The commentary by Lippman et al. (1) provides a useful review of the topic by organizing a massive amount of information in an organized way. However, because this review will be widely read and referenced, there are two general points made by the authors that could be challenged. The authors define two of the four criteria for identifying a “definitive” chemoprevention trial as “primary end point of cancer incidence” and “large scale (n ≥ 1000) with the definitive sample size and duration based on anticipated event rates in the intervention arm (treatment effect) and placebo arm.”

One major goal of chemoprevention research is to determine the protective effect of an agent while placing as few participants as possible at risk. One straightforward way to accomplish this goal is to understand the process of the disease (carcinogenesis) sufficiently to conduct trials with markers that predispose to or predict the final end point of cancer. In general, most histologically identifiable precancers (e.g., cervical intraepithelial neoplasia, Barrett’s esophagus, adenoma polyps, actinic keratoses, and dysplastic nevi) evolve to cancer with a sufficiently predictable frequency to conclude that their reversal or suppression can be used to predict cancer development and to assess the value of a chemoprevention agent. There are, in fact, as the authors note, several studies (2–6) that have addressed the effectiveness of chemoprevention in this manner. The medical community accepts hypertension and cholesterol as surrogate “preneoplasias” of cardiovascular disease risk and their modulation as indicative of a favorable or unfavorable drug effect. Modulation of the pathobiology of precancers is as valid a marker of carcinogenesis as is the end point of cancer. Undoubtedly, advances in our understanding of carcinogenesis will allow us to identify and develop new agents by the modulation of a biochemical event earlier in the carcinogenic process and this should be a major goal of chemoprevention research.

Large numbers may provide comfort to the investigator that a definitive result has been obtained. However, there are many instances in medicine when large numbers were not required to make the point, i.e., where the underlying cause...
was stumbled upon (e.g., scurvy), recognized (e.g., pellagra), or mechanistically defined (e.g., rickets). One would hope that, with the large number of molecular targets that have been identified for chemoprevention [e.g., Table 2 in (1)], at least one of them will be equivalent to the examples of nutrient deficiencies cited above. Intervention early in the disease process of carcinogenesis in a highly specific manner based on rational therapeutics will lead to sustainable advances at considerably less cost and effort than huge, expensive, and lengthy trials that use cancer as the end point. Although “definitive” large randomized trials will continue to be necessary to advance the field of chemoprevention, they should be uncommon and conducted only after convincing experimental and clinical work has been done (7).

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REFERENCES


NOTE

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RESPONSE

We appreciate Dr. Meyskens’ thoughtful letter on our commentary (1), which highlights the two definitive trial criteria with which we wrestled the most. After careful consideration, we chose cancer as the definitive end point because it is conservative, falling between the liberal end point—premalignancy—and the most conservative end point—cancer mortality. In harmony with Dr. Meyskens, we stated that the cardiovascular disease-related “surrogate end points of high blood pressure and cholesterol are accepted as valid surrogate end points (or diseases). Similar surrogate end points must be established in cancer.” We caution readers that pharmacologic suppression or reversal of premalignant lesions, such as cervical intraepithelial neoplasia or others cited by Dr. Meyskens, has not yet been associated with longer term reduction in cancer incidence. Therefore, premalignant lesions are not yet generally accepted as surrogate end-point biomarkers (SEBs) for definitive trials designed to establish efficacy of an agent in reducing cancer risk.

We also deliberated at length on our choice of 1000 or more subjects as the definitive size, which we based largely on our choice of a cancer end point. This size generally is sufficient to meet the requirements of two of our other criteria—two-sided hypothesis testing and randomization with placebo control. We acknowledge that the size criterion is subject to debate, and future definitive trials (with cancer end points) may well require smaller sample sizes, especially if the anticipated treatment effect and/or event rate (e.g., achieved by targeting high-risk subjects) are sufficiently great.

We concur with Dr. Meyskens’ view that developing new agents through molecular targeting approaches or modulating a biochemical event earlier in carcinogenesis (i.e., using an established SEB) should be a “major goal of chemoprevention research.” Following the etiologic example of linkage between vitamin C and scurvy (cited by Dr. Meyskens) or between hepatitis B and hepatocellular cancer (cited in our commentary), we and other investigators (1–3) are searching for “surrogate end-point biomarkers [e.g., molecular targets] in preinvasive carcinogenesis that are tightly linked to cancer development” (1). We further commented that “the surrogate end-point biomarker would have to be inextricably implicated in the causal pathway between an agent’s effects and cancer development.” For many years, M. D. Anderson’s cancer chemoprevention programs have investigated potential SEBs and will continue along this critical path of discovery. Much of the second half of our commentary is devoted to this issue.

Dr. Meyskens’ call for conducting “‘definitive’ large randomized trials . . . only after convincing experimental and clinical work has been done” echoes our statement that “basic science and translational studies continually re-examine criteria for initiating definitive cancer chemoprevention trials on the basis of strong scientific rationales and well-conceived hypotheses.” Certainly, we believe that biologic advances will strengthen hypotheses, thus increasing the potential for positive results. This is not, however, to discount the value of past negative (harmful or neutral) definitive trials that have saved “lives . . . and resources by directing research efforts away from ineffective [or harmful] interventions” (1). Another benefit of large-scale, long-term trials is in detecting infrequent or delayed and chronic side effects that would never be detected in relatively short-term definitive trials with surrogate, rather than cancer, end points. Nevertheless, we also believe that it would be highly desirable to develop trials with, as Dr. Meyskens described, “considerably less cost and effort than huge, expensive, and lengthy trials that use cancer as the end point.” Repeating a theme we expressed 10 years ago in another commentary (3), we stated that “valid surrogate end-point biomarkers . . . would . . . [allow] shorter, smaller, and less expensive definitive chemopreventive trials” (1).

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REFERENCES


NOTES

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