PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Migration status and prevalence of diabetes and hypertension in Gauteng province, South Africa: effect modification by demographic and socio-economic characteristics-A cross-sectional population-based study
AUTHORS	Motlhale, Melitah; Ncayiyana, Jabulani

VERSION 1 – REVIEW

REVIEWER	Hans-Friedemann Kinkel Charité - Universitätsmedizin Berlin, Institute of Tropical Medicine and International Health, Germany and University of Pretoria, Dept. of Family Medicine, South Africa
REVIEW RETURNED	29-Jan-2019

GENERAL COMMENTS	I fully understand the importance of migration as a risk factor for health in many aspects and I appreciate the authors attempts to explore an existing data base (SANHANES) in that regard. However, to my opinion the paper does not fulfill the STROBE criteria in many ways. I only comment on a most critical issues here. Imprecise and unclear title. Grammatically not correct. It is not clear why the study was conducted. Did the authors want to find out whether migration is a risk (or protection) factor for diabetes mellitus (d. mell.) or arterial hypertension (art. ht)? Then a multivariate analysis should have been performed with d. mell (art. Ht.) as dependent variable and “migration status” together with age and all the other factors as independent variables. The authors have chosen a different Approach though: A comparison of population subgroups by migration status is what the authors did. They found that the prevalence of d. mell. and art. ht. is lower in the “migrant” groups. That this is due to the differences between the groups is banal to my opinion and does not need further exploration or proven by analysis. If the table 1 would have been presented the percentages “vertically” (the strata per variable in each population group summing up to 100%) and not horizontally (each stratum across the population groups summing up to 100%), this would have become easily detectable and a simple Chi squared test could have been used to confirm the differences. So why did the authors analyse the data in the way they did is? This leads to the next fundamental critique, that is the justification of this

	particular study. It is not clear to me why the authors did this particular study and why the study would be of importance, respectively. Certainly not to tell us that there are differences between migrants and residents in the prevalence of d. mell and art. ht. It does not come at a surprise at all, that in younger population groups the prevalence of chronic disease is lower. So what did the authors have in mind? Unfortunately also the introduction in which underlying concepts should be introduced and the justication should be provided, does not give further clues. Just stating that migration is a risk for health is superficial and inadequate for an academic paper. The statement that "little is known" is inacceptable as justification to my opinion, if we don't get an idea of what the authors would like to know and why. What are the authors hypotheses about migration (short/long time ago, once/multiple times moved, at what age moved?) and chronic non-communicable diseases (incidence? control/treatment?). The authors presume age, race and SES as effect modifiers but don't explain what the nature of the effect might be. And why not other categories? The lack of substantiation of the study also becomes evident in the recommendations and the conclusions, which leave the reader with phrases like: "migrant status is associated with two non-communicable diseases prevalent in Gauteng province" or "The study contributes to the pool of knowledge". The reader wants to know how the factors are associated with migration and what knowledge does this study add. Further fundamental issues: the categories of the anyway controversial variable "race" are very specific for South Africa and non-South Africans (particularly "external migrants") typically do not or inconsistently use those categories to describe themselves. There is also an issue with the stratification of the year when a person migrated. Several categories e.g. "moved before 1985" don't apply for certain age groups. That questions also the concept of "migration" the authors used. How would "migration" affect a 20 year old regarding D. mell. or Art. ht. who moved as a 5 year old? Or a 75 year old already with D.mell, who moved 2 years ago? Another issue: regarding the household income. In order to estimate the SES the PER CAPITA income would have been more appropriate to my opinion than the crude household income. The discussion deals with aspects that are not related to the results (e.g. increases in prevalence) I am very sorry.

