We respond below to the comments raised by the reviewer, setting the reviewer's words in *italics* with our response following below.

Response to Referee 2

This paper addresses observations of ion acoustic waves (IAW) in the vicinity of 67P nucleus a few months before perihelion. The paper is based on observations of 4 instruments of the Rosetta Plasma Consortium and of ROSINA-COPS. The IAW are observed in the region of high current drift (near closest approach to the nucleus) in connection with high current drift, while they were not further from the nucleus where there was no significant current drift observed.

The paper is in general written in good style, but the formulation of physical processes is sometimes more qualitative than quantitative, even vague at times. The identification of the IAW lacks clear characterization of their properties. Some numbers of the observed or derived parameters are given but not always substantiated.

Reference is made to the first detection paper of IAW at 67P. The detection conditions, including the plasma parameters seem to be different, but those differences, and their consequences on the IAW properties may not discussed in depth.

We have amended the manuscript, and we think that it now adequately describes how the ion acoustic waves were identified, that the derived parameters are sufficiently substantiated.

Beyond the plasma physics interest of the detection of IAW in 67P environment, In the discussion, I would have like to see a short paragraph discussing the interest of the detection of IAW in terms of cometary physics and Cometary sciences. As written in the first sentence in the introduction, "observations of waves can give us information of the plasma physics in which they are generated and through which they have travelled" (a rather strange formulation by the way). In the discussion I would have expected reading something of what the detection and characterization of IAW bring in terms of understanding the comet plasma (and neutral ?) environment how do they help in constraining physical processes at work inside the coma.

The abstract may not fully reflect the content of the paper. It should include something on:

- *Hot ions are not contributing the IAW*
- *IAW waves are detected when a current flow is present as determined from B-field measurements*
- *The high spacecraft charging complicates the interpretation of the observations.*

We have put all those things in the abstract, and we have also added text to the discussion section to highlight how wave observations in combination with wave theory supplement the measurements by the other instruments and the interpretation thereof.

Line 10. Replace « travelled » by « propagated » ?

Changed as suggested.

Line 12: Add "charged" in front of "particle"

Changed as suggested.

It's hard to appreciate the importance of the Doppler shift without the mentioning of the frequency range of the waves. Explain the relation between the bulk velocity and the Doppler shift.

This must be referring to the mentioning of the Doppler shift in the abstract. We have modified the sentence in questions so that it also states the frequency range of the waves in the plasma frame.

"Near closest approach the propagation direction was within 50° from the direction of the bulk velocity, leading to a Doppler shift of the waves, which in the plasma frame appear below the ion plasma frequecy $f_{\mathrm{pi}} \approx 2\,\mathrm{kHz}$, to the spacecraft frame where they cover a frequency range up to approximately $4\,\mathrm{kHz}$."

Line 21: Provide the parameters that lead to a LHF < 15 Hz.

The reader is referred to the original papers by Andre et al (2017) and Karlsson et al. (2017) for further details. The purpose of the introduction is merely to provide context so that the present work can be located in the greater scheme of things and to briefly review the relevant history of the field, which in this case concerns other observations by Rosetta, mostly but not only wave observations.

Line 23-26: Is there a relation between the steepened waves only observed outside the diamagnetic cavity and the waves in the LHF range observed on both sides

Stenberg Wieser et al. (2017) reported that they appear in the same position at the same time outside of the cavity. One may speculate about how energy is transferred between different wave modes and between different parts of the comet environment, but the present paper is hardly the place for that.

At this stage, it would be useful to recall how IAW modes are identified. During the review, I just came acrosss recently published paper (that was not available at the time of the paper submission) that addresses well the identification of IAW modes (Mozer et al. 2020). One characteristic used is the phase. Can information about the phase be obtained with the LAP probe signal ?. Would the availability of both P1 and P2 signals (although with different amplitudes) help in obtaining information about the phase?

