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ACPD

9, S170–S182, 2009

Interactive Comment

Interactive comment on "Do gravity waves significantly impact PSC occurrence in the Antarctic?" *by* A. J. McDonald et al.

Anonymous Referee #2

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1 Summary and Recommendation

This paper combines POAM III PSC and H_2O measurements, and both CHAMP-GPS and Met Office temperatures to assess whether subgrid-scale temperature variability in CHAMP, due to resolved gravity waves, has any impact on the Antarctic PSCs observed by POAM. CHAMP temperatures are divided into fitted "synoptic" and gravity wave-induced components to assess the impact of the latter in dropping temperatures below representative T_{NAT}, T_{STS} and T_{ICE} thresholds. The authors report PSC occurrence rates in June-July that exceed those anticipated by synoptic temperatures alone. These events correlate with enhanced threshold crossings associated with gravity wave-induced temperature fluctuations resolved in the CHAMP profiles in June-July, which are possibly preferentially concentrated near elevated orography of the Palmer Peninsula, suggesting a potential orographic gravity wave influence here in the early winter season.

This is an important topic and the observational analysis throws up some suggestive correlations whose wider implications could certainly be of interest to ACP readers. This observational approach to the problem seems capable of providing some interesting new insights on the open question of how gravity waves impact Antarctic PSC formation. However, there are some major loose ends in the science as presented that complicate and occasionally obvuscate the conclusions that are drawn right now. The major points I really need cleared up are: (a) implicit biasing due to latitudinal drift in POAM sampling and possible omission of type-2 (Z_{min}) POAM PSCs; (b) observational filtering of the gravity wave fields resolved by CHAMP; (c) better statement of science goals and final conclusions based on reported evidence, taking into account changes forced by considering issues (a) and (b). These comments are articulated in greater depth below. Right now I'd defer recommending final publication in ACP pending satisfactory responses to those comments. Some additional data analysis is probably going to be necessary.

The comments that follow are organized such that longer comments come first, and then shorter specific comments are listed in order after that.

2 Organization of Material in the Introduction

The introduction contains good material but I felt it was not organized very well. Near the start the authors introduce the nominal PSC types but then start reviewing various observations, with key microphysical concepts dropped in at various points along the way, such as temperature formation thresholds for ice (P3404 L8), NAT (P3405 L7-22) and STS (P3406 L5). These thresholds should be introduced at the start immediately

ACPD

9, S170–S182, 2009

Interactive Comment

after introducing types 1a, 1b and 2, and then noting that $T_{NAT} > T_{STS} > T_{ICE}$, which is key.

The central microphysical issue focused on in this paper seems to be how these formation thresholds are attained in the Antarctic and how far below T_{NAT} one needs to go to get NAT to nucleate in practice. Those key science questions should be stated first, which then motivates the review of various studies that provide different and possibly conflicting perspectives on this overarching issue. Yet it is not until the paragraph on P3405 L7 that the authors really state this key science issue. I think the first three sentences of this paragraph, for instance, need to appear near the start after the basic type and temperature threshold definitions. Once the basics are set forth, the later paragraphs should be organized to speak to each of the specific uncertainties and complexities, leading from there to the possible but uncertain role of gravity waves and then to the current observational work.

This may not be the only way to do it, of course. I would just strongly encourage the authors to give serious considerations to a more logical development of the material here, to motivate this analysis better.

3 Systematic Latitudinal Drift in POAM III Measurements

Another issue that follows logically from the preceding discussion is the much greater stability of the Antarctic vortex, which yields colder intravortex temperatures (relative to the Arctic) with a stabler latitudinal temperature gradient. This in turn means that the T_{NAT}, T_{STS} and T_{ICE} limits all tend to be attained synoptically and that those threshold locations vary with equivalent latitude, such that T_{ICE} crossings occur mostly near the pole and T_{NAT} crossings nearer the vortex edge. This in turn yields a more regular synoptic distribution of PSCs in the Antarctic relative to the Arctic, with type 2's nearer the pole and type 1a's nearer the vortex edge (e.g., Fig. 2 of Höpfner et al., 2006). Yet

ACPD

9, S170–S182, 2009

Interactive Comment

nowhere is this made clear.

