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Interactive comment

Interactive comment on "Dynamics Geomagnetic Storm on 7–10 September 2015 as Observed by TWINS and Simulated by CIMI" *by* Joseph D. Perez et al.

Anonymous Referee #1

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[GENERAL COMMENTS]

This paper presents the equatorial plasma pressure distributions obtained by the TWINS observation and by the drift kinetic simulation CIMI for the moderate storms of 7-10 September 2015. The general features of the plasma pressure in the inner magnetosphere are similar to each other, whereas some differences are found in terms of peak location, anisotropy, and spatial distribution. The authors attributed the differences to the shielding effect and spatially-localized, short-duration injections of hot plasma.

The direct comparison between a sophisticated observation and an advanced drift ki-

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netic equation is highly valuable, and is promising to overcome the difficulties arising from in-situ satellite observations. The provided data is basically very interesting, and

I admire the authors' efforts to derive the pressure and anisotropy.

The authors thank the referee for the thoughtful and helpful comments.
We respond positively below to each question and comment individually. Nevertheless, we cannot answer every question posed by referee with a full, unambiguous explanation. Such requires extensive investigations that are underway but are beyond the scope of this paper. We believe what many have said, i.e., good research raises at least as many questions as it answers.
Our responses are shown in bold font for ease of distinguishing our responses and the referee's comments.

However, I have 3 major concerns as follows. First, the physical interpretations made by the authors are unclear. Because of the lack of proper interpretations, I cannot catch new scientific knowledge, or insights in the current version of the manuscript.

We certainly hope that our responses are sufficient so that the referee and other readers are able to "catch new scientific knowledge or insights. . . "

Secondly, the reliability of the plasma pressure obtained by TWINS is also unclear. The spectral shape of the ion flux is almost the same at 4 different points, which seems unlikely to occur.

The intent of showing the spectral shapes in Figure 10 was not to present an extended discussion of the spectra but rather to indicate that were two peaks, one at low energy, i.e., below 20 keV, and a second near 40 keV and to illustrate why the paths of 40 keV particles are shown. We are not sure that this is unlikely to occur. Details of the magnitude and shape of the energy spectra will be addressed in an extensive study that is underway.

As to the overall reliability of the plasma pressure, that is a difficult question to answer definitively and quantitatively. In the description of the methodology used to obtain the ion pressure from the TWINS ENA images, we have referenced the extensive testing of the ion distributions obtained from the TWINS ENA images. (See Section 2.2, lines 190 ff in the version of the paper where corrections are accepted and lines 194 ff in the version of the paper where corrections are marked.)

Thirdly, the plasma pressure mentioned in this paper is "partial" so that the "true" distribution of the plasma pressure would be different. Careful description is needed when the authors intend to say the distribution of the pressure.

The referee is correct. What we show in this paper are "partial" pressures. The intent is to have the TWINS and CIMI results consider as nearly as possible a similar energy range. In the revised paper, all references to the particular pressures from TWINS and CIMI are now designated as partial pressure.

[SPECIFIC COMMENTS]

(1) On the interpretations. The authors concluded that the difference between the ob- servation and the simulation can be best explained by enhanced electric and magnetic shielding and/or spatially-localized, short-duration injections. First of all, please explain the meaning of the electric and magnetic shielding in more detail. Most of the readers may not understand the meaning of it. The electric shielding is supposed to result from the ionospheric electric field redistributed by the Region 2 field-aligned current.

Excellent suggestion. We include in the revised version the following explanation as to what we mean by electric shielding. (See Section 6, lines 436 ff in the version of the paper where corrections are accepted and lines 442 ff in the version of the paper where corrections are marked.)

What is the magnetic shielding?

“magnetic shielding” was an improper term. Better to say enhanced electric field shielding and/or induction electric fields caused by spatially-localized, short duration injections. The term is omitted in the revised version.

What is the expected effect of the shielding on the pressure distribution and pressure anisotropy?

It has been demonstrated that the electric shielding can affect the ring current morphology. (See Section 1, lines 77 ff in the version of the paper where corrections are accepted and lines 80 ff in the version of the paper where corrections are marked.) The purpose of this paper is to show much more explicitly what are the expected effects. We are currently undertaking a project to couple the CIMI code in the inner magnetosphere with a 3D hybrid code that simulates the rest of the magnetosphere. We expect the results to address these issues in even greater detail.

CIMI/RCM takes into account the shielding. What physical processes or parameters does CIMI/RCM need to consider properly to explain the observations?

The CIMI simulations presented in this paper do not have localized injections into the inner magnetosphere. This has been demonstrated to have an effect on the pressure distributions. (See Section 6, lines 520 ff in the version of the paper where corrections are accepted and lines 530 ff in the version of the paper where corrections are marked.) While the CIMI simulations in this paper do include some contributions from induction electric fields, the Tsyganenko magnetic field is not updated on short enough time scale to capture all of the induction electric fields. We, of course, do not know whether the steps we are taking to provide localized and short term injection effects will answer all the questions.

