Page 2, Introduction, line 24: About Equation (1), note that it applies only if Bi-Maxwellian velocity distributions can be assumed. Generalized instability criterion for any velocity distribution has been discussed by Hellinger (2007). It could be mentioned that the classical bi-Maxwellian approximation is usually relevant for the plasma conditions here.

The referee is correct that the bi-Maxwellian approach is usually appropriate for situations as discussed in the current paper, and thus the widely used instability criterion is given in the text. We have added a small comment that a more general approach can be found in Hellinger (2007).

Page 3, lines 52-54: The bow shock location can vary a lot depending on external solar wind conditions and mainly on its dynamic pressure. Though distances larger than 15 Re should generally be enough to avoid to be inside the shock region (or the magnetosheath), have the authors ensure that it is always the case in their data set especially for time intervals where the dynamic pressure is very low? The variation of the bow shock location has been studied for instance recently by Meziane et al. (2014). Since mirror modes are very often observed in the magnetosheath, using such approach could help to suppress such intervals (if any).

This would be a possibility, but that would mean to start over completely. We do, however, check a small sample of events for their location to get an estimate of the percentage of magnetosheath events.

Page 4, line 76: The width w is an apparent temporal width in the observations (in the time series). This is not an intrinsic physical scale and depends obviously on the spacecraft velocity and the dynamics of the structure in case it is also varying with time. So I would rephrase it such as 'The apparent temporal width w of the LHM structures in the time series is …'

The text has been revised accordingly.

Page 4, line 81-85: This comparison hold for one single spacecraft assuming that the structure does not evolve in size with time and does not propagate itself with respect to the ambient solar wind plasma (stationary structure). MMS is a four-spacecraft mission so basically it should help to disentangle between spatial and temporal variation in principle.

Yes, one would think that the 4-spacecraft mission would help entangle spatial and temporal variations through the methods developed for the Cluster mission. Unfortunately the inter-spacecraft separation of MMS is so small that the structures we are looking at in this paper are too large, and all four spacecraft basically see the same thing. The Bt for all four spacecraft is added to the data figures, and that shows that e.g. timing analysis is not possible. However, it can be done for sub-ion scale magnetic holes, where there is a clear distinct feature at each spacecraft (Wang et al., 2020).
This was already mentioned in the Discussion and Conclusions section of the paper, line 198. However, we have extended this comment a bit further.

Page 4, line 84: This is the thermal Larmor radius. It should be written.

The text has been revised accordingly and also the velocity is now called the thermal velocity.

Page 4, line 89: Hot flow anomalies are not the only kind of foreshock transients which can be observed. What about e.g. the foreshock cavities (e.g. Sibeck et al., 2002), density holes (Parks et al., 2006), foreshocks cavitons (Blanco-Cano et al., 2009; 2011; Kajdič et al., 2013)? Some of the characteristics of these transients are common with the phenomenological criteria used here to characterize the magnetic holes. It is mandatory to clearly explain the differences between them.

Indeed, there are many more foreshock structures than just HFAs. We have added the various structures that the referee mentions in this comment. However, we think that it is well beyond the scope of this paper to discuss these structures in detail.

Page 5, Figure 1: There are many things to say about this Figure and this case:
a) first, there is an inversion in panel (a) in the legend between By and Bt (for Btotal I guess, which is the magnitude of B since it is not explicit neither in the text nor in the caption, and different from the t,n,m components on line 154 for instance). I guess the purple curve which is always negative here is By and the red curve is B total. It seems to be the case for other Figures like 6 and 7.

Unfortunately, the colours of the lines got disturbed through the inclusion of burst mode data (blue on top of red turns into a purple), which have now been removed from the paper. The use of Bt (as in Btotal) has been used as later a minimum variance coordinate system is used in the paper with l, m and n components. It would thus be confusing to use Bm for the magnitude of the magnetic field and Bt is used.

b) there is an obvious issue on panel (b) since the ion and electron densities shown are different. This is systematic and even worse on Figure 6 (with nearly 50% difference!). Since quasineutrality must be ensured in the solar wind plasma at the considered scales, there is a need for some explanation here!
…
The description of the ion and electron data set is quite short in the present paper. I would strongly suggest to add a better description in part 2.

This is an omission in the paper, as there is no discussion about the fact that the MMS FPI instrument was not developed for solar wind conditions, even though the instrument has a solar wind mode. The differences that are seen in e.g. the densities of the protons and electrons are purely instrumental. We have added two plasma specialists to our team: Owen Roberts and Ali Varsani, of whom the first has statistically studied the behaviour of FPI in the solar wind by comparing the data sets with OMNI data (something similar was done for the ARTEMIS mission by Artemyev et al, 2018). We have added a discussion about this in the paper. The electron densities are in good agreement with the OMNI data, the ion densities are, statistically, under-estimated (where of course there are exceptions, such that the ion density is over estimated).

c) the other issue for panel (b) is that the behavior of the ion and electron densities (whatever the time resolution) is different for the time where the magnetic field magnitude depression is observed in the center of the Figure. Electron density seems correlated while ion density seems mostly anti-correlated! Moreover the ion density is strongly varying around the magnetic hole with some irregular fluctuations (ULF waves?) on a scale similar to the magnetic field depression (where it is the least variable). The authors should comment on that.

