Reply

Shinzo Takamori

Ann Thorac Surg 2003;76:978

come from compression by the assist cup. If the end-diastolic volume becomes smaller with compression, the cardiac work generated by the native heart also becomes smaller, although total cardiac work is unchanged. Because of work by the assist cup, myocardial oxygen consumption should be smaller. Therefore, when the end-diastolic volume becomes smaller as observed in in vivo hearts assisted by dynamic cardiomyoplasty, a reduction in oxygen consumption can be explained by the same mechanism.

Although we can assume myocardial wall stress is smaller with compression, it is not necessary to invoke Laplace's law because wall thickness is unchanged when the assist device compresses the heart. We simply recognize that total cardiac work generated is a summation of work done by the heart itself and work transferred to the heart by compression in synchrony with the heart beat. However, if the effect of wall thickness is as Drs Misawa and Fuse surmise, an oxygen-saving effect could be observed even with adynamic cardiomyoplasty. This effect may be an additional benefit of dynamic and adynamic cardiomyoplasty.

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MISCELLANEOUS

Optimal Thoracotomy Analgesia

To the Editor:

We read with interest the study by Takamori and colleagues [1]. We found the result of interest, although at the same time, it raises concerns about continued research in this area using similar methodology. In their study, epidural analgesia by 1.0% mepivacaine 4 mL/h starting during surgery and maintained for 5 days was successfully supplemented, in terms of pain scores over the first 3 postoperative days, with 8 mL of 0.25% bupivacaine internally injected alongside four intercostal nerves.

A number of points make interpretation difficult. It was not stated that correct epidural catheterization was confirmed, and no local anaesthetic bolus was given. We question, therefore, whether adequate epidural analgesia was in fact used, and this point may be reflected in the marked stress responses, measured with serum ACTH and cortisol, shown in both groups. Questionable quality pain relief in the presence of an acknowledged markedly painful surgical wound [2, 3], does set the scene for observation of the effect of additional analgesia.

For adequate thoracotomy surgical anaesthesia, it is likely that up to seven dermatomes need to be blocked [3]. As only four intercostal nerve blocks were done in these patients, it is likely that dermatomal coverage would have been suboptimal. We also wonder whether the quality of the intercostal block was adequate. This is because only bupivacaine 0.25% was used in a volume of 8 mL. This is a fairly safe amount to use, particularly taking into account the known hazards of direct injection into the neurovascular bundle with internal intercostal blocks in an open chest situation [4–6]. Efficacy would have been compromised not just through the use of a slow-onset local anesthetic and a relatively small volume of a weak solution, but also through initiation of the block well into the surgical procedure, through lack of a preemptive element.

Whatever afferent nerve blocking technique is used, it ought to be optimized, otherwise inherent risks are difficult to justify. The known hazards of cannulation of the thoracic epidural space need to be justified, and that may not have been done in this study.

In our own unit, we strongly recommend the use paravertebral nerve blocks from the points of view of efficacy and safety. We and others have compared paravertebral nerve blocks (using much higher doses of local anesthetic) with epidural nerve blocks and found them to be superior in all measurement parameters [7, 8]. Epidural nerve blocks are therefore reserved for the few situations where paravertebral blocks are contraindicated.

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Reply

To the Editor:

We thank Drs Richardson and Anikin for their comments on our report of intraoperative intercostal nerve blockade for postthoracotomy pain [1].

In the study, the anesthesiologist confirmed correct placement of the epidural catheter before thoracotomy, but did not give a local anesthetic bolus. Our rather high pain scores may reflect

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inadequate pain relief. Therefore, the aim of our study was whether the additional intercostal nerve blockade provided an additive benefit for pain relief.

The dermatomal coverage for thoracotomy was described as blocking up to seven dermatomes [2]. An antero-axillary incision in the study was between 15 and 25 cm in width. We performed four intercostal nerve blocks focusing on pain relief but not on dermatomic coverage.

Although we have no experience with paravertebral nerve blocks, their experience was excellent [3]. The method of paravertebral analgesia is an attractive alternative to epidural analgesia. We need a study comparing the two methods of analgesia where each technique is individually optimized.

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Induction Chemotherapy Followed by Concurrent Chemoradiotherapy and Operation in Locally Advanced Non–Small Cell Lung Cancer

To the Editor:

In the article by Cyjon and associates [1], induction chemotherapy (CHT) followed by chemoradiotherapy (CHT+RT) and surgical intervention in locally advanced non–small cell lung cancer achieved a median survival time of 16 months and a 3-year survival rate of 22%. The authors provided only survival data and not relapse-free survival data. Therefore, on the basis of statements such as "the pathologic specimen showed no evidence of residual tumor in 5 patients" and "at the time of analysis, 16 patients were alive, 9 of them with no evidence of disease," we are left with between 5 (9%) and 9 (16%) potential long-term survivors. This is an important issue, as these patients are at risk for the development of distant metastases, which would decrease overall results with further follow-up. Also, there was an 11% total mortality rate and substantial toxicity (grades III and IV neutropenia, 61% of patients, and grades III and IV anemia, 18% of patients). Such were the results in achieving no evidence of residual tumor in the pathologic specimens of 5 patients (9% of all patients and 17% of patients having complete resection. The latter (the better) figure is identical to that obtained using induction CHT followed by RT without operation as reported in a French multiinstitutional study [2], which also had much less toxicity.

Another option in these patients is exclusive concurrent CHT+RT, which is superior to RT alone [3, 4] or induction CHT followed by RT [5]. Although CHT+RT is frequently considered too toxic, in various prospective, randomized phase III trials, toxicity was largely dependent on CHT administration [3, 4, 6, 7]. Toxicity was very low with concurrent low-dose daily CHT (no grade 5 toxicity, and infrequent acute and late grade 3 or 4 toxicity [3, 4]. Many consider exclusive concurrent CHT+RT the standard treatment of patients with locally advanced non–small cell lung cancer. This therapy offers median survival times of 20 months or more and 5-year survival rates of 20% or higher [4–6], and provides a target with which to compare various current and future therapies.

Finally, Cyjon and co-workers, like others, believe the theoretical advantages of induction CHT to be (1) a reduction in local-regional tumor burden and (2) a lower systemic failure rate as a result of the increased chemosensitivity of micrometastases, which may have a faster growth rate and a higher proportion of cells in the S phase. Note that points 1 and 2 are in sharp contrast! If the log cell-killing nature of RT and CHT is correct, then point 2 is easier to achieve. The completely opposite situation (again theoretically) pertain at the local-regional level a bigger tumor burden and therefore less chemosensitivity, ie, increased chemoresistance, a slower growth rate, and a lower proportion of cells in the S phase). Therefore, point 1 becomes an impossible goal to achieve. Indeed, induction CHT followed by RT led to a lower rate of systemic failures (not local-regional ones) and improved survival compared with RT alone [2].

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