Anonymous Referee #1: Interactive comment on Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2019-57, 2019. (Received and published: 31 May 2019)
Response to referee 1:

16th August 2019

We would like to thank the reviewers for their time taken to read and feedback very helpful advice and comments on our paper. Our responses are below.

We have added an extra figure (Figure 3) to illustrate the geometry of the CHAMP ascending and descending orbits, and the projection of the FPI wind vectors onto the CHAMP cross-track direction.

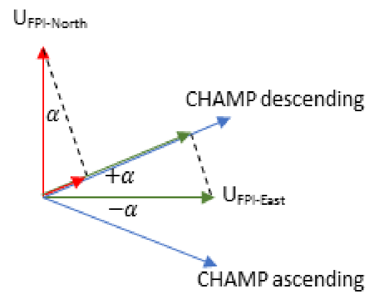

Figure 3: Geometry illustrating the projection of FPI look direction wind components onto the CHAMP cross-track direction for the ascending and descending tracks.

This is in response to both referees requesting that we do this projection for a fairer comparison with the CHAMP cross-track winds. This has required a renumbering of the figures as shown in the table below:

Original Figure Number	New Figure Number
1 CHAMP solar max 2001-2003 Longyearbyen	1 CHAMP solar max 2001-2003 Longyearbyen
2 CHAMP solar max 2001-2003 Kiruna	2 CHAMP solar max 2001-2003 Kiruna
3 CHAMP solar max 2005-2007 Longyearbyen	3 geometry for projecting FPI winds onto CHAMP cross-track direction
4 CHAMP solar max 2005-2007 Kiruna	4 CHAMP solar max 2005-2007 Longyearbyen
5 FPI solar max and min 2001-2003 Longyearbyen	5 CHAMP solar max 2005-2007 Kiruna
6 FPI solar max and min 2001-2003 Kiruna	6 FPI solar max and min 2001-2003 Longyearbyen
7 CHAMP vs HWM87 and HWM90 and FPIs measurements made by UCL + Alaska (1980)	7 FPI solar max and min 2001-2003 Kiruna
8 CHAMP vs FPI for 2- <= Kp < 4+ for Longyearbyen and Kiruna	8 CHAMP vs HWM93 and FPIs measurements made by UCL + Alaska (1980)
9 frequency distribution of CHAMP/FPI for solar max and both Longyearbyen and Kiruna	9 CHAMP vs FPI for 2- <= Kp < 4+ for Longyearbyen and Kiruna
10 CMAT2 model demonstration of effects of changing viscosity	10 frequency distribution of CHAMP/FPI for solar max and both Longyearbyen and Kiruna

11 height profiles of CMAT2 model zonal winds and comparison with the red line emission intensity profile.	11 CMAT2 model demonstration of effects of changing viscosity
12 frequency distribution of Kp	12 height profiles of CMAT2 model zonal winds and comparison with the red line emission intensity profile.
13 CMAT2 zonally averaged winds at Longyearbyen and Kiruna	13 frequency distribution of Kp
14 global maps of CMAT2 zonal winds comparing winds at 240 km with height integrated winds	14 CMAT2 zonally averaged winds at Longyearbyen and Kiruna
15	15 global maps of CMAT2 zonal winds comparing winds at 240 km with height integrated winds

Responding to Ref 1 comment 2 has also meant a re-numbering of the remaining equations:

Original Equation Number	New Equation Number
1	deleted
2	deleted
3	deleted
4	**1**
5	**2**
6	**5** (corrected original number which was out of order)
7	**3**
8	**4**

Some additional points we noted:

We noted that Figure 12 required some more explanation of the simulations of the height profile of the zonal winds (lines 633-645):