We added the following text:

"Ion acoustic waves can be identified by measuring either the variations in plasma density or in the electric field. At comet 67P/Churyumov-Gerasimenko, Gunell et al. (2017b) detected the waves in electric field oscillations and Gunell et al. (2017a) in density variations. In this work, the detection relies on density variations."

As to the suggestion about adopting the method of Mozer et al. (2020), those authors measured two components of the electric field, using four probes, and computed the phase difference between those two electric field components. This enabled them to assess the polarisation of the wave. Rosetta does not have four probes. During the observations reported here, probe 1 measured the density variations (see also the answer to one of the comments below), whereas the negatively biased probe 2 recorded a displacement current or possibly a combination of a displacement current and an ion current. Thus, the two probes recorded vastly different quantities, and the electric field polarisation cannot be addressed with the methods used by Mozer et al. (2020). A comet-bound spacecraft equipped with four, or better six, probes would of course be vastly superior, but that is not the spacecraft that we have.

> *Line 28: Define undefined variables, e.g. omega, k, kB, mi. The need to define variables used applies also to other parts of the manuscript.*

Definitions have been added.

> *Line 33-40. It is said that no diamagnetic cavity was seen during the flyby studied,. The plasma conditions, and characteristics of the IAW confined in the diamagnetic cavity (Gunell et al, 2017) seem to be somewhat different from those reported in this article (no diamagnetic cavity had formed). There is no discussion of the similarities and differences between the two studies.*

In response to both this and the comment after next we added the following text to the end of the introduction:

"Thus, the spacecraft was situated in the inner coma, where the plasma was of cometary origin. The plasma parameters, although not exactly the same, were in a range similar to that of the diamagnetic cavity observations near perihelion. The magnetic field environment was dominated by magnetic pileup and draping during the flyby, while the diamagnetic cavity has its own peculiar magnetic field environment with a sharp discontinuity at the boundary."

> *Line 41: "likely" is a vague statement. Could it be that the cavity had formed closer to the nucleus, where the s/c did not go on that flyby ?.*

It is unlikely that it had formed based on an analysis by Goetz et al. (2016a). We have put the citation in at the end of the statement for justification.

> *Line 43: explain the implication of the infant bow shock (from simulation) and the fact that the diamagnetic cavity had not formed (or was not observed).*

See our response to the comments two comments above of this.

> *Line 46: Not sure CSEQ is known to all potential readers. A reference, beyond the definition that follows, would be desirable.*

The original definition comes from the SPICE kernel. We added this and a reference to Acton (1996).

Line 56: replace "plasma waves" by "probe current variations attributed to waves"

Changed as suggested.

Line 57: clarify how the probe current variation relates to plasma waves

This is discussed in the second paragraph of Sect. 2.2.

Line 59: You may not have said before that there were two Langmuir Probes

We inserted the following sentence a little bit above:

"RPC-LAP is constituted of two spherical probes, 5 cm in diameter, that are mounted on booms protruding from the spacecraft."

Line 61: It would be useful to indicate the value of the S/C potential, in order to better appreciate the difference between a probe at +30 V and one at -30V.

The spacecraft potential was approximately -20 V. We have added this piece information to the text.

Line 62: Was the bulk speed of the ions "measured" or "estimated"?. See further questions later

We have changed to "derived".

Line 67: add "magnetic field" in "The (magnetic field) components"

Changed as suggested.

Figure 2:

Would be useful to say in the legend or to write in the top two panels the bias value for each of the two LAP probes.

We have put that piece of information in the figure caption.

Is there a physical explanation for the sharp transition of the plasma density derived from MIP measurements before and after 10:00 while the RPC-ICA spectra are quite similar. Any explanation as to why there are no RPC-MIP measurements after 20:00?

Trying to estimate the plasma density from RPC-ICA spectra is not advisable under circumstances where the spacecraft is not negative enough to allow more than a small fraction of the ions can be detected by RPC-ICA (see also the reply to your comment further below about the invisibility of the cold ions). After 20:00 the density falls under the detection limit for the RPC-MIP Short Debye Length mode.

