Author's response to reviews

Title: Health care utilization for acute illnesses in an urban setting with a refugee population in Nairobi, Kenya: a cross-sectional survey

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Version: 2 Date: 5 April 2014

Author's response to reviews: see over
Dear Dr. Arredondo:


Thank you very much for your email dated 28th February 2014 and for the comments and suggestions of the reviewers. We have been through the reviewers’ comments and suggestions and have revised our manuscript accordingly. We made every effort to address and incorporate all the comments of the reviewers into the manuscript and respond to others in the point by point response section below.

Please find for your kind consideration the following:

1) Point by point response to the comments and suggestions of the reviewers below.
2) Revised version of the manuscript.

We hope that these changes meet with your favourable consideration. In the meantime please do not hesitate to get in touch if you require any further information.

Yours sincerely,

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**Reviewer #1 Responses**

Reviewer comment #1: Eliminate the OR and CI in the text because they are already in the tables.

Authors’ response #1: Some OR and CI are in both the tables and the text while some OR and CI are only in the text and not in the tables. In consideration of the reviewers comment we have removed the OR and CI in the text that were repeated in the table.

Reviewer comment #2: Put some of the data in graphics in order to highlight the most important findings.

Authors’ response #2: We have included a pie graph of where study participants sought health care to highlight this finding. This can now be found as figure 2.

**Reviewer #2 Responses**

Reviewer comment #1: The manuscript focuses on the seeking of health services and related factors. Title could be modified to this topic.

Authors response #1: According to the BioMed Central website the following directions are given for title of any manuscript to be submitted, “Titles should be as concise and informative as possible with no unnecessary capitalization. They should include the type of intervention, the condition or type of participants or subjects, and the methodology. For example: "A versus B in the treatment of C: a randomized controlled trial" or "X in the aetiology of Y: a case control study". We feel that our use of the term “Health Care Utilization” is concise and informative and generally includes related factors that could affect health seeking behaviours. Changing the title of the manuscript to include “related factors that affect health care utilization” could be perceived as repetitive and not concise but we are willing to change it if reviewers believe it could be beneficial and not in violation of the BioMed Central directives.

Reviewer comment #2: Manuscript would be more interesting to readers if authors do a brief theoretical review on utilization of health services and related factors/determinant, both seeking and use of health services. Experiences form other countries with similar populations would be included in this. A good option is to use the model developed by Andersen (1995). “Revisiting the behavioural model and access to medical care: does it matter?”. J Health SocBehavi 36 (1): 1-10 or a more modern version of this model.

Authors comment #2: The recommended article is on models of health care use, however, in our study we did not follow any specific model of health use but services utilization and factors that influence it.

Reviewer comment #3: Why the study was conducted in the North of Eastleigh? Why in the region 2 of Eastleigh? According to the manuscript this region has local business, thus is it possible that households are few in this region?

Authors comment #3: This is a good point and we agree that this may be confusing to readers. We have changed the following line: “.....which is considered the business and cultural hub of the greater Eastleigh area” and modified it to read, “.....which has a large number of multiple
dwelling settlements and single-room occupancy units as well as a large number of small shops” to avoid confusion. Please see page 5, lines 122 to 123

Reviewer comment #4: Participants live in the area during three months or more during the last year, but the time to explore pneumonia was one year. Why these times are different? Is it possible that newcomers found health attention before arrive to Eastleigh (in other city or country)? If these facts occurred is not possible to relate this with local health system.

Author comment #4: We agree that it can be very challenging to correctly relate health seeking behavior to the local health system. In order to do this, to minimize recall errors, we asked about respiratory disease during the previous one year. This is a different time period from our inclusion criteria, which was, “an individual was considered a member of a selected household if he/she slept within a compound, apartment, or room within the study for at least 3 of any of the preceding 12 months.” It is not related to the illnesses we surveyed of the participants. We established this inclusion requirement due to the high mobility of this population as we described in the introduction section, “The urban refugee population is highly mobile and reluctant to come forward due to immigration laws and encampment policies in Kenya.” Also in the introduction we state, “The constant multidirectional movement between countries of origin, refugee camps, and the Eastleigh area poses risk of introduction and transmission of communicable diseases in the region and beyond the borders of Kenya.”

We describe the unique mobility of this population as it relates to public health concerns and therefore the residency requirement of the survey ensured that we would receive participation of this unique and mobile population and not limit them to an inclusion.

On the other hand we put duration of illness for pneumonia to be one year, this was because pneumonia is very serious but rare disease. Due to the seriousness of the illness it is easy to remember and due to its rarity we asked for people who had the disease in the past 12 months so as to capture good number of individuals who suffered from the illness.

We appreciate the challenges that might arise from this time differences and mentioned this as a limitation of the study in the limitations section.

Reviewer comment #5: Which are the definitions of standard cases of fever, diarrhea and ARI? Are they based on definitions of any medical society, Ministry of Health, or the World Health Organization?

Authors response #5: The case definitions in our study are loosely based on WHO definitions with modifications to suit community understanding.

Reviewer comment #6: Since the “seeking health services” was the dependent variable, is required to include a detailed description of methodology used to ascertain about it. For example, the construct “search services” implies that an individual was treated for services where he sought attention?

Authors response #6: Thank you for the question. To clarify the questions we asked in part four of the survey that aimed at health care utilization specifically, we added clarifying language in
the methods section on page 7 lines 171 to 173. Table 3 also outlines the information collected.

Reviewer comment #7: Authors report that conducted a multiple analysis, but they do not specify what type of analysis were used (which regressions?). In fact, these analyses were not presented in the results.

