Dear Referee1,

Thank you very much for reviewing the manuscript.
Text of the revised paper was shortened by 4.5 pages.

Dear Referee3,

Thank you very much for your constructive comments and helpful suggestions. We add data which demonstrate important role of dynamic pressure variations in the subsolar foreshock region. Correspondingly, sections 2 and 3 (Observations and Discussion) were substantially revised. We try to soften all formulations concerning the assumed scenario. The revised text is marked in blue. Here we address all your concerns.

Major comments by Referee3:

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.

Reply:
In selection of peaks, we paid more attention to a sequence "depletion-compression" in the magnetic field, because (and Reviewer is absolutely right here) these signatures are also found in the ground observations. Small-amplitude peaks noted by Reviewer can be considered as just subsidiary because they do not appear in Figures 9-10.
We clarify our selection of magnetic peak and change the text:
"Prominent magnetic "dimple-hump" structures are indicated by dashed lines (as 1, 2, and 3) and their peaks are listed in Table 2. We select peak-to-peak amplitudes exceeded ~5 nT in the GOES data (Figure 6c). The dimple-hump structures show the largest amplitudes up to 15 nT in THEMIS data (Figure 6b)."

Referee:
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 peak 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.

Reply:
We thank Reviewer for the suggestion. We detrend the THEMIS magnetic data and add or replace corresponding panels in Figures 6, 9, 10 and 11. Indeed, after this procedure a magnetic peak at 12 UT from the THEMIS-D data became more prominent.

It is quite difficult to detrend the ground magnetometer data, especially at daytime, because of long (5h) time interval. Also, our notice about amplitudes at the dayside ground stations is not important and has been removed.
We add the following text around Figure 6:
"The THEMIS magnetic data were detrended using the Tsyganenko T04 geomagnetic field model (Tsyganenko and Sitnov, 2005) and IGRF-2005 model (see Figure 6b). The IGRF model describes the Earth's main magnetic field and the T04 model represents magnetic fields from magnetospheric current system. "

Referee:
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.

Reply:
We thank Reviewer for this comment. We delete the paragraph including this sentence, because it is not significant in our study and we try to shorten the paper according the recommendation of another referee.

Referee:
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.

Reply:
The mechanism of fast transport with ULF-waves with amplitude of a few nT during non-storm condition was shown to be invalid for filling the slot region (L<3) by Park et al. (2010). We believe this conclusion undoubtedly is right for lower L (L<1.2). However, for shortening the paper, we delete everything about ULF waves from section 2.5 and as well as this statement (Line 567).

Referee:
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?

Reply:
We already discussed this issue in details and presented Figure 11 (see Lines 536-539, 580-586 in Discussion of the revised manuscript). Particularly, we wrote:

(Line 628, in revised version Line 536) "The first FEE injection (F1) at ~1250 UT was preceded by several geomagnetic pulses observed by GOES-12…."
(Line 648; in revised version Line 570) "Previously, simulations by Su et al. (2016) have showed that it is not necessary for electrons to be transported earthward all the way during a single injection. Hence, we can consider a multi-step radial transport produced by a number of short pulses of *E*."
and
(Line 658, in revised version L 580) "…the first FEE injection requires a long time (~hour and longer) and several pulses of *E* in order to transport energetic electrons from undisturbed edge of the inner radiation belt to *L*~1.1. Then, >30 keV electrons populate *L*-shells from 1.15 to 1.1 that makes possible to transport electrons to 900 km heights for a short time of ~10 min by one pulse of strong *E*. The latter pattern is applicable for the FEE injection F2 and others."
Here we shortly note that first injections (F1 at 1250 and F2 at 1315 UT) are weaker than subsequent ones and are preceded by several weak magnetic peaks. We add in section 2.4 (Line 322-323) : "From 11 to 13 UT, one can see several increases of a few nT observed by GOES and/or THEMIS at ~1125, ~1200, ~1245 and ~1300 UT (Figure 6b)."

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.

Reply:
Thank you for the suggestion. In contrast to the OMNI pressure, TH-C observations show large variation of the dynamic (and total) pressure in the subsolar foreshock region.
We revised Figure 5 and Figure 7 adding panels with solar wind pressures from TH-C. In Table 2, we add a column with timing of the foreshock pressure pulses.

We add the following (Line 285-297):
"Figure 5d demonstrates large difference in solar wind dynamic pressure acquired from the TH-C probe, the ACE upstream monitor and OMNI data. The ACE data are shifted by 60 min. In contrast to OMNI and ACE, TH-C observed strong fast fluctuations in the dynamic pressure during intervals of subsolar foreshock (see Figure 5c). Note that ACE shows in average a smaller pressure than OMNI predicts, and it is more close to the TH-C observations. The fluctuations in the TH-C measurements are characterized by pressure pulses, which exceed sometimes the dynamic pressure from ACE (e.g., at 1320-1330, 1350, 1420, 1440, 1530 and etc.). The pulses were originated from plasma density enhancements because the plasma velocity remained practically constant at that time (not shown). Similar foreshock phenomenon was described by Fairfield et al. (1990). Apparently, the foreshock pressure pulses were further transported by the solar wind to the magnetosheath and could affect the magnetopause. Similar foreshock pressure pulses and their compression effects in the magnetosphere-ionosphere were reported by Korotova et al. (2011). "