The ion density is varying with a ~20-s period, similar to the variations seen in the electron velocity in panel (e). This is the spacecraft spin tone, which has not been removed correctly from the data.
We have added a comment on the spin tone in the paper.

d) how are the different ion temperature components on panel (c) computed? Do they come from a diagonalization of the pressure (or temperature) tensor or are they computed in a frame whith one axis along the ambient magnetic field (average on a larger scale) using the magnetic field data so that the parallel and perpendicular temperatures are the real true ones?
…
Also, some clear ULF fluctuation is observed mostly on the blue curve (parallel temperature for ion fast mode) for all the time interval shown (and maybe on a larger one?) which make any conclusion about its variation where the magnetic depression is observed quite delicate.

This is beyond the scope of this paper and for readers interested in this specific knowledge the references to the instrument papers are in the text.
The purported ULF waves are the spacecraft spin tone.

e) There is a big issue with the electron velocity components shown on panel (d). There is strong regular modulation on the main component (Vex) and also on Vey. This also observed systematically for nearly all the other cases shown in the following figures (6, 7, 8, 9 and 10). This too regular fluctuation (of the order of 20 seconds) seems to be an artefact of the moment calculation for a reason I cannot infer (it does not seem to be related to a spacecraft spin period effect?).
…
It would be interesting to show the electron spectrogram time series (like panel (f) for the ions) to see whether such modulation is visible or not. It may be systematically shown for all cases.

The 20-s signal that is seen is the spacecraft spin tone.
The electron time-energy spectrograms have been added to all data figures.

f) the authors should explain why there is so much difference between the ion and electron fast and burst mode temperature sets on panels (c) and (e).

As there is no burst data for most of the events in this paper, these have been taken out of the paper.
This choice is recalled in Section 2.

Also, we do not see any specific signature of the magnetic hole in these panels. Is it really useful to show these data for this case? We get the feeling the authors systematically display all the data they have available without any specific purpose

We are surprised by this comment, as all the data that we show are actually used to calculate the physical size of the MHs (velocity) in order to scale them to the local Larmor radius (B, N, T).
In the first event, figure 1, there is a signature in the ion temperature, which is shorter than the 20-second spin tone and aligns well with the magnetic hole. This might have been hidden through the addition of the burst mode data, which have now been removed altogether to avoid confusion.

Another general comment: the labels are very small and not easy to read. Also it should be always mentioned for which MMS spacecraft the data come from (both in the text and in the caption).

We will take care that the labels are larger in the final version of the paper. Also, a mention is now made in the paper that only the data from MMS1 are used.

Page 7, line 109: I do not agree that the two temperature are 'basically the same' 'Outside'. This is true before the magnetic hole but not after.

The text in the paper was not correct, and this paragraph has been rewritten, also in view of the fact that the burst mode data have been taken out of the paper.

Page 7, line 112: How can the authors infer that the mirror instability criterion is fulfilled without simply checking it? At least the increase of Tperp is consistent with it but not sufficient.

The referee is correct and an extra panel with the instability criterion has been added to the figures, using the condition Rsk from Eq. (1).

Page 7, line 114: The two cases
…
Again the observations are shown for only one spacecraft.

The reason for this has been explained above.

Page 8, line 125: How can the authors state that? There is a clear need for an elaborated reasoning before concluding that the structure are merging. For instance, is there any support from a simulation result to propose this? It would also be interesting to check whether the time separation between the two holes in the times series differ for the other MMS satellites?

This comment follows from MHs developing out of MMs. If the Bohm-like diffusion, proposed by Hasegawa & Tsurutani, works on two neighbouring MMs, then through growth of these to MMs they might merge and create this "double dipped" structure. Therefore is it is stated in the text that COULD be an indication. More text is added in the discussion section in order to describe what we were thinking here.

Page 9, line 131: I suggest to write '… the structures are ' likely ' MM-unstable.' or to give the result of the instability criterion (equation 1) to be able to state that (same remark as my point (9) above).

We have added the instability criterion to the figures and added a comment in the text.

Page 9, lines 132-133

Fluctuations have been explained above

Page 9, line 139: Is this case really in the solar wind? The ion spectrogram display a very hot distribution which is clearly not the pristine solar wind. Same for the bulk ion velocity which seems very low (around 200 km/s). As I already mentioned, I strongly recommend the author to check whether this is not a case in the magnetosheath by looking also to the electron spectrogram for instance (electron variation is strong at the shock) and/or to check with a bow shock location model taking into account the (true) upstream solar wind pressure (from another spacecraft like ACE for instance).

