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Tirofiban usage and prognosis after myocardial infarction

To the Editor,

I read the article by Kaymaz et al. (1), entitled "The effects of tirofiban infusion on clinical and angiographic outcomes of patients with STEMI undergoing primary PCI." which was recently published online in your journal, with great interest. In their study, the authors reported that additional tirofiban usage significantly improves myocardial reperfusion, ST-segment resolution, in-hospital mortality rate, and in-hospital sudden cardiac death in patients with ST-segment elevation myocardial infarction (STEMI). I would like to make a critique on the methodology and results of the present study.

Tirofiban usage may be beneficial in patients with STEMI, but its effect on mortality is unclear. In the present study by Kaymaz et al. (1), there are no data about medications that are known to significantly reduce mortality and cardiovascular events in patients with STEMI. It is well known that statins, angiotensin-converting enzyme inhibitors (ACEIs)/angiotensin-receptor blockers (ARBs), and beta-blockers significantly reduce in-hospital and long-term mortalities and cardiovascular events in patients with STEMI (2). Also, Kaymaz et al. (1) did not report any data on the left ventricular ejection fraction for the patient groups. A low left ventricular ejection fraction is a strong predictor of mortality after myocardial infarction, and it is a predictor of in-hospital mortality in patients with STEMI who underwent primary percutaneous coronary intervention (3, 4). Additionally, aldosterone receptor antagonists significantly reduce mortality in post-myocardial infarction patients with left ventricle dysfunction (5). Therefore, lower medication rates with statins, ACEIs/ARBs, aldosterone antagonists, and beta-blockers and a lower ejection fraction in the non-tirofiban group may be another reason for higher mortality rates and cardiac events. The authors should state the mean ejection fraction and medications for each group and should compare the groups based on their medications and ejection fraction.

In conclusion, tirofiban usage may have beneficial effects in addition to standard therapy in patients with STEMI. However, to define its exact role on mortality, ejection fraction and medications that are known to reduce mortality should be taken into consideration.

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Author`s Reply

To the Editor,

I read with great interest the letter related to our manuscript entitled "The effects of tirofiban infusion on clinical and angiographic outcomes of patients with STEMI undergoing primary PCI" published in Anatol J Cardiol 2014 Dec 25. Epub of ahead of print by Kaymaz et al. (1) I am going to try to answer the long list of questions within the word count limits.

As summarized in this letter, we showed that tirofiban treatment (TRT) in addition to aspirin, high-dose clopidogrel, and unfractionated heparin prior to primary PCI significantly improves myocardial reperfusion, ST-segment resolution, in-hospital sudden cardiac death, and in-hospital all-cause mortality rates in patients with STEMI without an increased risk of major bleeding. The major limitation was the absence of prospective and randomized clinical trial designs because of the critical difficulties in the reimbursement of treatment cost. Despite this limitation, the comparison of baseline characteristics permitted us to assess the efficacy and safety issues of TRT among groups. Despite the higher TIMI risk score in the pre-PCI or upstream TRT group than in the other groups, the benefit in TIMI flow grade, corrected TIMI frame count, ST- segment resolution, in-hospital sudden cardiac death, and in-hospital all-cause mortality were also significantly higher in the upstream TRT subset than in the other subset. As I said before in my reply to first letter; our results should be considered to provide important data concerning the use of TRT combined with dual antiplatelet therapy (DAPT) including aspirin and high-dose clopidogrel, but it cannot be generalized to DAPT combinations with prasugrel or ticagrelor. Our bridging TRT was targeted to minimize the risk of intracoronary rethrombosis within the first hours of primary PCI in which the level of platelet inhibition still remains subtherapeutic because of the kinetics of clopidogrel, even with a 600-mg loading dose, and the well-known procoagulant state of STEMI.

It may not be appropriate to compare a study based on non-randomized and retrospective data with the FINESSE trial showing no appreciable benefit and only harm in starting GP IIb/IIIa inhibitors in the prehospital setting for patients treated with primary PCI (2). The comments of Jeremias et al. (3) were based on the meta-analysis of five randomized trials. They concluded that the routine use of abcix-

imab in patients with STEMI treated with primary PCI does not appear to be beneficial in those who receive pre-PCI thienopyridines (3). However, their comments are limited to five abciximab series and cannot be compared with the main results of our retrospective study in a total of 994 patients with STEMI in whom TRT was used prior to, during, or after primary PCI. Recent studies confirmed our positive results on upstream TRT (4, 5).

