Author's response to reviews

Title: Emergency room (ER) visits of children under age one: effects of immigrant status and maternal education level. A cohort study in the province of Reggio Emilia (Italy)

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Author's response to reviews: see over
Dear Editor,

We prepared a new version of the manuscript in which we tried to take into account all the reviewers' comments. We are sincerely convinced that the paper has been improved by the reviewers' work and we want to thank them for their suggestions. Please note that all of the authors listed concur with the content of this paper and its submission to your Journal. The manuscript has been read and approved by all authors. The article is not under consideration for publication in any journal. Neither funding, whether past or future, nor any other relationship could lead to a conflict of interests.

Best regards

**Reviewer's report**

**Title:** Emergency room (ER) visits of children under age one: effects of immigrant status and maternal education level. A cohort study in the province of Reggio Emilia (Italy)

**Version:** 3  **Date:** 8 March 2013

**Reviewer:** claudia pileggi

**Reviewer's report:**

The objective of the study was to assess the differences in Emergency Room utilisation between Italian and immigrant children under age one and to analyze the combined effect of maternal educational level and immigrant status on ER utilization rate. Data were collected retrospectively by record linkage of Regional’s administrative health databases.

1) I think the paper lacks clarity. This is partly due to language problems, partly due to the structure and to the form of display of information. Also, the rationale behind investigating the associations between immigrant status, maternal education level and ER care is lacking.

RE: The new version of the text has been reviewed by an English mother-tongue editor. We have also tried to reframe the structure of the Results and Discussion. We agree with the reviewer that the introduction did not clearly present the rationale for studying ER utilisation in children and the differences between immigrants and Italians. See also answer to the other reviewer's first comment.

2) In the Method's section: I do not understand what Authors means when they talk about "triage color code...white.......the visits is probably not appropriate". What are the conditions that allow to consider appropriate an ER visits coded white?

RE: We have briefly explained the triage code in Italy, which adopts an international standard for ER access priority setting. We have added a reference for a more detailed description (pag 7, chapter “Outcomes and other variables”, 1st paragraph). G.U. n. 285 del 7/12/2001 Accordo tra il Ministro della salute, le regioni e le province autonome sul documento di linee-guida sul sistema di emergenza sanitaria concernente: “Triage intraospedaliero (valutazione gravità’all'ingresso) e chirurgia della mano e microchirurgia nel sistema dell'emergenza - urgenza sanitaria”. Roma; 2001.
3) In the Statistical analysis's section: I have never heard of the "principle of parsimony". Please, provide a more detailed description and, above all, adequate references.

RE: The principle of parsimony is the application of Occam's razor to any scientific problem. In particular, in the choice of a statistical model to interpret the relations between a dependent variable and some explanatory variables, we must set a threshold between complexity and simplicity. In this case, if two models give substantially the same results, the parsimony principle (i.e. Occam's razor) suggests adopting the model that uses fewer parameters. In this case we adopted a binomial model if the model was not different from a binomial model with zero inflated correction because the first requires fewer parameters to interpret the relations in our dataset. We rephrased the sentence in the Methods (pag 8, chapter “Statistical Analyses” 3rd par.).

4) The results section is overly repetitive for some results that are already included in the tables and difficult to read and interpret for the results that are not included in a table.

RE: the Results section has been reframed and shortened. In particular, we have tried to avoid repeating information presented in tables and text.

5) Finally, in the Discussion section's the Authors state that "Our study confirms a higher use of ER service..." indeed many previous studies have arrived at the same conclusions and then, what's the novelty of this research?

RE: While it is for the editor to decide whether the data and the analyses presented in this manuscript are worth publishing, we strongly believe that they are novel enough to merit so. The confirmation of previous results is as important as finding contradicting results, otherwise an enormous publication bias in observational studies would be justified and supported. Furthermore, our study is the first observing a higher access rate for immigrants than for non-immigrants in newborns and children under age one, confirming what other studies observed for other age classes. Many other studies could not distinguish the effect of higher morbidity and different behaviours in service utilization, and still other studies observed an opposite effect in selected causes of ER utilisation (injuries). Finally, the interaction between socioeconomic status and citizenship has been described by very few studies and never in newborns and children.

