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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Small-scale gravity waves in ER-2 MMS/MTP wind and temperature measurements during CRYSTAL-FACE" *by* L. Wang et al.

**Anonymous Referee #2**

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This paper analyzes high-resolution data acquired from 10 ER-2 flights in the subtropical lower stratosphere near southern Florida during the CRYSTAL-FACE mission: specifically, vector wind velocities $(u, v, w)$ and temperatures $T$ acquired by the Meteorological Measurement System (MMS) and vertical profiles of temperatures and vertical temperature gradients acquired several kilometers above and below the aircraft by the Microwave Temperature Profiler (MTP). The authors identify level linear flight segments for analysis of fluctuations in these data, which they ascribe to gravity waves. "Coherent" wave events are identified when S-Transform (wavelet) spectra identify common enhancements in temperature and at least one velocity component. 136 wave events are identified and characterized three-dimensionally using Stokes parameter cross-ST analyses of MMS data, temperature and temperature gradient analyses of MTP to infer vertical wavelengths, cross-ST analysis between winds and temperatures to infer absolute horizontal propagation directions, and cross-ST spectra between horizontal and vertical velocities to infer vertical fluxes of horizontal GW pseudomomentum densities. Reverse ray tracing of characterized waves is performed to locate their tropospheric source regions, with 76% originating from nearby tropospheric convective systems.

This is an interesting paper that I enjoyed reading. It addresses an important topic (convectively generated GWs) about which little is known observationally, using a novel complement of field experiment data and analysis techniques to fully characterize a large number of waves. The results will be of interest to the ACP readership.

However, this ACPD draft contains perceived weaknesses, the more major of which are outlined below. These will need to be addressed before the paper can be published. A list of more minor comments and corrections follows thereafter.

## 1. Major Comments

**1. Page 11380 L9-14**

The GW studies cited here used MMS and MTP data acquired in previous ER-2 missions. The MMS and MTP data acquired during CRYSTAL FACE are not substantially different from those acquired in these previous missions and analyzed in the cited studies (e.g., MMS has issued 1 Hz winds and temperatures for over a decade). Thus, I see nothing in these MMS and MTP datasets from CRYSTAL FACE that leads me to conclude that they provide any substantial new information over previous versions of these data, and so I disagree with the statement the authors are making here.

A case could possibly be made that this study provides a more sophisticated analysis of



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both MMS and MTP data for their three-dimensional gravity wave content than previous studies, but that case is not made here or elsewhere in the paper at present.

**2. Page 11381 L14**

The statement about the intrinsic MTP horizontal resolution being 1.3–2.5 km is very confusing. If the sampling rate is 0.1 Hz and airframe velocity 200 m s$^{-1}$, this seems to immediately imply a minimum horizontal resolution of 2 km. I think (?) the issue here is that, for a given scan, temperatures are derived from radiances acquired in two MTP channels, which peak at different altitudes, yielding essentially two temperature profiles for a single scan, separated horizontally by the airframe motion, as depicted in Figure 9 of Denning et al. (J. Geophys. Res., 94, 16,757, 1989). If so, this is a very subtle instrumental issue that requires some explanation. If not, the authors must explain how MTP can obtain sub-Nyquist horizontal resolution (i.e. $<$2 km) for a 10 s integration time. Also, how do the broad exponential weighting functions of the $O_2$ microwave radiances at different scanned elevation angles (e.g., Figure 7 of Denning et al., 1989) factor in to the final quoted horizontal resolution of the profiles?

These are important issues to resolve, since GWs studied here have $\lambda_h$ $\sim$10-20 km (Figure 5). If MTP's horizontal resolution was significantly coarser than the 1.3-2.5 km currently claimed, it casts doubt on waves apparently resolved in the data.

Additionally, line 17 claims an MTP vertical resolution of $\sim$100 m at flight level. Figure 9 of Denning et al. (1989) does not appear to support that: values nearer 200-300 m seem to be implied, and this figure does not account for additional broadening from the line-of-sight weighting functions at each point (Figure 7 of Denning et al.).

