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Journal Name:	**Physical Science International Journal**
Manuscript Number:	**Ms_PSIJ_29209**
Title of the Manuscript:	**Ion cyclotron (IC) oscillations excited by nonlinear waves propagating in collision-free auroral ionosphere**
Type of the Article	**Original Research Article**

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PART 1: **Review Comments**

	Reviewer's comment	**Author's comment** *(if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)*
Compulsory REVISION comments		
Minor REVISION comments	The paper is an interesting research devoted to very complex problem, interpretation and explanation of the results of satellite measurements in the ionosphere. The paper can be published. But, to make the text more readable and to be useful for more readers, some improvements and explanations should be done. 1. Pp. 1,2. The historical comments are interesting, but they occupy a lot of place. It is better to reduce them and to explain in more details what the electron solitary wave is. Namely, what kind of soliton it is (whistler-like? pulse? envelope? It seems that the pulse-like), what a possible origin is, detailed parameters. Some Figure would be useful that describes the distribution of the electric field and the variation of density within the soliton, as an additional part of Fig. 1.	Thanks for the comments! 1. Dear referee, this paper aims at demonstrating the origin of IC oscillations due to the presence of nonlinear waves propagating in the collision-free (usually at altitudes of and above F) auroral ionosphere. This is an innovative topic out of my PhD research. The present paper is one of a series of publications. The most closely related peer-reviewed papers are Ma and Hirose (2009,2010), and Ma et al. (2009). Because the theme of the topic is unique by arguing that it is the nonlinear mechanism, rather the commonly accepted linear one, which is responsible for the IC oscillations, I have to describe the background in details in the Introduction section to offer readers an extensive and intensive overview on the previous theoretical and observational studies which had already provided evidences and considerations of such a subject, however, without proposing a physical model in depth. This is why the historical comments in the Introduction deserves 5 pages out of a total of 23 pages. For the sake of readers, I sincerely apply to keep the 5-page historical review unabridged. But I reorganized and streamlining here and there in the revised version, along with updates as mentioned in the following. I am very in favor of referee's advice about (1) what the electrostatic solitary wave is; (2) what kind of soliton it is; and (3) what a possible origin is. All the answers are given in the Introduction: The answer to question (1) is given on Page 4, the whole middle paragraph which clearly defined that electrostatic solitary waves are nonlinear electric field structures ... carrying space-charge electric fields: they are "space-charge carriers which contribute strong transverse electric fields to space plasmas". At the beginning of the following paragraph, it is stressed that

	2. Are the 'clump' and the 'soliton' are	"It is now clear that the electrostatic solitary waves are solitary structures that behave as are space-charge carriers to contribute strong transverse electric fields to space plasmas," as shown on Page# 6 of the revised manuscript. The answer to question (2) is given by Chiueh & Diamond (1986), as summarized in the last paragraph on Page#5 and the first paragraph on Page#6. In the revised manuscript, I clearly stressed this point in the introduction of Chiueh & Diamond (1986)' work: In the IC mode, "this new ingredient is space-charge ``clumps", the so-called electrostatic solitons, which describes the incomplete blending of a Vlasov plasma: wave-particle interactions make stochastic orbits of particles by turbulent electric fields; the phase-space density tends to decrease to smaller scales in a finite time, and thus generate phase-space space-charge density granulations (that is, clumps); these clumps ballistically propagate at the resonant velocities in a finite time. See the lower Page 3. The answer to question (3) is also given by Chiueh & Diamond (1986). I summarized the theory at the end of Page#5, following the answer to the last question in the revised manuscript: "In the development, the turbulent forces produced by the turbulent electromagnetic field tend to tear the space-charge chunk of particles apart and cause the decay of the clumps. However, the size of the space-charge structures is so small that they keep every particle feels the same force. Thus, the elements retain their structural integrity for a relatively longer time than the average correlation time of the system." See the end of Page 3 and the beginning of Page 4. In addition, by following referee's advice, I updated Figure 1 by inserting an additional part to show the distribution of the cylindrically symmetric electric field which increases linearly in space away from the center. This field is produced by the spatially uniform charge density which is constant within the soliton tube and this constancy does not need a separate panel to show it. The validity of the model was discussed in a peer-reviewed paper, Ma et al. (2009). 2. I admire referee's resourcefulness! Yes, the 'clump' and the 'soliton' are synonyms. However, the concept of "clump" is in view of Chiueh & Diamond's theoretical study, but the concept of "soliton" is in view of satellite observations. Thus, we have to use them separately in different fields. To stress this point to readers, I indicated again in the Conclusion part of the paper by saying that "After Chiueh & Diamond predicted the so-called

