

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Life-Course Socio-Economic Status and Breast and Cervical Cancer Screening; Analysis of the WHO study on Global Ageing and Adult Health (SAGE)
AUTHORS	Akinyemiju, Tomi; Ogunsina, Kemi; Sakhuja, Swati; Ogbhodo, Valentine; Braithwaite, Dejana

VERSION 1 - REVIEW

REVIEWER	Jissa Vinoda Thulaseedharan Achutha Menon Centre for Health Science Studies(AMCHSS), Sree Chitra Tirunal Institute for Medical Sciences and Technology (SCTIMST) Trivandrum, Thiruvananthapuram - 695011, Kerala (State), India .
REVIEW RETURNED	18-Jun-2016

GENERAL COMMENTS	<p>I am very happy to get an opportunity to review this manuscript. I appreciate your efforts in preparing a well written paper. However I have few comments/ suggestions which are listed below.</p> <ol style="list-style-type: none">1. In the abstract, you have said that you used “survey weighted multivariable regression analysis”, but in the statistical analysis part (in the article) nothing said about survey weighted, rather you mentioned “multivariable logistic regression models”. If you did survey weighted analysis, please incorporate what exactly the meaning of survey weighted, and the steps you have done etc so that it will avoid confusion. Or else you correct the abstract.2. In the Introduction part, you started with the statement “Breast and cervical cancers remain the most common malignancies in women worldwide”. The referenced article was published in 2003, please use most recent literature. I found that cervical cancer is the fourth most common cancer among females (after breast, colorectal, and lung cancers) (ref: Globocan 2012).3. The second statement in the first paragraph of Introduction seems quite vague, better to give more data on incidence and mortality trends of cervical and breast cancer in LMIC’s and UIC’s separately. Please verify whether incidence and mortality trends of cervical and breast cancer show same or different patterns in LMIC’s and UIC’s. Here also you have to have a more detailed literature search.4. Introduction, first paragraph, line number 10 (Whereas IARC.....poor health issues.): better to use two simple sentences rather than combining them.5. I observed another discrepancy between abstract and Introduction. In the abstract you mentioned that SES difference in screening have not been well documented in developing countries. But in the second paragraph of introduction (first part of first sentence), you were saying that “multiple studies in LMICs have shown that low SES is a major predictor of the lack of cancer
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	<p>screening [24-27].</p> <p>6. Coming to the methods and results section, education and employment status were used to better capture the variability in SES as they were considered as robust measures. However I am confused with few things, more concern is about employment status. I am not aware about other countries but at least in India a large proportion of women are home makers (housewives), they have no wages. They are dependence of their husbands or other family members. So just have a think about what exactly the employment status in this study stands for? Further you created two categories “employed” and “unemployed”. So, now my concern is about where these large groups of women (homemakers) were categorized into? If the variable is a proxy to measure whether the women have some resources (salary of their own) to attend screening, then these women should be categorized into unemployed. Also it is important to make sure that the employment status of women and employment status of their mothers was also defined in a similar way. Please do clear these doubts.</p> <p>7. Better to add in the methodology that the life course SES was defined based on maternal and paternal factors separately.</p> <p>8. So, the next concern is about the definition of life course SES. If the mother’s SES and fathers SES have independent role in determining the women’s SES, it will be useful to separately define the life course SES based on maternal and paternal factors. However there may be a chance of identifying a woman with stable (or low) life course SES based on one of the parental factor, but she may have another life course SES based on the other parental factor. Would it be possible to have another variable which shows the parental SES (for eg: Parental employment status: both mother and father unemployed, any one of the parent employed, both parents employed) and connect it with employment status of women and define the life course SES. So, that you would be able to explain the life course SES by considering the maternal and paternal SES together. Then the tables (describing life course SES) would be expanded with two additional variables parental educational status and parental employment status. (However you need to have some additional analysis for doing this, so it is optional)</p> <p>9. In titles of table 4 and 5 add “multivariable logistic regression analysis”</p> <p>10. In table.5, I really have a concern about the adjustments with own SES and parental SES variable. For eg: you have a life course variable “life course educational status defined using educational status of mothers”. Of course you can include the variables: mother’s employment, father’s employment, own employment, and father’s education in the model. But do you really need to include mother’s educational status and own educational status in the same model? I am not sure whether it is a problem with the explanation given under the table.</p>
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REVIEWER	Dr Bernadette Fisher Universities of Birmingham and Manchester, UK
REVIEW RETURNED	25-Jul-2016

