Author's response to reviews

Title: Evidence for the healthy immigrant effect for older Chinese immigrants: a cross-sectional study

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Author's response to reviews: see over
Editor Comments

(1) Abstract: Please include the context info of your study in the Background section of the Abstract. Please update both the abstract in the manuscript file and in the submission system.

We have added the following sentence to the Background section of the abstract: “Previous work has found that first-generation immigrants to developed nations tend to have better health than individuals born in the host country.”

Reviewer - Victor Chang

Discretionary revisions

1) The CAFEH study examines 3 neighborhoods that abut I-93. It is implied, but not explicitly stated that the Chinese population studied comes from Boston Chinatown. This should be clarified in the methods section as one might conclude that responses from Chinese residents in all 3 neighborhoods.

The following sentence was added to the methods to clarify in which communities the participants resided: “The Chinese immigrants in our sample lived primarily in Chinatown (n = 119) or Malden (n = 26) while the U.S. born whites lived primarily in Somerville (n = 78), Dorchester (n = 64), or Malden (n = 20)” (page 4-5, lines 99-101).

2) Asthma is a childhood disease. It may not be as highly prevalent in adults, although this is an air pollution study and rates may change over time.

Although the majority of incident asthma cases are seen among children and some children do grow out of asthma, the latest BRFSS data from the CDC indicates that 8.9 percent of adults in the U.S. and 10.8 percent of adults in Massachusetts have current asthma [1]. Additionally, the annual incidence of asthma among adults in the U.S. is estimated to be 3.8/1000 [2]. The risk of developing asthma as a child and as an adult were added to the discussion on page 10, line 234.

3) Can a claim be made about changes over time? Twenty years is not a long time. However, staying in an enclave could have a protective effect as well.

In the limitations, we added a note that we do not know about individuals’ health status changes over time (page 12, lines 284-285). We also added a sentence about the mean length of time participants had resided in the U.S. to the limitations (page 12, lines 285-288).

4) Some of the references come from Canadian studies. The authors may find this reference helpful.


Thanks for the helpful suggestion. We have added a comment to the discussion (page 9, lines 203-204) and cited this book.
5) Another potential shortcoming is the lack of acculturation measures and socioeconomic status indicators in the original study design, which was for air pollution exposure. Measures of social stress might be a proxy variable for this.

Socioeconomic status was included in the analysis. Income and education were reported in Table 1. Education was included in the fully adjusted model for asthma. SES was not significantly associated with CVD after adjustment for other confounding variables. While we also analyzed the influence of perceived stress, the lack of acculturation measures was noted in the limitations section (page 12, lines 286-288).

Minor essential revisions

1) Spelling: Engrained or ingrained

Thank you for catching this error. We have fixed it on page 3, line 61 and on page 9, line 201.

2) Tables: In Table 1, p values have been entered for some of the 95% confidence intervals

95% confidence intervals have been substituted for p values.

Reviewer - Gordon Fung

1. Is the question posed by the authors well defined? Yes. The question was well formulated and the research design was appropriate to answer the question.

Thank you.

2. Are the methods appropriate and well described? The actual detailed description of the methods and study population was referred to in 2 other articles and not summarized in this article. As I was unable to review those articles, there were questions about the specific results and criteria used to conclude.

Additional details have been added to the methods, with particular attention to the comments below.

a. The first question was what period of time was the study? This is important since in 2010, the “Asian” population by the CDC were the first population to have a change in the leading cause of death from cardiovascular disease to CA. So that if the definition of healthy was the absence of disease, was adequate CA screening performed to determine if the most common types of CA were absent.

The time period for data collection was added (page 5, line 108).

b. The second question was the actual definition or criteria used to determine “healthy”. The conclusion of “healthy older Chinese” had less asthma and CVD needs to compared to the actual criteria used. The American Heart Association spent 2 years developing the criteria of optimum, intermediate, and low cardiovascular health based on 7 criteria. Of note only 3 of 1500 people in a particular study achieved optimum cardiovascular health with all 7 criteria met. None of the 7 included asthma or stress or actual cholesterol levels or inflammatory markers, so it would be important to know what the actual definition of healthy was or criteria were.
More specific language was used to clarify what was meant by “healthy” throughout the manuscript. From the AHA criteria, the presence of seven health factors and behaviors contribute to optimum cardiovascular health. We explicitly address five of these seven criteria (blood pressure, physical activity, healthy diet, healthy weight, and smoking status). We considered HDL and LDL cholesterol instead of total cholesterol. We did not address the seventh, blood glucose, although we did consider diabetes [3].

c. Another question that I had was the location sites chosen for the study. It has been shown that being located close to the freeway with elevated environmental pollution is associated with higher incidence of cardiac events and CVD. Although it would affect both the Chinese and non-Hispanic white population equally, it is not clear if the entire study population was at higher risk than the average population of the city that included people not proximal to freeways. The proximity strata were specifically noted (page 4, lines 79-80).

d. Another question that I had was the diversity of the population of these communities. I saw in the references that the MESA trial was used as a reference. Were there other ethnic populations that could be included in the data analysis? I question this for two reasons: one is that the majority population of non-Hispanic whites is already no longer the majority population in other areas of the US so using the white population as the reference population may not be meaningful. Secondly, elderly Chinese may not be as willing to be as active walking in the community in very diverse locations. (that is certainly the sense I get from casual surveys in my clinical practice).