**3. Page 11382 L5-11 and Page 11403 (Figure 3)**

I did not find Figure 3 to be very impressive. If coherent GWs were present in the MTP data, as is argued later, I would expect to see sloping 2D linear phase lines in this figure connecting positive and negative GW temperature phases, as is routinely seen, for example, in lidar profiles acquired from the NASA DC-8 (e.g., Dörnbrack et al., J. Geophys. Res.,107(D20), 8287, doi:10.1029/2001JD000452, 2002). No such structures are evident in Figure 3. Rather, the fluctuations in Figure 3 divide into three vertical ranges that barely correlate with each other: (a) upper altitudes $>$21 km, which show vertically deep, short horizontal wavelength structures; (b) lower altitudes $<$18 km which show much the same sort of structure, but relating in no obvious ways to those in (a); (c) near flight altitudes $\sim$18-21 km, which show longer horizontal wavelength structure which disappears at higher and lower MTP altitudes, with little evidence of the short wavelength structures seen above and below this layer.

I wonder whether the height-dependent vertical resolution of MTP (Page 11381 L 17) is observationally filtering out GWs with altitude, in a somewhat analagous way that stratospheric $O_2$ microwave limb radiances from satellites observationally filter out GWs versus altitude and location (e.g., Alexander, J. Geophys. Res., 103, 8627, 1998; McLandress et al., J. Geophys. Res., 105, 1947, 2000)?

Linearly-detrended temperature anomalies might not be the best way to find sloping 2D GW structures in MTP data. Previous MTP studies have had more success finding such structures when plotting potential temperature surfaces (e.g., Bacmeister et al., Geophys. Res. Lett., 17, 81, 1990; Bacmeister et al., Wea. Forecasting, 9, 241, 1994).

**4. Stokes Parameters and Cross Spectra: Pages 11385–11386**

First, a general comment. Here the authors analyze MMS and MTP data based on Stokes parameters and cross-spectral S-transform methods, which they contrast with the hodographic method (Page 11385, L5). Eckermann (J. Geophys. Res., 101, 19,169, 1996) showed that Stokes parameter analysis (either real or spectral), hodographic analysis, and cross-spectral and rotary spectral analysis are all mathematically related and hence equivalent. All these techniques characterize specific aspects of polarized wave structures using decomposition algorithms. So, for example, the authors' discussion of their new technique to use of the phase of the cross S-Transform spectrum (Page 11385 L18-25) to evaluate $\delta$: this too is just a straightforward consequence of standard equations linking cross-spectra and spectral Stokes parameters: see equations (13)–(16) of Eckermann (1996).

That having been said, no discussion or plot of derived $\delta$ values are provided. What is noted (e.g., Page 11386 L27) is that all GWs have high intrinsic frequencies, such that $|f/\hat{\omega}|$ ≈0. This in turn implies that all these GWs should be linearly polarized, since elliptical GW polarization only occurs for $|f/\hat{\omega}| \neq 0$ (in the absence of strong horizontal wind shear). This in turn implies that $\delta$ values for these GWs should all be very close to 0° or 180°. Is this what was returned by the cross-ST analysis? The authors need to show some results. If so, it is powerful additional evidence that they are measuring high-frequency linearly polarized gravity waves. If not, the inferred GWs are elliptically polarized, which is not consistent with polarization relations and the authors will need to explain why this occurs and how it can be reconciled with GW physics.

If the cross-ST analysis did return $\delta$ values clustered ∼0° and 180°, this considerably simplifies the discussion of how temperature oscillations are used to infer absolute horizontal propagation directions using eq. (9) on Page 11386, L5. Since the numerator in the arctangent argument in eq. (9) now vanishes, it simplifies to $\Phi_T - \Phi_u = \pm\pi/2$, and the analysis just infers whether the temperature oscillation leads or lags the horizontal velocity oscillation by 90°. This is completely consistent with the standard polarization relation in eq. (A10), since the sign of the RHS of (A10) is determined by the sign of $k_h$, which in turn is determined by the absolute direction of horizontal propagation. I would encourage the authors to use some version of this argument, since it provides a simpler and more physical picture of how and why this algorithm works.

These ST amplitudes and phases are used on Page 11387 to derive vertical fluxes of GW horizontal pseudomomentum densities *via* eqs. (12) and (13). Its not clear to me why the spectral phase difference terms $\Phi_w - \Phi_u$ and $\Phi_w - \Phi_v$ are simply multiplied here. My back-of-the-envelope scribblings suggest that the correct terms here should be $\Re\left[\exp\imath(\Phi_w - \Phi_u)\right]$ and $\Re\left[\exp\imath(\Phi_w - \Phi_v)\right]$, respectively, where the $\Re[]$ operator extracts the real component of a complex number. The authors need to clear this up and, if the errors in eqs. (12) and (13) suggested here are confirmed, they will need to repeat



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their calculations using these correct formulae and assess how the modified fluxes affect their results and conclusions.

Finally, the term "coherence" is used a lot in the paper in relation to the cross-ST analyses used to find wave perturbations for analysis and characterization (e.g., Page 11384, L2). This nomenclature is initially quite confusing since, in standard cross-spectral analysis, "coherence" has a precise mathematical meaning. The cross coherence is the normalized cross spectrum (Eckermann, 1996). Further, cross coherence spectra have been used to extract atmospheric GWs from noise and characterize them as here (e.g., Cho, J. Geophys. Res., 100, 18727, 1995). Thus, the immediate assumption is that cross-ST coherences are being evaluated and used to identify coherent GWs, as in Cho (1995). But in fact that is not the case: "coherent" is being used here as a vaguer nonmathematical adjective, more like "apparently wavelike" or "quasi-monochromatic." So it would be helpful to the reader if, early on, the more *ad hoc* "wave coherence" cross-ST analysis performed in this paper was clearly contrasted with the formal mathematical cross-coherence GW analysis method of Cho (1995) and others.

**5. Reverse Ray Tracing: Page 11389**

The reverse ray-tracing described on lines 5-8 states that (quote): *The reverse ray-tracing was terminated when any of the following conditions was met: the tracing time reached 3 h, the ray reached the ground, or the wave was refracted so much that its intrinsic frequency was beyond the possible range of a propagating GW, i.e., being smaller than $f$ or larger than $N$.*

The latter part of this sentence is concerning, since correctly-coded nonhydrostatic GW ray equations should never yield $\hat{\omega}$ values less $|f|$ or greater than $N$. Ray groups start slowing down as they approach their Jones critical level ($f^2/\hat{\omega}^2 \rightarrow 1$) and eventually "stall out" as they ever more slowly asymptote towards it. Near turning points ($\hat{\omega}^2/N^2 \rightarrow 1$), WKB violations activate in the ray code and eject the GW. In all cases, the limits $f^2 < \hat{\omega}^2 < N^2$ are never violated. The authors should explain why their ray equations are yielding $\hat{\omega}$ values outside these ranges. For example, an irrotational hydrostatic



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dispersion relation could give rise to this effect.

As discussed later in the text, the GWs to be traced have westward ground-based phase speeds but eastward intrinsic phase speeds. Such GWs must be treated very carefully in a ray-tracing algorithm, since these different directions lead to sign ambiguities which must be taken into account when setting the signs of wave parameters in the model. For example, Eckermann (J. Geophys. Res., 97, 15,849, 1992) shows that, using his sign convention, such GWs must have negative intrinsic frequencies in the ray code to maintain the correct sign values for phase and group motion in both the intrinsic and ground-based frames (see his Figure 2). The authors should check whether their traced GWs maintain similar consistency: if not, it may explain the occurrence of frequencies outside free-propagating ranges in their ray calculations.

