low-up and has rehabilitated himself by performing social work within the prison walls. This case persuaded me to research the law and resulted in a publication in the *Journal of Indian Law Institute*.² The subject was capital punishment and medical science.

In the case referred to in the editorial, it appears that medical professionals and legal professionals can come together and seek an alternative where valuable human resource is not wasted in the hope of retribution. I am referring to the large number of prisoners waiting a death sentence in US prisons. They could be potential organ donors for the community. With the assistance of the medical profession, each prisoner who is to be executed can provide organs for 8 to 10 patients.

This would certainly appeal to society at large. The article published in the *Journal of Indian Law Institute*² has interested the law commission in India, which is considering this suggestion. However, capital punishment itself is rarely given out in Indian courts. Perhaps this thought may be worth a closer look in your community, where the number of convicts on death row is large.

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Reply to the Editor:

Readers will be grateful for the informative letter from Dr Sampath Kumar, who raises two interesting themes for ethical analysis. The case that he reports shows how changes in contextual details can clarify values and illuminate moral reasoning developed from analysis of an index case, like our heart transplant patient. This approach

to ethics is referred to as casuistry. In his case, the patient was not just in prison but was on death row, intensifying the apparent strength of the argument from justice to withhold treatment because of the seriousness of the crime. The treatment was an expensive valve operation, although it did not consume as scarce and as valuable a resource as a donor heart. The principle that guided his decision was that prisoners are our fellow citizens and fellow human beings, to whom physicians have a fiduciary duty to provide care within the boundaries of resource availability. It is enriching that he can give us the outcome, underlining the appropriateness of his decision to apply this principle.

The second theme that he raises is the issue of allowing prisoners to be organ donors. The voluntary or involuntary removal of vital organs after execution in China is an efficient but potentially abusive solution to the donor shortage. Clifford Bartz, a federal inmate in Pennsylvania, described the Inmate Organ Donor Network in the March issue of the *Kennedy Institute of Ethics Journal*.¹ Bartz and his colleagues propose that qualified inmates who pledge up to three organs on their death should be given 1 year of suspended prison time, and those who serve as living donors should receive 7 years of reduced time. He describes several voluntary donations of organs from prisoners to their needy relatives. These are examples of a form of trade, trading time for organs. In my Minnesota case of the prisoner who wanted to collect "rent" for his donated kidney, described in my reply to Dr Richenbacher's letter,² this proved to be a Faustian bargain, but this single example should not lead to a categorical ban. I recommend a more empirical approach to test the optimistic hypothesis that under the best circumstances, prisoner organ donation can be an act of heroism leading to trust and rehabilitation of a fallen comrade on the road of life.

We need rewards for donation. As a blood donor, I favor some advantage within the health care system for those who enrich it by donation. This might parallel the airlines' policy of rewarding frequent flyers for their loyalty with expedited boarding and free air travel on some under-filled flights. Blood donors, and particularly organ donors, deserve reasonable rewards within the health care system, as

long as they do not unfairly disadvantage other patients. Such a reward system would strengthen the value assigned by society to the act or intention to donate.³ Finally, the use of financial rewards should be explored as even Veatch,⁴ a longtime opponent of this policy, has grudgingly come to accept. The Council on Ethical and Judicial Affairs of the American Medical Association, stimulated no doubt by Bob Sade's presence on the council, has recommended that an empirical trial of financial rewards for organ donors should be conducted to determine whether it will favorably or unfavorably affect the overall rate of donation or reduce the donor pool.⁵

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Bivalirudin as alternative to both danaparoid and heparin in off-pump coronary artery bypass grafting To the Editor:

I enjoyed the recent article "Heparin Versus Danaparoid in Off-Pump Coronary Bypass Grafting: Results of a Prospective Randomized Clinical Trial" by Carrier and colleagues.¹ I favor off-pump coronary artery bypass grafting in patients with heparin-induced thrombocytopenia (HIT). Did Carrier and colleagues¹ consider using a short-acting thrombin inhibitor such as bivalirudin (Angiomax) rather than danaparoid as the heparin alternative?

