Secondly, several studies failed to find correlation between heart rate and the dispersion of ventricular recovery times measured with QTd. The precise relation between the heart rate and the dispersion of recovery times is still an unresolved issue. However, QTd measured in the standard 12-lead ECG is not based on (and thus should not be corrected for) the heart period in the same way as the QT interval (2). Also, a previous study showed that QTd remains unchanged during atrial pacing at heart rates up to 120 beats/min in individuals without structural heart disease and in patients with a history of sustained ventricular tachycardia (3). In our study, corrected QTd was not calculated since the previous studies have shown that rate correction of parameters of repolarization dispersion is probably unnecessary and may even distort the values and predictive usefulness of QTd.

Thirdly, in the current study, correlation analysis was not performed. According to the author's suggestion, Pearson correlation analysis was performed for indirect (I) bilirubin (B). IB had a negative correlation with QTd (r^2=0.047, p=0.003) and Pd (r^2=0.090, p=0.001), but had no correlation with heart rate. B may decrease the risk of arrhythmias with unknown mechanism. However, the mechanism is still not fully understood.

B is a well-known antioxidant. Small dens low density lipoprotein and oxidative stress markers have been found to be low in patients with Gilbert's syndrome (GS). Additionally, in a previous study, it has been reported that B decreases the release of large and active thrombocytes to peripheral blood stream by decreasing proinflammatory cytokines so may prevent arterial and venous occlusive cardiac diseases such as myocardial infarction (4). Also, in another study, high B level has been found to be negatively correlated with epicardial adipose tissue thickness (EAT) and elevated adiponectin levels which has anti-atherosclerotic effect (5). Decreased EAT may lead to lower release of proinflammatory cytokines and lower atherosclerotic heart disease. Another study has displayed the anti-atherosclerotic effect of mild elevation of B due to lower pulse wave velocity in GS patients (6). There is a need for further studies investigating the mechanisms of cardioprotective effects of B.

We, sincerely thank the authors for their contribution to our work.

Erkan Cüre
Department of Internal Medicine, Faculty of Medicine, Recep Tayyip Erdoğan University; Rize-*Turkey*

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Address for Correspondence: Dr. Erkan Cüre,
Recep Tayyip Erdoğan Üniversitesi Tıp Fakültesi, İç Hastalıkları Anabilim Dalı; Rize-*Türkiye*
Phone: +90 464 213 04 91
E-mail: erkancure@yahoo.com
Available Online Date: 04.02.2014

Real-time three dimensional transesophageal echocardiography has an incremental value in delineation of paravalvular leakages

To the Editor,

We have read with great interest the article entitled 'The relation between location of paravalvular leakage and time to reoperation after mitral valve replacement: an observational study" published in Anadolu Kardiyol Derg 2013 Sep 10. (1). The authors aimed to evaluate any potential link between location of paravalvular leakage (PVL) and time to reoperation in patients undergoing redo mitral valve surgery. Thanks to the authors for their contribution of the present study. On the other hand, we want to make essential criticisms about this study from different aspects.

First of all, there is major concern regarding the methodology of the study. The study group was divided into 2 groups; Group 1 (Leaflet) and Group 2 (Commissural). This classification is vague and to the best of our knowledge this has not been described and reported in the literature previously. Recently, most authors (both cardiologists and cardiovascular surgeons) have used clock-wise format to describe the localizations of PVLs (2-4). It would have been much better if the authors had taken into consideration the terminology of PVL localizations in an understandable manner.