Intracoronary TRT was the choice in all patients of the peri-PCI TRT group, whereas only the intravenous route was used in the upstream or post-PCI TRT groups. Although the median difference in pain-to-balloon time was only 25 min between the upstream and peri-PCI TRT groups, more positive results with upstream TRT can be considered consistent with the potential benefit of earlier TRT over intracoronary injection of this drug at Cath Lab.

At the time of the enrollment, a manual aspiration catheter was not available in our center. In our opinion, "pain-to-balloon time" instead of "first medical contact-to-balloon time" seems to be a more appropriate measure for the estimation of total ischemic time, and the definition also includes the time delay from the occurrence of pain to the first medical contact. Data from angiographic and ST-segment resolution in the pre-PCI, peri-PCI, and post-PCI TRT subsets can answer your question concerning the effect of TRT on the no-reflow phenomenon. All patients with no-reflow or high thrombus burden without satisfactory ST-segment resolution underwent repeat angiography after TRT. In case of renal insufficiency, bolus TRT was not followed by infusion.

Finally, I would like to thank you for this letter, which led to a discussion concerning the use of upstream TRT as an adjunct treatment to DAPT in patients who underwent primary PCI.

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Uniform criteria for diagnosing noncompaction by cMRI and echocardiography are warranted

To the Editor,

We read with great interest the article by Akhbour et al. (1) published in Anatol J Cardiol 2015; 15: 550-5 entitled "Electrocardiographic findings in correlation to magnetic resonance imaging patterns in African patients with isolated ventricular noncompaction" on cardiac magnetic resonance imaging (cMRI) and electrocardiographic (ECG) findings in 24 patients with left ventricular hypertrabeculation (LVHT)/noncompaction. Systolic function and arrhythmia were not correlated with the number of non-compacted segments or the number of segments showing late gadolinium enhancement (LGE) (1). We have the following comments and concerns.

Though LVHT is presumably congenital in majority of the cases, it can be also acquired, such as in neuromuscular disorders (NMDs), (2) pregnant females (3), and athletes (4). Acquired LVHT suggests that LVHT is not only due to the failure of the embryonic compaction process but also may result from the adaptation of the myocardium to hemodynamic dysfunction.

We do not agree with the definition of LVHT for not allowing the presence of any other cardiac abnormality except LVHT (isolated LVHT). Non-isolated LVHT is frequent and is also LVHT.

How do the authors explain the missing correlation between the number of LGE segments and ventricular tachycardia? Was the group size too small? Was the correlation different when subendocardial, transmural, and mid-myocardial LGE were separately evaluated? Was the LGE pattern patchy or diffuse? Possibly, cMRI fails to display all degrees of fibrosis, particularly fibrosis of the endocardium or early evolving fibrosis? Possibly, ventricular arrhythmias are not correlated with the number of LG -segments but with the volume or area of the LGE lesions? It is also conceivable that fibrosis in LVHT is ethnically different; for instance, Caucasians show a positive correlation between fibrosis and arrhythmias, whereas Africans do not, similar to the results in the present study. How did the authors quantify arrhythmias to correlate them with the number of LVHT fibrotic segments?

Arrhythmias may not only result from myocardial fibrosis but also result from ischemia. There are some indications that perfusion of the non-compacted layer is worse than that of the compacted layer (5). Possibly, the amount of arrhythmias correlates with myocardial scintigraphy. The frequent occurrence of LBBB may not only result from myocardial fibrosis but also from trabeculations, which predispose for prolonged propagation of the excitation.

We do not agree with the statement that cMRI is the method of choice to diagnose LVHT (1). The method of choice is echocardiography, but in case the echocardiographic diagnosis is uncertain, cMRI should be performed. Both techniques supplement each other, but they produce false positive and false negative results. As long as there are no common generally accepted LVHT diagnostic criteria either for cMRI or for echocardiography and as long as there is no gold standard for diagnosing LVHT, the reliability of both methods remains limited.

Atrial fibrillation was found in 17% of patients (1). Did these patients also present with thrombi within the intertrabecular spaces?