GENERAL COMMENTS	This is a very interesting paper and findings could help healthcare workers/promoters better target interventions to increase screening uptake. However in the introduction, I feel more justification is needed regarding the selection of included countries and particularly how they differ or are the same in areas associated with outcomes,
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	<p>e.g. whether national screening programmes are offered, if free, the availability of compulsory/free primary/secondary education, literacy levels etc. among the target populations. A summary table highlighting these data together with some justification in selecting this particular country cluster, should be included in the Introduction. The results section was very detailed but country specific detail often obscured more general points and trends, which should clearly feed into the Discussion. The latter also needs to be less repetitive of findings, refer more to the literature and suggest ideas/ways that findings might be used by public health, educational or social practitioners to address lack under utilisation of screening at key points in the life course. Otherwise, it all seems rather academic.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Abstract

1. In the abstract, you have said that you used “survey weighted multivariable regression analysis”, but in the statistical analysis part (in the article) nothing said about survey weighted, rather you mentioned “multivariable logistic regression models”. If you did survey weighted analysis, please incorporate what exactly the meaning of survey weighted, and the steps you have done etc so that it will avoid confusion. Or else you correct the abstract.

Response: The methods section has been updated to include the use of survey weighted regression analysis

Introduction

2. In the Introduction part, you started with the statement “Breast and cervical cancers remain the most common malignancies in women worldwide”. The referenced article was published in 2003, please use most recent literature. I found that cervical cancer is the fourth most common cancer among females (after breast, colorectal, and lung cancers) (ref: Globocan 2012).

Response: The introduction and references has been updated with the latest Globocan report and statistics

3. The second statement in the first paragraph of Introduction seems quite vague, better to give more data on incidence and mortality trends of cervical and breast cancer in LMIC’s and UIC’s separately. Please verify whether incidence and mortality trends of cervical and breast cancer show same or different patterns in LMIC’s and UIC’s. Here also you have to have a more detailed literature search.

Response: A more detailed description of incidence and mortality trends for breast and cervical cancer in LMICs and UICs have been provided in the introduction

4. Introduction, first paragraph, line number 10 (Whereas IARC.....poor health issues.): better to use two simple sentences rather than combining them.

Response: Thank you for the suggestion. This has been edited

5. I observed another discrepancy between abstract and Introduction. In the abstract you mentioned that SES difference in screening have not been well documented in developing countries. But in the second paragraph of introduction (first part of first sentence), you were saying that “multiple studies in LMICs have shown that low SES is a major predictor of the lack of cancer screening [24-27].

Response: Thank you for the observation. This has been updated to reflect that few studies have examined SES in relation to cancer screening in LMICs, in particular focusing on lifecourse SES

Methods and Results

6. Coming to the methods and results section, education and employment status were used to better capture the variability in SES as they were considered as robust measures. However I am confused with few things, more concern is about employment status. I am not aware about other countries but at least in India a large proportion of women are home makers (housewives), they have no wages. They are dependence of their husbands or other family members. So just have a think about what exactly the employment status in this study stands for? Further you created two categories “employed” and “unemployed”. So, now my concern is about where these large groups of women (homemakers) were categorized into? If the variable is a proxy to measure whether the women have some resources (salary of their own) to attend screening, then these women should be categorized into unemployed. Also it is important to make sure that the employment status of women and employment status of their mothers was also defined in a similar way. Please do clear these doubts.
Response: Thank you for the suggestion. The methods section has been updated to further clarify that ‘unemployed’ for both mother and father includes homemakers, retired, and those otherwise not employed for any reason

7. Better to add in the methodology that the life course SES was defined based on maternal and paternal factors separately.