Legend: I would say the plasma density is "derived" rather than "measured" from the RPC-MIP measurements.

Changed as suggested.

Line 73-74: Not obvious in the figure that the frequency scale starts at 200 Hz. Setting the origin of the Y-scale at 0 and leaving the space between 0 and 200 Hz blank would make it clear.

The information is stated in the figure caption.

Line 75: Not clear if signal is a wave signal or noise enhancement. It would be useful to show the non-Doppler shifted LHF line for reference

The lower hybrid frequency is far below the lower limit of the frequency range shown. Estimated at closest approach the lower hybrid frequency is

$$f_{\mathrm{lh}} = \frac{f_{\mathrm{pi}}}{\sqrt{1 + f_{\mathrm{pe}}^2 / f_{\mathrm{ce}}^2}} \approx \frac{2.0\,\mathrm{kHz}}{\sqrt{1 + \left(\frac{360\,\mathrm{kHz}}{1.0\,\mathrm{kHz}}\right)^2}} \approx 6\,\mathrm{Hz}$$

Line 77-78. I would put it the other way around. Probe 1 being dominated by electron current, and probe 2 being dominated by ion current, leads to the fact that the power spectral density of probe 1 is several orders of magnitude higher.

It is written in this way, because it answers your next question about why the variations in probe current are proportional to the the density variation of the wave.

Line 77: clarify « ..... signal proportional to the density variation of the waves » should be substantiated

For increased clarity we have inserted a new sentence, mentioning the two different regimes, which of these the present measurements adhere to, and indicating where this matter is discussed in more detail:

"The probe operated in this regime also in the previously published observations of waves in the diamagnetic cavity (Gunell et al., 2017a), whereas in the first published observations (Gunell et al., 2017b), the probe was capacitively coupled to the plasma. A simple test to distinguish between the two regimes is to measure the wave amplitude as a function of probe bias, where a negatively biased probe suppresses the particle current (Torvén et al., 1995). In the present case, we compare the two probes that are biased differently (see also Gunell et al., 2017b)."

Explain why the wave signal observed is identified as IAW. What are the wave characteristics that allow to infer that?

We have added the following sentence at the end of the first paragraph of Sect. 2.2:

"The waves are identified as ion acoustic waves, because they are compressional, showing a plasma density variation, and other wave modes can be excluded for the spatial and temporal scales where they are observed as shown in Sect. 2.4."

Line 80: This statement about the plasma density being comparable in both events is not verifiable as the density measurements are not illustrated in Fig. 3 of the 2017 paper.

The density is illustrated in both the 2017 papers: in Fig. **1** of (Gunell et al. 2017a). In (Gunell et al. 2017b) there is an entire section on density estimates, including an illustration in Fig. **5** of values obtained from different instruments, and there is a table of values that were used in the computations in that paper. There is also an assessment of the results of those computations and what that implies for the tabulated values. All of this confirms the statement made in the present paper.

> *Line 82: Clarify "condition when signal is observed trough displacement current (capacitive coupling) vs particle current? If I understand well, a decrease of a factor of 10 of the current implies that the wave can no more be detected by the current variation, but instead by capacity coupling. This statement should be elaborated.*

The clarification to the question above answers this question too:

"The probe operated in this regime also in the previously published observations of waves in the diamagnetic cavity (Gunell et al., 2017a), whereas in the first published observations (Gunell et al., 2017b), the probe was capacitively coupled to the plasma. A simple test to distinguish between the two regimes is to measure the wave amplitude as a function of probe bias, where a negatively biased probe suppresses the particle current (Torvén et al., 1995). In the present case, we compare the two probes that are biased differently (see also Gunell et al., 2017b)."

