Response to Reviewer's comments on our revised manuscript:

We thank the reviewer for the useful comments. Below, we give our response to the reviewer's comments. Comments are given between double backslashes followed by our responses.
(i.e. \\ reviewer comments\\
Our response)

\\ After addressing two rounds of review comments, authors have improved the quality of this paper. The lidar-observed additional Na peak below the main layer peak and the OH-imager-observed mesospheric frontal systems are interesting phenomena. Authors explained the observations using downward transport of Na species, H, and O by the mesospheric bores as well as with the enhanced temperatures, which is quite reasonable. The data and explanation may inspire future modeling and observations. However, there are still some issues with the paper on three main aspects: 1) Authors still have some misunderstanding of the thermosphere-ionosphere metal layers versus the sporadic Na layers. 2) The vertical wind data have significant bias, which is not reasonable. 3) Horizontal advection should be mentioned as another possibility to explain part of the observations. There are also numerous grammar issues. Therefore, I would like to recommend the paper for publication in ACP after authors address the following comments that go by page numbers:\\

We thank the reviewer for the review comments and positive opening remark. We address the comments below and made necessary modifications to the manuscript based on the review comments.

\\1) Page 1, line 15: Change "This would have liberated …" to "**Both factors** would have liberated …"\\

Modified as suggested

\\ 2) Page 2, line 30: Change "They form due to the wind shears collecting …" to "They form due to the **wind shear mechanisms** collecting …". Note that wind shears themselves cannot accumulate ions or atoms, but it is the wind shear mechanisms via $V \times B$ to collect ions.\\

As per suggestion changed to 'They form due to the wind shear mechanism producing ion convergence in a narrow altitude range'.

\\ 3) Page 2, lines 32-39: **Authors should move Collins et al. (1996) reference from line 38 to line 32**; that is, it is a reference for sporadic Na layers (SSLs) but not a reference for the thermosphere-ionosphere metal (TIMt) layers. It is necessary to recognize that Collins et al. (1996) paper reported high-altitude sporadic Na layers, which were NOT the TIMt layers; therefore, this paper should be moved to line 32 along with Cox and Plane (1998) etc., and removed from line 38 (Chu et al., 2011; etc.). For authors information, the high-altitude SSLs reported in Collins et al. (1996) are very similar to the sporadic Fe layers around 110 km from 18 UT to 21 UT in Figure 1 of Chu et al. (2011), but they are very different from the thermospheric metal layers reported by Chu et al. (2011), Wang et al. (2012), etc. Authors should pay more attention in referencing proper papers at proper places.\\

The reference is moved to line 34 along with Cox and Plane, 1998 etc.

\\ 4) Section 2 "Data used" – this section is very long while still not informative enough. For example, it is unclear what resolutions were used in the temperature data retrieval. Can authors use a table to tabulate related information but shorten the section text? Descriptions on how lidar data were retrieved and how OH images were analyzed are quite lengthy but

aren't they standard procedures? Anything new authors developed? If not new things, why don't you reference some papers and then shorten the description?
If authors feel strong to tell readers how they handled the data for certain purpose, why don't you put such contents to Section 3 when related results are presented. Otherwise, it is a bit frustrating to read the lengthy Section 2 before knowing what results you got. \\

The reviewer may note that we did not discuss the lidar data retrieval in this section. Previous works are referred for the data retrieval methods (Nozawa et al., 2014 and Kawahara et al., 2017). We only mentioned the essential information like spatial and temporal resolutions of the raw data. Rest of the section describes the type of averaging we made, and elaborates on calculation of other relevant parameters like Buoyancy frequency. If any interested researcher intends to reproduce the results in future, they need to know how each step is carried out and hence we prefer to keep the current section.

However, following the comment no. 11 of the reviewer, we have removed Figure 10 of the previous version showing vertical winds and Richardson numbers. This helped in removing the part of text describing the vertical winds and calculation of Richardson numbers in section 2. Length of Section 2 is reduced due to this modification.

