Commentary

Staffing level: a determinant of late-onset ventilator-associated pneumonia

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See related research by Hugonnet *et al*., http://ccforum.com/content/11/4/R80

Abstract

A body of knowledge exists to suggest an association between nurse staffing and adverse patient outcomes. Hugonnet and colleagues add further evidence by linking nurse staffing to late-onset ventilator-associated pneumonia. Discussed are a number of concerns surrounding the analytic component of this study, including the construction of variables and the statistical models. The authors' estimation that hospitals maintaining a nurse-to-patient ratio above 2.2 could decrease the risk of health care associated infections is based on findings that are potentially biased and unrealistic.

Hugonnet and colleagues [1] present an interesting article on nurse staffing and ventilator-associated pneumonia (VAP). Although this study joins a number of other studies on nurse staffing and adverse outcomes of hospitalized patients, I feel compelled to address several important limitations of this study. What I find disappointing is the fact that these authors describe an observational study in which I expect they had an opportunity to add something substantial to the body of literature on nurse staffing and adverse health care related outcomes, but failed to do so.

There is sufficient evidence in the literature to suggest that nurse staffing is significantly associated with health care associated infections [2-4], but we lack data on the process of nursing care that may very well inform us as to why this staffing association exists. The authors do state that some of the process of care measures were not consistently recorded, but they do not state that all of those measures were missing. Because these data on the process of care are of such importance, I would hope that the authors considered a method of imputation before making the decision to eliminate these data from the analysis. They control for central venous, peripheral, and urinary catheters, but it would have been of great value to include data on nurse process of care measures, such as the presence or absence of mouth care [5,6], which is a potential risk factor for VAP. In addition, I am surprised that the authors did not include hand hygiene as a risk factor of interest, because there is a well established link between hand hygiene and health care associated infections, and one that the authors have worked with extensively [7,8].

The authors painstakingly constructed a comprehensive risk adjustment model that includes, but is not limited, to the Charlson comorbidity index [9], the Acute Physiology and Chronic Health Evaluation II score [10], and the Projet de Recherché en Nursing acuity score. I am concerned, however, that these measures overlap and, although not mentioned, I hope that the authors verified that there were no issues with collinearity. The Cox hazards model is an appropriate choice when measuring time to VAP, but I have a few concerns surrounding the method of censoring and construction of the variables that are time dependent. I can understand the exposure period for the nurse staffing variable, but I think it best to construct all other of the time-dependent variables as days from admission to censoring. As for censoring, I also do not agree with censoring 5 days post-extubation. The authors' choice of censoring prohibits taking into account the patients who might well have experienced respiratory compromise and required re-intubation. Because the extubated patients were censored (removed from the analytic model) on day 5 after extubation, these patients are no longer included in the sample for analytic purposes, even though they are still presumably at risk for VAP.

What I found most troublesome with this analysis is how the authors computed what they refer to as the risk factor of interest, namely nurse staffing. They refer in the text to the nurse staffing per shift, and in fact they provide the median nurse-to-patient ratio for the morning, evening, and night

VAP = ventilator-associated pneumonia.

shifts as 0.8, 0.6, and 0.6, respectively. However, in the final hazard models nurse staffing is computed as the total number of nurses working in a 24-hour day divided by the patient census. Such a computation inflates the nurse-to-patient ratio, as indicated by the fact the median daily ratio ranged from 1.4 to 5.3 nurses per patient. Nurse staffing has been computed differently in a number of studies in the literature, such as full-time equivalent registered nurses per adjusted inpatient day [11], registered nurse hours per adjusted inpatient day [12], and nurse-to-patient ratio [13]; although these computations differ, the final recommendations make some sense from an administrative point of view. The estimation by the authors that hospitals maintaining a nurse-to-patient ratio above 2.2 could decrease the risk of health care associated infections is based on findings that are potentially biased and unrealistic.

Even though Hugonnet and colleagues provide what I consider to be suboptimal estimates of nurse-to-patient ratio, I applaud their attempt to forge along the causal pathway that links nurse staffing to health care associated infections in an attempt to improve the quality of patient care.

Competing interests

The author declares that they have no competing interests.

Authors' response

Stéphane Hugonnet

We thank Cimiotti for her detailed commentary on our study [1] and focus on the few relevant criticisms she makes in our response.

Although knowledge in this field is still partial and further research is required [14], our study adds to the increasing evidence in the literature that adequate staffing is a prerequisite for patient safety. It provides additional data on the epidemiology of VAP; few studies have investigated the association between workload and pneumonia [15,16], and none have specifically focused on late-onset VAP.

The optimal method with which to estimate how much time and care each patient received in order to derive some sort of an 'offer/demand' ratio would be to measure it individually, but this is unrealistic. Computing a workload measure per shift or over 24 hours does not make any difference, as explained in our report. Neither is there any fundamental difference between measuring nurse-to-patient ratio, full-time equivalent nurses, or number of nurse hours per patient. These details should not blur what is by far the main problem; these measures are all of an ecological nature [3,12,17] and this is seldom acknowledged.

We agree with Cimiotti that the risk factor analysis for VAP is not straightforward. Because we investigated only VAP, the analysis of time or time at risk cannot start before initiation of mechanical ventilation, and precisely how long a patient remains at risk after extubation is unknown. We agree that 5 days is an arbitrary cut-off value, but it seems very reasonable to assume that a pneumonia developing 7 days after extubation is unrelated to mechanical ventilation, as long as there is no intervening re-intubation. Of note, a patient who was extubated and re-intubated 3 days later was still in the at-risk period and included in the analysis.

We agree that the process of care is an important issue, but lies in the causal pathway between workload and infection. However, the priority is surely not to demonstrate that busy health care workers do not fully comply with infection control recommendations, but rather to improve the process of care, define adequate staffing levels, and refine statistical and mathematical techniques in risk factor analysis [14,17].

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