Also, on line 4: are you or are you not using the fitted local time variations in the NCEP fields to update the ray equation for $\omega$ via a ray equation of the form $d\omega/dt = \partial\Omega/\partial t$? If so, how much does the ground-based frequency $\omega$ vary along ray paths in response to these time variations? This needs to be clarified in revision.

**6. Vertically Trapped Stratospheric GWs or Critical Levels?: Pages 11391-11392**
Here (Page 11391 L14) the authors argue for (vertically) trapped GWs. How does this trapping occur? The only evidence in the paper for vertical reflection at turning points is on Page 11390 L3, which argues for a turning point somewhere between 13 km and 20 km. But for stratospheric trapping, there must also be another turning point somewhere above 20 km altitude. That is not discussed. Indeed, from Figure 11, it seems that if this wind shear from 13-20 km yielded turning points at lower altitudes, the similar shear above 20 km should decrease intrinsic frequencies and not yield turning points higher up. Am I missing something here?

But then on Page 11392 L7-11, the authors argue that ground-based phase speeds are weakly westward at 20 km. From zonal winds in Figure 11, this would imply not that these GWs reach turning points below 20 km, but instead reach *critical levels* somewhere below 20 km! Such GWs would then also clearly reach turning points somewhere above 20 km given the shear in the zonal wind profile and their high intrinsic frequencies at 20 km. This scenario would then not yield trapped GWs. Instead, forced GWs would propagate upwards, reflect vertically somewhere above 20 km, then propagate downwards and be absorbed at critical levels somewhere below 20 km.

This confusion in the GW physics here needs to be cleared up. Is it turning points or critical levels below 20 km? Why do the authors believe their GWs are stratospherically trapped? What is the mechanism they envisage here? Can they justify their hypothesized trapping model better?

## 2. Minor Comments

**Page 11378**
**L2**: This opening statement uses three unexplained acronymns in its first 4 words, none familiar to anyone bar a few specialists. The authors need to explain this better so a nonexpert ACP reader, perusing abstracts online, can judge whether they want to read this paper. For example: "Lower stratospheric wind and temperature measurements made from NASA's high-altitude ER-2 research aircraft during CRYSTAL FACE...."
**L8**: Again, "the MTP temperature gradient method" has no meaning (not even to experts) unless section 3.2 is read in full. Thus the use of this phrase in the abstract is conveying zero information to ACP readers.
**L13-14**: You need to define the symbols $\lambda_z$ and $\lambda_h$ in the abstract if you want readers to understand this. Also, the phrases "generally short horizontal scale" and "$\lambda_h$ generally shorter than 20 km" in this same sentence seem to be saying the same thing two different ways. I would omit the former phrase.
**L24**: replace "cooling rates" with "heating/cooling rate amplitudes."

**Page 11379**

**L2**: The authors start off here using the abbreviation “GW” for gravity waves, as previously defined in the abstract (Page 11378 L4). That’s fine, but then later (Page 11381 L19) the authors redefine this abbreviation. So, either delete this redefinition on Page 11378, L4, or else move it to this point in the text.
**L10**: delete comma after “Bacmeister.”
**L13**: “Floria” (sic)
**L14**: there is a missing right parenthesis here (there should be 2).
**L27-28**: I would delete this last sentence, since its a bit weak and I think the preceding sentences in this paragraph make the point anyway.

