Letters to the Editor

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Concerning the article by Johnson DL et al.

Severe Pediatric Head Injury: Myth, Magic, and Actual Fact

The title of this paper immediately arrests the reader attention. The paper lives up to its title and delivers one fact: that the mortality rate for children 18 years of age and younger in this particular data base is 37%. It abolishes no myths and is significantly deficient in magic.

The reader is given no useful data to come to any conclusion. The mean age and standard deviations of the cohorts of patients are not given. It appears from figure 1 that only 30–40% of the patients are 11 years or less, thus the mean age of the patients in the 3- to 18-year age groups is presumably quite a bit older than in the CHOP and CHOW studies that the authors spent most of the paper discussing. No mention is made of this.

Only children with a GCS of <8 are included in this study, yet both the studies the authors compare included children with a GCS = 8. Since the GCS = 8 group had the lowest mortality in the CHOW and CHOP studies some reference by the authors to this population difference is expected. In addition the authors do not divide their patients by GCS, and give only an overall mortality figure which is meaningless. In the two studies they discuss, the only group with a major difference in outcome were the children with a GCS of 8 (table 5). All of the difference in mortality between the two studies can be explained by the better outcome of this group. Most centers dealing with pediatric head injuries would be appalled to have a 23% mortality rate in this group of injuries and would be trying to improve the outcome of this very salvageable group of children, GCS = 8.

In table 3 those children with some 'motor response' have a mortality rate of 5.8%. No differentiation is made between those under 11 and older than 11, yet there is no explanation from the authors as to why they are not separated in this table. This category, 'some motor response' would equate to a motor score of 2 or more (1 is flaccid i.e. no motor response) and a full GCS of 4 or more. These figures would give the authors data base results that are much better than any reported by CHOP or any other center, 6% mortality for GCS 4–8. There is not a single comment in the paper regarding this startling information!

In table 3 the results of outcome related to hypotension are shown, yet nowhere in the paper is there a definition of hypotension. Hypotension for a 3-year old is certainly different than for an 18-year old, and how can the reader make any use of this table without basic facts. In addition, from table 3 one can extract that the mortality rate went from 30 to 85% in the 3- to 11-year group, and from 40 to 90% in the 12- to 18-year group, yet in the text only the older age group is said to have 'a significantly increased risk of dying'. If the authors have statistics to support this conclusion the reader should have access to them. Also missing from this part of the paper are the number of patients who were hypotensive. How much of the overall mortality did they account for and what was the mortality rate for those with a GCS of 4 or more who were not hypotensive? Was there any difference in outcome in the normotensive children between those 3–11 and those 12–18 years of age? Apparently, no statistics were applied to any of the information since there are none given anywhere in the paper. Generally, facts are subjected to statistical analysis before significance is reported.

This non-referenced statement that 'The GCS is touted as a major innovation in the care of head injuries' is made. The GCS has nothing to do with the care of head injuries, it is purely a grading system.

For me at least this paper lacks more than it delivers and if this is the actual fact I prefer to believe in the magic. All this data base search delivers is that the lowest mortality rate reported is 30% for children between 3 and 11 years who have an initial GCS of <8 and that no difference was identified, no statistics, after MVAs between younger and older children. There is no information presented that even relates to what the expected best outcome could or should be. Despite the pompous title of this paper it would appear to be an apology and a justification for not reporting better results. The outcome from the treatment of severe head injury can be improved but only if those of us looking after the children are willing to accept the challenge. Based on the information presented the conclusion that 'a mortality rate of <10% is not achievable' is given no factual support. The last sentence for me sums up the paper when it again uses imprecise, nonscientific, language in an effort to make a positive claim, 'most likely'. Understandably the authors prefer their own myths.

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Response to Dr. Bruce’s Review

The authors hoped to benefit from Dr. Bruce’s review but are disappointed that he chooses to ignore medical facts in favor of rhetoric and magic. We acknowledge that the science in medicine is complemented by the art of medicine. The difference between the art of medicine and the magic of the shaman is in the reproducibility and timeless merit of the results. Our response will be deservedly brief and to the point.

No useful data. This is the largest series of severe pediatric head injuries ever reported. The ages of the 4,041 children are stratified into clinically meaningful groups: <3 years to capture and separate the population most likely to be abused (highest mortality rate), 3–11 years for the core of the pediatric population, and 12–18 years of age for the physiologically more adult-like children.

Mean study age is probably older than the CHOP and CHOW series, which accounts for higher mortality rate. Our results show that the mortality rate of older children (12–18 years) is higher than that of younger children (3–11 years), although the mortality of children less than 3 years is as high as the 12- to 18-year-old group when all mechanisms of injury are considered. There is no clinically significant difference for the age groups 3–11 and 12–18 involved in motor vehicle accidents. The fact remains that the mortality rate of this PA series is the same as the CHOW study (37%), and is 6 times higher than the CHOP series reported by Dr. Bruce in 1978.

Since children with the more favorable GCS = 8 were not included in this PA series, the mortality rate is understandably higher. Only 3 patients in the CHOP series had GCS 8, whereas 23 in the CHOW study were GCS 8. A series of 3 does not explain the better outcome of the CHOP series nor could it possibly explain the worse outcome of the CHOW series.

The difference in mortality rate between the CHOP series (6%) and CHOW (37%) can be explained by the inclusion of children with GCS 8. Only 5.7% of the CHOP series had GCS 8.

A mortality rate of 5.8% for children with some motor response is remarkably better than other reported series. ‘Some motor response’ is a reliably reported observation from a large database and effectively eliminates GCS 3 from consideration. Since children with GCS 3 have a very high mortality rate, it is not surprising that the mortality rate for the remaining children would be 5.8%.

A mortality rate of 23% for children with GCS 8 is appalling. The authors would agree, but Dr. Bruce misinterprets the facts. The 23% refers to the percentage of children with GCS 8 (23/98), not to the mortality rate.

Generally facts are subjected to statistical analysis before significance is reported. ‘The Statistical Analysis Systems software program was used for statistical analysis of the results’, and was appropriately referenced. Clarity of presentation prompted simple, unexaggerated statements of significance.

The GCS has nothing to do with the care of head injuries, it is purely a grading system. The management of head injuries is dictated by the GCS and allows results from different series to be compared. Without the GCS there would be no basis for comparison of treatment results.

Despite the pompous title of this paper it would appear to be an apology and a justification for not reporting better results. This paper only reports the facts recorded from 5 level 1 trauma centers who care for children (4,041 cases), one of which was CHOP. The results are much worse than those reported in 1978 by Dr. Bruce at CHOP. Moreover, no other institution in the world has ever validated Dr. Bruce’s results. This paper has shed some light on the disparate results which he reported and concludes that a 6% mortality rate for severely head-injured children is a myth and can be appropriately shelved. We had hoped that Dr. Bruce would share some of the magic of his results. He has left us to wonder what he will pull out of his hat next, and then we will again wonder, ‘Now, just how did he do that?’

The authors are unwilling or unable to accept the challenge to improve the outcome of severely head-injured children. Unrealistic expectations are a source of confusion, frustration, and inevitable failure. The data that we have presented provide the factual basis for comparison, as contrasted to the mythical basis presented by Dr. Bruce. The results of treatment of head-injured children can be improved: not with magic and miracles but with hard work.