Review report on “Using the Galilean Relativity Principle to understand the physical basis for magnetosphere-ionosphere coupling processes” by Anthony J. Mannucci et al.

General comments

The authors discuss the problematic interpretation of the electric field in ionospheric electrodynamics, using the “Galilean Relativity Principle” which is a limiting case of the theory of special relativity. While this assertion seems reasonable, the paper includes unacceptable reasoning as I comment below. Also the content of the paper is generally “reinterpretation” of the previous work, without much of scientifically new. However, I think the problem presentation by the authors is useful to the space science community, because such discussion cannot be found in textbooks nor in journal papers. I would like to recommend publication after revision in response to the following comments.

Specific comments

(1) Although I agree with the authors on the conventional problematic interpretation of ionospheric electrodynamics in terms of the electric field, I disagree with the authors’ conclusion that the magnetospheric currents that close in the ionosphere play no role in the magnetosphere-ionosphere coupling (on lines 25-27, lines 414-415, lines 438-440, lines 454-490, lines 522-524).

We agree that we should not be suggesting that these currents play no role in the coupling. Rather they are important “by-products of momentum transfer” that occurs for other reasons than the currents themselves. We discuss this topic further in response to the referee’s next comment, where we seek clarification. In the revision, we should remove language that suggests “no role”. Lines 414-415 could be removed. In lines 438-440, we should simply remove the phrase “and are not fundamental to the causal chain that couples the ionosphere to the magnetosphere”. For lines 545-490, we are referring specifically to the momentum argument, which is further discussed below. We should modify the last sentence in this part to read as follows: “We conclude that momentum changes associated with neutrals are larger than the momentum carried by FACs entering from the magnetosphere…The changes in neutral momentum are due to collisions with ions~~, not due to the field aligned currents~~.” The lines 522-524 can remain if our momentum arguments are correct and relevant.

This conclusion is drawn on lines 452-490 by comparing the kinetic momentum of current-carrying electrons with the kinetic momentum of neutrals. I cannot understand the authors’ logic employed. The density of electromagnetic momentum carried by the field-aligned currents is given by

$$\mathbf{p}_m = \frac{\mp \frac{1}{\mu_0} B_0 \Delta \mathbf{B}_\perp}{V_A} = \mp \sqrt{\frac{m_i n_i}{\mu_0}} \Delta \mathbf{B}_\perp \quad \left(\begin{array}{l} \text{the upper minus sign applies to the northern ionosphere} \\ \text{the lower plus sign applies to the southern ionosphere} \end{array} \right)$$

and can be comparable to the momentum density of neutrals ($\Delta \mathbf{p}_n$). In fact, in the case of a magnetospheric dynamo, what causes a nonzero relative velocity between ions and neutrals ($\mathbf{u}_i - \mathbf{u}_n$, which is initially zero) is the ion acceleration by the $\mathbf{j} \times \mathbf{B}$ force in the ionosphere. The ions are accelerated until the $\mathbf{j} \times \mathbf{B}$ force balances the collisional force $-m_i n_i \nu_{in} \left(\mathbf{u}_i - \mathbf{u}_n \right)$.

We appreciate this comment from the referee. We would benefit from a reference to the above equation for $\mathbf{p}_m$ so that we are sure to understand its derivation. Electromagnetic momentum is usually proportional to ~$\mathbf{E} \times \mathbf{B}$ (e.g. Jackson, 1975, equation 6.125). The above expression might be derived by relating the electric field to a current that generates the perturbation magnetic field. However, we are not sure where the perturbation magnetic field is evaluated in this expression. The perturbation magnetic field will in general not be spatially uniform?

We note that FACs might also carry momentum due to the motion of the particles themselves, and not solely via the electromagnetic field. Momentum conservation is discussed in the paper by Vasyliunas (Annales Geophysicae, 2007 doi: 10.5194/angeo-25-255-2007) starting with momentum conservation (their Equation (1)). The linear momentum per unit volume is defined (their Equation (2)) as consisting of an electromagnetic term and a term proportional to the mass density and bulk flow of the medium. Just above their Equation (3), it is stated that "Under the usual assumptions of charge quasi-neutrality of the plasma, nonrelativistic bulk flows, and Alfven speed [much less than the speed of light], the electromagnetic contribution to the linear momentum density (second term on the right-hand side of Eq. (2)) and the electric-field terms in the Maxwell stress tensor can be neglected." We are not suggesting that electromagnetic momentum can be ignored in the specific example of FACs that the referee has mentioned. However, we are seeking guidance on this subject.

Our own calculation of the term $\mathbf{p}_m$ above yields a value of $\sim 4 \times 10^{-12}$ using a perturbation magnetic field value of 100 nT and other values listed in the paper (lines 463-480). This is about a factor of 10 smaller than the representative value we use in the paper for the change in neutral momentum ($\sim 3 \times 10^{-11}$). However, we agree that larger perturbation magnetic fields have been observed (~1000 nT) that could result in comparable values between $\mathbf{p}_m$ and the neutral momentum change. Thus we are very interested to have more details on the derivation of $\mathbf{p}_m$.

We do not understand the comment regarding the magnetospheric dynamo. The jxB force is the result of the magnetic component of the Lorentz force. The magnetic component of the Lorentz force does not increase the speed of plasma particles. It only changes their direction. So, we are not sure how the jxB force results in accelerating the ions so that they achieve speeds to balance the collisional force.

(2) In all descriptions in this paper, an initial finite $\mathbf{u}_i - \mathbf{u}_n$ with $\mathbf{j} = \mathbf{0}$ seems assumed. This is the case of ionospheric dynamo (neutral dynamo). At the same time, however, the case of magnetospheric dynamo (magnetospheric flow increases suddenly while $\mathbf{u}_i - \mathbf{u}_n = \mathbf{0}$ and $\mathbf{j} = \mathbf{0}$ in the ionosphere) is often discussed in parallel or in mixture. This is very confusing, and sometimes the reasoning is incorrect. Examples are on lines 291-292, lines 419-421, lines 429-436, and lines 451-452. The authors should separate the discussion.

Thanks to the referee for pointing out this potential source of confusion. We should clarify the presentation. In lines 291-292, we are referring to the "ionospheric dynamo" without reference to how the initial velocity difference was created. Ultimately, it must have arisen due to increased magnetospheric flow. We could remove this statement, as it is not really needed. We also agree that lines 451-452 are may cause confusion and can be removed.

(1) (Minor comment) On line 431, "Through flux conservation": What kind of flux do the authors mean?

Thanks to the referee for pointing this out. In a revision, we will refer to the "frozen-in flux" condition that occurs in collisionless plasmas (as described in Bellan's text on plasma physics). The frozen-in flux condition is why the plasma can move collectively and transfer momentum from one location to another, despite there being no collisions between the constituent particles.

(2) (Minor comment) I cannot understand the statement on lines 506-507. In what context did the authors add this statement?

We appreciate that the referee pointed this out. This statement should be explained in more detail. It is based on the literature on Galilean electromagnetism. The context can be found, for example, in Preti et al. (2009). Preti et al. apply the "standard" Galilean transformation equations (our Equations (3) and (4)) to the four Maxwell's equations in differential form (Preti's Section 3.2). The conclusion is that Gauss' and Ampere's laws (divergence of E, and curl of B, respectively) are not invariant under the Galilean transformation. This contradicts relativistic invariance, demonstrating the problematic nature of Galilean electromagnetism as typically applied. We can expand on this point in the revision.

Technical comments

None. The manuscript is well written.