For more than 15 years I have worked intermittently on this twofold problem, but most of what I have done in empirical analysis still remains unpublished. A little bit is in the “Methodology” chapter (chapter 4) in Brackenridge and Elder’s text (cited by Winsemius), and somewhat more is in Singer and Kita’s (“Guidelines for the Evaluation of Follow-up Articles and the Preparation of Mortality Abstracts,” J Insur Med. 1991;23:21–29) (also chapter 4 in the Medical Risks 1991 Compend), and more in Abstracts 607 and 642 in chapter 6 of the 1990 Medical Risks monograph. In 1995, Robert Pokorski quizzed me on this problem by telephone, so I sent my viewpoints to him.

First, I feel strongly that the initial problem of mean $q$’ when only mean age and SD or range are given is one that is crucial to preparing accurate tables of comparative mortality. (The progression of mean $q$’ with duration is a separate problem I will discuss below.) The methodological subject is highly important. Second, the author has introduced an innovative approach that employs mean age and SD of the mean. When age distribution is not given (which is true most of the time) it is necessary to rely on the mean and SD (or range) for a group, all ages combined. The table presented by the author at duration 0–1 year can be very valuable to the preparer of an abstract in making a better estimate of the adjusted age. This is what I have always called “tabular age corresponding to the mean $q$’.” What the author refers to as “my rule of three” is not really a rule at all, but a rough approximation suited to some cardiovascular series such as those in Abstracts 607 and 642 and in the Singer and Kita article. A normal distribution is the best way to start
the estimation, although many patient series do not have a normal distribution. In a graph for patients after coronary bypass surgery (CBPS, 8 series combined), the distribution is very close to a normal one, but the curve for post-myocardial infarction survivors is definitely not a normal one. Its peculiar shape can be traced to the fact that it is the composite of male and female curves, which have different mean age values and different SD values. Female MI patients are older than male MI patients, so the mode of the female curve comes on the right shoulder of the male curve. The author does consider that the distribution may be non-normal and proposes that he will tackle this in part 2, a future paper. This is all well and good, but two things, in my opinion, are not well and good: the failure of the author to state the population used from the 1989–99 US Life Tables for the rates in his table and his failure to provide at least two tables, one for the male population and one for the female. I suspect that the table used was the one for the total US population. It would be a very serious error, in my opinion, to assume that all groups of patients or subjects under follow-up (FU) study had the same sex distribution as that in the US Life Tables. In the CBPS curve on my graph, the proportion of females was less than 15%. In the post-MI curve, the proportion increased from a very low value at the youngest ages to more than half at ages 75 and up. At mean ages for all disease groups except the very young, male mortality was twice the female mortality, or more. It is always necessary to allow for the sex distribution reported for the group subject to FU study. If the author provided male and female tables, the reader could interpolate between the tables for the given sex distribution.

Regarding the presentation of the author’s method, I applaud the use of tabular data and various graphs, which are very helpful to the reader. However, I do have reservations about some of the presentation, particularly the second basic problem or progression of q’ with FU duration, which the author has, mistakenly I think, tried to combine in a table and figures with the first basic problem described above. The reasons for my concern are as follows:

1. The two problems have very distinctive features and require separate consideration.
2. Age distribution in the observed cohort changes with duration in ways unique to each group. Mean age of survivors, as compared with initial mean age, may show any fraction of duration years from 0 to 1 and may even decrease below the initial age. This is something entirely different from the mean age/mean q’ problem for the initial age.
3. Expected mortality and deaths by duration must be matched precisely to the exposures by duration, attained age and sex. The method used by the author relies on a development of the expected deaths as a cohort determined solely by an estimate of the first-year age distribution of the observed cohort, without regard to sex or race distribution. The progressions of cumulative deaths and mean q’ assume a separate independent cohort, unrelated to the progression of the survivors in the observed cohort.

When the 1976 Medical Risks cancer tables were being made, Louis Levinson was dispatched as the envoy extraordinary of the monograph committee to explain exactly this point to Dr Myers (who provided all the data from Report No. 4). This is described in the text as “organization of (Cancer) Results,” paged 1–5 to 1–6, in the introduction to the Cancer Abstracts, 100–190. For survival, Dr Myers was convinced that a separate expected cohort was the way to go. However, actuaries and medical directors, in all of the insurance mortality studies, have always tied expected mortality at each year of duration with the age and sex distribution of the exposed to risk in that year of duration. To me, this is the only logical way to make the comparison.
4. I consider it preferable to use annual mortality rates by duration (q and q') rather than cumulative (Q and Q'), as the author does in his Table 1. The cancer tables (and others) in the 1976 Medical Risks book show mortality ratios as both 100 q/q' and 100 Q/Q' in side-by-side columns. For duration intervals subsequent to the first-year, interval mortality ratio gives a true picture of the pattern by duration. The cumulative MR, 100 Q/Q', gives a blurred pattern for the progression of all durations up to the end of the period. If a duration interval is more than one year, for example, 5–10 years, it is still preferable, in my opinion, to use the average annual rates, rather than the interval rates. This is one disadvantage of the abridged US Life Tables: The mortality rates are for a 5-year interval instead of a single year.

5. All of these problems severely limit the usefulness of all the data in the table and the figures of the present manuscript beyond the first year. I would recommend against including any adjusted mean age data or expected deaths beyond the first year. This would remove the necessity of a complex explanation of the limitations of these results and would enhance the usefulness of the results for 0–1 year, both the explanatory text and the data. This might make it desirable to modify the form of the table and the figures.