

Swarm Langmuir Probes' data quality and future improvements

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Reviewer's detailed comments

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Line 2:

« very accurate » needs to be quantified. One knows that LP measurements are not that accurate in absolute terms.

Line 4: Clarify the « operational use » that is being made of LP measurements

Line 9: Replace « conduced » by « conducted »

Line 9: Note sure that how the feed back from the user community is addressed in the manuscript. It should be noted that the intend to involve the feedback from the science community is excellent and will certainly contribute to the data product quality.

Line 12: add « in the derived plasma parameters » after « improvements »

Line 17: I am wondering whether the accurate determination of the plasma parameters contributes to the improvement of the magnetic field measurement analysis? If not, please ignore my question, if yes it would be interesting to say a few words on the subject.

Line 18: Replace « divers » by « diverse »

Line 31: Rephrase « insights on the space weather »

Line 33: Replace « reveled » by « revealed »

Line 41: remove the comma after « mission »

Line 41: I doubt the word « climatology » is the right word. Would « meteorology » be more appropriate? as it seems related to space weather effects.

Line 42: add the word « performed » after « variations »

Line 45: replace « this new statistics » by « This new statistical study »

Line 47: Precise « Antarctic » which usually refers to the continent

Line 59: Provide a reference to the « PLASMA operational processor »

Line 60: Is « PLASMA processor » the same thing as « PLASMA operational processor »?

Line 61: add a comma after « research »

Line 63: Precise if you refer to « data quality » or « Plasma parameters quality »

Line 68: Explain « admittance »

Line 69: Precise « given biases »

Line 71-73: The position of the LP sensor real close to the Satellite skin probably affects the accuracy of the spacecraft potential measurements (and probably to a certain extent plasma density and temperature) , as the LPs are most likely inside the plasma sheath of the satellite. The Debye Length of the effect should be discussed. This is a potential issue which is not addressed in the paper. Perhaps it was addressed in a reference publication. Its impact on the accuracy of the plasma parameters measurements should be addressed in this paper.

Line 74 (and 77): Replace « signal » by « current »?

Line 78: replace « interfere » by « interfer »

Line 82: What was the rationale for the gain switch between the two probes in 2019?

Line 84-86: The sentence « Out of concern »... ; please discuss whether if there is a difference in terms of degradation between Au and Ti due to Atomic oxygen.

Knowing the subject, I may add that, one of the concerns, was also the oxidation of TiN on ground.

Line 87: Explain what « testing before launch » was made

Line 90 and following

It's rather hard for the reviewer (and will most likely be hard for the readers) to follow the meaning of all acronyms used. A list of acronyms would be useful.

Line 103-104. I am myself not aware of a previous use of the HM method in space. I am wondering how the set point of the applied bias signal (and its amplitude) is selected and adjusted as satellite potential must significantly vary along the orbit. I note that it is written that « Sweep mode data are not used in the PLASMA processor... ». Has a comparison of the plasma parameters regularly obtainable from the I-V sweeps and the HM method been made.?

Line 112: Replace « sessions » by « sections »

Line 115 and following

Would it be useful to summarize in a table, the differences between the baselines 04 and 05?

Line 129-130. Clarify « second shunt resistor ». What's the role of the first one?

Line 132: The sentence « In practice the values often differ which we suspect is because of the different probe gain » is problematic. The sudden jumps mentioned in line 135 should be further discussed. Also, statement in line 138-139 casts doubts on the accuracy of the measurements at the gain transition.

It seems to point to the fact that the measurements performance in high and low gain are not fully understood, thus question the validation of the derived plasma parameters. It may be useful to look at this issue by looking at the regular sweeping I-V characteristics.

Line 139 and following: Provide details on the method used to derive the ion density rather than the electron density. This should be supported by modelling the ion sheath in the vicinity of the LPs. Assumption the $N_i = N_e$ at the LP location may be questioned.

Line 147-148: « .. the region (singular) . ; are (plural) « . Correction needed.