I have used bivalirudin,² a short-acting thrombin inhibitor, in several patients with

HIT who have required valve replacement. Bivalirudin's half-life of 25 minutes, the ability to monitor activated clotting time, and the lack of cross-reactivity with heparin antibodies were advantages that convinced my group to use it for these patients with HIT. The patients did well, requiring an average of 2 units of blood per patient.

My suspicion is that off-pump revascularization with bivalirudin rather than danaparoid would result in lower use of blood products. It is likely that even on-pump coronary artery bypass grafting with bivalirudin as the anticoagulant will result in lower blood product use in the HIT cohort.

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Reply to the Editor:

I thank Dr Baciewicz for his comments regarding our article and thank you for the opportunity to respond to him. We chose to compare low dose of the anti-Xa inhibitor danaparoid with standard heparin in off-pump coronary artery bypass grafting because of our significant clinical experience with the former drug. Although it is not superior to standard heparin, low-dose danaparoid offers a safe alternative for patients undergoing off-pump coronary artery bypass grafting when heparin is contraindicated.

My group and I have read with interest the reports on bivalirudin, but we remain concerned by the limited clinical data available. In fact, we found only two reports of cardiac surgical patients in the literature, with one of the patients showing a large blood drainage through the chest tubes.^{1,2}

Our current options for patients with heparin-induced thrombocytopenia are to use low-dose danaparoid and off-pump coronary artery bypass grafting or to wait for disappearance of the antiplatelet antibodies and use a standard protocol of heparin.

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Apical versus basal partial ventriculectomy

To the Editor:

With regard to the recent study of Koyama and colleagues published in the *Journal of Thoracic and Cardiovascular Surgery*,¹ your readers should be aware that the authors have done no more than confirm the previous observations of Savage and colleagues,² namely, that reducing volume by constricting or resecting the apical half of the ventricular cone has no significant effect on ventricular pump function. Their findings also confirm extensive clinical studies³⁻⁶ showing that reduction of the radius of the ventricle in its upper two thirds improves pump function, provided that the ventricle had been markedly dilated prior to such an intervention. Thus, simply by using a figure-of-eight symmetrical technique of resection, Konertz and colleagues⁵ have achieved results that surpass the current success of cardiac transplantation.

Your readers should also note that the technique used by the Japanese group to induce cardiac failure¹ produced minimal alterations in left ventricular function. Thus, having reduced the radius of the left ventricle, the diameter in their experimental study was smaller than under control conditions. As is well established, how-

ever, any persistent therapeutic effect of reducing ventricular radius in the clinical situation is dependent in the degree of pre-existing ventricular dilation.³⁻⁶

Furthermore, due to the beta-blockade used as part of the experimental setup, the ensuing bradycardia will have prevented their hearts from compensating adequately for the confined stroke volume, as would have occurred under physiological conditions simply due to an increase in heart rate. It is irrelevant, therefore, to measure cardiac output under these experimental conditions. At all events, stroke volume increased by one quarter when the radius was reduced along the basal two thirds of the heart.

There are then several other problems with the description and interpretation of the Japanese group¹ that need to be drawn to the attention of your readers. It is incorrect to state that Batista advocated the apical region of the left ventricle as a primary area for resection. His primary intention was simply to reduce the radius of the dilated left ventricle. Furthermore, contrary to the conclusion drawn by the authors,¹ the experimental results show clearly that plication of the apical segment produces no positive therapeutic effect. Nor, contrary to the assertions made by Torrent-Guasp and his colleagues,^{7,8} does such plication have any major detrimental impact on global ventricular pump function. When considering the differences observed in left ventricular function after apical or subbasal reduction of radius in this study, we need to remember that it is the extensive circular muscular layer enclosing the upper two thirds of the left ventricular cone that is largely responsible for left ventricular ejection. By reducing its radius, working conditions for the left ventricle are improved, thus ameliorating its pump function. This positive effect, however, is mitigated by plication of the interpapillary segment as performed by the Japanese investigators, as this procedure plicates also the marginal arteries. As has been shown,⁹ the resulting ischemic damage extends well beyond the plicated segment. Indeed, such collateral damage may well have been more significant in those hearts that were plicated up to the base in the Japanese study as compared with those plicated only along the apical half. The positive effect of reducing left ventricular radius probably would have been more pronounced had a less traumatic