	synonyms? If yes, please use a single term.	"space-charge Clumps", numerous high-resolution observations (e.g., FAST, Polar) confirmed the existence of the structures which were nothing else but the nonlinear electrostatic solitons (Matsumoto et al. 1994)." 3. it is 1 kHz, the envelope is a series of spiky structure. This detailed information was provided by Ergun et al. (1998c) and is summarized on Page 20. It is not a terminological misprint.
	3. What is the 'soliton frequency', p. 20? Are these solitons envelope-like? If yes, please write down their frequency directly. But it seems rather like a terminological misprint. 4. It is better to insert into the beginning of Section 2 'Physical Modelling…' a brief description of the ionosphere parameters (F-layer).	4. Thanks! By taking referee's advice, the following description is inserted at the suggested position of the beginning of Section 2 on Page 7 to show the ionospheric parameters in and above the F-layer: The background to set up such a model is identical to what was presented in Ma and St-Maurice (2008,2015) where we focused on the auroral region which is in and above 140 km, well with the F-layer. In this region, numerous data were collected by satellites, radars and rockets which exposed the existence of solitary waves, as introduced in the above Section. Based on previous studies, this region owns a magnetic field of 0.5 Gauss; ion temperature of 1000 K; stochastic transverse electric fields up to an order of 50 mV/m or greater; an ion-neutral collision frequency of an order of 0.01-1 Hz; and, ion gyrofrequency of an order of 50 Hz. See Ma and St-Maurice (2008,2015) in details.
	5. The authors consider the case without any collisions. But, as a principal result, a change of the ion temperature is pointed out. This is interesting, but needs more comments, see p. 12, Eq. (32) and below. Specifically, the transition from Eq. (32) to Eq. (33) needs more explanations.	5. Yes, the present paper considered only collision-free case. As pointed in Ma & St-Maurice (2008), the collision effect has only to be included if the time scales under consideration are longer than the collision time, which is in the order of 1 s in the F region. However, the ion gyro-period is merely in the order of 1/1000 s << 1 s. This is why the paper did not take into consideration the collision effect. It is true that a change of the ion temperature is changing. The transition from Eq. (32) to Eq. (33) employed a so-called backward mapping technique which was explained in great details in Ma & St-Maurice (2015). To explain the transition, I add the following on Page : Here, we employ a "Backward mapping" approach to transform this initial distribution f0 to the final solution of the Boltzmann equations. As introduced in details Ma & St.-Maurice (2008, 2015), the core of this approach lies in the

	6. Please remove some misprints like …Besides, for a plasma density n_0 ~ 5.7 cm^{-3},…	task of finding an explicit connection between the initial phase-space state, {r0, v0, t0}, and the final one, {r, v, t}. Luckily, this relation can be obtained if we solve the set of differential equations of motion as done in Ma & St.-Maurice (2008): there are two identical solutions for the same set of equations of motion (Gartenhaus 1964): one provides {r, v, t} expressed by using {r0, v0, t0} (forward mapping), while the other provides {r0, v0, t0} expressed by using {r, v, t} (backward mapping), whereby the description of the characteristics of motion is traced backwards but is otherwise unaltered. We use the latter method to solve fi(r, v, t). This method allows us to relate the 6-dimensional phase point {r, v} at any time t to {r0, v0, t0} and to therefore find the distribution at time t, since the initial distribution is already fully known, as given by Eq.(32). Finding the distribution function is then just a matter of expressing r0 and v0 in terms of r, v, and t in the expression for the initial condition f0 …... 6. Thanks! It was updated.
Optional/General comments	The paper can be published after some improvements.	