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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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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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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

Hodgkin's disease who experiences cardiac arrest from an anaphylactic reaction to an antibiotic). I also agree that there are legitimate CPR concerns regarding cost, priorities, and stewardship. The determination of a terminal malignancy is, however, a clinical judgment.

I did not subdivide cancer patients into those with metastatic and nonmetastatic disease, as the majority of articles (14 of 16) did not make this distinction. Ebell's[1] meta-analysis of 14 reports contains unpublished data and a report published after my cutoff date of July 1990. Nonetheless, his grouped cancer CPR success rate of 5.8 percent (16 of 276) closely approximates and in fact slightly exceeds my 4.9 percent (9 of 185) result. Ebell's finding of a 0.0 percent CPR success rate among patients with metastatic cancer is clinically helpful and plausible. I do agree, however, that this model needs prospective testing; and I repeat, "there are seldom zeros or one hundreds" in clinical encounters.

Dr. Ebell's objection to the inclusion of "older studies" is curious in light of his reference to his article[1] that contains a 1960 citation (probably a typographical error) in Table 4. Moreover, Cummins[2] refers to a meta-analysis of pooled data (3765 patients, 12 hospitals) from a recent prospective British study[3] that showed a 17 percent CPR success rate (discharge to home).

Dr. Ebell would like the Mantel-Haenszel test "used more widely." In direct contrast, Dr. Katerndahl would not permit the test at all, as none of the 96 CPR reports were randomized trials. Such a restrictive posture allows only minimal investigation (e.g., a meta-analysis of high- versus routine-dose epinephrine) of the myriad of questions and mounds of data that have accumulated in the last 33 years. I did utilize the more computationally tedious Mantel-Haenszel test for the major comparison of younger and older CPR patients, as is expected by American editors and readers. In many comparisons, however, either no test was reported or a traditional chi-square test was used. *P* values were consistently very low, and the Mantel-Haenszel test actually resulted in more extreme values than the chi-square test. With 20,000 CPR patients divided into two groups, a difference of only 1 percent often yields a clinically suspect, yet highly significant statistic.

The real problem is that the Mantel-Haenszel test, by comparing trait A and its opposite, answers the wrong question (or at least an irrelevant or trivial one). Yet, it is often desirable to compare one group with another (e.g., uremia versus myocardial infarct patients). Cancer patients do, of course, have a significantly lower CPR success rate than those without cancer.

Finally, the British report[3] correctly notes "that numbers were great enough to show highly significant differences" and "formal statistical tests were kept to a minimum." Truly, in an especially refreshing and forthright manner, these researchers employed a single statistical test.

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2. Cummins RO. Cardiopulmonary resuscitation as a medical intervention (Editorial). J Am Board Fam Pract 1993; 6:191-3.
3. Tunstall-Pedoe H, Bailey L, Chamberlain DA, Marsden AK, Ward ME, Zideman DA. Survey of 3765 cardiopulmonary resuscitations in British hospitals (the BRESUS Study): methods and overall results. BMJ 1992; 304:1347-51.

Obscure Gastrointestinal Bleeding

To the Editor: In their article on obscure gastrointestinal bleeding in a recent issue of *JABFP*, Drs. Rizzolo and Newton[1] state accurately that angiography will not demonstrate bleeding from an intestinal site unless there is active bleeding at a rate greater than 0.5 mL/min. The assertion, however, that the results of a study of slower bleeding rates will therefore be negative (and presumably of little value) is not entirely correct.

Angiography has been shown to provide a diagnosis in 43 percent to 74 percent of patients with recurrent gastrointestinal bleeding of obscure origin.[2,3] This procedure should be strongly considered in any patient with recurrent bleeding severe enough to warrant multiple transfusions, even in the absence of active bleeding.[4] I personally had the opportunity to care for a patient in whom selective mesenteric angiography demonstrated a benign leiomyoma of the small bowel, even though there was no extravasation of dye.[5]

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2. Best EB, Teaford AK, Rader FH. Angiography in chronic recurrent gastrointestinal bleeding: a nine year study. Surg Clin North Am 1979; 59:811-29.
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4. Spechler SJ, Schimmel EM. Gastrointestinal tract bleeding of unknown origin. Arch Intern Med 1982; 142:236-40.
5. Jurica JV, Ross JE. Recurrent gastrointestinal bleeding of obscure origin. Illinois Med J 1987; 172:367-9.

The above letter was referred to the authors of the article in question, who offer the following reply:

To the Editor: Angiography can be diagnostic in localizing the site of obscure gastrointestinal bleeding in the individual who is actively bleeding. Because it is impossible to establish with certainty which patients are actively bleeding, the overall sensitivity of the angiogram is greatly diminished — most results falling in the 50 to 60 percent range. One must weigh this diagnostic yield against the risks of this invasive procedure.