

Reviewer 1

Comments

Reviewer 1 - Initial submission, 09/08/2026

The paper presents a well-structured methodological study proposing an adaptive surrogate assisted optimization framework for buckling-delayed shear-link dampers, combining supervised ML models. The manuscript can be recommended for publication after addressing the following comments:

- The full boundary conditions and applied loads need to be clarified, as Section 3 only describes them qualitatively without reporting actual displacement amplitudes, contact penalty stiffness, or how support flexibility was calibrated.
- The mesh size is not reported, and Figure 4 shows visibly different mesh densities; a mesh sensitivity study is needed, particularly since TFDMap is a strain-based indicator known to be mesh-dependent.
- The setup of the explicit solver should be described in more detail, including mass scaling, loading rate, and a kinetic-to-internal energy ratio....
- The total number of simulations (maximum 64 for the five-window family) is small for training ML models on a 5D input space; the accuracy and generalization limits of the surrogate models in this regime need to be clarified and discussed.
- The definition of the quantities of interest requires clarification.
- The distribution of the input features actually sampled by the LHS-maximin DoE should be shown rather than only stating the sampling ranges in Table 3.
- No experimental validation of the final optimized geometries is presented; the framework is validated entirely against FEM.
- Differential Evolution is stochastic, but no information is given on repeated runs, seed sensitivity, or convergence statistics for the optimization results reported.
- The computational cost is not quantified in the paper.

Reviewer 2

Comments

Reviewer 2 - Initial submission, 25/08/2026

The engineering feasibility criterion in the adaptive optimization loop requires fundamental reconsideration. The manuscript defines $D=100$ as complete failure and adopts $D_w^*=90$ and $D_{fmax}=90$, yet the acceptance criteria in the adaptive loop only require surrogate prediction error below 5%, objective-function error below 10, and stabilization of the window thicknesses. They do not require the FEM-validated candidate to satisfy the damage limits themselves. This produces a serious contradiction in the reported results. For F2, the final RBF FEM solution has approximately $D1=109.69$ and $Df=96.63$, while the supervised-ML solution gives approximately $D1=109.65$ and $Df=97.36$. Both exceed the stated damage targets, and the window indicator even exceeds the value identified as complete failure. Such configurations should not be described as successfully accepted damage-controlled designs without a substantially different interpretation of D . The optimization should include explicit feasibility constraints, for example $D_i \leq D_{wmax}$ and $D_f \leq D_{fmax}$, or the authors should clearly classify families for which no feasible geometry exists within the manufacturing bounds. The distinction between a soft target and an admissibility limit must also be made explicit. The study: “Cyclic performance of novel composite beam-to-column connections with reduced

beam section fuse elements” provides a useful structural comparison for discussing controlled sacrificial response and the requirement that damage remain confined to intended dissipative components rather than simply being penalized numerically. The term representing dissipative performance in Equation 3 does not appear to be an actual energy quantity. The manuscript uses a contribution proportional to the squared local shear distortion multiplied by thickness and an area factor, although the FEM model itself is capable of calculating cumulative dissipated energy, as demonstrated during experimental-numerical validation. A strain-based proxy cannot automatically be interpreted as hysteretic energy because dissipation is path-dependent and depends on stress history, cyclic plasticity, degradation, and material volume. This distinction is especially important because the paper repeatedly describes the optimization as maximizing energy dissipation. The authors should either replace the proxy with an actual FEM-derived plastic or hysteretic energy measure, preferably resolved by dissipative window, or quantitatively demonstrate a robust monotonic relationship between the proposed proxy and the true dissipated energy across all geometry families. The methodology represented by 10.1016/j.istruc.2025.110172 may provide a useful comparison for relating local energy-dissipation components to the actual seismic response of structural energy-dissipation links. The weighting of the objective function is currently too heuristic for the optimized geometry to be considered unique or physically robust. The manuscript explicitly states that the dissipative term is several orders of magnitude smaller than the damage penalties and is intentionally left unscaled, which means that energy dissipation acts essentially as a tie-breaker. The quadratic window penalty, cubic frame penalty, target value of 90, and relative scale of all terms determine the final optimum, yet no sensitivity analysis is provided. The authors should evaluate alternative penalty exponents, scaling schemes, threshold values, and relative weights and demonstrate whether the resulting window thicknesses remain stable. A genuine multi-objective formulation using a Pareto front between damage, frame protection, and dissipative performance would also be more transparent than embedding several priorities in one dimensionally heterogeneous scalar objective. The surrogate optimization strategy in 10.1038/s41598-026-54677-9 provides a relevant methodological comparison for discussing multi-objective surrogate-assisted design and should only be considered in that methodological context. The physical interpretation and numerical robustness of TFDMap require substantially stronger validation. The manuscript explicitly describes TFDMap as a post-processing damage-screening indicator rather than a constitutive fracture criterion, but subsequently associates $D=100$ with complete failure and constructs optimization thresholds around this value. The precise normalization and physical meaning of values near and above 100 therefore need clarification. More importantly, each regional damage output is defined as the average of the 12 nodes with the largest TFDMap values. The choice of exactly 12 nodes is not justified and is inherently dependent on mesh density. If the element size changes, those 12 nodes represent a different physical volume. The authors should demonstrate mesh-objective damage aggregation, for example through a fixed physical volume, percentile, integration volume, or another mesh-independent regional measure, and conduct a sensitivity study demonstrating that optimized geometries do not depend materially on this arbitrary aggregation rule. The FEM model is the numerical ground truth for every subsequent surrogate and optimization result, but its validation is not yet sufficient for the local quantities being optimized. Figure 6 demonstrates global agreement in hysteretic force-displacement response and cumulative dissipated energy, but the surrogate targets are local window distortions and local damage indicators. Agreement in global hysteresis does not establish that local plastic strain, stress triaxiality, window damage, frame damage, or buckling localization are predicted accurately. The authors state that three experimental BDSL tests were used, yet the manuscript should present quantitative

