Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2020-84-RC1, 2021

Annales Geophysicae Discussions

Interactive comment on "Steepening of magnetosonic waves in the inner coma of comet 67P/Churyumov-Gerasimenko" *by* Katharina Ostaszewski et al.

Anonymous Referee #1

Received and published: 12 January 2021

This manuscript presents an extensive statistical study of steepened magnetic field structures observed in the environment of comet 67P-CG by the Rosetta spacecraft over the course of its mission. The analysis concentrates on magnetic field measurements, and uses machine learning methods, presented in another paper, to automatically detect the structures. The variation of the occurrence rate of the structures and of their shape as a function of the comet's activity are investigated. Using minimum variance analysis, the authors determine that they propagate roughly in the direction perpendicular to the ambient magnetic field. Because of this transverse propagation, and of the density variations accompanying these structures as reported in other studies, the authors conclude that they are likely magnetosonic in nature. Finally, a 1D

MHD model is used to simulate the non-linear evolution of plasma and magnetic field pulsations into steepened structures.

This paper is well-written and contains interesting new results. It lacks however a more detailed comparison with previous works published on very similar observations by Rosetta, and more discussion about the physical processes at play and the possible sources of the waves. I have listed my comments below, first these major points, then minor comments. The manuscript should be suitable for publication in Annales Geophysicae after these are addressed.

Major comments:

Introduction: It would be interesting to include more information in the introduction regarding the sources of low-frequency waves in cometary environments, which are relevant to the present study. At the moment, it is only mentioned at lines 386-387 that waves can be triggered by "various plasma instabilities", while the survey of low-frequency wave observations at comets provided at lines 32-39 in the introduction only reports on the wave properties, and not their inferred sources and the physical processes at play.

I would also recommend to include more details on the studies by Engelhardt et al. [2018] and Hajra et al. [2018b], which report on similar structures observed by Rosetta, the former outside of the diamagnetic cavity and the latter inside the cavity. It is worth noting for example that Engelhard et al. find that the duration of the structures are typically a few second to a few tens of seconds, and that their occurrence rate is largest near perihelion, consistent with the present study. This would allow to better place the present study in the context of the existing literature. Furthermore, the comparison with these previous studies raises some interesting questions, in particular regarding the nature of the steepened magnetic field signatures. Both Engelhardt et al. and Hajra et al. conclude that the observed signatures are more likely to be plasma structures rather than waves, which are associated with the boundary of the diamagnetic cavity (either filamentary structures extending from the diamagnetic cavity, or structures inside the cavity caused by disturbances at its boundary). In contrast, in the present study, it is assumed that the observed magnetic structures are steepened fast-magnetosonic waves. More discussion is needed to reconcile these different assumptions regarding the nature of these steepened magnetic field signatures, either already in the introduction, since the authors then rely on this assumption to estimate the velocity of the waves later in the study, or in the conclusion section.

Another aspect of the study which, in my opinion, requires more discussion is the selection of the events. The description of the event selection method is rather brief, as the authors have published a more detailed presentation of their method in another paper, but there are two points that would deserve some clarification in the present manuscript: First, was the position of Rosetta within the cometary environment taken into account when selecting the events? In particular, were the intervals when the spacecraft was within the diamagnetic cavity excluded from the analysis? Or was it assumed that there would be no detection of steepened waves inside this cavity, since the magnetic field is almost zero in this region? Second, and more importantly, the study by Hajra et al. [2018b] reveals that the crossings of the boundary of the diamagnetic cavity display signatures that are extremely similar to those of the steepened waves presented here (see their Figures 1 and 2). Would these crossings be picked up by the detection method used in the present study? If so, how would this affect the analysis and the conclusions?

The study by Engelhardt et al. [2018], which focused on similar structures but taking into account plasma measurements in addition to magnetic field data, concluded that the occurrence of these structures strongly depended on the distance from the electron exobase. Would it be possible to calculate this parameter for the present, much more extended, data set, or at least for part of it when the relevant data are available, and check whether a similar distribution of the observations is found?

Minor comments:

Page 2, line 25 "In such regions, conditions for the steepening of compressive modes are exceptionally favourable." Can the authors briefly explain why this is the case for cometary interaction regions?

Figure 1: I would suggest to add one plasma parameter, for example an ion time-energy spectrogram, to showcase that the spacecraft remains in the same plasma region, and that the non-linear enhancements are indeed waves rather than boundaries between plasma regions.

Page 6, lines 111-112: "The exact nature of this transition region and the processes governing it require a more in-depth analysis, which is out of the scope of this paper." I am not sure I understand which transition region is referred to here. Is it the transition between the magnetic cavity and the outside solar wind, or the transition from low-activity cometary environment (with the "singing comet" waves) to high-activity cometary environment (with steepened waves)? Could you please clarify?

Page 7 and Figure 3: the occurrence rate shows a plateau above a certain mass-loading rate. It would be interesting to add more discussion about what could cause that. Is this physical, or could this be due to errors in the measured neutral gas density? Or could it be that the ionisation rate is incorrectly modelled above a certain activity level?

