

CORRESPONDENCE

Letters to the Editor

Stress Cardiomyopathy

We were interested to see the report of Hurst et al. (1) describing 4 women with stress (tako-tsubo) cardiomyopathy and systolic “midventricular ballooning.” As noted by the investigators, this left ventricular (LV) contraction pattern differs from that of many other patients with stress cardiomyopathy because the distal portion of the chamber at the LV apex demonstrates a normal contraction pattern (apical sparing). Indeed, in our initial report of women with stress cardiomyopathy we also reported normal contraction of the apical LV segment in 9 of 22 patients (41%) based on cardiac magnetic resonance imaging (MRI) (2). In addition, Abdulla et al. (3) also recently reported apical sparing in 14 of 35 patients (40%) with stress cardiomyopathy. Therefore, this particular reversible pattern of abnormal LV contraction is very common in stress cardiomyopathy, and it may well have been an overestimation on the part of Hurst et al. (1) to regard this form of the condition as a novel variant.

Conversely, such patients clearly represent a subset within this disease spectrum, although of uncertain mechanism and clinical significance at this time. This diversity of phenotypic expression would, however, underscore the superiority of the term “stress cardiomyopathy” to describe this diverse entity rather than the ultimately confusing “midventricular ballooning” or “apical ballooning syndrome” (4–6). At this relatively early juncture in the evolving description of stress cardiomyopathy, application of clear and consistent nomenclature seems essential.

***Scott W. Sharkey, MD, FACC**
John R. Lesser, MD, FACC
Martin S. Maron, MD
Barry J. Maron, MD, FACC

*Minneapolis Heart Institute Foundation
920 East 28th Street, Suite 60
Minneapolis, Minnesota 55407
E-mail: scott.sharkey@allina.com

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Reply

We appreciate the interest of Dr. Sharkey and colleagues in our report on transient midventricular ballooning of the left ventricle (1). Even after careful review of the study by Sharkey et al. (2), we are unable to find any evidence to substantiate their statement that “we also reported normal contraction of the apical LV [left ventricular] segment in 7 of 17 patients (41%) based on cardiac MRI [magnetic resonance imaging].” To the contrary, in their study the investigators state “*All* [our emphasis] exhibited a large wall-motion abnormality that involved akinesia or hypokinesia of the distal one-half to two-thirds of the LV chamber, which created a distinctive ‘apical ballooning appearance’.” Accordingly, we are unable to explain the discrepancy. The report by Abdulla et al. (3) was published after our study was submitted, making it impossible to have previously acknowledged.

Although the assertion that “this particular reversible pattern of abnormal LV contraction is very common in stress cardiomyopathy” may prove to be true, we did not comment on the prevalence of transient midventricular ballooning in our study. In fact, it would be anticipated that recognition of this midventricular variant would increase through a heightened awareness of transient ballooning syndrome, and this has proven correct as demonstrated in the report by Abdulla et al. (3), the recent MRI image from Steen et al. (4), and a case report by Shimizu et al. (5). We believe the “novel” aspect of the cases was recognizing the implications in determining the etiology underlying transient ballooning syndrome rather than the rarity of the occurrence.

The naming of this syndrome may be one of personal preference; however, we would suggest that “transient ballooning syndrome” as a descriptive nomenclature seems most appropriate. “Stress cardiomyopathy” implies a cause-and-effect relationship that, at present, has not been fully elucidated. Stress is ubiquitous, yet an associated transient cardiomyopathy is not!

***R. Todd Hurst, MD**
Christina S. Reuss, MD
A. Jamil Tajik, MD
J. Wells Askew, MD

*Mayo Clinic Arizona
Cardiovascular Diseases
13400 East Shea Boulevard
Desk 3A
Scottsdale, Arizona 85260
E-mail: hurst.todd@mayo.edu

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Contrast Nephropathy: Isosmolar and Low-Osmolar Contrast Media

We read with interest the meta-analysis by McCullough et al. (1) regarding the lower incidence of contrast-induced nephropathy (CIN) in patients who received isosmolar contrast medium (IOCM) iodixanol, as compared with those who received low-osmolar contrast media (LOCM). Nevertheless, we believe the study presents some important methodological limitations that could reduce its value.

In the meta-analysis, the greater part of the patients (789 of 1,345) included in the group receiving LOCM were given an ionic contrast medium (CM), and only 69 patients received iopamidol, the contrast agent that, according to recent data, seems to be the safest of the LOCM (2,3). Therefore, the results of the meta-analysis could derive from the small number of patients receiving iopamidol in the LOCM group, rather than to the renal safety of isosmolar iodixanol. Moreover, apart from the small meta-analysis by Clauss et al. (4) comparing the nephrotoxicity of the IOCM iotrolan with different types of LOCM (iopamidol, iopromide, and iohexol), the previous major comparative studies supporting the possible safety of IOCM have been performed only between iodixanol and the monomer iohexol (5-7), which is found to be one of the CMs most responsible for CIN (2,3). Thus, at present, we do not perceive any definitive evidence of the presumed advantage derived from the use of IOCM in comparison with all of the LOCM (8).

In addition, the investigators themselves note that only 18.3% of patients included in the meta-analysis had their final creatinine (Cr) values measured on day 3 or later (1), whereas CIN is defined as an increase of serum Cr levels of 0.5 mg/dl (or 44 μ mol/l) or a 25% or greater relative increase from baseline 48 to 72 h after a diagnostic or interventional procedure requiring CM administration (9). We would like to understand how the researchers completed Table 4 in their study summarizing the incidence of CIN occurring within 72 h if only 18.3% of patients have their Cr values recorded on day 3.

Finally, as underscored by the investigators (1), another important bias could be identified in the lack of data relative to the amount or type of intravenous hydration prophylactic protocol given before and after CM administration, which could influence the outcomes of each trial (10).

We believe that prospective, double-blind, randomized, controlled trials comparing iodixanol with all LOCM would be necessary to confirm the results of this meta-analysis.

***Simona Detrenis, MD**
Michele Meschi, MD
Giorgio Savazzi, MD

*Department of Internal Medicine and Nephrology
University of Parma
Via Gramsci, 14
I-43100 Parma
Italy
E-mail: simonadts@libero.it

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Reply

Dr. Detrenis and colleagues point out that only 69 patients in the low-osmolar contrast media (LOCM) group received iopamidol and suggest it may be a special case among LOCM agents. As a LOCM, iopamidol has an osmolality of 796 mOsm/kg H₂O (Isovue, Bracco Diagnostics, Princeton, New Jersey; 370 mg iodine/ml, viscosity at 37°C, 9.4 Cp), which is very similar to iohexol (844 mOsm/kg H₂O, Omnipaque, Amersham Health, Princeton, New Jersey; 350 mg iodine/ml, viscosity at 37°C, 10.4 Cp) (1,2). Both of these agents are nonionic monomers. By contrast, iodixanol, an iso-osmolar contrast medium (IOCM), is a nonionic dimer with an osmolality of 290 mOsm/kg H₂O (Visipaque, Amersham Health, 320 mg iodine/ml, viscosity at 37°C, 11.6 Cp) and when compared to the nonionic LOCM monomers in our study had a significantly lower rise in serum creatinine (Cr) after contrast exposure ($p = 0.001$) (3,4). Although iopamidol and iohexol are nearly identical in their physicochemical properties as