

## Peer Review

# Review of: "Aedes Distribution and Meteorological Effect on Ovitrap Index in Coastal Area of Besut, Terengganu: An Entomological Study"

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The Abstract includes the study objective and reports key results but suffers from ambiguous phrasing, lack of methodological clarity, and overinterpretation of the findings. It does not explain how ovitraps were deployed or how species were identified, and it fails to contextualize or cautiously interpret the statistical outcomes. I strongly recommend revising the Abstract for grammar, specificity, and a more balanced conclusion. Mention of statistical methods (e.g., Pearson correlation), exact temperature ranges, and a brief reflection on the implications or limitations would greatly enhance its quality.

The Introduction claims:

“To the best of our knowledge, there is no well-published study between Ovitrap Index with meteorological variables in Terengganu state..”

This is vague and insufficient as a clear gap statement. What about nearby coastal states? Or nationwide comparisons? Has such a study never been done in a coastal context? This should be supported with a brief literature comparison and a clearer rationale for why Kuala Besut is a meaningful study site.

The Materials and Methods section would benefit from significantly more comprehensive detail regarding the ovitrap surveillance protocol. At present, the manuscript does not specify key information about the ovitraps themselves—such as their design, color, material, dimensions, water volume, or whether oviposition substrates (e.g., paddles or filter papers) were used. These characteristics are critical to report, as oviposition behavior in *Aedes* mosquitoes is highly influenced by the physical traits of the trap, and reproducibility depends on such detail.

In addition, the manuscript refers to “counting and identifying on day 7 and day 11” without explaining:

What is being counted (eggs or larvae?).

Whether larval hatching procedures were applied after egg collection.

Whether both day 7 and day 11 involved different stages (e.g., preliminary collection on day 7 and post-hatching identification on day 11).

The method and criteria used for larval identification to species level (e.g., magnification, identification key, stage of larvae).

This dual timeline (day 7 and day 11) deviates from standard ovitrap monitoring protocols, where typically ovitraps are collected after 5–7 days, and either eggs are counted immediately, or larvae are allowed to hatch and identified within a standard incubation window. Without clarification, the scientific rigor and interpretability of the larval count data are compromised.

It is recommended that the authors explicitly describe:

The trap structure and oviposition medium,

What was counted at each time point (day 7 vs day 11),

The conditions for hatching, if applicable,

The taxonomic keys or identification tools used,

And how positive traps were defined for the Ovitrap Index calculation.

These improvements would greatly enhance the reproducibility and transparency of the entomological surveillance methodology presented in the study.

The Results section presents species-specific counts of *Aedes aegypti* and *Aedes albopictus*, implying confident identification of thousands of larvae. However, the manuscript does not explain how species identification was conducted, raising serious concerns about the accuracy of these results. Specifically:

What morphological criteria or identification keys were used?

At what larval instar was identification performed?

Were larvae preserved or reared to adult stage for confirmation?

What tools (e.g., stereomicroscope) were used, and were identifications verified?

Given the morphological similarity between early instars of *Ae. aegypti* and *Ae. albopictus*, particularly in large-scale field studies, these methodological details are critical for the credibility of the reported

species distribution. I strongly recommend the authors include a dedicated subsection in Methods detailing their larval identification process and any quality assurance procedures undertaken.

The Discussion assumes reliable identification and species-level differentiation between *Aedes aegypti* and *Aedes albopictus*, citing the dominance of *Ae. aegypti* in the study area, patterns of outdoor breeding, and temperature preferences.

However, as noted in previous comments, the Materials and Methods section does not describe the larval identification process, and the Results present species-level data without supporting the validity of that identification. This undermines the strength of the conclusions presented in the Discussion, especially those related to:

Species dominance (“*Ae. aegypti* was dominant”),

Habitat preference (“mostly breed outdoors”),

Temperature-specific conclusions (“temperature between 30–32°C contributed to highest number of *Ae. aegypti* larvae”).

Without clear evidence of how larvae were distinguished morphologically—or whether they were reared to adults for verification—these interpretations remain speculative. Furthermore, the authors compare their results with past studies involving *Ae. aegypti* dominance in coastal/fishing villages, reinforcing a conclusion that may not be solidly grounded in their own data.

To improve the scientific rigor of the Discussion, I recommend:

Rewriting species-level comparisons in a more cautious tone, unless identification procedures can be clearly documented and justified.

Acknowledging the limitation in species-level identification in the Discussion, especially if only larval stages were used and no adult confirmation was performed.

Focusing more on general *Aedes* spp. trends unless species-level accuracy can be demonstrated.

If species identification is confirmed, a summary of distinguishing features and validation methods should be briefly reiterated in the Discussion to lend credibility to the interpretation.

The Conclusion and Recommendations section restates key findings—particularly the dominance of *Aedes aegypti* and its outdoor breeding behavior—while emphasizing temperature as a major driver of *Aedes* density. It then recommends vector control interventions, such as source reduction and biological control, with some mention of climate change adaptation.

However, these conclusions and recommendations rest heavily on species-level interpretations that are not adequately supported by the described methodology. The absence of a clear, validated larval identification process in the Materials and Methods and the lack of explanation for species differentiation in the Results raise serious concerns about the strength and reliability of species-specific conclusions.

## **Declarations**

**Potential competing interests:** No potential competing interests to declare.