validation for all three tests and clearly distinguish calibration specimens from independent validation specimens. Comparisons should include peak force, initial and post-yield stiffness, cyclic degradation, cumulative energy, local buckling onset, failure location, and any available local strain or deformation measurements. The study: “Experimental and numerical investigation of mechanical behavior of segmental joint of shield tunneling strengthened by prestressed CFRP plates” provides a cross-domain structural example of combining experimental and numerical evidence, but its relevance here is methodological rather than device-specific.

Their relevance is to validation philosophy and nonlinear energy-dissipation modelling, not specifically to BDSL devices. The finite element discretization and explicit-dynamic solution procedure are not sufficiently documented for reproducibility. Figure 4 provides a visual representation of the mesh and boundary conditions, but the manuscript does not report characteristic element dimensions in the windows and frame, the total number of elements, through-thickness discretization, mesh refinement near window corners, or a mesh-convergence study. Because the optimization directly uses local strain and damage fields, mesh sensitivity is more important here than in a study concerned only with global force. The authors should demonstrate convergence of D_i , D_f , local distortion, hysteretic energy, and global force-displacement response. For the explicit solver, the stable time increment, loading rate, mass scaling if any, damping, kinetic-to-internal energy ratio, contact stabilization, and criteria used to establish quasi-static cyclic conditions should also be reported. The use of very small training datasets raises concerns about the reported supervised-learning comparison. The initial datasets contain only 8 samples for each two-window family and 16 for each three-window family, yet RF, GBR, XGBoost, SVR, GPR, and MLP models are all subjected to Bayesian hyperparameter optimization with 40 evaluations. Optimizing many hyperparameters and then ranking several algorithms using the same cross-validation estimates can produce substantial model-selection optimism, especially when $N=8$. Leave-One-Out cross-validation alone does not remove this bias. The authors should either use nested validation or clearly distinguish hyperparameter-selection error from an independently estimated generalization error. At minimum, repeated resampling, bootstrap uncertainty, or an independent FEM test set should be introduced to quantify the instability associated with such small samples. The comparison between supervised ML and RBF interpolation is not fully like-for-like. Supervised models undergo Bayesian hyperparameter optimization and are assessed using dataset-size-dependent cross-validation, whereas the RBF is configured separately and evaluated through Leave-One-Out prediction. Final comparisons then emphasize FEM errors at the optimized candidate rather than full-domain predictive performance on an identical set of held-out geometries. A surrogate may be accurate near one accepted optimum yet poor elsewhere, and this does not establish general predictive superiority. The authors should evaluate RBF and all supervised alternatives on the same outer folds or the same independent FEM validation designs and report normalized RMSE, MAE, maximum error, uncertainty, and computational cost using identical outputs. The claim in the Highlights that RBF “matches supervised ML” should be retained only if such a controlled comparison supports it. The manuscript reports counts of how often each algorithm was selected across 100 output-specific modelling problems and interprets the frequency of SVR selection as evidence of its competitiveness. These 100 problems are not necessarily statistically independent because many arise from the same geometry families, adaptive iterations, and correlated FEM outputs. Moreover, the selected model is not always the lowest-RMSE model because the manuscript allows models within 5% of the best RMSE and then selects according to relative fold-wise dispersion. The 5% competitive threshold is itself arbitrary. The authors should provide the actual distribution of normalized prediction errors for each algorithm and preferably use paired comparisons across common modelling tasks. A sensitivity analysis on the 5% selection threshold is

