Subdural hematoma

To The Editor: In their manuscript, Miranda et al.1 (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 fractures2 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.3 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.,3 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)3 further supports this possibility (in most studies, the ratio is 3:1 or greater).1,2,4

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

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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 impactor 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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Disclosure

Dr. Greenwald has a royalty arrangement with Riddell on HITS-related products.

Reference

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/second, and angular velocities up to 24.1 radians/second. Using on-field context, Rowson et
al.\textsuperscript{10} found that 97% and 99% of on-field impact dosage magnitudes are less than 60 G and 6000 radians/second\textsuperscript{3}, respectively.

The letter states that we should have studied high-gravity impact dosages. We state in our article that acute concussion due to high-gravity dosages is very important. However, we chose to study the much more common sub- and near-concussive impact dosage ranges. It is notable that all of the letter authors have supported the theory that common head impact magnitudes may be associated with the long-term and potentially deleterious effects of repetitive subconcussive trauma dosage accumulation.\textsuperscript{1-3,12,13} The latter is accepted as a causative factor in chronic traumatic encephalopathy and tau body formation.\textsuperscript{5} Hence, the study of common on-field impact dosages in addition to high-gravity impact dosages is imperative.

Finally, the upper range of our impact dosages can absolutely be considered near- or subconcussive. Pellman et al.\textsuperscript{9} showed in NFL concussion reconstructions that some concussions were associated with head acceleration (20%), angular acceleration (40%), and angular velocity (24%) below our test magnitudes. Of the nonconcussed reconstructions from the same study, the majority had head acceleration (67%), angular acceleration (100%), and angular velocity (67%) below our magnitudes. Rowson et al.\textsuperscript{11} calculated that for concussed football players, there is an approximately 3% risk of concussion at head accelerations of 64 G.

2) The letter authors are wrong here. The striking VSR-4 helmet did not absorb all of the impact energy. This is evidenced by the fact that the struck headform moved; hence, kinetic energy was transferred from the struck to the striking headform in every impact. Furthermore, the headform in the leather helmet tests absorbed similar or higher energy as compared with the modern varsity helmet tests in 12 of the 15 conditions at 2.0, 3.5, and 5.0 m/second.

3) The authors’ contention that we “present no hypothesis to be tested, present no data, and have no statistical analysis” is, again, wrong. First, as stated in our article, “the object of this study was to compare the head impact doses and injury risks with 11 widely used 21st century varsity helmets and 2 early 20th century leatherheads and to hypothesize what the results might mean for children wearing similar varsity helmets.” And that is what we did.

Second, our article is flush with data. Furthermore, the data are supported by discussion and relevant clinical interpretation. At the reviewers’ request, we pared down approximately 800 unique helmet impact tests and 50 million impact dosage data points into a clinically relevant format digestible by the readership.

Third, we applied descriptive statistics in Table 1 and Figs. 3–5. We further simplified the analysis by presenting only the 5.0-m/second impacts, but we do have similar data and results for impacts at 3.5 and 2.0 m/second. Thus, we are confused regarding the reference to inadequate “statistical data.” We have interpreted our data and provided conclusions for a clinical audience.

Lastly, the authors state that it is unfortunate that the Journal of Neurosurgery published our article. Further, they assert that we are irresponsible in publicly stating that “leather helmets are similar to modern helmets.” Wrong again. We correctly asserted in our article, and the supporting data are irrefutable, that “in many instances the head impact doses and head injury risks while wearing vintage leatherheads were comparable to or better than those while wearing several widely used 21st century varsity helmets.” It was emphasized that we did not observe an appreciable decrease in impact dosage with varsity helmets. However, we tempered this finding by adding that we “do not advocate reverting to leather headgear.” We are certainly aware that vintage leatherheads may not provide adequate protection in all impact scenarios, particularly high-gravity head-to-head collisions.

In conclusion, we wonder how the authors could conclude that the publication of objective data potentially relating to the brain health of more than 4 million young athletes competing in collision sports in the US on an annual basis is irresponsible. We are disappointed in their unified response to our article. We are also disappointed by their interpretation and misrepresentation of the article, as well as the facts presented. In addition, we disagree with each of their criticisms and stand by our article and its clinically relevant findings. Furthermore, it is obvious from the data presented in our article that modern helmets have yet to be optimized toward common, everyday blows like those we studied, despite their documented ability to mitigate catastrophic injury. Our results do not imply superiority of or equivalence with leatherhead helmets or a failure on behalf of any institution, manufacturer, or governing body. They simply indicate that we have a long way to go in mitigating concussion and subconcussive impact dosage accumulation. Therefore, we reasonably advocate, especially for youths who are potentially subjected to years of impact dosage accumulation, “instituting helmet safety designs and testing standards, which encourage the minimization of linear and angular impact doses and injury risks in near- and subconcussive head impacts.”

We look forward to collaborating with the entire traumatic neuromechanics community, including our critics, on further work in this arena. We know the authors will join us in that endeavor. We can only advance the field through perpetual vigilance and scientific investigation.
They showed that dural invasion by an ACTH-positive adenoma was histologically proven in 30 (34%) of 87 patients. Invasion of the cavernous sinus wall was evident in 18 (60%) of 30 patients, and invasion of the sella dura accounted for 40% of dural invasion cases. Larger adenomas (10.9 ± 7.8 mm) were associated with a higher incidence of dural invasion. They concluded that dural invasion by ACTH-secreting adenomas preferentially happened laterally into the cavernous sinus wall.

