Reply to Reviewer 1:

Dear Sir,

First of all let me thank you for accepting to review our work. Really your kind notes were of great importance and actually enriched our work.

(1) For the logistic prediction model, you want to perform; we think that this out of the scope and beyond the aim of our paper. Our current paper just outlined the relationship between the reperfusion modality in the setting of acute myocardial infarction and incidence of ventricular arrhythmias. We didn't aim to assess the predictors of such arrhythmia. This could be a subject of further research drawn from the same patient population. According to previous literatures, left ventricular ejection fraction (LVEF) <40%, hypertension, older age, larger infarct sizes, an ECG surrogate like ST elevation in >6 leads, microvascular obstruction, history of ventricular arrhythmias, infarcts in the anterior wall of the heart and elevated levels of cardiac enzymes, such as troponin, all are associated with an increased risk of ventricular arrhythmias after reperfusion in STEMI.

(2) For the criteria of successful fibrinolysis, we do know that the sensitivity an specificity of such variables are low. However, according to Pomés Iparraguirre et al. we defined successful fibrinolysis as the presence of at least two of the following criteria:

* Disappearance of chest pain within 90 minutes of starting the fibrinolytic infusion,

* Resolution of ST-segment elevation by more than 50% after starting fibrinolytic infusion in the lead with maximum elevation on baseline ECG,

* Or an abrupt initial increase in cardiac enzyme levels within the first 24 hours following onset of symptoms.

(3) As for the time window; we clearly stated that the second ECG was recorded 24 hours after successful revascularization either by fibrinolysis or PCI guided. The reported arrhythmias were encountered during the patients stay in CCU. This could be either before the recording of second ECG or thereafter.

(4) The difference in QT max., QT min., QTc and QTD between the two initial groups mostly didn't differ significantly and if there were any statistically significant difference, it was in favor of fibrinolytic group. There was a trend for some prolongation among PPCI group in comparison to fibrinolytic group which was totally reversed after revascularization. This strengthens our results.

(5) Of course, automated measurement of QT interval that we used in our study, has been described long time ago e.g. https://onlinelibrary.wiley.com/doi/epdf/10.1111/j.1542-474X.2011.00423.x. However, in studying the difference between two revascularization modalities in the setting of acute myocardial infarction, we were the first to use automated QT interval measurement. As described in the section of discussion, some authors described the idea however, using manual measurement e.g. https://bmccardiovascdisord.biomedcentral.com/articles/10.1186/s12872-020-01767-9.

(6) For the suggested reference "Association of QT dispersion with mortality and arrhythmic events—A meta-analysis of observational studies" (doi: 10.1002/joa3.12253), it has been successfully cited.

(7) The results section and tables were refined as recommended.

Reply to Reviewer 2:

Dear Sir,

Thank you very much for reviewing our work. Really your comments were valuable and added a great value to the manuscript.

(1) The inclusion and exclusion criteria were clear within the methodology section. Of course, we enrolled those patients who achieved successful revascularization either through thrombolytic therapy or PPCI. Those who didn't show criteria of successful thrombolysis after fibrinolytic therapy were excluded.

(2) Unfortunately, and due to economic reasons, the only available fibrinolytic therapy was streptokinase. We know well that it has low reperfusion rate among thrombolytic therapy. Anyhow, we recruited those patients who achieved successful reperfusion according to the proposed three criteria mentioned within the methodology section.

(3) Although more patients in group II had smoking, hypertension, diabetes, family history of CAD and longer time intervals, this didn't affect our results as these baseline differences didn't reach statistically significant level.

(4) As for multiple regression analysis, we didn't use it as our current work just outlined the relationship between the reperfusion modality in the setting of acute myocardial infarction and incidence of

ventricular arrhythmias. We didn't aim to assess the predictors of such arrhythmia post myocardial infarction.

(5) The drug history was nearly identical among the two study groups so it didn't affect our results.