

# ***Interactive comment on “Ionospheric Plasma Density Measurements by Swarm Langmuir Probes: Limitations and possible Corrections” by Piero Diego et al.***

## **Anonymous Referee #2**

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This paper presents analysis of potential deficiencies in the electron density measurements made by the Langmuir probes (LP) located on the Swarm satellites. After an extensive and very detailed description of the LP operation (very good and appropriate), the authors present the actual data for daytime and nighttime measurements of a single orbit. They compare the data with similar data collected on-board the Chinese satellite CSES-01, which crossed the equator at about the same local time. The comparisons show reasonable agreement between the instruments, provided that the Swarm data are corrected by two factors: 1) the plasma sheath widening effect on particle collection (more critical at high latitudes with larger Debye lengths) and 2) variations in the floating potential of the probe with respect to the satellite, along its trajectory. The data

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presented indicate that the corrections have to be significant; reduction by a factor of 5. The paper is important in that a significant Swarm data base is available and the data can be used, and has already been used, for various ionospheric studies.

I have serious concerns about this paper and I cannot recommend it for publication. Although the paper is well written and has many good components, the major results reported are highly unexpected and raise a number of questions. I hope my comments below will help the authors to improve the manuscript and re-submit the paper.

1). The most critical question is: what is the final conclusion of the paper? A validation paper needs a clear conclusion and I did not find one.

2). The paper cites the validation study by Lomidze et al. (2018) but never discusses the tremendous differences in the correction coefficients in that study and in the present paper. Lomidze et al. (2018) showed that the Swarm densities agree reasonably well with ionosonde and ISR measurements at middle latitudes, by considering two classical instruments that have never been questioned in terms of the data quality. The authors should explain their reasoning as to why the paper by Lomidze et al. (2018) can be ignored and new correction factors for Swarm electron density data need to be introduced.

3). Validation of the Chinese data is an open and unresolved issue. For this kind of paper, co-authorship, contributions or at least confirmation from the CSES-01 team are absolutely required, especially in view that serious doubts are expressed with respect to the Swarm data quality on the ESA website.

4). The paper clearly mentions that one measurement might be an anomaly and statistical assessment is needed (Lines 41-44). Then the value of the analysis of a single event is questionable. Statistical analysis is required to make a confident judgment.

5). Since the proposed corrections are quite drastic, does that mean that individual researchers cannot use Swarm electron density data directly from the ESA website?

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This needs to be commented on in the paper.

6). Since the proposed corrections are straightforward and follow the equations given, why not evaluate new “corrected” densities versus those on the ESA website? Comparisons with various models is another possible and reasonable step. These actions would be instructive in assessing by how much the Swarm data would have to be re-scaled, on average.

7). The Debye length factor is more important at high latitudes (Line 167). I expected that the authors would seek Swarm-CSES-01 conjunctions at high latitudes and not at equatorial latitudes. It is understandable that this might not be easy, or may be impossible. Comments are required on this issue as well.

8) In Figure 4, data for the nightside and the dayside are presented. I expected that the “point-by-point” comparison would be done on Figure 6, not only for the dayside but for the nightside as well. The reasons for the difference in agreement need to be discussed.

Technical issues:

I found that the authors use excessively commas, it makes reading more confusing. Below I report several items that the authors might wish to fix.

L15: extent

L23: LPs need to be spelled out as this is first appearance

L23: represent

L26,27: remove “free”

L36,37: remove “even though”

L38: ISR and ionosondes (these are different instruments)

L36: all altitudes

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L39: all latitudes

Eq (1): “k” needs to be introduced here and dropped in a couple of times later. For eq.(1), reference is required in line 80.

L90-95. Split on several sentences

L108: deviation

L109: et al.

L115: Reference is required here

L140: Knudsen et al showed how . . .

L144: q and V have been already introduced

L149-154: These statements do not prove that LP works correctly

L169: meaning of the statement is not clear. “saturation” comes out of blue

L179: fix SQRT

L180: “K” has already been introduced

L230: efficiency »> validity

L255: 1984;

L299: remarkable »> reasonable

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