Review of: "Using a Health and Demographic Surveillance System to Assess Stillbirths Trends and Risk Factors in Siaya County, Kenya between 2008 and 2019"

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A review of the manuscript

“Using a Health and Demographic Surveillance System to Assess Stillbirth Trends and Risk Factors in Siaya County, Kenya between 2008 and 2019."

This paper responds to the important need to document stillbirth levels, trends, and covariates in an African population. As the authors note, this topic is rarely addressed in the scientific literature owing to the absence of reliable data. The analysis that is presented is based on longitudinal demographic and health data compiled from a large population in Kenya over a 12-year period that is covered by a population surveillance system. The data permit assessment of maternal age covariation and the ordinal time observation of stillbirth rates.

Demographic surveillance as a methodology

The title implies that the paper has a focus on demographic surveillance as a method of data compilation. The presentation is appropriately focused on levels and time trends in stillbirths. Yet, the health and demographic surveillance system (HDSS) tool merits a basic description that is deficient in minor ways.

1. With the following statement, the authors appear to claim that they are the founders of the system that has provided the data for this research: “We established a health and demographic surveillance system in a rural area of western Kenya to measure the burden of infectious diseases and evaluate public health interventions.” It is the impression of this reviewer that the design, implementation, and management of the system antedate the years of surveillance used for this study. It is possible that the authors were not involved in the process of designing and implementing the system. If this is the case, the sentence should be revised with citation of the original system developers. .
2. The KEMRI computer system is unmentioned in the paper, despite the fact that computing technology is vital to the HDSS paradigm. There are multiple HDSS systems operating in Africa and Asia, some of which represent technical applications of generalizable systems, such as the Open-HDS (1), the Household Registration System (HRS) (2), the Agincourt System (3), or other HDSS applications (4). Whether the KEMRI system is unique or adapted from other such systems merits clarification and relevant citations. The technical review by Adazu et al. (5) is a helpful citation, but brief mention of the technical platform merits citation or clarification.

3. The paper was written in 2023, but the data used for the analysis terminate in 2019. Given the fact that the KEMRI DSS is updated in six-month cycles and the system continued to function in 2023, why is the time series truncated at 2019? Extending the analysis for the maximum duration would permit appraisal of the significance of the observed stillbirth decline. It is possible that the time trend is non-linear, reaching a lower asymptote or possibly sustaining further decline. Given the importance of the time trend, it is vital to apply all available data to the time series analysis.
4. The HDSS method of inference has important advantages that the authors note. However, there are limitations associated with the HDSS concept that merit citation. Perhaps citation of classical literature on this method would address this minor presentation gap.

Statistical methodology.

1. At times, the authors employ unconventional syntax in describing statistical outcomes. If results are statistically significant, that constitutes a null result that should be stated as such. The following passage is an example of text that reads as an attempt to represent a null result as a significant outcome:

Although not statistically significant, the study findings show that the chances of experiencing stillbirth are directly proportional to age.

Again, there is another passage that is similarly incorrectly expressed:

Attending at least four antenatal care (ANC) clinic visits shows a protective effect (OR = 0.61, 95% CI: 0.34, 1.09), although not statistically significant.

Instead, the finding is appropriately stated, “The variable scored as 1 for attending at least four antenatal care (ANC) clinic visits versus zero for three or fewer visits showed no protective effect (OR = 0.61, 95% CI: 0.34, 1.09)..

1. The statistical methodology is flawed. Consider first, Figure 1. This portrays a set of time series lines for stillbirth rates observed in three counties over 12 years, constituting 36 observations plus a series for the average across all three counties. Since readers are unlikely to be concerned about cross-county variance, the key message presented in the text appropriately focuses on the average. As a reviewer, I recommend presenting the data as a scatter plot of the 36 points with a time series trend line for the average. This would be a polynomial fitted to the points, with a squared term for time that permits appraisal of the non-linearity of the pattern that is apparent in Figure 1. Early in the series, rates decline, but as time progresses, the rate of change decreases with the trendline eventually flattening. This pattern could also be portrayed with a regression line fitted to individual level data, but for the purpose of the Figure, aggregated data will suffice. What is incorrect is presenting these data as a portrayal of decline without acknowledgement of the leveling effect. The data suggest that the decline is not a linear trend. Connecting the 36 points with lines denies the reader of any interpretation of the information that Figure 1 portrays.
2. Living in Rarieda sub-County and having pregnancy terminations in two of the years is unusual, but unexplained by the data. Conditions such as epidemics or health care interventions could account for these outliers, but speculation is unlikely to be convincing given the data limitations.

