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In Reply:—The purpose of our study was to identify factors that may affect subarachnoid distribution of catheter-injected local anesthetic. In particular, we sought to identify factors that might favor maldistribution and to determine concentrations of local anesthetic that might result if maldistribution occurs.

Wendell and Cianci question our choice of injection rates. However, they have apparently misinterpreted "clinically relevant" to mean "normal" rate of injection. It was not our intent to perform injections using the mean "normal" injection rate determined for each catheter. Our purpose was to characterize each catheter over a range of clinically relevant rates. Consequently, our initial observational study included more than one type of injection. Each anesthesiologist was asked to inject 1 ml of solution: first, as if actually administering anesthetic to a patient ("normal") next, as rapidly as each believed acceptable in a clinical setting ("fast"); and finally, as fast as physically possible ("fastest").

Wendell and Cianci ascribe too much importance to these "normal" injection rates. By (our) definition, the "normal" rate reflects clinicians' perceptions of how fast a catheter should be injected; such perceptions are obviously influenced by a host of factors and change over time. Were we now to perform the same observational experiment, the "normal" injection rates likely would be significantly faster. (Certainly, if Wendell and Cianci now recommend that microcatheters be injected at a rate of 1 ml in 30 s, the "normal" injection rate will increase.)

Wendell and Cianci suggest that the data presented in our article do not support our conclusion that, when injected at clinically relevant rates, the 28-G catheter produces the greatest maldistribution of drug. We agree. This conclusion was based primarily on the observed differences in exiting streams of local anesthetic, that of the smaller catheter being more restricted. The relative importance of this early phase is not known; we suspect the later distribution (as presented in the histograms) to be, in fact, more critical. Thus, we presented data at 3 min because we believe that this phase of distribution is more relevant than the initial stream.

We believe velocity to be important. When injections were made at identical flow rates, the 28-G microcatheter distributed anesthetic more uniformly than either of the two larger catheters; it was our postulate that the higher velocity stream of the small catheter promoted mixing of the two solutions. However, velocity and flow rate are not independent variables; thus, a comparison of overall importance of velocity and flow rate has little meaning.

We have not underestimated the importance of the microbore catheter's "velocity profile"—we believe the larger catheter to have a better profile. The data in figure 2 of Wendell and Cianci's letter do not represent a "velocity profile" for either of the two catheters, but rather the calculated *average* velocity when each is injected only at its respective "normal" rate. The figure does not include data comparing stream velocities for catheters injected at achievable rates faster than the "normal." (Such data are particularly relevant given their current suggestion that a microbore catheter be injected at faster than the "normal" rate presented in the graph.) The mean time we obtained for a "fast" injection through a 28-G catheter was 27.5 s; Wendell and Cianci recommend that a 30-s injection become "normal." A "fast" injection through a 20-G catheter is accomplished in 3 s; the velocity is approximately 25% greater than for a "fast" injection through the microbore. When the two catheters are injected "as rapidly as possible," the larger catheter has approximately an 80% greater velocity.

We are concerned that, based on our data, Wendell and Cianci have recommended an injection rate of 1 ml in 30 s be used when performing continuous spinal anesthesia with a microbore catheter. Such recommendations should be based on carefully controlled clinical trials, not solely on data derived from a model. Moreover, our study was not designed to identify the most effective technique for continuous spinal anesthesia, but rather to identify factors that might contribute to local anesthetic maldistribution. We hypothesized that although sacral placement of a spinal catheter occurs infrequently, it was likely to result in local anesthetic maldistribution. Consequently, we studied only sacrally directed catheters. Our results suggest that a higher-velocity stream promotes mixing, but we suspect that within the clinically relevant range, this will not adequately compensate for sacral placement of a catheter.

Erian agrees with our conclusions but is concerned that each specific injection was not repeated multiple times. Although it might have been preferable to repeat each injection, our studies used an experimental model that has little variability: a calibrated mechanical injector was used to administer local anesthetic into a rigid spinal model. Our conclusions were not drawn, as he suggests, from "single injections at a given rate, through a given catheter type and a given position." Each catheter injection resulted in a set of eight samples, and the anesthetic distribution associated with each catheter was composed of a set of multiple injections performed at various rates.

We do not believe that the model injections performed by Erian and co-workers can "validate" our work. The experiments he describes did not examine or even control for injection rate; studied only one diameter of catheter; used a solution that did not contain local anesthetic; and, from the limited description, may not have included "cerebrospinal fluid" sampling. We also question the value of conclusions drawn from cephalad injections in a model that is without a spinal cord.

Erian is concerned that our samples were taken at 3 min. His own results appear to indicate a 10–20% increase in the spread of solution occurring between 3 and 5 min. It is difficult for us to evaluate his comment adequately because the information in both his letter and his abstract is limited. If, in fact, "cerebrospinal fluid" was sampled, it is possible that continued spread of solution was an artifact created by repeated measurement; measurements are described as repeated every minute postinjection in his letter, and at 1, 2, 3 and 5 min in his abstract. More importantly, "stabilization" is unlikely to occur *in vivo*—local anesthetic settles into a relatively fixed distribution in a model because physiologic factors such as uptake, elimination, movement, arterial pulsations, and cerebrospinal fluid flow are not present. Consequently, it actually may be preferable to withdraw samples prior to "stabilization." Furthermore, continued movement of anesthetic is unlikely to have biased our comparisons, because each set of our samples was withdrawn in the same order.

It is our clinical impression that continuous spinal anesthesia is best performed with a cephalad-directed multiport catheter. However, we do not believe results obtained in a model are a sufficient basis for a clinical recommendation to avoid using distal port catheters because they "run the risk of a large incidence of high spinals." Again, we believe such conclusions require data from carefully controlled clinical trials.

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