Epidemiol. Infect. (2009), **137**, 542–543. © 2009 Cambridge University Press
doi:10.1017/S0950268809002209 Printed in the United Kingdom

Correspondence

Epidemiol. Infect. (2009)
doi:10.1017/S0950268809002209

Chikungunya epidemic in Réunion Island

To the Editor:

We were puzzled by the paper published recently online in your journal by Economopoulou *et al.* [1]. As physicians, we were directly involved in the Chikungunya epidemic in Réunion Island and we think that some of the reported data do not correspond to the real situation [2, 3]. Indeed, we wish to inform your readers about several issues that need to be raised, as some results of this study can be dangerously confusing.

There is a major bias in the inclusion criteria for 'atypical' cases. In this paper, a case was considered 'atypical' if signs other than fever and polyarthralgia were present. According to this definition, a patient presenting with a skin rash was considered 'atypical'. Several studies have shown that skin rash is fairly frequent, with a prevalence between 14% and 77% in the different reports, in patients with Chikungunya infection [2, 4–7]. Can we consider as 'atypical' a patient presenting with a sign that is present in almost half of the patients (40% of patients in our series)?

The definition of 'severe case' is unclear. What do the authors mean by 'the maintenance of at least one vital function'? Does it correspond to the commonly used definitions of shock, respiratory failure and renal insufficiency? If not, which criteria have been used?

The number of cases with pneumonia (102 patients, 16·7% of the cohort) is somewhat unlikely, especially in a population with a low prevalence (19%) of underlying respiratory disease (unfortunately, we do not know if these patients had an underlying cardiac condition). What was the case definition of pneumonia? Respiratory involvement during Chikungunya infection was rarely, if ever, reported in the majority of previous studies, including ours, that were performed during the epidemic described in this paper. Only two recent reports described respiratory symptoms during Chikungunya infection, but in one case these were secondary to simultaneous respiratory viral infections and, in the other, concerning a cohort of 33 patients hospitalized in the intensive care unit, the six cases of pneumonia were mostly related to a bacterial co-infection [8, 9].

We are therefore surprised by the prevalence of pneumonia in this study. Considering the results of the previous studies, the statement of the authors that describes pneumonia as a 'new clinical form' of Chikungunya fever appears unbelievable and the greatest caution should be exerted in the interpretation of these data.

In our opinion, this number of cases of reported 'pneumonia' is clearly overestimated. Moreover, in French hospitals, chest X-rays are not evaluated by a radiologist. Their interpretation, especially difficult in elderly people with comorbidities, can be easily mistaken, with an excess of pneumonia diagnosis in feverish patients. In this study it seems there was no assessment of the chest X-rays by a specialist.

The authors state that there were 35 cases of myocarditis or pericarditis. How was myocarditis, whose diagnosis is particularly difficult, defined?

The authors also state that diabetes mellitus was a new 'clinical form' of Chikungunya fever. In Réunion Island, where the prevalence of diabetes is around 20%, it would be more likely that diabetes pre-existed.

To conclude, we are surprised to see this paper published in your journal. Some of us were contacted by the authors for advice. Unfortunately, none of our comments were accepted and consequently one of us declined to be associated as a co-author. There is a major bias in the methods. All these data were obtained by a person without any medical experience reviewing retrospectively many medical files. We do not think this is an acceptable means to obtain reliable data.

Declaration of Interest

None.

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The authors reply:

We would like to remind the authors of the above letter that this was an epidemiological study based on clinical files and not a case review.

We are criticized for considering as 'atypical' a patient presenting with a sign that is generally present in about half of the patients (40% of patients in their series). In response we would like to state that this case definition was made *a priori* and was approved by the Chikungunya scientific committee of arboviruses (a committee composed of epidemiologists and hospital practitioners), before discovering that about half of our patients would present with this sign and symptom. It is well known that changing a case definition according to the result is unscientific.

Regarding the definition of a severe case being 'unclear' and the reference to 'the maintenance of at least one vital function, does it correspond to the commonly used definitions of shock, respiratory failure and renal insufficiency', we maintain that it corresponded to the commonly used definitions. Again, the protocol was approved by the Chikungunya scientific committee in which there were participating clinicians of the four hospitals of the island.

Regarding the case definition of pneumonia, our role in this outbreak was to collect data from the patient's medical file. We were ourselves surprised that so many patients had this diagnosis. The diagnosis was assessed by physicians in the hospitals of Réunion Island based on the routine procedures used. Since in these hospitals it is not necessary for X-rays to be evaluated by a radiologist, the use of an expert radiologist for the assessment of the cases for this study would lead to a bias. More information on the procedure used is given in the report by InVS [1].

Regarding the case definition of myocarditis or pericarditis, we collected data from the medical files and reviewed the cases with the chief cardiologist at one hospital.

According to the study protocol 'patients with no recorded diabetes mellitus or treatment for diabetes' were recorded as new cases.

It is true that clinicians from St Pierre hospital did not collaborate, but their colleagues from the hospital of St Denis were very cooperative.

We are surprised to learn that one or more of the authors of the above letter refused to be co-authors. We never asked them to contribute at any stage of the article preparation and we never asked them to be co-authors.

A person does not have to be medically qualified to perform data collection. My colleague is an experienced epidemiologist; a background of veterinary medicine allowed her to understand perfectly the needs of the study. Moreover, she received special training for the data collection for this study.

Declaration of Interest

None.

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