Author's response to reviews

Title: Use of different subjective health indicators to assess health inequalities in an urban immigrant population in north-western Italy. A cross-sectional study.

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

Thank you for giving us the opportunity to resubmit our manuscript (ID of the original submission 2135344745907190) for publication in your journal. The reviewers’ comments, which were insightful and justified, have all been addressed. The manuscript has once again been revised by a statistician (Dr. Alessio Signori) and a native English speaker (Dr. Bernard Patrick).

Below, you will find the reviewers’ reports with point-by-point replies and explanations of the revisions made. The reviewers’ comments are shown in *italics*.

**Francesco Vitale**

The paper is suitable for publication

**Domenica Rasulo**

The paper has improved to a great deal. I still have the following comments.

**Abstract**

No need to report odd ratios in the results section.

Odds ratios have been deleted, as suggested by the reviewer.

**Background**

The background should be tailored to the study purpose. The authors focus on how the health of migrants can change through time but this is not their study aim and cross-sectional data would not allow. As their aim is to analyse differences within the immigrant population, literature review should concentrate on this topic (e.g. similar studies have been carried out in Sweden and in the Netherlands). The authors should also quote their research questions based on the variables they are going to use. For example, how health is expected to differ across countries using the HDI classification which they adopt?

The Background section has been completely revised. In particular, statements on transitions of migrant
health over the time of residence have been removed, as advised by the reviewer. The literature review has been enriched by the inclusion of cross-sectional studies with similar outcomes conducted in different European settings (e.g. in Sweden, Germany, the Netherlands, the Czech Republic). The research question has been formulated. Disparities in health by country of birth have been conceptualized and some European studies on this issue have been cited. The, Background section therefore follows this line of reasoning:

– First paragraph: immigration patterns in Italy and Genoa;
– Second paragraph: factors affecting migrants’ health;
– Third paragraph: very brief description of migrant studies (underlining the higher prevalence of studies comparing migrants with natives, i.e. between-group comparisons) and possible health outcomes of migrant populations (essential to understanding different points of view on migrants’ health);
– Fourth paragraph: social and lifestyle health determinants (used as dependent variables in the present research);
– Fifth paragraph: concept of subjective health indicators (outcomes of the present study);
– Sixth paragraph: some examples of the use of subjective health indicators in European migrant studies;
– Seventh paragraph: concept of within- and between-group health inequalities: why immigrants differ from non-immigrants; distinctive features of migrants (migration itself, reasons for migration, different birth countries);
– Eighth paragraph: research question, aims and hypothesis.

At the end of the Background section, it is written “Analysing between-group disparities alone can hardly eliminate health disparities, as their causes differ among groups. On the basis of that assumption, we hypothesized the presence of such disparities within the immigrant population in Genoa.” What do they mean?

The reviewer is right. This sentence was not clear and has been rewritten.

**Socio-demographic characteristics**

The authors should distinguish between the variables which adjust for confounding effects and those which are study variables.

All socio-demographic characteristics have been considered as study variables (“well-known determinants” of health – this is described in the Background section) as the main goal of the present study was to determine the presence of “within-group” disparities in the health indicators used.

**Statistical analyses**
The authors mention that they run statistical tests but the purpose of these tests is not clear. For example, it is a bit generic to say “The chi-square test was used to compare categorical data”. Later test results are not described in the main text.

The specific use of all statistical tests has now been described in the Statistical analysis section.

The authors write “multivariate regression models ... were utilized to determine whether outcomes were associated with demographic and lifestyle variables. Those characteristics with a p-value lower than 0.15 on univariate analysis were considered in a multivariate model.” P-value is meant to be lower than 0.01. On the other hand, I wonder why in stepwise regression the significance level was set at 0.10.

In our study, as in most other studies, the level of significance was set to 0.05 and not to 0.01. This is clearly stated in the Statistical analysis section. In the stepwise regression models, the significance level used to exclude a variable from the multivariate model was set to 0.10 because a relatively small set of independent variables was investigated; moreover, we wished to highlight possible trends in some variables even if the differences did not reach statistical significance. However, only the variable of age-class in the model with poor SRH outcome resulted at the limit of threshold of significance (p=0.053); this result was deemed to be worth reporting.

At the end of the statistical analysis section, it is written “the independent variables considered in univariate regression models were: ...” All the models appear to be multi-regression models (see Table 5 and 6).