"Figure 12 illustrates how ground-based FPIs make measurements of the neutral winds at 240 km altitude. The left plot shows a height profile of the CMAT2 zonal mean zonal winds at the latitude of Longyearbyen. There are 6 simulations to demonstrate the effect on the height profile of the zonal mean zonal winds when changing the viscosity. CMAT2 uses a viscosity term that is the weighted mean divided by the scale height of two coefficients of viscosity: the molecular viscosity μ_m; and the turbulent viscosity μ_t. The simulations represent a comparison with the original molecular viscosity (dark blue). The other lines are for low (yellow - divided by 100) and high molecular viscosities (pink - doubled). The low and high turbulent viscosities are represented by the Prandtl numbers 0.7 (red) and 100 (green), where 2 is the default value used in CMAT2; which is relevant for the height at which gravity waves deposit momentum (Liu et al., 2013). The light blue line labelled "Mata" is an intermediate profile. As can be seen, the molecular viscosity dominates in the thermosphere above 100 km and at the altitudes where the FPI is measuring. The dark blue and yellow lines are representative of a vertical slice of Figure 11 left and right, respectively, for the latitude of Longyearbyen."

Replaced Nov-Jan with DOY 300-65 in abstract and line 424. This is the correct range of DOY used in the FPI selection criteria, as well as the CHAMP data.

Added the following:

i) an extra affiliation for MF
ii) data availability
iii) Co-author Rosie Hood's recently awarded PhD thesis as a reference

Moved and consolidated description of red line emission profile and winds with respect to height to follow Figure 12.

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Dear Editor, I have reviewed the paper by Aruliah et al. titled "Comparing high-latitude thermospheric winds from FPI and CHAMP accelerometer measurements." The paper describes polar observations from FPIs and the CHAMP satellite. Observations from different instruments are compared and discrepancies discussed. It is important to the community to understand the differences between the FPI and CHAMP observed thermospheric winds. CHAMP winds are known to be larger than the FPI measurements.
While the cause for such discrepancy may be unknown at the moment, at least, we should know how the two sets data are different. Hence, the paper is very important and should be considered for publication. It can be a good reference for future users of the two data sets. The paper in the current form, however, has some significant issues need to be addressed.

1. The paper somehow lost focus. It got into too many sub-topics: FPI (old) -FPI (new) differences, HWM87-HWM90 differences. I think for the purpose of understanding the CHAMP and FPI difference, we should avoid using very old data. If the topic were the long-term trend, then we should examine long data string. The value of this paper is on the CHAMP and FPI comparison.

We beg to differ on this comment. It is important to provide a context and full argument for this important finding, i.e. that there is a serious discrepancy between the two methods of measuring thermospheric winds; but that the UCL FPI results are consistent with the U.Alaska FPI. Then the FPI procedure is discussed in section 6.2. However, we have added a sentence (lines 480-481): "These are interannual and inter-solar cycle discussions for a later paper."

The HWM87 and HWM90 plots have been replaced with a single plot using the later version HWM93.

The calibration of FPIs is an important section attempting to understand the discrepancy in the CHAMP-FPI winds. The University of Alaska FPI measurements at Longyearbyen, made in the early 1980s, are used to show that their FPI measurements are consistent in phase with the UCL FPI measurements 20 years later. They are also of a magnitude closer to the UCL FPI than the CHAMP winds. Even then Hedin et al. (1991) noted that the U.Alaska ground-based FPI zonal wind magnitudes in 1980 showed smaller magnitudes than satellites.

2. The section L67-89 has some major issues. The thermospheric dynamics is governed by the momentum, energy, and continuity equations. It cannot be expressed in the formulas listed in the section. CHAMP's larger winds do not necessarily lead to

larger temperatures. Smaller FPI winds are not always connected to the lower temperatures. We cannot use temperature to verify wind values.

Agreed that this is a very simplified argument, but the purpose is to highlight the repercussions of overestimating the neutral wind when dividing energy between heating and acceleration of the neutral gas. But considering the referee's concerns, we have decided to remove lines 67-92 and replace with a general comment on the partitioning of energy (lines 65-71):

"With incorrect scaling, there arises a problem of distortion of energy budget calculations of the upper atmosphere. A precise estimation of energy supply to the system is hindered essentially, because the partitioning of kinetic and thermal energy channels becomes obscured. The acceleration of the neutral air in 3-D space with respect to the active driver of the plasma motion is important to estimate, for instance, the Joule heating rate as one of the most important thermal energy inputs. This has a knock-on effect on the calculation of the absolute density of the gas, which is an important parameter used in, for example, satellite orbit calculations."