REVIEWER	Barbara Rogers MD The Ohio State Wexner Medical Center
REVIEW RETURNED	17-Apr-2019

GENERAL COMMENTS	very interesting

REVIEWER	Per Wändell Karolinska Institutet Sweden
REVIEW RETURNED	16-May-2019

GENERAL COMMENTS	Remarks 1. In ref. 2 the initial of the first author is wrong (should be J

	Westman and not E Westman). Please check the correctness of the reference articles. The question is how valid the cited study is for the situation in South Africa, this can certainly be questioned. There are other studies on the effect by migration on health status. 2. The authors could mention the prevalence of diabetes and hypertension in the world, there are studies in the Lancet (diabetes 2016, hypertension 2017). Besides, the review article by Misra and Ganda from 2007 is essential in a study such as the present. 3. The sampling is described, though a little hard to understand. Non-participation rate? Representativeness? 4. Percentages in Table 1 shown for the variables; it would be more informative to show the frequencies for each group (non-migrants, internal migrants and external migrants). 5. In Table 3 it would be helpful if the Models were explained better, i.e. which separate factors are included in the three Models. 6. The Figures are hard to understand, which colour denotes which group? Now we can see that there are differences between the groups, but not more than that. 7. Although the results are interesting, they must be discussed in a broader perspective. Results from European countries show that some groups have a higher risk, others a lower risk, for hypertension and diabetes, respectively. How can we understand the situation in the Guateng province in South Africa? What is the reason for migration? Could the healthy migrant effect be of importance? The authors have mentioned the possibility of under-reporting, as data on hypertension and diabetes are self-reported. There are data on the validity of self-reported hypertension or diabetes, with data on diabetes showing higher validity. The results in these studies may not be valid for the South African context, but the authors should discuss this. 8. Another factor of importance is the diabetes prevalence in the countries of origin. What is known, what could be expected in migrants from these countries?

VERSION 1 – AUTHOR RESPONSE

REVIEWER #1

The following however need attention:

• I fully understand the importance of migration as a risk factor for health in many aspects and I appreciate the authors' attempts to explore an existing database (SANHANES) in that regard.

We appreciate the reviewers comment on the dataset used. The dataset used in this study is the fourth Quality of Life (QoL) Survey conducted by Gauteng City Region Observatory (GCRO) in Gauteng province in 2015 not the SANHANES dataset. We cited SANHANES study in our background, we think that is where the confusion arise.

• Imprecise and unclear title. Grammatically not correct.

The reviewer's comment is greatly appreciated but the title captures what the study is all about. The study design has been included in the title.

• It is not clear why the study was conducted.

We appreciate the reviewer's comment. We wanted to understand the differences in morbidities according to different migration status in Gauteng province. This can be used in the long-run to inform policy and to target high risk groups in provision of services and to arrest the growing burden of certain diseases. Gauteng province is the economic hub of the South Africa, therefore most migrants

(both internal and external) are drawn to this province and we have stated this point in the methods section.

- If the table 1 would have been presented the percentages “vertically” (the strata per variable in each population group summing up to 100%) and not horizontally (each stratum across the population groups summing up to 100%).

We appreciate the reviewers comment on table 1. We presented the percentages “vertically” (the strata per variable in each population group summing up to 100%). Chi square results were not presented, as the table 1 is already too busy.

- Further fundamental issues the categories of the anyway controversial variable “race” are very specific for South Africa and non-South Africans (particularly “external migrants”) typically do not or inconsistently use those categories to describe themselves.

We appreciate the reviewers comment on age as a categorical variable. In the primary study even external migrants had to identify themselves according to the given categories; African, Coloured, Indian /Asian, white and others. So for the secondary data analysis the categories were used as they were.

- Several categories e.g. “moved before 1985” don’t apply for certain age groups. That questions also the concept of “migration” the authors used. How would “migration” affect a 20-year-old regarding D. mell. or Art. ht. who moved as a 5 year old? Or a 75 year old already with D.mell, who moved 2 years ago?

The categorical variable year moved to Gauteng province, the effect of different age groups was taken into consideration by adjusting for age in the models.

- Another issue: regarding the household income. In order to estimate the SES the PER CAPITA income would have been more appropriate to my opinion than the crude household income. Unfortunately, per capita income was not collected during the survey (primary study).
- The authors presume age, race and SES as effect modifiers but don’t explain what the nature of the effect might be. And why not other categories?