also needed before concluding that one model family is systematically preferable. The adaptive FEM validation procedure verifies the surrogate locally but does not demonstrate that Differential Evolution has found a reproducible global optimum. Only the population-size factor, maximum iteration count, and convergence tolerance are reported. Mutation strategy, mutation factor, crossover probability, boundary handling, initialization, stopping logic, random seed, and the number of independent DE runs should be documented. Multiple independent optimizations should be performed for each final surrogate to establish whether the same geometry is repeatedly obtained. Otherwise, apparent thickness stabilization between two adaptive iterations may reflect optimizer repeatability problems rather than physical convergence. The absolute objective-error tolerance $|e_J| \leq 10$ is not dimensionally or statistically uniform across the geometry families. The final objective values range from approximately zero to several hundred. Consequently, an absolute error of 10 has very different meanings for F1/F3/F4 and F2. The acceptance metric should be normalized or expressed relative to an appropriate objective scale, with special handling for objective values near zero. The justification for the 5% variable-error threshold and 5% geometry-stability threshold also requires sensitivity analysis rather than being introduced as fixed empirical values. The F2 results deserve a separate mechanical discussion rather than being treated as another successfully optimized family. Both surrogate strategies drive one design variable to or close to its upper thickness limit, while the resulting damage indicators remain above the target and, for one window, above the stated complete-failure value. This suggests that the admissible design space may contain no feasible solution under the current loading protocol and fixed frame dimensions. If this is the case, F2 should be explicitly reported as infeasible under the imposed constraints. The authors should investigate whether the problem requires changes in frame thickness, window geometry, width, or other design variables instead of accepting a penalized but physically unacceptable optimum. Only window thicknesses are optimized, even though the manuscript presents damage redistribution as a general BDSL design problem. Keeping window height, spacing, corner radius, frame thickness, overall proportions, and other geometric parameters fixed may severely constrain the attainable damage distribution. The authors acknowledge this as future work, but it directly affects the interpretation of the F2 infeasibility. The conclusions should therefore avoid implying general optimization of BDSL geometry and instead state explicitly that the reported solution concerns thickness optimization within five predefined geometry families. The use of local shear distortion should be defined more carefully. The notation ϵ_{xy} is described as maximum local shear distortion, although engineering shear strain is commonly distinguished from the tensor shear-strain component. Depending on the convention, a factor of two may be involved. This becomes particularly important because the quantity is squared in the objective. The manuscript should define whether ϵ_{xy} is tensorial strain, engineering shear strain, or another distortion measure extracted from COMPACK and provide its exact computational definition and units. The robustness of the optimized geometries against uncertainty is not investigated. Material parameters, yield stress, cyclic hardening, contact properties, support flexibility, manufacturing tolerances, and experimental loading all contain uncertainty, yet the optimization is deterministic. A geometry located close to $D=90$ may cross the damage threshold under small perturbations. The authors should conduct at least a local uncertainty or sensitivity analysis around the final designs. Manufacturing tolerance on window thickness is particularly important because thickness is the principal design variable and several solutions lie near admissible bounds. The claimed computational efficiency should be quantified at workflow level. Figure 10 reports surrogate-training times, but the practical advantage of the framework depends primarily on the number and cost of nonlinear FEM analyses avoided. The manuscript should report average FEM runtime, hardware, total initial FEM cost, number of adaptive