What is also not made clear until well into the paper (and then only in passing) is that POAM PSC observations are restricted to a narrow band of latitudes that migrates poleward during the winter (top panel of Figure 8). The combined latitudinal dependence of Antarctic synoptic temperature, dominant PSC type and POAM sampling is liable to have a substantial biasing effect on the PSC-temperature correlations sought here.

For example, in June and early July POAM is observing vortex edge latitudes where synoptic temperatures are warmer and much more liable to be impacted by additional gravity wave-induced cooling to push them below T_{NAT}. In August through October the POAM measurements are mostly near the pole (Figure 8) at latitudes where synoptic temperatures are much colder and thus where gravity waves are less liable to be needed to form PSCs. Little wonder then that the biggest gravity wave effects on PSCs in Figure 9 are noted in June and the smallest in August-October. This result seems an almost trivial consequence of the poleward march with time in POAM sampling of a stable Antarctic vortex, rather than evidence that gravity waves act on PSCs mostly in early winter as the authors argue (e.g., P3419 L3-5). The role of latitudinal sampling on these findings receives scant attention, just a few vague sentences in the final paragraph of section 4.

To me this seems like the obvious controlling effect and thus to indicate that POAM is not the ideal instrument for studying these particular effects, unless some effort is made to correct for them in some way (e.g., perhaps via remapping the PSC data to a PV-based range of expanded equivalent latitudes in each month as in Alfred et al. 2007). Another instrument like MIPAS that attains synoptic PSC maps at all vortex latitudes in every month seems like a better data source, since then no latitudinal (and hence temperature) biases are built into the gravity wave PSC analysis.

Some credible assessment of this effect needs to be made. Furthermore, the systematic latitudinal drift in the POAM III measurements needs to be introduced in the first or second paragraph in section 2.

4 GPS Visibility and Gravity Waves

As noted on P3410 L26, the horizontal resolution of limb occultations is ∼200–400 km along the line of sight (LOS) yielding a Nyquist horizontal scale of ∼400-800 km. Since gravity waves exist with horizontal wavelengths in the ∼5-500 km range, gravity waves whose horizontal wavenumber vectors point roughly along the LOS cannot be resolved. Only those long waves that serendipitously align orthogonal to the satellite LOS stand a chance of being resolved.

This complex instrumental “visibility” to gravity waves of different wavelength and orientation is endemic to all satellite temperature limb sounders and has been discussed in detail a number of previous gravity wave studies that used limb temperatures/radiances from instruments such as MLS, CRISTA, LIMS, and others (e.g., Alexander, 1998; Preusse et al., 2002; Jiang et al., 2004; Wu and Eckermann, 2008). For some reason this issue has been all but ignored in GPS-based gravity wave studies to date even though GPS temperatures will suffer from them as much as any other limb sensor (see the review of Wu et al., 2006). Liou et al. (2004) provided some very preliminary modeling of the effect for GPS, but (to the best of my knowledge) only with the very recent study by Alexander et al. (2008) have these important effects received any serious modeling attention in GPS-based modeling studies. The effect is completely ignored in this work.

The general point is that GPS, like all limb sensors, can only resolve a much reduced subset of the full space-time temperature spectrum of gravity wave motions due to limb smearing. Previous studies have shown that this “visibility” effect is absolutely critical to reconciling the resolved gravity wave variances with those resolved by other instru-

ACPD

9, S170–S182, 2009

ments (e.g., Preusse et al., 2000) or with gravity wave modeling results (Alexander, 1998; Jiang et al., 2004). At the broadest level, this means that the GPS data here resolve only a limited portion of the full spectrum of gravity wave-induced temperature perturbations potentially impacting Antarctic PSC microphysics and thus, at a minimum, these GPS-based results must represent some undetermined lower bound on the full impact of gravity wave temperature perturbations. This at the very least needs to be stated clearly in the paper.