From the tested variables age, race and SES were found to be effect modifiers. The association between migration status and diabetes and hypertension differs depending on any of the above-mentioned variables.

REVIEWER #2

- Very interesting.

We appreciate reviewer’s comment. We wish more helpful comments could have been provided.

REVIEWER #3

- In ref. 2 the initial of the first author is wrong (should be J Westman and not E Westman). Please check the correctness of the reference articles. The question is how valid the cited study is for the situation in South Africa, this can certainly be questioned. There are other studies on the effect by migration on health status.

We edited Reference 1; all references were edited and checked for correctness.

- The authors could mention the prevalence of diabetes and hypertension in the world, there are studies in the Lancet (diabetes 2016, hypertension 2017). Besides, the review article by Misra and Ganda from 2007 is essential in a study such as the present.

The global prevalence of diabetes and hypertension were included in the introduction.

- The sampling is described, though a little hard to understand. Non-participation rate? Representativeness?

The dataset is fairly representative of the study population. This was a secondary data analysis. The non-participation rate was not discussed in the full report. The initial sample size was 30 000 and

28,456 successful interviews were completed (95%). The interviews were distributed across 16,400 SALs out of a total of 17,840 SALs (92%).

- Percentages in Table 1 shown for the variables; it would be more informative to show the frequencies for each group (non-migrants, internal migrants and external migrants).

We appreciate the reviewers comment on table 1. We presented the column percentages as also suggested by Reviewer #1.

- In Table 3 it would be helpful if the Models were explained better, i.e. which separate factors are included in the three Models.

The models were explained in text and there was a note just below table 3. Four progressively adjusted multilevel models were fitted: model 0 with no covariates; model 1 including only sociodemographic characteristics at the individual level; model 2 additionally analysing municipal deprivation as contextual variable and model 3 is the full adjusted model. The models were adjusted for years in GP, age, sex, race, dwelling, education level, household size, household head, physical activity, medical aid, grow own vegetables, household food security, sampi, year moved to GP and socioeconomic status quintile.

- The Figures are hard to understand, which color denotes which group? Now we can see that there are differences between the groups, but not more than that.

Figures are showing the OR plots the variables modifying the association between migration status and hypertension and diabetes. Different colours denoting different categories in each variable. The figures were edited to indicate what each colour denotes.

- Although the results are interesting, they must be discussed in a broader perspective. Results from European countries show that some groups have a higher risk, others a lower risk, for hypertension and diabetes, respectively. How can we understand the situation in the Gauteng province in South Africa? What is the reason for migration? Could the healthy migrant effect be of importance? The authors have mentioned the possibility of under-reporting, as data on hypertension and diabetes are self-reported. There are data on the validity of self-reported hypertension or diabetes, with data on diabetes showing higher validity. The results in these studies may not be valid for the South African context, but the authors should discuss this.

The study indicates that migration status is associated with prevalence hypertension and diabetes conditions. Internal and external migrants had lower odds of both hypertension and diabetes than people born in Gauteng province. Age, race and SES of the respondents were significant effect modifiers of the association between migration status and morbidities

Low prevalence may be due to Healthy migration effect. Validity of self-reported morbidities may also be a contributing factor. Results on validity were from these studies may not be valid for the South African context. This calls for more research on migration status and morbidities, as well as validity studies of self-reported morbidities in the South African setting

- Another factor of importance is the diabetes prevalence in the countries of origin. What is known, what could be expected in migrants from these countries?

We really appreciate the comment on prevalence in the countries of origin. For this study there were 7,819 internal migrants from 8 different provinces in South Africa and 2,161 external migrants from 68 countries, it would be difficult to factor in the prevalence of diabetes and hypertension in the countries of origin.

VERSION 2 – REVIEW

REVIEWER	Per Wändell Karolinska Institutet Sweden
REVIEW RETURNED	27-Jun-2019

GENERAL COMMENTS	The authors have revised the manuscript in a satisfactory way.