There are some challenges in the surgical management of CD. First, since the study’s investigation of dural invasion was based on histological analyses of surgical specimens, sampling bias would be unavoidable. Second, tumor and dura curettage are often accompanied by concomitant bleeding, which needs suctioning to keep the surgical field clear while resecting tumors. Therefore, the loss of dura and tumor samples through suction would lead to an underestimation of dural invasion. Third, non-visualized microadenomas are often undetectable on preoperative MRI, leading to difficulty in finding a tumor during explorative resection of pituitary tissues. Therefore, frozen-section verification of tumor parts as well as venous sinus blood sampling are crucial to making sure the level of ACTH drops. These points are important for neurosurgeons to know for the end point of surgery. Moreover, Park et al. reported that concurrent ACTH- and corticotropin-releasing hormone (CRH)–related ectopic Cushing syndrome could be induced by malignant gastrinoma with multiple liver metastases. Neurosurgeons should remember the differential diagnosis when cortisol and ACTH are both highly elevated; the differential diagnosis of pituitary hyperplasia with elevated CRH should be considered preoperatively. We fully agree with the authors’ proposal that dural sites with doubtful or gross invasion should be totally removed when possible to avoid sampling bias; these points are important for neurosurgeons to better understand dural invasion and clinical measures of neurologic function in collegiate football players.

Disclosure

The authors report no conflict of interest.
that are not seen on MRI before resection can be more
avoiding a sampling error, as underscored in our paper by
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In this prospective study we analyzed patients with CD
incidence of adenoma-associated dural invasion in Cushing
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RESPONSE: We appreciate the interest that Drs. Hueng,
Ma, and Chen expressed in our recent paper. In that arti-
cle, we characterized the frequency and features of dural
invasion by CD-associated ACTH-secreting adenomas.
In this prospective study we analyzed patients with CD
who underwent systematic evaluation of the anterior sella
dura and sites of invasion by ACTH-positive adenomas
identified during resection. Data from these patients re-
vealed that ACTH-secreting adenomas preferentially in-
vade dura forming the wall of the medial cavernous si-
num, that an increasing tumor size was associated with an
increased frequency of invasion, and that dural invasion
often was not detected by MRI. These findings indicate
that the systematic removal of invaded dura, including
dura forming the wall of the medial cavernous sinus, is
essential to achieve lasting biochemical remission and
avoid recurrence.

As Dr. Hueng and colleagues assert, CD can be a sur-
gically challenging disorder. However, the use of well-
described microsurgical techniques for ACTH-positive
adenoma removal and the application of points empha-
sized in our paper can overcome these challenges. Spec-
ifically, wide bloodless exposure of the entire anterior
and anterior inferior portions of the sella dura, as well
as the medial half of the cavernous sinuses, is necessary
to clearly identify and resect potential sites of dural in-
volvement and to avoid inadvertent suctioning of tumor
or normal anatomical structures involved with tumor,
which could result in a sampling error. Moreover, care-
ful circumferential dissection around the adenoma using
its histological pseudocapsule will facilitate its removal
from a field that is not obscured by bleeding and will per-
mit the precise identification of dural invasion sites while
avoiding a sampling error, as underscored in our paper by
the direct association of histologically confirmed sites of
dural invasion identified at surgery.

As Dr. Hueng and colleagues describe, adenomas
that are not seen on MRI before resection can be more
difficult to identify during surgery. This is particularly
the case if the adenoma is very small and embedded in
the normal anterior pituitary gland without an anterior
surface presentation. In such cases, it is essential to sys-
tematically explore the anterior pituitary gland by using
vertically oriented incisions spaced 1.5 to 2 mm apart
until the adenoma pseudocapsule is identified beneath
the surface, and then the tumor can be removed, as de-
scribed. While Dr. Hueng and colleagues suggest using
frozen tissue sections for adenoma identification, we have
found that these are not reliable or helpful in guiding re-
section. Rather, direct visualization of the adenoma and
circumferential dissection around it are critical for con-
firmation of a tumor. For the small fraction of tumors not
identified by the technique described above, it may be
necessary to perform a partial hypophysectomy.

Clearly, careful endocrinological evaluation that es-
ablishes the underlying source of cortisol excess in pa-
ients with Cushing’s syndrome is necessary, as referred
to by Dr. Hueng and colleagues. There are well-defined
and established endocrinological and imaging stud-
ies that are routinely used to accurately distinguish the
source of cortisol excess in Cushing’s syndrome, even in
the rare circumstance of excessive ectopic CRH secre-
tion. Predictably, establishing a diagnosis of CD in pa-
ients with Cushing’s syndrome is important for optimal
management, including resection of the underlying pitu-
itary adenoma and invaded dura, which is the treatment
of choice in CD. Similarly, as was done in our study, it
is important to confirm the successful treatment of CD
resulting in biochemical remission after surgery by using
declared endocrinological testing, including serum cor-
tisol, urine cortisol, and serum ACTH levels.

Because adenoma-associated dural invasion in CD
is common (34% of patients in our study), complete re-
moval of the adenoma and invaded dura will be neces-
sary for biochemical remission and for avoiding recur-
rence. We have not needed neurointerventional support or the
use of biomaterials or stents to bridge the dural defect, as
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Please include this information when citing this paper: published online May 11, 2012; DOI: 10.3171/2012.2.JNS11890.