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Interactive comment on "Warm protons at comet 67P/Churyumov-Gerasimenko – Implications for the infant bow shock" *by* Charlotte Goetz et al.

Anonymous Referee #1

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This study performed a survey of warm protons at 67P which are studied due to their association with a newly formed bow shock. I think this survey has value. Such large data surveys are highly informative for qualitatively and quantitatively characterizing new environments and it is appreciated that these can be difficult to draw conclusions from when using a single spacecraft with no upstream monitor. I, however, do not believe the hypothesis or focus of this investigation is particularly well defined and, as it is currently written, the conclusions of the study are therefore not supported by the data presented. I am therefore unwilling to recommend publication until the following points are addressed:

- Line 65: More justification is needed as to why the warm proton flux is used as the main signal of the shock over other parameters, or over being used in conjunction with

other parameters. The discussion mentions reasons as to why other parameters are not sufficient, i.e. hot electrons might be missed. Such arguments should be moved up in the manuscript and used as an assumption and justification for using the warm protons. What other phenomena could produce warm protons?

- Line 155: The selection criteria is outlined as a 'pronounced decrease' in the proton energy and a 'broadening of the energy band'. Figure 2a however shows values above and below unity, does this not contradict the selection criteria? If vm,h in Figure 2 is different to the H+ (E/q) column in Table 1 this needs to be described. Some clarity is needed here.

- Line 200: Statistics such as Figure 2 (can (a) be quantitatively represented on line 200 with a percentage?), 60% electron energy increase on line 205, 68% on line 210 for the B field decrease, 52% on line 210 for the s/c potential decrease, indicate to me that the detection criteria for the Infant Bow Shock is not sufficient – i.e. the shock has not been persistently detected.

- Line 40: There isn't mention of an induced magnetopause or the plasma boundaries referred to in the first line of the abstract. Surely this should follow on from the introduction to how an observable shock forms due to increased mass loading. Where is the induced magnetopause expected to form in relation to where these events are detected? Could these events be associated with this instead?

- Line 160: The use of the spacecraft potential is I believe not appropriate. That the spacecraft potential follows the density is only true when assuming a constant temperature. This study is focused on studying a shock which will alter and heat the plasma.

- Line 240: This sentence is an evaluation of the hypothesis of whether these events are indeed the shock. 250 to 270 then appears to go into some, perhaps reasonable but certainly speculative, arguments about why the statistics are not good. The study therefore, with reference to line 345 and 355, seems to go on to imply that all warm proton detections are associated with the shock. This may be true but is not supported by the analysis presented. See point below:

- Line 345: The sentence "All parameters, except for the magnetic field magnitude, behave according to what was defined for the infant bow shock" doesn't seem to me to agree with the statistics presented in Figure 2 and the next sentence which states that only 10% agree. Some clarity is needed here.

- Line 355: The study concludes that the shock is an asymmetric structure persistently observed. This to me is in contradiction to the statistical results - surely the conclusions are that the shock is not persistently detected given many of the statistics are not much better than tossing a coin. It is this aspect of the paper that I feel is biased in a certain direct and therefore draws conclusions not fully supported by the data.

- Line 245 states that some events could be examples of phenomena other than a weak shock. What are the comparative phenomena? Line 130 mentions some other effects such as changes in solar wind velocity/density/cometary ion density but these aren't adequately described or referenced. This discussion on alternate production mechanisms for warm protons is central to the arguments being made in this manuscript and ties back to my first point regarding Line 65.

In addition to the above conceptual points I noticed the below points as I read through the manuscript which also should be addressed:

- Line 220: Heliocentric distance I think should be mentioned before now to orient the reader. How are the 370 events selected? How many Rosetta orbits are there in total outside the cavity? How many with good data that the 370 are selected from? Some explanation and context is needed here for reproducibility. I recommend changing Line 110 to include this information.

- Line 40: Can the "few percent" be better constrained or referenced with respect to 67P?

- Line 70: Should "easily" be "earlier" instead?

- Line 100: Can you please reference the "is believed".

- Line 130: What publications does the "(as stated in previous publications)" refer to?

- Line 159: Why is the following sentence interesting? "Interestingly, the flux diminishes at the same time that the proton energy increases gradually".

- Line 265: The description of the solar wind "pushing" the shock is not accurate, shocks are modes as opposed to pressure balanced structures. Rewording is needed.

- Line 308: I do not believe the authors have "made attempts to conclusively show that this structure is indeed a shock in the fluid dynamics sense." This is far too strong and I think would only be correct if sufficient effort had been made to deal with the RH jump conditions.

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