PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (http://bmjopen.bmj.com/site/about/resources/checklist.pdf) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

| TITLE (PROVISIONAL) | LIFE EXPECTANCY: WHAT DOES IT MEASURE? |
|---------------------|----------------------------------------|
| AUTHORS             | Modig, Karin; Rau, Roland; Ahlbom, Anders |

VERSION 1 – REVIEW

| REVIEWER           | Joseph T. Lariscy |
|--------------------|-------------------|
| University of Memphis, USA |               |
| REVIEW RETURNED    | 09-Mar-2020       |

GENERAL COMMENTS

This Communication submission describes how life expectancy is calculated, describes problems with life expectancy, and shows how presenting relative risk ratios or hazard ratios may be a more appropriate alternative when comparing mortality risk between groups or time periods. The two key problems with life expectancy the paper mentions are that life expectancy 1) most often refers to a synthetic cohort rather than an actual birth cohort and 2) does not wholly standardize by age.

I feel this paper fits well as a Communication rather than an Original Research article. It has a clear message that can be communicated without the structure of a full article (Introduction, Methods, Results, Discussion).

I have one suggestion about making a stronger recommendation for readers. I feel the authors should encourage researchers to report both absolute differences and relative differences when possible. The current recommendation the authors put forth in the Abstract is, "We conclude that life expectancy is not the measure of choice in etiological research or in research with the aim to identify risk factors of death, but that life expectancy may be a compelling choice in public health contexts." This take-away message to readers seems mixed about the value of life expectancy over risk ratios. In some applications, rate ratios may be misleading. Even when a rate ratio or hazard ratio of mortality is statistically significant and seems large, the difference in mortality risk may be quite modest when translated to an absolute measure of mortality difference, such as life expectancy or potential years of life lost.

Related to the suggestion above, an article published in BMJ by King and colleagues (2012) reviewed the literature to observe how often health inequalities research reports relative effects and/or absolute effects. I provide the citation below. King et al. make a compelling case (as the authors of the current manuscript do) that relying exclusively on absolute measures (e.g., life expectancy) or relative measures (e.g., rate ratios) may be misleading. King et al. also note, “two initiatives aimed at improving the reporting of
evidence in biomedical literature—the consolidated standards of reporting trials (CONSORT)12 and strengthening the reporting of observational studies in epidemiology (STROBE)13—now recommend reporting both absolute and relative measures of effect whenever possible.” Thus, I feel the authors of the current study could make a similar recommendation.

King, N. B., Harper, S., and Young, M. E. (2012). Use of relative and absolute effect measures in reporting health inequalities: Structured review. BMJ 345(e5774): 1–8.

**GENERAL COMMENTS**

Life expectancy provides a summary measure of age-specific mortality rates. This measure is often times used to evaluate public health (and social security) policies. The current contribution asks what life expectancy is (really) measuring. The paper motivates the importance of the question by highlighting how previous recent works have failed to account for age-standardizations (or clearly stating assumptions) in the conversion from death rates to life expectancy. Moreover, using an explanatory example of hypothetical “cancer eradication”, the authors show how misleading conclusions one can make.

Generally, I have great sympathy for paper’s endeavors to constructively correct for misinterpretations in previous literature. I agree with the authors that we should be careful with the use of different mortality measures for different purposes. While the authors provide some references I am no sure how frequent such misinterpretations are in general. My sense is that some of the key conclusions that paper makes are already conceptually known. The first conclusion in the “Discussion and Conclusion” section clarifies the distinction between period and cohort life expectancy. We should be careful about interpreting the results, as period life expectancy assumes that we can use older cohorts’ mortality rates at higher ages to evaluate current younger cohorts’ life expectancy. I believe it is well known that this would only be the case in an unchanged society after all.

