

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Local prevalence of extended-spectrum beta-lactamase (ESBL) producing Enterobacteriaceae intestinal carriers at admission and co-expression of ESBL and OXA-48 carbapenemase in Klebsiella pneumoniae: a prevalence survey in a Spanish University Hospital
<b>AUTHORS</b>	DIAZ AGERO PEREZ, CRISTINA; LOPEZ FRESNEÑA, NIEVES; Rincon Carlavilla, Angela; Hernandez Garcia, Marta; Ruiz-Garbijosa, Patricia; ARANAZ ANDRÉS, JESÚS MARIA; Maechler, F; Gastmeier, Petra; Bonten, Marc; Canton, Rafael

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Zerouali Khalid Faculty of medicine and Pharmacy, University Hassan II - Casablanca, Morocco
<b>REVIEW RETURNED</b>	09-Aug-2018

<b>GENERAL COMMENTS</b>	<p>The article is interesting it meets the objectives of the authors, it is a relevant study that gives an evaluation on the epidemiology of carriage of ESBL enterobacteria and carbapenemase producing enterobacteria.</p> <p>Nevertheless, there are clarifications to make</p> <ol style="list-style-type: none"> <li>1 - the technique described for the detection of ESBL in enterobacteria is not appropriate for Acinetobacter spp, ESBL in Acinetobacter has it been confirmed by another technique</li> <li>2- In Table 4, 3 strains (2 E coli and 1 K pneumonia) showed the presence of CTX-M type ESBLs. Could not they be typed?</li> <li>3- The authors described the presence of one case of KPC-3 and one case of NDM-1. Is there concrete information for these 2 patients?</li> <li>4- In the discussion add some information concerning the patients carrying ESBL and CP (provenance, transfer of another hospital, recent hospitalization, recent trip abroad ...)</li> </ol>
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<b>REVIEWER</b>	Sika DOSSIM UNIVERSITÉ DE LOMÉ, FACULTÉ DES SCIENCES PHARMACEUTIQUES, LABORATOIRE DE MICROBIOLOGIE, CENTRE HOSPITALIER SYLVANUS OLYMPIO, COUNTRY: TOGO
<b>REVIEW RETURNED</b>	11-Oct-2018

<b>GENERAL COMMENTS</b>	<p>Text written in English quite easy to understand. The theme is interesting except that the prevalence of ESBL has largely been achieved in different cities within their country and the results found does not show a significant difference.</p> <p>The methodology is well described. The only problem is that the authors announce that they do the determination of the enzymes</p>
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	<p>however at the level of the results only some were realized and without any explanation.</p> <p>Thus, within the limits of their study, they should specify that the result found for the different resistance enzymes could be different if all had been identified.</p> <p>Abstract: Conclusion: the sentence "The prevalence found in our study is very similar to that found in the literature" should be replaced by "the prevalence found in our study is very similar to those in other parts of Spain". This will be more accurate.</p>
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## VERSION 1 – AUTHOR RESPONSE

### Reviewers' Reports:

Reviewer: 1

Nevertheless, there are clarifications to make

1 - the technique described for the detection of ESBL in enterobacteria is not appropriate for *Acinetobacter* spp, ESBL in *Acinetobacter* has it been confirmed by another technique.

The referee is right. The presence of ESBLs in *Acinetobacter* has been scarcely reported. When this happens, it has been reported in carbapenemase producing strains and phenotypic tests should not be conclusive, We have reviewed the data of this isolate and ESBL expression was not later confirmed. Therefore, we have decided to delete this isolate from our report. We have deleted 1 *K. pneumoniae* as well, it was at first classified as ESBL but considering the antibiogram it looks more like AmpC. Consequently, several figures have changed. We apologize for this inconvenience.

2- In Table 4, 3 strains (2 *E. coli* and 1 *K. pneumoniae*) showed the presence of CTX-M type ESBLs. Could not they be typed?

We firstly amplified ESBLs of different groups (CTX-M, TEM, and SHV) and latter for different groups of CTX-M. If the amplification of different groups was not obtained or was not conclusive, we referred to as CTX-M-type. We have revised that and now that problem affects only to 1 *E. coli*.

3- The authors described the presence of one case of KPC-3 and one case of NDM-1. Is there concrete information for these 2 patients?

We have added information about those 2 patients (pages 13-14; lines 279-285)

4- In the discussion add some information concerning the patients carrying ESBL and CP (provenance, transfer of another hospital, recent hospitalization, recent trip abroad ...).

Unfortunately, that is not possible, no other risk factors were recorded, only the data shown in the paper (age, gender, ward, previous colonization).

Reviewer: 2

1-The methodology is well described. The only problem is that the authors announce that they do the determination of the enzymes however at the level of the results only some were realized and without any explanation.

We added an explanation, it was due to budget issues so we made a random selection (page 14; lines 303-304).

Thus, within the limits of their study, they should specify that the result found for the different resistance enzymes could be different if all had been identified.

We added one sentence addressing this limitation (page 14; lines 305-307).

Abstract: Conclusion: the sentence "The prevalence found in our study is very similar to that found in the literature" should be replaced by "the prevalence found in our study is very similar to those in other parts of Spain". This will be more accurate.

We don't agree with the reviewer, we show similar data from a Dutch study, so we decided to keep that sentence this way.

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Sika DOSSIM UNIVERSITÉ DE LOMÉ, TOGO
<b>REVIEW RETURNED</b>	26-Nov-2018
<b>GENERAL COMMENTS</b>	Very interesting study. Good job.