Other limb and nadir instruments have well-developed forward and inverse models that allow these gravity-wave visibility effects to assessed, quantified and even sometimes corrected to some extent. It would seem highly useful to develop and apply such a model to these GPS temperatures to assess how much gravity wave-induced temperature variance is not being resolved here and thus what the full impact of both resolved and unresolved waves on Antarctic PSCs might be.

5 Shorter Comments and Typos

P3402 L11: The authors use a lot of acronyms that are never defined by spelling them out prior to first usage. In this particular case, hasn't the acronym "UKMO" been superseded now by the new moniker "Met Office"?
P3403 L17: replace "is" by "being"?
L18: Since you explain types 1a, 1b and 2, you need to explain what type 1a-enhanced PSCs are. The most salient aspect here is perhaps that they are due to tiny NAT (type 1a) particles of anomalously high number density, with the "enhanced" referring to the off-the-scale backscatter ratios that such PSCs yield when profiled by aerosol lidars. I'd also start a new paragraph here.
L20: here and throughout remove the capital "M" from "Mountain wave".
L22: here and throughout the umlaut is missing from "Höpfner".

ACPD
9, S170–S182, 2009

Interactive Comment

Full Screen / Esc
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Interactive Discussion
Discussion Paper

L26: replace "affect" by "effect."
P3404 L7: Given the review of data from many Antarctic stations, I would strongly recommend that a simple additional figure be added here that maps the locations of all the cited stations: Syowa, Davis (L25), South Pole, and Neumayer (P3406, L23). This seems particularly important given the later inference of an enhanced gravity wave effect for stations near elevated Antarctic mesoscale terrain.
L8: Condensation is vapor to liquid. I think you mean deposition. T_{ICE} is more usually referred to as the frost point temperature.
L16: "mesoscale microphysical simulations" is not really accurate. The simulations involved a global trajectory model with microphysics driven by analyzed winds and temperatures with subgrid-scale temperature perturbations due to unresolved mountain waves parameterized using fields from a separate mountain wave forecasting model. See Höpfner et al. (2006) for further details. You should ideally also spell out the acronym "MIPAS."
L17: 2003, not 2005.
L19: This modeling hypothesis for the origin of this June 2003 NAT PSC outbreak was recently validated independently by Eckermann et al. (2009) with the aid of gravity wave-resolving measurements from the Aqua satellite. See also P3418 L9-11.
P3405 L2: "makes" $\rightarrow$ "make"
L3: Confusing on two counts. First, there were two SOLVE missions, the first in 1999-2000 and the second in 2002-2003. Which one are you referring to? The confusion increases when you refer to "SOLVE-THESO" (sic) on P3419 L10. Second, and more importantly, these were *Arctic* missions. This needs to be pointed out to avoid confusing the reader.
L23: the authors should also consider citing the Buss et al. (2004) study of PSCs formed by a nonorographic gravity wave.
P3406 L3: is $\rightarrow$ are
P3407 L7-L17: this bias with respect to radiosonde and GPS was noted in earlier studies: for example, Gobiet et al. (2005) and Höpfner et al. (2006b).

