LETTERS

Jensen's Address at APA Meeting

In a briefing (News and Comment, 19 Sept., p. 978) concerning my address before the American Psychological Association (APA) annual meeting in Chicago, it is stated, "Some APA officials were uncomfortable about having Jensen on the program at all, and were irritated that Jensen, in a press release, appeared to represent himself as having been invited by the leadership of APA when he was in fact invited by the division of educational psychology."

The only press release that was issued was written and sent out by the Office of Public Information of the University of California, Berkeley, and contained just one sentence concerning the status of my presentation, as follows: "Jensen reported his findings this afternoon (Tuesday, Sept. 2) in an invited paper at the 83rd Annual Convention of the American Psychological Association in Chicago." The official program of the APA convention lists my paper in two places (pages 163 and 215) under the heading "Invited Addresses on Test Bias."

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Oil Spill Effects

The article by Mark Panitch, "Offshore drilling: Fishermen and oilmen clash in Alaska" (News and Comment 18 July, p. 204), contains several quotations regarding research findings from the Auke Bay Fisheries Laboratory of the National Marine Fisheries Service (NMFS). Some of these statements are largely speculative, some are preliminary, and others are solidly backed by past or ongoing research.

In the first category, I meant by the statement "Any spill situation will exceed these [LD_{50}] values even at depth" that any *major* crude oil spill situation in which the mixing energy (such as storm-driven waves) is sufficient to result in the formation of oil emulsions at depth (as occurred in the Chedabucto Bay, Nova Scotia, spill of Bunker C fuel oil) will likely produce water-soluble oil concentrations at depth in which these LD_{50} values (1 to 5 parts per million as determined by infrared spectrophotometry) are exceeded. The duration and extent of these water-soluble oil concentrations depends upon circulation patterns, flushing rates, and sediment loads in the spill area. The potential for these values occurring will be greatest in areas of poor flushing and circular gyres. My

prediction is partially based on the results of studies of the behavior of oil-water mixtures in our laboratory, but more data on the distribution and dynamics of water-soluble concentrations of crude oil resulting from oil spills under a variety of environmental conditions is needed to correlate laboratory bioassays with real spill situations.

Furthermore, because of the dynamic complexities of oil in contact with biological systems and the specificities of various chemical methods, determination of LD_{50} values with other analytical techniques in addition to infrared spectroscopy (ultraviolet and fluorescent spectroscopy, gas chromatography, and mass spectroscopy) may be necessary before we can obtain complete information on the relative and synergistic toxicities of various components of crude oil. Thus, LD_{50} values quoted in the article (based on infrared data) should not be viewed as the final answer with regard to the toxicity of crude oil.

Comments on the relative shrimp production of Kachemak Bay and the Gulf of Mexico by Evan Haynes, as they appear in the article, may be misleading to the reader. On the basis of catch per unit effort, the shrimp production of Kachemak Bay may indeed exceed ten times the *average* catch rate in the Gulf of Mexico. This type of comparison is probably not valid, however, since the Kachemak Bay shrimp fishery is a day fishery with little effort being expended in locating shrimp, while the Gulf fishery is typified by fishing trips of 2 to 27 days during which considerable effort is expended on exploratory fishing. There is no doubt that Kachemak Bay is an extremely productive area where shrimp production may be equivalent to or exceed some of the most productive areas in the Gulf of Mexico (*1*).

Other comments identified by Haynes as speculative in nature are supported by available data. The existence of anticyclonic circulation or low flushing rates in outer Kachemak Bay is strongly suggested by NMFS current meter data, drogue studies by the Alaska Department of Fish and Game, and by Haynes' data on larval shrimp and crab distribution. Haynes suggests a circular current in the area holding larvae hatched there for several months, but larvae may also be carried in and out of the bay by prevailing currents. Therefore, the accumulation of larvae may result from the low flushing rate in the area rather than a circular flow per se. Important unknown factors are the duration and seasonal persistence of the current and the circulation at depth.

