Response to RC1: 'Comment on angeo-2024-6', Anonymous Referee #1, 17 Jul 2024

The topic is important, and some interesting cases were chosen. However, there are several incorrect assumptions made. Most importantly, the assumption that equatorward propagating TIDs are generated by geomagnetic sources is not in line with peer reviewed literature and existing scientific knowledge regarding GWs/corresponding TIDs.

Reply 1: *We disagree. What does the reviewer imply by this statement? In Introduction we have sufficiently referenced papers back to 1960s and 1970s that demonstrated equatorward propagation of aurorally generated GWs/TIDs. For example, review by Hocke (1996) already discussed it in great length. This is also in line with our most recent paper (Prikryl et al., 2022) and many papers referenced there. Yes, recently, it was shown that polar vortex generates GWs that can also propagate equatorward, and we referenced paper by Frissell et al. (2016). However, this does not replace all the extensive previous work on GWs/TIDs from auroral sources, which in this paper we show can be further poleward than any ground magnetometers that could detect the geomagnetic activity associated with ionospheric currents*

Furthermore, only a cursory analysis was provided for several cases. In some instances, arrows do not correspond to events that are appropriately matched in time, or arrows point to data that does not demonstrate anything.

Reply 2: *Of course, the arrows in Fig. 2 do not show exact correspondence between the IMF clock angle and TIDs. The IMF observed by ACE does not represent exactly the IMF impacting the dayside magnetopause. It would require at least a spacecraft in front of the bow shock to monitor the IMF (e.g., Prikryl et al. 2002). But the observations of pulsed ionospheric flows and corresponding TIDs provide sufficient evidence that points to sources of these TIDs in the high-latitude ionosphere. Such analysis that the reviewer chooses to call "cursory", provides clear evidence for sources of equatorward propagation TIDs (Prikryl et al., 2022).*

Additionally, improper use of citations is present throughout the manuscript. Given these issues, I cannot recommend this manuscript for publication.

Reply 3: *Although citations of conference papers are not improper, and the AGU provides the citation form for the conference presentations, we will remove all these references in the revised manuscript. The authors of these presentations have not published their results.*

In terms of lack of rigor in the analysis, the authors do not do a comprehensive lower atmosphere analysis for Jan 8 as was done for the Nov 2014 case (a brief mention of a figure for Jan 8 is included in the supplemental material). That said, the interpretation of lower atmospheric data is problematic for the Nov 2014 case. Dotted lines are drawn to denote regions of GW generation, yet ERA 5 shows widespread GWs in the troposphere. No analysis has been performed for GWs propagation in the stratosphere (this would be difficult to assess with ERA5). However, during times of the polar vortex, upper stratospheric conditions play a significant role in GW filtering, generation, and coupling into the thermosphere.

Reply 4: *There is no need for a lower atmosphere analysis for January 8, 2013, because the TID sources are shown to be in the ionosphere. For the lower atmosphere, the sources of the observed TIDs are associated with intensifying extratropical cyclones and we pointed to likely process generating the GWs by referring to works by Uccellini and Koch (1987), Koch and Dorian (1988), and Plougonven and Zhang*

(2014). We used ERA5 reanalysis data for the 150-hPa level to support it, similarly to what the latter authors have done. While whole atmosphere modeling could be used to further assess the circumstances at play, such modeling is beyond the scope of the present study, and we believe that the use of an existing peer-reviewed approach is sufficient at this time.

In terms of citations, the manuscript cites several conference abstracts multiple times, in some cases appearing to disagree with the presenters. However, there is no publicly available link to the corresponding talks. Most concerning, peer reviewed publications associated with/building off the cited conference talks were NOT cited. For example, this manuscript attempts to demonstrate that TIDs observed on Jan 8-9 were from geomagnetic sources, cites Bossert et al., 2021 multiple times (an AGU conference abstract, no publicly available talk) to state that Bossert et al said the TIDs are from the polar vortex, but does not give any greater context of the authors' findings. Yet, Bossert et al., 2022 has already demonstrated quiet-time/low Kp index TIDs that were likely generated by geomagnetic sources, and this work is not acknowledged. Similarly, a Becker et al EGU conference abstract is cited, but no Becker papers are referenced in the manuscript. This lack of proper citation of the relevant literature is unacceptable for a scientific paper.

Generally, the manuscript lacks adequate citations of current research regarding GW propagation and multi-step coupling from the lower to upper atmosphere, yet attempts to demonstrate one such observation of TIDs from the lower atmosphere on this topic (e.g. the Nov 2014 case).

