

Letter to Editor

Relaxin-acute myocardial infarction

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I am writing regarding the recent article:

Dongxia Zhang, Yun Wang, Songben Yu, HuaNiu, Xingji Gong, Xia Miao: Serum relaxin levels as a novel biomarker for detection of acute myocardial infarction. *Int J Clin Exp Med* 2015;8(9):16937-16940 that appeared in your journal.

I find this article extremely troublesome for at least two reasons:

Firstly, the authors are trying to report on serum relaxin concentrations using an assay with absolutely no description. They simply refer to a "commercially available ELISA Kit (Guangzhou, China)". This is totally unacceptable for a scientific paper to allow this. There is not even the mention of the name of the company, let alone a description of specificity, sensitivity, controls and other aspects of an assay that is being reported for the first time. We have absolutely no assurance that this assay measures relaxin and the fact that this is a commercial assay does not relieve the authors the obligation for proper assay validation.

Secondly, even if we were to believe this is a valid assay, the authors have not explained the physiologically impossible concentrations they

report. The concentrations of serum relaxin in the control group of 9.2 ng/ml is higher than encountered in pregnancy by a factor of about 9 fold. The AMI group concentrations of 27.4 ng/ml exceed even that seen in IV pregnancies. There is no discussion of this that can be found in the paper.

The authors subsequently reported to you that this assay was from ThermoFisher but did not answer the questions regarding any sort of proper validation for measuring relaxin in serum. They also responded to you that they were in error and values should have been 10X lower at 0.92 and 2.74 ng/ml. However, these values are still out of physiological ranges and would only be found during pregnancy yet they did not explain these concentrations.

I have to dispute the conclusions of the article that serum relaxin is connected with AMI from the data presented.

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