9. Light R, Pillemer D. Summing up. Cambridge, MA: Harvard University Press, 1984.

The above letter was referred to the author in question, who offers the following reply.

To the Editor: Dr. Katerndahl's letter discusses some important issues related to the utilization of meta-analysis as a useful analytic tool in medicine. In my article the meta-analysis used for estimating a summary "measure of effect" was not that of physical inactivity and coronary heart disease (CHD), which had been performed in 1987 by Powell, et al.[1] and more formally in 1990 by Berlin and Colditz,[2] but rather was the study of the independent or multivariate relation of physical inactivity and CHD. Both of the former meta-analyses performed quality estimates using a scoring system of 0 to 6 in Powell, et al. and 0 to 32 in Berlin and Colditz based upon the measure of activity, the measure of outcome, and the epidemiologic methods. The 12 studies included in my meta-analysis received scores from 4 to 6 on Powell, et al.'s scale and 18 to 26 using Berlin and Colditz's scale. These were not included in the description of the studies for the sake of brevity, as the reasons for inclusion were outlined in the Methods section in paragraphs 2 and 3. Only studies that used easily standardized hard endpoints and adjusted for confounders of age, smoking, lipids, and blood pressure were utilized in my analysis. Additional exclusions were studies that assessed cardiovascular fitness (attribute) and not physical inactivity (behavior). It is for this reason that only 12 out of the 75 studies were included, and all had quality scores near the highest categories established by Powell, et al. and Berlin and Colditz.

The term *historical cohort* used to define the studies by Dr. Ralph Paffenbarger, including the San Francisco longshoremen study[3] and Harvard alumni study,[4] refers to the fact that these two studies used job classification and mail surveys, respectively, to define physical inactivity in a previously established cohort. They certainly included a control group of unexposed individuals and used the same criteria for evaluating dependent and independent variables in both groups. Perhaps Dr. Katerndahl is confusing case series with historical cohort studies. These two studies[3,4] are considered critical studies in our understanding the relation of physical inactivity and CHD, because the San Francisco longshoremen showed an apparent threshold effect, and the Harvard alumni study showed an effect not only on CHD mortality, but also on total mortality. It seems unwise to have had left out these important studies from my analysis.

Of more than 75 articles reviewed, 14 studies met the standards of evidence described above and evaluated the *independent relation* of physical inactivity and CHD. Two studies (Health Insurance Plan[5] and British civil servants[6]) listed in Table 1 used stratification to analyze confounding and thus could not be included in the weighted summary measure used in my meta-analysis and were therefore not included in Figure 1. Lists of the other 61 references are available in the bibliographies of Powell, et al. and Berlin and Colditz for 53 references and in Part II of this review regarding cardiovascular fitness and secondary prevention trials. I will be glad to make these available upon request to the interested reader.

Dr. Katerndahl's concern about the possibility of publication bias needs further comment. While this is an important concern of meta-analysis in general, it is unlikely that unpublished studies related to the multivariate relation of physical inactivity and CHD were as problematic as he has suggested. First, this review follows that of Berlin, et al., who attempted to find unpublished data. Second, I reviewed the manuscript with several "content experts" in both cardiovascular epidemiology and exercise science-sports medicine who were unaware of any other unpublished materials. Since the submission of meta-analysis at least four additional publications related to physical inactivity and CHD have been published. The Adventist mortality study[7] showed a decreased mortality with physical activity but did not take into account lipids in its multiple variable analysis. The Alameda County study[8] showed benefit to physical activity in all age groups but only accounted for smoking and body mass index in its adjustments. The MRFIT 10.5-year mortality follow-up study[9] showed essentially identical results as those it had published previously and were included in my original meta-analysis. The Dubbo study,[10] while showing a beneficial effect of physical activity using the "soft endpoint" of any CHD, did not show a protective effect using a definite hard endpoint of myocardial infarction.

The funnel graph depicted by Dr. Katerndahl shows nicely the fact alluded to in my manuscript that the more precise studies with larger sample sizes generally show measures of effect closer to 1 (null hypothesis) than those studies that were less precise and therefore with smaller sample sizes. The relation between sample size and the width of the confidence interval comes from the fact that the standard error of the relative risk is inversely related to the sample size. This fact can be seen visually by reviewing Figure 1 of my original manuscript and recognizing that those studies with the smallest 95 percent confidence intervals are closer to 1, whereas those with the greatest relative risks have large confidence intervals. It is for this reason that the often quoted median relative risk of 1.9 for physical inactivity based upon the review of Powell, et al. is spurious and that the relative risk of 1.37 derived from my weighted estimate accounting for sample size is a better estimate of the true independent relative risk of physical inactivity.

