

Guidelines for Randomized Trials to Determine Incidence
of Perioperative Myocardial Infarction

To the Editor:—Although the study of Rao *et al.*¹ lacks adequate methods for prognostic matching of patients undergoing surgery following myocardial infarction (and, thus, cannot establish any advantage for aggressive, invasive, anesthetic techniques), their data provide useful guidelines for the design of the “prospective randomized trials” suggested by Lowenstein *et al.*² in the accompanying editorial.

Rao and colleagues emphasize the low risk of perioperative myocardial infarction (MI) in most patients undergoing noncardiac surgery within 25 months of MI. Thus, perioperative MI developed in only 1.9–7.7% of their 1097 patients. This low overall risk of MI resulted from the dilution of a small number of high-risk subjects by a much larger population of low-risk patients. Risk stratification appeared possible using readily identifiable, clinical variables, *e.g.*, surgery within 6 months of MI, cardiac failure, angina, or hypertension. For example, 12 of 28 (43%) Group 1 patients, and 4 of 14 (36%) Group 2, respectively, developing perioperative MI had had early surgery. Conversely, the risk of MI in early postinfarction patients was as high as 36%. Whether the decreased rate of perioperative MI in Group 2 patients is related to aggressive invasive monitoring and prompt hemodynamic therapies is open to question. Due to the smaller absolute and relative number of patients in Group 1 *versus* 2 operated within 6 months (42 of 364, 11.5% *vs.* 138/733, 18.7%, respectively), a few high-risk patients coincidentally may have contributed to the higher mortality of Group 1 patients. Clearly, randomization of early post-MI surgical candidates to conservative *versus* intervention-oriented anesthetic management would resolve this question.

Who should be randomized in such a trial? Rao *et al.* demonstrate (Table 7 in their article) that patients without congestive heart failure or angina *never* develop perioperative reinfarction, regardless of the anesthetic approach. Thus, the majority of patients undergoing post-MI noncardiac surgery are without such risk factors and need *neither* aggressive invasive anesthetic management nor trials of its efficacy. The advantage of these techniques for the smaller high-risk subset with obvious cardiovascular disease remains open to question. Based upon these observations, a prospective randomized trial restricted to this high-risk subgroup of post-MI surgical patients with obvious cardiovascular disease seems both ethical and necessary. Although such a study may require a multicenter cooperative trial for adequate sample size, the results of such a study finally would establish or refute the supposed therapeutic superiority of “sophisticated,” expensive, and potentially dangerous anesthetic techniques.

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2. Lowenstein E, Yusuf S, Teplick RS: Perioperative myocardial reinfarction: A glimmer of hope—a note of caution. *ANESTHESIOLOGY* 59:493–494, 1983

(Accepted for publication March 2, 1984.)

Were Differences in Reinfarction Rates due to Differences in Populations?

To the Editor:—We read with great interest the article by Rao *et al.* concerning reinfarction following anesthesia.¹ We wondered if many of the patients in the post-1977

cohort (Group 2) had undergone coronary revascularization between the time of their initial myocardial infarction and their participation in the study. Inclusion of

these patients, presumably at lower risk of reinfarction than nonoperated patients, would lower the rate of reinfarction. Exclusion of these patients means that the pre-1976 and post-1977 populations differ importantly.

Could these differences in population rather than differences in anesthetic technique, monitoring, or intervention account for the lower reinfarction rate?

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61:214, 1984

In reply:—In response to Rooke *et al.*, I would like to clarify that in our report, neither group included patients who sustained myocardial infarction and had a coronary revascularization procedure.¹ Patients in both groups sustained myocardial infarction and, either due to the emergency nature of the noncardiac operation or the patients' coronary status (inoperable distal lesions or no other lesions detected other than the lesion in the vessel supplying the infarcted area or lesions in other vessels too small to be bypassed), did not undergo coronary revascularization. Thus, patients in both groups are comparable and the difference in outcome between the two groups is *not* because of differences in population.

Anesthesiology
61:214–215, 1984

Complication of Fiberoptic Bronchoscope

To the Editor:—We wish to report a complication associated with the use of a fiberoptic bronchoscope. The patient was a 68-year-old woman with a tumor of the right lower lobe who was scheduled for bronchoscopy and right thoracotomy. The patient was induced with a balanced anesthetic technique and intubated orally with an 8.0 mm Hi-Lo National Catheter Endotracheal Tube.[®] The patient then was placed on the ventilator and a Portex Swivel Adapter[®] was interposed between the endotracheal tube and the breathing circuit.

The surgeon then proceeded with the fiberoptic bronchoscopy utilizing an Olympus[®] Adult BF type 4B2 fi-

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(Accepted for publication March 2, 1984.)

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(Accepted for publication March 2, 1984.)

beroptic bronchoscope, which had been lubricated with surgilube. The bronchoscope had just been returned from the company after repair. The bronchoscopy proceeded without incident until the surgeon began withdrawal of the unit. The bronchoscope had been withdrawn to approximately 13 cm when it suddenly became impossible to withdraw it any further. There was no change in circulatory parameters, the high-pressure alarm on the ventilator did not sound, but, a significant gas leak was noted where the bronchoscope passed through the Portex adapter. The cuff of the endotracheal tube immediately was deflated, and under direct vision the endotracheal