Have the authors tested CIMI/RCM with different conditions/parameters to explain the observations?

There have been investigations that demonstrate that changing the input at the boundary of the CIMI simulations does impact the ring current morphology. Also it has been shown that injecting non-isotropic pitch angle distributions impacts the parameter of the ring current. (See Section 6, lines 509 ff in the version of the paper where corrections are accepted and lines 516 ff in the version of the paper where corrections are marked.) The authors have not tried, however, to reverse engineer the input to the CIMI simulations presented in this paper to attempt to match the data.

Secondly, please explain the expected effect of spatially-localized, short-duration injections on the pressure and anisotropy.

As described above and is illustrated to some extent in previously referenced work, the authors expect that spatially-localized, short-duration injections will impact the spatial and temporal locations of the pressure peaks. We also expect it to be a key factor in explaining the observation of multiple peaks in the ring current.

Have the authors modeled spatially-localized, short-duration injections to explain the observations?

As stated above, it has been demonstrated that spatially localized injections can affect the ring current morphology, but we have not tried to match the observations for this particular storm without some experimental or theoretical guidance. There is an ongoing investigation to couple CIMI with a 3D hybrid simulation of the injections from the tail explicitly intended to address this question.

Thirdly, please explain the reason why the CIMI result always shows parallel anisotropy of the plasma pressure in the dawn-midnight-dusk region. The pressure anisotropy is largely different from the observations. Detailed explanation is needed.

An explanation is given in the revised paper. (See Section 6, lines 490 ff in the version of the paper where corrections are accepted and lines 495 ff in the version of the paper where corrections are marked.) Whether this is the complete explanation is uncertain at this time. The authors expect to develop a more definitive explanation as part of an ongoing investigation to couple CIMI with a 3D hybrid simulation of the injections from the tail.

(2) On reliability of the plasma pressure. In Figure 10, the differential fluxes of the ions are shown as a function of energy at 4 points. The intensity of the flux is different but the spectral shape is almost the same with each other. Why is the spectral shape of the flux almost the same at the 4 points? According to in-situ observations, the spectral shape of the flux depends on L-value and magnetic local time (e.g., Milillo et al., 2001, 10.1029/2000JA900158), so that it seems kquite unlikely to be the same spectral shape at 4 points. Please explain the validity of the spectral shape of the flux and the plasma pressure distribution presented in this paper.

We reference the published examples of validation of all the features of the ion distributions through comparisons with in-situ measurements. (See Section 2.2, lines 190 ff in the version of the paper where corrections are accepted and lines 194 ff in the version of the paper where corrections are marked.) These examples were chosen because the satellites happen to be in the right place at the right time to see the features of interest. It is true, of course, that such comparisons do not guarantee the complete validity of the current results.

The main reason for showing the measured energy spectra was to show why the paths of 46 keV ions were used to display the location and time of the injections of the ions that had 46 keV energy at the peaks. What the authors think has the relevant validity in this case is the high energy a low energy peaks in the spectra.

The outstanding work by Millilo et al , 2001 will make an important contribution to our current investigation into the details of the energy spectra during geomagnetic storms during the TWINS mission. The fact that the average results presented in their paper is for AE < 100 nT does not directly invalidate the results presented here.

Finally, as was stated above, the focus of this paper is not the details of the energy spectra. If the referee would prefer, we could remove them from Figure 10.

(3) On the plasma pressure. I suppose that the plasma pressure was calculated from integration of the differential flux over the energy range from 2.5 keV to 97.5 keV. The energy range is probably insufficient to cover all the ions trapped in the inner magnetosphere because the ions with energy greater than 100 keV is also known to contribute to the plasma pressure (energy density) largely (e.g., Smith and Hoffman, 1973, 10.1029/JA078i022p04731; Williams, 1983, 10.1016/0032-0633(81)90124-0). If the high energy ions remained during these storms, there would be another peak of the pressure, which may stay at L ~2.5 – 3.0. I recommend discussing possible impacts of the high energy ions (>100 keV) on the conclusion. I also recommend emphasiz- ing that the plasma pressure distribution is "partial" so that the pressure distribution is incomplete.

The referee is correct, the plasma pressure presented in this paper should be referred to as partial pressure because it was calculated by integrating from 2.5 to 97.5 keV. The paper by Smith and Hoffman, 1973 certainly shows that higher energies can make significant contributions to the energy density (pressure). It is to be noted, however, that they say

“To contrast the development of the ring current for the two storms, we now consider those protons (1- to 138-kev protons were used) which contribute substantially to the storm-time ring current. While protons in this energy regime contribute only 20% or less to the total energy density out to L -• 4 during magnetically quiet periods, their enhancement during magnetic storms, combined with a depletion of protons with energies greater than about 170 kev, make them the dominant (greater than 90%) contributors to the storm-time energy densities.”

