

lipid levels. We supposed it is necessary to present the data showing no statistically significant difference between the patients and control subjects with regard to influential factors.

Finally, the international Toronto Consensus supports the spectral analysis of HRV beyond CARTs, which are currently accepted as the gold standard (2). We think that the presence of CAN in patients with rheumatoid arthritis is shown with the spectral analysis of HRV as well as the reflex tests used in the study by Javady et al. (1).

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Author's Reply

To the Editor,

We appreciate the precise review and insightful comments by our dear colleagues regarding our recent study entitled "Cardiovascular Autonomic Neuropathy in Rheumatoid Arthritis assessed by Cardiovascular Autonomic Function Tests: A cross-sectional survey" published in *Anatol J Cardiol* 2015; 15: 722-6.

In our study, we assessed cardiovascular autonomic neuropathy (CAN) in rheumatoid arthritis (RA) patients compared with control subjects by bedside autonomic function tests. Our study failed to show any statistically significant difference between cardiovascular autonomic function tests in RA patients and the control subjects (1).

Although in studies autonomic function tests are considered indices mainly of parasympathetic or sympathetic function, according to Ewing et al. (2), "The autonomic pathways involved in all cardiovascular reflex tests are however extremely complex and include both parasympathetic and sympathetic fibers to a greater or lesser extent. While heart rate responses are primarily mediated via cardiac parasympathetic pathways, additional sympathetic influences, particularly in the Valsalva maneuver,

can alter these responses. We and others have previously classified these tests into parasympathetic and sympathetic, depending on whether heart rate alone or both heart rate and blood pressure control was affected. This approach has proved to be extremely useful clinically because it reflects the sequence of damage seen in diabetic subjects and has therefore been widely used. However, we would stress that although clinically useful, such a classification should not be considered physiologically precise because of the complexity of autonomic pathways" (2).

About influential factors of heart rate variability such as body weight, body mass index, insulin resistance, and blood lipid levels (3) that our dear colleagues mentioned, these are not among the variables in our study. We agree these factors can provide complementary information. Therefore, these factors needed to be considered in future studies that will assess the difference between the cardiovascular autonomic function of RA patients and the general population.

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Predictors of successful percutaneous transvenous mitral commissurotomy using the Bonhoeffer Multi-Track system in patients with moderate to severe mitral stenosis: Can we see beyond the Wilkins score?

To the Editor,

We read the original investigation entitled "Predictors of successful percutaneous transvenous mitral commissurotomy using the Bonhoeffer Multi-Track system in patients with moderate to severe mitral stenosis: Can we see beyond the Wilkins score?" by Farman et al. (1) published in the *Anatol J Cardiol* 2015; 15: 373-9. with great interest. We would like to touch on some points regarding this article.

Undoubtedly, several factors such as appropriate patient selection, proper echocardiographic imaging, and use of suitable techniques may affect the success of mitral balloon valvuloplasty (MBV) (2).

The Wilkins valve score is the main topic of this study (1, 3). It is well known that an increase in the valve score leads to reduced MBV success. However, the report solely indicated that the valve score was <8 in both groups. Emphasizing the numerical values of the valve score in the successful and failed valvuloplasty cases and if present, mentioning the difference between them would be more illuminative. We believe that a valve score of <8 in both groups does not mean that the two groups will have similar success rates. The success rate will quite likely decrease with the increasing valve score (3).

Although two-dimensional transthoracic echocardiography (TTE) is widely used as the first-line imaging method to evaluate the structure of the mitral valve, recently 3D echocardiography and even transesophageal (TEE) 3D echocardiography gained popularity in assessing the structure of the valve structure and presence of a possible thrombus in the left atrial appendix. (2). An accurate echocardiographic evaluation is crucial in the decision phase of MBV. The statement "patients with a clot in the left atrium (LA) were excluded from the study" points that some patients underwent transesophageal echocardiographic evaluation. However, it is not stated how many of them were examined by TEE. It can also be estimated from the statements in the manuscript that TTE was used in calculating the Wilkins valve score. However, TEE is recommended in the evaluation of the Wilkins valve score and commissural anatomy (4). Therefore, the assessment of the valve structure via TEE even if 3D TEE is not available would be more suitable.

Considering that the mitral valve structure was also evaluated via fluoroscopy, the calculation of lung and Cormier score (5) would make the report more interesting. Furthermore, although recent studies depicted that asymmetrical commissural fusion deteriorates the success of MBV, the frequency of this entity in successful and failed MBV groups was not mentioned in the study (3). We hope that the authors are willing to comment on these issues.

