Reviewer’s report

Title: Physicians' Knowledge and Practice on Death Certification in the North West Bank, Palestine: Across sectional study

Version: 0 Date: 25 Oct 2016

Reviewer: Myles Leslie

Reviewer's report:

Thank you for the opportunity to review the manuscript entitled: Physicians' Knowledge and Practice on Death Certification: A cross sectional study, Palestine. I present my technical comments first, and then move on to a higher level discussion of the paper's existing and potential arguments.

I read the authors' work with interest and pleasure, but would strongly recommend a 'native English check' of the manuscript to bring its use of definite and indefinite articles into line with more standard usage, and to streamline a number of other anomalies and awkward turns of phrase that make their story harder rather than easier to follow.

A further technical concern lies in the authors' description of their coding methods. I have checked the box 'unable to assess' in answer to both the methods and control questions posed by the journal's standard review form. Specifically, readers will need a more transparent description of:

* Who performed the primary coding work,
* Who checked that researcher's work,
* How disagreements in coding were dealt with and resolved, and if possible,
* What the inter-rater reliability was between the primary and secondary coders.

At present, the passive voice construction on page 4, line 45, gives rise to more questions about methods and controls than it gives answers. Further development of this section will improve the paper.

At page 3, line 16, the authors cite a single South African source in support of their contention that DNF completion is 'common practice for physicians.' Further citations confirming the
centrality of physicians in completing DNFs across common and civil law traditions will give the paper greater depth.

A final technical concern focuses on pages 4 and 5. Page 4 line 20 references 'the North East Bank.' This proper noun and location needs to be more fully specified, with the authors giving some statistics and demographics for the area's population, death rates and mechanisms, access to medical care, etc. The authors should also check page 5 line 15 where they use 'the North West Bank.' Is this the same location and a typographical error, or a demographically different location?

In addition to these technical issues I would strongly encourage the authors to revisit and clarify the central purpose of their paper. For the moment, the paper rolls together or hints at three potential arguments, or analyses. Developing one, or all of these, will help the paper a great deal.

The introduction, as it stands, describes the general state of the literature, which is to say it establishes that, when it comes to death certification by physicians, there is a great deal of not just inaccuracy, but concern about the policy implications of that inaccuracy. The authors then move on to describe a particular death data collection tool (the US DHHS DNF) and then adopt that tool's definitions. This sets up a fairly technical story in which the authors examine the US-DNF in action, focusing on rates and types of errors as physicians in Palestine (mis)use it. For this story to be compelling to readers - and not just a snapshot of medico-bureaucratic life in Palestine - we need to know more about the US-DNF and its potential applicability, or inapplicability, to operational conditions in Palestine.

[see my call above for greater specification of 'the North East Bank' as a place to use the US DNF]

Once this sort of background is established, the authors will be able to draw out broader conclusions about the fit and adjustments necessary to the US-DNF form as it travels internationally. For the moment, these questions of fit and applicability are set aside, with the authors concluding that training, guidelines, and manuals are the best way to ensure physicians in Palestine complete the US-DNF in a more accurate way. What if the form, itself, is part of the problem? What if the US-DNF just doesn't fit local needs or norms? Might not other countries, or international agencies like the WHO, who are looking to move towards a standard DNF be interested in learning from the Palestinian experience? Here is where this technical story of rates and types of errors in DNF completion could be leveraged into something with more appeal to an international audience.
A second potential focus for the authors lies in using their Palestinian case study as a window on an old observation in the literature. That the data entered into DNFs are inaccurate (by epidemiological standards) and vary to a great degree across jurisdictions is well documented in the literature, as the authors show at page 8 line 57. How does the Palestinian experience (and so the authors' paper) add to, rather than merely replicate, these findings? Given that it is not a new conclusion that further training and attention detail are necessary - see AmJForMed&Path: Goodin and Hanzlick 1997, Hanzlick and Goodin 1997, Lu et al 2006 - how might Palestine's experience (the demographics and training of its physicians) be a helpful lens for viewing efforts to write training and encourage attention to detail?

A third potential focus - and so a third point where the authors might more closely integrate their research into the existing literature - comes on page 8 line 52 to page 9 line 6. The finding that physicians who complete DNFs feel they DO NOT need training when their performance at completing the DNF's suggests that they DO, is fascinating. Particularly in a paper that has the potential to engage the US-DNF as an international form out-of-synch with local norms and operating conditions. Why is there such massive variation between Qatari and Bahraini perceptions of the need for training, and how might the Palestinian experience reported in the present paper help us understand that gap in perception better? Here is a true hole in the literature, as it were.

To sum up, my broader concern with the paper, beyond its technical issues - which are easily dealt with - is that it is not presently telling a compelling story that is well integrated into the existing literature. As it stands, the authors tell a typical story of (DNF accuracy) problems and (training and attention) solutions at play in Palestine. There is a modest contribution to be made in reporting these data. The authors have the potential, however, to make a much larger contribution by closely describing their case study and drawing out lessons about how a form like the US-DNF can be better adapted to local conditions and training routines. I would encourage them to revise their manuscript with this sort of contribution in mind, and encourage the editors to give them the space to make such changes. There is much of importance here, and a great deal of excellent work has already been put into the manuscript. The authors are to be congratulated.

Are the methods apprropriate and well described?
If not, please specify what is required in your comments to the authors.

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