

## REFEREE'S REPORT

on

Polar Wind Survey with TIDE/PSI Suite Aboard POLAR by Y.-J. Su, et al. JGR  
Manuscript 098.0068

It is good to see an analysis of TIDE/PSI data finally appearing in print. The plasma parameter values presented for low (5000 km) and high (8 Re) altitudes are a valuable addition to the rather meager polar wind database that has been accumulated to date and will serve to constrain future polar wind models. I recommend that the paper be published in JGR once the authors have adequately addressed the few issues described below.

First a couple of minor points. On page 11, a reference is made to the "occurrence frequency histograms" in Fig. 8, but Fig. 8 does not consist of histograms. Also, on page 17 line 17, I believe the authors meant  $\text{He}^+/\text{O}^+$  rather than  $\text{O}^+/\text{He}^+$ .

The authors use an iterative procedure involving a bi-Maxwellian distribution and starting with zeroth-order moments obtained from TIDE data to "fill in" the gaps in the measured  $\text{H}^+$  and  $\text{O}^+$  velocity distributions. The validity of all of the results presented in the paper depends on this procedure, but I do not think the procedure is adequately described. Perhaps my inexperience with data analysis is to blame here, but it was not clear to me (and probably would not be to many readers) how the procedure works. How are "zeroth-order" moments extracted from the raw data? How are "first-order" moments derived from the bi-Maxwellian? Wouldn't integration over the bi-Maxwellian function simply give the zeroth-order moments back again? How many iterations are necessary to reach convergence? The authors should elaborate.

A question related to the one described above concerns the  $\text{H}^+$  distributions before (Fig. 2b) and after (Fig. 3) "filling" by the iterative procedure. The central region of the distribution is apparently filled in with a relatively high density of particles, as would be expected and hoped for. The gaps in the outer regions (higher velocities) of the distribution in Fig. 2b appear, in Fig. 3, to simply be filled in with a uniform low density of particles, not at all what I would expect if the entire distribution were being constructed based on a bi-Maxwellian (or any other distribution for that matter). Again, this needs to be explained in greater depth.

While the results of this study constitute a valuable addition to our knowledge of the polar wind, the results are based on a limited number of satellite passes at one time of year and at one point in the solar cycle. How did  $K_p$  vary over the time interval encompassing the satellite passes? While these limitations can be surmised from a careful reading of the paper, I think the authors should be completely explicit about the seasonal/solar-cycle/etc. limitations of their study by stating these limitations both in the abstract and in the discussion section of the paper.

**Journal of Geophysical Research - Review Form**

**Manuscript No. 98.0068 “Polar Wind Survey with TIDE/PSI Suite Aboard POLAR” by Y.-J. Su et al.**

1. *In your opinion, does this paper describe interesting and substantial new results? If yes, briefly describe their nature and potential impact.*

This paper presents several new and interesting results. For example, it presented the physical parameters (density, ...) of the different ions ( $H^+$ ,  $O^+$ ,  $He^+$ ) at both high (8 Re and low (5000 km) altitudes. This detailed study fills many of the gaps in the observational literature of the polar wind. The results presented here puts important constraints on the models of the polar wind. For example, it is an important step toward resolving (via modeling efforts) the relative significance of the polar wind and the cusp region as sources of the different ion species above the polar cap.

2. *In your opinion, does this paper adequately put the progress it reports in the context of previous work? (This includes both representative referencing as well as introductory discussion.) If no, suggest possible improvements.*

The authors did a good job in putting their work in perspective with respect to the previous work. However, I suggest the following improvements in that regard:

a) Discuss the ion temperature anisotropies predicted theoretically (e.g., Demars and Schunk, *Rev. Geophys.*, 25, 1659, 1987; and *Planet. Space Sci.*, 37, 85, 1989). This is especially relevant to the observed anisotropies at low and high altitudes that were presented here.

b) The authors emphasize (correctly) the need for a 3-D time dependent model. They should give a more adequate discussion of the work that was done in that direction (e.g., Schunk and Sojka, 1997, 1989; Demars et al., 1996, 1998).

c) The authors should **discuss** other **relevant** observations **such as** Persoon et al. (*J. Geophys. Res.*, 88, 10123, 1983) **and** Pollock et al. (*J. Geophys. Res.*, 95, 18969, 1990), **and** compare **these works** with the **results** given here.

3. *Is the paper clearly and concisely written? (Note it is not necessary to include every detail to be "clear.") If no to either, suggest possible directions for improvement.*

The **paper** is very **well written**. Although it is quite **large** in **size**, **all** of **its** contents are **relevant**, especially for **helping** the future **modeling** efforts. The authors might consider **dividing** the **work** into a **series** of **two papers**, or **replacing** the figures (7, 8, 9, 11, 12, 15, 16, 17, 18, 19, 20, 21, 22, 23) by 2 to 3 **tables** that **list** the mean, standard deviation, and range for each **variable**.

4 *Will readers outside of the specialty of this paper be able to appreciate at least the motivations and general conclusions of the reported work?* **Yes**

### **Additional Remarks:**

I found this paper to be very interesting. I strongly recommend it for publication in JGR. However, I recommend the revised version to accommodate the following remarks, as well as the ones mentioned in the attached review form.

1. There **is** no discussion of the limitation of the measuring **technique**. **It is essential to discuss** these limitations to help the reader **interpret** the **results** properly.
2. The authors mention implicitly (and even explicitly sometimes) that their results support that the  $O^+$  above the polar cap is mainly supplied by the cusp region (ion fountain) The ion fountain model produces **results** that **disagree** with the observations (in this paper) at least as much as they agree with them I believe that the discussion on the relative importance of the two ion sources (cusp and polar cap) can only be decided by a modeling investigation that considers the observational constraints given here. I suggest that the authors should state, more explicitly, that this issue has not been resolved yet.
3. It is not clear how the residual potential (while the PSI was turned on) was estimated. The authors should discuss this issue,
4. Page 21, lines 8-11: The "fountain effect" injects ions at altitudes much higher than the ring where  $H^+$  is produced via resonance charge exchange (about 1000 km). This means, on the contrary of the author's suggestion, the  $O^+$  fountain ion would not significantly control the  $H^+$  density above the polar cap.
5. Page 22, line 3 (from the bottom): Sojka, 1989, should be Schunk and Sojka, 1989; with the corresponding change in the list of references.
6. Page 30, line 5 (from the bottom): gray-scare should be gray-scale.