6) Would be necessary to reorganize the conclusion of the full text to include the implications for practice and implications for research.

RE: We have tried to summarise the implication for policy and further research in the Conclusions. Pag 14-15: “Based on this epidemiological evidence, it appears important to plan a public health intervention targeting foreign and low education level Italian mothers in the first days after delivery, as supported by international literature [38-39] and recent Italian experiences [40].”
Reviewer’s report
Title: Emergency room (ER) visits of children under age one: effects of immigrant status and maternal education level. A cohort study in the province of Reggio Emilia (Italy)
Version: 3 Date: 1 July 2013
Reviewer: Evelina Pappa

Reviewer’s report:

1. The aim of the study was to assess the factors that determine the utilization of E.R. of children under age one focusing on immigrant status and maternal educational level. The background for E.R. utilization is well-documented, but there are many issues which should be taken into consideration. Discussion and Conclusion are not adequately supported by the data.

RE: Discussion and Conclusions have been modified according to reviewers’ suggestions.

Major Compulsory Revisions
2. One basic issue concerns the theory of health care utilization which is the theoretical background of the present study. In my opinion, the theoretical background and the method are not sound. According to the theory of health services utilization research, it is considerably different whether the initial contact is studied during a giving period and whether the frequency of visits is studied. Therefore, two different things should be studied: first, the utilization of E.R. by assessing the characteristics of those using E.R. compared to those not used (zero visits) and second the frequency of use by assessing those having used E.R. once, compared to those having used it more frequently.

RE: We agree with the reviewer that first access is different from the subsequent access. This is particularly true for services in which fidelization is part of the mission of the service itself. In ER, the intention of the service is to avoid any recurrence, particularly if the access was not appropriate. Nevertheless, we tried to use models able to identify if and when the first utilization induced a different probability of using the service again. In fact, the negative binomial regression models and the zero inflated binomial models test the hypothesis that the events are overdispersed and that the group with zero accesses may be composed of different populations.

In the new version of the paper we explained more in detail the meaning of data overdispersion and the meaning of an excess of zeroes (see results, pag 9 chapter “ER Utilization” last sentence and pag 10 chapter “Analysis of urgency and appropriateness” 1st par. last sentence). In particular, a new paragraph in the Discussion focuses on the meaning of having a zero inflated component for white triage code accesses: pag 12, last paragraph.

We thank the reviewer for this suggestion; we are convinced that it has improved our paper.

3. Following the above, another comment concerns the statistical analysis that used to assess the utilization of E.R. If I understand well, you treat the utilization of E.R. as a count variable, which is an inappropriate method. The scope of your analysis was to assess the probability of use and the frequency of use investigating their predictors. Logistic regression analysis is the proper method
RE: We only partially agree with the reviewer. The rationale of the study, now more explicitly reported in the Introduction, is to identify those factors that lead to inappropriate utilization or preventable accesses so as to decrease them. The focus on immigrants is supported by two principal facts: 1) about one third of newborns in our area are from immigrant mothers; 2) the emergency medicine clinicians have noted an unexpectedly high proportion of immigrant children accessing ER. If the rationale of describing the phenomenon is to inform decision makers to plan policies that reduce ER workload by avoiding preventable accesses, it is essential to be able to distinguish between the mother who makes one access and the one who makes 5. Logistic regression is only able to model a yes or no dependent variable. Binomial regression can also model the number of accesses. Furthermore, the binomial zero inflated can model the number of accesses when the population who has zero accesses has two or more components. If our purpose is, as the reviewer states, “to assess the probability of use and the frequency of use investigating their predictors”, the logistic could only answer the first part of the question, while binomial regressions can also answer the second.