> *Line 97: Provide the reference to the publication for the artefacts ? It seems that the artefacts are harmonics of 1 kHz as well evidenced in the PDS line at 19:08:54. This is within the range of the Fce value given in line 168. Can it be excluded that those "artefacts" are harmonics of Fce. It would be desirable to provide the value of Fce for the period. Can it be ruled out that part of the detected noise is enhanced (excited) by those artefacts ? Are those artefacts discernable in the P2 data?*

The artefacts were also seen in ther diamagnetic cavity (Gunell et al., 2017a). These are not harmonics of the electron cyclotron frequency, because the frequencies of the artefacts are constant throughout the day, while $|\mathbf{B}|$, to which f_{ce} is proportional, varies by about a factor of two. Examples of f_{ce} within the range in which it varies are now provided in Sect. 2.4. As seen in figure 2a the amplitude of the artefacts is significantly below that of the wave signal until that signals starts to fade away. The artefact may excite a low amplitude wave at the isolated frequencies where they are seen, but not the much higher amplitude waves of interest here, which also are present in a much wider frequency range than the artefacts. See also the figure next to this piece of text (Fig. A) for two time series of the probe current.

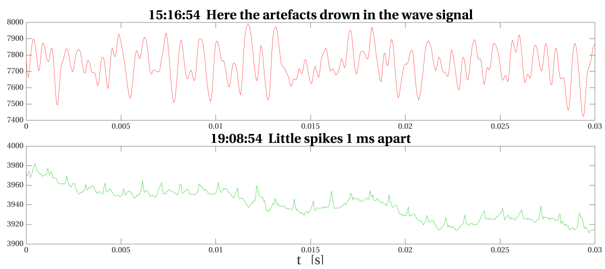

Figure A: *Time series of the probe currents for two times during the day of 28 March 2015.*

> *Line 101-102: How do you quantify plasma inhomogeneities at 10%. What is the accuracy of the MIP measurements?*

See the electron density plot to the right. The accuracy of the data in the figure is 5–10 cm^{-3}, the variations are one order of magnitude larger, and the mean is lager by yet another order of magnitude. The accuracy of the MIP measurements is also about 10%. The accuracy of the electron density from the MIP measurement is computed from the frequency resolution of the instrument, which measures the plasma frequency from the identification of the plasma frequency line in the mutual impedance spectra.

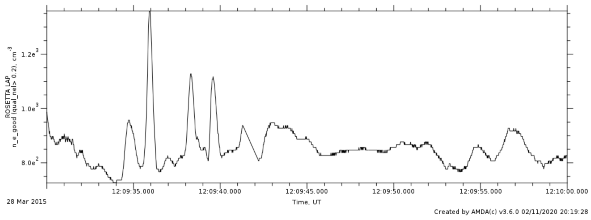
Figure B: *LAP plasma density sample figure for this day.*

Line 104-105: Process by which the ions are getting heated (6eV) and how this temperature is derived?

We merely observe those ions. The heating process is outside the scope of this paper. The temperature is obtained from the same fit that gave the density, mentioned a few lines below. We have introduced words to say that the temperature is also obtained by fitting.

Line 105: You should say at least once that those are positive ions. Apologies if it was said before and I missed it.

We inserted the word positive before ions in the first sentence of this paragraph. In other places in the manuscript we asssume the ions to be H_2O^+ ions.

Are there also negative ions present in the plasma? if yes, how would those negative ions affect the IAW generation and damping ?

There are no significant amounts of negative ions in the plasma, at least not from a plasma physics point of view. Measurements at comet 1P showed that fewer than one ion in 10^4 are negative.

Line 107. I suppose the fit is performed with a drifting maxwellian population. Please confirm

Confirmed. We updated the text to say “drifting Maxwellian”.

Line 110-115. I Not convincing argument as to why most of the ion population is not visible, all ions (warm and cold) should be accelerated by the S/C potential, should they not ?
I have difficulties to follow the reasoning about the non-detectability of the cold ions. Should they not be accelerated to 20 V as well. If the fraction of that population that is detected may not be distinguishable from the ions belonging to the warm population, should they not appear in the maxwellian fit described earlier. This seems to be somewhat in contradiction when saying that it may explain that the cold water ion population (still accelerated to 20 V) is invisible to RPC-ICA. “May” means that there could be other explanations. Please elaborate.

