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Interactive comment on "Terrestrial ion circulation in space" *by* Masatoshi Yamauchi

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Reply to Reviewer 1's comment

Thank you again for valuable comments to improve the manuscript. While my intention of general direction of the revision is already described in an independent comment file, I hereby outline how to improve based on individual comments.

* English: Institute's native speaker promised to check the manuscript after I myself will double-check it.

* Roles of electrons: After the scope of this paper is explained in the extended introduction (as described in the separate answer), I will add a statement that this paper does not mention the acceleration mechanisms or simulations, like: "Acceleration mechanisms for the outflowing ions such as wave particle interaction, electrostatic field, centrifugal acceleration, are not covered in this paper and hence roles of electrons are not discussed. Similarly, numerical simulations are not covered."

* Outline expected from Title: I will expand the introduction to justify the outline (as described in the separate answer). I also consider making the title more specific, like: "Terrestrial ion escape and relevant circulation in space" or "Ion loss routes from the Earth"

* Cold refilling: Thank you for pointing out my failure. The field-aligned velocity of order of 1 km/s observed by these references will be mentioned in §2.1. I will also move the estimated total outflow (refill) flux from §5.1 to §2.1.

* Polar wind and suprathermal are well discriminated: Thank you for pointing out my failure. I will replace the paragraph, like: "The source of these supersonic cold ions are most likely the polar wind because the velocity is similar to the prediction of the polar wind models and observations (Pollock et al., 1990; Yau and Whallen, 1992; Moore et al., 1999a). These observations show substantial O+ content with slower bulk velocity compared to H+. The difference from the Cluster observations (no O+) can be explained by the velocity filter effect, by which slow O+ are convected toward lower latitude than fast H+ (Chappel et al., 1987). Also slow O+ stay at low-altitude, where the wave-particle interaction is the most active, much longer than fast H+. This makes O+ energized to suprathermal energy range (next subsection) and detectable by standard hot ion instruments. "

* Figures 2 (Freja) and 14 (Viking): I will drop Figure 14 (and even Figure 4 of Prognoz-7 if needed). In the original manuscript, I included them because they are published in subscription journals which are not open access. For Figure 2, I prefer to keep it if space is available because this is still the only event when both outflow and inflow (assuming north-south symmetry) are detected, showing mass-independent energization without the help of statistics, and because the accumulated knowledge from Cluster

made the presented interpretation (as one of 5 possibility in 2005 paper) solid. If I need more space, I will remove this figure. I will add a statement that Cluster contributed the interpretation.

* line 88-91 (Simultaneous observations by TIMAS on Su et al.): Thank you very much for additional information for higher energy on Figure 14 of Su et al. (1998). I will remove this energy threshold argument.

* Figure 3: I will cite these papers explicitly (Schilling paper shows the solar wind dependence found at low-altitude (citations) are valid even for escaping ions to the space in the plasma mantle).

* Figure 5 (summary drawing): I will sub-divide auroral region and dayside, while I keep the same structure, because (1) I selected the most important route in estimating the total loss rate and (2) the suggested classification by the source has been done in many papers while only few works have been done with a classification by destinations that is important for total loss estimate.

* Figures 7-9: All figure are used in sections 4.3, 4.4, and 4.5. Figure 8 and Figure 9 (and could even Figure 7) will be moved to these sections.

* §4.4 Direct injection: By using Figure 7 here (and mentioning the lay-tracing in the original paper), I will mention that Cluster has shown a case of outflowing O+ without any bounce has finite PA outside the loss cone. Figure 9 also show the direct injection from above the auroral bulge with wide pitch angles at the equatorial plane.

* §4.7 Loss process: I will add the role of the reconnection, like: "This leaking process does not require local magnetopause reconnection (Sibeck et al., 1987; 1999; Sauvaud et al., 2001; Marcucci et al., 2004), while it is most effective during strong sunward convection sustained by nightside activities or during magnetopause erosion, both of which take place during increasing solar wind dynamic pressure and southward turning of IMF, and hence, very often coexists with the local magnetopause reconnection (Toledo-Redondo et al., 2016)"

* §5.2(a) O+ escape effect on solar wind interaction: I will remove the Axford reference (viscous like interaction), and add the geopause, like: "The concept of such second openness, i.e., two-time definition of the open-closed boundary, in two-time definition of the open-closed boundary,the open-closed boundary, first by the solar wind access point to the magnetosphere, and second by the access point of the outflowing ionospheric ions to the solar wind, is similar to multipleâĂŘbranch discontinuity model by Vasyliunas (1995) and geopause concept by Moore et al. (1995) because prosent model can be achieved by replacing the upward propagation of electrodynamic information of the ionosphere by ion motions. "

* §5.2(b) The effect on the reconnection rate: I will add Fuselier et al. (2017) at just before §5.1, like: "Along this context, dayside part of the consideration of the O+ outflow has also been improved by treating the cusp outflow separately (Glocer et al., 2018), or by considering mass loading at the low-latitude dayside magnetopause (Fuselier et al., 2017)."

Since the proposed process is independent from reconnection, the word of "reconnection rate" was used just for comparison of efficiency, and since the nearby reconnection from this mass loading region is anti-parallel reconnection in which the zero magnetic field cannot stop the solar wind inflow whereas Fuselier's works concerns component reconnection which is driven by compression (more close to nightside reconnection), I am afraid referring Fuselier (2019) may cause a misleading. I simply refer Sonnerup (1974) for the reconnection rate.

* §5.2(c) Simulation including O+: Thank you many for suggested papers. This section deals only the direct feedback from the mass loading in the cusp-mantle region, while existing multi-fluid models considered feedback trough magnetotail (e.g., Wiltberger et a., 2015, 2017; Liemohl et al., 2016, 2018; Welling and Liemohl, 2016). Even Glocer et al. (2018) with good consideration of the cusp outflow did not treat such local

feedback. Therefore, I will mention only Winglee's work (pioneer on this matter) and Glocer's work.

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