Removal of these paragraphs and 3 equations has required re-numbering of the remaining equations as outlined in the table above.

3. I am lost in the section L279 – L316. I understand that the CHAMP cross-track winds are not aligned with the eastward direction on the ground. The angle is different for the ascending and descending nodes. The angles are +/- 7.2 deg and +/-13.3 deg for KEOPS and Longyearbyen (ascending and descending). So the obvious thing to do is to compare separately the ascending and descending nodes. Use the ground based meridional and zonal components to compute the wind value along the direction of CHAMP cross-track measurement (mostly to add the contribution from the meridional winds). I don't understand why that was not done. I do not see an argument to let me believe that I can ignore the viewing angle difference between the ground based zonal wind and CHAMP cross track winds. So I suggest the authors use the ground based FPI data to form the wind values along the CHAMP cross-track direction to do a direct comparison. Or alternatively, using the CHAMP ascending and descending node measurements to remove the contribution from the meridional wind and compute the zonal wind. Hopefully, that would give you better comparison between the FPI and CHAMP observations.

The FPI vector winds have been projected on the cross-track direction for the ascending (East - alpha) and descending (East + alpha) as suggested. The discrepancy in magnitude is still large. An extra Figure (new Fig 3) has been added to illustrate the geometry.

4. Figure 7 should be the focus of the paper. I really don't see much value having two HWM model runs results shown here. You only need one. The FPI (Alaska) probably does not add much and more likes a distraction. The old and new FPI comparison should be discussed in a different paper, you can have inter-annual variations here.

The two HWM models have been replaced with the later HWM93 model. The FPI (Alaska) data remains because it shows that the UCL FPI data are consistent with the Alaska FPI under fairly similar f10.7 and mean Kp. Indeed Hedin et al (1991) had noticed a systematic discrepancy between satellite and ground-based FPI winds even then.

5. The solar minimum data are not very useful since there are no CHAMP observations.

The original Figs 3 and 4 (now called Figs 4 and 5) show CHAMP data from solar minimum.

6. I think the conclusion should be clearer that the CHAMP winds are overestimated. As the paper points out that the CHAMP winds are almost the same magnitude as ion drift at Kiruna, which is incorrect.

Agreed. This point is discussed in Section 6 for various aspects such as the role of viscous action in the upper thermosphere (Sect. 6.1) or the ion-neutral comparisons (Sect. 6.4). To accentuate this set of problems, we emphasized this with a expanded paragraph referring to the Fiori et al (2016) Swarm measurements of ion velocities (lines 767-774), and two additional sentences at the end of the Conclusions (lines 834-837). This is a key point for further discussions and also for more modelling/simulation studies (lines 837-839).

"Fiori et al (2016) compared ion velocities measured by the Electric Field Instrument on Swarm with the CS10 statistical ionospheric convection model by Cousins and Shepherd (2010) which is based on 8 years of data (1998-2005) collected by 16 SuperDARN coherent scatter radars. The climatology represented by the CS10 model in Fiori et al's Figure 3a indicates speeds in the few hundreds of ms^{-1}, while the instantaneous values along the Swarm satellite pass (their Figure 3d) show much stronger drift peak values on the resolution level of seconds or shorter. Even after allowing for offsets, their 1-sec resolution corrected cross-track ion drifts achieve horizontal velocities well over 1,000 ms^{-1}, which probably indicates the highly dynamic behaviour in the auroral regions compared with quasi-stable conditions used for empirical models. However, recently Koustov et al. (2019) compared the Swarm cross-track ion drifts with the SuperDARN radar network and found that the Swarm ion velocities are a factor of 1.5 larger. They suggest reasons for the disparity, including refining the calibration of Swarm and the differences in spatial/temporal resolution."

"We may also need to rethink the procedure of comparing different spatial and temporal resolutions of in-situ satellite versus remote ground-based FPI measurements in terms of the geometry of cross-track winds at high latitudes."

7. While Aruhiah et al. 2005 reference is listed for the instrument information. It will be a great help to give a short paragraph on the two FPIs (gaps, aperture size, detector, from which year to which years) given that instrument upgraded over the years.