In discussion and conclusion sections, the authors concluded that echocardiographic evaluation should include location of the paravalvular leakage during follow-up of patients with PVL after mitral valve replacement. However, due to methodology of the study only transthoracic echocardiography (TTE) was performed for assessment of PVLs preoperatively and during follow-up and transesophageal echocardiography (TEE) was only performed at the end of the surgery for assessment of residual PVL (TEE was not performed neither preoperatively nor during the follow-up). TTE is certainly the initial choice of evaluation of prosthetic valves and complications but is unable to delineate localization of PVLs. Use of both 2D TEE and particularly real time three dimensional transesophageal echocardiography (RT-3D TEE) is mandatory for defining location and size of the paravalvular leakage during follow-up. Recently, RT-3D TEE has been introduced into clinical practice which has permitted assessment of PVLs precisely by 'en face view' from atrial (surgical) side of view (4, 5). Since the authors did not perform any TEE examination during follow-up of PVLs which may stand for a major limitation, it causes a conflict with the authors' conclusions. Furthermore, the authors did not consider this as a major limitation of the study.

Another noteworthy issue is the mortality rates for reoperation in Group 2 (Commissural PVL) which is reported as 0%. In current literature the mortality rates for reoperation of paravalvular leaks is much higher

(range between 8% to 12%), so we are curious which specific technique they have used during surgery that made their study free of mortality.

As a result, we believe that the methodology of the present study is inappropriate for drawing a conclusion regarding evaluation of PVL localizations and size, which almost always necessitate complementary 2D and RT-3D TEE assessment. The messages given by the study are inaccurate and misleading. Hence, we feel that this study has low teaching value.

Mehmet Özkan, Mustafa Ozan Gürsoy[1]
Department of Cardiology, Faculty of Medicine,
Kars Kafkas University; Kars-*Turkey*
[1]Clinic of Cardiology, Gaziemir State Hospital; İzmir-*Turkey*

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Address for Correspondence: Dr. Mustafa Ozan Gürsoy,
Gaziemir Devlet Hastanesi, Kardiyoloji Kliniği; İzmir-*Türkiye*
Phone: +90 506 371 78 23
Fax: +90 216 459 63 21
E-mail: m.ozangursoy@yahoo.com
Available Online Date: 04.02.2014

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Author`s Reply

To the Editor,

We read with great concern 'the letter to editor' for our artice titled 'The relation between location of paravalvular leakage and time to reoperation after mitral valve replacement: an observational study" published in The Anatolian Journal of Cardiology (1). We would like to thank to the authors for their contributions to our study.

The three issues that needs to be explained are as follows.

First is about the description and classification of leakage point. As mentioned in methodology-operative procedure section of the article, the leakage point was classified in the operation table and it was a surgical evaluation as commisural side or anterior/posterior leaflet side. As it is emphasized by the reader, recently most authors (both cardiologists and cardiovascular surgeons) have used clock-wise format to describe the localizations of PVLs (2-4). It would have been much better. Thanks to the reader for this advice and it will be taken into consideration in next studies.

Second issue is about TEE. As it is stressed by the reader, TTE is certainly the initial choice of evaluation of prosthetic valves and complications. Even recently real time three dimensional transesophageal echocardiography (RT-3D TEE) has been introduced into clinical practice. Use of both 2D TEE and particularly RT-3D TEE is mandatory for defining location and size of the paravalvular leakage during follow-up. (4). In our article, we did not focused to compare the preoperative and operative PVL locations and/or size. However we focused on initial TTE parameters to diagnose the PVL and the period till operation. We are grateful to reader to remind us that TEE is one of the major limitation of the study that not mentioned.

Third issue is about mortality rates. As mentioned above, this is an observational study. Mortality rates may vary between centers (5, 6). The mortality rate in our study is just for 30 days. Considering the small number of cases in our study, larger series and next studies focused on early and late mortality more than 30 days and of reasons may be more elucidative.

Mehmet Yanartaş
Clinic of cardiovascular surgery, Kartal Koşuyolu Heart Training and Research Hospital; İstanbul-*Turkey*

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Address for Correspondence: Dr. Mehmet Yanartaş,
Kartal Koşuyolu Eğitim ve Araştırma Hastanesi, Kalp ve
Damar Cerrahisi; İstanbul-*Türkiye*
Phone: +90 216 500 15 00
E-mail: myanartas@yahoo.com
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