

6. Horwitz RI, Feinstein AR. Methodologic standards and contradictory results in case-control research. *Am J Med* 1979; 66:556-64.
7. Sacks HS, Berrier J, Reitman D, Ancona-Berk VA, Chalmers TC. Meta-analyses of randomized controlled trials. *N Engl J Med* 1987; 316:450-5.

The above letter was referred to the author of the article in question who offers the following reply:

To the Editor: I appreciate the letter by Dr. Katerndahl and his comments on measures to ensure publication of high-quality meta-analyses. I am puzzled by the specific concerns used by Dr. Katerndahl to imply that the report on oral contraceptives and breast cancer did not meet the definition of a high-quality meta-analysis. The technique for meta-analysis has become an extremely popular research methodology. A recent MEDLINE search revealed more than 1500 publications in 1992-93 employing this technique. I am in full agreement that it is in the best interest of all concerned to have authors make use of specific guidelines to limit the problems inherent in this research technique. The research design for the oral contraceptive project carefully followed guidelines recommended by L'abbe, Detsky, and O'Rourke.¹ Despite what was stated by Dr. Katerndahl, this included measures to describe a detailed study protocol, to minimize the potential for publication bias, and to develop and utilize properly a quality-assessment instrument. A substantial portion of the discussion was used to explain the nature of the conflicting results within this field of literature.

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References

1. L'abbe KA, Detsky AS, O'Rourke K. Meta-analysis in clinical research. *Ann Intern Med* 1987; 107:224-33.

In-Hospital Cardiopulmonary Resuscitation

To the Editor: I would like to comment on the Schneider, et al. exhaustive survey of the literature on in-hospital cardiopulmonary resuscitation (CPR). It is interesting to note that in general the results are similar to those of a previous meta-analysis, especially regarding the effect of increasing age on survival, despite differences in methodology between the two studies.¹

I would differ, however, with the authors' interpretation of these results, especially their implication that if a subpopulation is found to have a 0.0 percent rate of survival following CPR, the finding "should be suspect and generally can be attributed to low numbers or special populations." While this statement is true in the strictest sense, in clinical practice an invasive therapy with a survival rate that has an upper bound of the 95 percent confidence interval

(CI) of 2.0 percent would be considered by many to be consistent with clinical futility.

For example, in the meta-analysis mentioned above, 144 patients had a diagnosis of metastatic cancer at the time of resuscitation. The survival rate was 0.0 percent, with an upper bound of the 95 percent CI of 2.1 percent.² It is unlikely that many patients would choose to undergo an invasive, painful therapy with a survival rate under 2.1 percent, especially when my research shows that the cost per survivor approaches \$250,000, and the average survivor lives approximately 3 years.

It is also important to note that both the composition of the inpatient population and the techniques of resuscitation have changed dramatically in the past 30 years and that older studies are of questionable applicability to the modern clinical setting. In addition, the paucity of strict inclusion and exclusion criteria meant that dissimilar studies were pooled, a questionable technique. Finally, I am curious why the Mantel-Haenszel statistic was not used more widely throughout the analysis, rather than the simple pooling of data; this technique is preferred for combining 2 × 2 tables across studies in a meta-analysis.³

Certainly CPR should not be abandoned; it is a valuable and appropriate medical intervention for many patients. I believe, however, that it should be possible to single out subpopulations of patients who are poor responders to CPR, using predictive instruments, artificial intelligence, and such meta-analytic techniques as pooling the raw data from similar studies. In this way, CPR can be applied where it will do the most benefit and the least harm.

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References

1. Ebell MH. Prearrest predictors of survival following in-hospital cardiopulmonary resuscitation: a meta-analysis. *J Fam Pract* 1992; 34:551-8.
2. Breslow NE, Day NE. Statistical methods in cancer research. Volume 1: the analysis of case-control studies. Lyon, France: International Agency for Research on Cancer, 1980.
3. Sacks HS, Berrier J, Reitman D, Ancona-Berk VA, Chalmers TC. Meta-analysis of randomized controlled trials. *N Engl J Med* 1987; 316:450-5.

The above letters of Dr. Ebell and Dr. Katerndahl were referred to the author of the article in question, who offers the following reply:

To the Editor: Dr. Ebell and Dr. Katerndahl raise a number of important issues. I would agree with Dr. Ebell that cardiopulmonary resuscitation (CPR) for a patient with metastatic cancer would in general be clinically extraordinary (e.g., a young parent showing a favorable response to treatment of advanced