Line 147: « Larger » than what? use « large »?

Line 176: Replace « where » by « were »

Line 178; add «, the » after « 05 »

Line 179: Add « the » after « gaps »

Line 186: replace « today » by a specific date

Line 210: How « good » is good?. Quantify goodness.

Line 212: Add « the reader » after « refer »

Line 220. LP can't measure negative plasma densities!!!. The processing provides negative densities which obviously points to the limitations of the used algorithm.

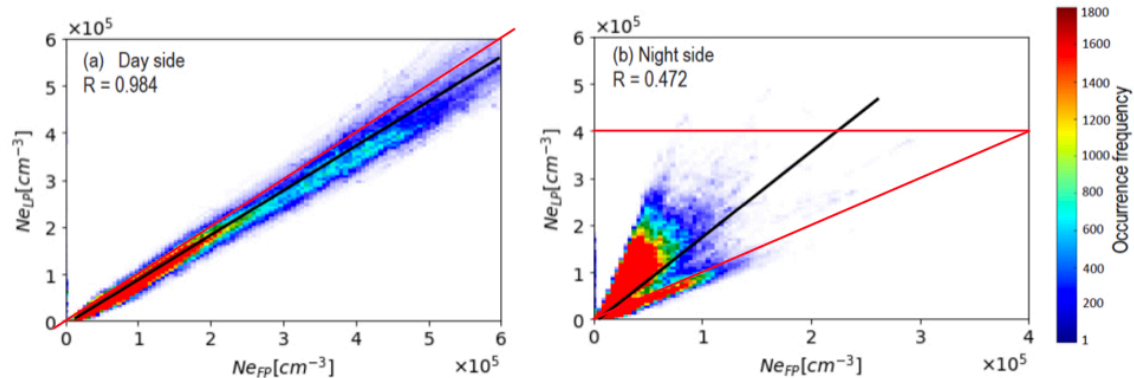
Line 221: Are measurements invalid or is the processing invalid?

Line 230 and following

Figure 8. Indeed, the correlation for the night side is low. Looking closely at the figure, I have explored in my own way the figure. There is a pretty good correlation on the night side for one of the lobes of the scatter point distribution. See figure below, where the red lines figure the 1:1 correlation.

On the day side, the correlation seems to be better for lower densities than it is for higher ones. In order to put forward a possible explanation, it would be required to know the nature of the surface coating of the FP, and be reminded of which of the LP is used (Au-coated or TiN-coated) for the figure.

The assumption that the plasma composition (O+ only) is relevant because the Ion density is determined rather than Ne. It would certainly be useful to show as well the determined Ne (which is independent of the ion composition)



Line 239-240. The statement: « the comparison between the LP and FP ... » is not well supported by the Fig 8 results on the night side.

Line 247-248. Indeed there are a few anomalies which would need to be further worked in order to validate the plasma parameters. A clear statement on the validation and the validity of the determined plasma parameters would need to appear in the conclusions (and on the data server). I am looking forward to the description of the LP calibration measurements in baseline 06.

Line 255-256. It would be needed to ascertain that the 20 000 K Te values are not a result of the processor being out of limit (as is probably the case for the negative densities). A correlation between the derived spacecraft potential and the setting bias value of the applied LP bias waveform would certainly be informative.

Line 267; It would be informative for the reader to provide the range of the specific solar illumination angle. For information I am aware of a paper (currently under revision, I cannot say more) that discusses LP « measurement peculiarities » at specific solar illumination angles at both the day-night and night-day transition.

Providing the solar illumination range when the anomalies occur would be useful to the reader.

Line 282. Earlier it is said that the LP calibration would be introduced in the baseline 06. What LP calibration are you referring to here in this paper?

Line 290.
It's hard to assess the improvement made in baseline 05. A table comparing the baselines would help.

Line 294-295:
« plasma density measurements are more accurate during higher solar activity » I can't remember a discussion earlier in the paper that allows this conclusion. Please expand.