ACPD
9, S170–S182, 2009

Interactive Comment

P3408 L21: POAM III measures things. Scientists do the research.
L26: you should add "described in subsequent paragraphs" to the end of this sentence, since at this point this odd-sounding algorithm is presented as if the reader should know what it is and what it yields.
P3409 L15: “none PSC” → “PSC-free”
P3410 L5-8: this suggests to me that the algorithm will also miss small isolated outbreaks of Antarctic PSCs, as may be the natural way in which gravity wave-induced PSCs are formed, at least initially, given their limited mesoscale extents. Can the authors comment?
L13: *from* measuring...
L10-14: I’m confused. As I understand it, PSCs are identified via limb occultation through either (I) an enhanced layer of extinction or (II) an extinction profile that stops at an anomalously high altitude (so-called Z_{min} events), with (I) usually corresponding to type 1 PSCs and II corresponding to type 2’s. Comparing this to the discussion here, am I to infer that your algorithm only searches POAM data for signals of the form (I) above (see also P3416 L2-4)? If so, it begs the question as to why you are also not screening for Z_{min} events to yield data on type 2 PSCs. Please clarify.
L14: extinctions → extinction
L18: was → were. See also L25.
L20: deatiled (sic)
L23: a more precise definition of what you mean by “dry temperature” is needed.
L27: I don’t know a lot about CHAMP, but I can only asssume that an intrinsic resolution of just 1–3 km across the LOS must somehow mean that the GPS transmitting and receiving antennae on each satellite must both point directly behind or ahead of each satellite’s orbital velocity vector – otherwise the orbital motions of each satellite would surely also cause the limb position to move rapidly in the horizontal too and smear the measurement cross-LOS during data acquisition (unless the limb sampling is incredibly fast). Could you clarify?
P3411 L4: it is also strongly dependent on atmospheric pressure.

ACPD
9, S170–S182, 2009

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Interactive Discussion
Discussion Paper

L20: why would you approximate T_{ICE} in this way when simple analytical parameterizations exist for it that are far more accurate than this gross NAT-based approximation (e.g., Marti and Mauersberger, 1993; Murphy and Koop, 2005)? This odd T_{ICE} approximation needs to be justified convincingly by the authors.
L24: Figures are out of order here: you've introduced Figure 4 before Figure 3.
L27-29: You need to clarify this a bit. Higher values above are, to first order, due to methane oxidation. I think you are referring to the second-order effect of wetter values at upper levels in October relative to June?
P3412 L7: "analyses" $\rightarrow$ "analysis". See also P3414 L10, P3431 L2.
L13: this is not at all clear to me in June-July. The green PSC contour in Fig. 3 is vertical whereas the temperature contours vary in both time and height. If you're looking for a more direct visual correlation, wouldn't you have been better off converting the temperature contours to something more microphysical, such as saturation ratio?
3413 L13: minor point, but here and elsewhere there's a lot of talk about competing mechanisms and which one is correct. Seems to me slightly misguided, in the sense that the mechanisms are probably all viable (if the observational/modeling studies supporting them are to be believed) but all the necessary conditions for them may not exist. Recall T_{NAT} is a necessary but *insufficient* condition. For example, the presence or absence of nuclei such as from meteoric dust must surely be critical to whether PSCs form a few K below the NAT point or else require much colder ice-forming temperatures. See also P3414 L24-25.
L27: See earlier comment on P3411 L20 above.
3414 L17-19: this is surely just a consequence of the latitudinal POAM sampling of lower latitudes with generally warmer temperatures.
L27: The June 2003 enhancement was also observed and modeled by Höpfner et al. (2006), who considered all major nucelation mechanisms and showed that only heterogenous nucelation on ice formed in orographic gravity waves could reproduce the observed PSC morphology, as later validated by Eckermann et al. (2009).
3415 L15: seems like the nearest Met Office profile should be superimposed too.

