



## Letters to the Editor

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Letters should be submitted in duplicate, double-spaced (including references), and should not exceed 400 words.

### A Fox in the Chicken Coop? Epidemiologic Surveillance of Medical Care

On the cover of the June 1987 *American Journal of Public Health* appeared a poster for the APHA annual meeting, which asks: "Health care for People, or for Profit?" The poster reflects a deepening contradiction in US society over the role of government; it also suggests an imminent fight within the health professions.

Inside this same issue of the Journal, Dr. Philip Caper's editorial<sup>1</sup> reproduced "cost containment" arguments we have all heard. "Cost containment" has already taken an awful toll on the nation's health. So when anyone states that: 1) the public is demanding cost containment; 2) no one seemed to understand (in 1964-5) that health care industry subsidization would get out of control; and 3) the effects of cost cutting on quality and accessibility of care yet remain to be determined, I surmise: here's the fox back in the chicken coop.

The government stimulated the health care industry, accelerated the development of high tech, and enhanced the public sector market. That mission accomplished, apologists arrived to annul the facts that, with some qualifications, more health care has meant better health and *access worked*.<sup>2</sup>

There is evidence that cost containment has hurt the aged, the young,

the poor, and the underinsured. We are left with a two-tier system of medical practice standards and delivery of care<sup>3</sup> which Dr. Caper—and others—ignore. Newspapers<sup>4</sup> have exposed the effects of these cuts. *The Nation's Health*<sup>5</sup> has recently published National Center for Health Statistics data<sup>6</sup> showing a plateau in life expectancy (an outcome measure) from 1982-86 for the whole U.S. population, after the most dramatic rise from 1970-79 of .33 years per year.

Dr. Caper wants to ascertain whether cost containment damages the quality of care. He suggests the work of Pasley, *et al.*,<sup>7</sup> as the kind of epidemiologic application that will lead us to understand the relationships between access, cost, and quality. Ironically, the outcome variable—hospital discharge or surgical procedure—in Pasley, *et al.*,<sup>7</sup> was not correlated with general, or disease-specific, morbidity or mortality in the 63 counties of New York State. Am I the only reader who is wondering whether the counties with high risk of surgical procedures have lower or higher morbidity and mortality?

How can health outcome be so easily ignored by epidemiologists? Does Dr. Caper truly think William Farr would tolerate such nonsense? And with tens of billions available, daily, for speculation on Wall Street, why say our country can't afford to spend what is needed on public health?

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### Dr. Caper's Response to Dr. Sapir

Cost containment in the health care industry is a reality, whether we like it or not. The latest surge of health insurance premium increases is going to intensify cost containment pressures. My editorial was simply a statement that means to measure *both the processes and some outcomes of medical care* now at hand. We should begin to use these means to measure what we're doing while we're doing it, enabling us to channel cost containment efforts in appropriate, non-destructive directions. Doing this well will require the active support of all the health professions, support which has not been freely offered in the past.

Small-scale analytical efforts to date, such as those reported in the paper by Pasley, *et al.*, have almost uniformly demonstrated large, unexplained variations in the per capita costs and utilization of medical care among seemingly homogeneous populations. Substantial variations in the outcomes of care provided in different hospitals to similar cohorts of patients, such as the study of prostatectomy outcomes by Wennberg, *et al.* (*JAMA*, April 1987), are also beginning to appear. Small area and other studies have also shown repeatedly that neither high volume nor high cost ensure high quality. Indeed, the per capita rates of utilization and costs for communities served by institutions widely acknowledged for their quality standards—Palo Alto CA, Rochester NY, Rochester MN, and Iowa City IA, for example—are consistently at or below their own state average rates.

Efforts to restrain the per capita costs of medical care are going to increase in the near future, with or without the cooperation of the health professions. Simply complaining that cost

containment will damage quality is no longer a credible position. Rather, I believe support and assistance from the health professions would be warmly welcomed by those in both public and private sectors who are concerned with attenuating the chronic cost spiral. Our major challenge will be creating and nurturing a constructive, participatory role in improving the productivity of our medical care system.

