Review of: "Prevalence and antimicrobial susceptibility pattern of urinary tract infection among pregnant women attending Hargeisa Group Hospital, Hargeisa, Somaliland"

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First of all, I would like to appreciate the authors in conducting such research finding that will contribute some scientific information to the adversary of possible future pandemic, antimicrobial resistant, from the other corner of the globe, Somaliland. Being it was collected from the pregnant women, would have also additional importance. The sample size seems sufficient even though the authors did not explain through scientific sample size calculation.

In general, even though I can't say the paper is flawless, its abstract, introduction, methodology, results, and discussions were well written. The authors raised some important findings and compare and contrast it with other findings from Ethiopia, other African nations, Asia, Europe....

Despite those strength of the study explained above, it would have appreciated if the author had clarified/explained the following important points before it was published.

1. Regarding ethical concerns

• It written two times. One under the method part while the second one was under its own heading. And it might be redundancy.

• The other serious thing was that: why two different reference numbers given from Hawasa University. The one under the method part state the ref. no. of the ethical approval as (Ref no:IRB/204/12), but the second statement under ethical approval heading stated (Ref no.: IRB/231/11). Why the same institution gave two different ethical approvals for the same research project?

• The authors got the ethical approval from the Hawasa University which found in Ethiopia, but the actual research was conducted in Hargeisa Group Hospital (HGH) which found in Somaliland. And the authors did not tell us/wrote anything about the ethical approval from the HGH of Somaliland even though it is known that Ethiopia and Somaliland (part of Somalia) are two different African countries. Where did the urine sample processing, analysis, culture, drug susceptibility test the authors conduct? In Ethiopia or Somaliland? If it was in Ethiopia (i.e, in Hawassa comprehensive specialized hospital microbiology), was that even possible to transport urine samples of one country to the other countries? Any quality assurance for the samples if it were already transported to Ethiopia? The authors need have explained explicitly how and where the samples were processed.

• The two forms of consent stated in the paper. The authors need not have reported the two at the same time. It had better if it was either informed consent or written informed consent than creating ambiguity.

2. Method part

• The quality assurance and control followed during urine sample collection and analysis (culture and DST) were not explained

• Moreover, the types of standard strain used to check for culture performance and drug disks were not explained. The types of standard strain used and its source must be stated for quality control and assurance of culture and DST.

3. Results part

• The scientific name of the bacteria must be written in italic form. But the acronomy/abbreviation of species spp must not written in italic since it does not indicate the bacterial name. However, spp was frequently used for Citrobacter (Citrobacter spp is incorrect whereas Citrobacter spp is correct). Besides, in figure 1, table 3 and elsewhere the bacterial names were not written in italic.

• The definition of MDR was not scientifically defined. For instance, if a given bacteria developed resistance to the three or more drugs from the same structural classes (let it was: nalidixic acid, norfloxacin, and ciprofloxacin) of the antibacterial, could we say it was MDR? To conclude the bacterium was MDR, it must develop resistance to at least ≥3 antibiotics from different structural classes of antibiotics. So that, I fear that the figure stated for MDR in table 5 might be incorrect.

• In table 5, the authors presented those bacteria showed resistance to 3 or more antibiotics as R3,R4,R5,R6 and R≥7 (NB: be mindful with my concern on the definition of MDR the authors used). However, the authors did not tell us about which antibiotics were there in R3, R4, R5, R6, and R≥7. It would have highly recommended if the authors had included additional table (probable Table 6) that shows antibiotics in each R with each bacterium. For instance, we cannot know to which antibiotics the E.coli developed MDR. If those combination/s of antibiotics was/were not stated clearly it would be difficult to accept the recommendation of the authors. For clarification, the authors can go through different published papers on the same topic.

• My other serious concern is the results presented in the table 6 of the paper. The 95% CI for age and marital status was 0.000. It happened due to too few sample size when the authors stratified the data in to each group. And the CI could not be 0.000. As a remedy, the authors should aggregate some variables with too few samples, or exclude it from analysis. Unfortunately, the age showed significant association with the presence of UTI with 0.000 95% CI (for age group 36-40) in both COR and AOR. Therefore, the AOR and P-values generated for educational status, family income, history of catheterization and history of UTI might be invalid unless the above conditions for 95% CI satisfied.

4. References

• Reference no.6 and 14, 5 and 51, 24 and 31 are identical.

5. Punctuation

• Occasionally, I have observed absence of full stop at the end of the sentences, and not starting with

capital letter for the sentences.