ACPD
9, S170–S182, 2009
Interactive Comment

L22: surely again controlled mostly by the poleward drift in POAM sampling during the winter to colder intravortex latitudes.
P3416 L1: calculation → mean?
L29: 4 and 2 → 2 and 4; suggests → suggest.
P3417 L10: affect → effect
L12: this is hard to see using the Fig. 7 color scale.
L21: can not → cannot. In terms of the points you are trying to make here, wouldn't it be better to change Figure 7 from the two panel temperature plot into a single panel contouring the differences between the two? Given the color scale it is very difficult for me to see any substantial difference between the two.
P3418 L7-9: "confirmed" seems a bit optimistic given that these correlations are circumstantial evidence at best. Surely you'd need a microphysical model to test this properly? See especially the larger comment below for P3419 L29 where I develop this point a little more.
L11: different year though: 2003, not 2005.
3419 L29: Well, yes ... but then Wang et al. say in their very next sentence (pgh[8] L6), quote: "*..the observed PSC distribution pattern cannot be explained by mountain wave effect*," a statement in *total disagreement* with your claim (P3418 L7-9,11-12; P3419 L3-5) to have "confirmed" enhanced PSC occurrence over this region due to gravity wave activity. So who is the reader to believe? The authors need to state conclusions that aren't self contradicting, but state clearly and defensibly what these results do and do not show.

A few personal opinions to maybe help out here. The Wang et al. assumption that PSCs formed in mountain waves must be closely confined to the parent orography is wrong, disproven by countless observational and modeling studies too numerous to review here (for a salient Antarctic example, see Höpfner et al., 2006). It is presumably based on the misunderstanding that reversible gravity wave-induced temperature perturbations must necessarily produce reversible microphysical effects, whereas the strongly nonlinear hysteresis inherent in PSC microphysics means that such waves

ACPD
9, S170–S182, 2009

Interactive Comment

can and do yield *irreversible* microphysical effects, such that wave PSCs can survive and grow downstream if synoptic temperatures remain cold enough. Indeed, this is really the only reason why we are much interested in gravity wave-induced PSCs. If they were confined just to cold wave phases, they would be interesting but (as Wang et al. try to argue) too geographically isolated and transient to have any significant net effect on synoptic PSC distributions and hence on ozone loss. Instead, gravity waves can in certain cases continuosuly create ice nucleation sites for solid NAT to form and survive, thus processing large volumes of air to fill out large downstream volumes with type 1a's that otherwise might not have formed.

P3421 L2: affect → effect.

L7: data are...

L28: after searching around the web I think I found this reference, and the author list shown here looks wrong. Since you supply URLs to other straightforward citations you should include a URL to aid ACP readers in findings this obscure U.S. Army technical report: e.g., `http://www.arl.army.mil/www/default.cfm?Action=17&Page=239&Topic=TechnicalReports&Year=2007`.

P3426: Physical units (Kelvin) are missing from the color bar and/or caption. L-2 "...18 km, following Hanson....". Since this fit is based on pressure variations, it would be clearer if you stated the pressure you adopted as corresponding to 18 km altitude.

P3427: Physical units (ppmv) are missing from the color bar and/or caption.

P3428: Physical units (Kelvin) are missing from the color bar and/or caption. The dotted green curve is almost invisible in my printout.

P3429 L-2: these error bars are almost impossible to see on these small panels. Consider maybe plotting representative ranges for each curve separately along the right inner edge of each panel.

P3430: L2 "represents" → "approximates"; L3 "represent" → "represents".

P3431: This caption seems to have multiple errors: L2 "black stars" should presumably be "green stars"; L3 "black lines" should presumably be "red lines"; L4 the end of the sentence presumably omits to mention that these are a plotted with blue lines; L5

ACPD

9, S170–S182, 2009

Interactive Comment

"grey lines" presumably should be "blue lines". I can also see no evidence of the error bars cited in the caption in these plots.
P3432: as mentioned earlier, this highly saturated red-blue color scale blurs most of the detail here and is probably completely illegible for any colorblind readers. Is it that hard to overplot a few labeled contours, say every 5 K? The observation year or years should also be stated in the caption.
P3433: the two separate panels are unlabeled yet the caption refers to a "**(b)**"? **P3434**: there should be both x- and y-axis error bars on all these points.

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ACPD
9, S170–S182, 2009

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ACPD

9, S170–S182, 2009

Interactive Comment