associated with the maldistribution of hyperbaric solutions,[1] has already been debated in the literature.[3–5] According to Wendell and Cianci[3] and Erian,[4] neither the catheter diameter nor the baricity of the injected solution was a factor of maldistribution. However, once again, these results were derived from experimental models. Using 19-gauge catheters, we demonstrated that maldistribution did not occur more often with either isobaric or hyperbaric bupivacaine. Nevertheless, the comparison has not been clinically studied using microcatheters. As such, I find it difficult to advise against the use of hyperbaric solutions *via* microcatheters before clinical evaluation. In one study, although retrospective, the required doses of hyperbaric lidocaine, 5%, administered *via* microcatheters were not greater than those using macrocatheters.[6] Finally, Horlocker *et al.* reported, also in a retrospective study, that the incidence of inadequate anesthesia was no greater when using microcatheters rather than macrocatheters.[7] As such, in light of these experimental[3,4] and clinical results,[6,7] we cannot conclude that microcatheters and hyperbaric solutions are factors of maldistribution. The only current, clinically demonstrated factor of maldistribution is the caudal orientation of the catheter tip.[8]

It is important to note, however, as highlighted in our manuscript, that the danger of maldistribution does not lie in its occurrence but rather in its not being diagnosed, leading to the administration of high doses of potentially neurotoxic local anesthetics. The diagnosis and early management of maldistribution, as well as abandoning the administration of high doses of local anesthetics (lidocaine, 5%), should limit the occurrence of cauda equina syndrome after continuous spinal anesthesia.

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Intrathecal Sufentanil Produces Sensory Changes without Hypotension in Male Volunteers

To the Editor:—The article by Riley *et al.* regarding sensory changes after intrathecal sufentanil was well written, detailed, and informative. The authors stated that the basis for the neuroselectivity of the different stimulus frequencies used in the CPT evaluation performed by the Neurometer® CPT device (Neurotron, Inc., Baltimore, MD) was "theoretical and unsubstantiated." Unfortunately, the authors must have been unaware of the significant number of peer-reviewed studies that have been published during the past 10 years, establishing the neuroselectivity of the CPT stimuli.[1,2] These studies include, but are not limited to, comparison with other neurodiagnostic tests,[3] peripheral nerve demonstrations of neuroselectivity,[4] and spinal cord demonstrations of neuroselectivity.[5] In fact, there have been more than 190 articles published in peer-reviewed journals using and validating the clinical use, reproducibility, and sensitivity of the CPT evaluation.

Apparently the only statistically significant change detected in CPTs before and after intrathecal administration of sufentanil was at 250 Hz at the knee. I agree with their point in the discussion section that there should have been a greater effect at 5 Hz. The reason for this discrepancy could be the way the data were analyzed. CPT values before and after intervention should always be expressed as a percent change as opposed to change in intensity (mA) because the amount of charge delivered is different for a 5-Hz *versus* 2,000-Hz sine wave stimulus. For instance, a 1-mA, 5-Hz sine wave stimulus delivers approximately ×400 the charge (coulombs) as a 1-mA, 2,000-Hz sine wave stimulus. Therefore, a 10-CPT unit (100 μA) change at 5 Hz results in approximately ×400 greater difference in charge delivery than a 10-CPT unit change at 2,000 Hz. Perhaps looking at the data as a percent change before and after sufentanil administration would reveal a significant effect at 5 Hz.

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In Reply:—We appreciate Dr. Chado's comments and interest in our article. We have reanalyzed the data as suggested by Dr. Chado. The ratio of pre- and posttreatment current perception threshold values were not significantly different (table 1). There was a trend for the 250- and 5-Hz lumbar groups to have a greater change posttreatment (as would be predicted), but the variability was too great to demonstrate this difference statistically. It is possible that a larger sample size or a crossover study design would have decreased the variability and demonstrated the predicted differences (we have considered both factors in subsequent studies). Another factor may be that the neurometer is not sensitive enough to measure the mild sensory changes effected by intrathecal opioids.

Finally, we agree with Dr. Chado that there is good evidence that the neurometer selectively stimulates various nerve fibers. However, to our knowledge, definitive patch clamp experiments have yet to be performed.

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Table 1. Ratio of Pretreatment and Posttreatment Current Perception Threshold Values

	Cervical			Lumbar		
Group	2,000 Hz	250 Hz	5 Hz	2,000 Hz	250 Hz	5 Hz
Saline	1.1 ± 0.2	1.0 ± 0.3	0.9 ± 0.5	1.0 ± 0.1	0.8 ± 0.3	0.9 ± 0.7
Sufentanil	1.1 ± 0.3	1.3 ± 0.5	1.1 ± 0.2	0.9 ± 0.2	1.3 ± 0.6	1.9 ± 1.7

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Valve System Performance

To the Editor:—I read the laboratory report, *Testing the Competency of the Hemostasis Valve in Introducer Catheters* published in ANESTHESIOLOGY 1998; 88(5):1404–6, with great concern and alarm.

Arrow® strives to manufacture our hemostasis valves to the highest standards of performance. However, we think that it is important that practitioners not misread the results of this testing to infer that any manufacturers' valve system is infallible. Another concern is that many practitioners refer to an *introducer system* and a *hemostasis valve* in