CORRESPONDENCE

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Pitfalls in Performing Meta-analysis: II

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To the Editor:—A recent paper by Sorenson and Pace[1] describes meta-analysis of anesthetic techniques during surgical repair of femoral neck fractures and analyzes data from 13 papers in the world literature. I am the first author of three of these.[2–4]

Regrettably, from the point of view of meta-analysis, the three studies are not of three separate study populations but of one population of 148 patients,[2] the long-term outcome of which was described in the 1984 paper.[3] The other two papers examined oxygenation in a subgroup of 100 patients[2] (although outcome at 4 weeks was discussed briefly) and deep vein thrombosis in a subgroup of 40 patients[4] (in which outcome was briefly discussed).

The concept of meta-analysis was unknown to me at the time of writing these studies, and it did not occur to me to mention that these patients were not separate groups. It was not implied in any way that there were three absolutely separate populations. The papers examined different aspects of sequelae of anesthesia for patients with hip fracture. Thus, I presume that the statistics of the meta-analysis will require recalculation.

I feel I must point out that it has been established that there is no significant difference in long-term mortality between regional and general anesthesia in patients with hip fractures: All of the studies that have examined this have been in agreement (see references quoted in the paper by Sorenson and Pace[1]). I therefore question the need for a meta-analysis of mortality at 4 weeks.

Perhaps works conducting a meta-analysis, particularly with older data, should contact any authors with multiple studies to check whether populations were indeed separate.

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In Reply:—Higgins and Stiff are to be commended for their careful reading of the source material for our recent meta-analysis comparing regional and general anesthesia for patients having surgical repair of femoral neck fractures.[1] They correctly identified two errors in our data abstraction—namely, our failure to recognize the partial duplicate reporting of the same patients by Davis *et al.* in 1980 and 1981 and our mistaken tally of patients with diagnostic quality ^{125}I-fibrinogen leg scans for deep venous thrombosis (DVT) in those reports.[2,3]

McKenzie has revealed partial duplicate publication of the same clinical study in three journal reports.[4–6] This produced double counting in our data tallies. This duplicate publication is characterized by the meta-analyst as an instance of multiple publication bias.[7] The 1979 Guide to Contributors for the journal in which the three studies were published stated that: "The purpose of the *British Journal of Anaesthesia* is the publication of original work in all branches of anaesthesia. . . . Papers submitted must not have been published in whole or in part in any other journal. . . ."[8] In our literature search for relevant clinical trials, we interpreted these standards—and similar editorial instructions in other journals—to prohibit unapprised, duplicate publication even within the same journal. If, in fact, duplicate publication of any type were common, it would be essential—as suggested by McKenzie—to confirm the originality of every journal report for inclusion in any type of literature review, either a narrative summary or a meta-analysis.

McKenzie also questioned the necessity for a meta-analysis that

Table 4. Treatment Efficacy

Outcome Event	No. Studies	No. Patients (GA/RA)	Difference (95% Confidence Interval)	Odds Ratio (95% Confidence Interval)
Mortality				
Epidural studies[11,12]	2	226 (166/60)	−2.2% (−10.6–6.3%)	0.75 (0.10–5.50)
Spinal studies[3–6,13–18]	8	1,601 (823/778)	3.1% (−1.4–7.6%)	1.44 (0.94–2.20)
All studies	10	1,827 (989/838)	1.9% (−1.7–5.4%)	1.40 (0.91–2.14)
Morbidity				
Deep venous thrombosis[3,6]	2	116 (59/57)	33.8% (11.5–52.9%)*	4.60 (1.66–12.8)*

GA = general anesthesia; RA = regional anesthesia.

* Risk difference ≠ 0 or odds ratio ≠ 1, $P < 0.05$.

used a 30-day/4-week mortality rate as the outcome variable. We were motivated to perform this research by the controversy about choosing between general and regional anesthesia. We desired both to maximize the amount of evidence to be included in a meta-analysis and to choose a relevant outcome variable. We continue to believe that the use of 30-day mortality was reasonable and relevant.

