Reply to Referee's comment 4:

We very much appreciate the comments that have been made by the Referee. They have certainly made our paper better. But at this time, it seems that we having trouble communicating. We are not sure how to respond. Please see comments below: (The Referee's comments are repeated in italics.)

The authors revised the equation for the pressure terms. However, the equation seems to be different from the equation given by De Michelis et al. (1997, http://doi.wiley.com/10.1029/96JA03743) by a factor of πm. I am curious to know the reason why the equation is different.

I assume that the referee is referring to Eqs. (7) and Eqs (8) in De Michelis et al. (1997). Let's look at the unit of those eqs. For the pressure to have the correct units of energy per unit volume, the units of the "differential flux intensity", *J* in Eqs. (7) and (8) must be 1/(vol * m * v). The reason for the different factors is that *J* is not the flux we have in our equation. Our flux, *F(E,n,cosα),* as it says in the text, has units of #ions/(energy*time*area*steradian). One can check the units of the equation we have in the paper and they come out to be pressure. In fact if one substitutes the proper equation for a Maxwellian distribution into the equation in the paper, i.e.,

$$F(E,n,\cos\alpha)=\frac{n}{\sqrt{2m}(\pi T)^{3/2}}E\,e^{-E/T}$$

and performs the integrals, the result is nT , precisely what one expects for a Maxwellian.

The definition of the plasma pressure is changed.

The definition of the plasma pressure has not changed. We have been discussing a general definition of pressure. That has not changed, just the way it is presented has been made clearer with the help of the Referee.

Does this change have any impact on the result? I suppose that lower energy protons may have more impact on the pressure.

We assume this is in reference to the previous comment regarding the change in the definition of the pressure. We assume that the referee is referring to the fact that the pressure we calculate and present as results is the partial pressure, i.e., it is integrated from 2.5 to 97.5 keV for TWINS and 1 to 133 keV for CIMI. The referee correctly requested that we distinguish the pressure calculated in this paper as the partial pressure and we have done so. There is no change that would impact the results in this paper.

I recommend removing n from F(E,n,cosα) because F is an arbitrary function and n is an independent variable.

This comment may somehow be at the heart of the miscommunication that we are having at this time. Yes, it is in some sense arbitrary, i.e., in the expression for the pressure, it is whatever it is in a particular physical situation. In what we are presenting, however, the *F(E,n,cosα)* is definitely not an arbitrary function. For the TWINS results, it is what is obtained from the ENA images. For CIMI, it is what is obtained from the simulations. As stated above it has units of #ions/(energy*time*area*steradian). We feel that it makes the most sense to express the pressure in terms of what it is obtained, i.e., from the measurements and simulations. It is then integrated as expressed in the formulas to obtain the pressures we present. We might also note that previous publications of TWINS and CIMI results have shown the average of $F(E,n,\cos\alpha)$ over pitch angles as a function of energy.