Added to section 5.2.

Minor

1. In the comparison section, the FPI (Alaska) was used, but there is no mention of it in the abstract and earlier instrument description. It should be added to the abstract, if it is to be used. Personally, I think the data should be dropped.

The U.Alaska FPI measurements at Longyearbyen illustrate that the UCL FPI measurements 20 years later are consistent. They are a reference measurement, so do not need to appear in the abstract.

2. Many the figures have very low resolution and are difficult to read.

Figures 5-8 have been increased in size on the page. In particular, Figs 5 and 6 have been split so that a,b and c,d are on 2 separate pages. (These are now re-labelled Figs 6-9)

3. Figure 5, the FPI from one direction has error bars and the other does not. Why?

Only one set of error bars were included for the sake of clarity of the plots. However, we have now included the error bars for all the FPI look directions.

4. L 444 to LL448. 'During the 1980s and 1990s we used state-of-the-art UCL designed and built Imaging Photon Detectors (McWhirter et al., 1981) and then EMCCDs (Andor iXon 887/885) were installed around 2005 (McWhirter, 2008). The revolution over the last 30 years in : : :' Was the UCL FPI running with the imaging photon detector from 2001 to 2003, it is not clear in the paper. That is why I ask for a more detailed instrument description to be added.

We have added some more explanation in section 5.2, in particular the dates of changes of etalon. The etalon gap is important in the calibration of the measured Doppler shift to wind calculation, which is described in section 6.2. The effect of the detector is to improve the sensitivity to the photon counting by electon multiplying. This reduces the error of measurement. It does not change the calibration of the wind speeds (lines 445-459).

"During the 1980s and 1990s we used state-of-the-art UCL designed and built Imaging Photon Detectors (McWhirter et al., 1982). Astrocam Antares cameras replaced the IPD in the Svalbard FPI from 1998, and in the KEOPS 630 nm FPI in 2002. However, these cameras had the disadvantage of slow readout times which were essential for the best noise performance and so time resolution was compromised. In 2003 the first Electron Multiplying CCDs revolutionised low light level imaging. These cameras combined superior signal to noise ratio with very fast readout times. The first one was put into service at KEOPS in 2003, followed by Svalbard in 2005 (McWhirter, 2008). The huge advancement over the last 30 years in low light detectors has allowed atmospheric gravity wave observations using exposure times as little as 10 seconds at auroral latitudes (Ford et al., 2007). Note that the upgrade of the detector is to improve the photon sensitivity which reduces the error of measurement. It does not change the calibration of the wind speeds.

Any changes of etalon required re-calibration of the measured Doppler shift to calculate winds, as discussed in section 6.2. The KEOPS FPI used a 10 mm etalon gap up to January 2002, when it was replaced with an 18.5 mm gap etalon. Then in January 2003 a 14 mm etalon was put in, which has been there until the present time. For the Longyearbyen FPI there was a 14 mm etalon until April 2005, which was replaced with an 18.5 mm etalon from September 2005 until the present time."

5. I do not see Harris 2001 paper (L554) in the reference. Or the reference date is wrong? It is not 2017, should it be 2001?

Yes, my typo mistake. The reference is to the Harris PhD thesis in 2001.

6. Equ. 5 is wrong. It does not match the two references.

The coefficient of viscosity is based on Dalgarno and Smith (1962), where it is given as

viscosity = $3.34 \times T^{0.71}$ micropoise

Equation 5 is the conversion to SI units. The Banks and Kockarts reference is removed.

7. There should be some discussion on hypothesis C (L520).

Hypothesis C – the assumptions of the FPI and CHAMP measurement techniques are discussed in sections 6.1-6.3. A sentence has been added to make this explicit.

8. Why is the HWM model run not included in Figure 8 comparison for Kiruna?

The HWM93 model has been run for both Longyearbyen and Kiruna and appears in the renumbered Figs 8 and 9.

We have removed old Fig 8a (Longyearbyen 15 min averages) and instead the renumbered Fig 9 shows only the Kiruna zonal winds as 1 hour averages, to match the Longyearbyen 1 hour averages shown in the renumbered Fig 8.