We agree with the reviewer. Tables 5 and 6 (now Tables 7 and 8) report results of only multivariate models. The sentence on univariate regression models in the Methods section could be unclear to the general reader and has been rewritten. In accordance with the reviewer’s previous comments, the results of univariate regression models are no longer reported. Moreover, reporting univariate regression seems to be somewhat repetitive, as Table 4 shows “percentages of subjects who reported poor or fair health status and median SF-12 scores according to the main variables considered”, which provides some idea of the univariate analysis. Moreover, in accordance with the classic statistical approach, the results of univariate analysis should be considered before multivariate analysis is undertaken. For this reason, we have decided to report only the results of multivariate analyses, as these are the more important.

Descriptive statistics

At the moment the reader has to look at the tables to get information on the frequency distribution of the variables. I suggest the authors to create tables which also incorporate results not included in tables but described in the main text. Then only the main patterns are described.
We have taken the reviewer’s advice and introduced a table “Self-rated health and median SF-12 scores reported by subjects” (see Table 3). The only frequency distribution presented in the main text is that of the percentages of the single morbidities reported. We do not feel that it would be appropriate to introduce a new table in this case, as this kind of data should not be difficult for the reader to follow.

There is no reference to Table 3 in the main text.

The reference to Table 3 (now Table 4) is to be found in the Results section, ninth sentence.

Table 3 indicates, through a subscript, whether a test was carried out. However, test results are not reported.

As suggested by the reviewer, we have reported the results of all statistical tests (z-value of Mann-Whitney test and chi-square for Kruskal-Wallis test [see Tables 4 and 5] and t value for Student’s test [see main text of Results section]).

For column two and three, I wonder what is the purpose of running a Mann-Whitney test which is not parametric (and so suitable for populations without a normal distribution) and then also adding in confidence intervals. These latter contradict the results of the Mann Whitney test (eg BMI) as the CIs of the categories overlap. This also applies to Table 4.

Non-parametric tests, i.e. Mann Whitney test and Kruskal-Wallis test, were used because the distribution of both SF-12 (PCS-12 and MCS-12) scores was not normal, being negatively asymmetric. This has been clearly stated in Statistical analysis section. Moreover, the skewness coefficients of both SF-12 scales have been reported in the Results section. The tables do not contain confidence intervals but Interquartile Ranges (IQRs). This is clearly reported in both Statistical analysis and the Table headings.

Figure 1 is mainly a scattergram – can the authors add instead the coefficient of correlations in one of the previous tables?

As advised by the reviewer, Figure 1 has been removed and a table on coefficients of correlation with p-values has been added (see table 6).

Regression analysis

This is one of the main sections of the study and is very short. Results should be described in more details – comparatively, the abstract reports more details on regression. Some odds ratios are later reported in Discussion.
Regression analyses have been reported in greater detail, as advised by the reviewer. No odds ratios from Results have been repeated in the Discussion section. An odds ratio from a study conducted by Dunn and Dick (see ref. 57) is reported.

Discussion

Discussion is hard to follow – would suggest to add subparagraphs based on the study variables.

The reviewer is right. The Discussion section has been revised and split into subparagraphs.

I wonder whether the relation with “religious reasons” is spurious. What does exactly mean to migrate to Genoa for religious reasons? On the other hand, conclusions on the effect of religious involvement on health can follow from questions on the frequency of religious practices which hasn’t been asked in the questionnaire. For the same reason, at the top of the third page of the Discussion, I wouldn’t say “religious immigrants” as compared to those who migrated for family reasons.

We agree with the reviewer that the association between religious reasons for migration and health outcomes could be spurious. We have added a comment on the possibility of a spurious relationship and inserted an appropriate reference in the Discussion section. Little is known about the complex question of the possible association between religion and health. Indeed, research into this relationship has often yielded contradictory results; some of these studies have been summarized in the Discussion section. Immigrants for religious reasons were defined as those whose first residence permit was issued for religious reasons; this has been stated in the Socio-demographic characteristics section. We agree that our questionnaire did not contain any question on the frequency of religious practices; however, these subjects (n=20) were recruited at parish centers (this has been mentioned in the Data collection section) by means of two initial seeds, who were members of a religious association, a feature which is indicative of religious involvement. All comparisons among migrants for different reasons have been removed, as advised.

On the third page of the Discussion, it is written “Our results showed that being overweight or obese is a risk factor for poor health reporting as well as for lower scores of physical HRQoL, even without comorbidity conditions”. Are the authors referring to a model which didn’t adjust for Medical conditions? Table 5 reports both the variables (BMI and Medical conditions) – should another model be added in?

We agree with the reviewer. This conclusion was not exact; the phrase “even without comorbidity conditions” has been deleted.

Conclusions
Conclusions mention differences between migrants and natives while these were not investigated.

As suggested, the sentence on differences between immigrants and Italians has been deleted.

Thank you for your kind attention.

Yours faithfully,

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