I hope we're up to it.

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## On HIV Transmission in Homosexual/Bisexual Men

Winkelstein, *et al's*, report of human immunodeficiency virus (HIV) transmission in homosexual/bisexual men in a large, prospectively studied cohort,<sup>1</sup> is clearly valuable, but also warrants some critical commentary.

I have difficulty with their "estimated 22.8%" infection rate in their sample for the last half of 1982. This figure was derived from the 49 of a subgroup of 215 (22.8%), who were seropositive on entry into the study in 1984, but who reported no receptive anal-genital contact with ejaculation or needle-sharing during the prior two years.

This estimate appears quite uncertain, because of: 1) inferences from a subsample with demographics skewed in multiple parameters from the larger group (addressed in their discussion<sup>1</sup>); 2) reliance on the unknown validity of self-reports of specific sexual and needle-sharing behaviors over a prior two-year period; and 3) assumption that the only causes of HIV seroconversion in this population were receptive anal-genital contact with ejaculation, or needle-sharing. Other practices have caused HIV transmission (e.g., transfusions; female-to-male intercourse<sup>2,3</sup>—relevant for bisexuals), or are considered potential risks—e.g., sharing toothbrushes or razor blades, anilingus, intimate kissing, insertive anal-genital contact, oral-genital contact (although the authors studied and elsewhere concluded the latter two unlikely, they reported wide confidence intervals,<sup>4,5</sup> and another group reported evidence for receptive oral-genital transmission<sup>6</sup>).

Despite such sources of uncertain-

ty, their estimate was reported with three significant figures, and plotted on a graph where it appreciably shaped a curve. It was also used in deriving a three-significant figure annual seroconversion rate, and in further quantitative analysis of the "saturation effect" they inferred (from the seroconversion early in the epidemic, of those most susceptible because of their most frequently engaging in high-risk sexual behaviors). How certain can the authors be, that their estimated 22.8% wasn't, in actuality, 12.8%? or 32.8%?

Without the distraction of the misleadingly reported precision, it is not difficult to believe that the annual seroconversion rate in their cohort has declined, partly due to some decrease in identified high-risk sexual activity in seronegatives, and partly due to a saturation effect. However, they assume the latter to be a function only of self-reported frequency of identified high-risk sexual behavior, ignoring, for example, the common finding of constitutional differences in resisting infection. Indeed, a recent paper reports dramatic variation in HIV seroconversion rates related to genetic heterogeneity of the Gc protein.<sup>7</sup>

Nevertheless, this study's importance is considerable. Perhaps a more striking finding than the decline in reported high-risk sexual behavior was the large number of those who reported that they continued to practice insertive and receptive anal-genital sex, presumably often without condoms (did Winkelstein, *et al.* ask about use of condoms?). Perhaps, as has been recommended,<sup>8</sup> it is time to encourage the voluntary and confidential learning of one's HIV antibody status, as a step in promoting the acceptance of the reality of the risks and personal responsibility in halting the spread of this tragic epidemic.

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## Response from Winkelstein, *et al.*

Dr. Greenberg has indicated a number of considerations which bear on the interpretation of the findings from our recently reported study of a reduction in the transmission of HIV infection in San Francisco.<sup>1</sup> We should like to address one of those considerations, the estimate of HIV seroprevalence among homosexual/bisexual study subjects two years before the San Francisco Men's Health Study (SFMHS) was begun. As Greenberg pointed out, we estimated this value from the prevalence (23%, 95% CI, 17%-29%) among study subjects who claimed no high-risk sexual activity or needle sharing during the two years prior to entry. He questions the validity of this estimate for reasons put forth in his letter. We have, therefore, made a retrospective estimate from data reported in our paper which the reader may verify.

In Figure 1, HIV seroprevalence from the City Clinic Cohort (CCC), another prospective study of HIV infection in San Francisco, are shown for five time intervals since 1978.<sup>2</sup> Because two of these intervals coincide closely with the time periods during which HIV seroprevalence was determined in our study and because those estimates bear a constant relation to each other, a