our knowledge the regulatory situation remains as described in our report.

In our calculations we did not "tune" our model to obtain hydroxyl radical (HO) concentrations to "force fit" the 8-year residence time for methychloroform. The HO concentration was generated by the model, as we described in detail. Singh is quite correct in stating that the residence time, and consequently, the conclusions reached, depends directly on the HO concentration. We have made this point very explicitly in the report. However, we also point out that there are constraints in any model calculations to any "tuning" for HO; in our case higher concentrations of HO would be incompatible with methylchloroform measurements. We understand that there are discrepancies among scientists' measurements of methylchloroform, particularly with respect to hemispheric ratios, and would support further measurements to improve the assessment of the problem.

In conclusion, we would not want to argue that EPA was unjustified in their 1970 regulatory action based on the scientific knowledge available at that time. But it does seem worthwhile to point out the need to be aware of the kinds of pitfalls that may ensue from regulations and to urge a continuing reassessment of their consequences in the light of new information.

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An Endothermic "Nessie?"

In Jean Marx's discussion of the dinosaur endothermy-ectothermy debate (Research News, 31 Mar., p. 1424), it is pointed out that dinosaur fossils have been recovered in Cretaceous Arctic Circle regions, where, perhaps, only endothermic animals could have survived.

Curiously, one of the arguments against the supposed *Nessiteras rhombopteryx* of Loch Ness (see *Science*, 9 Jan. 1976, p. 54) being a "prehistoric reptile" has been that such ectothermic animals could not survive in the almost constant 42°F temperature of the 1000-foot-deep loch.

If the endothermy school is correct, it makes the existence of *Nessiteras* more reasonable. Or, put the other way

around, maybe Jean Marx will, after all, see the day when a few dinosaurs are "rounded up and studied directly."

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Yale's Discontinued Department

The article " 'New wave in academia' wipes out department at Yale" (News and Comment, 17 Mar., p. 1189) contains several inaccuracies which should be put straight. The initiative for discontinuing the Department of the History of Science and Medicine at Yale did *not* originate in the School of Medicine. The fourth professor was George (not Charles) Rosen. At no time did the medical school "want to deploy the vacant professorship in a subject such as the ethics of medicine." In fact, the School of Medicine is currently engaged in a search for an individual to fill a senior faculty position at the rank of professor or associate professor as the head of a Section of the History of Medicine.

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Sex Differentials in Salaries: Faults in Analysis of Covariance

Much criticism has been leveled against the use of covariance procedures to adjust for known differences among populations in order to test for hypothesized differences among them. The criticisms offered by Woodward and Goldstein (9 Sept. 1977, p. 1096) of the research on "communication deviance in the families of schizophrenics" apply, with minor variations, to the work of Bayer and Astin (23 May 1975, p. 796) relative to salary differences between men and women on university faculties.

The major conclusion of Bayer and Astin is that men are paid more than women of similar academic rank, departmental affiliation (*1*), number of publications, and so forth. Variables such as number of publications are, however, fallible indicators of constructs, and being fallible they control incompletely for the target construct, research productivity. As a result, one cannot infer a salary differential because of sex from the analysis offered by Bayer and Astin.

Instead of reiterating criticisms of analysis of covariance offered by Woodward and Goldstein and many others, I refer the reader to Fig. 1, which is derived from Bayer and Astin's data (*2*). In that figure it is evident that there are sex differences in both number of publications and salary. However, inspection of this figure, and other figures (*3*) based on the means of other homogeneous groupings in the same population, indicates that there is no systematic salary differential attributable to sex per se. Although in some of these groups men seem to have been paid more than women, the opposite seems true equally often.

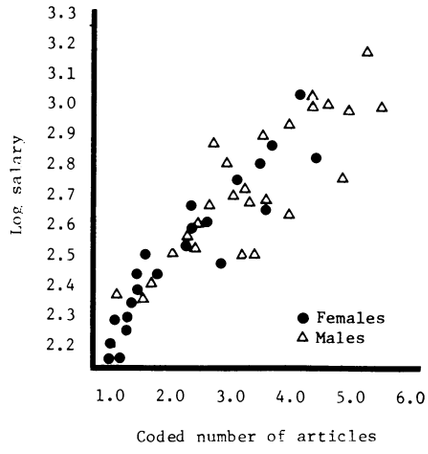

Fig. 1. Mean log salary plotted against mean number of articles for groups homogeneous with respect to sex, rank, and departmental affiliation (*3*). For each group $N \geq 25$.

In many respects this way of looking at the data is also vulnerable to criticism. No claim is made from this analysis that a sex differential in salary is not present. It seems reasonable to suppose, however, that one should be able to detect a sizable differential from inspection of such plots. On the other hand, the covariance analysis used by Bayer and Astin is known to be biased in the direction the results indicate: that is, the group higher on the fallible covariate will tend to appear disproportionately higher on the variate (when the variate and the covariate are positively correlated) even when there would be no such disproportionate difference if an infallible covariate were used.

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References and Notes

1. Departmental affiliation is a grouping of faculty according to kind of department: business, education, biology, physical sciences, social sciences, fine arts, humanities, health.
2. I thank Bayer and Astin for making these available.
3. Tables upon which Fig. 1 and the other figures are based are available from the author.
4. Supported by contract NIE-C-74-0115 from the National Institute of Education.