Subdural hematoma

To The Editor: In their manuscript, Miranda et al.1–3 (Miranda LB, Braxton E, Hobbs J, et al: Chronic subdural hematoma in the elderly: not a benign disease. Clinical article. J Neurosurg 114:72–76, January 2011) crystallized what many neurosurgeons have felt empirically for some time: Whilst for a majority chronic subdural hematoma (CSDH) is a benign disease, for a significant minority it heralds a malignant decline. The comparison with hip fractures4 is therefore apt.

The key support to their article derives from demonstrating that their patients had a worse outcome relative to matched controls. Their methodology presents prima facie support for this notion: The authors calculated anticipated survival by age and sex matching with current data from the Centers for Disease Control and Prevention.5 Notwithstanding, it would have been important for Miranda et al. to have enumerated all the comorbidities suffered in their patient population. This would have allowed other groups to have compared directly with their own patient population, since comorbidities (such as ischemic heart disease and diabetes mellitus Type 2) have recognized survival patterns across societies. It is speculative to have attributed outcomes to comorbidities when the authors did not actually list any! Such attribution is especially speculative without having listed any causes of death in their study either. Deaths, for example, due to accidents, misadventure, or suicide might not have directly related to “underlying chronic diseases” in a way that Miranda et al. have implied.

The principal flaw with the article of Miranda et al., however, relates to whether their population compared favorably to the “average” population admitted to neurosurgical wards with CSDH. Miranda et al. freely acknowledge that their in-hospital mortality (16.7%) “modestly exceeded” the rates recorded by most series (0%–15.6%).3 However, since Miranda et al. used drains in all their patients (which their low recurrence rate certainly endorses), their mortality should have been significantly lower than that of most reported series. This fact was proven in the prospective, randomized study of Santarius et al.,5 which showed that mortality at 6 months was significantly reduced from 18% in those treated without a drain to 9% in those treated with drains (p = 0.0424); in-hospital mortality was 4% with drains and 8% without drainage (p = 0.23). The mean age in the study of Santarius et al. (77 years) was similar to that in the study of Miranda et al. (80 years). Such data strongly suggest that the patient sample of Miranda et al. significantly differed from a similar group managed uniformly with bur holes and drainage in a large teaching hospital elsewhere; it may therefore have been unrepresentative of the “average” sample admitted to neurosurgical wards. The low male/female ratio in their study (1.7:1) further supports this possibility (in most studies, the ratio is 3:1 or greater).1–3

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Disclosure

The authors report no conflict of interest.

References


RESPONSE: We wish to thank Drs. Manickam and Marshman for their thoughtful comments. As we have previously stated in another response to a letter to the editor1 we freely acknowledge that our in-hospital mortality is the highest yet reported but feel it is directly related to the series being the most elderly yet reported. The assertion that our routine use of drains should have made our mortality “lower than that of most reported series” implies a fantastic ability of a drain to trump patient biology, an assertion that the literature does not support. Whereas we did not enumerate the litany of medical comorbidities found in our study population we are unaware of any methodologies which would allow “other groups to have compared (our patients) directly with their own patient population” as indicated by the authors, and felt that the life-table analysis was therefore the most apt comparison. Although we were not able to obtain direct cause of death in our population, we believe that the likelihood of “accidents, misadventure, or suicide” in this cohort within their
Neurosurgical forum

1st year after hospitalization to be so low as to not imperil our overall conclusions.

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Disclosure

Please include this information when citing this paper: published online May 18, 2012; DOI: 10.3171/2011.10.JNS111316.

Helmets

TO THE EDITOR: We biomechanists, researchers, and, in the case of Drs. Gennarelli and Cantu, neurosurgeons wish to express our dismay regarding the conclusions in a recent paper that leather football helmets performed similarly to new or modern football helmets (Bartsch A, Benzel E, Miele V, et al: Impact test comparisons of 20th and 21st century American football helmets. Laboratory investigation. J Neurosurg 116:222–233, January 2012).1 We believe the journal did a disservice to its readership by publishing this article in its current format.

The paper has many methodological design flaws. 1) The effective energies were so low as to elicit a low force of gravity averaging around 50 G, which is about half the level of the 98 G that the National Football League (NFL) found with concussions and less than half the 105 G that Virginia Tech found in their study of concussions. A wider spectrum of energies should have been studied. 2) The VSR-4 helmet was the impacter and with these low energies was absorbing most (all) of the energy. It would have been a far more valid study if the same model or type of helmet—leather impacting leather or modern helmet impacting the same modern helmet—had been used as the impacting and impacted helmet. 3) The authors present no hypothesis to be tested, present no data, and have no statistical analysis. The conclusions are thus not supported by any statistical data.

We strongly believe it is unfortunate to have published this paper with the above flaws and irresponsible for the authors to publicly state that it proves leather helmets are similar to modern helmets—a patently false assertion.

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RESPONSE: With disappointment, we read the Cantu et al. Letter to the Editor regarding our article published in the Journal of Neurosurgery. We are fully cognizant of the prowess, notoriety, and academic prominence of these authors. Although we respect them and their unified opinions about our article, it is first and foremost essential to note that our article was subjected to multiple rounds of peer review at a top-shelf journal and was deemed important enough by the Journal of Neurosurgery editors to land on the cover. Further, months ahead of submission, the objective data collected were formally and informally vetted among esteemed members of the biomechanics community on multiple occasions, and several of the letter authors were presented with data, slideshows, and in one case the entire manuscript. None of them expressed any of the unified opinions contained in the letter. We are thankful that one of the authors provided constructive criticisms, which were incorporated into our article ahead of publication. Regardless, we address each of the criticisms introduced in the letter in order.

1) As stated in our introduction, we purposefully did not include high-energy collisions in our test matrix, like those referenced in the letter, because we wanted to better understand the efficacy of modern helmets as compared with vintage leatherheads for impact dosages resulting from common on-field impact magnitudes. Therefore, we conducted omnidirectional impact testing, eliciting head accelerations up to 63.9 G, angular accelerations up to 6220 radians/second2, and angular velocities up to 24.1 radians/second. Using on-field context, Rowson et