This is a strange case. The bulk velocity is about 300 km/s (both Vx and Vy are ~200 km/s). The MMS spacecraft are located at (4, 21, 0.2) Re, the solar wind conditions are nominal from Wind B~3.5 nT with positive Bz, Vx ~380 km/s, ni ~ 4/cc, dynamic pressure ~1.1 nPa and an Alfven Mach number of ~10, with a bow shock sub solar point at ~14.5 Re.
It is not impossible that this is in the magnetosheath, the spacecraft seems to be at least near the nominal bow shock as shown in Cairns et al. [1995]. But the MMS website shows that the spacecraft is (deep) in the magnetosheath.
Indeed, checking at the MMS website, the spacecraft locator shows the spacecraft in the magnetosheath.

https://lasp.colorado.edu/mms/sdc/public/plots/#/historical-orbit?year=2020&month=11&day=09&time=10:00:00&plot_type=XY

Taking the set of the nearest events, i.e. R < 16 Re, only 3 (all on the same day) out of 22 are in the magnetosheath.

Page 13, line 142: There seems to be another small case before 04:1150 UT. Is there a reason (from the selection criteria) not to select it? Also, it would be very nice to add vertical dashed lines surrounding the tile interval where the magnetic hole structure is supposed to be identified (also on other Figures like that).

The figures have been adapted, to have the vertical lines in all panels, that was a mistake in the plotting routine. And yes, because of the selection method some hole may not be selected. This has been explained in Volwerk et al., 2020, and this results in an underestimation of about 10%. This has been added to the text describing the second event.

Page 13, line 145: The ion spectrogram does not reveal a superimposition of clear solar wind population plus a very energetic (backstreaming ions) component as it should be in the ion foreshock. I totally disagree with the authors there. See my point (14).

Yes, this might indeed be the magnetosheath, see above.

Page 13, line 146-149: Here I agree this very nice observation is inside the ion foreshock and moreover inside the ULF foreshock waves boundary since clear typical nonlinear '30-s ULF waves' are seen both on the magnetic-field and the ion density. The ion spectrogram clearly reveals the waves on the solar wind (red peak) with a clearly separated ion foreshock population. The ion velocity (which seems to be correctly computed contrary to the electron one) also displays the effect of these waves. So my question: how this structure shown here differs from the so-called 'cavitons'?

In Fig 11 we have transformed the data into a MVA coordinate system, which shows that this structure is possibly a flux rope or a hot flow anomaly. The cavitons as in Kajdic et al. (2013) have durations greater than approximately 1 minute (see their table 1), which is much longer than the structure in Figs. 10 and 11. We see no evidence for cavitons to have a flux rope structure in Kajdic et al. (2011, 2013).

Page 13, line 159: This sentence seems strange here since the pressure balance has not been yet proven!

True, we should have stated that the structures "are assumed to be in pressure balance", now corrected.

Page 13, line 168: Have the authors check the reason why these 5 cases are outliers?

We have checked the 5 "outliers" (and updated the figure). The text has been adapted and the reason is that these are influenced by the foreshock.

Page 16, line 176-178: There is something I do not understand here (and I guess some kind of reasoning error about the foreshock). Basically this angle on Figure 14 deals with the radial component of the magnetic field. When considering Fig. 13 giving the locations of the observations which are all on the dayside, it seems obvious that a nearly radial field will always intercept the bow shock surface.

Yes, the referee is correct, this deals with the "radial component" of the magnetic field, insofar that the direction of the magnetic field around the MHs is determined, and the angle of B with the radial direction from the Earth's centre to the spacecraft is determined. Figure 14 shows that for the "cold" category the events are mainly observed at a large angle, which means that they are unconnected to the bow shock/foreshock region. In the other categories, it looks slightly different, with smaller populations at large angles.

Page 21, line 224: Could the author provide any reference to explain why this is expected? There is no mention of any theoretical work on the nonlinear evolution of the mirror mode instability in the present paper.

It is our understanding that when an instability is triggered in a plasma, this leads to a relaxation of the instability criterion outside of the formed structure. However, there are no papers (as far as we know) that show that inside the MH the instability criterion should not be fulfilled. However, there are papers that show electron and ion vortices in MHs, which would indicate a temperature asymmetry. This has been added to the paper, including this topic being the centre of an ISSI team, which will first meet next year.

Page 4, line 87: ‘ the Earth and’ its ‘bow shock’

corrected

Page, 6 line 97: Upper delta T at the end of the line

Corrected

Page 13, line 162: ‘center’

We prefer British spelling for a European journal.