**Page 11380**
**L1**: Recent studies have shown that...
**L1-2**: delete “were found to.”
**L2**: “high and cold” is somewhat redundant. Since they’re so high, the pressure-dependent frost point temperature implied by ice physics is low and so immediately tells us that the local temperatures here must be very cold, and we also know that the TTL is cold. Hence, just “high cirrus” is enough.
**L8**: This is an indirect effect: they can also affect local chemistry much more directly since these cloud particles are sites for classes of heterogenous chemical reactions, much like PSCs in polar winter which process chlorine from reservoir into activated ozone-destroying forms (e.g., Bregman et al., J. Geophys. Res., 107(D3), 4032, doi:10.1029/2001JD000761, 2002).
**L9**: “predict” doesn’t work here. Better perhaps to change the first instance of “to predict” to “in modeling” and the second instance to “to specify.”
**L14**: again, this last sentence is best deleted, since its vague and the preceding sentences make the point more directly.
**L20-21**: replace “In the end” by “In Sect. 6,” and delete “summary and.”
**L26**: replace “with” by “at.”
**L26-27**: suggestion: *The dates and times of all 10 ER-2 flights are listed in Table 1.*



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**Page 11381**

**L1**: Suggested rewrite: *...started either late in the morning or early in the afternoon, and ended late in the afternoon.* Also, you can delete "(in local time)" since morning and afternoon are intrinsically local-time specific.

**L5**: capitalize here, i.e. "Meteorological Measurement System."

**L7**: likewise, "Microwave Temperature Profiler."

**L9**: delete the degrees symbol here: i.e. "0.1°K" should just be "0.1 K."

**L10**: if MMS data were acquired as in previous missions, then I think its more corrrect to say that the MMS sampling rate is nearer 10 Hz, but postprocessing averaged down to a final MMS time series of 1 Hz to improve signal-to-noise.

**L24**: suggested rewrite: *In total, 136 flight segments of this kind were selected, ranging from ~50–1100 km in length, with the majority in the ~200 km range.*

**Page 11382**

**L1**: Here would be a good place to define mathematical symbols for winds and temperatures, which you use later (e.g., Page 11383 L24) without prior definition, i.e.: "Figure 2 plots MMS zonal velocities $u$, meridional velocities $v$, vertical velocities $w$ and temperatures $T$ for the flight segment..."

**L3**: "grid" implies two dimensions. Better perhaps to say "...interpolated to a regular 0.2 and 2 km horizontal resolution along the flight track for MMS..."

**L7**: ..raw temperatures were...

**L12-14**: There's a lot of changing of tense in the writeup. Here, for example, I'd just stick with present tense instead of changing to future tense.

**L24**: the use of $w$ here and subsequently to define your mother wavelet clashes with the later use of this same symbol $w$ to define MMS vertical velocities. Change one or the other symbol throughout the text to avoid confusion.

**Page 11383**

**L1**: You need to define what you mean by "apparent wavelength." In fact, later on Page 11384 lines 4–7 you provide that definition. It belongs here, so I would cut that text and place it here before the sentence beginning “The mother wavelet...”

**L4**: probably OK to delete “and $\imath$ is the imaginary unit” since that should be clear enough, especially if you use the latex “imath” symbol. If not, delete “unit” since imaginary numbers don’t have intrinsic physical units.

**L15**: *...less clearly in $w$...*. Note once again that you have used $w$ here to depict MMS vertical velocity without previously defining it as such, and that this implicit definition clashes with the current definition for $w$ as the mother wavelet in eq. (2).

**L23**: insert “of” before “10–20 km.”

**L24**: It might be worth remarking somewhere that these “average” wave amplitudes (peak or rms?) for $u$ and $v$ of $\sim$0.7 m s$^{-1}$ are much smaller than what is usually seen in the inertia gravity waves typically resolved in radiosonde profiles, for instance. But such smaller values are consistent, via polarization relations, with the muich higher intrinsic frequencies $\hat{\omega}$ inferred for these GWs.

**Page 11384**

**L12**: you defined $\imath$ earlier in eq. (2).

**L17**: change “from” to “by.”

**Page 11386**

**L14-15**: clarify whether these frequencies are in radians per second or cycles per second.

**L16**: why didn’t you calculate Brunt-Väisälä frequencies directly from mean temperatures and vertical temperature gradients measured by MTP?

