PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	SDG3-related inequalities in women's, children's, and adolescents' health: an SDG-monitoring baseline for Latin America and the Caribbean using national cross-sectional surveys
AUTHORS	Sanhueza, Antonio; Carvajal-Vélez, Liliana; Mújica, Oscar; Vidaletti, Luis; Victora, C; Barros, Aluisio

VERSION 1 – REVIEW

REVIEWER	Chandrashekha Sreeramareddy Manipal College of Medical Sciences, Community Medicine
REVIEW RETURNED	30-Jan-2021

GENERAL COMMENTS	The authors of this manuscript have presented succinctly a wealth of data on gamut of indicators covering SDG3. The paper is very timely and relevant for various stake holders for monitoring the progress towards the improving the coverage. The inequalities presented were by ordered variable (wealth & education), and other variables including spatial variables. Though all these are important to highlight those are "left behind" which is basic tenet of SDGs, the methods and data sources used are very not well explained. however, the interpretations that are to be done using the SII and CI are well explained, Importantly more details maybe needed on the following. 1) Recommended that SII and relative index of inequalities are usually reported for wealth and educational inequalites in most literature. However, authors choose SII and CI and CI estimations is not explained in methods. 2) Wealth index in DHS & MICS are those of households but not necessary that of women interviewed. Given that education is rather more directly linked to women's autonomy and empowerment much of the indicators presented here would vary much more by women's education. There is plethora of research on WOMEN's education and health status. Why "leave behind" women's education by presenting just the household wealth instead. There is a mention about education under methods but much of results (table 1 where only 8 not 9 indictors are shown, but not indicated if SII is wealth or educaton). The figure 2 was for wealth inequalities Further concern is that CI is not presented in either table or figure. 3), Spatial variables national vs. sub national are presented but there are not any recommended inequality measures reported e.g. gini coefficient. 4) Limitations were related to the design of surveys and sample sizes. Would there reporting bias as well. This is not well discussed. 5) though it seems like authors used microdata to estimate the

	inequalities, it is unclear as so which indicators were computed from micro-data and which were extracted from aggregate data from statcompiler, childinfo.org 6) table 1 does not show which source of data was used alongside the year of survey. The study highlights overstates, up-to-date estimates but the limitations states that some country data was early 2010 decade, contradicting own statements since 11 of the countries had data from 2011-13. Further, authors also speculate that the change may not be of a larger magnitude. there is not much information presented on figure 3 Though it is well know DHS & MICS are comparable, authors need to add a statement on this under methods. overall, even this paper presents a wealth of information, i request authors to address, the above concerns on choice of marker of inquality, measure of inequality of wealth and education, measures of inequality for spatial variables, comprehensively address the limitations including the reporting biases.

REVIEWER	Ana Ortigoza Drexel University School of Public Health, Urban Health Collaborative
REVIEW RETURNED	09-Feb-2021

GENERAL COMMENTS	Thank you for allowing me the revision of this manuscript. The focus of this work is certainly relevant and very much needed to assess the state of countries in relation to women, maternal, child and adolescent health and to direct health strategies in the post-pandemic scenario. The paper is well designed, executed, and written. Results are clearly described with tables and figures describing meaningful information. I only have some suggestions to improve the interpretation and discussion of these findings. 1) as Latin America is a highly urbanized region, I think it would be interesting to have more information about the level of urbanization (i.e. proportion of population living in urban areas) in each country to better understand and interpret the pro-urban inequalities mentioned in the results. 2) it could be of valuable information if author could provide additional interpretation regarding potential underlying mechanisms related to the distribution of coverage by wealth seen for some health indicators. For example, even when pro-rich inequalities are narrowing with increasing coverage of demand of family planning satisfied by modern technology, in most of the countries coverage is not greater than 90%, while coverage of indicators related to perinatal care (skilled attendance, antenatal and post-natal care) reach almost 100% for at least some groups. Are barriers related to reproductive justice involved in coverage of family planning beyond the socioeconomic inequalities described?

