Author’s response to reviews

Title: Feminisation of the health workforce and wage conditions of health professions: an exploratory analysis

Authors:

Geordan Shannon (geordan.shannon.13@ucl.ac.uk)
Nicole Minckas (nikiminckas@gmail.com)
Des Tan (dwc.tan@gmail.com)
Hassan Haghparast-Bidgoli (h.haghparast-bidgoli@ucl.ac.uk)
Neha Batura (n.batura@ucl.ac.uk)
Jenvieve Mannell (j.mannell@ucl.ac.uk)

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Author’s response to reviews:

University College London
Institute of Global Health
3rd floor, Institute of Child Health
30 Guilford Street
London WC1N 1EH

James Buchan, Mario Dal Poz
Human Resources for Health Journal
BioMed Central
The Campus, 4 Crinan Street
London N1 9XW

Subject: Submission of revised manuscript
Dear Editors,

We would like to submit the second round of major revisions of the paper entitled “Feminisation of the health workforce and wage conditions of health professions: an exploratory analysis,” by Geordan Shannon, Des Tan, Nicole Minckas, Hassan Haghparast-Bidgoli, Neha Batura, and Jenevieve Mannell, to Human Resources for Health Journal.

We thank you for the detailed comments on the revised manuscript. We note that there was substantial differences in the nature of reviewer one and two’s comments, and that a third reviewer has since been recruited to provide input on this document. As such, we have attempted to address all reviewer concerns and/or highlight areas of disagreement or discrepancies.

We also note the long time-lag in receiving the second round of reviews. We have done our very best to provide comprehensive, considered responses and a high quality paper in response to these reviews. Because we feel this is a topic of utmost importance to the current dialogue, we kindly request that you process the current revisions in a timely manner.

Please find our responses to the reviewer comments below.

All authors confirm to have read and approved this paper. We are willing to assign copyright to the publisher, if the article is accepted. The submitted manuscript has not been previously published, orally presented, or is under consideration elsewhere. Please direct all correspondence to Dr. Geordan Shannon.

Yours truly (On behalf of the authors),

Dr Geordan Shannon
Institute of Global Health
3rd floor, Institute of Child Health
Reviewer reports:

Reviewer #1: The authors have made a number of edits to more appropriately nuance their paper as "exploratory" rather than an "impact" study. I do not believe they have adequately addressed the underlying limitations of their rationale and study design as raised in the previous reviews.

The first sentence "The global health workforce is feminizing" remains unreconciled with the contents of the paragraph and the study. The authors have included a few examples specifically within the physician workforce, in one instance from a country that is not even included in their analysis (Canada), in another citing a source (reference 2, Gupta et al.) that indicated the physician workforce was already predominantly female in the 1990s from a country included in their analysis (Russian Federation). There is no demonstration that the health workforce as a whole is feminizing over time or from a global perspective.

We have revised the introductory paragraph (see below). So as to remain focused, and given Reviewer 3’s comments on reducing the overall word limit, we felt it was more appropriate to provide a summary with supporting evidence, rather than demonstrating at length specific examples.

The feminisation of the health workforce - the movement of women into occupations where they were formally under-represented1 - is a phenomenon that has been extensively documented in global health research. In medicine, women have moved from exclusion from the profession to the majority of medical graduates in many countries around the world. Feminisation of the medical profession has been recorded in countries as diverse as Bangladesh, Canada, Cape Verde, Guinea Bissau, Israel, Mozambique, Oman, the UK and the US. In dentistry, the proportion of women is projected to increase to 28% globally by 2030. Women now comprise
approximately 75% of the global health workforce, and over 90% of nursing and midwifery professions. Despite the shifting gender balance of the health workforce, women still tend to belong to lower cadres of health workers, are under-represented in positions of leadership, are over-represented in unskilled and unpaid work and earn less than men.

The theoretical/conceptual background remains fragmented, with confusing interchange between the constructs of "sex" (biological) and "gender" (socio-cultural). For example, the authors describe their purpose as using "gender disaggregated data" on page 5 and include "gender" among the list of variables on page 6, although conceptually these are clearly referring to "sex" as a demographic variable. They introduce "transgender" as a critical factor on page 3, but then remain silent on this for the rest of the paper.

As per Reviewer 3’s suggestion, we have revised our terminology in order to remain consistent throughout. Our analysis is predominantly based on gender as a socio-cultural construct based in power relations, as opposed to sex. We defined gender in our last revision as the “socially constructed norms that impose and determine roles, relationships and positional power for all people across their lifetime.” We also note that gender and sex do interact: “…gender interacts with sex, the biological and physical characteristics that define women, men and those with intersex identities.”

