



SDI FINAL EVALUATION FORM 1.1

PART 1:

Journal Name:	International Journal of TROPICAL DISEASE & Health
Manuscript Number:	2014_IJTDH_9362
Title of the Manuscript:	The study of social profile and some health problems associated with body weight disorders in upper Egypt male youth

PART 2:

FINAL EVALUATOR'S comments on revised paper (if any)	Authors' response to final evaluator's comments
The authors have substantially improved their manuscript and most of the issues have been clarified. Thank you for this effort. I still find the justification of the sample size calculation rather weak. A clear sign of problems with sample size design are the extremely few cases of reported DM and hypertension. Essentially this is due to the much lower prevalence of DM/hypertension in the target age group as compared to obesity/overweight which the authors used to calculate their sample size. In consequence, the small sample size lowers the “value” of Table 2 substantially. Hence, the authors may want to consider to drop DM/hypertension entirely and concentrate on the “social factors” of obesity/overweight. Technically, the issues with Table 1 have been explained. However, the purpose and rationale for presenting Table 1 in this way are, in my opinion, quite unusual. To document the contribution of individual states to overall prevalence of obesity/overweight is one thing. Unfortunately, this approach does not help to understand why within states prevalence (in the sample) are substantially different and hence contribute to overall prevalence with weights which are in some cases substantially different from what can be expected from their respective shares of the total sample. In my opinion, the paper could further be improved by addressing the two issues mentioned above. However, these are minor issues.	Dear respected reviewer, With respect to your emphasis on critiquing the presence of small number of diabetics and hypertensives who happened to be obese, etc., this is not our fault, nor is it the obese subject’s fault. This is simply the epidemiology of DM and HTN among young-age overweight/ obese individuals in Upper Egypt. In fact, this is one of the secondary aims of the research. We need to identify the prevalence of these ailments in association with obesity in young male workforce in this community. Why cannot we do that? This is an already young-aged population stratum. We do not expect 18-30y-old to immediately have DM and HTN in high rates once they start experiencing obesity or overweight. It takes years and years to have these complications. And this is what we wanted to know. Further, increasing the sample size, yet keep being confined to such young age stratum will not change anything. The proportion of DM/HTN with obesity will stay the same, won’t they? Suppose the sample size was 10,000, not 500, a more or less same proportions of diabetics among the overweight/obese, which is 2-3% will be obtained, as long as the source population is the same, sampling technique is ok, representative is assured, probability sample, etc. You will unlikely get anything higher than that in this very population. Is not this the spirit of the 95% CI? If the same population is sampled 100 times, in only 5 times the same effect estimate will happen by chance? On my part, I spoke with my co-authors, showed them this very critique of yours, and your suggestion to cancel DM and HTN from the research. The all screamed out this means that we have to give up one-third of this research’s aim. The research is principally about social class and obesity in this specific age group (Egyptians, overweigh/obese, youth, Upper Egypt, workforce); and secondarily about the rates of DM and HTN among them. No one agreed to drop this aim. Expectedly, not only us, but also the local public health authority and decision makers were so interested in seeing such updating data about our young obese, what risks they are exposed to, what chronic diseases, etc. This is a genuine part of the research. And again, increasing the sample size won’t change this trend. And as I said before, we only calculate n according to expected prevalence of obesity in the youth, not according to DM among young obese. And by the way, even trying to do so against the general rule of the need to have a condition the rates if which is >20% in order to use sample size formulas or a proportion (using computation by hand or by Epi-info), you will not get a sample size more than 75-85: Say the rate of DM in obese young patients is even 5%, so, $n = Z^2 (P) (1-P) / e^2 = 3.84(0.05)(0.95) / (0.05)^2 = 73 \text{ individuals, instead of 500!}$ Is this what we want to do? 75 subjects? Now, if we still insist to make our hypothesis that “the odds of DM comorbidity from the paper, among diabetics is significantly higher than the odds of no DM among non obese,” we simply need to find another population to who this hypothesis applies, e.g., people who are 41-60. I came up with a compromise; actually an extension research to this work, and that this time I’ll be the PI from the start to be in better control of everything: we will follow our study cohort for 5-10 years, and see what will happen to them, either those who will sustain having obesity, regarding DM/HTN, and probably some other chronic health consequences. This is going to be one of our recommendations for future research. And this extension will answer your concerns and may resolve the whole issue. I really appreciate your extremely fair judgment, describing the remaining point as a minor issue, looking to the article from the broader view. I’d like to reassure you that I am keen to minimize the likelihood of the study



SDI FINAL EVALUATION FORM 1.1

	limitations, confounding, etc., to least possible, but this is not always feasible, looking to other obligations researchers often have to deal with, such as the prevailing epidemiologic profile, the time frame, the stage and nature of the study project, requirement for certain local data, etc. Thank you a lot again for the high quality, highly professional critique you have brought and meticulous work and the time and you have devoted in this endeavour. I understand so well everything you have addressed in this article. I really enjoyed the scientific debate we have been through, for the sake of quality and benefit of the scientific community. Having low prevalence rates of DM and hypertension (3.2% and 6.5%, respectively) in the high BMI group in this study may well be explain by the fact that our study population is already young (18-30), and time may still be needed for them to advance to diabetes or hypertension. In order to verify this probability, a futuristic prospective study to follow up this study cohort over the next 5-8 years is suggested. As such, a preventive approach to minimize the hazardous health consequences of obesity in the young age group is a priority. The modification of the eating habits, adopting strategies encouraging a reduction in sedentary behaviors, and an increase in physical activity may well be most effective in alleviating the burden of obesity upon the community. Corresponding author.
--	--