Author's response to reviews

Title: Quality of Medical Training and Emigration of Physicians from India: an observational study

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Response to Reviewers

We thank both reviewers for their insightful comments that we believe have helped improve our paper and sharpen its message. Our detailed responses to their comments are provided below.

Reviewer 1: Dr. Onyebuchi A. Arah

1. “Unfortunately, authors fail to capitalize on these global between-country results to parallel and contextualize their within-country findings.”

Response: We are grateful to Dr. Arah for drawing our attention to this point. In the revised version of our manuscript, we directly acknowledge the possible relevance of our results to recent cross country findings by Dr. Arah and colleagues that link differences in migration rates to differences in economic and human resource abundance. We included additional references in the paper that emphasize this link, and the text in the introduction (paragraph 2) as well in the discussion (see p. 13, paragraph 2) was modified to highlight this point.

2. “Another important revision the authors should consider is the flow of the METHODS section. …suggest the flow i) study setting and data; ii) measures – outcome and explanatory variables and iii) analytical strategies – these should be more detailed than in the paper.” “I wonder why the authors did not consider a multivariable regression analysis.”

Response: We have modified and expanded the methods section along the lines suggested by Dr. Arah, and think it is a really good idea. Specifically, sub-headings are now provided in the revised section for ‘Data’, ‘Measures’ and ‘analytical strategies.’

In general, we agree with Dr. Arah that “multivariate regression analysis” would be a good idea to really pin down the drivers of emigration rates among physicians in India. Unfortunately, this is not a straightforward exercise for several reasons. (a) Lack of data at the individual and college levels: We lacked data on medical graduates’ social and economic status, we lacked data on the information on college- (or university-) specific factors that could influence emigration; (b) Scope for province level multivariate analysis of emigration fractions: It would have been nice to undertake a provincial-level analysis to assess whether emigration fractions vary across provinces (for which we do have good socioeconomic data). Unfortunately, with generally poor information on human resources for health, and the fact that admissions are generally open to candidates throughout India (who can conceivably work anywhere they want), it is difficult to carry out a careful multivariate assessment of who migrated from where.

We do, however, make a careful qualitative assessment of many of the potential confounders in the discussion section (pp.13-14). We also note the limitations of our “bivariate” analysis in the analytical strategies section (p.11).
3. “On the measures of quality for ranking: why not also alternatively relate publications to faculty capacity (not just undergraduate seats)?”

**Response:** This is an important observation. We did not do so for two reasons. The minor reason is that we did not have data on the number of faculty members in each school over the entire study period of around 50 years, which is what we really needed for this exercise. However, the **important factor** is that we used the number of undergraduate seats as a proxy of faculty capacity. In India, the Medical Council of India (MCI)-the accreditation agency regulating medical education in India decides the number of undergraduate seats based on the school infrastructure including faculty capacity-faculty/seat ratios etc. In that respect, we believe that our proxy captures reasonably well the information on faculty capacity.

We have slightly revised the text on p.7 (see lines 13-15) to highlight this point.

5. “I am not sure I understand the order of presentation of the TABLES and FIGURES.”

**Response:** We have truncated the footnotes by referring to footnotes table 2. We have merged tables 2 and 3 indicating physician emigration to USA and UK (now a revised version of the original table 2).

6. “The results appear to mix in discussions of the findings.”

**Response:** Following the reviewer’s suggestion we have reorganized the results and the discussion section. The results section is now truncated to include the key findings only and any discussion related to these findings has been moved to the (next) discussion section (see revised text on results on pp. 11-12).

7. “Sometimes the authors refer to the emigration of individual graduates…”

**Response:** We agree with the reviewer’s concern. Because the analysis is at the school/university level, the inference must be at that (school/university) level as well. We have gone through the manuscript and addressed this point.

8. “…I miss in the discussion reflections and references to the broader work on correlations between human development and emigration…”

**Response:** We agree and have taken this into account. Please see discussion on (p. 13, paragraph 2) in the paper.

9. “Consider minor revision of the ABSTRACT to reflect clearly….”

**Response:** As suggested by Dr Arah, our revised abstract now clearly states in the method section:
“We calculated the fraction of medical graduates who migrated to the United States and the United Kingdom based on rankings of colleges and universities according to three indicators of the quality of medical education (a) student choice, (b) academic publications, and (c) availability of specialty medical training.” (See Revised Abstract)

10. Note: We have also incorporated all suggested discretionary revisions.
Response to Dr. Danette McKinley

1. “I believe that the authors missed a couple of relevant references…”

Response: In revised manuscript, we have included more recent references, as noted by the reviewer, such as Dr. Arah’s work in AJPH and BMC Public Health. This work is actually quite relevant to our analysis, a fact that we refer to briefly in our revised introduction (see p.3., bottom), and in the discussion section (p.13, paragraph 2). Upon reflection, we were less comfortable with the need to include the reference by Boulet et al. (2006) article in Health Affairs, given its focus on broad international trends in ECFMG examinees from different countries. Our focus was on the relationship between the quality of medical education training in Indian medical colleges and its association with emigration fractions; it was not immediately obvious how we could use the Boulet findings to shed light on our main results.

2. “The authors do not adequately state the challenges of developing quality indicators for the private medical colleges…”

Response: This is a valid point. The reviewer correctly lays out the difficulties in developing quality indicators for all medical colleges as acknowledged by AAMC position (reference 29 in revised manuscript [1]).

Nonetheless, ranking of medical schools is an important exercise for a variety of reasons, even apart from concerns about emigration. Policy makers need transparent and uniform methods to evaluate heterogeneously managed medical colleges, whether to fund expansion of medical education based on current quality and future needs, or to maintain and improve medical education. In addition, incoming students also need some guidance to potential methods to evaluate and assess different professional schools.

To overcome weakness of uni-dimensional ratings, rather than creating artificially weighted ratings, we have used multiple, transparent and objective ranking methods, covering distinct domains of medical education [1].

For ranking, when data was available e.g. at college level, we do not assume that private medical colleges are of lesser quality. Indeed, some of the private (non-government owned) medical schools like Christian Medical College (in Vellore) and St. John’s Medical college (Bangalore) are in the top quintile of our rankings but based on our ranking methods, the vast majority of private medical colleges have a low rank. However, when we pooled data from college level to university level, missing information on private colleges would result in university ranking being biased upwards. As mentioned in discussion, this misclassification, would result our results towards the null.

3. “Also, the authors discuss the possibility of developing high-quality research….”

Response: The reviewer correctly points out that there is scant data to suggest that emigrating physicians contribute to high-quality research and policy institutions in host-
countries. Despite the difficulties in finding an appropriate comparison group for such a study, a study by Akl et al., on Lebanese graduates in US, suggest that emigrant physicians, at least from Lebanon, are more likely to be board certified than US born physicians [2]. The data and quality of evidence about the position of returning physicians is limited and because until now, very few physicians return to the host countries [4]. In fact, in India, until recently, residency and fellowships abroad were not recognized for employment in government sector. In a letter to editors, prominent academics in Pakistan pointed out that physicians returning to Pakistan are generally recognized physician leaders[3]. Nonetheless, we have made the connection more tenuous in the revised manuscript [see revised text on p.16, paragraphs 1 and 2].

Supporting References