**L17-22**: This wouldn’t work, but not because of noise. Instead, all the GWs you are measuring are high-frequency GWs for which $f/\hat{\omega} \approx 0$, and thus linearly polarized. Thus they cannot be diagnosed for their ellipticity to infer intrinsic frequencies.

**L19**: “Stoke” should be “Stokes.”

**L24-25**: I think it is important to point out here that the MTP vertical resolution of $\sim$2 km limits the analysis from eqs.(3)-(4) to detecting GWs of $\lambda_z \geq$4 km. That bias comes out in the results, for example in Figure 6 (left panel) on Page 11406.



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**L27**: I assume you are talking about *horizontal* phase and group speeds here?

**Page 11387**

**L6**: eq. (10) does not describe a force but an acceleration. You would need to multiply it by $\rho$ to make it a body force.

**L12**: Since all the GWs analyzed here have $f^2/\hat{\omega}^2$ very close to zero, this probably justifies deleting the (1-$f^2/\hat{\omega}^2$) factors in eqs.(11)-(13). However, since $\hat{\omega}^2/N^2$ is not negligible it would seem pertinent to carry the $1 \pm \hat{\omega}^2/N^2$ terms in the dispersion and polarization relations through to these relations to see how they scale down the momentum fluxes from the Reynolds stress terms $< \tilde{u}\tilde{w} >$ and $< \tilde{v}\tilde{w} >$.

**Page 11388**

**L24**: replace "event" with "ray group" and insert "ground-based" before "group velocity."

**Page 11389**

**L2**: your use of the symbol $k$ here to define the zonal component of GW wavenumber clashes with the earlier definition of $k = 2\pi/\lambda'$ in eq.(2) to be the apparent wavenumber. One or the other symbol needs to be changed throughout to avoid confusing readers.

**L12**: The color scale of Figure 8 (Page 11408) needs to be modified. First, the black flight track gets lost in the black image background. More importantly, to my eyes the pink ray lines are extremely difficult to differentiate from the red flight segment. Colorblind readers would have no chance. Finally, the units and range of the NEXRAD data are not provided, nor are the blue 3 character site markers explained in any way.

**L20**: The relation between the final positions of the reverse-traced ray locations and the intense convection in the maps of Figure 8 does not strike me as particularly close. Maybe the significance can be explained a bit more.

**L26**: what exactly is the criterion used to argue that $\sim$76% of the reverse ray paths were traced back to convective events? How close to NEXRAD convection do these paths have to be to qualify as close enough? Right now we are just taking the authors' word for these final numbers. More information or maybe even a plot would be helpful.

## Page 11390

**L3**: "Their $\hat{\omega}$ exceeded $N$ above 13 km." What does this mean exactly? Does this mean a turning point $\omega \rightarrow N$ occurs at an altitude somewhere between the flight altitude of 20 km and 13 km? If so, this should probably be stated more explicitly.
**L6-7**: This sentence is confusing. The first part discussed the "source to event direction" whereas the remainder of the sentence defines it in opposite terms of going *from* the event *to* the source, i.e. an event to source direction. Which is it?
**L9**: replace "Discussions" by "Discussion."
**L17**: more correct here to say that this follows from a hydrostatic irrotational approximation to the dispersion relation?
**L22-23**: ...were found to have short horizontal wavelengths and high...
**L25**: ...carry no *net* momentum flux.

## Page 11391

**L4-6**: Figure 10 is interesting but also possibly a little misleading as currently presented. Inspection of it indicates that there is a broad region of ($\hat{\omega}$,$\lambda_h$) space where no waves exist. But the lack of wave signals in these regions is a consequence of the vertical resolution limits of the MTP instrument. For example, the $\sim$2-2.5 km vertical resolution of MTP data limits it to detecting GWs of $\lambda_z \geq$4–5 km. So let $m_5 = 2\pi/$ 5 km. Then, using the dispersion relation for GWs, the range of GWs in ($\hat{\omega}$,$\lambda_h$) space for which $\lambda_z \geq$5 km is demarked by the line

$$\frac{\hat{\omega}}{N} = \frac{k_h^2}{k_h^2 + m_5^2}, \tag{1}$$

where $k_h = 2\pi/\lambda_h$. If you evaluate (1) and plot it as a line in Figure 10, you should see that it produces a hyperbola that marks out the separation between colored pixels and no pixels in these plots. This curve should be included in these plots, since it points out that GWs in these other regions are probably present as well, but that these data are not sensitive to them.
**L24**: "previous studies as referenced here." Which cited studies are you talking about?



