Letters to the Editor

The Editor welcomes submissions for possible publication in the Letters to the Editor section that consist of commentary on an article published in the Journal or other relevant issues. Authors should:

- Include no more than 500 words of text, three authors, and five references
- Type with double-spacing
- See <http://jtcvs.ctsnetjournals.org/misc/ifa.html> for detailed submission instructions.
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Letters commenting on an article published in the JTCVS will be considered if they are received within 6 weeks of the time the article was published. Authors of the article being commented on will be given an opportunity to offer a timely response (2 weeks) to the letter. Authors of letters will be notified that the letter has been received. Unpublished letters cannot be returned.

Indications for gadolinium for coronary angiography

To the Editor:

We read with great interest the article by Nicolosi and associates¹ in the July 2002 issue of the *Journal* dealing with the beneficial effects of gadolinium on regional stunning in the canine heart in vivo.

Gadolinium is currently used to enhance magnetic resonance imaging and is known to be well tolerated by the kidneys. We² recently reported the first clinical use of gadolinium for coronary angiography in a patient with elevated serum creatinine. We are currently conducting a prospective randomized trial to evaluate the exact role as well as the safety of gadolinium for coronary angiography in patients with abnormal renal function. Although none of our patients had any enzymatic or electrocardiographic changes in relation to myocardial damage, we are still wondering what the exact effects of this drug are on the myocardium. The article by Nicolosi and colleagues suggests that gadolinium is beneficial for the myocardium. This article, together with the promising results of our ongoing trial, might pave the way toward future indications for gadolinium for coronary angiography.

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Reply to the Editor

In response to the letter to the Editor regarding our article,¹ we find the notion of using a gadolinium-based contrast agent for coronary angiography intriguing, particularly in the context of acute coronary syndrome. Our study documented the salutary effect of gadolinium chloride (GdCl₃), administered before an ischemic insult, on contractile function after 3 hours of reperfusion. There was no observed effect of gadolinium on a load-independent index of contractility in the baseline state, nor were there effects on heart rate or cardiac output. The mechanism(s) by which gadolinium accelerates recovery of contractile function in stunned myocardium are yet to be determined. Its may modulate ischemia/reperfusion-induced pathophysiology of intracellular ions, particularly Ca²⁺, via effects on stretch-activated ion channels, and/or it may directly attenuate superoxide production. These effects, however, have been observed only with the use of GdCl₃. The gadolinium chelates used as contrast agents for magnetic resonance imaging and used by Sarkis and associates³ for coronary angiography are generally eliminated by the kidney unmetabolized. These agents do not bind plasma proteins, nor are they thought to react with biologic structures. One cannot assume that these chelates would exert effects similar to those of GdCl₃. Further studies of gadolinium chelates would therefore be necessary before considering them for possible dual roles as both contrast agents and protective agents in coronary artery disease.

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Risk adjustment in analysis of surgery for congenital heart disease To the Editor:

The article by Jenkins and associates,¹ in the July 2002 issue of the *Journal*, is clearly an important advancement in risk adjustment when analyzing mortality outcome in the field of surgery for congenital heart disease. Further statistical analysis is needed in centers with high case volumes as the RACHS (Risk Adjustment in Congenital Heart Surgery) methodology evolves.

The concern regarding the large spectrum of mortality among risk categories 2 to 4, with some institutions displaying a threshold increase or decrease in mortality as higher-risk procedures, is disturbing for a potential severity model. This mismatch of observed/expected ratios is not surprising for severity scores in surgery in general.² Several factors involved that stem from inherent surgical practice and original logistic regression model of 5 variables were not discussed in the article.

The inherent surgical practice could be divided into physiologic and operative features. The operative variables, including in-hospital redo cases and estimated blood loss, should be evaluated. The first can have a dual knock effect on mortality. The estimated blood loss factor can have direct effects by contributing to child hypoxia and later the side effects of blood transfusion. Multivariate preoperative physiologic variables were not described in the original article by Jenkins and coworkers,³ for example, hemoglobin, plasma sodium, and potassium. Recently, a hemoglobin concentration of 100 g/L or less had a 5-fold higher in-hospital mortality rate than those with higher concentrations.⁴

A widely recognized European severity score, POSSUM (a Physiological and Operative Severity Score for the enUmeration of Mortality and morbidity), used in vascular and colorectal surgery, is a simple scoring system incorporating only a total of 18 variables, of which preoperative electro-

lytes determined outcome.⁵ There may be room for more specific and relevant variables to be included in RACHS-1.

Another aspect that could lead to the diversity of outcome in the intermediate risk category factors is whether a linear versus exponential model was adopted in the original equation. A linear method of analysis of the risk for each mortality group is artificially taken to the median as opposed to the mean in that risk category group. Although it may apply well to high-risk patients with smaller *n* values, it may overpredict death in the low-risk population, as was illustrated in centers A, F, O, S, and U in category 2 versus 3 and 4. Reanalyzing the data may produce different trends.

It is possible that these factors were considered; however, a discussion of these factors would be necessary for the reader. This would reduce chance-related outcome and might revisit the question regarding institutional inquiries, especially with regard to assignment of certain types of cases to specific surgeons and location of post-operative care.

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Ranking institutions To the Editor:

Jenkins and Gauvreau¹ illustrated the use of a novel risk adjustment method in congenital heart surgery and chose to present their results largely in terms of institutional rankings. However, ranks are a notoriously inaccurate comparator for performance—someone always has to be bottom and top of a league table, no matter how much the play of chance may have contributed to their performance. Figure 1 shows the risk-adjusted standardized mortality ratios (SMRs) with 95% confidence intervals, as ranked by Jenkins and Gauvreau¹ according to outcomes from 22 institutions in 1996. We first note that a formal test that all the centers have SMRs of precisely 1 is barely significant ($\chi^2 = 35.6$, $df = 22$, $P = .03$, after transformation of all values to power 0.3 to bring to approximate normality), so there is not even strong evidence of any heterogeneity among centers. We can also estimate the “true rank” of each center. This requires the methodology described by Marshall and Spiegelhalter,² in which the “true SMRs” are repeatedly simulated from the confidence intervals in Figure 1 and then ranked at each iteration of the simulation. The resulting estimated true ranks and their 95% confidence intervals

TABLE 1. Probabilities of being “true best” and “true worst” centers for the 8 highest and lowest ranking centers

Center	Probability that “true best” center	Center	Probability that “true worst” center
C	0.27	J	0.01
D	0.16	Q	0.01
H	0.20	L	0.06
E	0.06	A	0.15
M	0.03	O	0.09
F	0.11	N	0.05
G	0.00	P	0.27
S	0.12	B	0.21