LETTERS

Antibiotics: Use in Animal Feed

Thomas H. Jukes (Letters, 6 Apr., p. 8) accuses me of misrepresenting myself as having been "naïve" at the time I agreed to serve on the Council for Agricultural Science and Technology (CAST) task force on antibiotics in feeds because an article on that subject appeared in the *Journal of Commerce* of 23 September 1977 under my name. If one views this accusation in the light of the actual sequence of events, of which it appears Jukes was aware (*1*), it becomes clear that Jukes' statement is wrong.

When I and several other plasmid biologists were approached by CAST in April 1977 and invited to serve on the task force, I was, indeed, naive, never having heard of CAST and knowing no more about the use of antibiotics in feeds than that it was a common practice and that the Food and Drug Administration was involved in an attempt to curtail it.

Having accepted that responsibility, we naturally set about becoming better informed, an effort that required virtually the entire spring and summer (of 1977). Early in September 1977, I was invited to testify before the Senate subcommittee on agricultural research and general legislation (21–22 September 1977) on the subject of antibiotics in feeds. Since the work of the task force was not yet complete, I requested and received permission to testify from Virgil Hays, chairman of the task force. Shortly thereafter, the *Journal of Commerce* published (without my knowledge) an excerpt from my testimony, which is the substance of the article upon which Jukes' accusation is based.

Two other points in Jukes' letter deserve comment. Regarding a telephone call James McGinnis made to me, alluded to by Jukes (I wonder how he knew about that call), McGinnis furnished data demonstrating that one can compensate for substandard livestock feed by pumping the animals full of antibiotics. I had already sifted through masses of data on this subject showing that optimal livestock management practices can abolish the "growth-promoting" effects of feeding antibiotics; included in these data was a set of such results by Hays himself. I have strong misgivings about the waste of one of our most valuable medical resources as a cover for substandard practices in the rearing of livestock, even if it is sometimes effective and saves the time, money, and effort needed for decent animal management. Moreover, Jukes is now advocating the use of drugs not for "growth promotion" but for wholesale antibacterial prophylaxis: "today's production of meat requires preventive medicine rather than treatment of sick animals" (*2*). Blind, "broad spectrum" prophylaxis has long been considered useless and dangerous in human medicine because it fosters the development of multiresistant bacteria; on the basis of bitter experience, some meat producers have finally come to the same conclusion.

With regard to chloramphenicol (Cm) and chlortetracycline (Tc), Jukes seems to be unfamiliar with the voluminous literature of the past 20 years on R factors, in which it has been found by practically everyone working in the field that one of the most common plasmid linkages is that between Cm and Tc resistances—so that the feeding of Tc automatically and with high frequency selects for bacteria that are resistant to Cm (and several other antibiotics) also.

Finally, in view of the ad hominem nature of Jukes' attack, I would have liked the opportunity to respond simultaneously.

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References

1. T. H. Jukes, personal communication.
2. ———, *Anim. Nutr. Health* **32**, 9 (1977).

Food Safety Report

Media coverage of the recent National Academy of Sciences–Institute of Medicine food safety report, as might be expected, has been variable in accuracy and scope. Regretfully, R. Jeffrey Smith's extensive piece in the 23 March issue of *Science* (News and Comment, p. 1221) does not rank high in the spectrum of accuracy.

I shall limit myself to two comments, one minor but to which I can testify personally; a second major but which depends upon more careful reading of the committee majority consensus. Smith called me about the source of the report's recommendation of three qualitative levels of risk. I told him that my recollection of the complex discussions within the committee did not allow me to identify any single source. I said that I had been one of the proponents of the concept of a relatively few broadly defined levels of risk and that during discussion I had referred to the utility of the concept with respect to recombinant DNA. I did *not* say that the committee's recommendation "was patterned on the different degrees of containment for recombinant DNA research," as stated by Smith. A passing remark in discussion is given far too much weight by that interpretation.

More consequential is the statement that the committee's recommendations "constitute a significant return to the philosophy of caveat emptor," a theme sounded in the subhead of the article in even bolder form. I do not recall that the philosophy in question was ever discussed within the committee and for good reason. Caveat emptor was a battle flag in an earlier era of controversy, when the issue was whether the federal government should intervene in any way on behalf of consumers. The issue as seen by the committee, in this era, was how the federal government could most intelligently and effectively intervene—taking into account different public interests, including those among consumers, and the increased complexity and changing nature of the technical information bearing upon decision. Rather than suggesting elimination of a federal role, the committee recommended wider discretion in the Food and Drug Administration's exercise of it, buttressed by more effective scientific input, more public information, and more widespread public participation. Caveat emptor? Perhaps—for the consumers of News and Comment in *Science* magazine.

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Recombinant DNA Experiments

I hope that DeWitt Stetten, Jr., has seen the irony that is implicit in his letter "rejoicing" in the report of the decrease in infectivity of polyoma virus DNA when recombined into plasmid or phage DNA and inserted into *Escherichia coli* (30 Mar., p. 1292). He mentions the "long-awaited report," but does not state that this report is not only long-awaited but is a cart-before-the-horse situation. Long after guidelines have been set up, we now have a report bearing directly on the experiments which *should* have been done before the guidelines were adopted. Is this not contrary to established scientific procedure? What we should rejoice about is that the experiment did not turn out the other way, which would have really highlighted the