**Evaluating Loneliness Measurements across the European Union**

**Revisions #2, Mary Louise Pomeroy**

As stated in an earlier revision, this article makes a much-needed contribution to advancing the research literature on loneliness. By evaluating psychometric properties of existing loneliness measurement tools, the study will yield important implications for policies and practices aiming to curb the growing loneliness epidemic. I applaud the authors for their methodological rigor. The text is well written and the authors provide an interesting summary of the findings from the exploratory fold. Furthermore, they have done a commendable job in addressing the reviewers’ previous comments. I have included several additional recommendations below that may strengthen the clarity and relevance of the article to end-users.

1. Line 121. Please consider citing the following article which discusses research measurement issues that stem from conflating social isolation and loneliness. Pomeroy ML, Mehrabi F, Jenkins E, O'Sullivan R, Lubben J, Cudjoe TKM. Reflections on measures of social isolation among older adults. Nat Aging. 2023 Dec;3(12):1463-1464. doi: 10.1038/s43587-023-00472-4. PMID: 37640906.
2. Line 121 and elsewhere. Please correct author last name “Probaska” to “Prohaska.”
3. Line 203. “Gap” should be plural.
4. Lime 297. I would like to see slightly more explanation on how the demographic quotas were determined.
5. Line 309. I believe “panelist” should be plural.
6. Pages 10 and 17. I do not see an initial definition for the acronyms NUTS, CINT, and JRT.
7. Line 680: I found that the way this data was presented was difficult to follow. Also, add a final percentage sign at end of the sentence.
8. Line 721. The authors go back-and-forth on whether the T-ILS is intended to measure general loneliness or social loneliness. I found it to be a bit contradictory and it confuses the authors’ interpretation of findings discussed in the final “Summary of the Exploratory Fold” section.
9. Line 735. Remove “the” from the sentence “…that ~~the~~ how…”
10. Line 738. Very interesting discussion that I truly enjoyed reading. After digesting the authors’ interpretation, there are two points I would like to push back on, asking the authors explore them further.
	1. In hindsight, I think it makes sense that the emotional loneliness subscale correlates more closely with depression, whereas the social loneliness subscale correlates more closely with social support. To some extent, I’d expect most measures of loneliness to correlate closely with indicators of depression. Social support might better capture access to resources, or possibly levels of social contact that are indicative of social isolation (or a lack thereof). Loneliness and social isolation are weakly to moderately correlated at best, whereas research consistently shows higher correlations between loneliness with depressive symptoms. Loneliness might be thought of as an aspect of mental health, not unlike depression. Thus, while counterintuitive, I am not sure I agree that these correlational relationships point to weaknesses in three of the four loneliness measures. I could be swayed, but need the authors to provide further rationale to convince me.
	2. I do not find myself fully satisfied with the authors’ determination that the DJG social loneliness subscale is preferable to the TIL-S. I think this determination should be clarified by revisiting the overarching purpose of the study. Is it to advance a single measure that can reliably monitor loneliness across the entire E.U. population? If so, the TIL-S demonstrated scalar invariance across all countries, plus superior factor structures and reliability. Or, is the goal to explore differences in loneliness by culture and country, so that we may inform the most accurate measurement of loneliness, even if measurement tools may necessarily differ by region? If so, perhaps the social loneliness subscale is the best fit, as it may vary by clusters of EU countries. I think each goal is equally valid and, if I am not misinterpreting the authors’ findings, each may lend itself to a different measurement recommendation. Further clarification and discussion may elucidate this.
11. Line 749. “with the exception of, for instance, Bulgaria.” What finding is this referring to?
12. General comment: At some point it will be helpful to see a discussion of the limitations, which I imagine will be written in a subsequent stage. I would like the authors to address two issues. First, what are the limitations of using an online survey? What does this mean for how the sample might be skewed? For instance, are we measuring these constructs in a slightly younger sample, or a sample with higher socioeconomic status? Are we excluding the *most* lonely individuals or those with less social contact, who by way of their social exclusion may be less likely to participate in this study? A second minor point is that I am curious about country-specific findings. For example, what are the implications for Finland, France, and Romania, wherein the authors did not observe insufficient internal consistency of the DJGLS-6?