The paper is referenced. (See Section 2.2, line 179 in the version of the paper where corrections are accepted and line 183 in the version of the paper where corrections are marked.)

The paper by William, 1983, describes the state of observations at that time with the conclusion, “It is found that the ring current energy density composition still has not been observed.”

The authors wholeheartedly agree with the recommendations of the referee.

[MINOR COMMENTS]

Introduction: I recommend citing papers related to plasma pressure distribution and anisotropy observed by satellites, for example, De Michelis et al. (1999, 10.1029/1999JA900310), Ebihara et al. (2002, 10.1029/2002GL015430), and Lui (2003, 10.1029/2003GL017596).

Definitely. The authors apologize for not recognizing these papers. (See Section 1, line 65, 70, 72 , in the version of the paper where corrections are accepted and line 68, 73, 75 in the version of the paper where corrections are marked.)

Line 47-57: Simulation results with different electric field and/or magnetic field models have been conducted by Angelopoulos et al. (2002, 10.1029/2001JA900174) and Ebihara et al. (2004, 10.5194/angeo-22-1297-2004).

Most definitely. The authors thank the referee for pointing us the these papers. (See Section 1, line 55, 61 in the version of the paper where corrections are accepted and line 58, 64, in the version of the paper where corrections are marked.)

Line 58-63: This paragraph seems not to provide information. What key spatial features do Wang et al. (2011) find?

The authors think it is somewhat harsh to say that the paragraph does not provide any information. In particular reference to the Wang et al. (2011) paper, the paper presents extensive data and comparisons with RCM modeling, but they are based upon statistical averages of events. The result from Wang et al (2011) that is relevant to this paper is what is stated in the last sentence of this paragraph. It is based upon the last sentence of the Abstract and the first sentence of the Summary.

Line 311: The equatorial pressure p_eq is difficult to understand. Please explain how to derive p_eq.

The authors are not sure why p_eq is difficult to understand. It is pressure at the equator as a function of position and pitch angle. It is the standard definition of pressure, i.e., the energy density of the ions, the integral of the distribution function times the velocity squared.

Line 489-496: Ebihara et al. (2009) also showed multiple peaks of the plasma pressure distribution in the inner magnetosphere by introducing temporal changes in the distribution function at the outer boundary of CRCM. It would be worth mentioning that the rapid changes in the distribution function in the plasma sheet could result in the multiple peaks of the plasma pressure.

A sentence has been added pointing out that the model calculations did show multiple pressure peaks inside of 4 R_E. (See Section 6, lines 534ff in the version of the paper where corrections are accepted and lines 545 ff, in the version of the paper where corrections are marked.)

Line 499 "But they do not provide incontrovertible evidence for the effects of spatially and temporally dependent injections into the inner magnetosphere." This sentence is difficult to understand.

The sentence has been removed.

Figure 10, caption: Please indicate the unit of the color bar (probably in keV), and pitch angle of the particle. What is the meaning of "Minimum – Maximum Energy for Each Path"?

An explanation has been added. (See Figure 10 caption, lines 534ff in the version of the paper where corrections are accepted and lines 1046 ff, in the version of the paper where corrections are marked.)

Line 520, "Peak 5" Does it mean "Peak 4"?

Yes. It has been corrected.

Line 497-527: The spectral shape of the differential flux of the ions is almost the same at the 4 points. Please explain the validity of the differential flux derived from TWINS?

I think we have addressed this issue in response to previous comments. The main reason for presenting these spectra is to motivate showing the paths of the 46 keV ions. As stated above, if the referee prefers, they can be removed.

At Peak 3, the ion is inaccessible from the outer boundary. I recommend tracing the ion trajectory backward in time by starting at slightly different points.

The authors are uncertain as to why the referee thinks the paths would show a chaotic dependence on the starting point. We tried a few slightly different points, and the result was the same.

Line 546-548: "This is not unexpected as the ions are being injected into regions of higher magnetic field, and conservation of the first adiabatic invariant would predict the enhancement of parallel pitch angles." I cannot understand this meaning. Please explain the reason why the conservation of the first adiabatic invariant results in the pressure anisotropy dominated by the parallel component?

A full explanation has been added. (See Section 6, lines 590ff in the version of the paper where corrections are accepted and lines 601 ff, in the version of the paper where corrections are marked.)

Line 548-550: "Nevertheless the parallel anisotropy is seen in the observations only

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during the main phase of the first storm. This is also an indication of stronger electric and magnetic shielding." Please explain the reason why the stronger shielding results in the parallel anisotropy?

The statement has been changed to be consistent with responses to previous comments by the referee. (See Section 6, lines 595ff in the version of the paper where corrections are accepted and lines 609 ff, in the version of the paper where corrections are marked.)

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