Letters to the Editor
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Safe, sensible, sagacious: responsible scanning of pacemaker patients

We were quite concerned reading the recent editorial by Dr Edward T. Martin. Some statements are erroneous and some recommendations made are potentially dangerous if followed by the readers of the European Heart Journal.

As earlier, Martin overstates the problem of denying device patients MRI. The cited study by Sakakibara and Mitsui does not report that ‘17% of patients with pacemakers were denied MRI in the previous year’ as stated by Martin. Rather, Sakakibara and Mitsui merely asked, ‘Have you ever had any medical problems for which MRI was recommended?’ 17.2% of the respondents said, ‘yes’.

Echoing Gimbel, Martin notes, ‘no deaths have been reported during physician supervised MRI procedures.’ Martin’s statement is simply not true as at least four pacemaker patients have died while undergoing physician supervised MRI. Importantly, Bartsch et al. also note, ‘in all four cases the patients were not pacemaker dependent.’ Thus, comments made by Martin (and previously Gimbel, made prior to the publication of Bartsch) are untrue. Physicians interested in performing MRI on device patients should note this important fact.

Martin observes that approximately 300 pacemaker patients have safely undergone MRI, although ‘there have been 10 deaths attributed to MR and pacemaker interactions.’ In recommending strategies to facilitate safe MRI of device patients, Martin leaves the pre-MRI reprogramming of the device as optional. This is a (non-)strategy, presumably based on the fortunate outcome of the 54 pacemaker patients who underwent MRI with minimal or no device reprogramming under his direction. In this EHJ editorial, however, Martin notes that reprogramming the pacemaker to ‘sub-threshold’ pacing outputs or ‘off’... ‘would diminish the potential for the rare episode of ventricular fibrillation.’ If Martin does feel it reduces the risk to the device patient undergoing MRI, we cannot understand why such a simple reprogramming strategy would not be clearly recommended? Patients have a reasonable expectation that a diagnostic study without therapeutic benefit such as an MRI scan will not cause serious harm to them. Is it not our obligation to make procedures as safe as possible for patients? It should be noted that virtually all investigators except Martin et al., recommend substantial device reprogramming prior to MRI. The frequent occurrence of ‘demonstrated magnet-mode behaviour during the MRI’ as reported by Martin et al. should not be viewed as benign.

We were puzzled that even after recently responding to a ‘Letter to the Editor’ in the Journal of the American College of Cardiology reporting, among other issues, the unreliability of ECG monitoring during MRI, Martin continues to state that ‘pulse oximetry monitoring is not necessary’ during MRI of a device patient. Given the seriousness of the potential complications (death) and the unreliability of ECG monitoring during MRI, we remain perplexed why such a simple safety measure is not recommended as has been by others.

Martin’s mystifying final recommendation to facilitate safe MRI of device patients is ‘scan modern pacemakers (manufactured after 2000).’ Nevertheless, in their recently reported series (as well as all the pacemaker patients cited in support of his recommendation in Table 1), virtually all of the patients safely scanned by Martin et al. had devices manufactured before 2000. Although not cited specifically, the recently published work by Roguin et al. is likely the source of this recommendation. Martin’s audience should be reminded that no device manufacturer has claimed MRI device compatibility, irrespective of the manufacture date of the device. Despite a manufacturer’s bold announcement of a ‘new and improved device’, with a different shape and a clever naming prior to MRI, the unreliability of ECG monitoring during the MRI remains as reported by others. Martin leaves the pre-MRI reprogramming of the device as optional. This is a (non-)strategy, presumably based on the fortunate outcome of the 54 pacemaker patients who underwent MRI with minimal or no device reprogramming under his direction.

After reading Martin’s editorial, we are reminded of the final thoughts in Achenbach et al.: ‘Carelessness or reduced awareness of the potential dangers could cost a patient’s life.’

References


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Limitations of the study evaluating fibrinolytic therapy and in-hospital PCI for ST-elevation myocardial infarction

We read with interest the article by McClelland et al., titled ‘Percutaneous coronary intervention and 1-year survival in patients treated with fibrinolytic therapy for acute ST-elevation myocardial infarction’. We would like to congratulate the authors on their study, but would like to highlight a few limitations that may have influenced the study findings.

The 154 patients treated by in-hospital percutaneous coronary intervention (PCI) in the study received fibrinolytic therapy more quickly compared with those who had no in-hospital PCI; the median pain-to-needle time was significantly shorter in those who later underwent PCI than those who did not. This would have clearly influenced the outcome, as those in the PCI group were likely to have salvaged more myocardium. The median time difference between the two groups was 1.4 h and when we consider that ‘minutes means muscle’, 1.4 h implies a lot of muscle. A recent systematic review article by Gersh et al., suggests that patients presenting within the first 2–3 h of symptom onset benefit most in terms of myocardial salvage by undergoing prompt reperfusion therapy. This being the case, those in the PCI group of the study by McClelland et al., would have benefited more, not necessarily because of in-hospital PCI but rather because of more prompt reperfusion with fibrinolytic therapy with a resultant bias favouring the PCI group.

A second factor that may have favoured the PCI group was also eluded in the accompanying editorial by Danchin. The majority of patients in the PCI group went to the cardiac catheter lab >24 h after admission (59%). This means that patients in the PCI group had survived at least up till the point of being taken to the cath lab. Those who died very early on in their admission may not have been taken to the cath lab because of this delay and would have been counted within the non-PCI group, resulting in bias favouring the PCI group. McClelland et al. do not provide any data on the usage of platelet glycoprotein IIb/IIIa inhibitors in their study. The combination of fibrinolitics with these agents has raised concerns about bleeding risks of subsequent PCI, but there is a paucity of data. However, clarification concerning the use of such agents would provide the reader with useful information.

We once again congratulate the authors on their study of this important topic, but highlight some limitations that may have affected its findings.

References

