

ABO blood groups in the group of SS=0 patients is equal to the distribution in SS=1-3 groups and how inclusion of that group in an analysis would influence the results of the presented study. If the O blood group really dominates in the non-CAD patients and patients with less complex CAD, we could expect significantly more patients with O blood group in this, relatively large group of excluded patients.

However, despite the mentioned differences, our two studies can be observed together as a significant contribution in investigation of pathophysiological link between ABO blood groups antigens and stable CAD. Different results obtained and conclusion drawn by different methodologies used in assessment of coronary atherosclerotic changes should be tested in the future prospective studies.

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## Author's Reply

To the Editor,

First of all we would like to thank the authors for their valuable comments for our article published in February issue of The Anatolian Journal of Cardiology 2014; 14: 55-60 (1). As known, there is a close relationship between ABO blood groups and development of coronary artery disease. Many researches have been done to reveal the relationship between ABO blood groups and the development of coronary artery disease so far. Some of these researches have conflicting results. We believe that one of the most important reasons for the conflicting results is methodology. For example, there could be different results between a prospective study investigating the relationship between ABO blood groups and development of coronary artery disease with a retrospective/cross sectional study investigating relationship between ABO blood groups and angiographic coronary artery disease. Besides definition and assessment of the coronary artery disease is significantly different in between the

manuscripts. Some studies included stable coronary artery disease and some acute coronary syndrome. Similarly, some authors accepted as angiographic coronary artery diseases as who have coronary lesions more than 50%, whereas some accepted as coronary lesions more than 25%. However, theoretically the basic way of the assessment of the coronary artery disease in an individual is intracoronary imaging (IVUS, OCT). We have used SYNTAX score, on the other hand, the authors assessed the coronary artery disease by Gensini score (1, 2). Although these two scores have been utilized in the assessment of the coronary artery disease, there are some differences as the authors defined. Besides, while the Gensini score reveals mostly the extent of the coronary artery disease, the SYNTAX score reveals mostly the complexity of the coronary artery disease (3). For that, while the Gensini score is mostly utilized for prognostic purpose, the SYNTAX score is mostly utilized in the determination of the treatment modality. The main aim of the our study was to investigate the association between the ABO blood groups and coronary artery disease complexity, as well as to show the importance of the blood group while determining the treatment modality. The authors suggested to include patients with a SYNTAX score =0 and reanalyze. We did not do this analysis for two reasons. Firstly, we did not collect data of the patients with SYNTAX score=0. Secondly, the aim of the our study to investigate the association between the ABO blood groups and coronary artery disease complexity, not investigate the association between the ABO blood groups and coronary artery disease presence.

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## Strict patient selection for renal sympathetic denervation may yield to more favorable results

To the Editor,

We read with a great interest the article by Drexel et al. (1) entitled as "Renal denervation-Review" that published recently in this journal. Renal sympathetic denervation (RSD) opened a new era in the treatment of

resistant hypertension (HT). However, there is a still large arguments about the efficacy of RSD in the long term period (2, 3). Some authors, proposed that, the effect is similar to placebo or Harthowm effect, whereas others defend the initial reported results showing the favorable effect. Drexel et al. (1) clearly demonstrated the pathophysiologic mechanism of RDS and Simplicity-1 and Simplicity-2 trials. They also criticized the limitation of this studies. Hence, recently published Simplicity HTN-3 trial increased the doubt about RSD (4). They reported that, 6 months result of patients undergone RSD was similar to those under medication. This study was the first blinded sham-control investigation and it reverse the all favorable wind direction about RSD. The ongoing EnligHTN-IV trial, a large randomized sham-controlled trial using a multi-electrode radio-frequency ablation catheter (St. Jude Medical, St. Paul, MN, USA), will show whether the Simplicity HTN-3 result is based on catheter or not. On the other hand, Simplicity HTN-3 result may set up new discussion about patient selection for RSD. Current criteria is drug-based approach and life style changes including relaxation therapy was usually overlooked. In addition, medication adherence is important problem which was also become evident in Simplicity HTN-3 trial (4). This trial also demonstrated the importance of placebo effect in the HT which could be provided by relaxation session. Inadequate dosage for HT medication is also another problem for reducing blood pressure (5). More strict selection criteria may generated the patient group who get more benefit from RSD. Before patient selection, adherence to life change modulation should be checked and dosage of medication should be optimized according to guidelines. Relaxation therapy, ie yoga séance, psychotherapy, screening for depression or anxiety disorders, should be encouraged.

In conclusion, RSD is safe and additional therapeutic option for resistant HT. Although, clinical effect is doubtful, strict selection criteria for RSD may bring more favorable clinical results. Further randomized investigation will show the certain faith of RSD.