FEM evaluations, cumulative training and optimization time, and an estimated cost for a direct FEM-based DE search or another appropriate high-fidelity optimization baseline. Without this comparison, statements about substantial computational efficiency remain qualitative. The data and code availability are insufficient for a computational methodology whose main contribution is algorithmic. Confidentiality of absolute industrial dimensions may reasonably prevent publication of proprietary geometry, but it should still be possible to provide normalized design variables, normalized target datasets, optimization scripts, random seeds, surrogate-training code, and sufficient solver settings to reproduce the methodological results. At minimum, the authors should specify exactly which data and code can be made available and which cannot. The loading-standard references require correction. The text states that the experimental tests followed ANSI/AISC 341-16 and ASCE 7-22, whereas References 32 and 33 are listed as AISC 2002 and ASCE 2017 documents. The cited editions must match the standards actually used, and the exact relevant provisions should be identified. This is particularly important because the loading protocol is part of the experimental and numerical validation basis. The novelty statement should distinguish more precisely among surrogate-assisted optimization, damage-aware objective formulation, and adaptive FEM enrichment. The literature review already includes several FEM-based, surrogate-based, statistical, shape, and topology optimization studies for metallic dampers. Therefore, the contribution cannot simply be characterized as applying surrogate optimization to a damper. The authors should identify which component is genuinely new and demonstrate it through direct ablation or comparison, for example optimization without adaptive retraining, optimization without damage terms, RBF without enrichment, and conventional supervised surrogate optimization. The conclusions currently make stronger statements than the evidence supports regarding optimized design reliability. The adaptive loop demonstrates local agreement between a surrogate and the same FEM model used to generate and enrich the dataset, but it does not independently validate the optimized geometry experimentally. The manuscript itself acknowledges that final optimized geometries should ultimately be tested. This qualification should be moved into the central conclusions, and the accepted candidates should be described as FEM-validated numerical optima rather than experimentally validated optimized devices. Finally, the revision should provide a technically traceable response to every issue above, particularly the physically infeasible accepted F2 solution, the energy-dissipation proxy, mesh-dependent damage aggregation, local FEM validation, small-sample surrogate comparison, and sensitivity of the objective and acceptance thresholds. Where reanalysis is required, the response should identify the corresponding revised equation, table, figure, dataset, or supplementary material. These issues affect the validity of the proposed optimization framework and cannot be resolved by textual clarification alone.

Reviewer 3

Comments

Reviewer 3 - Initial submission, 25/08/2026

Title: Comparison of surrogate strategies for damage-aware optimization of buckling-delayed shear-link dampers with adaptive finite element validation

The manuscript presents an adaptive surrogate-assisted optimization framework for buckling-delayed shear-link dampers using experimentally validated nonlinear FEM simulations, machine-learning/RBF surrogate models, and Differential Evolution. The topic is interesting and potentially suitable for Structures; however, several issues should be addressed before publication.

The authors considered a performance-based approach that is valid, but they also need to discuss how the use of the buckling-delayed shear-link dampers may improve the resilience, the is the state-of-the-art concept more than PBD. Please refer to the case studies for the assessment of resilience, such as:

10.1080/15732479.2025.2474714

10.1016/j.istruc.2025.108642

10.1016/j.rcns.2025.12.005

10.1016/j.soildyn.2026.110591

1. The practical seismic effectiveness of the optimized dampers should be demonstrated at the structural-system level, since the present study considers only isolated component response rather than the response of a building equipped with the proposed devices.
 2. At least one nonlinear time-history analysis of a representative structure incorporating the optimized damper would substantially strengthen the seismic relevance of the study and clarify its influence on global responses such as inter-storey drift, floor acceleration, and energy dissipation.
 3. The authors should discuss whether the optimum damper geometry obtained under the prescribed symmetric cyclic protocol would remain optimal when the device is subjected to irregular, asymmetric, and pulse-like deformation demands generated by real earthquakes.
 4. The seismic scope should be clarified because the study uses symmetric displacement-controlled cyclic loading rather than recorded earthquake-induced displacement histories.
 5. The FEM validation should be supported by quantitative error measures for peak force, stiffness, and cumulative dissipated energy rather than only graphical comparisons.
 6. A mesh-sensitivity study should be provided, particularly because the local damage indicators and buckling response may be strongly mesh dependent.
 7. The choice of averaging the 12 nodes with the highest TFDMap values should be justified because this definition may depend on the adopted mesh density.
 8. The authors should demonstrate that the distortion-based term used in the objective function is sufficiently correlated with the actual hysteretic energy dissipated by the device.
 9. The relative weighting of the damage penalties and dissipative-performance term should be justified because damage control clearly dominates the proposed objective function.
 10. The comparison between RBF and supervised ML models would be stronger if their predictive performance were evaluated using independent FEM test samples not included in the adaptive retraining process.
 11. The robustness of the supervised ML results should be discussed considering the very limited initial datasets, particularly the cases containing only 8 or 16 FEM simulations.
 12. The selected surrogate acceptance tolerances, including the 5% prediction error and objective-function error limit of 10, require clearer justification.
 13. The F2 results should be discussed in greater detail because the prescribed damage targets apparently cannot be fully satisfied within the selected design bounds.
 14. The generality of the conclusions should be moderated because only the window thicknesses are optimized while other important geometric parameters remain fixed.
 15. Additional numerical details such as element size, loading rate, time increment, contact parameters, and quasi-static verification should be provided to improve reproducibility.
 16. The Appendix tables appear in the middle of the References section; please correct the manuscript layout and place all Appendix material in the appropriate section.
- Overall, the proposed framework has publication potential, particularly because the surrogate-derived optimum designs are explicitly re-evaluated using FEM; however, the issues above should be addressed before the manuscript can be considered for