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References

1. Powell KE, Thompson PD, Caspersen CJ, Kendrick JS. Physical activity and the incidence of coronary heart disease. Annu Rev Public Health 1987; 8:253-87.
2. Berlin JA, Colditz GA. A meta-analysis of physical activity in the prevention of coronary heart disease. Am J Epidemiol 1990; 132:612-28.
3. Paffenbarger RS Jr, Laughlin ME, Gima AS, Black RA. Work activity of longshoremen as related to death from coronary heart disease and stroke. N Engl J Med 1970; 282:1109-14.
4. Paffenbarger RS Jr, Hyde RT, Wing AL, Hsieh CC. Physical activity, all-cause mortality, and longevity of college alumni. N Engl J Med 1986; 314:605-13.
5. Shapiro S, Weinblatt E, Frank CW, Sager RV. Incidence of coronary heart disease in a population insured for medical care HIP: myocardial infarction, angina pectoris, and possible myocardial infarction. Am J Public Health 1969; 59(6)(Suppl):1-101.
6. Morris JN, Everitt MG, Pollard R, Chave SP, Semmence AM. Vigorous exercise in leisure-time: protection against coronary heart disease. Lancet 1980; 2:1207-10.
7. Lindsted KD, Tonstad S, Kuzma JW. Self-report of physical activity and patterns of mortality in Seventh-Day Adventist men. J Clin Epidemiol 1991; 44:355-64.
8. Kaplan GA, Seeman TE, Cohen RD, Knudsen LP, Guralnik J. Mortality among the elderly in the Alameda County Study: behavioral and demographic risk factors. Am J Public Health March 1987; 77:307-12.
9. Leon AS, Connett J. Physical activity and 10.5-year mortality in the Multiple Risk Factor Intervention Trial (MRFIT). Int J Epidemiol 1991; 20:690-7.
10. Simons LA, Friedlander Y, McCallum J, Simons J, Powell I, Heller R, et al. The Dubbo study of the health of elderly: correlates of coronary heart disease at study entry. J Am Geriatr Soc 1991; 39:584-90.

Carwash Injuries

To the Editor: I wish to report a case in which a carwash injury led to a traumatic hyphema in an 11-year-old boy. The patient came to my office with a left eye injury that he received at a local coin-operated carwash. A friend had been washing his mountain bike with the "turbo-washer" when the pedal of the bicycle began spinning rapidly. The reflector on the pedal became dislodged, ricocheted off the concrete, and struck the boy in the eye. On examination there was an extensive soft tissue trauma around the orbit, visual acuity only to light, a small corneal abrasion, and a hyphema filling about one-third of the anterior chamber. The boy was hospitalized and subsequently regained normal vision in the eye.

Hyphemas generally occur in young men and are the result of trauma from projectiles. Not surprisingly, hyphemas also tend to occur in the spring and summer when young men are exposed to projectiles (e.g., balls).[1,2] Carwashes have not been recognized as a potential cause of traumatic hyphema. The practice of washing mountain bikes in a carwash is relatively new. According to Specialty Equipment West (Salt Lake City, Utah), coin-operated carwashes develop 1000 psi at the nozzle, a pressure that certainly could damage the eye.

There is a small but definite body of research on the health effects of carwashes. Aside from the obvious deleterious effects on driving performance from waxed windshields, more subtle risks have been reported. One Scandinavian study noted very high concentrations of organic solvents in the air of carwashes.[3] A case of carwash tachycardia was reported in 1981,[4] which, thankfully, turned out only to be an artifact on a Holter monitor. In the days of implanted defibrillators, however, carwash tachycardia might lead to carwash defibrillation.

Regardless of whether one decides to have a clean car or bicycle, carwashes must be approached with the same caution we exercise with other technologies of modern living.

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References

1. Kennedy RH, Brubaker RF. Traumatic hyphema in a defined population. Am J Ophthalmol 1988; 106:123-30.
2. Agapitos PJ, Noel LP, Clarke WN. Traumatic hyphema in children. Ophthalmology 1987; 94:1238-41.
3. Niemela R, Pfaffi P, Harkonen H. Ventilation and organic solvent exposure during car washing. Scand J Work, Environ Health 1987; 13:424-30.
4. Smith CR Jr, Elbaum D. Car wash tachycardia [letter]. Ann Intern Med 1981; 95:122-3.

Colposcopy Training

To the Editor: It is with great interest that I read the article by Ferris and Miller entitled "Colposcopy Practice and Training in Family Practice Residency Programs."[1] The article provides good documentation of training, educational programs, and strategies, as well as colposcopic resource materials and equipment.

I performed a similar study in spring 1990, which was conducted approximately 1 week later. My survey instrument was only 1 page and contained fewer questions. It also contained a self-addressed stamped return envelope. The return rate was 75 percent. The most significant difference found was 59 percent of programs provided colposcopy training as opposed to the 45 percent reported by Ferris and Miller. These data are reported in *Family Medicine*.[2] The geographic distribution of these programs providing training are also reported as well as the regional differences.

Our conclusions are essentially the same: family practice residencies are meeting the needs of our trainees in providing colposcopy training, and it is critical for us to establish guidelines for quality assurance. My concern is that since these two studies were carried out within 1 week of each other, what is the true number of programs providing training? Is it possible that some of the respondents to Drs. Ferris and Miller's study did not respond to my inquiry and, indeed, the number of programs provid-