represent a contraindication for metformin medication (insufficiency of cardiovascular, pulmonary, or renal function; infections; catabolic metabolism) does not differ significantly if operations of the same size are performed in regional anesthesia. In case of ambulatory surgery we have concerns, and the development of contraindications might proceed unnoticed.

Although we agree with Lustik *et al.* regarding the importance of good diabetic control, we prefer to continue our rather restrictive practice of perioperative metformin therapy.

Sodium dichloroacetate (DCA), as proposed by Preiser and Vincent, could be an interesting future option for the therapy of lactic acidosis, especially because it could provide more than just symptomatic therapy.

However, DCA does not belong to the standard therapy of biguanide-induced lactic acidosis. Further, the clinical trials Preiser and Vincent refer to do not suggest DCA to be a magic bullet. Because metformin-induced lactic acidosis is a rare phenomenon and our personal experience is limited, we did not consider a therapy besides the recommended standards in this case.

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Implementation of Pharmaceutical Practice Guidelines—Once Again

To the Editor:—In a recent Letter to the Editor[1] in response to an article by Lubarsky *et al.*[2] I pointed out that anesthetic agents and muscle relaxants may have a significant effect that lasts beyond the PACU period, and therefore an investigation of the effect of practice guidelines on clinical outcome and cost-benefit analyses should include the period *after* the patients are discharged to the ward. I referred to our recent prospective, randomized, controlled study of postoperative pulmonary complications (POPC) after the use of pancuronium, atracurium, and vecuronium in nearly 700 patients.[3,4] We found that not only was the incidence and the degree of residual block in the PACU significantly increased in the pancuronium group, but also significantly more patients in this group developed POPC in the ward (16.9%) compared with the two other groups (5.4%).

In Dr. Lubarsky's response, he argued that the practice guidelines he proposed regarding the use of pancuronium are sound as long as enough medication is administered to achieve appropriate reversal, and "that reversal should be monitored by standard train-of-four monitoring, making sure a twitch is present before reversal, and one should assess the ability to sustain tetanus for 5 s in addition to having 4/4 twitches of near equal magnitude (>07, T4/T1)."

The problem is that during routine anesthesia given by anesthetists without any special interest in neuromuscular blocking agents, residual neuromuscular block after long-acting neuromuscular blocking agents is *not* eliminated using routine clinical tests and tactile or visual evaluation of the response to train-of-four or tetanic stimulation as described by Lubarsky.[5,6] It is *not* possible manually or visually to judge the degree of train-of-four or tetanic fade (50 Hz) with sufficient certainty to exclude residual block[7-10] (Viby-Mogensen *et al.*, unpublished observation). In studies claiming the opposite, the anesthetists evaluating the response to train-of-four nerve stimulation were dedicated and experienced observers.[11,12] To exclude clinically significant residual neuromuscular block after the use of the long-acting neuromuscular blocking agents during routine anesthesia, more objective methods of monitoring such as mechanomyography, electromyography, acceleromyography, or possibly double burst stimulation must be applied.[13-15]

Dr. Lubarsky claims that our patients received an inadequate dosage of neostigmine. It may or may not be so in some cases, although this is not the issue. The issue is that the anesthetists in our study were instructed to do their best to avoid residual neuromuscular block. They were instructed to aim at a level of neuromuscular block during surgery corresponding to one or two responses after train-of-four stimulation, as proposed by Lubarsky *et al.* A minimum of two responses was required to be present before initiation of reversal. Reversal was induced with neostigmine, 2.5 mg, but supplementary doses of neostigmine, 1.25 mg, could be given up to a total of 5 mg, if judged necessary by the anesthetists. Tracheal extubation was performed when four equal responses were felt after train-of-four stimulation and when clinically sufficient respiration was judged to be present. So once again it was documented that during routine anesthesia, clinical evaluation with manual evaluation of the train-of-four response does not exclude residual neuromuscular block after pancuronium.

On the basis of the previous, it is difficult for me to accept the conclusion of Lubarsky *et al.*: the routine use of the long-acting agent pancuronium did not adversely influence outcome. Lubarsky *et al.* have not convincingly documented by objective methods that their patients did not have residual neuromuscular block in the recovery

ward, nor did they include the period *after* the patients were discharged to the ward in the observation period. Therefore the burden of proof that Dr. Lubarsky *et al.*'s patients did not experience clinically significant residual block that might have adversely influenced outcome still rests with the authors.

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Pharmaceutical Practice Guidelines: Do They Actually *Cost* Money?

To the Editor:—Lubarsky[1,2] dismisses the concerns of Riley[3] and Bailey and Egan[4] who question the magnitude of savings that would be attained using Lubarsky *et al.*'s pharmaceutical practice guidelines.[5] When Riley noted that a 3-min increase in "emergence time" would increase costs at his institution, Lubarsky replied that this cost would be incurred only at Riley's institution. However, the same increased costs would be incurred at our hospital (and possibly others), in which nurses chronically work overtime. In addition, Lubarsky dismisses a 3-min savings as not detectable by an accounting system. We doubt this. As an analogy, if General Motors could shave 3 min off the production time for each vehicle, it would certainly do so!

Similarly, Lubarsky claims that Riley is "mistaken in his analysis of the one case of prolonged mechanical ventilation resulting from pancuronium administration" because the difference in incidence of adverse events "was not any different before *versus* after the implementation of practice guidelines." Although he is correct, he should acknowledge that his study is underpowered for detecting an increased incidence of severe (and potentially extremely costly) adverse events.

A more important issue has been completely ignored by Lubarsky *et al.* in their economic analysis. If anesthesiologists are under pressure to reduce costs, so are surgeons (and other operating room personnel). In our institution (which is presumably similar to Lubarsky's), surgical attendings are now present during a larger percentage of the procedure than in past years, and skin closure is no longer delegated to undersupervised medical students. In support of this, Macario *et al.*[6] recently reported that operating room costs for pa-