

REPORT ON JINST_003T_0125

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Title: The commissioning progress of microwave imaging reflectometer on EAST tokamak

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Editor report

The paper "The commissioning progress of microwave imaging reflectometer on EAST tokamak" describes the installed two-dimensional imaging reflectometer diagnostic and the recent improvements to optimise the signal to noise ratio (SNR).

The main difficulties of operating such a diagnostic are discussed. A bench-top experiment is presented, in which the minimum SNR for identifying the signal from possible background noise is discussed.

Finally, preliminary experimental results of channels distributed on the same radial position but different poloidal location indicates the diagnostic is able to detect coherent modes.

The paper is well-structured overall, but certain sections could benefit from reorganization and a more detailed explanation to enhance clarity and impact, especially regarding the experimental results and conclusions.

I suggest the authors consider the following points to strengthen the manuscript and bring it to the standard required for publication. For the following reasons, I would recommend a major revision of the work.

Chapter 4.1 is probably the less clear and I have the following points:

1 - The manuscript includes the use and presentation of DIII-D data. However, there is no citation, acknowledgment, or mention of DIII-D contributors.

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I would kindly ask the authors to clarify this point and ensure proper attribution to align with standard publication practices

2 - Figure 7 requires significant clarification. The figure lacks x- and y- axis, it is not mentioned whether the data are comparable, under which experimental condition the MIR data have been collected and so on. Furthermore, please specify the radial and poloidal positions instead of only providing the channel numbers to improve interpretability

3 - It is unclear what the authors mean with the sentence "it is found that the evolution of raw data provide direct clues to calibrate the optics and optimize the MIR performance" (line 77, page 7). Could the authors please clarify this point for better understanding?

Regarding the rest of the paper:

4 - It is stated in the introduction that the performances are far from satisfactory if compared to ECEI. It would be beneficial if the authors added a sentence clarifying the advantages of an MIR system over an ECEI.

5 - Page 1, last line: The sentence "the published images of 2-D fluctuations are very limited" is unclear. Could the authors clarify which specific images they are referring to in this context?

6 - Figure 1: Could the authors clarify what MIR RX and MIR TX stand for? Please provide a brief description either in the caption or in the main text to help the reader understand.

7 - Figure 2: the image is not very well readable. It might be beneficial to use a higher quality version.

8 - Section 3.2, page 5, line 142 - 148. Is it possible to quantify the contribution of the crosstalk to the overall noise?

9 - Section 3.3, page 6, line 163 - 167: here it is not fully clear to me what these ranges refer to. Could the authors add a couple of sentences or briefly restructure the chapter to make clearer the relation between the radial observation range (50-400 mm), the wavefront curvature and then the radial resolution which can be adjusted from 18.8 - 30.8 mm?

10 - Please explain what the acronym HHT stand for.

11 - In section 4 it is shown under which SNR the system is able to detect fluctuations, however in section 5 the noise level is not discussed. How do the signals after the optimisation look like with respect to Figure 7b? I would suggest to highlight in section 5 how the improvement/analysis performed in section 4 contributed to the results.

12 - Section 5: It would enhance the paper to briefly introduce the experiments the data refer to. For example, could the authors clarify the difference between shots 141699 and 141745? Additionally, why was the correlation analysis presented only for the second shot? Including these details would provide helpful context for the reader.

14 - Do the authors have any insight into what these modes might represent? Are they turbulence-related, and have they been detected by other diagnostics? Additionally, with the available data, would it be feasible to conduct a preliminary (poloidal) mode number analysis?

15 - Conclusion: I recommend the authors include a few additional sentences to better emphasize the achievements of this upgraded version compared to the system described in reference 11. Specifically, it would be helpful to clarify whether the system is now comparable to the ECEI diagnostic or if certain issues persist.

16 - Additionally, I have noted a list of typos and areas of poor English for correction:

- Abstract, third line: "frerquecy" -
- Page 1, line 53: "publised"
- Page 3, line 192: Consider replacing "messy" with a more formal term, such as "suboptimal."
- Page 5, line 143: "chose" and "differemy"
- Page 10, line 246: "ploidal"