\lambda_z =$ 1.6–3.2 km and (presumably) a period T close to the local inertial period of 12–24 hours. Since neither model is discounted, the net sum of all this analysis of GWs in the radiosonde data is that the inferred vertical wavelength could be anywhere between 1 km and ∞ and the GW period could be anywhere between 5 min and 1 day. Since this covers just about the entire range of permissable GW periods and vertical wavelengths, this result is equivalent to saying that the authors have no idea what the intrinsic parameters of these GWs actually are.

Which begs the final question: of what possible use is a wave-parameter analysis that can provide no definitive numbers for the wave parameters it is designed to quantify?

2.4 Polar Vortex Section 3.4

While a correlation between vortex location and GW activity can be valuable, the way this is motivated in section 3.4 is bizarre and fundamentally misguided. The authors claim (I think) that there is theoretical precedent for such a relationship since EPV depends on static stability, as does N which defines the GW upper frequency limit.

This supposed mutual N-dependence is not the reason why EPV and GW activity might be correlated. In the stratosphere PV is a long-lived dynamical tracer and thus provides a convenient way of identifying air masses of different origin (intra-vortex, mid-latitude and tropical air). Thus, a change of stratospheric EPV values over a radiosonde station is a convenient way of flagging when there is a significant movement of the vortex edge overhead, such that high (low) EPV means that the station is generally inside (outside) the vortex. The reason this is important is that vortex edge winds seem to preferentially transmit GWs into the middle atmosphere, so EPV maps provide a convenient way of monitoring when the vortex edge is moving across the local station since the EPV overhead will change.

The key point here is that EPV is used as a proxy for vortex edge winds overhead. This N-dependence that the authors discuss is a red herring and totally confuses the scientific rationale for such an analysis.

Figures 7-8 need to be analyzed physically using this awareness rather than this odd hypothesized mutual N coupling.

3 Minor Comments

P29972 L18: "...transport toward and descent over the pole..."
L25: if the authors are alluding to vertical rather than horizontal wind shear (?), then they have the situation confused. On average, winds weaken substantially during SSWs and vertical shear is substantially reduced. By far the largest mean vertical wind shears occur in cold undisturbed winters, where the vortex edge winds increase substantially with height.
P29974 L16, 17,19: the acronymns/abbreviations "a.s.l.," "SRS" and "AMSU-A" need to be spelt out prior to their first use here.
L22,25: "used" or something similar is better than "taken."
L25: A total of about 109 profiles are displayed....
P29975 L15: Not sure what is meant by the awkward phrase "we hesitate to do the neglection."
P29976 L24-28: the text here is rendered confusing by a missing right parenthesis.
P29992 Fig. 6: the white wind vectors on these plots are next to useless since no reference velocity scale is provided. Or are they just "directions" and not velocities as the caption claims?

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