This manuscript reports on an event from 1 Aug 2008 where >30 keV electron flux is observed to be enhanced in the drift-loss cone ("quasi-trapped" electrons) at very low L values, L<1.2. Electron flux is rarely observed to be elevated in this region and understanding the physical processes that can transport energetic electrons to such low L values is interesting and worthy of study. Several recent works have been looking at what controls the energetic electron dynamics in these inner regions, so this topic is timely. The injection event is noteworthy also because it occurs during very quiet conditions; the only activity present is a weak, isolated substorm of (~300 nT) that occurred roughly 4 hours <u>after</u> the injection activity begins. Such deep energetic particle injections are usually associated with intense substorm activity but this event is not, and thus presents a unique challenge to understand what causes the deep injections. The authors note that they have found more than three dozen days (over a 10 year period) with such 10s keV electron injections but without substorm activity. Previous work by the author (Suvorova (2017)) has suggested an important role of the auroral ionosphere in the occurrence of these injections.

The present work builds on these ideas by presenting a scenario where transient foreshock waves and magnetosheath plasma jets cause global perturbations in the geomagnetic field. These compressions introduce magnetosheath plasma into the magnetosphere, which then precipitates to the dayside ionosphere at high latitudes. This in turn results in a local increase of the ionospheric conductivity and generation of transient localized electric fields, which are able to penetrate from high latitudes to very low latitudes. The authors estimate that the strength of these electric fields would need to be ~5 mV/m to produce the deep injections, which is consistent with prior estimates. This whole proposed scenario is supported only by circumstantial evidence, but it is at the very least plausible. In that sense, I agree with the other referees in their assessments that the causal links are weak and could be just due to chance. However, whether or not the scenario is viable, the observations themselves are unique and worthy of publication in the scientific literature. Perhaps the scenario could be softened as merely a speculative after thought, and the paper changed to more focus solely on the observations? That way an educated reader can judge for themselves whether or not the scenario presented is indeed viable. I also have some concerns related to the way the data is analyzed and presented, particularly with regards to the wave amplitudes and peaks identified, that I would like to see addressed before I can recommend the manuscript for publication.

<u>Major Comments</u>

1. Several aspects of the analysis are somewhat qualitative and should be made more quantitative. For example, at L351: *"Prominent magnetic peaks are indicated by dashed lines and listed in Table 2."* – what is the criteria for determining these peaks? It is not stated, and there are clearly some peaks in the same interval that are not identified or called out as peaks. (e.g., at 1445 UT, 1350 UT…). Why are these peaks not included? I suggest that the authors use a quantitative criterion to identify the peaks, so that they are not ambiguously and arbitrarily chosen. It almost seems as though they are chosen to match the ground magnetic perturbations shown in Figure 9. I also suggest that the authors detrend the three THEMIS time series and the ground magnetometer data so as to reveal the peaks in all the time series more clearly. Note that this will help better confirm the claim on L421 that "*the first magnetic pulse at ~1200 UT can not be emerged from THEMIS data because of the large background magnetic field in the inner magnetosphere."* Background trends can easily (and should) be removed by detrending.

Similarly, at L497 the authors state: "*Smaller amplitude at daytime is a result of an amplifying integral effect from the Chapman-Ferraro current at the magnetopause and ionospheric Sq-current at the ground."* - The ground magnetometer data are not presented in such a way that one can determine whether or not the ground field perturbations are weaker on the dayside than the nightside. The data need to be detrended. The scales are larger on the dayside stations (going up to >10 nT in some cases) so it is difficult to determine from the figure whether these perturbations are lower than those on the night side stations. I suggest that the data be detrended and Fourier analyzed to calculate the RMS wave power at each station to quantitatively assess the amplitude of the ground perturbations for comparisons.

Also, at L567, the authors state: "*We find that the magnetospheric ULF waves are not strong enough to produce anomalous radial transport of energetic electrons at L < 1.2*" – how is this determined? I see no discussion along these lines or any such calculations anywhere in the manuscript.

Finally, it is noted that the first magnetometer pulse is at 1330 UTC, which is after the first appearance of >30 keV electrons observed at L<1.2. This is not entirely consistent with causality, with the perturbations leading to enhanced electric fields that produce the injections. How do the authors reconcile this?

2. The role of dynamic pressure variations: At L363: *"The magnetic variations associated with compression-expansion effects could not be caused by the solar wind pressure variations, which were gradual and small during the interval (see Figure 3)."* Here the authors are referring to the OMNI data as evidence for this claim, which is supported by the OMNI data. However, the authors have just argued that there are significant differences between the OMNI data and what is actually observed just upstream of the magnetosphere by TH-C. Thus, it seems as though TH-C data should be presented, in terms of in-situ pressure variation observations. The authors go on to say that THEMIS cannot observe in the magnetosheath at this time, but what about what TH-C observes locally in the solar wind just upstream of the bow shock? Are there pressure variations (magnetic or dynamic) observed there? I would like to see those data, as they would bolster these claims significantly. In addition, at L477 the authors state: "*we expect that variations in the geomagnetic field (if any) should result from the local magnetosheath pressure pulses."* Why? There are a number of other mechanisms that can cause activity in ground magnetometer during relatively quiet times (e.g., ULF waves driven by Kelvin Helmholtz; ULF waves generated internally by plasma instabilities, etc…). Have the authors considered any of these? Why do they believe that these are not occurring at the time of the ground perturbations?

Minor Comments

- Figure 3 caption: "The shaded box denotes the time interval from 13 to 23 UT" – there is no shaded box in the figure
- L171: "quite" -> "quiet"
- L181-182: You might mention here that this is the "0-degree telescope," since this is how it is commonly referred to in the literature.
- L183: Is this the definition of the forbidden zone? If so, you should state that "The forbidden zone is defined as L<1.2 ….." What field model are you using to define the L values?
- L193-194: "*Fluxes of the >100 keV electrons and >30 keV protons did not increase also (not shown).*" You should indicate here whether you are referring to quasi-trapped, precipitating, or both.

- L199: This labeling of the POES vehicles is not standard. Is this what you mean? “(P2 = MetOp2, P5 = NOAA-15, P6 = NOAA-16, P7 = NOAA-17, P8 = NOAA-18)”? If so, you should state this here.
- L220-221: “*All remaining enhancements F2, F3, F5, F6, F8 and F10 of >30 keV electron fluxes were observed in the early morning (5 LT) for a long time interval of ~4 h*” – I don’t know how you can easily see this from the figure. I think you need to label each of the curves in Figure 2(a) with the corresponding FEE number.
- L283: How do you know that TH-C is upstream of the bow shock? There's no bow shock model shown. Is this simply inferred from the TH-C measurements?
- L287: “GMS” -> “GSM”
- L300: “After 1500 UT, the OMNI data do not match the TH-C observation any more, even with time correction.” – it would be nice if you showed also a smoothed version of the TH-C cone angle time series in Figure 5(c)
- L401: Are the ACE data time shifted here?