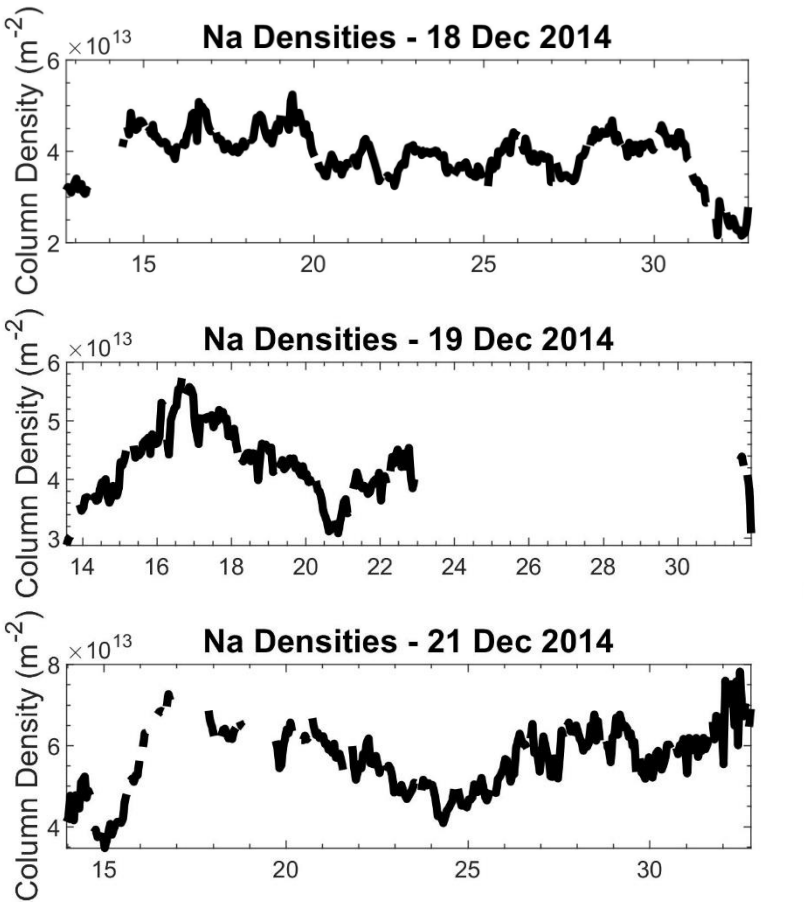
\\ 5) Page 9, line 210: change "creation of sodium atoms" to "**production** of sodium atoms" \\

Changed

\\ 6) Page 9-10, Figure 3: Authors wrote "This is further confirmed by the observation that the column abundance was reduced after the disappearance of the lower altitude sodium peak". However, the Na abundance level near the end of the observation was higher than that at the beginning of the observation, i.e., the Na layer did not return to the original state after the passing of frontal systems. Therefore, **it is necessary to show how Na column abundance changes through a normal night without mesospheric frontal systems**, which will check whether the increase of column abundance during the frontal systems is unusual when compared to a normal night. \\

The reviewer may please refer to Figure 12 (Figure 13 in the previous version) and the discussion in the lines 355 – 362. We quantify that about 65% of the column abundance increase is due to the formation of lower level peak and the remaining enhancement is due to the increased sodium concentration in the main sodium layer and its topside. We mentioned 'column abundance was reduced' since we never expect that to be the same value prior to the passage of the bores. This is because there are other processes that can bring about the sodium abundance variations. As the reviewer may well be aware of, the column abundance often varies with tidal and gravity wave variations. Since we never claimed that we identified the mesospheric fronts with the column abundance variations, we feel it is not necessary to include column abundance figure from some other normal night. Another problem is how to define 'normal night'? One day may have significantly stronger tide and another day may have an intense gravity wave and so on. Also, our understanding on how the mesospheric sodium concentrations respond to different level of auroral activity is not complete. Tromsø being auroral site, defining 'normal night' is not an easy task.

