

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Towards Defining the Surgical Workforce for Children: A Geospatial Analysis in Brazil
AUTHORS	Rocha, Thiago; Vissoci, Joao; Rocha, Nubia; Poenaru, Dan; Shrime, Mark; Smith, Emily; Rice, Henry

VERSION 1 - REVIEW

REVIEWER	Dohyeong Kim University of Texas at Dallas, United States
REVIEW RETURNED	05-Oct-2019

GENERAL COMMENTS	<p>This manuscript aims to examine the surgical workforce for children across Brazil and test association between the surgical workforce and measures of childhood health. The following factors should be considered and refined for possible publication:</p> <ol style="list-style-type: none">(1) Using “global” in the title is misleading. This paper analyzed the data only from Brazil.(2) Lines 37-39, page 6: The statement is wrong – Getis-Ord Gi analysis is used for finding “spatial autocorrelation” within each indicator, not association between variables.(3) Line 51, page 6: For bivariate scatterplots, did you use the raw data or other processed data? If you draw a simple scatterplot for the surgical workforce density and U5MR at the municipality level, it does NOT show geographic relationships or spatial autocorrelation. Although the hotspot maps (Figure 5) show very high spatial autocorrelation, there is no discussion or consideration on spatial autocorrelation. The authors should assess the level of spatial autocorrelation among all the indicators and adjust them in any subsequent statistical analysis. Please see the difference between Pearson’s R and Lee’s L found in Kim et al (2018), “A closer look at the bivariate association between ambient air pollution and allergic diseases: the role of spatial analysis,” International Journal of Environmental Research and Public Health, 15(8):1625.(4) The detailed results for the quadratic regression models used need to be presented for clarification, including covariates and their significance, model fit, spatial factors, etc. It is unclear how the maps in Figure 5 and scatterplots in Figure 6 were created without such details.(5) The five regions are not clearly demonstrated in all maps. Both region and state boundaries should be presented because many of data analyses were done at the regional level.(6) Some municipalities may have zero (close to zero) surgeons probably because of its geographic characteristics (eg. Amazon
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	forests, etc.). It is difficult to understand the message from the maps without basic understanding on local geography in Brazil. (7) The Result sections seem so sketchy. Figure 5 is the only figure showing the outcome of a geographic analysis, but its interpretation is very sketchy and somewhat incorrect. Again, Gi map does not show a direction association between the surgical workforce and U5MR.
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REVIEWER	Christopher Hughes Connecticut Children's USA
REVIEW RETURNED	23-Oct-2019

GENERAL COMMENTS	<p>This study is a cross-sectional analysis of the surgical workforce for children in the Brazilian public health system. The authors use publicly available databases and geospatial analyses to examine the association between workforce and under-5 mortality in children. It is well written, thoughtful, and interesting, and it serves as a thought-provoking examination of the need for a certain level of provider density in health systems development. I have a few comments that I believe should be addressed in a revision prior to publication:</p> <ol style="list-style-type: none"> 1. The authors accessed multiple databases to identify children who received surgical care. Were there any safeguards to ensure non-redundant counting? Were the cases necessarily mutually exclusive? Did the authors account for this in their analysis? 2. According to the authors, POMR is more closely related to surgical procedures and U5MR seems to be a broader characterization of all mortality, not necessarily related to surgically treatable diseases. If this is true, how do the authors account for those deaths that may not necessarily be related to surgically treatable conditions? The outcome measures may therefore not be appropriate to the association that is being tested. 3. Along those lines, the authors accounted for the surgical workforce in each region. Did they account for the non-surgical workforce and factor that into their analyses as well? If, for example, there were more child deaths in poorer regions due to malnutrition or infectious processes secondary to poor sanitation, then those deaths could have been perhaps best treated by a non-surgical provider (among other things). Does surgical workforce correlate with overall provider density? If not, how can we be sure that the association between surgical workforce density and mortality is a real one?
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VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Dohyeong Kim

Institution and Country: University of Texas at Dallas, United States

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

This manuscript aims to examine the surgical workforce for children across Brazil and test association between the surgical workforce and measures of childhood health. The following factors should be considered and refined for possible publication:

(1) Using “global” in the title is misleading. This paper analyzed the data only from Brazil.

We appreciate the comment and have changed the title accordingly. Please note that the title of the revised manuscript is slightly different from the original manuscript, and now reads: “Towards Defining the Surgical Workforce for Children: A Geospatial Analysis in Brazil”

(2) Lines 37-39, page 6: The statement is wrong – Getis-Ord Gi analysis is used for finding “spatial autocorrelation” within each indicator, not association between variables.

We appreciate the reviewer’s careful read of these methods, and we agree that our methods are somewhat misleading as written. Therefore we have rewritten them as seen on page 6 of the revised text: “To further identify potential associations between the surgical workforce and U5MR, we performed Getis-Ord Gi analysis.¹⁷ This measure of spatial heterogeneity is used to identify spatial autocorrelation within each indicator.”

(3) Line 51, page 6: For bivariate scatterplots, did you use the raw data or other processed data? If you draw a simple scatterplot for the surgical workforce density and U5MR at the municipality level, it does NOT show geographic relationships or spatial autocorrelation. Although the hotspot maps (Figure 5) show very high spatial autocorrelation, there is no discussion or consideration on spatial autocorrelation. The authors should assess the level of spatial autocorrelation among all the indicators and adjust them in any subsequent statistical analysis. Please see the difference between Pearson’s R and Lee’s L found in Kim et al (2018), “A closer look at the bivariate association between ambient air pollution and allergic diseases: the role of spatial analysis,” *International Journal of Environmental Research and Public Health*, 15(8):1625.

