

## RESPONSE TO RICHARD KATZ MD

Chicago, Illinois  
March 9, 2011

I was requested to prepare a life expectancy report concerning a five year old girl with cerebral palsy (CP), in September and October, 2010.

As part of this review, I had the opportunity of reading an August 23, 2010 report written by Dr. Richard Katz, of St. Louis, Missouri.

The reason for this online posting is that the Katz report had a long list of totally incorrect statements concerning my own research results. More importantly, it has recently come to my attention that these same misstatements have been, and are being, repeated. The only way to address this sort of total inaccuracy is to provide correct information public, as a web posting.

The life expectancy report that Dr. Katz wrote had very many errors and misstatements of the published literature. I will not discuss any of these issues here. Rather, I will only address the litany of errors concerning his comments about my own work, and my accomplishments.

Each error in Katz's statements will be discussed separately, in order of appearance in the Katz report.

In my report (Plioplys et al., 1998), the 6 groupings were based on a previous publication, Eyman et al. (1993). Katz claimed that in defining these 6 groups, I relied on Eyman et al. 1990—this reference was not used in defining these 6 groups, since it defines only 3 groups, not 6. Even a cursory reading of my article makes this point very clear.

Katz claimed that in my report (Plioplys, et al., 1998) there were 251 children who had significant mental impairment and cerebral palsy. This is incorrect. The correct number from page 163 of this article is 371. However, when the definitions of group numbers 1 through 6 were applied, that reduced the number to 251. All of this information is clearly stated in my article.

Katz argued that we had small numbers and for some reason this is a weakness. Given the severe degrees of impairments of these patients, and the need for all of them to have 24 hour a day nursing care, the numbers that we studied are actually quite large. The Eyman et al. (1993) study, that our comparison was geared to, presented data in the Tables that was suspect. For example, in group 3, 19.8% did not even have CP, and only 42.5% had severe CP. Yet the Tables and graphs in this Eyman et al. (1993) report all deal with patients who have severe CP. If one looks at the Eyman data, and corrects for these distortions, in comparing to our skilled nursing facility placement, the Eyman et al. 1993 report had only 114 patients, and we had 251 patients—a much

greater number (Plioplys, 2004a). Thus, when “comparing apples to apples” our study size was 220% the size of the comparable Eyman et al. (1993) data set.

It is no impairment of a study to report results honestly and thoroughly. In my reports (Plioplys, et al., 1998; Plioplys, 2003), all the study patients were residents of skilled nursing facilities, whose medical care needs required 24 hour a day nursing. These unfortunate individuals had medical care needs that exceeded the abilities of their families to care for them. Is there a problem, a “weakness” in providing the medical care disabled people need? If optimal medical care is provided, better clinical results will occur. Providing good quality medical care is to be praised, not repudiated.

Dr. Katz criticized the statistical method, chi-square, which was used for the statistical analysis presented in Tables 3, 4 and 5 in our report (Plioplys, et al. 1998). In these tables, two pairs of data set numbers were compared in each case. This is a totally valid use of the chi-square statistical approach. Katz cannot show that using chi-square is incorrect. [It should be noted that in his report Katz cited the O’Brien and Zaharia (1998) life expectancy study which used this same chi-square statistical tool.]

To generate the graphs that were published in our 1998 article, we used the techniques that Hutton et al. (1994) used. We finished collecting our data in 1996, and wanted to use statistical methodology which was the most current. The Hutton et al. (1994) study was the one of the most recent CP life expectancy publications at the time, and we tried to copy the techniques presented there. Furthermore, it should be noted that Dr. Katz himself cited 7 articles authored or co-authored by Hutton, without any criticism of the statistical tools and approaches used by Hutton.

Katz criticizes a typographical error in our article, Plioplys et al. (1998). There were 371 CP patients. Later in the article the number 367 appears, which is a typographical error. I will not dignify this with a response.

Dr. Katz quoted me as supposedly saying, during a 2006 deposition, that my first submission of the 1998 manuscript to the American Journal of Diseases of Childhood, was rejected “because of statistical problems.” This is not true. I never made such a statement in my deposition. On pages 195 through 197 of the deposition, I repeated 3 times that the primary reason for rejection was one reviewer’s comment that my manuscript was “politically unacceptable.” The other reviewer made comments about statistics, but I could not then, and still cannot remember whether this reviewer requested changes or clarifications. When the manuscript was submitted to the Southern Medical Journal it went through a thorough peer-review and was accepted for publication. Subsequently, my summary of the same data, with all of the same statistics, went through two different peer-review processes, independently, for the Journal of Child Neurology, and the Seminars in Pediatric Neurology. In both cases, the rigorous peer review was positive and my article was accepted for publication. From my medical resume, I have been the recipient of 5 major medical research grants totaling \$2.8 million, the author of 73 medical research articles, and 62 medical research abstracts.

Almost all of these required proficiency in statistics. To claim that my research has “statistical problems” is incorrect.