They are accelerated by the spacecraft potential, but the ability of the instrument to detect low energy ions is severely limited. The sensitivity of the detector is low for such low energies, and the angular range that allows entry into the instrument narrow. Thus, a mono-energetic low energy beam, which the cold ion population is, can go completely undetected. We described that in terms of a field of view, vanishing at low energies. This description seems to be inefficient as a form of communication, and we have changed the text to provide a better explanation.

"The sensitivity of RPC-ICA is low in the lowest energy range, and the angular range that allows entry into the instrument is narrow. A mono-energetic low energy beam is liable to be undetected. Thus, the discrepancy between the RPC-ICA measured ion density and the plasma density measured by RPC-MIP is explained by a cold water ion distribution that is invisible to RPC-ICA."

Finally, to avoid misunderstanding, of course the Maxwellian fit is to the part of the distribution that is above the limit for detection.

> *Line 116: Clarify how the various photoemission current is taken into account in the ion part of the I-V curve.*

The photoemission comes in as an offset in the ion current (below the plasma potential), and as such, does not play a role in the slope of the ion current. Also, LAP1 is in shadow behind the spacecraft for the first sweep. We added the following at the end of the paragraph:

"Photo emission comes in as an offset in the ion current, below the plasma potential, and as such, does not play a role in the slope of the ion current. Furthermore, for the first of the sweeps, the probe was in shadow behind the spacecraft."

> *Line 116: Discuss the deviation from linearity of the ion portion of the I-V curve at negative potential clearly visible at 13:25:26, but also discernable at 17:52:06. It is said earlier that the I-V curve is acquired in the -30 +30 V range. If so, it would be interesting to show the hidden part of the curve, between -30 V and -30 V.*

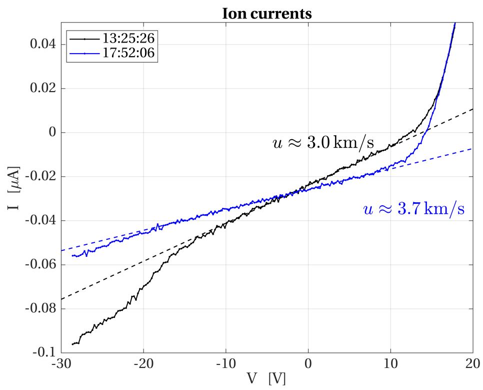

Figure C: *LAP characteristics starting from the beginning of the sweep.*

It is possible that the deviation from linearity is the onset of secondary emission from ion impact. However, it is not always present, does not behave very similarly from sweep to sweep, is sometimes nonlinear and wavelike in nature, As a full Langmuir probe sweep takes 3.2 seconds, (i.e. is not instantaneous), it is more likely that this is a capacative coupling to some excited wave propagated by ions with an observed frequency near 1 Hz and grows larger in amplitude when more ions are attracted by the probe. This is also confirmed to be present when looking at the continuous (60 Hz) ion current data from LAP 2. Therefore, we restrict the ion current fit to a region below the spacecraft potential where the ion current is linear, and any wave amplitude is small. A figure showing the "hidden" part is shown here (Fig. C). The figure in the paper has also been updated but in a different way with some new panels.

> *My examination of the I-V curve indicates that the local plasma potential is about 20 V, confirming that the S/C is charged to about -20V. The energy of the ions hitting the probe may reach 50 V (60 V if the probe is polarized at -30 V). In this energy range, is it possible that secondary emission plays a role? is photoemission of the probe surface taken into account in the probe current ?*

We have reexamined the I-V curve and agree with the local plasma potential and spacecraft potential estimation.