Response: Thank you for the suggestion. This has been updated

8. So, the next concern is about the definition of life course SES. If the mother’s SES and fathers SES have independent role in determining the women’s SES, it will be useful to separately define the life course SES based on maternal and paternal factors. However there may be a chance of identifying a woman with stable (or low) life course SES based on one of the parental factor, but she may have another life course SES based on the other parental factor. Would it be possible to have another variable which shows the parental SES (for eg: Parental employment status: both mother and father unemployed, any one of the parent employed, both parents employed) and connect it with employment status of women and define the life course SES. So, that you would be able to explain the life course SES by considering the maternal and paternal SES together. Then the tables (describing life course SES) would be expanded with two additional variables parental educational status and parental employment status. (However you need to have some additional analysis for doing this, so it is optional)

Response: Thank you for the suggestion. We agree that this would be a useful addition to the manuscript and have included lifecourse SES based on both parents SES combined in relation to individual SES in table 5.

9. In titles of table 4 and 5 add “multivariable logistic regression analysis”

Response: Thank you for the suggestion. This has been edited

10. In table.5, I really have a concern about the adjustments with own SES and parental SES variable. For eg: you have a life course variable “life course educational status defined using educational status of mothers”. Of course you can include the variables: mother’s employment, father’s employment, own employment, and father’s education in the model. But do you really need to include mother’s educational status and own educational status in the same model? I am not sure whether it is a problem with the explanation given under the table.

Response: We have clarified the footnote to reflect that each life-course SES variable (defined based on parental and own SES) is analyzed in separate models, and adjusted for study covariates.

Reviewer: 2

Introduction

1. I feel more justification is needed regarding the selection of included countries and particularly how they differ or are the same in areas associated with outcomes, e.g. whether national screening programs are offered, if free, the availability of compulsory/free primary/secondary education, literacy levels etc. among the target populations. A summary table highlighting these data together with some justification in selecting this particular country cluster, should be included in the Introduction.

Response: We have included a justification in the methods section stating that we focused on five countries: China, India, Mexico, Russia and South Africa, one from each continent, to allow examination of middle-income countries that have experienced major economic and health transitions over the past few decades and face rising burden of non-communicable diseases.

Results

2. The results section was very detailed but country specific detail often obscured more general points and trends, which should clearly feed into the Discussion.

Response: Thank you for this suggestion, we edited the results in order to be more succinct
Discussion

3. The latter also needs to be less repetitive of findings, refer more to the literature and suggest ideas/ways that findings might be used by public health, educational or social practitioners to address lack under-utilization of screening at key points in the life course. Otherwise, it all seems rather academic.

Response: Thank you for this suggestion. The results and discussion has been edited to be more succinct and to focus on potential strategies to improve screening rates.

VERSION 2 – REVIEW

REVIEWER	Jissa Vinoda Thulaseedharan Achutha Menon Centre for Health Science Studies (AMCHSS), Sree Chitra Tirunal Institute for Medical Sciences and Technology (SCTIMST), Trivandrum, Kerala, India.
REVIEW RETURNED	13-Sep-2016

GENERAL COMMENTS	<p>It is with great pleasure I have gone through the revised manuscript. Now it is more legible without having much ambiguity especially in methodology part. However, I have few comments;</p> <p>1. The result and conclusion in the abstract needs further revision. Your objective is to examine individual, parental and life-course SES differences in breast and cervical cancer screening. So better to add important results of all these three items, here you missed parental SES differences in screening.</p> <p>Word limit is an issue while writing abstract, but the statement should have a complete sense when limiting the words. For example, “stable life-course SES” increased breast cancer screening by 3 fold. Instead you can clearly mention “stable life–course SES based on maternal employment” increased breast cancer screening by 3 fold. Also the statement should end up with “compared to stable low life-course SES”. Otherwise it is incomplete. Most often people read the abstract first and then only read the full text, so it should be clear and concise. Similarly the last statement of the result section (please add the reference group).</p> <p>The first part of conclusion seems good, but the second statement (addressing social...) is very general. I wonder whether this statement weakens the message of this study.</p> <p>2. In the discussion part, you can discuss few more things, for eg: You can discuss on availability of organized screening programmes</p>
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	<p>in the health systems of all countries included in the study. For instance, almost all states in India have no organized screening programme, currently only 2 out of 29 states in India incorporated cervical and breast cancer screening in their health systems. That is the reason for observing a very low screening rate irrespective of SES in India. If there is no facility then SES has no role in determining the screening rate because women in all strata lack the facility. Hence the association between SES and screening participation is country specific.</p> <p>Another point that can be discussed is the mechanism of the association between parental employment status and daughter's screening status. Mother's employment in public sector may be connected with higher educational attainment of daughters. From your data itself you can see the interplay between these two variables if you just cross tabulate them.</p>
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REVIEWER	Dr Bernadette Fisher Universities of Birmingham and Manchester, UK
REVIEW RETURNED	18-Sep-2016