REVIEWER	Francois Cohen
REVIEW RETURNED	13-Feb-2021

GENERAL COMMENTS	The report is clear and results are not surprising but still interesting

	to track progress on SDG 3. I have said no to the questions "4. Are the methods described sufficiently to allow the study to be repeated?" and "7. If statistics are used are they appropriate and described fully?". I believe it is important that you provide more detail on the two measures of inequality that you use in appendix (the slope index of inequality (SII) for absolute inequality and the concentration index (CIX) for relative inequality). I also think that we would need a page describing the weights used and reporting of standard errors. A limitation of the article is that it only provides a snapshot of the state of inequalities at a given point in time. I feel the authors could discuss a bit more planned surveys in each country, to show how their methods could be used in the future to assess progress on SDG 3. I also wonder if the latest surveys were always used (we are in 2021 and the median is for 2014). If there are several surveys, we would ideally want to know more about the progress on the indicators evaluated, but I leave to the appreciation of the author and editor if this is a required revision for publication. I also wonder if some of the results of Table 2 and 3 could be presented a bit differently. Authors only provide point estimates for clarity, but it is then unclear to the reader how accurate these estimates are at a glance. In contrast, the authors provide too much information for anyone to process, especially from page 31 onwards. They should select indicators or arrange them in a better way.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1
Dr. Chandrashekhar Sreeramareddy, Manipal College of Medical Sciences

Comments to the Author:

The authors of this manuscript have presented succinctly a wealth of data on gamut of indicators covering SDG3. The paper is very timely and relevant for various stake holders for monitoring the progress towards the improving the coverage. The inequalities presented were by ordered variable (wealth & education), and other variables including spatial variables. Though all these are important to highlight those are "left behind" which is basic tenet of SDGs, the methods and data sources used are very not well explained. however, the interpretations that are to be done using the SII and CI are well explained, Importantly more details maybe needed on the following.

Thank you very much for your kind comments.

1) Recommended that SII and relative index of inequalities are usually reported for wealth and educational inequalities in most literature. However, authors choose SII and CI and CI estimations is not explained in methods.

Thank you for comments here. The concentration index (CIX) is also recommended as a complex and robust measure of relative inequality – and it is equally ubiquitous in the literature. The relative index of inequality (RII), on the other hand, has two versions (i.e., Pamuk's and Kunst-Mackenbach's) which yield different results, and introduces ambiguity in the literature. Therefore, we chose the CIX over the RII as a measure of relative inequality in our study. We added a short explanation of the CIX estimation along with a reference for the interested reader.

2) Wealth index in DHS & MICS are those of households but not necessary that of women interviewed. Given that education is rather more directly linked to women's autonomy and empowerment much of the indicators presented here would vary much more by women's education. There is plethora of research on WOMEN's education and health status. Why "leave behind" women's education by presenting just the household wealth instead. There is a mention about education under methods but much of results (table 1 where only 8 not 9 indictors are shown, but not indicated if SII is wealth or education). The figure 2 was for wealth inequalities.

Thank you for bringing up a very good point, with which we agree on. All our stratified estimates, including by woman's education, are presented in the Supplementary Table S3. But per your suggestion, we added a set of equiplots by woman's education (new Figure 2). We also added another table to the supplementary materials (Table S3) with the absolute and relative gaps for all the indicators by woman's education. Regarding the SII and CIX, as explained in methods, they were only calculated for wealth.

Further concern is that CI is not presented in either table or figure.
Given space restrictions and ease of interpretation, we presented the full set of SII and CIX with their 95% confidence intervals in Supplementary Table S2.

3) Spatial variables national vs. sub national are presented but there are not any recommended inequality measures reported e.g. gini coefficient.
All estimates for subnational regions are presented in the supplementary materials. Given the large number of countries and estimates, we did not focus on subnational inequalities given that in a multicountry analysis they are hard to compare given they are country specific. We did highlight some of these inequalities in the results section. We did not compute the Gini coefficient both because we were not interested in income inequality and because we did not have any income data.