3. The regression analysis uses maximum likelihood procedures that assume independence across observations. The authors have employed STATA for their analysis. A generalized logit model is indicated that allows for the grouping of observations by time point and by county.
4. The procedure for developing the multivariate model is unconventional. The authors appear to have applied bivariate analysis to seek statistically significant relationships that are then used as a multivariate specification that seeks to identify significant net effects. (This may be an incorrect interpretation. Table 3 has a full set of indicators). If the selective procedure has been used, a more conventional approach is recommended that would include all variables in the analysis, with a pre-specified theory of their effects and regressions undertaken to test that theory. Moreover, there are possible conditional relationships that merit investigation. For example, relative poverty might have no main effect, but interact with other variables, such as 4+ ANC visits, to show that ANC intensity has limited relative effects among most women, but significant benefits among the very poor. Including multiplicative effects to test interactions merits consideration.
5. In the regression, year is expressed as a series of dummy variables. To gauge the effect of time, year should be a single continuous variable scored as an ordinal count of the year of observation. Then, to assess non-linearity, a squared term for time would assess the significance of a departure from a linear downturn.
6. Exploration of time interactions may be informative. This would involve sorting the data by early time, mid-time, and late and repeating the table 3 analysis for each subset of data without time in the model. Comparing the regression OR across time-specific results would assess the proposition that the relative roles of covariates has changed as the still-birth risk has declined with the passage of time..

The discussion

Interpreting results requires clarification of what types of care are associated with ANC services. If the quality and range of care options are limited, 4 visits are, by definition, ineffective. But, if there are procedures for detecting risk, referring cases, and caring for high-risk pregnancies, then results could be readily related to care options. The fact that one county has lower stillbirth rates than other countries suggests that something about the system of care in that locality is superior and that these features of the care system merit replication and scale-up elsewhere. Without adequate clarification of the care context, utilization of the results is impaired.

Conclusion of this review.

This manuscript represents an important investigation using data of unusual quality and completeness for the study of stillbirths. As the authors note, the opportunity that KEMRI data provide may be unparalleled. The work that is presented therefore merits publication, but the credibility of the analysis could be greatly enhanced if basic revisions are undertaken.

Descriptive outcomes of this investigation merit publication because of the observational rigor that demographic surveillance accommodates, and also because observation is population based, rather than clinical facility based. As the authors note, clinical data are subject to selection biases and other limitations that are not associated with demographic

surveillance. Moreover, there are other demographic surveillance sites in Kenya, neighboring countries, and regions of Africa that could benefit from the example that this investigation represents. While the analysis that is presented is sound in important respects, there are limitations of this presentation that constrain replication of this research. These limitations could be readily addressed with a revision. Future studies of still birth levels and trends could benefit from this investigation if limitations of the presentation are addressed.

Citations appearing in the comments

1. Homan T, Di Pasquale A, Kiche I, Onoka K, Hiscox A, Mweresa C, et al. Innovative tools and OpenHDS for health and demographic surveillance on Rusinga Island, Kenya. BMC Res Notes. 2015;8(397).

2. Macleod B, Leon D, Phillips J. The Household Registration System: A Database Program Generator for Longitudinal Studies of Households. Soc Sci Comput Rev. 1992;10(3).

3. Kahn K, Collinson MA, Gómez-Olivé FX, Mokoena O, Twine R, Mee P, et al. Profile: Agincourt Health and Socio-demographic Surveillance System. Int J Epidemiol [Internet]. 2012;41(4):988–1001. Available from: https://doi.org/10.1093/ije/dys115

4. Sankoh O, Sharrow D, Herbst K, Kabudula CW, Alam N, Kant S, et al. The INDEPTH standard population for low- and middle-income countries, 2013. Glob Health Action. 2014;7(November 2013):23286.

5. Adazu K, Lindblade K, Rosen D, Odhiambo F, Ofware P, Kwach J, et al. Health and demographic surveillance in rural western Kenya: a platform for evaluating interventions to reduce morbidity and mortality from infectious diseases. Am J Trop Med Hyg. 2005;73(6):1151–8.