GENERAL COMMENTS	<p>A few minor points: Page 11, the second paragraph (lines 41 to 54) is written rather unclearly obscuring the points being made; page 15, line 43, should be, 'college degree OR higher' not of; page 17, line 34, Schmeisser et al, needs a date and the last sentence of this paragraph (lines, 37 to 40) doesn't make sense. Otherwise, its very good.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer 1:

1. The result and conclusion in the abstract needs further revision. Your objective is to examine individual, parental and life-course SES differences in breast and cervical cancer screening. So better to add important results of all these three items, here you missed parental SES differences in screening.

Word limit is an issue while writing abstract, but the statement should have a complete sense when limiting the words. For example, “stable life-course SES” increased breast cancer screening by 3 fold. Instead you can clearly mention “stable life–course SES based on maternal employment” increased breast cancer screening by 3 fold. Also the statement should end up with “compared to stable low life-course SES”. Otherwise it is incomplete. Most often people read the abstract first and then only read the full text, so it should be clear and concise. Similarly the last statement of the result section (please add the reference group).

The first part of conclusion seems good, but the second statement (addressing social...) is very general. I wonder whether this statement weakens the message of this study.

Response: We have revised the abstract to provide a clearer picture of the study design, results and conclusions, and included future strategies such as integrating screening programs for breast and cervical cancer into routine healthcare systems in low- and middle-income countries.

2. In the discussion part, you can discuss few more things, for eg:
You can discuss on availability of organized screening programmes in the health systems of all countries included in the study. For instance, almost all states in India have no organized screening programme, currently only 2 out of 29 states in India incorporated cervical and breast cancer

screening in their health systems. That is the reason for observing a very low screening rate irrespective of SES in India. If there is no facility then SES has no role in determining the screening rate because women in all strata lack the facility. Hence the association between SES and screening participation is country specific.

Another point that can be discussed is the mechanism of the association between parental employment status and daughter's screening status. Mother's employment in public sector may be connected with higher educational attainment of daughters. From your data itself you can see the interplay between these two variables if you just cross tabulate them.

Response: We have discussed the significant need for initial investment for establishment of effective screening programs in LMIC. Also, we have specified the need for future research and interventions incorporating country-level differences in availability of screening programs in LMICs.

Reviewer: 2

A few minor points: Page 11, the second paragraph (lines 41 to 54) is written rather unclearly obscuring the points being made; page 15, line 43, should be, ' college degree OR higher' not of; page 17, line 34, Schmeisser et al, needs a date and the last sentence of this paragraph (lines, 37 to 40) doesn't make sense.

Otherwise, its very good.

Response: We thank the reviewer for their comments. Suggested changes have been made in the text.

VERSION 3 – REVIEW

REVIEWER	Jissa Vinoda Thulaseedharan Achutha Menon Centre for Health Science Studies (AMCHSS), Sree Chitra Tirunal Institute for Medical Sciences and Technology (SCTIMST), Medical College PO, pin-695011, Trivandrum, Kerala, India
REVIEW RETURNED	12-Oct-2016
GENERAL COMMENTS	Now the paper seems well written. Objectives, methods and results would be very clear to the readers, and hope it will give some scientific contribution to the literature. Best wishes,