I very much appreciate the time and efforts the referee spent on reviewing this paper.
In the following, referee comments are given in blue and responses in black.

Here, I try to provide my comments about this manuscript.

In general, the topic is appropriate for this journal, and I'm not aware of a similar study for the German Bight, which it makes it worth of publishing.

The used tools (PELETS in combination with surface current velocities obtained from BSH) are state-of-the-art and appropriate. Furthermore, the different quantitative measures to identify Lagrangian Coherent Structures (LCS) or objects with similar meaning are appropriate.

I must admit I'm not an expert for LCS and similar structures. However, reading the article and the theory included was very informative and helpful for me. Furthermore, using more the one quantitative measure is quite illustrative.

Major comments and questions:

- Tides are very important for this region. Might it be possible to give some idea how and if the FTLE structures will change when tides are not subtracted from the currents. So, what might happen when not using residual currents.

You are right that tidal currents are very important in the German Bight area. Accordingly, all drift simulations were conducted using the full hydrodynamic fields stored on a 15min basis. Looking at the trajectories in Fig. 1a, for instance, tidal movements can easily be identified. In addition, however, these trajectories show also the more long-term movements related to residual currents. In the paper, the term 'residual currents' is used only in the discussion, tidal currents were never separated from the total flow fields when conducting numerical simulations.

- It was very illustrative to see how variable the FTLE structures are in time. What would happen, if one considers time averaged FTLE-fields. Perhaps averaged over a season or half a year. Will there be stationary or more robust FTLE ridges visible?

I wasn't able to find any meaningful non-trivial mean fields. The reason for this is that even when similar structures occur, the sharp FTLE ridges usually reside in different locations, so that any averaging results in extremely smooth, non-informative fields.

Due to the strong influence of wind forcing, a classification with regard to wind directions would probably be the most promising approach. In the revised manuscript, each figure now contains a wind rose that summarizes the recent history of wind conditions. Winds are never constant and the recent history of wind forcing differs for each specific date. This means that FTLE structures can hardly be classified in a simple way. The only chance might be some more fuzzy classification of FTLE fields which, however, could be quite a challenge.

- Instead of showing the temperature and salinity fields, perhaps one could use the density distribution. Would this do any difference to the interpretation of the relationship between FTLE ridges and T/S fields.

Temperature and salinity were used just as simple indicators of the accumulated effects of surface divergences over some period of time. Surface temperatures have the advantage that they could even be observed by remote sensing. The problem with finding a proper colour scale was solved in the revised manuscript (see Fig. 1c therein). Density fields could also be interesting to look at, but unfortunately these fields were not be available from the archived data underlying this study.

Some minor comments and questions:

- How do you calculate the residual currents from the BSH data? The current velocities of the BSH include tides, which are important for this region. Are the results dependend on the way these residual currents are calculated?

All simulations were based on hydrodynamic fields including tidal effects. See my comments above.

- Do you think it might be helpful to plot the absolute value of the gradient of temperature or salinity instead of the pure fields. So, one could see the sharp fronts more clearly.

This might indeed be one option to solve the problem of showing small local differences in the presence of substantial changes on a larger scale. I decided, however, to skip the former Fig. 4, keeping just Fig. 4a, which is now presented as Fig. 1c. The colour scale was changed in such a way that now the structure corresponding with the FTLE and FDLD fields in Fig. 1a and 1b can well be recognized. Higher temperatures very near to the coast were discarded in the revised plot.

Some direct comments to the text:

- Line 13: Gap at the end of sentence; (2017). Stanev . . .

Corrected

- Line 143: The year of the Huntley citation is missing

Corrected

- Equation (5): It is divergence of the 2d surface velocity field, isn't it?

Yes, thanks for the hint. I changed symbol ∇ into ∇_H and mentioned 'horizontal divergence' in the text.

- Figure 2a: The endpoint of the red drifter starting at 54 30' is missing?

Has been corrected, thanks for checking the plot so carefully!

- Perhaps one could also include the river Ems to the plots.

Location of river Ems is now indicated in all figures.

- Figure 4, caption: . . . times of the FTLE fields . . . ?

Has been changed.

- Additional figure S3: The unit of salinity should be different from PSU. Perhaps no unit or g/kg

Thanks for this advice, but the salinity was removed from the revised paper, focussing on temperature distribution, which is now shown in Fig. 1c.