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REVIEWER'S REPORT

Manuscript No.: IJAR-58683

Title: Toward SDN-Native Cybersecurity: Unified Threat Modeling, Formal Assurance, 7 Behavioral Detection, and Multi-Controller Resilience.

Recommendation:

Accept as it is ....

Accept after minor revision... ....

Accept after major revision ...... yes ....

Do not accept (*Reasons below*) ....

Rating	Excel.	Good	Fair	Poor
Originality				
Techn. Quality				
Clarity				
Significance				

Reviewer's ID: JPR-Dr.Shaweta Sachdeva

Detailed Reviewer's Report

1. The manuscript presents an interesting combination of CNN-Lite, IWHO-Lite, and Random Forest; however, the incremental novelty over the authors' previously published CNN-DNN/IWHO work should be demonstrated more convincingly. The authors should provide a direct comparison highlighting what is fundamentally new in the present methodology.
2. The proposed IWHO-Lite algorithm is central to the contribution, but the manuscript should provide a more rigorous explanation of how the binary version differs mathematically and computationally from the original IWHO algorithm.
3. The study uses only three independent seeds (42, 2027, and 314159). This is relatively limited for drawing conclusions about the stability of a stochastic metaheuristic optimization process. More repeated runs should be considered.
4. The authors should report the computational cost of IWHO-Lite itself, including optimization time, number of objective-function evaluations, and total training/selection time for each dataset. Dimensionality reduction alone does not establish computational efficiency.
5. The comparison between selected-latent and full-latent representations is currently not a fair controlled ablation, because the selected model uses 300 Random Forest trees whereas the reference uses 150 trees. Consequently, the observed performance differences cannot be attributed exclusively to latent-dimension selection.
6. The authors should conduct an equal-budget ablation experiment, keeping the Random Forest configuration, dataset size, preprocessing, and evaluation protocol identical for selected-latent and full-latent models.
7. The paper would be significantly stronger if it included comparisons against standard dimensionality-reduction methods such as PCA, mutual-information selection, recursive feature elimination, L1-based selection, and random latent-coordinate selection.
8. Since IWHO-Lite is a metaheuristic optimizer, comparison with other established binary optimization algorithms would help establish whether the observed performance is attributable to IWHO-Lite or simply to the general process of subset search.

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9. The Farm-Flow result is particularly concerning from a practical agricultural-IoT perspective. Although recall is very high (0.9523), precision is only 0.3143 and specificity is 0.3167, indicating a very high false-positive burden. The manuscript should provide a detailed analysis of the sources and consequences of these false alarms.
10. The extremely high CICIoT2023 F1-score of approximately 0.9953 should be examined carefully. The authors should provide class distributions, confusion matrices, attack-category-wise results, and additional validation to determine whether dataset characteristics or artifacts contribute to this performance.
11. The study uses Farm-Flow as the agricultural anchor and CICIoT2023/UNSW-NB15 as reference datasets. However, the manuscript should be careful not to imply cross-domain generalization, because strong performance on generic IoT/network datasets does not establish effectiveness in real agricultural environments.
12. The authors should include class-wise performance metrics rather than relying primarily on aggregate F1, precision, recall, and specificity. This would be especially valuable for identifying which attack categories are difficult to detect.
13. A confusion matrix for each dataset should be included. This would make the false-positive and false-negative behavior much easier to interpret.
14. The manuscript claims to use a leakage-aware protocol, which is a strength; nevertheless, the authors should clarify that exact-vector overlap removal does not eliminate dependencies caused by devices, sessions, farms, temporal correlations, or capture environments. The current discussion recognizes this limitation, but the distinction should be emphasized throughout the paper.
15. The choice of 128 latent dimensions as the initial representation should be experimentally justified. A sensitivity experiment using different latent sizes would establish whether the reported compression rates are robust to the initial representation dimensionality.
16. Similarly, the sparsity penalty and optimization parameters should be subjected to sensitivity analysis. The manuscript should demonstrate how population size, iteration count, sparsity coefficient, and restart count influence the selected dimensionality and final detection performance.
17. The authors should report baseline results without latent-dimension optimization, using the complete CNN-Lite representation with the same downstream classifier and identical experimental budget.
18. The paper should include a comparison between random latent-dimension selection and IWHO-Lite selection. Without such a baseline, it is difficult to determine whether the optimization algorithm is actually discovering informative subsets.
19. The calibration stage deserves further analysis because logistic score mapping does not appear to improve the aggregate calibration metrics. The authors should explain whether calibration provides any practical benefit and consider alternative calibration techniques.
20. The fixed F1-oriented threshold is suitable for reproducibility but may not represent a realistic agricultural cybersecurity deployment. The authors should investigate cost-sensitive threshold selection, where false negatives and false positives have different operational costs.
21. The claim of suitability for edge/gateway deployment should be moderated. The current experiments measure software-level latency, throughput, and model size, but do not measure peak RAM, energy consumption, thermal behavior, battery impact, or execution on an actual agricultural gateway.
22. If possible, the authors should evaluate the proposed model on a representative edge device, such as Raspberry Pi, NVIDIA Jetson, or another agricultural IoT gateway, and report inference latency, memory usage, CPU/GPU utilization, and energy consumption.

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23. The relationship between latent dimensionality and Random Forest model size is not monotonic, as demonstrated by the reported results. This is an important observation and should be analyzed more deeply rather than treating dimensionality reduction as synonymous with overall model compression.
24. The manuscript should report confidence intervals or statistical significance tests where appropriate rather than relying only on mean ± standard deviation from three seeds.
25. The related-work section could be improved through a comparative table of recent agricultural/IoT IDS methods, including dataset, model, feature-selection approach, dimensionality, F1-score, computational cost, and deployment platform.
26. The authors should clarify whether the proposed framework is intended primarily as a feature-selection method, representation-sparsification method, or complete intrusion-detection architecture. At present, these roles occasionally overlap conceptually.
27. The selected latent dimensions are encoder-dependent. The manuscript correctly notes that latent coordinate identities are not necessarily comparable across independently trained encoders. A useful additional experiment would therefore examine the stability of selected subsets using similarity measures such as Jaccard similarity across repeated runs.
28. The manuscript would benefit from a Pareto analysis of compression versus detection performance rather than reporting only a single selected solution. This could demonstrate whether IWHO-Lite produces meaningful trade-offs between sparsity and predictive performance.
29. The authors should clearly distinguish representation compression, model compression, computational efficiency, and energy efficiency, since these are different concepts. The current manuscript recognizes this distinction, but the terminology should remain consistent throughout.
30. The conclusion is appropriately cautious and acknowledges that the results are dataset-dependent. Nevertheless, the paper would benefit from a clearer statement of what is definitively demonstrated by the experiments and what remains an open hypothesis.

Overall recommendation

Major Revision.

The manuscript has a promising methodological idea and a strong emphasis on evaluation integrity. The most important revisions are to add fair equal-budget ablations, stronger baselines, more repeated runs, sensitivity analysis, detailed Farm-Flow false-positive analysis, and real edge-device evaluation. These additions would substantially strengthen the evidence supporting the proposed IWHO-Lite latent-dimension selection framework.