Letter to the Editor

Gender and ST-elevation myocardial infarction

Sexo e infarto agudo de miocardio con elevación del ST

To the Editor,

We have read the recent article by Tizón-Marcos et al. in *Revista Española de Cardiología*[1] with great interest. In their conclusions, the authors state that "Compared with men, women with a first STEMI had similar 30-day mortality and complication rates." This conclusion appears to contradict the results of our work, which show higher in-hospital mortality for STEMI among women.[1,2]

To some extent, this discrepancy may reflect design differences between the study by Tizón-Marcos et al. and our own. These differences are related to the datasets analyzed (the *Codi-IAM* registry vs the Spanish National Health System Minimum Data Set [*Conjunto Mínimo Básico de Datos*, CMBD]), the study period (2010-2016 vs 2005-2015), the catchment area (Catalunya vs all of Spain), the inclusion criteria (first STEMI vs any STEMI event), the size of the study population (14 690 [24% women] vs 272 407 [28.8% women]), and the outcome measure (30-day mortality vs in-hospital mortality).

However, the study by Tizón-Marcos et al. also raises some methodological concerns that should be addressed. The authors adjusted their models for confounding factors associated with STEMI prognosis when the proportion of unavailable values was below 8%. Either because $\geq$ 8% of the values were missing or because the confounder in question was not a defined parameter in the *Codi-IAM* registry, Tizón-Marcos et al. did not consider some of the comorbidities that showed an association with mortality risk in our studies.[2,3] The final adjustment variables were age, diabetes mellitus, year of recruitment, symptom onset to balloon time, and Killip class.

Tizón-Marcos et al. do not provide information on the calibration or discriminative accuracy of the adjustment models or the odds ratios for the risk factors considered, apart from female sex. Therefore, the validity of the adjustment models cannot be used to discriminate between the contradictory results in their study (model 1 vs models 2 and 3).

It is also important to note that the decision to adjust for an independent variable unrelated to the characteristics of patients stratified by sex will depend on the goal of the comparison. In the Tizón-Marcos et al. series, symptom onset to balloon time was considerable (231 min) and was significantly shorter among men than among women (200 min [140-320 min] vs 231 min [160-375 min]; $P = .001$). When the intervention was included as an adjustment factor in the model, the authors compared results between men and women after adjusting the dependent variable (mortality, for example) for the intervention, not for sex-attributable differences in STEMI treatment, which was the objective of our studies.

Another methodological issue with the study by Tizón-Marcos et al. that merits reconsideration is the pair-matching method used. The authors indicate that they matched by age ($\pm$ 2 years), and the selected adjustment parameters were diabetes, symptom onset to balloon time, registry year, and the treating hospital. The authors do not explain why they did not use robust pair-matching methods such as propensity score matching, and neither do they offer any information regarding the matching rate achieved in their adjustment models, either in the Methods section or in the supplementary data.

Finally, the authors conclude that "30-day mortality in women was similar to the rate in men". However, they also state that "crude 30-day mortality was significantly lower in men than in women" (5.1% vs. 9.9%; $P = .001$) and yet present no calculation of risk-adjusted mortality. We deduce that they are referring to the mortality risk expressed by the odds ratio for female sex in the adjustment models they used; however, the results of these models are contradictory, and the authors do not explain why they selected one model rather than another in reaching their conclusions.

Therefore, aside from the differences in study design, we see nothing in the analysis by Tizón-Marcos et al. that would allow us to determine the degree to which their conclusions might conflict with our findings.

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