We appreciate the comment, and have markedly revised this section of the analysis. We added a new bivariate correlation plot and a choropleth map with the High and Low clustering as suggested. We clarified in the figure legends that the scatterplots in Figure 5 are not adjusted for spatial autocorrelation. The intent was to identify the optimal volume of SAO in relation to POMR, using state level data, not municipality data. These steps are an alternate approach to data analysis, in addition to the geographical relationship analysis.

(4) The detailed results for the quadratic regression models used need to be presented for clarification, including covariates and their significance, model fit, spatial factors, etc. It is unclear how the maps in Figure 5 and scatterplots in Figure 6 were created without such details.

We appreciate this suggestion and agree that further detail of the regression models is required. We added an additional table with the regression details (Table 2). Figure 6 (now Fig 2) is the bivariable scatterplots, Figure 5 was not developed out of that analysis.

(5) The five regions are not clearly demonstrated in all maps. Both region and state boundaries should be presented because many of data analyses were done at the regional level.

We agree with this suggestion and have added the state and region boundaries to all figures.

(6) Some municipalities may have zero (close to zero) surgeons probably because of its geographic characteristics (eg. Amazon forests, etc.). It is difficult to understand the message from the maps without basic understanding on local geography in Brazil.

We appreciate this comment and concur that much of the distribution of workforce is related to geography. However, our analysis is focused on differenced in workforce density per population unit, and therefore geographic variables (such as remote regions of Amazonia, etc.) are corrected for the underlying population density. We emphasize this important distinction in the methods as well as the discussion on page 8.

(7) The Result sections seem so sketchy. Figure 5 is the only figure showing the outcome of a geographic analysis, but its interpretation is very sketchy and somewhat incorrect. Again, Gi map does not show a direction association between the surgical workforce and U5MR.

We understand this reviewer's concerns. We have completely restructured and edited the results section extensively to describe the exact steps of our data analysis and to reduce any confusion.

Reviewer: 2

Reviewer Name: Christopher Hughes

Institution and Country:

Connecticut Children's USA

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

This study is a cross-sectional analysis of the surgical workforce for children in the Brazilian public health system. The authors use publicly available databases and geospatial analyses to examine the association between workforce and under-5 mortality in children. It is well written, thoughtful, and interesting, and it serves as a thought-provoking examination of the need for a certain level of provider density in health systems development. I have a few comments that I believe should be addressed in a revision prior to publication:

1. The authors accessed multiple databases to identify children who received surgical care. Were there any safeguards to ensure non-redundant counting? Were the cases necessarily mutually exclusive? Did the authors account for this in their analysis?

All procedure data came from only one database, the SIH. Mortality data came from the SIM. Given the ecological nature of the study, these databases are not linked and therefore not mutually exclusive. The entries on SIH are marked by procedure, not individual. So the analysis need to be understood on the procedural level. We added this challenge as a limitations of this study in the discussion.

2. According to the authors, POMR is more closely related to surgical procedures and U5MR seems to be a broader characterization of all mortality, not necessarily related to surgically treatable diseases. If this is true, how do the authors account for those deaths that may not necessarily be related to surgically treatable conditions? The outcome measures may therefore not be appropriate to the association that is being tested.

This reviewer raises a good point which is actually the main thrust of our analysis. We certainly recognize that many confounding and modifying variables other than the surgical workforce impact the U5MR. However, by focusing only on surgical outcomes (such as perioperative mortality rate), we risk negating the impact which surgical care has on general childhood health (as summarized with U5MR). As summarized in a detailed argument in the discussion, we view surgical care as a core component of a highly functional health system. As discussed in the paper, we argue that it is not important to dissect how much of U5MR is related to surgical workforce or other core components, rather that a surgical workforce, along with other health system components of a functional health system, are associated with optimal U5MRs. Although it would be of great interest to quantify which

degree of which element of the healthcare workforce or health care disease burden contributes in the most important ways to U5MR, this is well beyond the capacity of the current report.

Similarly, as our report is an ecological study, we cannot link mortality to specific operative procedures. Our analysis is conducted at a geographic level, with health care deliver and general health outcomes used as a proxy measure of health system performance. There are no outcomes linked to the procedure database that could be used for this analysis, as high-quality POMRs are not available. U5MR is the best outcome we have to model a health system based metric to understand the population health impact of workforce and its shortage.

3. Along those lines, the authors accounted for the surgical workforce in each region. Did they account for the non-surgical workforce and factor that into their analyses as well? If, for example, there were more child deaths in poorer regions due to malnutrition or infectious processes secondary to poor sanitation, then those deaths could have been perhaps best treated by a non-surgical provider (among other things). Does surgical workforce correlate with overall provider density? If not, how can we be sure that the association between surgical workforce density and mortality is a real one?

We appreciate the observation of this reviewer that other aspects of a health workforce, other than surgical providers, may contribute to U5MR. Again as summarized above, to dissect which health system components are most contributory to U5MR, relationship between surgical workforce density, etc. is a difficult analysis and beyond the capacity of this current report. However, this certainly merits further investigation and we have added appropriate comments in the discussion on page 10.

VERSION 2 – REVIEW

REVIEWER	Dohyeong Kim UT Dallas
REVIEW RETURNED	15-Feb-2020

GENERAL COMMENTS	The authors have addressed all my comments and I believe the revised manuscript is much improved.
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REVIEWER	Christopher Hughes Connecticut Children's, USA
REVIEW RETURNED	23-Dec-2019

GENERAL COMMENTS	The authors have adequately addressed my prior comments and have amended the text where necessary. It remains an interesting paper that raises many questions. I congratulate the authors on their work.
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