

## ***Interactive comment on “Inter-instrument calibration using magnetic field data from Flux Gate Magnetometer (FGM) and Electron Drift Instrument (EDI) onboard Cluster” by R. Nakamura et al.***

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We thank the reviewer for all the helpful comments from both reviewers. Response to the comments are given below followed by a list of further editing corrections.

Response to the Report from the Reviewer #1

We thank the reviewer for all the helpful comments. We corrected the manuscript by taking into account the comments as given below. The comments by the reviewer is

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indicated with "COMMENT:"

COMMENT: The paper presents a straightforward description of comparisons between FGM calibrations and EDI magnetic field results. The paper is generally acceptable for publication as is, but I do have some minor comments and a couple of questions (#5 is my primary question).

We appreciate the positive response to our works. The answers to the questions are given below.

COMMENT: 1) The abstract is well-written, but is missing a sentence or two about the main conclusion of the paper.

Following sentences are added: "It is shown that the method works best when the magnetic field magnitude is less than about 128 nT and when the magnetic field is aligned near the spin-axis direction. A remaining spin-axis offset of about 0.4 ~0.6 nT was observed between July and October, 2003 at Cluster 1."

COMMENT: 2) Rows 1 and 2 of Figure 1 look virtually identical. My conclusion from this is that it doesn't matter if one time matches or not, as long as one has enough data. Is this the point here? If so, I don't see how time matching is useful statistically, and wonder if it's even needed in this paper.

Although the example shown here is from a period when the number of the returning beam are quite evenly distributed, EDI data depend on the availability of the returning beam and can be also very sparse some time. Hence it is essential to compare with the time-matched FGM data as shown in the Rows 1 and this method is used in this study. As the reviewer's comment, the same EDI data are plotted indeed in Rows 1 and 2. Actually, the main purpose of Row 2 was to show the original FGM 22 Hz data (without time-matching) and also to compare the FGM data with different calibration files. Plotting also EDI data here was therefore misleading. In the new Figure 1, we only plotted the FGM data in Row 2. In the new manuscript we explained the reason

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of the time-matching in the second paragraph of page 5.

COMMENT: 3) Row 3, column 2, shows very large scatter in the 1Hz FGM data. Where did this come from? The only difference between the first and second column is a CAA calibration vs CSDS calibration? This scatter is very odd.

Row 3, column 2, uses the calibration data file, which is used for the 4 sec spin resolution data (PP data) or 1 min-average data (SP data) in Cluster Science Data System (CSDS). The relatively large scatter of the 1 Hz data comes from the spin-tone, which can be more clearly seen in the 22Hz data in Row2 column2. For the CSDS dataset available for the users, such spin-tone will not appear due to the low resolution, but if one use this calibration file for high-resolution data the spin tone remains and therefore needs to apply another calibration step as given in column 3. The difference between these calibration arises due to different approach in calibration procedures, i.e. differences in which step of the calibration procedures the spin-tone is removed. Whereas such details of the FGM calibration approach are beyond the scope of this paper, we still think that it is important to show that the remaining spin-axis offset can be different among the two data sets, which are both publicly available. We have add one sentence to explain the scatter of 1 Hz data in the second paragraph of page 5.

COMMENT: 4) Figure 7 shows a comparison of Bc3-Bc1 using EDI, FGM and Tsyganenko data (description in lines 317+). The paper makes a point that dBedi provides an 'empirical value' which is a statement I do not understand. Just like FGM, EDI is making measurements of the actual field. If these measurements are close to the model field, it means the model is pretty good, and you've chosen appropriate model parameters (Kp, for example). In the second column, the slightly larger difference between observations and model could be removed by changing Kp, I would think.

Why we mentioned "empirical value" for Tsyganenko model is that its profile is obtained based on fitting a number of previous satellite data to an analytical function expressing the external field and therefore we interpreted that it represents some averages of

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different "offsets" among the different previous missions expecting a near-zero offset magnetic field (based on average of a number of independent "offsets"). If the differences between the model and FGM observation are only due to "wrong" Kp, the difference should not have any cosb dependence. Such cosb-independent deviations are in fact seen between the EDI(or FGM) and the model in the 2006 plot and between EDI and the model in the 2003 plot. On the other hand, FGM has systematic cosb-dependence in the deviation from the model (or EDI) in 2003 plot. This suggests that FGM deviation is not only Kp issue but most likely due to the offset. To make this point more clear, we added explanations on the model values in the last paragraph of section 3 (in page 10).