The second conclusion puts another example of how the age-dependent weights used in life-expectancy measures can be misinterpreted. That LE is a construct of age-specific mortality rates, also seems to be a well-known feature of the measure. The motivating cancer eradication example is very illustrative, but could be misleading. The analysis relies (as the authors mention) on the independence assumption of competing risks. It is well known that this is a very crude assumption, which can lead to erroneous conclusions. See for instance Honore and Lleras-Muney, 2006 (https://doi.org/10.1111/j.1468-0262.2006.00722.x). Some clarifications need to be done: Table 1 requires much more explanation. I am not sure how the numbers in the text correspond to those in the table.
This Communication submission describes how life expectancy is calculated, describes problems with life expectancy, and shows how presenting relative risk ratios or hazard ratios may be a more appropriate alternative when comparing mortality risk between groups or time periods. The two key problems with life expectancy the paper mentions are that life expectancy 1) most often refers to a synthetic cohort rather than an actual birth cohort and 2) does not wholly standardize by age.

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King, N. B., Harper, S., and Young, M. E. (2012). Use of relative and absolute effect measures in reporting health inequalities: Structured review. BMJ 345(e5774): 1–8.

Authors response: Reviewer 1 points out that both relative and additive measures may be misleading and suggests reporting also basic rates whenever possible; a suggestion to which we agree. However, often when life expectancy is used the purpose is a one-dimensional measure of mortality, or even public health, in which case there may be no place for basic numbers. We have nevertheless revised the text and now emphasize that whenever possible raw data should be provided, and not least in connection with such a convoluted measure as life expectancy. We thank Dr. Lariscy for the reference about usage of relative and absolute measures, and have included this reference in our manuscript.
Life expectancy provides a summary measure of age-specific mortality rates. This measure is often times used to evaluate public health (and social security) policies. The current contribution asks what life expectancy is (really) measuring. The paper motivates the importance of the question by highlighting how previous recent works have failed to account for age-standardizations (or clearly stating assumptions) in the conversion from death rates to life expectancy. Moreover, using an explanatory example of hypothetical “cancer eradication”, the authors show how misleading conclusions one can make.

Generally, I have great sympathy for paper’s endeavors to constructively correct for misinterpretations in previous literature. I agree with the authors that we should be careful with the use of different mortality measures for different purposes. While the authors provide some references I am not sure how frequent such misinterpretations are in general. My sense is that some of the key conclusions that paper makes are already conceptually known.

Authors response: Reviewer 2 suggests that some key conclusions in our paper are conceptually known already which is absolutely correct as evidenced for example by reference #6 in our manuscript. Yet, the life expectancy measure of mortality is frequently used with no consideration of these limitations and concerns. We therefore thought that a fresh look at the issues with a different mode of presentation might be helpful. During the work on this paper it has also become evident to the authors that while the issues related to use of period data are easily accessible to many, yet easily forgotten, the nature of the way that life expectancy conducts age standardization is less evident. We have revised the text to clarify this.

The first conclusion in the “Discussion and Conclusion” section clarifies the distinction between period and cohort life expectancy. We should be careful about interpreting the results, as period life expectancy assumes that we can use older cohorts’ mortality rates at higher ages to evaluate current younger cohorts’ life expectancy. I believe it is well known that this would only be the case in an unchanged society after all.

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Authors response: Reviewer 2 points out the importance of considering dependency between risk factors for different causes of death when estimating the effect on life expectancy from preventing one cause of death, in our example cancer. We have emphasized this point even further in the text. We thank Dr. Nielsen for the reference on this topic. The question to what extent eradication of cancer would increase the mortality from some other disease, and through that also total mortality is one additional level of complexity that our example does not include.
Some clarifications need to be done: Table 1 requires much more explanation. I am not sure how the numbers in the text correspond to those in the table.

Authors response: We thank the reviewer for this comment and conclude, after some thought, that this section of the manuscript can be removed without altering our message.

**VERSION 2 – REVIEW**

| REVIEWER           | Joseph T. Lariscy                               |
|--------------------|-------------------------------------------------|
|                    | University of Memphis, USA                      |

| REVIEW RETURNED    | 27-May-2020                                     |

| GENERAL COMMENTS   | The authors satisfactorily addressed my comments from the first review. |