Reply 5: *We removed all references to conference abstracts. References to more recent publication of GWs generated by polar vortex will be added and discussed in the revised manuscript.*

Section 3.1: There is a somewhat cursory approach used here to justify that the TIDs originated from solar wind forcing. It is stated on lines 208-210 *"The clock angle controls the reconnection rate at the magnetopause (Milan et al., 2012). The TIDs can be approximately associated with the southward IMF turnings (positive deflections of the clock angle values towards 180 degrees marked by arrows in Fig. 2)."*

First, is it only the clock angle? Or is it also IMF? Is there a threshold at which Bz or By need to be perturbed in order for a TID to form? The perturbations are only a few nT at their strongest. Is that significant? Is there a modeling study that could be cited?

Reply 6: *The clock angle IS the IMF. Yes, the IMF perturbations of a few nT are sufficient to modulate the magnetic reconnection, resulting in PIFs/TIDs (e.g., Prikryl et al., 2002, 2005).*

Will the TIDs only form at a specific location or are they expected to form all over the polar cap and auroral oval? The ACE data could be applied to quite a range of areas in the auroral region, but localized effects can vary drastically.

Reply 7: *All these TIDs were generated by dayside sources of GWs in the cusp (ionospheric footprint of the magnetospheric cusp).*

It would appear that Figure 2 has hand drawn arrows pointing from the power fluctuations observed in PGR to the ACE clock angle plot. These arrows are not necessarily correlated. Some are drawn directly upwards, some are drawn at a slant in order to match some of the power fluctuations with perceived spikes in the ACE data. Some of the arrows (e.g. one drawn just after 22UT) doesn't even appear to correspond to any fluctuation in the IMF clock angle at all. Some of the larger power fluctuations have an arrow pointing to a very tiny change in the IMF clock angle. There appears to be no obvious correlation here, and no quantitative analysis was performed. This is not enough to demonstrate a link between TIDs present and geomagnetic activity.

See Reply 2. *Also, we are not demonstrating a link between the observed TIDs and geomagnetic activity (Kp index or ground magnetic field), rather, fluctuations in the IMF modulating ionospheric flows, which are observed by the King Salmon radar. Regarding the arrows, they are drawn to point to clock angle deflections towards 180°. Sure, some of them are subtle but as we pointed out ACE does not necessarily observe the IMF impacting the magnetopause. Unfortunately, there are few spacecraft in the upstream solar wind and none in the magnetosheath.*

Furthermore, there isn't a baseline for comparison of just how strong the ACE perturbations need to be, and what sort of TIDs and corresponding amplitudes would be generated. Prikyl 2022 is cited, but again, the data shown there are overplotting ACE data, and there is not a quantitative analysis performed. That case seemed to have a better match at least with the perturbations. So, it is difficult to compare the case presented in this paper with hand drawn arrows. Are there no other datasets that can be used to "provide evidence the observed TIDs could have originated from the magnetosphere/solar wind forcing" as is mentioned in line 212?

An additional question would be whether the TIDs that are assumed to be associated with southward IMF turnings have a specific mechanism of generation? Are these related to gravity waves in any way? What is the expectation for TIDs being generated through "magnetosphere/solar wind forcing" (discussed on lines 2012-2013)? The author cites an AGU talk from 2021. While there does not appear to be a readily available copy online, was part of the Bossert et al 2021 argument that the observed TIDs/TADs on this date followed gravity wave polarization relations? Would this also be expected for cases of TIDs originating from magnetosphere/solar wind forcing? The authors will need to provide adequate references on this and more detail describing the mechanism of TID generation as well as the specific location with regards to ionospheric convection/currents. How close does this need to be to the region of observed TIDs?

Reply 8: *The mechanisms for energy transfer to the thermosphere can be Joule heating, precipitation, or simply ion drag by swings in convection (PIFs) generating GWs. The GWs modulated ionospheric densities – TIDs that are observed by SuperDARN radars. For simple ray tracing of GWs from ionospheric sources that eventually cause TIDs, see, e.g. Prikryl et al. (2005).*

Regarding section 3.2:

Figure 5: Again, magnetometer perturbations are shown plotted above radar power fluctuations and LOS velocities. There is no context for geographic location. One single magnetometer is used. It is also not clear from the plots that the magnetometer perturbations correspond to fluctuations observed in the radar power or LOS velocities.

The text bounces back to figure 4: *"For the first event, Figs/ 4b and 4c show the FFT spectra of detrended time series of IMF Bz, solar velocity Vz, and NAL X-component, and the Hankasalmi radar ground scatter power displaying peaks at similar frequencies/periods."*

It is not surprising that the ACE data (Bz and Vz) are showing similar periods. The NAL X and radar are an interesting comparison, but this needs to be discussed more. The only information given is "beam 11, gate 25, slant range 1305km)" is being used. Why just one altitude? Why just one beam? Looking at Figure 5b, the previous statement seems a bit of an incomplete story as to what is going on. There are clear perturbations in the power near 1305km from 8-11UT, but not necessarily perturbations in the NAL X magnetometer data. Then, from 12-16UT, there are shorter period perturbations, and these do not appear to correlate with NAL X perturbations during 12-16UT. So, the analysis used does not seem to be appropriately describing the data.