Also, we revised correspondingly text around Figure 7 in section 2.5 (Line 366-370, and etc.):
"As one can see, most of the magnetic peaks at panel d and/or magnetosheath ions at panel b were preceded by the foreshock pressure pulses within 1-5 min (panel f), for example at ~1549, ~1611, ~1625 UT and etc. (see Table 2). There are exceptions for plasma penetrations #6 at 1648 UT and #7 at 1651:30 UT. Note that those events were preceded by IMF discontinuities as one can find in rotation of the cone angle (panel e) at 1645 and 1650 UT, respectively."
(Line 432-434):
"Most of the penetrating magnetosheath jets correspond to the foreshock pressure pulses. All jet-related plasma structures caused local compression effects at the dayside."

We add corresponding commentaries in Discussion section (Line 495-509):
"Comparative analysis of the THEMIS, OMNI and ACE data showed that the geomagnetic perturbations were not driven by the dynamic pressure of the pristine solar wind. Note that significant discrepancies between the OMNI data and THEMIS near-earth observations under quasi-radial IMF were reported frequently (e.g., McPherron et al., 2013; Suvorova and Dmitriev, 2016). THEMIS observations show firmly that geomagnetic perturbations were rather related to changes in the IMF cone angle and pressure pulses in the subsolar foreshock.
We demonstrated that in the magnetosheath, foreshock pressure pulses could be transformed to fast and dense magnetosheath streams, so-called jets. We found that 5 out of 7 magnetosheath jets were preceded by the foreshock pressure pulses. These results support well the previous findings that the plasma jets are typical consequence of the foreshock dynamics and variations in the IMF orientation (e.g., Fairfield et al., 1990; Lin et al., 1996; Archer et al., 2012; Dmitriev and Suvorova, 2012; 2015; Plaschke et al., 2018). In addition, similar effects of the foreshock pressure pulses and magnetosheath jets in the magnetosphere were reported (e.g., Sibeck and Korotova, 1996; Korotova et al., 2011; Heitala et al., 2012)."

Referee:
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?

Reply
We have revised the statement and put it to the end of Section 2.6:
"Thus, the low and middle latitude ground-based magnetic observations demonstrate similarity in the magnetic variations of "dimple-hump" pattern at 1200-1600 UT with the satellite observations in the dayside magnetosphere. It should be noted that the magnetic peaks are not regular and are characterized by periodicities of tens of minutes that distinct them from magnetospheric quasi-periodic ULF waves with periods 1 – 600 s. Hence, the variations observed in the geomagnetic field should result from pressure pulses of foreshock and/or magnetosheath origin."

Minor Comments by Referee3:

- Figure 3 caption: "The shaded box denotes the time interval from 13 to 23 UT" – there is no shaded box in the figure

Reply:
The box was missed accidentally during the previous revision of the manuscript. We correct the Figure 3.

- L171: “quite” -> “quiet”

Reply:
Corrected.

- 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.

Reply:
We change accordingly: “The data shown in Figure 1 are from the 0-degree telescope oriented along the orbital radius-vector…”

- 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?

Reply:
We change the text accordingly:
“The forbidden zone is defined as $L < 1.2$ in the longitudinal range from 0° to 260°E (or 100°W) that is beyond the South Atlantic anomaly (SAA). The drift L-shells are calculated from IGRF-2005 model.”

- 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.

Reply:
We clarify this accordingly: “Fluxes of the precipitating and quasi-trapped >100 keV electrons and >30 keV protons did not increase also (not shown).”

- 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.

Reply:
Corrected.

- L220-221: “All remaining enhancements F2, F3, F5, F6, F8 and F10 of >30 keV electron fluxeswere 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.

Reply:
These labels were missed accidentally during the previous revision of the manuscript. We correct the Figure 2.

- 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?

Reply:

Yes, it is inferred from the observations. It is also supported by the average location of the subsolar bow shock from the paper by D. Faifield (1971). We clarify this in the text around Figure 5 (Line 236-244):
"During the time interval from 1200 to 1800 UT, the THEMIS-C satellite (TH-C) moved from the subsolar region (17.2, -0.3, -5.9 Re GSM) toward dusk (18.1, 3.4, -5.9 Re GSM) (see Figure 4). From the TH-C plasma and magnetic measurements (Figure 5), we infer that the probe was located upstream of the bow shock, whose average subsolar position was estimated as ~14.6 Re for *P*d~1.5 nPa (Fairfield, 1971). Figure 5a shows measurements of the THEMIS-C/FGM fluxgate magnetometer in GSM coordinates with a time resolution of ~3 s (Auster et al., 2008) and the ion spectrograms from THEMIS-C/ESA plasma instrument (McFadden et al., 2008). The ion spectrogram clearly demonstrates that hot ions (~ 1 keV) are of the solar wind origin and magnitudes of magnetic field components correspond to IMF components in Figure 3."

- L287: "GMS" -> "GSM"

Reply:
Corrected.

- 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)

Reply:
We add the smoothed curve in Figure 5c.

- L401: Are the ACE data time shifted here?

Reply:
Yes. We add in the text (Line 286) and caption of Figure 5: "The ACE data are shifted by 60 min."