Reviewer's report

Title: Community paramedicine model of care: an observational, ethnographic case study

Version: 2  Date: 4 September 2015

Reviewer: Richard N. Bradley

Reviewer's report:

I appreciate that this paper addresses an important emerging topic in the field of emergency medical services. I understand the differences between qualitative and quantitative research, however many of the statements in this paper do not seem to be adequately supported. I think this paper is an interesting product, however I would rather see it as an opinion piece.

MAJOR COMPULSORY REVISIONS

My first major concern is that research question was not well defined. The authors must revise the paper to eliminate discrepancies between the abstract and the introduction to the paper, and clarify the a priori research question. In the abstract the authors state that “our aim was to identify and analyse how community paramedics create and maintain new role boundaries and identities in terms of flexibility and permeability and through this develop and frame a community paramedicine model of care.” However in the introduction to the manuscript, the authors state that, “This paper addresses the second of these challenges [[applying] a theoretical framework to the analysis of CP as an emerging model of care], bringing together the RESP model and new data from Canada to form a coherent framework (RESPIGHT) that describes the conceptual basis of a CP model of care, distinguishes it from other innovations in paramedic service delivery and provides guidance to providers contemplating its introduction.” The authors should clarify the research question. Was it to identify and analyse role boundaries and identities or was it to describe the conceptual basis of a CP model of care?

In the ‘Background’ section, the authors imply that “a coherent framework (RESPIGHT)” was determined after the completion of the data analysis. It appears that the data collected in this study was used to form the RESPIGHT model. Since the RESPIGHT model was not available a priori, it should not be described in the section describing the Background to the research question.

My second major concern is that this study does not sufficiently account for potential biases. I did not see any consideration of a bias in the results introduced by the Hawthorne Effect. Neither did the authors discuss the use of triangulation as described by Reeves in 2008.

My final major concern is that while the authors have described their observations, these do not adequately support the development of the
RESPIGHT model. For example the ‘G’ in the acronym, which stands for ‘governance and leadership’ is supported only by one sentence of evidence (“a number of physician participants suggest”). While I do understand the limitations that one encounters with qualitative research methodologies, I do believe the evidence presented for a developing a new model should be more than “a number of participants suggest.”

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.