4. It would be more interesting to investigate the urgency of visits by addressing a multinomial logistic regression analysis by categorizing the dependent variable “urgency of visits” to three categories such as: white, red/yellow, hospitalization. This will allow to compare multiple groups through a combination of binary logistic regressions.

RE: We discussed in detail whether it was better to use a polytomous logistic regression or to make independent models. First of all the mutually exclusive classification of the dependent variable is only the colour code and not the presence of hospitalization. The possible polytomous logistic would have the following outcomes: red, yellow, green, or white. The first problem is that this is a scale of urgency in which the red, yellow, and green codes have a clear order, and urgency and short-term severity are often related. On the other hand, the white code has a slightly different meaning, i.e. inappropriate access, perhaps for a severe, but not urgent, disease. We used the three different outcomes to test different hypotheses, clearly stated *a priori*: is the higher utilization rate in immigrants due to higher morbidity or to a different approach to health services? If the morbidity is higher (acute and urgent morbidity), the risk of yellow and red accesses, as well as access followed by hospitalizations, will be higher in immigrants. The theory behind this is that for very severe cases the influences of behaviours is less relevant. If it is primarily a question of behaviour, the risk of white codes will be higher. The theory is that for inappropriate accesses the role of behaviours and cultural factors is definitely more relevant. We added a sentence in the discussion (pag 10-11).

5. Concerning table 5, it is not clear to me why geographical area is studied separately. Why the geographical area i.e. the country of origin of mother is not included as an explanatory variable in the statistical models?
RE: Table 5 has been dropped. The area-specific risks are now reported in Table 3 as suggested by the reviewer.

6. On p. 11, Discussion section, 1sr par. at the end, it is mentioned the age of children in months. Where is the variable in the statistical analysis? How did you come to this conclusion? Why is the age of children not included in the statistical analysis as a covariate?

RE: We included an analysis of the relative risks by age in months and citizenship. This is not included in the model since the relative risks are not biased (all children are followed for the same time, i.e. the first year of life, consequently the denominator for each month is exactly the same. The exclusion of pathological children also excludes any bias due to mortality during the period). The inclusion of a time-dependent variable in this model would make the computation very complex. We added a paragraph in the results: pag 10, first par.

7. On p. 13, Discussion section, 3rd par, it is mentioned that “for women on the same educational level, immigrants have higher risk compared to Italians”. How did you come to this conclusion? Statistical analysis did not show something like that. Can you be more specific?

RE: Figure 3 illustrates what we reported in the text: the utilization risks reported in the figure have been obtained from the models, i.e. the statistical analyses. We added confidence intervals to show the uncertainty associated with each risk: the test of interaction, testing that the two populations experience different risk ratios for socioeconomic status, is reported in figure legend and is highly significant. In the discussion the sentence has been changed (pag 13): “For women with the same education level, immigrants have a higher risk of visits compared to Italians, although this difference decreases at lower education levels.”

8. On p. 13, Conclusion section, 1st par., at the end, rewrite the two vulnerable population subgroups. Par example, “immigrants regardless of their education level” is very wide and does not consist the target group of your study.

RE: The Conclusions and Discussion have been completely rewritten and the sentence has been dropped.

9. On p. 14, Limits section, I think that the first limit does not apply to your study, because you have already used the classification of the educational level according to schooling years.

RE: We agree that it was misleading. Unfortunately, this source of misclassification is present in our study because we have estimated the years of schooling based on the school degree reported by mothers (pag. 14, 1st sentence).

10. On p.15, Conclusion section, 2nd par. at the beginning, it is mentioned the first month of age. The conclusion is not in accordance with the results. See also above the comment 5.
RE We produced relative risk and 95% confidence intervals for the month (first month vs 3rd - 11th months and second month vs 3rd - 11th months), now presented into results section (pag 10, first par.).