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In Reply:—We appreciate the opportunity to clarify some apparent confusion between what we demonstrated and what we then speculated. We demonstrated that in a population of patients with documented coronary artery disease, myocardial ischemia occurring before cardiopulmonary bypass (diagnosed as ≥1.0 mm of ST segment depression on a narrow-bandwidth SL-MON ECG system) was associated with an increased risk of postoperative myocardial infarction.[1,2] We next demonstrated that the ST segment recorded by the SL-MON narrow-bandwidth ECG system was consistently more negative than the standard diagnostic-bandwidth ECG (ECG-100) and as a consequence led to a diagnosis of myocardial ischemia twice as frequently as did the ECG-100.[3] These observations were supported by abundant data.

From these we speculated that myocardial ischemia diagnosed by the narrow-bandwidth SL-MON system had high sensitivity (more frequent diagnosis of ischemia) and high specificity, because: 1) the incidence of ST-segment displacement was directly related to heart rate; 2) ST-segment displacement responded to treatment by propranolol or nitroglycerin; 3) ST-segment displacement was a significant independent predictor of postoperative myocardial infarction; and 4) the magnitude of ST depression (including 1.0 mm) was directly related to incidence of postoperative myocardial infarction. We claim high specificity on this basis because there is no independent measure of myocardial ischemia applicable to this clinical situation for use as a "gold standard". Neither wall motion abnormalities nor coronary sinus lactate extraction possesses high specificity for myocardial ischemia. Without some "absolute" measure of ischemia, specificity of any of these indices cannot be calculated, but must be inferred.

We then speculated that the ECG-100 system and ≥1.0-mm ST depression criterion, standards adopted for primary diagnosis of ischemic heart disease by exercise testing, may be too stringent for the ECG diagnoses of myocardial ischemia *in patients with documented coronary artery disease in the perioperative setting.* If tested, a criterion of less than 1.0-mm ST depression using either the ECG-100 or the SL-MON in these patients and this setting may prove to be diagnostic of myocardial ischemia with high specificity by the specificity criteria listed above.

Conversely, we also speculated that when an ECG system with the narrow bandwidth of SL-MON is used for the diagnosis of myocardial ischemia *in patients not known to have coronary artery disease* (as reported for patients undergoing cesarean section), displacements of ≥1.0 mm may not be diagnostic of myocardial ischemia (false positive) because of all the physical and patient factors influencing the position of the ST segment described in our discussion.[3] These factors will always generate some unpredictable false positive results in some patients at some time.

Finally, we did not recommend that the SL-MON replace the ECG-100 in operating rooms. We merely noted that the reason for a narrow-bandwidth ECG in the operating rooms still existed (electrical noise), and when used for patients with documented coronary artery disease, high specificity for the diagnosis of myocardial ischemia could be expected.

All of these speculations require confirmation or refutation by data directed specifically to each issue. The senior author remains quite consistently "an outspoken critic of monitoring without demonstration of efficacy".

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The Cervical Spines of Dwarfs

To the Editor:—I read with interest and appreciation the recent review by Berkowitz *et al.*[1] of anesthetic implications of dwarfism. I am sure that it will be a widely used reference. I was, however, troubled by a few statements made and a few statements omitted by the authors in discussion of the cervical spine problems of these patients. The cervical spines of patients with pseudoachondroplasia, diastrophic dysplasia,

spondyloepiphyseal dysplasia tarda, and campomelic dysplasia deserve further comment.

The authors state in the appendix that patients with pseudoachondroplasia have normal cervical spines. This is in direct contradiction to one of the cited appendix references[2] and also conflicts with earlier work by one of the authors.[3] In both of these references, odontoid hypoplasia is noted in pseudoachondroplasia, and Perovic *et al.*[3] reported that three of their four patients with this combination of findings had cervical spine instability. Similarly, patients with diastrophic dysplasia and spondyloepiphyseal dysplasia tarda are noted by Berkowitz *et al.* as having normal odontoid processes or completely normal cervical spines, respectively.[1] Odontoid hypoplasia and its attendant problems are less common in these patients than in those with one of many other types of disproportionate dwarfism, but they have been reported.[2,4] Other cervical spine problems are mild but may progress over time.[5] Finally, patients with campomelic dysplasia virtually always have cervical spine problems, often severe.[6] Preoperative evaluation of the cervical spine, including complete odontoid evaluation, is important in all of these patients,[7] especially considering the likelihood of difficult intubation. Practitioners encountering a patient with any disproportionate dwarfism are well advised to be extremely conservative in their approach to the cervical spine and to intubation.

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In Reply:—Dr. Audenaert has correctly pointed out misquotations in the appendix of our review[1] in regard to cervical spin abnormalities of patients with pseudoachondroplasia, diastrophic dysplasia, spondyloepiphyseal dysplasia, and camptomelic dysplasia. We agree with his comment that the preoperative evaluation of the cervical spine is important in the above groups of patients as well as in other patients with disproportionate dwarfism.

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An Aid for Simultaneous Instructor and Trainee Viewing of Orotracheal Intubation

To the Editor:—Tracheal intubation is among the most important techniques that anesthesia trainees learn during graduate medical education. Yet anyone who has attempted to teach this skill and to view laryngoscopy from a distance (*e.g.*, over the shoulder) knows how difficult it is to confirm just exactly what the student is visualizing. We report a simple modification of readily available tools that will assist those teaching laryngoscopy and tracheal intubation while affording a continuous and direct view of the procedure.

A standard curved laryngoscope blade is modified (fig. 1) by drilling two sets of 1.5-mm holes to allow attachment of an intubating fiberoptic bronchoscope (Olympus LF-1). Each parallel set of holes is 4–5 mm apart, with the sets distanced 5 and 9 cm from the tip of the blade. The fiberoptic bronchoscope is attached loosely through these holes with rubber bands, still allowing some flexion of the scope's tip.

Assembled prior to induction, the blade is attached to a standard handle and the bronchoscope attached to a light source. Once placed into the oropharynx by the student, the teacher may follow exactly where the laryngoscope travels. No oral airway is required to protect the fiberoptic attachment, since the laryngoscope prevents jaw closure. Several advantages are noted with this apparatus: Lighting conditions are supplemented by the attached fiberoptic light source compared to the traditional battery handle, providing maximal viewing. The teacher can scan (roughly a 75° field of view with a 240° arc, for the Olympus LF-1) the periglottic area to note abnormalities the student might miss,