In Reply:

The letter by Casson et al. shed light and clarification on the entity of chronic traumatic encephalopathy that was lacking in the article by Omalu et al. (1). As they note, it was originally characterized by a tetrad of findings, including abnormalities of the septum pellucidum, cerebellar scarring, degeneration of the substantia nigra, and widespread neurofibrillary tangles in the cerebral cortex and brainstem. They correctly point out that the case presented by Omalu et al. (1) had only one of these pathological findings, raising concern as to whether the individual actually met the criteria for chronic traumatic encephalopathy. Based on these criteria, it would seem not. Consequently, the authors’ assertion that the individual’s pre-mortem cognitive decline, depression, and Parkinsonian symptoms where a manifestation of chronic traumatic encephalopathy seems largely unfounded. Unfortunately, as previously noted by several of the reviewers of the original manuscript, there was no evidence presented linking the player’s NFL career to his neurocognitive decline or to the neuropathological findings. Furthermore, the player had no documented concussions during his career and he played mostly as an offensive linesman which is one of the positions associated with the lowest frequency of concussion. Ultimately, the authors’ lack of alternative explanations for the neuropathological findings, the selective and seemingly inappropriate use of the definition of chronic traumatic encephalopathy, and the “N of 1” nature of this study severely weaken it. Given this lack of hard evidence and the overly assuming nature linking the available data together, I agree that retraction or a major revision by the authors is warranted. Casson et al. are to be thanked for further educating us about the entity of chronic traumatic encephalopathy and completing the editorial process.

Daniel F. Kelly
Los Angeles, California

More important, however, is the issue of the first several paragraphs of their letter, which consist of repetitious reiterations regarding how Omalu et al. incorrectly used diagnostic nomenclature. They do not dispute his findings, they simply dispute the name Omalu et al. have given to those findings. This seems to be an issue that requires brief discussion and clarification, not relentless table-pounding.

In summary, I see the Casson et al. letter as raising several valid points regarding the intrinsic limitations of the case material used in Omalu et al.’s study. However, because these limitations were noted by Omalu et al. in the published version, I do not see the point of publishing a letter reiterating them. Additionally, and perhaps more importantly, I see no value in the repetitious initial paragraphs of the letter devoted entirely to nomenclature.

Lastly, there is the issue of the “tone” of the letter. Disagreements among clinicians and scientists are important and should be published, but they need to reflect appropriate collegial respect.

Joseph Bleiberg
Neuropsychologist
Washington, D.C.

In Reply:

My review of the first submission from Omalu et al. (1) identified many of the issues noted in Casson et al.’s letter to the editor. I recommended that Omalu et al. explicitly state the weaknesses of their case report material. This was combined with a corresponding recommendation that sweeping generalizations, based upon this single case, be removed. I concluded my review with the statement: “...this paper raises more questions than it answers...”

Omalu et al. resubmitted a substantially revised and truncated paper, much more modest in scope, and much more cautious regarding sweeping generalizations. I thought they had been responsive to my initial review.

The above statements are regarding the last few paragraphs of the letter by Casson et al.

Alex B. Valadka
Houston, Texas

In Reply:

The authors of this letter should be thanked for compiling this detailed historical review of our understanding of the neuropathology of chronic brain injury. They argue that the structural findings reported by Omalu et al. (2) are insufficient to meet the criteria of chronic traumatic encephalopathy as defined by Corsellis et al. (1) in 1973 and modified by Roberts et al. (3) in 1990. In addition, this letter emphasizes the paucity of information about the medical history of Omalu et al.’s patient.

Omalu et al.’s report may serve to stimulate interest in the area of neurodegenerative histological findings in athletes. However, the bar has clearly been raised. Future studies will need to use standardized or widely accepted histological criteria in addition to firm and accurate medical histories.


In Reply:

Drs. Casson, Pellman, and Viano’s letter asserts that Omalu et al.’s (1) article in Neurosurgery contains a serious misinter-


In Reply: