

## Can neutrophil-to-lymphocyte ratio be a valuable marker in defining peripheral artery disease severity?

To the Editor,

I read with great interest the article entitled "Neutrophil-to-lymphocyte ratio may be a marker of peripheral artery disease complexity," which was published online in *Anatol J Cardiol* 2015 by Aykan et al. (1). In their study, the authors reported that LDL and HDL cholesterol levels and neutrophil-to-lymphocyte ratio (NLR) were independent factors for predicting a higher TASC class in patients with peripheral artery disease (PAD). However, they did not include the severity of coronary artery disease (CAD) in the multivariate logistic regression analysis. Sönmez et al. (2) demonstrated that NLR was an independent predictor of high SYNTAX score and strongly associated with the complexity of CAD. I think that the severity of CAD should be considered in the statistical analysis instead of the presence of CAD. Therefore, I was wondering if there was any difference between the groups in terms of the severity of CAD?

Moreover, obesity is associated with higher levels of inflammatory cytokines in the circulation (3). Ix et al. (4) demonstrated that higher body mass index is associated with PAD in patients who had never smoked. Because NLR is a new biomarker in cardiac and non-cardiac disorders, authors should state the body mass index for each group. To verify whether NLR is an important predictor of PAD complexity, the abovementioned factors should be taken into consideration.

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### Author's Reply

To the Editor,

We thank the authors for the interest they have shown in our article entitled "Neutrophil-to-lymphocyte ratio (NLR) may be a marker of peripheral artery disease complexity" published online in *Anatol J Cardiol* 2015 (1).

NLR is associated with both obstructive coronary artery disease (CAD) and CAD ectasia (2, 3). We previously showed that CAD is common in patients with peripheral artery disease (PAD) and the severity and complexity of CAD was associated with the severity and complexity of PAD (4, 5). Evaluation of SYNTAX score together with NLR may give additional information for calculating a probability score. However, this study is not designed that way. The objective of this study was to evaluate the relationship between PAD severity and complexity, as evaluated by TransAtlantic Inter-Society Consensus-II (TASC-II) classification, and NLR. Therefore, we evaluated if gender was associated with CAD severity. Body mass index, presence of metabolic syndrome, and waist-to-hip ratio were important markers for CAD (6). Our study was a retrospective cross-sectional study.

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## Mitral annular calcification: left atrial size and left ventricular dysfunction

To the Editor,

We read with great interest the article entitled "Assessment of left atrial volume and mechanical functions using real-time three-dimensional echocardiography in patients with mitral annular calcification" by Bayramoğlu et al. (1) published in *Anatol J Cardiol* 2016; 16: 42-7. We have some commentaries related to the left atrial (LA) volume and left ventricular diastolic dysfunction.

According to brand new recommendations in chamber quantification (2), assessment of the LA size using only the antero-posterior diameter assumes that when LA enlarges, all its dimensions change similarly, which is often not the case during LA remodeling. In this paper, in patients with mitral annular calcification (MAC), changes in the LA diameter seem to be in accordance with the indexed LA volume.

The peak Ea velocity can be measured from any aspect of the mitral annulus from the apical views, with the lateral annulus most commonly used. However, I was wondering how difficult it was to measure TDI parameters in lateral mitral annulus due to the artifacts/noise related to these annular calcifications and how accurate is it.

The authors said that "there were no significant differences in age, gender, smoking status..." I believe that it is important for this study that smoking status was actually statistically significant different between MAC group and controls (36.7% versus 13.3%;  $p=0.037$ ; please see Table 1).

Also the authors concluded that "LA mechanical function was impaired in patients with MAC". Indeed, all parameters of LA mechanical function, assessed by RT3DE, were statistically significant different between the MAC group and controls, but mitral late-diastolic velocity, assessed by TDI (Am), was not ( $8.9\pm 2.1$  cm/s versus  $8.4\pm 1.0$  cm/s;  $p=0.296$ ). How could this be explained?

Patients from the MAC group did not have LA dilation compared with those from the control group according to normal values for RT3DE (3), and even these volumes were statistically different (LA volume index was  $26.9\pm 6.1$  mL/m<sup>2</sup> versus  $20.5\pm 2.4$  mL/m<sup>2</sup>;  $p<0.001$ ). Therefore, these patients with MAC have had left ventricular diastolic dysfunction without LA dilation.

MAC could be related to coronary artery disease, which is frequently associated with left ventricular diastolic dysfunction. It was showed that in patients aged  $\leq 65$  years, MAC is associated with an increased prevalence of severe obstructive coronary artery disease (4). Could we know if these patients did not have asymptomatic non-obstructive coronary artery disease? Also, LV diastolic dysfunction could be associated with arrhythmia risk. In spite of the fact that this is a little bit far from the subject of this study, I am wondering if these patients with MAC underwent arrhythmia risk assessment.

In conclusion, I agree that "LA volumes and fractions reflect the severity of the left ventricular diastolic dysfunction". In this study, LA size, assessed by RT3DE, in both study and control group patients was not dilated. Therefore, could we talk about the left ventricular dysfunction in the absence of LA dilation? This is not in accordance with the current guidelines for left ventricular dysfunction (5). Should we also change the cut-off values of LA volume from the current algorithm of the left ventricular diastolic dysfunction?

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