Correspondence

Pre-hospital period in patients with myocardial infarction in Turkey. *Methodological and statistical pitfalls*

To the Editor

I have read with interest the recently published article by Dr. Sadikoglu et al in your prestigious journal.[1] I have some comments because of the importance of the subject. *First:* the authors mentioned in the abstract that their objective was to "identify the causes that affect the time from the onset of symptoms to admission to the hospital". In fact, cross-sectional studies do not prove causality, but only the association. The authors rephrased their aim in the last few lines of introduction by mentioning that "the aim of the study was to determine the time spent … and the factors responsible….". Again, their statement is inaccurate and imprecise because they did not report the mean (SD) of "time spent" to hospitalization, and the factors studied were the associated factors with delay in hospitalization and not the cause, nor the responsible factors for it. *Second:* in tables 3 and 4, the authors used cross tabulation bivariate analysis to study the association of "< 6" *versus (vs)* "> 6 hours delay in admission", with some factors. The authors erred in non-disclosure of tests of significance used in the tables. Assuming they either used Chi squared test, or Fisher Exact-, if Chi values is not valid because expected cells below 5-, they mentioned the *p* value of tests used for each category of the variable used. In other word, they put a *p* value in front of the "males", as well as the "females" categories of "gender" variable, whereas it should only be one single *p* value for this 2, on both table.[2] Ironically, they interpreted their flawed presentation of results by stating in the abstract, and results text that "male patients seemed to present earlier than females" and adding the 2 *p* values < 0.05, < 0.05 successively. They put in table 3 the column percentages (79.7, 20.3; 59.5, 40.5), whereas they should put the raw percentages to show the percentage of male (or female) patients admitted before *vs* after 6 hours. Accordingly they should mention that 83.82% of males admitted before 6 hours *vs* 16.18% after 6 hours; and 65.91% of females admitted < 6 hours *vs* 34.09% after. The same could be said on their multiple *p* values for the different "education" categories or "socio-economic status (SES)" categories. In Table 4, they did not mention tests of significance used. Doing the statistical calculation myself, I found that they put the *p* of the uncorrected Chi squared except for "Cardiac arrest" and "Dementia" variables' rows where they put the *p* of Fisher Exact test. Moreover, the *p* value in front of "cardiogenic shock" variable should be 0.512 instead of 0.550. *Third:* the SES scale used in the study was totally ambiguous to the reader. The authors did not explain what are its items or components. Let mention the reference they gave (reference #20), which addresses depression after delivery and in which the reader should presume the details SES. I think they have to discuss briefly the components of their scale besides the direct and correct reference for it. Moreover, they examined the association of time to admission of "education" variable as well as "SES", which is a manifest redundancy given that education is a strong predictor of SES, if it is not one of its components. *Fourth:* the categorization of time to admission to below 6, and after 6 hours based on an old reference (LATE study, published in 1993, reference #12) could also be questionable. They mentioned in the last paragraph of their methods that the aforementioned reference was the basis behind "why [their] study groups were allocated into 2 groups". Reviewing the author's references in general revealed that 23 out of the 28 references used were before the year 2000, and the most recent reference used dated to 2001 (reference #2). I wonder how the authors used such old references in a study dated to 2004-2005, and touching a very rapidly updating topic. Hence, I would like to add to their list 2 references assessing time to reperfusion treatment of myocardial infarction in which shorter cut-off time to treatment have been used.[3,4] Finally, in the introduction, the authors cited Capewell et al[5] study, where they-mistakenly-stated that "two thirds of [decline in mortality rate due to coronary heart disease] being the result of a decrease in risk factors for coronary heart disease". Capewell et al[5] concluded in their paper that "half [and not two thirds] of the Auckland cardiovascular heart disease (CHD) mortality rate fall, apparently, attributable to reductions in major risk factors, particularly smoking". I also wonder what urged the authors to quote that in an existing furious epidemic of non communicable diseases witnessed in the Middle East, the Gulf Cooperation Countries (GCC), and Asian countries, in general. Capewell et al[5] study was based on routine health statistics and data from the Auckland Region Coronary Or Stroke (ARCOS) Study, a World Health Organization MONICA[6] project. Using the same methodology, in the only MONICA center in a developing country in Beijing, Critchley et al[7] concluded that the dramatic CHD mortality increases can be explained by the rise in total cholesterol, reflecting an increasingly "western" diet. They added that "without cardiological treatments, increases would have been even greater". Therefore, I could humbly say that Capewell et al's[5] results could neither be generalized, nor

projected on developing countries , the Middle East, or the GCC countries, where obesity prevalence ranged between 20-35%, diabetes ranged between 12-18%, and smoking is prevailing with an increasing trend, especially among females.

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Reply from the Author

No reply was received from the Author.

References

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4. Fu Y, Goodman S, Chang WC, Van De Werf F, Granger CB, Armstrong PW. Time to treatment influences the impact of ST-segment resolution on one-year prognosis: insights from the assessment of the safety and efficacy of a new thrombolytic (ASSENT-2) trial. ***Circulation*** 2001; 104: 2653-2659.
5. Capewell S, Beaglehole R, Seddon M, McMurray J. Explanation for the decline in coronary heart disease mortality rates in Auckland, New Zealand, between 1982 and 1993. ***Circulation*** 2000; 102: 1511-1516.
6. Kuulasmaa K, Tunstall-Pedoe H, Dobson A, Fortmann S, Sans S, Tolonen H, et al. Estimation of contribution of changes in classic risk factors to trends in coronary-event rates across the WHO MONICA Project populations. ***Lancet*** 2000; 355: 675-687.
7. Critchley J, Liu J, Zhao D, Wei W, Capewell S. Explaining the increase in coronary heart disease mortality in Beijing between 1984 and 1999. ***Circulation*** 2004; 110: 1236-1244.

Erratum

In manuscript "Percutaneous K-wiring for Gartland type III supracondylar humerus fractures in children" Saudi Medical Journal 2007; Vol. 28 (4): 603-606, The Baumann's angle was within the normal range should have appeared as follows:
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