Reply 9: *Similar periods are evidence that the solar wind Alfven waves modulated the PIFs, associated currents and TIDs. Regarding magnetometer data we will add 1D equivalent currents estimates that use all IMAGE magnetometers in Figures 5 and 6.*

Line 247-248: "The equatorward TIDs were observed at least down to latitude of 50N."

How was the propagation direction determined in this case?

Reply 10: *Animations of TEC maps showing equatorward propagation of TIDs are provided in the Supplement material.*

-The data presented in section 3.2 (e.g. Fig 9 b) may be more in line with what has been observed with LSTIDs and aurora (e.g. Zhang et al., 2019 **https://doi.org/10.1029/2019JA026585**). That said, further discussion regarding how these are classified as LSTIDs (what is the horizontal wavelength and the period) should be included. Also, a discussion of how the propagation direction was determined needs to be included.

"In summary, the cases discussed in sections 3.1 and 3.2 highlight the importance of solar wind coupling to the MIT system, particularly on the dayside, in the generation of AGWs/TIDs." Lines 282-283.

Section 3.1 has not provided adequate analysis to justify this statement, though the notion is important for further discussion. Section 3.2 presents a case similar to those which have previously been presented for LSTIDs generated from aurora.

Reply 11: *PIFs and ground magnetic field fluctuations are all observed in the ionospheric cusp in the noon sector and are followed by TIDs. This is sufficient evidence they are the result of solar wind-MIT coupling. TIDs with wavelength greater than 1000 km are classified as LSTIDs (Hunsucker (1982).*

Section 4

Line 292: "At high latitudes, they propagate predominately equatorward suggesting likely auroral sources." This is not scientifically accurate or in line with gravity wave theory. Studies have demonstrated GWs generated from lower altitudes can propagate equatorward (for example Becker et al., 2022 https://doi.org/10.1029/2022JA030866).

Reply 12: *Well, this study shows they originated in the cusp (see, Reply 8-9) through direct coupling of solar wind to dayside magnetosphere. This is not the same as nightside auroral sources, which are due to impulsive electrojet intensifications, for example during auroral substorms.*

Line 293 "At mid to low latitudes, the azimuth of MSTIDs varies suggesting sources in the troposphere." Again, there is no scientific basis for this statement. It also doesn't suggest tropospheric sources over other atmospheric sources.

Lines 296-300: This section needs clarification, specifically the statement “with southeastward propagation more likely in winter months, suggesting that cold season low pressure systems in the northeast Atlantic are sources of the GWs.” Is this inferred for the current research or cited research from Chum et la., 2021. Chum et al., 2021 states the higher likelihood of southeast propagation in the winter months, but does not make the inferred connection to low pressure systems in the Atlantic. In that case, GWs in the ionosphere were found to propagate against background winds.

Reply 13: *This can be inferred from the cited research by Chum et la., 2021. But the eastward propagation is a clear indication the sources are not auroral. Presence of intensifying extratropical cyclones suggests a tropospheric source.*

Lines 309-320 (and Figs 11 and 12): Please provide more information about the analysis performed here. What are the errors associated with the retrieved periods and propagation azimuths. Is a particular analysis method being followed? Please provide a reference here.

Reply 14: *Chum et la., 2021*.

Line 325-327: “During the period from November 1 to 8, 2014, we distinguish between aurorally-generated TIDs propagating equatorward from high latitudes (Section 3.2) and south-eastward propagating MSTIDs at mid latitudes by observed origin location.”

Again, this method cannot actually determine whether a wave is aurorally generated. Any wave generated in the lower atmosphere can propagate equatorward. This is especially true for gravity waves in the presence of the diurnal tide on the dayside.

Reply 15: *As we already stated in the above replies, we show evidence of TIDs propagating equatorward from sources in the cusp.*

Line 328-330: “Low-pressure systems deepening over the Northeast Atlantic shown in the surface pressure analysis charts, were likely sources of MSTIDs propagating eastward to southeastward.”

It is certainly possible that this is a source of GWs that relate to MSTIDs. However, there is no other analysis provided here that would further justify this assertion.

Reply 16: *Provided in Section 4.2*

Line 332-333: “At the same time, the vTEC maps on both days also reveal equatorward propagating TIDs at latitudes down to 50N that originated in the cusp ionospheric footprint”

Again, “equatorward propagating” does not definitively prove TIDs originating from auroral regions.