However, we attach a Figure showing the column abundances for 18, 19 and 21 December 2014 to our response (19 December being day of this study). It may be seen that other nearby days also show fluctuations in the column abundance but they did not reveal bores to our knowledge. In the present case the mentioned enhancement in column abundance coincided with formation of the lower peak which is caused by successive passage of bores. We don't include this Figure to the manuscript as we feel this is not required for the present study.



\\ 7) Page 10, line 230: Change “horizon” to “edge of the image” \\

Changed

\\ 8) Page 12, Figure 5: What is the reference point for “Distance” in the x-axis label, i.e., “Distance” from which point? \\

Please see lines 174 to 180 describing about the distance axis for extracted cross sections. The distance is from the starting point of the extracted cross section.

\\ 9) Page 13, line 263: Change “Now we discuss” to “Now we **present**”. Page 13, line 264: Change “The temperature profile” to “The temperature **contour**” \\

Both the changes are made.

\\ 10) Page 14, Figure 7: The color scales for N^2 plot are unclear – does the blue color represent negative N^2 or not? \\

The color scales are chosen after trial and error method to clearly reveal the duct location. More contours may bring confusion to the readers. The dark blue represent negative N^2. Blue does not represent negative values. Please refer to the colorscale added to the right.

\\ 11) Page 15, Figure 10: **The vertical wind data is unacceptable** because it shows a very large negative wind bias. Majority of the vertical winds are between 0 m/s and -10 m/s, which cannot be true for the real atmosphere. It appears that the Na lidar on 19 Dec 2014 exhibited

a large pulsed laser frequency offset (or frequency chirp), and authors didn't correct the frequency offset – leading to the negative bias in the results. Authors should either correct the vertical wind data or remove the vertical wind plots from the paper – the current Figure 10 top plot is unacceptable.
Also, the higher values of vertical velocities near 85 km (line 293) appear to be dominated by noise or measurement errors. Authors should be really careful in using the vertical wind data or in the interpretation. \\

We sincerely thank the reviewer for noticing this bias and pointing it out. We agree that there is a bias. We have removed Figure 10 from the revised version. The vertical winds and Richardson number estimations are not critical for explaining our results. We referred to the Richardson number only once in the discussion. Hence we removed Figure 10 following the suggestion of the reviewer and it also helps in reducing the length of the manuscript, in particular length of section 2 (See comment no. 4).

\\ 12) Page 21, Figure 14: For the integrated densities from 88-95 km, they are positively correlated with the mean temperature quite well up to 17 UT, but then the correlation becomes negative. This result makes me wondering whether some of the variations are caused by the horizontal advection of the Na layers. This factor should be mentioned on page 25 in the paragraph above line 480.¨ \\

Based on the reviewer's comment, we have added the following statement in lines 385 – 389 where we discuss Figure 14 (Figure 13 in the present version).
'On the other hand, in the presented observations after 17:15 UT and between 88 and 95 km, the sodium density variations do however not correlate with temperature. This may be either due to the horizontal advection of sodium atoms or due to the ion chemistry as this time also coincides with onset of aurora.'

\\ 13) Page 22, line 411-413: How can Na layer peak affect the formation of bores? \\

Please note that we never mentioned that Na layer peak affect the formation of bores. We meant that the enhanced strong thermal ducting occurred coincident with the region of the main sodium layer. The sentence is slightly modified to avoid confusion while reading.

\\ 14) Page 24, line 444: Change "The principle loss" to "The **principal** loss" \\

Changed.

\\ 15) Page 25, line 472: Change "…the bores have lead to …" to "…the bores have **led** to …" \\

Corrected.

We believe that we have satisfactorily addressed the comments of the reviewer. We once again thank the reviewer for the review, in particular pointing to the bias in the vertical wind.

We also thank the Editor for handling the manuscript.