Regarding secondary emission, it is certainly possible, but the quantum yield of ion impact ionisation is low for metals particularly at low energies, less then unity. And for a ceramic surface layer like TiN, it is likely that the quantum yield is even lower. We have added a sentence explaining that if secondary emission is present also at the low (10eV) energy range where our ion current analysis begins, then this would inflate our ion velocities with a factor equal 1+ the quantum yield. It would only have a very limited impact on Probe current variations attributed to waves.

Regarding photoemission, the LAP1 is in shadow between 11 and 14:30 and is not photoemitting. Also, at the potentials the ion current analysis is performed, the photoemission is an offset, and does not play a role in determining the effective ion speed.

> *In eq (1), define variable V. Is such a formula directly applicable for a drifting ion population?. The hot ion population does not seem to be considered in the overall ion current. Justify. Discuss the applicability of eq (1) to the current plasma conditions*

The variable V, representing the probe to plasma potential, was already defined above the equation together with the bulk speed u and the current I. The warm ion population is neglected because of its low density, and the equation is applicable because the thermal speed of the cold ions is far below their bulk speed. A sentence to that effect has been introduced just before Eq. (1).

> *Taking the ion density equal to the plasma density ignores the hot ion population contribution. Is this justified?*

Yes, it is. The warm ion density is $4\,\mathrm{cm}^{-3}$ and the plasma density $1600\,\mathrm{cm}^{-3}$. The factor of 400 between them ensures that the error is negligible.

> *A formulation of the I-V curve taking into account all current contributions should be written.*

With the changes made outlined above, we feel that all relevant current contributions have been described.

> *A proper discussion of the various measurement uncertainties would be desirable*

We answer this now and refer back here, whenever this comment reappears below. We have written a new section about the uncertainties. We have not removed any existing discussions from the other section, which leads to some overlap, but we don't find that overly troubling. The new section appears at Sect. 2.5 in the revised manuscript.

> *Probably not surprising that the numbers are within the range of those observed by Odelstad et al. (2018) if the same method of analysis is used (I did not check that point). Point to be clarified.*

We do not claim it to be surprising. However, since the observations by Odelstad et al. (2018) were performed in a different magnetic field environment and at a different distance to the nucleus, it was no certainty beforehand that the results would be similar. We have added text, clarifying the different conditions.

> *Line 126: Replace " a upper " with "an upper"*

The sentence has been removed in its entirety as, under careful examination, it has been found to be incorrect.

> *Line 129: Not clear why the Biver et al 0.02 eV neutral temperature is compared to the 1eV (ion) kinetic energy. Please elaborate the argument.*

We added the sentence

"This confirms the assumptions stated above Eq. (1) and ensures the applicability of the equation."

> *Line 136. The slope of the two electron current curves are clearly different. Why do they provide the same value of Te (about 0.2 eV)?*

As the slope is proportional to both electron density and electron temperature, two different slopes measured at plasmas with different densities can have the same temperature. Also, for the temperatures obtained here, the values 0.16 and 0.22 were both rounded off to 0.2.

> *Not clear how the plasma potential is estimated to 12 and 14 volts. Elaborate. My estimation is more around 20 V (see above). In fact, the plasma potential is derived from a measurement made inside the plasma sheath of the charged spacecraft. Discuss the uncertainty of this value?*

We have made a new assessment of the plasma potential, and also of the warm electron temperature. The new version of the manuscript says that the plasma potential is approximately 20 V and the warm electron temperature $k_B T_{ew} = 2\,\mathrm{eV}$.

> *When revising the paper, I would advice to discuss this spacecraft charging effects with reference to the recently published paper by Johansson et al. https://doi.org/10.1051/0004-6361/202038592*

We have added the following sentence in connection with the determination of the spacecraft potential.

"It was shown in simulations by Johansson et al. (2020) that the spacecraft potential is driven negative by positively biased elements on the solar panels that collect cold electrons from the plasma."