Reply 17: *Same as Reply 15.*

Regarding Fig 13 a,b and Lines 332-334*: “At the same time, the vTEC maps on both days also reveal equatorward propagating TIDs at latitudes down to ~50N that originated in the cusp ionospheric footprint over Svalbard, as already discussed in Section 3.2.”*

While section 3.2 provided data from Nov 1 and Nov 5, no analysis was performed for Nov 5. For Nov 1, the NAL X perturbations do not necessarily correspond to the perturbations in the radar power, and no adequate description of the formation of GW-like TID features has been provided. While it is possible the TIDs are linked to geomagnetic activity on this day (Nov 5), this is a hypothesis at this point. The datasets shown in 3.2 certainly don't prove that TIDs on different days, such as Nov 8, originated near the cusp. What is concerning about this instance on Nov 8, is that there is a notable polar vortex globally, which can be a source of GWs in the stratosphere (see publicly available MERRA2 data, demonstrates the polar vortex and strong wind shears in the stratosphere at the beginning of November). Additionally, there are now studies demonstrating GW generation from the northern side of the polar vortex and propagation across the polar region (e.g. see Becker et al, 2022 doi:10.1029/2022JA030866 for example Fig 6e,f), which results in TIDs/TADs from GWs in the thermosphere.

Reply 18: *This study shows they originated in the cusp (see, Reply 8-9) through direct coupling of solar wind to dayside magnetosphere.*

Section 4.2:

This is interesting discussion. However, a quantitative analysis is again lacking. In Fig 15, was the axis of inflection drawn on, or was this calculated? How was this determined? More references are needed here to back this up. Additionally, one can look at satellite data for this time period and see that GWs are all over the stratosphere around the region of the polar vortex and not just at the dotted line region drawn in Fig 15a. The divergence of the horizontal wind at 150-hPa can certainly give an idea of where GWs are present in ERA5 output, and in the cases presented, they are quite widespread. It is also odd that Nov 8 ERA data are used to try and describe TIDs observed on Nov 8. It would take GWs some period of time to propagate from the troposphere to the thermosphere. 11 hours is on the shorter side of assumptions for slow moving GWs generated from jets. It would also be appropriate to use data from Nov 7 as well.

Lines 390-397: "Bossert et al. (2021) observed GWs generated by stratospheric vortex. There was an extratropical cyclone intensifying just south-west of Alaska. Using the ERA5 reanalysis, similar to Figs. 15e,f, north-eastward propagating GWs in the stratosphere are found (Fig. S7 in the Supplement) but no corresponding TIDs can be resolved in the detrended TEC maps, possibly because of sparce coverage by GNSS receivers. However, mesoscale GWs propagating eastward and upward into the stratosphere generated by geostrophic adjustment processes and shear instability may be common and could be driving MSTIDs."

Unfortunately, this is not a thorough investigation. Further instrumentation would need to be included to assess GWs in the stratosphere, as ERA5 does not necessarily capture dynamics on the top side of the polar vortex in the upper stratosphere/mesosphere. A quick check online from AIRS Jan 8 and 9 2013 (https://datapub.fz-juelich.de/slcs/airs/gravity_waves/html/view_2013_009.html) also shows significant poleward GW activity in the stratosphere north of AK, and these breaking GWs could produce secondary GWs that propagate equatorward. Again, a conference talk is being cited, so it is hard to tell what is being referenced here. That said, the plots provided in the manuscript have not provided definitive evidence that the TIDs are indeed geomagnetically induced as opposed to driven from the lower atmosphere.

Finally, I will note here that the citations of conference talks are not appropriate. Especially considering the talks themselves are not publicly available. The authors and coauthors cited, Becker et al, and Bossert et al, have published work in recent years that likely builds on previous conference presentations. These citations should be included in this paper. Interestingly, the work here builds off what has been written in Becker et al., 2022, Vadas et al., 2022, and Bossert et al., 2022. Becker et al., 2022 (https://doi.org/10.1029/2021JD035018) discusses methods for assessing vortex generated GWs from model output. It is the combination of MKS and MPS as calculated by a model that determine the regions of jet generated GWs. Additionally, this paper shows examples of over pole GW propagation (e.g. see Figure 3a). Vadas et al., 2022 discussed observations of polar vortex generated GWs and subsequent secondary GW generation in the polar region (https://doi.org/10.1029/2022JD036985). Bossert et al., 2022 (https://doi.org/10.1029/2022GL099901) discussed a strong TID/TAD event observed during an SSW (a few days after the Jan 2013 date discussed here) and suggested that the large-scale TID/TAD was related to geomagnetic activity despite a low Kp. This is not a new concept, though it is important to furthering the discussion of waves and variability in the thermosphere.

So, a more thorough discussion and consideration of current work should have been addressed in this manuscript.

Reply 19: *We will acknowledge and reference these works but to undertake such approach is beyond the scope of this study.*