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#### Author's Reply

To the Editor,

Thank you for the interest in our recent review article published in March issue of *The Anatolian Journal of Cardiology* 2014; 14: 186-91 (1). We are in agreement with the excellent comments stated. Though physiological experience in animal models and humans as outlined by us and prior human experience with surgical sympathectomy suggests a sound scientific basis for renal denervation, as demonstrated in the past on numerous occasions, physiological evidence does not uniformly translate into the expected clinical results. The reasons for an absence of the expected blood pressure response after renal denervation in Symplicity HTN-3 remain unclear.

Possibilities include technical shortcoming including operator experience and device related limitations. However, the outcomes apparently did not differ between operators who performed a larger number compared with those who performed a minimal number (2). In this context, it is noteworthy that the average number of procedures per operator was 3.3, a number that most would consider inadequate for most percutaneous procedures (2). It is also not clear if the number of ablations (3.9 per artery) is sufficient to reach the intended results (complete circumferential sympathetic denervation) though apparently the blood pressure reduction was no different in those with a higher compared with those with a lower number of ablations (2). In addition, patient selection may be an important aspect of procedural success. In subgroup analysis, African Americans and elderly patients seemed to have no benefit whereas non-African Americans and younger patients experienced a significantly more pronounced blood pressure reduction after renal denervation compared with sham (2). A smaller blood pressure reduction than reported in Symplicity HTN-1 and -2 was to be expected given the design limitations of these trials including placebo effect, selection bias, patient and operator bias, regression to the mean (or "big day bias") and "check once more" bias (3). With a smaller than anticipated blood pressure reduction in a patient population that differed from the first two Symplicity trials, Symplicity HTN-3 may have been underpowered in the studied population. The aforementioned are hypotheses. It is also conceivable that renal denervation simply does not lower blood pressure. However, it would be premature to make this conclusion based on the available data. It is important to await ongoing trials and more carefully re-evaluate the concept taking into consideration the knowledge that we have gained by Symplicity HTN-3.

In the meantime, we agree that there are a number of treatments, medical and non-medical (as listed in the letter to the editor) that have blood pressure lowering effects and should be tried first before turning to renal denervation. It should remain a procedure for patients that, despite all efforts, continue to have uncontrolled hypertension with no other options. Under these circumstances enrollment in trials or registries should be strongly encouraged to allow us to advance our knowledge and hopefully, one day, determine the merits of this concept.

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## Internal mammary artery as a graft in obese patients

To the Editor,

We greatly appreciate the authors for this really large patient pool study published in *The Anatolian Journal of Cardiology* (1). Reported results of this study related to obese patients confirm the existing clinical practice in our settings and the literature. Sternal dehiscence and sternal infections are commonly encountered complications in obese patients although underlying factors can differ (2, 3). One of the underlying factors is harvested internal mammary artery (IMA) (unilateral or bilateral) as coronary grafts (2-4). For this very reason, use of IMA in obese patients can be challenging and often conservatively denied. We were wondering if there was any significant difference between obese patient groups with IMA as a graft and without IMA in terms of wound infection or sternal dehiscence. There was also considerable difference in terms of bleeding between two groups as the authors stated. IMA can also be a reason for re-explorations due to bleeding postoperatively though it is undeniable for its excellent long-term patency rates (5). We find it really important to gather more information for further considerations on the issue. We would kindly ask the authors to share their valuable knowledge related the above-mentioned topics.

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### Author's Reply

To the Editor,

We would like to thank the authors for their contribution to our study of published in *The Anatolian Journal of Cardiology* (1). They have emphasized on occurrence of sternal wound infections and dehiscence, and the use of bilateral internal mammary artery (IMA) grafts. This debate on whether use of bilateral IMA causes increased risk of sternal wound infections and/or dehiscence is still ongoing, in diabetic patients, in obese and non-obese patients. But briefly, in 2010, Arterial Revascularization Trial by Taggart et al. (2), it has been documented clearly that use of bilateral IMA (n:1548) caused a slight increase in requirement for sternal wound reconstruction compared to use of single IMA (n:1554). In our study, use of single IMA, actually left-sided, was 96.7% (n:530/548) in non-obese group whereas 97.1% (n:235/242) in obese group (p>0.05). Despite the low number of patients whom IMA was not used for various reasons (n:25), the disuse did not affect the requirement for revision or occurrence of sternal wound infections or dehiscence (p>0.05 for both).

In our study, we have not used any bilateral IMAs, therefore it is not possible to make assumptions on this subject.

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