Reviewer's report

Title: Prevalence of urinary colonization by ESbetaL Enterobacteriaceae among permanent catheterised inpatient in Italian LTCFs

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Reviewer: Christopher Crnich

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The manuscript submitted by Arnoldo et al. to BMC Infectious Diseases represents a cross-sectional study of the prevalence of beta-lactamase producing organisms among Italian long-term care facility (LTCF) residents. There is increasing recognition that long-term care facilities (LTCFs) contribute to the regional spread of multidrug-resistant organisms (MDROs) so studies such as this are a welcome contribution to the field. That said, I encountered a number of problems while reviewing this manuscript that, in my opinion, should be addressed prior to publication. I have summarized these concerns below in numbered format:

1. The grammar throughout the article was poor and made it difficult to read the manuscript in numerous locations. I would recommend that the authors seek contribution from a writing assistant in order to improve this aspect of the manuscript.

2. The method used to select participating facilities was never specified in this manuscript. This is critical information for the readership. The authors admit that their study may not provide a representative view of the extent of beta-lactamase producing organisms in Italian LTCFs but I am left wondering the rationale for the way facilities were selected. A revised manuscript should provide much more detail on how participating facilities were selected. This will allow readers to better determine how representative (or unrepresentative) the study is.

3. In a related manner, the authors provide no detail on how subjects within study facilities were selected for participation. I understand that only residents with indwelling urinary devices were included but I am not sure what they mean by permanent urinary devices. In my mind, presence for >48 hours does not necessarily represent a permanent urinary device. If presence for >48 hours was indeed the only selection criteria then the authors need to remove reference to “permanent” throughout the manuscript. It was also not clear if suprapubic and condom catheters were included in this study. Finally, I have no sense of how many potential subjects were excluded from this study. A revised manuscript should provide more detail on how many subjects were in the study facilities on the day the point-prevalence study was performed (both those with and without indwelling urinary devices) and the proportion of residents that were eligible to participate based on their inclusion criteria. This will allow the reader to determine how representative the study population was compared to the source
population.

4. Were residents consented for this study? Was this study reviewed by an IRB?

5. It is not clear why certain data were collected. Were these selected with specific hypotheses in mind? For example, why is recent surgery important? Why was data on current antibiotic use collected in contrast to recent precedent antibiotic use? The way these data are collected raises some concerns for protopathic bias (i.e., suppression of growth from urine leading to identification of antibiotics as a protective factor). In my opinion, it would be important to have data on antibiotic exposures in the past 30-90 days. Why were diabetes, cancer and renal failure the only comorbidities examined? Is there any way to capture data on functional status of residents? Impaired mobility and functional status have been consistently identified as a risk for colonization with MDRO in the long-term care literature. A revised manuscript should provide more detail on the rationale for data that was collected. I would also like to see more detail on recent antibiotic use in addition to current use.

6. In reporting out the patient characteristics, it is important to denote how long residents were in the study facilities, how long they were catheterized at the time the sample was collected, and whether these characteristics differed among those with ESBL colonization compared to those without. I am not particularly familiar with the long-term care environment in Italy, however, I found it surprising that nearly 78% of the subjects in the current study had resided in study facilities for <6 months. Is this atypical for Italian long-term care facilities?

7. The decision to examine risk factors only using bivariate statistics does not make sense as the observed relationships are very likely confounded by other risk factors (both those collected and not collected by the authors). It is impossible to determine the extent to which individual risk factors contribute to resident risk of colonization with ESBL organisms. A revised manuscript should include a multivariate logistic regression model to better isolate the effects of various risk factors associated with ESBL colonization.

8. The authors should be more conservative in the interpretation of their findings. Specifically, they should focus more of their discussion on the representativeness of their findings (e.g., should acute care facilities accepting LTCF residents in transfer be concerned about ESBL organisms in all situations? only in those subjects with an indwelling urinary catheter? only those subjects with a catheter and other risk factors?). The authors should remove the statement “…these data seem to confirmed the possible spreading of resistance genes between LTCFs and Acute Care Facilities” from the manuscript. Their data do no such thing.

In prioritizing my concerns, I would make the following suggestions:

Major Compulsory Revisions:

1. A revised manuscript should include more detail on how facilities were selected for inclusion in this study.
2. A revised manuscript should include more data on the source population in study facilities (overall number of residents, overall number of catheterized residents, proportion of eligible residents who participated in the study).

3. A revised manuscript should report if the study protocol was reviewed by an IRB and whether or not informed consent of subjects was performed as part of the study protocol.

4. A revised manuscript should include data on length of residence in study facilities, length of catheterization prior to data collection, and a bivariate comparison of these characteristics and the ESBL colonization outcome.

5. A revised manuscript should include a multivariate logistic regression model of the risk factors associated with ESBL colonization (including methods for selection of candidate risk factors, methods for model building, and methods for assessing model fit).

6. The authors need to delete the phrase “…these data seem to confirmed the possible spreading of resistance genes between LTCFs and Acute Care Facilities” from the discussion section of the manuscript.

Minor Essential Revisions:

1. I would like to see more detail on the rationale for selection of certain variables in a revised manuscript.

2. I would like to see data on recent (not current) antibiotic use in a revised manuscript. If this data is not available, this should be specified in a revised manuscript.

3. Figure 1 was not included in the draft I reviewed.

4. The grammar of the manuscript needs to be greatly improved before it can be published.

5. I would like to see a much more conservative discussion section in the revised manuscript in which the authors devote more of the discussion to the internal validity of their study and much more focus on the external validity of their study.

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

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare I have no conflicts of interest pertaining to this review