Reviewer’s report

Title: Molecular prevalence of intestinal parasites infections in children with diarrhea in Franceville, southeast of Gabon.

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Reviewer: José Guillermo Esteban

Reviewer's report:

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Perhaps, the most relevant aspect in the present study is the concept of diarrhea, which is usually defined as the decrease of the consistency and, at the same time, an increase of the frequency of evacuations. Diarrhea is actually a sign that reveals the physio-pathological alteration of one or several functions of the intestine (secretion, digestion, absorption or motility) and, finally, is an indicator of an intestinal disorder related to the transport of water and electrolytes. Perhaps, the concept of diarrhea should be defined, as it may refer to "acute" (not exceeding a duration of two weeks), "persistent" (a duration of two to four weeks) or "chronic" (when ongoing for more than four weeks). Infections that often provoke acute diarrhea, just as is the case with chronic diarrhea, are due to intestinal parasites.

It remains unclear what is really meant by "direct microscopic examination", as there is nothing in the Ms that could indicate how the microscopic study of the intestinal parasite infections in the children affected by diarrhea in Franceville was carried out.

When the authors refer to "PCR detection", 18 parasite species are being mentioned, which are listed in Table 1. I do not understand why they are listed in that order as no particular criteria become evident. At least, initially protozoans (i.e. amoeba, flagellates, coccidia, ciliates, microsporidia and chromista), trematodes, cestodes, and nematodes. Moreover, the same species do not appear in Table 1, only E. dispar, without nuancing E. histolytica; neither do S. haematobium/mansoni appear. Also, rather surprising is the fact that neither in the Ms, nor in Table 1, the search for Giardia is mentioned as it is one of the most frequent protozoan parasites in low-income countries.

The PCR of the parasites is another relevant point. Yet, one thing is what the authors put, another thing is explain more about the technique in question.

With regard to the "study population" in the Results section, it would be more convenient to highlight the increase in the creatinine level, which is known to be generated from creatine; and which is known to be a waste product of the metabolism, being filtered by the kidneys in order to be eliminated through urine. Thus, measuring it in serum is indicative of the kidney function.
Perhaps, the question to be posed is: what does this parameter contribute in the context of intestinal parasitism? As several etiological agents (bacteria, viruses and parasites) are known to be able to alter this parameter.

Also, the fact that 60% of the 100 children studies presented with fever is relevant. The question becomes rather evident: were bacterial and virus infections ruled out? This point is of utmost importance in the context of the study carried out, as these etiological agents are usually present with fever.

With regard to "prevalence of intestinal parasites", the authors indicate that 81% of the children did not present parasites after the direct examination of the feces!!! This is rather surprising as there are no data concerning the microscopic vision of the direct examination. In fact, the eggs of Hymenolepis should be visible as the authors were certainly faced with H. nana which is transmitted by eggs, being quite normal in children below the age of five. Indeed, in the Discussion, they refer to H. nana as the most prevalent pathogen. Also, it is not clear why they mention this species in the Discussion. Moreover, at this level of prevalence of intestinal parasites, it would be important to get to know the total prevalence of each species detected as the results referring to parasite prevalences do not seem to coincide. Another thing is the detection of multiparasitism (= polyparasitism), which evidences the deficiencies of personal sanitary conditions as well as of the environmental conditions.

Concerning the "distribution of intestinal parasitic infections in relation to age and sex", it can be observed that children below age 4 are those most parasitized (52 children) vs older children (above age 4) who do practically not present any parasites (9 children). Also, it should be mentioned that the authors indicate that the prevalence of infectious parasites was higher in the older children. Frankly, this does not go together with the data presented in the tables.

Finally, concerning the conclusions, it is surprising that the authors conclude that STHs and intestinal protozoan parasite prevalences are very high in the children. This is rather surprising with regard to Trichuris, in which the result is not as relevant as the authors present it. Concerning the English language, there are several errors. The Ms should be revised by a native speaker of English.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
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Unable to assess

Are the conclusions drawn adequately supported by the data shown?
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