Discuss uncertainties in the derived numbers

See our response to this comment on page 9.

Line 143: Confirm that, in the presence of two equal-density electron populations, the MIP max represents the plasma frequency (how is it defined with two such different populations). It is noted that the MIP phase data are not referred to. Are they consistent with the amplitude data?

In the presence of two electron populations (for instance two equal-density electron populations), the main resonance in the mutual impedance spectra is indeed associated to the (total) electron density, and give therefore access to the total electron density. In some cases (e.g. large temperature ratio and similar densities such as for two equal-density electron populations) a second resonance associated to electron acoustic waves generated in the plasma by the MIP electric transmitter. The general instrumental response of the MIP mutual impedance probe in a plasma characterised by two electron populations with different temperatures is described in Gilet et al. 2017 <https://doi.org/10.1002/2017RS006294> and Wattieaux et al, 2020 <https://doi.org/10.1051/0004-6361/202037571>. We have added the following sentence to the manuscript:

"The general instrumental response of the RPC-MIP mutual impedance probe in a plasma characterised by two electron populations with different temperatures is described by Gilet et al. (2017) and Wattieaux et al. (2020)."

Line 162-163: What is the implication of the measurement uncertainty expressed by the sentence "Thus, the current density may have been both higher and lower that these average values during the flyby" ?

See our response to this comment on page 9.

Line 169: How is the wave frequency characterized? justify the important affirmation that the wave frequency does not follow the change of the magnetic field, used to justify that the waves observed are not electron cyclotron waves. Provide values.

We inserted the following sentence to provide values:

"For example, the PSD peaks at 700 Hz for both times 13:24:54 and 15:16:54, shown by the black and red curves in Fig. 3, respectively, even though the electron cyclotron frequency was 1.1 kHz at 13:24:54 and 0.57 kHz at 15:16:54."

Line 172: How was the "typical length" for the variation in wave amplitude deduced to 10 km ?. What is meant by "typical length"

This is described in the sentence immediately before the one where the word "typical" appears:

"The spacecraft was at 15 km cometocentric distance at closest approach and at 25 km at 18:00 when the wave amplitude started to decrease. Thus, the typical length for the variation in wave amplitude is about 10 km."

Line 177: clarify if you refer to electron or ion gyro-radius, or both

We have replaced “particles” by “electrons and ions”.

Table 1: Clarify parameters used. Electron VD?

We added an explanation of the notation to the table caption.

Line 184: the uncertainties in the measurements is not well reflected in the values reported in sect; 2. Can the measurement uncertainties be quantified?

See our response to this comment on page 9.

Line 190 -192 It would be desirable to provide the formula of the dispersion relation used, although indeed, proper reference is given. May be as important, if not more, than the formula for the distribution function.

We have updated the text to provide that, but since this is somewhat involved and requires new notation to be introduced and several equations to be displayed, we have put it in a new appendix, which is now Appendix A.

Line 194: define variable v? is variable “vd” used in the formula the same as “vD” used in the table ? use consistent notation.

v means velocity. This piece of information has now been added to the manuscript. We have also changed all instances of v_d v_D.

Line 211: The non-effect of the warm ions (the one detected by ICA) lead to consider the cold ions whose density is set equal to the « measured » electron density. This makes a strong assumption that the plasma is locally neutral, which may not be the case in the sheath around the spacecraft. Justify.

That the warm ions, due to their low density, have no influence on the wave properties does not tell us anything about the neutrality of the plasma. Also, the probe is outside the sheath as the Debye length, as estimated below, is only 26 cm.

Line 214: is it justified to assign a drift velocity to only one of the two electron populations?

As collisions cools the electrons to give them different temperatures they cal also obtain different velocities. However, this comment inspired another test, namely assigning equal drift velocities to both electron population. This is now distribution 9. The result is reported in the revised manuscript:

“In distribution 9 the current is carried by both electron populations, which each are given a drift velocity of $|\mathbf{v}_\mathrm{D}| = 19.6\,\mathrm{km\,s^{-1}}$. This yields a lower growth rate than for distribution 2, but the mode is still unstable for a range of k values.”