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For example, earlier in the paper you cited a range of studies like Bacmeister et al. (1990a, 1990b), Alexander and Pfister (1995) etc., and those studies analyzed MMS and MTP data and saw high frequency, short horizontal wavelength GWs. I think you are referring to radiosonde studies perhaps. Anyway, this needs to be rewritten to be much more specific about the earlier studies you are referring to.

**Page 11392**
**L14**: delete “out.”
**L15**: delete “effect.”
**L20**: The authors need to be careful with wording here. The “cooling rate” due to GWs is generally associated with the irreversible change in background temperatures produced by vertical convergence of vertical heat fluxes (due to GWs and/or GW-induced turbulence) when GWs dissipate. What is being discussed here is simply reversible fluctuations in $\hat{\omega}\tilde{T}$, the Lagrangian time derivative of GW-induced temperature perturbations, which averages out to zero to produce no mean cooling (or heating). I would encourage the authors to refer to this as “heating/cooling rate perturbations," or something similarly more accurate than “cooling rate,” to avoid confusion. Note that even though the microphysicists focus on the GW-induced cooling rate phase of GWs, since it is the more microphysically important term for forming aerosol, they do need both the heating and cooling rate phases in their models.
**L23**: ....with a certain peak heating/cooling rate amplitude.
**L28**: ...Most events had peak heating/cooling rate amplitudes less than 22 K h$^{-1}$....

The authors might give some thought as to how these heating/cooling rate amplitudes would be interfaced to the absolute GW temperature amplitudes by a modeler, since both are obviously needed in a microphysical model with appropriate phasing. Would a sum of intermittent sinusoids be adequate, or would an intermittent spectral parameterization be better (e.g., Bacmeister et al., J. Atmos. Sci., 56, 1913, 1999)?

**Page 11393**
**L6**: delete the “flight” that occurs before “segments.”

**L20**: "....using both the temperature and vertical temperature gradient oscillations measured along the flight track by MTP."
**L22**: as noted in Major Comments, the cross S-transform method is just a spectral Stokes parameters method in a different-looking formulation (or equivalently, spectral Stokes parameters are just cross S-Transform spectra in a different guise). They are all interrelated variants of a common analysis procedure: see Eckermann (1996).
**L28**: again, worth noting here that the MTP method for inferring $\lambda_z$ limits one to GWs with $\lambda_z$ $\geq$4–5 km, due to MTP's finite vertical resolution. See Figure 6.

**Page 11394**
**L1**: "...and they propagated their energy and phase predominantly eastward in the intrinsic frame...." These extra words are needed, because elsewhere you say these GWs propagate energy and phase predominantly *westward* in the ground-based frame.
**L4**: momentum density...
**L11**: replace "trace" with "locate."
**L22**: again, "cooling rates" here can be misinterpreted.

**Page 11401**
The navy blue line here is very difficult to distinguish from the black Florida coastline.

**Page 11406**
Since only 136 samples are available, and results in Figures 5-7 and 12 are split into anywhere from 15 to 30 separate bins, the bin statistics cannot be very robust. It would be helpful if some standard deviations based on Poisson counting statistics (i.e. uncertainties equal to $\sqrt{J}$ where $J$ is the number of samples in a bin) were given so readers can judge the statistical significance of shape or peak features in a given histogram.

**Page 11412**
The "1" in the $x$-axis label looks like a typo and should be removed.

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 11377, 2005.