Ion acoustic waves near a comet nucleus : Rosetta observations at comet 67P/Churymov-Gerasimenko, by Gunell et al.

General comments:

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.

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.

Detailed comments are provided below.

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

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

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.

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

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

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?

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.

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.

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 ?.

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).

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

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

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

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

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.

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

Line 67: add “magnetic field” in “The (magnetic field) components”

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.

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?

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

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.

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

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.

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

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

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.

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.

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?

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

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

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

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

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

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.

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

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.

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 ?

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

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

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

A proper discussion of the various measurement uncertainties would be desirable

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.

Line 126: Replace “ a upper “ with “an upper”

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.

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)?

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?

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

Discuss uncertainties in the derived numbers

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?

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” ?

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.

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

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

Table 1: Clarify parameters used. Electron VD?

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

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.

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

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.

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

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

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

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

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

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

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

Line 276: indicate that the waves were recorded as Langmuir 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?

Line 285: replace “electron volt” by “eV”

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

Line 295: remove « the » before « bulk »

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 ?

Current carried by cold electrons?

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