We recognise gender diversity as a critical factor both in gender analysis on the whole and in gender analysis for the health workforce. Unfortunately there were no data on transgender people in the WageIndicator survey. This is consistent with the paucity of information available on gender diverse people in global health and the global health workforce. We note this as a limitation in the discussion.

The authors still do not make a convincing case that their approach has been grounded in consideration of horizontal versus vertical wage gaps. They have purposely grouped into their classification of "traditionally male-dominated" exclusively higher skilled occupations (at the ISCO-08 1000, 2000 and 3000 levels), whereas the "traditionally female-dominated" classification includes many lower skill occupations (5000 level). This will skew the results by design. Moreover there is little support presented through either theoretical or empirical evidence from a global health workforce perspective of the sex-disaggregated or gendered nature of many
of these occupations (e.g., physiotherapists, medical records technicians or ambulance workers as "traditionally female dominated").

In response to Reviewer 2’s previous comments on horizontal and vertical wage gaps, we have grouped our analysis into technical, professional roles (traditionally dominated by men) and allied/semi-professional roles (traditionally dominated by women). In an ideal world, we would have liked to disaggregate further to explore horizontal divisions within occupational groups. Sadly the sample did not allow for this level of analysis over time (noting that exploring time trends was the priority in this paper).

The very point of this process, as you have rightly pointed out, is to capture the gendered nature of certain occupations. The gender gap, and the way in which occupations are valued, is shaped by broader gender stereotypes about men’s and women’s roles and value in society. Therefore, we do expect that women are over-represented in unskilled or lower paid work. This has been extensively documented in the health workforce literature, and we provide relevant references in our manuscript. Also in our manuscript, we present information on the gender ratios so that the reader can see that certain professions are comprised differently from a gender perspective. We are also able to look at gender differences in wages (presenting the results of our analysis in ratio form) to focus on wage inequality between men and women, regardless of the gender composition of the occupational grouping.

To clarify our approach, we have introduced a loose theoretical framework (Figure 1) which is based on our review of literature on the gendered nature of the health workforce. We establish some general principles of the gender division of labour in the health workforce and loosely define two categories of the health workforce (technical/professional and allied/semi-professional) based on extensive historical and sociological literature (references listed in the manuscript).

The authors indicate that they largely based their analysis on previous work that took into account "gender, age and level of education" but fail to make a convincing argument that their sample size of 48,282 observations is insufficient to control for age (even using age-adjusted rates) or education (which could also readily be considered by proxy in terms of the ISCO occupational group level) An analysis of simply proportions of females in certain occupations risks highly skewed and potentially misleading results by design, since females entering the
workforce would be at a lower wage rate compared to those with more years of labour market experience.

As we had already mentioned in our previous response to reviewer comments, we agree that education levels are most likely subsumed in the profession (in that professional achievements do reflect necessary pre-requisite education). We have clarified this in the manuscript. In relation to your query on age, may we refer you to the supplementary demographic material, which provides an overview of the age composition of the general workforce in our sample, and of the decomposition of the health workforce by country and year.

We draw your attention to the fact that our primary purpose was presenting trend information over nine years (between 2006 and 2014) and over multiple countries. Taking this into consideration, the numbers in some of our country-year cells were too low to disaggregate further into age groups. We looked at the possibility of presenting age-adjusted rates, but found that the results were misleading. We have listed this as a limitation in our discussion.

Reviewer #2: Thank you for the opportunity to review this revised manuscript. The authors did a fine job addressing the comments for both reviewers and the manuscript has improved considerably, especially bringing more clarity to the purpose and approach of the study. I only have two minor suggestions for the abstract.

1) L 56: consider replacing "gender wage inequalities" with gender wage differences. This is perhaps an important distinction given that your data does not have adequate controls to examine potential inequalities in a more robust fashion with multivariate regressions or decomposition techniques.

Thank you, we have amended accordingly.

2) L 68: add the word "women" for the comparison with men. i.e. this was associated with a wage gap for women of X compared to men.

We have also amended this accordingly.
Reviewer #3: Thank you for the opportunity to review this article. While exploratory in nature, it offers insights into an important area of workforce development. I have some methodological concerns that I would suggest be addressed prior to accepting this article for publication.