4) Limitations were related to the design of surveys and sample sizes. Would there be reporting bias as well? This is not well discussed.
In the discussion section, we did mention some limitation related to response biases: i) information on indicators such as antenatal or delivery care were asked for births that took place during the five years prior to the survey for DHS or two years for MICS prior to the survey, thus contributing to the time lag since these interventions took place; ii) a related issue refers to the precision of maternal recall about events that took place several years before the interview; this may affect variables such as antenatal, delivery and postnatal care.

5) though it seems like authors used microdata to estimate the inequalities, it is unclear as so which indicators were computed from micro-data and which were extracted from aggregate data from statcompiler, childinfo.org
All our results were obtained from reanalysis of original survey microdata. A phrase was added in methods to clarify this issue.

6) table 1 does not show which source of data was used alongside the year of survey.
We added a new column to Table 1 where the source data is shown.

The study highlights overstates, up-to-date estimates but the limitations states that some country data was early 2010 decade, contradicting own statements since 11 of the countries had data from 2011-13. Further, authors also speculate that the change may not be of a larger magnitude.
The study is as up to date as possible, in doing stratified analysis that are based on nationally representative surveys. In the discussion, we mentioned as a limitation that surveys in the countries were carried out from 2011 onwards, with a median date of 2014, and so this must be considered when interpreting the results presented in this study. We added a phrase to the discussion highlighting that this is the best that can be done with surveys.

There is not much information presented on figure 3
There is no figure 3 in the manuscript, but we assume you refer to figure 2. Now, we believe figure 2 shows very interesting results, where we can see that countries with the largest gaps between the poorest and the richest (based on the SII) are the ones with the lowest coverage at the national level.

This has important implications for policy and reinforces the idea that the rich fend for themselves even when the country level of coverage is low.

Though it is well know DHS & MICS are comparable, authors need to add a statement on this under methods.
In the method section there is already phrase supported with two references saying “Both survey families are highly comparable, including sampling approaches and questionnaires”.

Overall, even this paper presents a wealth of information, i request authors to address, the above concerns on choice of marker of inequality, measure of inequality of wealth and education, measures of inequality for spatial variables, comprehensively address the limitations including the reporting biases.
Thank you very much for all the comments and suggestions, which have improved our manuscript. In the revised version of the manuscript, we did take into consideration all your comments and suggestions, hopefully to your satisfaction.

Reviewer 2
Dr. Ana Ortigoza, Drexel University School of Public Health

Comments to the Author:
Thank you for allowing me the revision of this manuscript. The focus of this work is certainly relevant and very much needed to assess the state of countries in relation to women, maternal, child and adolescent health and to direct health strategies in the post-pandemic scenario.
The paper is well designed, executed, and written. Results are clearly described with tables and figures describing meaningful information. I only have some suggestions to improve the interpretation and discussion of these findings.
Thank you very much for your kind words.

1) as Latin America is a highly urbanized region, I think it would be interesting to have more information about the level of urbanization (i.e. proportion of population living in urban areas) in each country to better understand and interpret the pro-urban inequalities mentioned in the results.
As the data we are using come from surveys (microdata), we may be able to estimate the proportion of the population that lives in urban areas by considering the sample size for urban area divided by the total sample size in a country. We added figures to the supplementary materials with equiplots, including urban/rural comparison, along with the % of urbanization in each country.

2) it could be of valuable information if author could provide additional interpretation regarding potential underlying mechanisms related to the distribution of coverage by wealth seen for some health indicators. For example, even when pro-rich inequalities are narrowing with increasing coverage of demand of family planning satisfied by modern technology, in most of the countries coverage is not greater than 90%, while coverage of indicators related to perinatal care (skilled attendance, antenatal and post-natal care) reach almost 100% for at least some groups. Are barriers related to reproductive justice involved in coverage of family planning beyond the socioeconomic inequalities described?
Thank you very much for your important comments mentioned here. Based on the results obtained in this work, we described the inequalities that exist in the coverage indicators by the different stratifiers. The mechanisms, however, will not be clarified by this type of study and data sources. We believe that a deeper dive into such mechanisms is warranted and this is one of the interesting effects of our paper. We added to the discussion a comment on the need for such type of exploration.