Katz quoted a deposition statement that I said 31 patients in the study did not have CP. If you look at my report itself (Plioplys et al., 1998), on page 163, CP was defined as being a neurologic effect which occurred prior to 28 days of age. The number of CP patients was 371. The total number was 447. Thus, the number without the technical diagnosis of CP was 96. Most of these 96 were as neurologically disabled as the CP group, but the cause of their neurologic handicapped occurred after 28 days of age, such as from a motor vehicle accident. Katz is confused about the number 31 referred to in my deposition. That number deals with patients who had moderate and severe mental retardation, in addition to CP. The study groups 1 through 6 all had patients with profound mental retardation. On page 163 of this deposition, I explained that 22 CP patients had severe MR and 9 had moderate MR. Thus, adding 22 and 9 together results in 31 patients with CP who were not included in groups 1 through 6 because their cognitive functioning was above that of profound MR. The statement by Katz in his report about my study that “developmental disability patients made up 31 of the patients, and they did not have cerebral palsy” is totally incorrect.

Katz claimed that our 1998 report was a “review.” This is incorrect. This was a peer-reviewed, medical research report, not a literature review article.

Katz faulted my article for concentrating on the 1993 Eyman et al. study. The purpose of our study was clearly stated in our article, and that was specifically to define groups of patients that would be identical to those of Eyman et al. in their 1993 report. We wanted to do a comparison research study—a perfectly valid medical research approach. We accomplished what we intended to do.

Katz claimed that our result with tracheostomy patients, in that we did not see increased mortality in CP patients who had a tracheostomy, somehow conflicts with the literature. In our 1998 article I specifically discussed this issue and noted that our results were similar to those of Strauss, et al. (1997), a reference which was cited by Katz. In this study Strauss actually noted improved survival rates for tracheostomy patients who were fed by gastrostomy tube. These Strauss results were even better than our results, and disprove Katz’s criticism.

The two tracheostomy references that Katz quoted, in trying to disprove Strauss’s and our results, were not appropriate studies to cite. The Singer et al. (1989) report primarily dealt with newborn infants, many premature, who required prolonged ventilation due to respiratory distress syndrome (70% of the total studied population). After prolonged use of ventilators and endo-tracheal intubation, the mean age at tracheostomy was 3.7 months. With only 22% with neurologic impairment and mental retardation (29 of 130 cases), this is not the population that we treated. The second report cited by Katz, Ward et al. (1995), is equally inapplicable to our patient population since only 23% had tracheostomies for neurologic conditions, whereas 40% had

congenital anatomical airway abnormalities. Neither of these two studies provided outcome data concerning children with neurologic impairments.

Katz next claimed that the Southern Medical Journal is “outside the literary arena of most persons interested in this topic area.” This comment is erroneous. Within 2 weeks of this article being published, Strauss (the same Strauss who was repeatedly cited by Katz) called me by telephone and congratulated me. It is impossible for my study to have been “outside the literary arena...” The Southern Medical Journal is indexed by the National Library of Medicine (a branch of the US National Institutes of Health) and all medical articles in it are searchable, at no cost, over the internet. Thus, our published information has been readily available for anyone interested in the subject of CP life expectancy, since the study was published.

Furthermore, Katz claimed that the Seminars in Pediatric Neurology is an “obscure journal.” From this statement, it is clear that Dr. Katz does not have knowledge about child neurology publications.

Katz then mentioned the Strauss and Shavelle criticisms of my work which are themselves incomprehensible. This issue has been addressed, and fully discussed in two different responses that I have authored (Plioplys, 2004b; Plioplys, 2005). The Strauss and Shavelle criticisms were never peer reviewed, but rather appear purely as a web posting. It is important to note that the correct method of addressing methodologic issues is to write a letter to the editor of the publication, in this case the Southern Medical Journal, within a few months of the article appearing. Strauss knows this procedure, and has published many letters to the editor in precisely this fashion. In my case, Strauss did exactly the opposite. He phoned me and congratulated me about my published results, and informed me of the statistical errors that Eyman had done. This all indicates that Strauss agreed with my methodology at the time. It was fully 6 years later that Strauss created his commentary.

The final Katz criticism is actually absurd. He criticized that we compared our results to debunked and discredited data of Eyman et al. (1990). [As mentioned above, we designed our study to compare to Eyman et al. (1993)—not Eyman et al. (1990).] We completed collecting our data in 1996, prepared the manuscript in 1997, and it was published in February, 1998, in the Southern Medical Journal. The first publication clarifying and correcting the statistical and mathematical errors of Eyman was Strauss’s letter to the editor in Pediatric Neurology, which was published in September, 1998—7 months later. How is it possible for me to be responsible for including information that was only going to first appear 7 months after the publication of my article? Katz faulted me for not having and using a functional time machine!

It should be noted that I have not been able to find any original, peer-reviewed research studies conducted by Katz on the subject of CP life expectancy.

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