indicated, the intimal injury caused by the tip of the guiding catheter carries a high likelihood of being directed retrograde, and thus the flap can potentially close and seal under antegrade aortic flow pressure. Although the dissection may progress antegrade, a report of iatrogenic AD after cardiac catheterization found that most of the dissections were limited to the right coronary sinus, and there was a favorable outcome with conservative treatment.⁵ In support of their management decision, Timek and colleagues¹ cited an observational series of 10 medically treated patients with AD diagnosed after an interval as long as 12 years (mean 52.6 months) after initial cardiac surgery (half of them for aortic pathologies).² These patients simply represent a surviving selection of an unknown number of affected cases. In the absence of a denominator, this is insufficient evidence to define retrospectively a “paradigm change” in the management of an acute (potentially life-threatening) vascular complication. Late survivors of AD with aortic wall pathology and open false lumen must be distinguished from those with catheter-induced local aortic injury. Those patients with previous CS who are in hemodynamically stable condition may expect an excellent outcome with nonoperative management.^{1,5}

The decision for surgical or nonsurgical therapy remains challenging in patients with other causes of dissection. The 10-year survival with surgical treatment was reported as 68%, which was not significantly different from that for spontaneous AD.⁴ In the absence of sufficient data to estimate the prognosis of medically treated AD in patients with previous CS, the decision will remain difficult, particularly in the case of relatively young patients.

Olaf Stanger, MD, PhD
John Pepper, MD, PhD
Cardiothoracic Surgery

Royal Brompton and Harefield NHS
Trust
London, UK

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Reply to the Editor:

I appreciate the thought-provoking arguments that our case report has generated. Stanger and Pepper present a balanced discussion of the controversial subject of surgical intervention in patients with acute type A aortic dissection (TAAD) with previous cardiac surgery. I agree that patients with previous cardiac surgery and spontaneous TAAD should be treated surgically in the absence of prohibitive comorbidities. These patients do have significantly higher operative mortality, however, as illustrated by a study published since the acceptance of our case report. Klodell and colleagues¹ reported a disparity in hospital mortality of 9.7% versus 38.7% between primary sternotomy and re sternotomy among patients treated for acute TAAD in a propensity score-matched analysis. Interestingly, patients with previous surgery had a significantly higher incidence of aortic rupture, contained in most cases, suggesting that adhesions from previous surgical procedure may not provide “buttressing” after all against this catastrophic complication. Medical therapy

for TAAD has been associated with 58% mortality in a large registry experience,² and as such outcomes of surgical therapy in re sternotomy TAAD appear favorable, although patients chosen for medical therapy usually have a prohibitive risk profile. The patient described in our report was a young woman with stable hemodynamics and a catheter-induced TAAD. We agree that this represents an important distinction, because the mechanism of aortic injury in this setting may be more prone to healing than a spontaneous TAAD and the underlying aortic pathology may be less significant; however, clinical vigilance, close observation, serial imaging and echocardiographic assessment, and a low threshold for operative intervention remain paramount. Because scant data from the literature are available to guide the treatment of TAAD in patients with previous cardiac surgery, clinical judgment and local experience will most likely continue to determine the mode of therapy.

Tomasz Timek, MD
West Michigan Cardiothoracic
Surgeons
Meijer Heart and Vascular Institute
Grand Rapids, Mich

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REMOTE ISCHEMIC PRECONDITIONING IN CHILDREN WITH CYANOTIC HEART DISEASE: LOST IN TRANSLATION?

To the Editor:

I read with great interest a recent report of Pavione and colleagues¹ that

assessed the *late* remote ischemic preconditioning (RIPC) in 22 children who underwent cardiac surgery with cardiopulmonary bypass. The authors concluded that “late remote ischemic preconditioning did not provide clinically relevant cardioprotection to children undergoing cardiopulmonary bypass.” A few comments seem to be appropriate. It appears that at least 5 children in their RIPC group had cyanotic heart disease (Taussig-Bing anomaly, tricuspid atresia, and tetralogy of Fallot). Inasmuch as the 2 groups in this study appear comparable, the children with cyanotic heart disease have chronic hypoxia before surgery. Is it possible to further precondition the patients with chronic hypoxia by brief intermittent episodes of limb ischemia induced before surgery? This question remains to be answered.

My colleagues and I² have previously demonstrated in a randomized controlled trial a clinically relevant beneficial effect of the RIPC in children who underwent preconditioning immediately before surgery. Pavione and colleagues used the same RIPC stimulus to assess a *late* phase of protection. We^{3,4} have previously demonstrated that the RIPC modifies gene expression both early and late in human blood and in the myocardium of the experimental animals. Furthermore, it appears that intraoperative ischemia-reperfusion itself induces an early myocardial gene expression in the children undergoing heart surgery.⁵ Thus, both intermittent preoperative (ie, RIPC) and prolonged intraoperative ischemia-reperfusion induce changes at the genomic level. One would expect that chronic hypoxia of the cyanotic heart disease does the same. Do these genomic changes translate into proteomic response? This is yet unknown. Our recent, yet unpublished, randomized controlled trial performed in 40 children undergoing surgery for tetralogy of Fallot indicated that the myocardial phosphorylated protein signaling is already activated in children with chronic hypoxia. Thus, regardless of whether the

RIPC-induced activation of gene expression translates into proteomic response or not, its clinical effect may be lost in children with cyanotic heart disease.

I commend Pavione and colleagues on their interesting study and look forward to seeing whether the RIPC will find a clinical application in children undergoing heart surgery. Time will show. At the meantime, the researchers should be cautioned against including patients with chronic hypoxia into the trials inasmuch as chronic hypoxia may mask the effects of the ischemic preconditioning.

Igor E. Konstantinov, MD, PhD
Cardiac Surgery Unit
Royal Children's Hospital
Murdoch Children's Research
Institute
University of Melbourne
Melbourne, Australia

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Reply to the Editor:

We thank Dr Konstantinov¹ for his interesting comments on our article.²

We do not, however, believe that the presence of patients with cyanotic heart disease in the group undergoing remote ischemic preconditioning (RIPC) in our study may have masked the effects of the ischemic stimulus, because there were actually 9 patients with cyanotic heart defects in the RIPC group (75%) and 6 in the control group (60%; $P = .65$). In addition, preoperative oxygen saturations were similar in the RIPC and control groups (mean $\pm$ SD, $89\% \pm 11\%$ vs $89\% \pm 7\%$, respectively; $P = .84$). We emphasize that studies performed in adults undergoing coronary artery bypass surgery³ and in infants with ventricular septal defect,⁴ in which cases chronic hypoxia would not be a confounding factor, also failed to demonstrate any clinically significant effect of RIPC. Moreover, the usefulness of RIPC would be most important in patients at high risk of postoperative complications, such as those included in our study. Nevertheless, many infants with complex congenital heart disease have some degree of preoperative cyanosis. Considering that RIPC is an innocuous, inexpensive, and simple procedure, we hoped that it would decrease inflammation and postoperative morbidity. Unfortunately, we did not observe these effects. It thus seems evident that RIPC with brief periods of limb occlusion, as currently performed, lacks clinical applicability in patients undergoing heart surgery with cardiopulmonary bypass.

Ana P. C. P. Carlotti, MD^a
Fabio Carmona, MD^a
Marcos A. Pavione, MD^a
Margaret de Castro, MD^b

^aDivision of Pediatric Critical Care
Medicine

Department of Pediatrics

^bDivision of Endocrinology

Department of Internal Medicine

Hospital das Clínicas

Faculty of Medicine of Ribeirão Preto

University of São Paulo

Ribeirão Preto, Brazil