Distribution 9 is now also included in Fig. 8 and the discussion thereof (see the revised manuscript).

Line 216: Such a strong conclusion should be more substantiated.

Following the evaluation of distribution 9 this statement has been moderated:

"We conclude that to drive the ion acoustic waves unstable the current cannot be carried by the warm electrons alone."

This is substatiated by the computations based on distribution 9.

Line 225: word missing: « . . . is found to (be) similar.. »

Corrected. Thanks also to referee 1.

Line 245-248, and legend Fig 8: The notations used should be all defined (in the legend)

Notation explanation may now be found in the caption.

Line 250-251: Not clear what means a « reasonable spectrum » and how this observation is reached.

"reasonable spectrum" is replaced by

"an interpretation in which observations and theory are consistent. This is explained in what follows"

Line 251-273: The narrative discussions seem to be very qualitative. Not clear that the conclusions reached are well substantiated.

The approach employed is to assess the observations and compare those to theory to the extent the measurements allow. To that extent they are well substantiated. For example we are only able to confine propagation angles to a rather wide range, and we cannot give precise numbers, as that indeed would be unfounded in fact.

Line 275: specify that a multi-instrument data set was analyzed. Recall which data set were used.

We added as the new second sentence of the section:

"The multiple instruments used were RPC-LAP, RPC-ICA, RPC-MAG, and RPC-MIP, all part of the Rosetta Plasma Consortium."

Line 276: indicate that the waves were recorded as Langmuir probe current variations

We changed the sentence to read

"Waves which we interpret as current-driven ion acoustic waves were recorded by the Langmuir probe instrument RPC-LAP as probe current variations."

Line 283-284: The ion drift value, obtained from the analysis of the LAP I-V curve, was questioned above. What is the process causing the ion drift speed of 3 to 3.7 km/s. Could this be partly a local phenomena inside the sheath of the charged spacecraft?

Ions are accelerated by an ambipolar field radially outward from the nucleus. The sheath around the spacecraft is not affecting the measurment, as the probe is outside the sheath. In a plasma with a $2\,\mathrm{eV}$ electron temperature and a $1.6 \times 10^9\,\mathrm{m}^{-3}$ density, the Debye length is 26 cm. The probe is therefore outside the sheath. Note that this is an upper limit. Should cold electrons be included, the resulting value would be even lower.

The Debye length estimate compared to the boom length is in the new section on uncertainties, and we added the following text about the acceleration process in the discussion section:

"Theoretical estimates of acceleration by a radial ambipolar electric field lead to ion bulk speeds in this range (Vigren and Eriksson, 2017) in agreement with observations (Odelstad et al., 2018)."

Line 285: replace "electron volt" by "eV"

Changed as suggested.

Line 288: « ... possible to say something about it ». I found this statement very speculative with the limited cases tested.

We have said something about it, so it is possible. We make no claim of being able to say everything about it. The statment as it stands is not speculative.

Line 295: remove « the » before « bulk »

Changed as suggested.

Line 297: Discuss the processes that would increase the bulk temperature or form supra-thermal tails. Can wave-particle interaction contribute to the ion heating process ?

Particle populations can absorb energy through wave-particle interaction or by acceleration by dc fields, often followed by wave-particle interaction. However, it is wise not to speculate about matters that we cannot support by measurement.

Current carried by cold electrons?

This is part of the list of properties of the distribution that best agrees with the observations:

"It has the warmest cold ion distribution (0.04 eV), the current carried by the cold electrons, and no warm ion component as that was found to be negligible."

Line 313-316: Indeed, it seems pretty strong conclusions are reached from crude estimates and measurements with uncertainties (which are not quantified).

We think that in the amended manuscript, with the newly added discussion of the uncertainties, the conclusions are justified.