The article by Panitch is well written and, as a whole, appears to be the type of objective reporting one expects from *Sci-*

ence. However, several errors in spelling are readily apparent to Alaskan readers, namely Kasitsna Bay, not Kisitsna Bay; Evan Haynes, not Evans Haines; and Tony Mecklenburg, not Tony Micklenburg.

The $175,000 NMFS toxicity study mentioned in the article was cooperatively funded by a group of oil companies including the Shell Oil Company, Union Oil Company of California, Standard Oil Company of California, Marathon Oil Company, Phillips Petroleum Company, and Texaco, Inc. All comments in the article by investigators associated with this project and other NMFS studies should be regarded as personal opinions and not official NMFS positions or those of the funding organizations.

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1. K. W. Osborn, B. W. Maghan, S. B. Drummond, *Gulf of Mexico Shrimp Atlas* (Government Printing Office, Washington, D.C., 1969), pp. 1–20.

Lost Strain of Rats

The National Multiple Sclerosis Society has been interested in the use of experimental allergic encephalomyelitis (EAE) as a laboratory model for evaluating the efficacy of drugs for therapeutic treatment of multiple sclerosis. Such an evaluation is made difficult by the tendency of most strains of rats to recover relatively rapidly and spontaneously from EAE.

From 1968 to 1971, scientists at the Upjohn Company did an important series of therapy experiments (*1*) using a strain of Wistar rats from Manor Farms. In this strain, EAE was easily produced and the paralysis lasted for many weeks, so the therapeutic effects of drugs could be evaluated with ease.

Unfortunately, this strain of rats is no longer maintained commercially and may very well have been lost. If any scientists presently possess breeding colonies derived from Wistar rats obtained from Manor Farms during the period from 1968 to 1971, the society would appreciate the opportunity to obtain and test some of these animals.

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References

1. M. E. Greig, A. J. Gibbons, G. A. Elliott, *J. Pharmacol. Exp. Ther.* **173**, 85 (1970); G. A. Elliott, A. J. Gibbons, M. E. Greig, *Arch. Int. Pharmacodyn. Ther.* **204**, 62 (1973).

Age and Tenure

Since a journal such as *Science* should stress facts rather than fiction, I was concerned when I read the editorial by Frank Press (18 July, p. 126). Press states that there "are many university scientists in the age range 55 to 65 who believe that their contributions to science are behind them." Since he is below this most productive age range, I assume his statement is not a self-appraisal. It may relate to some scientists in his department, but I am confident that it is not typical of my associates.

As a scientist in this alleged obsolescent age range, who is in his most productive years, I am aware of juvenile propaganda which has resulted in many forced early retirements of productive scientists and limitations on earned income by those receiving social security benefits. However, I am unaware of any facts that might be used to support such unsound edicts.

With regard to the alternate careers suggested by Press for these discarded scientists, I have worked in local government and in foreign technical assistance programs, taught science at a small college, written textbooks, and served as a staff member of professional and educational organizations. Was I mistaken when I considered such assignments as worthwhile contributions?

I doubt that my experienced colleagues, such as Robert A. Alberty (age 54), Paul Doty (age 55), William Doering (age 58), R. B. Woodward (age 58), Glenn Seaborg (age 63), Norman Hackerman (age 63), or Melvin Calvin (age 64), would agree with Press's proposal. Perhaps they, like myself (age 63), would appreciate hearing of any factual data showing that their contributions are behind them. Please say it isn't so, Dr. Press.

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I cannot understand Seymour's response to my modest proposal that those scientists in the age range 55 to 65, who themselves believe they can contribute more to other endeavors than academic research, be allowed to do so in a respectable and financially rewarding manner. I am just as aware as Seymour of the many important contributions made by scientists over the age of 55, and I would be the last one to tamper with this reservoir of talent.

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