Author's response to reviews

Title: Nasopharyngeal carriage, serotype distribution and antimicrobial resistance of Streptococcus pneumoniae among children from Brazil before the introduction of the 10-valent conjugate vaccine

Authors:

FELIPE PG NEVES (fpgneves@vm.uff.br)
TATIANA CA PINTO (tati.micro@gmail.com)
MARIANE A CORRÊA (uff.mariane@yahoo.com.br)
ROBERTA A BARRETO (robertabarreto@id.uff.br)
LAIS SG MOREIRA (laisgouveiam@hotmail.com)
HAVANA G RODRIGUES (havanagomes@hotmail.com)
CLAUDETE A CARDOSO (claudete@huap.uff.br)
ROSANA R BARROS (miprosana@vm.uff.br)
LÚCIA M TEIXEIRA (lmt2@micro.ufrj.br)

Version: 3 Date: 21 June 2013

Author's response to reviews: see over
Cover letter
with answers to the Reviewers and description of the changes made

June 21, 2013

To the Editor in Chief

BMC Infectious Diseases

Dear Sir,

Enclosed, please find the revised version of the manuscript **MS: 3878626279032506** (“Nasopharyngeal carriage, serotype distribution and antimicrobial resistance of *Streptococcus pneumoniae* among children from Brazil before the introduction of the 10-valent conjugate vaccine”).

We have accommodated the suggestions of the reviewer #1, since the other reviewers did not have any other suggestions, considering the manuscript suitable for publication. The point-by-point responses are presented below.

We would like to thank the valuable comments and suggestions, which were helpful to improve the manuscript.

Sincerely,

FELIPE PIEDADE GONÇALVES NEVES

Dept Microbiologia e Parasitologia (MIP)
Instituto Biomédico
Universidade Federal Fluminense
Rua Prof. Hernani Melo, 101 - São Domingos, Niterói-RJ / Zip Code: 24210-130
Brazil
Tel: (5521) 2629-2440
E-mail: fpgneves@vm.uff.br
Reviewer's report:

Major compulsory revisions

Perhaps the authors do not understand my major comment # 2. Their revision about sample size and the saying about odds ratio being descriptive were unrelated to my comment. Despite my comment and explanation that, due to correlated data arising from their study design, their statistical inference (P-values and confidence intervals) was invalid, they continued to use the Chi-square test to provide misleading P-values (and unknown method to provide 95% confidence interval). I do not think such misleading information should be published. My previous suggestion was: "I would suggest the authors to remove all the p-values and confidence interval and provide a purely descriptive report of their findings from the two sites." Although the authors' reply letter said: "We agree with your comments on statistical analyses", they did not revise the manuscript accordingly. If the authors think there is a necessity to publish P-values and confidence intervals, they should find a method that does not make the assumption of independence between observations (which is clearly invalid in this study).

Response: We do agree with your comments on statistical analyses and, therefore, we remove all the p-values and confidence intervals and provide a purely descriptive report of our findings from the two settings.
Reviewer #2: Rosa Prato

Reviewer's report:
I'm pretty satisfied with the final version of this work. The authors have addressed my comments and the manuscript is significantly improved.
I have no other suggestions, and now I consider it suitable for publication.

Authors’ comment
Many thanks for your comments which really improved the manuscript.
Reviewer #3: Maria Cristina Brandileone

Reviewer's report:
I carefully read the manuscript after revised by the authors. All my comments regarding the information on the two study populations (day-care attendance and hospitalar) as well as information on the vaccination status of PCV10 were considered by the authors and were included in the text. The study design has been improved as well as the presentation of the results in the tables with statistical analysis. I have no further observations to be made and so I suggest publishing the article.

Authors’ comment
Many thanks for your comments which really improved the manuscript.