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Author's Reply

To the Editor,

The Wilkin's valve score was not the main topic of our study. As we mentioned in our initial segment of the paper (Introduction), we wanted to see beyond the Wilkin's score (1). It does not mean that we wanted to challenge the Wilkin's scoring system. The work that Wilkin's et al. (1) did is unmatched. It is well established and has so much worth. The writer correctly mentioned that we used the Wilkin's score for the selection of our patients in both groups and that they were having a score of <8. The problem was that despite careful selection using the Wilkin's score outcome in number of patients is used to be suboptimal. This is the crux of our study. We wanted to see if there was anything else that would be useful to improve our outcome besides Wilkin's score. Therefore, by endorsing the Wilkin's score in the double balloon (Bonhoeffer) (2) technique at the same time, we tried to see beyond that and reported this in our study. This is the reason we did not touch the numerical values of the Wilkin's valve score. It was neither our objective nor interesting to explore the thing widely investigated and reported elsewhere in literature.

In our institution, we usually perform transesophageal echocardiogram (TEE) in every patient who is going to undergo percutaneous transluminal mitral commissurotomy (PTMC), especially if the left atrial appendage is not visible on transthoracic echocardiography (TTE). Although these echo findings were included in our patient's selection criteria, we did not mention it in detail because it was not included in our methodology of PTMC. It is needless to mention that the assessment of the valve structure by TEE was also conducted in the echo department before the selection of patients for PTMC. In fact, a detailed echo study was the prerequisite for patients who were considered for PTMC. Unfortunately, our echo department was not equipped with 3D echo at that time.

It was correctly stated that the calculation of the lung and Cormier score (3) would make the report more interesting. However, we think that it needs thorough study and a detailed account. To keep the article simple and within the limits of word count, we left it for further studies. We are thankful for the advice and will definitely be looking our data again in this respect. However, it shows that the author of the letter agreed with our claim that a lot of work can be done to improve the outcome of balloon mitral commissurotomy and that we need to look beyond the Wilkin's score.

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Should we consider serum potassium level as a mortality predictor in ST-elevation myocardial infarction?

To the Editor,

I read the article by Uluganyan et al. (1) entitled "Admission serum potassium level is associated with in-hospital and long-term mortality in ST-elevation myocardial infarction" with great interest, which was published online in your *Anatol J Cardiol* 2015 Febr 11. In their study, the authors reported that the admission serum potassium (sK) level of >4.5 mmol/L was associated with increased long-term mortality in patients with ST-elevation myocardial infarction (STEMI) who underwent primary percutaneous coronary intervention (pPCI). I would like to emphasize some confounding factors that can affect the results of the present study.

First, Uluganyan et al. (1) reported that patients were treated with drugs according to the European Society of Cardiology guidelines on myocardial revascularization. However, there are no data about the type of dual antiplatelet therapy (DAPT). It has been demonstrated that DAPT with ticagrelor reduced mortality than DAPT with clopidogrel in patients with STEMI who underwent pPCI (2). Additionally, in patients with STEMI undergoing pPCI, prasugrel is more effective than clopidogrel for the prevention of cardiovascular death and ischemic events (3). Hence, the higher incidence of treatment with ticagrelor and prasugrel in patients with sK levels of <4.5 may be a reason for lower mortality rates for these patients. Authors should state the incidence of DAPT with prasugrel, ticagrelor, and clopidogrel for each group, respectively.

Second, the authors did not report any data about the usage of aldosterone antagonists. The study by Uluganyan et al. (1) includes patients with impaired left ventricular systolic function. Aldosterone antagonists significantly reduce mortality in post-STEMI patients with left ventricular systolic dysfunction (ejection fraction <40%) (4). Hence, less treatment with aldosterone antagonists may be a reason for higher mortality rates for patients with sK levels of >4.5 mmol/L.

Finally, in the present study by Uluganyan et al. (1), there are no data about time to reperfusion and door-to-balloon time. It is known that delay in reperfusion and longer door-to-balloon time cause higher mortality rates (5, 6). Delay in time to reperfusion and longer door-to-balloon time may be another reason for higher mortality rates in patients with sK levels of >4.5 mmol/L when compared with patients with sK levels of <4.5 mmol/L. Therefore, the authors should state the time to reperfusion and door-to-balloon time for each group, respectively.

In conclusion, sK levels of >4.5 mmol/L may indicate worse outcomes in patients with STEMI undergoing pPCI. However, medical treatments, time to reperfusion, and door-to-balloon time may still affect the results of the study by Uluganyan et al. (1). To define the sK level of >4.5 mmol/L as a predictor of mortality, all factors associated with mortality should be considered.

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Author's Reply

To the Editor,

We thank the authors for their comments about our article entitled "Admission serum potassium level is associated with in-hospital and