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Interactive comment on "In-flight calibration of double-probe DC electric field measurements on Cluster" *by* Y. V. Khotyaintsev et al.

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We would like to thank the referee for interesting comments and provide our answers below.

1. Add in fig captions what is used as a reference measurement.

It is not clear which particular figures the referee means. Also, my understanding is that at this stage it is not possible to edit the text of the paper.

2. The most natural dependence in Ex offset is s/c potential that is related to Debye length which again affects the photoelectron distribution.

Indeed, there is a clear dependence on the Debye length seen as a ~1 mV/m difference between the statistical offsets for SW/SH and for the magnetosphere. However, we were not able to find a simple dependence of the Ex offset on the Debye length. Change in the offset is clear only for cases where the plasma density is changes by at least 2 orders of magnitude; otherwise the change becomes too small compared to the accuracy of the offset determination.

3. Page 3, Line 20: it is not clear for which time interval of data the fitting is done, is several spins taken, one spin, or possibly ±2 sec

The fitting is done over 4 sec of data.

4. Page 5, line 7: the approach to the determination of the correction factor is good. However how it is known that there is no dependence on any parameter, e.g. on s/c potential?

There must be definitely a dependence on the Debye length, but it is rather weak in the case of EFW. Therefore we have chosen to use a constant factor, which makes the calibration procedure much simple.

5. Page 5, line 24. This is a good approach but it would be good to know a bit more of how the authors decided to select such a set of numbers. Do the result change much if other numbers are used and more or less than 7 spins are used?

This approach was selected after trying several different length of the averaging interval. We wanted the offset to be responsive to changes in plasma environment, but at the same time it should be averaged over sufficiently large number of spins, as a value obtained from one spin does not necessarily represent a true offset, but can be biased by strong natural electric fields. Therefore we empirically determined that 7 is the smallest number of spins meeting these criteria most of the time.

6. Page 6, line 4. The splitting of the orbit is not clear. Do you change the offsets when the magnetopause is crossed according to Shue model?

No, we split the orbit into parts according to the model magnetopause for which we then statistically determine the offsets. When applying the offsets, we change them according to the spacecraft potential value.

7. Page 6, line 22: the third panel shows only C1 and C3 where HIA is available. If an average is calculated for both C1 and C3 and this is used in the data, then EFW will not agree with HIA in all locations because average is likely for $\sim$0.5 mV/m while during the first four hours the difference is $\sim$1 mV/m. Explain in the fig caption that lines are for EFW and "+" marker is for HIA, or is it?

Yes, there is a difference in offset between the magnetosheath and the solar wind. Moreover, the solar wind offsets seem to be rather variable in response to changes in the solar wind speed, i.e. the offset becomes smaller for faster solar wind. So, taking such averages is not ideal, and in principle one could think of implementing something more complicated. But this will require knowing at least the times of the bow-shock crossings, determining which is very time demanding.

8. Figure 6: indicate the data points with a larger marker, e.g. with "+".

This plots shows intervals, rather then point, so we think the representation is correct.

9. Page 7, line 10-12: the condition for the offset assumes implicitly that there is no Vsc dependence. Once the Debye length is large, this may be quite well correct. However, one should consider to remove part of the transient data before averaging. How much data are included in the averaging, all that with the tail requirement on line 11 and the whole tail season? Or is this done orbit by orbit? One should not average too much because photoelectron characteristics can change.

Yes, this is done for the low-density magnetosphere, so the Debye length is typically large. We average over one tail season, with most of the data being collected during three month (August to October). For this we use the spin resolution data, which removes some of the strongest transient fields. Also phenomena like bursty bulk flows

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would make a non-zero contribution to Ey, while for Ex should average out to zero.

10. Page 7, line 13-14. One should show some examples of CIS data but also EDI. Is CIS CODIF or HIA. It should be CODIF.

In the central plasma sheet where such comparison is the most interesting EDI does not work due to weak magnetic filed. We have used both CODIF and HIA data making sure that the majority of the ion populations is within the CIS energy range. Indeed, for faster flows CODIF works better due to it's higher upper energy, but for flows exceeding $\sim$1000 km/s CODIF cannot determine the velocity, and no comparison is possible.

11. Fig 9. It is not clear why the Delta offset is divided into Ex and Ey. This offset is related to offset of raw signal and one expects it to be related to the front electronics, sensors etc. To me this should be non-directional quantity. The authors should at least present in one panel the total Delta offset.

We think the delta offset is mainly rated to properties of the probes and other part of the receiving element (booms, puck, thin wire), and their relative contribution will strongly depend on photo-illumination, i.e. with spacecraft spin – which parts are illuminated and how much. So the offset should be different for X and Y directions, which is supported by data plotted in Figure 9.

Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., 4, 85, 2014.