After another review of the data in all the cited studies (no additional errors were discovered), the statistical calculations for morbidity and mortality were repeated using the same hierarchical Bayesian model[9]; the duplicate studies were deleted, and the corrected tally of patients was used. These recalculated results are displayed in a revised table.

The summary risk difference and odds ratio for DVT are only trivially different from the original report (risk difference was 31.3%, now 33.8%; odds ratio was 3.99, now 4.60). Because the original calculations used a larger sample size (246 patients *vs.* 116 patients), the 95% confidence intervals are larger; however, statistical significance is achieved. This continues to demonstrate a higher incidence of DVT after general anesthesia. These basically unchanged summary statistics reflect the use of the correct proportions from the Davis *et al.* studies but incorrect counts. The importance of the difference in the incidence of DVT is uncertain; the two studies that systematically applied appropriate diagnostic tests failed to use DVT prophylaxis.[10]

After removing the duplicate data of the Davis *et al.* and McKenzie *et al.* studies, there are small changes in the summary risk differences and odds ratios for mortality, but these are sufficient to remove statistical significance for the summary odds ratios. There is now no statistical basis from either risk differences or odds ratios for claiming lower mortality with regional anesthesia. This justifies the cautious interpretation given the odds ratios in our original publication. The conclusions of our meta-analysis remain unchanged. There is insufficient evidence to make claims of lower mortality by the use of regional anesthesia. With the recalculated risk difference, one can project that about 3,000 additional patients would be required in new randomized controlled trials of general *versus* regional anesthesia to confirm statistically a 2% reduction in 30-day mortality.

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Anesthetic Care and Management of Patients Who Are Deaf and Mute

To the Editor:—Shulman and Polepalle[1] described problems involved in providing anesthesia for a deaf-mute patient undergoing prostate surgery.

The case report reached a large number of practitioners who may at some time have a significant impact on deaf individuals undergoing a surgical procedure. As such, this report should have been a vehicle to educate these professionals about a group of people with whom most of us have not been trained to interact.

Instead, the few useful recommendations for communicating with deaf patients that Shulman and Polepalle offer in the article are lost amid stereotypes and misinformation gleaned from a limited number of research references.

Although stating, "The association between hearing loss and paranoid states has not been demonstrated conclusively in any study," one is left with the distinct feeling that there is a relationship between deafness and paranoia. If no relationship has been proved, why bring the issue to the forefront? The impact of deafness on an individual is complex. Just as you objectively evaluate any hearing patient you may have, each deaf patient must be evaluated as the individual he or she is. Regardless of a person's hearing ability or level of education, surgery is a threatening, anxiety-producing situation. A patient's reaction to this stress is based on his or her perspective rather than on the facts. We have seen hearing patients become agitated in operating room situations when clear communication was lacking. This stress is heightened by a communication obstacle such as deafness. The feelings of being powerless and isolated can be overwhelming, and the patient's subsequent behavior could be perceived by others as "paranoia."

The authors make many statements about American Sign Language (ASL) that are incorrect and leave the reader with the belief that ASL is an inferior method of communication. In fact, ASL is recognized as a legitimate language possessing every element of a spoken language, including a complex vocabulary, grammar, syntax, and substructure.

Shulman and Polepalle learned from their clinical experience with a deaf patient that communication is paramount, and this communication ensures that the surgical experience will be successful from every participant's viewpoint. As health-care providers in the operating room, it is our responsibility to provide pre-, intra-, and postoperative information and constant feedback to our patients. This communication allows any patient to retain a degree of control over their circumstances. To this end, the deaf patient must be allowed the right to communicate in whichever mode he or she is most comfortable. This may be *via* one of the manual signing languages, ASL being the most common, or by speech reading or finger spelling. Clearly, the use of a qualified interpreter to facilitate this process cannot be overemphasized.

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