* Sex/gender: I appreciated the discussion around the distinction between sex and gender added to the introduction in response to comments made by reviewer 1; however, the nuance of this distinction isn't equally reflected in the methodology. Importantly, the authors are unclear about how gender is assessed in the survey. On line 222, the authors note that WageIndicator data is the only resource that contains sex-disaggregated data, and yet subsequently their analyses refer to gender ratios, and the gender wage gap. There are also several places where "male" and "female" should be replaced by "men" or "women". For example, in the results, line 235 and 326, the statement should read "corresponding to 64.4% participation by men" instead of "corresponding to 64.4% male participation". The authors should review the manuscript and change this throughout. Also the specific wording of the WageIndicator question is important to determine whether participants assume the survey is asking about their gender identity or biological sex. The wording of the question needs to be included explicitly in the manuscript, and the language used in the methodology and results section needs to follow that wording.

As we clarified above, our analysis is predominantly based on gender as a socio-cultural construct based in power relations, as opposed to sex. We have revised our document and ensured consistency of the term gender (and man/woman/men/women) throughout.

We can confirm that the WageIndicator surveys ask participants about gender with the question: “are you a woman or a man?” This has been clarified in the manuscript.

* A related limitation is the implicit assumption that the trends observed are related to the concept of gender, rather than the interaction between biology (sex) and gender. Women's career choices, trajectories, and wages are affected not only by societal norms but also biological considerations such as menstruation, child bearing, breast feeding etc. The effect of these biological considerations on a woman's career choices/options are likely also variable by country. A discussion of sex effects is warranted and the inability to disentangle sex and gender effects should be cited as a limitation of this analysis, linked again to the specific metric used to ascertain sex/gender in WageIndicator.
We defined gender in our last revision as the “socially constructed norms that impose and determine roles, relationships and positional power for all people across their lifetime.” We also note that gender and sex do interact: “…gender interacts with sex, the biological and physical characteristics that define women, men and those with intersex identities.” Whilst some biological considerations do shape women’s and men’s careers, we align our work with the prevailing view of current gender and health system scholars, including those on the Women and Gender Equity Knowledge Network and those published in the Human Resources for Health Journal - that it is the gendered nature of the health workforce that treats biological functions unfairly. For example, greater parental leave is often provided for women as opposed to men, yet both women and men are physically able to parent. Therefore, we have decided to remain focused on the gendered nature of the health workforce dynamics. We have also looked at adjusted hourly rates, which enables us to take into consideration full- or part-time work.

* The authors do a good of outlining the potential for selection bias in the WageIndicator data and make brief mention of the fact that previous analysis shows that these data deviate from national samples by gender, age and education level. What they do not outline, however, is how the deviation by gender in particular could potentially affect their results. For example, if women have lower levels of education in some countries and are less likely to participate (resulting in overrepresentation of educated women), the differences they observe are likely to be underestimates.

Thank you for raising this. May we refer you to the Supplementary Demographic Table that outlines the gender balance in our overall sample. We have acknowledged this limitation in the discussion section on sample and selection bias.

* The authors note that they were unable to conduct any formalized statistical testing because of insufficient sample size. This is a key limitation, and one that deserves a more fulsome discussion whether or not the analyses are painted as exploratory. A small sample, or one with substantial variability, does not negate the ability to perform bivariate statistical testing; it merely reduces the likelihood of finding statistical significance in any differences. Furthermore, because the results are presented solely as figures, it is impossible judge the quality of the data and therefore of the interferences that the authors are making. I would suggest that at a minimum, the authors should include a series of tables that provides, for each comparison the mean and
standard deviation for each of their outcomes preferably by year and by low-, middle-, and high-income country. These tables should include appropriate statistical testing. Readers need to be able to get a clearer sense of the variability with the data and whether that changes in that variability over time could be responsible for some of the trends the authors are observing.

We have presented a table with some basic information on the sample size (for gender ratio), standard deviation (for the gender wage ratio) within each country and occupational group (Table 3). We present the standard deviation of our sample with caution, as it is presented in relation to a ratio measure. We have used the Taylor expansion – in relation to propagation of error - to derive this information, which is presented in the Table 3.

We have also completed a basic trend analysis over time in order to examine temporal changes in the gender wage gap across health occupation and country groups. We did this by calculating the average annual percentage change (AAPC) and its 95% CI for each country grouping and health occupation group using the Jointpoint Regression Program V.3.5.4. AAPC represents a summary measure of the trend over a pre-specified interval of time, and is computed by taking the weighted average of annual changes over a period of multiple years. This approach at its application has been described in more depth by the National Cancer Institute, and has been previously applied in epidemiological research.