While we agree that it would be helpful to look at other ethnic populations, in the CAFEH study, the sample sizes of other sub-populations were too small to analyze. This has been noted in the methods section (page 4, lines 92-93) and in the limitations section (page 12, lines 281-282). Additionally, the sample sizes of the largest other racial/ethnic groups were included in the methods (page 4, lines 94-96).

e. Although there was no comparison of obesity or excess BMI, I wondered what classification of BMI was used? WHO uses a separate criteria for obesity for the East Asian population with the category of obese being > 27 and overweight being 24-27 BMI as opposed to the Framingham criteria with obese > 30 and overweight > 25 – 29.9. This would certainly play a factor in the “health” of the Chinese compared to the non-Hispanic white. BMI was analyzed as a continuous variable due to the differences in classification schemes between ethnicities and the potentially differential effect low BMI has on Asian individuals’ health was noted on page 11, lines 252-255. Additionally, if the WHO classification scheme for East Asians had been used, only 20 Chinese participants initially classified as normal weight would be re-classified as either overweight or obese. This has been noted in the manuscript (page 11, lines 255-258).

f. Another question was whether the stress study instruments were all validated or if the participants were just asked if they were under stress or on tranquilizers? Also, if depression screening was performed by qualified personnel? Since mental health is poorly understood and admitted to in the Chinese population due to the stigma, actually detecting stress and depression would be a significant challenge. From informal conversations that I have had with my local...
mental health specialists in my area, I have been told that there are no validated depression or stress survey instruments for the Chinese to date (albeit a few years ago).

The perceived stress scale is a validated scale and our group has previously validated the scale among our study population. This has been noted in the methods (page 5, lines 111-112). We did not specifically address depression since there are not validated measures for our study population. Nevertheless, the potential for inadequate assessment of participants’ mental health was noted in the limitation section.

g. It was mentioned in the article in the limitations that which part of the China – even rural vs metropolitan – the participants came from was not gathered, so unknown. This is particularly important to put the conclusion into perspective for the more recent immigrants since the pollution of China – especially in the metropolitan areas has been reported as among the worst in the world. One wonders whether the cleaner air of Boston could affect the rate of asthma or asthma exacerbations.

The fact that we do not know whether participants were from rural or urban areas has been noted in the limitations (pages 12-13, lines 289-291).

3. Are the data sound? This was a small population study with a limited number of participants as noted in the limitations portion of the study. So this affects the data. Also, all diseases are self-reported. This is particularly challenging for patients who might have heart failure and angina. It has been said that the Chinese are a model minority since they don’t complain of symptoms and mainly try to compensate for their exertional symptoms by decreasing their activity especially for angina and heart failure. So the actual detection of disease by symptoms may be significantly underestimated. Also, because of this, the actual prevalence of diabetes may be underreported as the prevalence of reportable symptoms are less and recently reported the actual A1c criteria should be modified to be set at > 5.8 for the diagnosis of diabetes in Asians. All the other measured parameters are fine as there have been no literature to designate any ethnic disparities with those measurements.

This important point has been added to the limitations section (page 12, lines 276-278).

4. The manuscript does adhere to relevant standards for reporting and data deposition. I could not interpret or understand the findings in figure 2 even with the interpretation in the results section of the manuscript.

Figure 2 has been modified for clarity and additional explanation has been added to the results section (page 11, lines 249-252).

5. The discussion and conclusions do not include any of the points of known ethnic disparities reported above. This is important to include to put the findings into perspective and be able to apply it to other populations of Chinese across the US. Otherwise the data do support the findings.

The fact that these ethnic disparities could limit the interpretation of our results has been added to the limitations section (page 12, lines 279-280).
6. The limitations are clearly stated in the discussion section of the manuscript.
7. The authors also clearly acknowledge previous published work in the area of study. There was no mention of unpublished work.

We did not use unpublished work in our manuscript except for one abstract presented at a professional conference. The language has been changed throughout the paper to reflect this.

8. The title mainly describes the type of study and the study question not the findings. The abstract does accurately convey the findings.

The title has been revised to: “Evidence for the healthy immigrant effect for older Chinese immigrants: a cross-sectional study.”

9. The writing is acceptable.
10. There are no major compulsory revisions for this paper.
11. I consider my questions and comments in the discretionary revisions category that would be helpful to readers to put the findings into better perspective.

Thank you for the helpful feedback.

12. I would recommend reconfiguring figure 2 or explaining it better.

As noted above, Figure 2 has been modified and additional text has been added to clarify the figure.

References

1. CDC - Asthma - BRFSS 2012 - Table C1 Adult Self-Reported Current Asthma Prevalence Rate (Percent) and Prevalence (Number) by State or Territory [http://www.cdc.gov/asthma/brfss/2012/tableC1.htm]
