Reviewer's report:

In this paper, Zhang and colleagues report the prevalence of cardiac structural and functional abnormalities in a middle-aged adult Chinese population. The data are important and the study is interesting. The authors are to be commended for a carefully conducted investigation and a well-written manuscript. However, there are some important points that need either additional analysis or further clarification.

Major Compulsory Revisions:

1. Definitions: Please provide the definitions used for alcohol intake and current smoking. What level of drinking was considered as the threshold to categorize a patient as a drinker? In fact, "intake" implies a quantitative variable (e.g. drinks per week). If you have this information, please do provide these data in the paper; otherwise, it might be more accurate to label this variable as "drinkers". For smoking, did you differentiate between current and past smokers – and based on what criteria? I understand that there are important cultural differences between Western and Chinese populations; still, the difference in alcohol use and smoking between women and men in the study is striking.

2. Analysis:

   a. Why was the z test used to compare continuous variables between groups? The z test implies known standard deviations or very large populations. The t test would be more appropriate in this case.

   b. Why were both weight and height forced into prediction models? Body mass index or body surface area alone - the latter is especially suitable for echocardiographic variables - would work better.

   c. Treating no-mild-moderate-severe diastolic dysfunction as an ordinal variable for multinomial logistic regression purposes is OK. However, this is not the case with concentric remodeling vs. eccentric hypertrophy vs. concentric hypertrophy. That is, the levels of this variable do not have an ordinal relationship. I would recommend performing multinomial nominal (not ordinal) logistic regression for this variable.

3. Results: The lack of data on the prevalence of left ventricular systolic dysfunction (LVSD) is a glaring omission. Although reduced left ventricular
ejection fraction (LVEF) is a rough surrogate of LVSD, and the cutoff point will be necessarily somewhat arbitrary, it is still a key precursor of heart failure and a major target of population screening efforts. Therefore, I urge the authors to categorize LVEF using a couple of different cut-off points at e.g. 40%, 45%, or 50% to provide the readers with estimates of LVSD prevalence in the study population.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests