To the Editor:

I read with interest the excellent article, "Herpes Genitalis and its Relationship to Cervical Cancer," by Raymond H. Kaufman, M.D., and William E. Rawls, M.D., which appeared in the September/October 1974 issue of Ca—A Cancer Journal for Clinicians.

An intriguing and provocative segment of this article dealt with the treatment of herpes simplex virus infections using proflavine dyes in conjunction with incandescent or fluorescent light. As the authors pointed out, dye-light therapy effectively eliminates herpetic infections but, under laboratory conditions, has resulted in oncogenically transformed cells. The implication is that this therapy may have dire consequences relative to the neoplastic phenomenon.

Nonetheless, the clinician is now confronted with an interesting dilemma. Dye-light therapy may effectuate a rapid cure of herpetic lesions although the long-term effects, initially subtle, may be devastating by enhancing the oncogenic potential of the herpes virus. The possibility that patients receiving dye-light therapy will have an increased incidence of carcinoma is, at present, unsubstantiated since this treatment is of too recent origin to yield meaningful data. Nevertheless, follow-up studies on the incidence of cancer in individuals who received dye-light therapy is certainly warranted. With the advent of new and varied pharmacological techniques, today's physician must balance the initial benefit to the patient against potentially harmful mutagenic effects on the infecting virus. Although this controversy currently involves the herpes viruses, the universality of this concept remains to be examined.

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Authors' Reply:

We certainly agree with Dr. Docherty's suggestion that follow-up studies on the incidence of cancer in individuals receiving dye-light therapy is warranted. Likewise, it is equally important that untreated patients be followed carefully for the development of both vulvar and cervical carcinoma. Certainly, there is a significant amount of scientific evidence to suggest a relationship between herpes virus type 2 and the development of cervical carcinoma.

Considerable alarm has arisen over the suggestion that the use of dye-photo inactivation for the treatment of herpes simplex infections may prove oncogenic. Much of this is based upon Dr. Fred Rapp's comments (JAMA, Medical News, 225:459, 1973) on in vitro studies with hamster kidney cells grown in tissue culture. Assessment of the oncogenic potential of a virus in humans by
examining it in the laboratory is based on assumption and far-reaching speculation. In addition, during the course of an untreated recurrent herpes virus infection of the skin or vaginal or cervical mucosa, at least 100 defective virus particles are generated for each infective one. It is defective viral particles produced during the photo inactivation process that Dr. Rapp fears may be oncogenic. We suggest that a continued and repeated assault in the area of a recurrent herpes lesion with an abundance of defective viruses produced under natural conditions may well constitute a greater risk of oncogenesis than the successful therapy for a recurrent herpetic infection, in which the replication of the virus is cut short by photo inactivation.

Clinicians continue to promote the photo inactivation treatment of herpes virus infections (Obstet. Gynecol. 41:74, 1973). Prudent evaluation of this treatment in carefully conducted double-blind studies and exceptionally diligent follow-up of patients treated by this technique is certainly needed.

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To the Editor:

I have read with dismay the article "Prostatic Cancer," which appeared in the September/October 1974 issue of Ca—A Cancer Journal for Clinicians.

The results of primary treatment by irradiation, external or interstitial, are said to be "at a clinical plateau." To the contrary, Bagshaw's paper in 1965, though preliminary, suggested that small volume supervoltage therapy could be very effective and systemic hormonal therapy might wait until the patient developed distant disease, utilizing radiation for pelvic tumor control. Ray and Cassady have followed up and expanded Dr. Bagshaw's figures in a 1973 publication demonstrating encouraging survival and excellent local control in a treatment group receiving radiation alone.

In a disease which manifests a long natural history, when the tumor is well differentiated, and when it is extremely difficult to obtain a hormone-free treatment group (as orchiectomy or estrogen therapy are often a major thrust of initial therapy for the disease whether early or late stage), the existence of the Stanford data serves as a cause for hope rather than the gloomy prognosis that the author assigns to radiation treatment.

Approximately one patient in 20 with prostatic cancer can be selected for surgery, while potentially curable megavoltage radiation therapy appears to be applicable to almost all patients with Stage I-III disease (confined to the pelvis). Although the data are still preliminary, my emphasis would be a positive one favoring radical radiotherapy with a good chance for local sterilization in patients with prostate cancer. Certainly personal observations of prostatic cancer patients treated over the past seven years on both a 4 Mev and 8 Mev linear accelerator only serve to confirm my opinion.

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