

Peer Review Report

Review Report on Health-related quality of life among Ukrainian war refugees compared to the general population in Estonia

Original Article, Int. J. Public Health

Reviewer: Atta Rehman

Submitted on: 06 Jan 2026

Article DOI: 10.3389/ijph.2026.1608807

EVALUATION

Q 1 Please summarize the main findings of the study.

Overall assessment

This is a well-conducted, policy-relevant comparative study using harmonized population surveys and a matched case–control design. The topic is timely, and the use of representative refugee data is a major strength. However, there are important methodological, analytical, and interpretative issues that must be addressed before the manuscript is suitable for publication in IJPH.

Several conclusions are overstated relative to the observed effect sizes, and key analytical decisions (matching, regression strategy, Tobit use, and variable exclusion) require clearer justification and sensitivity analysis.

Q 2 Please highlight the limitations and strengths.

Mentioned in comments below

Q 3 Please provide your detailed review report to the authors. The editors prefer to receive your review structured in major and minor comments. Please consider in your review the methods (statistical methods valid and correctly applied (e.g. sample size, choice of test), is the study replicable based on the method description?), results, data interpretation and references. If there are any objective errors, or if the conclusions are not supported, you should detail your concerns.

MAJOR COMMENTS (must be addressed)

1. Clinical and public health relevance of the observed EQ-5D difference is not justified

- The final adjusted difference between refugees and controls is -0.017 EQ-5D units ($p = 0.029$) (Table 2, Model 5).
- This difference is statistically significant but extremely small.

Problem:

- The manuscript does not discuss whether -0.017 represents a clinically or population-meaningful difference in HRQoL.
- Published literature typically considers 0.03–0.05 as the minimally important difference (MID) for EQ-5D-3L.

Required revision:

- Authors must explicitly discuss clinical relevance vs statistical significance.
- Either:

- o Cite EQ-5D MID literature and acknowledge the modest magnitude, or
- o Reframe conclusions to avoid implying substantial HRQoL impairment.

2. Interpretation of “refugee status effect” is analytically fragile

- In Model 3, the refugee–control difference becomes non-significant ($p = 0.932$) after adjustment for income and financial strain.
- In Model 4, the effect remains non-significant.
- The refugee effect becomes significant only after stepwise removal of non-significant variables (Model 5).

Problem:

- This pattern suggests that the “refugee effect” is model-dependent and sensitive to variable selection.
- Stepwise backward elimination is not recommended for causal interpretation and can inflate type-I error.

Required revision:

• Authors must:

- o Acknowledge explicitly that refugee status does not remain robust across models.
- o Justify the use of stepwise exclusion OR provide a fully adjusted model without variable deletion.
- o Consider presenting Model 4 as the primary model.

3. Matching strategy is incomplete and leaves residual confounding

- Matching was performed only on age and sex, yet Table 1 shows large residual differences in:
 - o Education
 - o Income
 - o Household financial strain

o Marital status

Problem:

- The design is described as a “matched case–control study,” but the matching controls for only two variables.
- Given the large socioeconomic imbalance, the study is closer to age–sex matched comparison, not full case–control matching.

Required revision:

• Authors must:

- o Clarify this limitation explicitly in Methods and Discussion.
- o Avoid implying that matching “minimized confounding” beyond age and sex.
- o Consider sensitivity analysis using:

- Propensity score matching or weighting, OR
- Standardization using inverse probability weights.

4. Use of Tobit regression requires stronger justification

- Tobit regression is used due to ceiling effects (EQ-5D upper bound).

• However:

- o Mean EQ-5D values are 0.742 and 0.763, not near the ceiling.
- o Proportion at full health (1.0) is not reported.

Problem:

- The manuscript assumes Tobit is optimal without demonstrating the extent of censoring.

Required revision:

• Authors must:

- o Report the proportion of respondents at EQ-5D = 1.0 in each group.
- o Justify Tobit over alternatives (e.g., OLS with robust SEs, fractional regression).
- o Ideally, add a sensitivity analysis using OLS to demonstrate robustness.

5. Apparent contradiction between stress, anxiety/depression, and discussion

- Table 1 shows lower perceived stress among refugees (21.7%) than controls (28.5%).
- Yet refugees report higher anxiety/depression on EQ-5D (63.5%).

Problem:

- This discrepancy is important but handled only speculatively in Discussion.
- It raises concerns about differential item interpretation or construct validity.

Required revision:

• Authors should:

- o Explicitly acknowledge this inconsistency as a measurement limitation.
- o Avoid speculative explanations unless clearly framed as hypotheses.
- o Consider whether EQ-5D anxiety/depression and stress measure different constructs, and state this clearly.

MINOR COMMENTS

6. Incorrect reference to EQ-5D-5L

- Discussion (line 181) refers to “EQ-5D-5L index score”.
- The study uses EQ-5D-3L throughout.

7. Overstatement of novelty

• The manuscript claims:

“first study to report HRQoL of Ukraine war refugees using EQ-5D”

Problem:

- References 14 and 15 already examine HRQoL among Ukrainian refugees.
- The novelty lies in population-based sampling, tool and direct comparison, not first use.

8. Reporting of prevalence ratios

- Methods mention “prevalence ratios with 95% CIs,” but only proportions are shown in Table 1.
- No PRs are presented.

9. Income variable interpretation

- Income loses significance after health adjustment.
- Discussion still emphasizes income effects.

10. Language and clarity

Minor but recurrent issues:

- “widower” vs “widowed”
- “statussince” (line 78)
- Some long sentences in Discussion reduce clarity

STRENGTHS (to be retained)

- Large, population-based refugee sample
- Harmonized instruments across surveys
- Transparent reporting of response rates
- Clear presentation of EQ-5D dimensions
- Policy-relevant interpretation

Recommendation to the Editor

Major revision

The manuscript has clear merit, but analytical robustness, interpretation discipline, and transparency must be improved before publication in the International Journal of Public Health.

PLEASE COMMENT

Q 4 Is the title appropriate, concise, attractive?

yes

Q 5 Are the keywords appropriate?

yes

Q 6 Is the English language of sufficient quality?

yes

Q 7 Is the quality of the figures and tables satisfactory?

Yes.

Q 8 Does the reference list cover the relevant literature adequately and in an unbiased manner?)

yes

QUALITY ASSESSMENT

Q 9 Originality

Q 10 Rigor

Q 11 Significance to the field

Q 12 Interest to a general audience

Q 13 Quality of the writing

Q 14 Overall scientific quality of the study



REVISION LEVEL

Q 15 Please make a recommendation based on your comments:

Major revisions.