COMMENT: 5) I'm confused by the conclusion that the enveloping of the data shown in Figure 4 is due to FGM gain uncertainty. I don't think this is plausible based on two things. 1) The offset resets itself at each EDI change. How does a gain change explain this? 2) The error is randomly positive or negative. A gain uncertainty should not produce such randomization, unless – and perhaps this is the case – the gain uncertainty is really down at the LSB level. A more plausible scenario may be a +/- uncertainty inherent to EDI. The authors could be correct here in their interpretation, but it needs some more discussion.

The offset-correction performed for Figure 4 does not take into account any gain correction. A gain error then should appear as a linear trend if all the other calibration are perfectly completed. Such a gain error curve, however, is difficult to differentiate from the EDI time-of-flight profile particularly when the resolution is not sufficient. Therefore each EDI range may show different resultant curve and not a continuous line in Figure 4c. In the low field region, we cannot see any systematic trend. But in the higher field range, particularly R6, the plus-minus fluctuation has on average some negative trend, which we have tried to examine whether it is possible to interpret as a gain error. As the reviewer pointed out this gain error is below the EDI resolution, although it may still within FGM resolution. So additional fluctuation is unavoidable. We add explanations

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on these problems of the resolution and modified the text so that the trend determination is limited in the last EDI range (R6) in the first paragraph of section 4 in page 11.

COMMENT: Minor wording changes COMMENT: \* Line 30, "Despite of the pre-flight", remove "of the"

Corrected

COMMENT: \* Line 91 "usind" = "using"

Corrected COMMENT: \* references are made to a 'gray' line in Figure 4b. Should this be green?

We have used two separate panels for the two datasets and plotted them all in red.

COMMENT: \* Line 313, "an" -> "a"

Corrected

Response to the Report from the Reviewer #2

We thank the reviewer for all the helpful comments. We corrected the manuscript by taking into account the comments as given below. Here the comments by the reviewer is noted with "COMMENT:".

COMMENT: This paper presents a clear description of the Cluster spacecraft magnetometer calibration using the electron drift instrument to refine the spin-axis offsets for the magnetometer data. I have only two minor comments. First, in Figure 2b, what happens if the medians are used rather than the mean? I consider the median a much more robust measure, less affected by outliers. Second, in Figure 4, panel b should perhaps be separated into two sub-panels as the grey points overlap and obscure many of the red points. This has the effect of making the mean (black line) look incorrect. Again, I would also suggest using the median.

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In the new Figures 2 and 4, we have shown both the medians and means. We also plotted the gray and red profiles in Figure 4 in separate panels, now both plotted in red. The median has indeed more systematic trend than the average in Figure 2, while there is hardly no difference between median and mean in Figure 4. We think that the large difference in Figure 2 is due to the inclusion of data with miscalculated sign of EDI spin-axis component for near-spin plane field configuration, which are removed from the dataset for Figure 4. Since there was no significant differences between the mean and the median for the cosb-selected data (Figure 4) we kept the other calculations as it is. According to the change in the figures we have corrected the explanation of the figures in pages 6 and 8. We also plotted only every 20th points from the entire data for better presentation quality.

Further technical/organization corrections:

1) We have moved 2 names, W. Magnes and M. Steller (both affiliated to the same institution as the first author), from the acknowledgement list to authors' list.

2) When replotting Figures 2 and 4, we also found that the EDI time-of-flight offsets used for R1-R3 shown in Figure 4 was not calculated using only data with  $\text{cosb} < 0.1$ , but data using all  $\text{cosb}$ , although in the text it was mentioned as ( $\text{cosb} < 0.1$ ). We changed the text by deleting this condition to be consistent with the Figure 2. This happened because we initially thought that applying this condition allows to remove the data with spin-axis offset. Yet the EDI offset determination is applied here after removing the spin-axis offset and therefore such condition was not particularly necessary.

In addition to the above changes we have made following corrections for the final manuscript:

1) The monthly average values compared in Table 1 We specified the spacecraft number, "Cluster 1", in Table 1 caption, relevant text in section 3, abstract, and conclusion.

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2) Figure 2: The previous plot wrongly showed a constant-Bz subtracted data set, although it was meant an "uncorrected data" plot, inconsistent with the text/figure caption. We have change the figure to show the uncorrected data as supposed to present in this figure. In the final revised figure, the necessity of an offset correction can be seen more clearly.

3)Correction: "15-chip or 27-chip" to "15-chip or 127-chip"

4)Grammatical and minor editing errors are corrected.

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Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., 3, 459, 2013.