* The authors note that respondents to the survey ranged in age from seven to 81 years. I would argue that including children in this analysis is not appropriate. They have not yet been affected by the main biological effects of sex, and their experiences as part of any workforce (including wage conditions) are likely to be driven by the fact that they are children more so than by their sex or gender.

We believe that gender effects begin from a very early age and shape access to the labour market at all ages. It is known that parents invest in their children differently based on gender. In traditionally patriarchal societies boys are more likely to received more inputs because parents believe that the returns on their investment will be higher relative to girls. So even at the age of 7, girls might be less likely to attend school than boys whereby they are affected by their gender. They might also receive lower wages than boys. Therefore we have not changed our original age range.
* The manuscript would be strengthened by the inclusion of a figure that tracks the inclusion/exclusion of individuals and observations. This could help to summarize the text in "country selection and grouping" and lines 268-274 in "health occupations and health worker wages", saving some words.

We have developed a basic schematic that provides a simply summary of the selection and analysis of data, available in Figure 2.

* In addition to an expanded definition of how sex/gender is captured in WageIndicator, explicitly noting how wages were captured would be helpful. In lines 268-271, the authors note that self-reported wages are transferred to gross wages per hour. I am assuming therefore that the way in which wages are reported adjusts for whether or not individuals are working full- or part-time? Including the question wording would make this clear.

In our approach, we have looked at adjusted hourly rates, which then takes into consideration full- or part-time work. We also note in the limitations that we were unable to quantify unpaid work, and that fixed categories of work often do not account for the varied and changing roles that many health workers take on in their profession. We have clarified this in the manuscript.

* The last year of data included is from five years ago. I'm wondering why the data stops at 2014, and whether the authors have any thoughts about what would have happened to these trends in the intervening five years. Could the sample size concerns be addressed with the addition of extra years of data?

Sadly, data quality was not sufficient after 2015 to the purpose of our analysis. We have clarified this in the manuscript.

* In lines 302-304, the authors note that they define gender wage gap gross hourly earnings for women as a percentage of the average for men; however, all the values are between 0 and 1, which is a proportion rather than percentage. Please correct this.
The original wording used terminology consistent with Magar et. al. in their chapter: Women’s contributions to sustainable development through work in health: using a gender lens to advance a transformative 2030 agenda. The wording (proportion, not percentage) has been amended in the text.

* I would like to echo comments made by reviewer 2 about the importance of over/underrepresentation of data by some countries, and the inability to account for horizontal vs vertical wage gaps. Some of these concerns could be addressed using some basic statistical measures.

In the previous round of revisions, Reviewers 1 and 2 suggested that we explore a way to explore horizontal and vertical wage gaps. Although we were unable to disaggregate our results by occupation, we were able to look at two types of occupation: those previously dominated by men and those traditionally dominated by women. Therefore, although the quality of the data and sample size limited us from performing individual-level analysis, we were able to present our findings grouped by healthcare occupations that were either traditionally male-dominated or traditionally female-dominated. This was linked to our framework, which looked at the historical gender division of labour, as introduced in the background and methods section.

Also in our previous revision we noted the limitations of the data, and described how we explored ways in which we could weight for the country size. However, given the discrepancies in the sample size of country-year cells, and the fact that certain country weights may differ substantially between years, we felt it would be misleading to apply these weights. We present our findings as exploratory, and discuss the limitations of this data, hoping that future datasets will be able to build upon these learnings and ensure adequate quality and sample sizes are achieved.

* Given many of the comments I've made, and those made by reviewers 1 and 2 with respect to concerns with methodology and data source, the authors should provide an expanded limitations section. In its current form, it is insufficient. It does not outline the specific selection biases likely to be present in WageIndictator (including over/under-representation of some countries, gender, age and education), nor the effect on results. Nor does it reference the limitations associated with the lack of statistical rigour, limited sample size for some comparisons, inability to distinguish sex and gender effects, or vertical vs horizontal wage inequities.
We have amended our limitations section in order to take into consideration the comments. We have reconfigured our discussion on the limitations of WageIndicator data (which was originally in the methods section) and ensured the limitations section in the discussion is more comprehensive.

* On a minor note, I found this paper to be extremely lengthy. I would suggest that the Discussion (particularly lines 485-577), and the Results sections are overlong and should be edited down.

We have edited out paper accordingly.