Authors response #7: We did logistic regression analysis and results are presented in the results section; on page 15, lines 338 – 343.

Reviewer comment #8: Authors did a stratified analysis according to country of origin and this decrease the statistical power. In this sense with a multiple regression it is possible to explore factors related with seeking health services. Inclusion of variables language and country origin as proxy variables of refugee status and to test interactions with demographic and health variables could help to explain use of health services.

Authors response #8: We have done this analysis as per the reviewer’s recommendations and included some more information in the results section. We re-looked in the country of origin of the father (looking at the father’s place of birth) and found it to be statistically significant and included this in the results section; page 15, lines 333 to 335. We have looked at household-language but this was not significant. Please also see table 4

Reviewer comment #9: Inclusion of a scheme or figure to explain different sample sizes used in the analyses could be a good option to understand the methodology. With different sample sizes in tables is very difficult to understand.

Authors response #9: A flow chart of sample sizes for each category is now included as figure 1, in order to address this concern.

Reviewer comment #10: Language is a proxy of migratory status; why it was not included in the analysis? I think that this variable could change results.

Authors response #10: We have looked at language but this was not significant. Since language may be of interest to readers, we have clearly stated this in the results. Please see page 15 line 342 to 343. Please also see table 4

Reviewer comment #11: It is not clear why the final sample was n=673. What did happen with the 785 participants? According with methods section when a household did not accept participate, caregiver was not in household or inclusion criteria were not fulfill, the household was replace. Is it correct?

Authors response #11: We had single replacement households that were randomly selected in advance and were used as substitutes for households that were not available for the survey, refused, or did not meet eligibility requirements as explained in the methods section in page 7 lines 160 to 163, however, in certain circumstances we encountered the same problems in the replacement household i.e. not available for the survey, refused, or did not meet eligibility requirements and that is why we did not reach the target of 785 households. Please see also figure 1.

Reviewer comment #12: What percentage of participants over 18 answered questionnaires
about themselves? Of 566 individuals with disease there were 434 respondents to find health 
services. Why 132 individuals were not interviewed?

Authors response #12: 1795 individuals which is about 60% of the participants were individuals 
above the age of 18 please see table 2 on page 22. Of the 566 individuals with illness 434 were 
interviewed this was because “If a household had multiple members with the syndromes of 
interest older than 5 years, one child(5-17 years) and one adult (≥ 18 years) were interviewed 
regarding their illness. If there was someone who died of the illness in the age group that person 
was selected, otherwise, the person most recently ill was chosen and If 2 people were ill at 
the same time, the older one was picked. All children <5 years whose illness fit the case definitions 
were included, regardless of the number in a household” please see page 8, lines 183 to 196. 
That is why 132 individuals with illness were not interviewed due to the formula explained 
above.

Reviewer comment #13: What is the meaning of "appropriate health services for each disease 
studied”? It was not defined in the methods section.

Authors response #13: When we used the statement "appropriate health services for each disease 
studied" we had in mind basic universal aspects of disease management for example if there was 
diarrhea we expected to see the use of oral rehydration solution (ORS) and stool analysis and if 
there was reported pneumonia we expected to see a chest X-ray done and antibiotic given etc. 
These were clearly explained in the survey tools. If the reviewer still wants to see this 
explanation in the text of the manuscript we are willing to do that.

Reviewer comment #14: Why variable age was grouped for analysis in younger and older than 
five years? International evidence suggests that factors associated with children’s utilization of 
health services are related with characteristics of caregiver or parent. Do you have this 
information?

Authors response #14: It is true that children’s utilization of health services are related with 
characteristics of caregiver or parent, however, considering the Kenyan situation where certain 
levels of health care providers charge cost sharing fees for people above the age of the five and 
all services are free for children under the age of five, this might critically affect health seeking 
behavior assuming that a parent might take a child under five years to the hospital because it is 
free but fail to take a child above five because he/she has to pay for that service. Also it is proven 
in many studies that the diseases we have looked at i.e. ARI, diarrhea and fever cause more 
morbidity and mortality in children under five years of age and it is the cut off age for many 
childhood interventions.

Reviewer comment #15: Table 3 the variable "overall" is unnecessary; really is the "n" of each 
category. Please remove it or change with the title "n" without "n" and "95%CI".

Authors response #15: We removed the variable “overall” and its corresponding “95%CI” in 
Table 3 and added “N” in the title

Reviewer comment #16: There are not data on multiple analyses indicated in methods section.

Authors response #16: We have presented this data in the results section. Please see page 15
Reviewer comment #17: Authors did emphasis in types of seeking health services, and forgot the opportunity of discuss on types of investigations and received treatments.

Authors response #17: This is a welcome point and a limitation of our study. We presented self-reported received treatments in the results section in pages 13 -14, lines 300 to 313 and in Table 3. However, we did not look critically in the quality of treatment and investigation services but rather availability of these services and that is why we did not delve deeper in the discussion section on the treatment and investigations. This is mentioned as a limitation in the discussion.

Reviewer comment #18: Manuscript needs a deeper discussion on causes of not-seeking of health services.

Authors response #18: We have appreciated this input and included some more discussions on the causes of not-seeking services. Please see page 18, lines 400 to 403.

Reviewer comment #19: In the manuscript is the sentence: “We found no difference regarding use of government facilities between the Kenyan and non-Kenyan residents”, but data related with this topic is not included in the results section.

Authors response #19: Thank you so much for highlighting this. We have removed this sentence to avoid confusion.