Page 7, lines 157-159: "In some cases, these sharp increases coincide with increases in the solar wind dynamic pressure. However, most of the time, no correlation between the pressure and time between observations is visible." It is not clear to me why it would be expected that higher dynamic pressure would lead to larger time between the wave observations. Longer intervals suggests a lower wave activity in the comet's environment, whereas high dynamic pressure rather corresponds to "disturbed" solar wind conditions. Or is it because the comet's environment would be compressed, resulting in the spacecraft being located in a different environment? Could the authors please elaborate on this point?

Section 4: Similar dispersive wave signatures are also observed in association with steepened waves in the Earth's foreshock [e.g. Hada, T., C. F. Kennel, and T. Terasawa: 1987, 'Excitation of compressional waves and the formation of shocklets in the Earth's foreshock'. J. Geophys. Res. 92(5), 4423–4435 and Greenstadt, E. W., G. Le, and R. J. Strangeway: 1995, 'ULF waves in the foreshock'. Adv. Space. Res. 15, 71–84]. How do they compare with the observations reported in the present manuscript?

Section 5: Is the fitting applied to all waves, or only to those that do not show dispersive effects? Does it affect the results?

Page 12, lines 232-233: Does the data set include waves with negative skewness, which were discarded due to the constraint on the skewness being > 0.6?

Page 13 and Figure 8: It would be interesting to discuss whether the algorithm could affect the final distribution of skewness. In particuar, does the algorithm detect more efficiently highly-steepened waves, thus introducing a possible bias?

Page 15, lines 264-265: The authors state that the waves are well-defined for a ratio > 13.7. Could you please provide a reference for this threshold? Or is this an observation made from the present analysis?

Page 17, point 1: "Influences of extreme solar wind conditions can be seen in occasional sudden increases of the time between observation of two events." In my opinion, the present study does not provide sufficiently convincing evidence of the influence of extreme solar wind conditions to support this statement. I would suggest to tone it down to be more in line with the findings of the present work ("may be seen" instead of "can be seen" for example). Also, as it is now, it contradicts what is stated in the abstract (at lines 8-9).

Figure 15 and associated text: I am not sure I understand how the parameters displayed in Figure 15 are obtained. Did the authors run their model for all steepened wave observations in their data set? And similarly, did they calculate the associated

values for the resistivity and viscosity based on Eq 25 and 26 for each interval, using plasma observations made simultaneously with the detection of steepened magnetic field structures?

Lines 508-509: "This change of the interaction region is most likely caused by transient solar wind events, which is supported by the observation of a smooth simultaneous increase of the mean magnetic field." According to Figure 5, an increase of the background magnetic field is observed during thoses intervals, rather than the "smooth increase" described here, which reads as if the field strength changes progressively over the course of the event. I would suggest to reformulate this. It could also be interesting to add to the discussion that measurements from SREM could provide additional information regarding the solar wind conditions, and Enlil simulations could show whether transient solar events may be reaching the comet at these times (see for example the study by Witasse et al., 2017, "Interplanetary coronal mass ejection observed at STEREO-A, Mars, comet 67P/Churyumov-Gerasimenko, Saturn, and New Horizons en-route to Pluto. Comparison of its Forbush decreases at 1.4, 3.1 and 9.9 AU", doi:10.1002/2017JA023884). This additional analysis lies of course beyond the scope of the present study, but it'd be worth mentioning.

Lines 514-515: "The pattern the minimum variance direction exhibits resembles the general ion motion close to the nucleus." It would be interesting to discuss what are the possible implications of this finding regarding the source of the waves. Would this hint at one of the instabilities mentioned in previous studies?

Lines 519-520: "While the skewness increases with rising neutral gas density, the amplitude decreases" Would it be possible to distinguish between the increase in neutral gas density due to cometary activity and that due to the distance from the comet? If so, could this help in identify the source region of the waves, assuming that the skewness increases as the waves evolve and steepen with time?

Technical corrections:

Page 6, line 115: "implies" -> "is associated with" ("implies" suggests that there's a direct causation, which cannot be established on the sole basis of the present study)

Figure 10: The right-hand side of the right panel seems to be cut: there's a few bins missing to reach 180 degrees, with only a vertical line remaining around 170 degrees.

Page 15, lines 273-274: "the following analysis was only performed for the periods in which diamagnetic cavities were available to adjust the offsets." → "the following analysis was only performed for the periods in which observations of the diamagnetic cavity were available to adjust the offsets."

Page 17, point 4: "at an angle to the Sun" -> to the Sun-comet line? to the Sun-spacecraft line?

Line 418: "es" -> "as"

Eq 22: "s" should be "S"

Line 471: "discrepancy" reads as if this difference between the two parameters is an error, whereas it is actually an observation that is made here, based on the model, unless I am mistaken. I would suggest to reformulate this sentence, for example "η and ν differ by a factor of $\sim$ 1000"

Line 483: "to low" -> "too low"

Lines 507-508: "During this period occasional transitions into regions free of wave events within the span of 1 - 2 days were observed." This sentence reads as if Rosetta was probing a different part of the cometary environment, which didn't have such steepened waves, during these intervals. However, based on the presented analysis (and the next sentence) it is rather that the waves "disappear" from the cometary environment during these intervals. I would suggest to reformulate this sentence to better convey this.

Line 515: Again, the angles "to the comet" and "to the Sun" should be rather to the

"comet-spacecraft line" and to the "Sun-comet" or "Sun-spacecraft" line to be unambiguously defined.

Interactive comment on Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2020-84, 2020.