Reviewer 3
Dr. Francois Cohen
Comments to the Author:
The report is clear and results are not surprising but still interesting to track progress on SDG 3.
Thank you for your comment. One of our purposes is exactly provide a baseline for SDG monitoring in LAC.

I have said no to the questions "4. Are the methods described sufficiently to allow the study to be repeated?" and "7. If statistics are used are they appropriate and described fully?".

1) I believe it is important that you provide more detail on the two measures of inequality that you use in appendix (the slope index of inequality (SII) for absolute inequality and the concentration index (CIX) for relative inequality). I also think that we would need a page describing the weights used and reporting of standard errors.
We have added further information on the estimation of the CIX to the methods section. Both the SII and the CIX are pretty standard measures of inequality and are described in detail in other freely available publications. We, therefore, do not believe it is necessary to duplicate such descriptions in our manuscript. We also added more information on the sampling design if the surveys used and a reference to the DHS sampling manual where all the details on survey design can be obtained. Again, this sampling strategy is fairly standard, and the word count allowed for the manuscript would not allow us to add even more details.

2) A limitation of the article is that it only provides a snapshot of the state of inequalities at a given point in time. I feel the authors could discuss a bit more planned surveys in each country, to show how their methods could be used in the future to assess progress on SDG 3. I also wonder if the latest surveys were always used (we are in 2021 and the median is for 2014). If there are several surveys, we would ideally want to know more about the progress on the indicators evaluated, but I leave to the appreciation of the author and editor if this is a required revision for publication.
In this work, we aimed to describe the status of socioeconomic inequalities on selected SDG3 indicators as a baseline for monitoring the 2030 Agenda. As mentioned in the discussion section, with a median year of 2014 (the baseline year for SDG3 indicator is around 2014 and 2015) for the surveys included in the analyses, our findings provide a baseline for monitoring progress during the SDG period (2015-2030) as new surveys become available. Our results will also help policymakers prioritize which interventions and population subgroups require special attention in their countries, in order to achieve the SDG vision of leaving no one behind. Our findings will also be important for monitoring the impact of the COVID-19 pandemic on levels and inequalities in health status and intervention coverage among women and children.

3) I also wonder if some of the results of Table 2 and 3 could be presented a bit differently. Authors only provide point estimates for clarity, but it is then unclear to the reader how accurate these estimates are at a glance. In contrast, the authors provide too much information for anyone to process, especially from page 31 onwards. They should select indicators or arrange them in a better way.
In the supplementary tables (Supplementary Table S2) we presented the SII and CIX for all the indicators with their 95% confidence intervals so that readers can assess the accuracy of the estimates. We also included in the supplementary materials sets of equiplots that will allow the interested reader to appreciate in more details the inequalities in each dimension studied. Finally, we reorganized the presentation of individual indicator results in a way that it is easier to deal with. Unfortunately, the journal will not accept additional files in a format different from PDF. We will make a more flexible Excel file available for download, adding the URL to the supplementary file.

VERSION 2 – REVIEW

REVIEWER	Chandrashekhar Sreeramareddy Manipal College of Medical Sciences, Community Medicine
REVIEW RETURNED	02-Jun-2021

GENERAL COMMENTS	The revised manuscript addressed an important stratifier i.e. education and authors presented some results using education as a stratifier. Other concerns about methods, inequality markers and limitations were satisfactorily addressed by authors in the revised manuscript.

REVIEWER	Ana Ortigoza Drexel University School of Public Health, Urban Health Collaborative
REVIEW RETURNED	24-Jun-2021

GENERAL COMMENTS	Responses were properly addressed and now the updated version describe results in a meaningful way with a thoughtful discussion of the findings and limitations. I suggest this manuscript is ready for publication