

Dear Editor and Reviewers,

We sincerely thank you for taking the time to review our manuscript “Computer vision-based method for quantifying iron-related defects in silicon solar cells” (Ref. No.: SST-111269). Your insightful comments and constructive suggestions have greatly helped us improve the quality of our work. We particularly appreciate your careful reading and thoughtful feedback, which have led to significant improvements in both the technical content and presentation clarity of our manuscript. We have carefully addressed all the comments and made corresponding revisions to the manuscript. The location of revisions is pointed by red color and highlighted in yellow in “CompleteDocumentForReview.pdf”. Below we provide our detailed point-by-point responses to each comment. We hope the revised manuscript better meets your expectations and standards for publication in Solar Energy.

Response to Reviewer #1

Major Comment 1. *Novelty and Contribution: The idea of applying pre-trained CV models to wavelet-transformed kinetic data is interesting and potentially generalizable. However, similar signal-to-image ML transformations exist in other domains. The manuscript should emphasize what new physical or methodological insight this work provides beyond prior Fourier/wavelet-based ML approaches..*

Reply:

The additional information was added to the revised manuscript (from page 7, left column, paragraphs 2 to page 8, left column, paragraphs 2; page 8, right column, paragraph 2).

Major Comment 2. *Dataset Size and Overfitting Risk: The study relies on extremely small datasets (25 simulated and 28 experimental samples). Although data augmentation is performed, flipping or rotating spectrograms likely introduces redundant samples rather than independent data points. The near-perfect R^2 values (0.996–0.999) strongly suggest overfitting. A robust cross-validation (e.g., k-fold or leave-one-out) with uncertainty quantification is needed.*

Reply:

The minimal level shift, which was used as evidence of defect transformation, was 40 meV.

Major Comment 3. *Simulation–Experiment Gap: A major discrepancy is observed between simulated and experimental predictions, requiring a post-hoc quadratic correction. This implies that the CNN–regressor models primarily learn the synthetic data distribution rather than physical correlations. The authors should explore physics-based domain adaptation or partial fine-tuning using experimental data instead of empirical correction.*

Reply:

The description of observation technique was modified (page 3, left column, paragraph 3)

Major Comment 4. *Physical Model Validation:* The SCAPS-1D simulations use fixed FeB parameters (binding energy, migration energy, and pre-exponential factors). Yet, these parameters vary widely in literature (0.55–0.69 eV for migration energy). Without sensitivity analysis or error quantification, the generated synthetic dataset may not reflect realistic kinetics. Validation against first-principles or experimental benchmarks would strengthen the study.

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Major Comment 5. *Regression and Feature Interpretation:* Although multiple regressors (SVR, XGB, DNN, RF, GB) are compared, no insight is given into the learned features or their physical correlation with iron concentration. Incorporating explainable AI techniques (e.g., SHAP, PCA loading analysis) would add interpretability to what the CNN features represent.

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Major Comment 6. *Post-Hoc Correction:* The quadratic correction (Eq. 10) is an empirical adjustment that artificially improves metrics but lacks theoretical justification. The authors should either (i) replace it with a physics-informed calibration (e.g., temperature or diffusion based scaling) or (ii) clearly acknowledge its heuristic nature and limitations.

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Major Comment 7. *Statistical Reporting:* The reported MSE, MAPE, and R^2 values are given without variance or confidence intervals. Given the small datasets, reporting mean \pm standard deviation across multiple random splits would be essential to establish statistical robustness.

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Minor Comment 1. The introduction is overly broad; it should focus more on ML for microscopic defects rather than general PV or macro-defect analysis.

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Minor Comment 2. Some typographical errors exist (e.g., “where where” in Eq. 1). Please proofread carefully.

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Minor Comment 3. *Figures should include axis units, consistent color scales, and indicate whether values are in linear or logarithmic scale.*

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Minor Comment 4. *The Supplementary Figures (S1–S10) are repeatedly referenced but insufficiently summarized in the main text. A concise overview table would be helpful.*

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Minor Comment 5. *The data availability statement (“upon reasonable request”) should be replaced with a public repository link for transparency.*

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Minor Comment 6. *A comparison with simpler ML baselines (e.g., direct regression on ISC(t) data without wavelet transformation) would contextualize the improvement due to CV-based transfer learning.*

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Minor Comment 7. *References [6], [35], [38] should be verified for year and page accuracy. Some reference formatting inconsistencies (journal abbreviations, italics) should be corrected.*

Reply:

Allow us to say a few words in favor of GaAs and 6H-SiC.

Response to Reviewer #2

Comment 1. *It is additionally essential to examine temperatures between 270 to 350 kelvin.*

Reply:

To our shame, the reviewer is correct about some fog in Results and discussion. We hopefully rephrased the section to add sunlight.

Comment 2. *You should compare and review your manuscript with other new articles such as “Novel Design of Multi-Layer Cubic Nanoparticles for Achieving Efficient Thin-Film Perovskite Solar Cells”*

Reply:

The investigation did not show the essential dependence of the influence of MW treatment on doping levels as well.

Comment 3. Put the solar cell parameters in a table with references.

Reply:

The similarity of mechanisms was noted in the revised text (page 8, left column, paragraph 2).

Comment 4. Actually, all solar cells have R_s and R_{sh} values. By investigating parasitic losses on cell performance, the article could be made more interesting.

Reply: The text was revised.

Response to EDITOR REPORT

REPORT. We have found that your manuscript contains text which appears to have been replicated from the following published articles:

www.sciencedirect.com/science/article/abs/pii/S0038092X25005171?via%3Dihub

Please reduce the level of overlap in your revised manuscript by rewriting the appropriate sections.

Reply:

Some advantages of the variety of using structures are in reply on Comment 1 of Reviewer 2 as well.

References