publication.

Reviewer 4

Comments

Reviewer 4 - Initial submission, 12/08/2026

The paper is well presented and it can be accepted after considering the following asked revisions:

- 1) The scientific merit and novelty of the article are not clear. The authors should explain clearly in the abstract what is the novelty of the proposed method and what is the added value in this article?
- 2) What is the motivation of the work? The problem considered does not have a sound motivation. The authors should clearly demonstrate the scientific interest of the objectives and results.
- 3) The literature review on FEM is very poor. For general readers, authors are encouraged to discuss other kind of works on FEM by discussing the following works: [(a) – Youzera et al. (2026), “Nonlinear Finite Element Model for the Dynamic Analysis of Sandwich Beams with a Frequency-Dependent Viscoelastic Core Layer Considering Normal Transverse Stresses”, *Mechanics Research Communications*, 154, 104719.; (b) – Bousmaha et al. (2026), “On the dynamic behavior of plates made of porous advanced materials reinforced with carbon nanotubes using a p-version of finite element method“, *Mechanics Based Design of Structures and Machines*, 54(1), 2534679.; (c) – Youzera et al. (2025), “Finite element formulation for free vibration analysis of porous FGM beams under thermal excitation”, *Computers and Concrete*, 36(4), 419-430.; (d) – Meftah et al. (2024), “Simplified homogenization technique for nonlinear finite element analysis of in-plane loaded masonry walls”, *Engineering Structures*, 306, 117822.; (e) – Tounsi et al. (2024), “A finite element approach for forced dynamical responses of porous FG nanocomposite beams resting on viscoelastic foundations”, *International Journal of Structural Stability and Dynamics*.; (f) – Belabed et al. (2024), “Accurate free and forced vibration behavior prediction of functionally graded sandwich beams with variable cross-section: A finite element assessment”, *Mechanics Based Design of Structures and Machines*, 52(11), 9144-9177.; (g) – Belabed et al. (2024), “Vibrational and elastic stability responses of functionally graded carbon nanotube reinforced nanocomposite beams via a new Quasi-3D finite element model”, *Computers and Concrete*, 34(5), 625-648.; (h) – Belabed et al. (2024), “Mechanical behavior analysis of FG-CNTRC porous beams resting on Winkler and Pasternak elastic foundations: A finite element approach”, *Computers and Concrete*, 34(4), 447-476.; (i) – Belabed et al. (2024), “Free vibration analysis of Bi-Directional Functionally Graded Beams using a simple and efficient finite element model”, *Structural Engineering and Mechanics*, 90(3), 233-252.; (j) – Lakhdar et al. (2024), “Free vibration and bending analysis of porous bi-directional FGM sandwich shell using a TSDT p-version finite element method”, *Acta Mech*, 235, 3657–3686.; (k) – Belabed et al. (2024), “On the elastic stability and free vibration responses of functionally graded porous beams resting on Winkler-Pasternak foundations via finite element computation”, *Geomechanics and Engineering*, 36(2), 183-204.; (l) – Bentrar et al. (2023), “Effect of porosity distribution on free vibration of functionally graded sandwich plate using the P-version of the finite element method”, *Structural Engineering and Mechanics*, 88(6), 551-567.].

- 4) Fig. 12 should be more discussed.
- 5) The conclusion section might be simplified. All findings should be given briefly, item by item.