Whether or not bluefin tuna and leatherback turtles can regulate body temperature by physiological means, their thermal inertia makes possible a kind of physical thermoregulation. Heat generated from metabolism is retained to produce advantageous (*3*) excess body temperatures, and the tissues are effectively protected from fluctuations of environmental temperature lasting even several hours. Such animals should enjoy a distinct ecological advantage over those that must rely solely on behavioral thermoregulation to maintain their tissues at the thermal optimum.

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5. An alternative approach was developed by G. A. Bartholomew and V. A. Tucker [*Physiol. Zool.* **36**, 199 (1963)] in work on heat exchange by lizards. Essentially, k's calculated from the relation

$$dT_b/dt = k(T_a - T_b)$$

were adjusted for metabolic heat production estimated independently from oxygen consumption data.
6. E. D. Stevens and F. E. J. Fry, *Can. J. Zool.* **48**, 221 (1970).
7. Assuming T_e was 25.5°C when T_a was 7.5°C,

$$k = \frac{dT_b/dt}{T_e - T_b} \simeq \frac{\Delta T_b/\Delta t}{T_e - T_b} \simeq \frac{-0.022°C\ min^{-1}}{(25.5 - 28)°C} \simeq 0.009°C\ min^{-1}\ °C^{-1}$$

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10. E. D. Stevens and F. E. J. Fry, unpublished manuscript. We will supply on request a graph summarizing the relationship between k and body size for a number of aquatic vertebrates.
11. We thank F. E. J. Fry for his comments on a preliminary draft of the manuscript.

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After reading Neill and Stevens' comment, I came away with the impression that the thermoregulation we observed in bluefin tuna is in some way invalid and am concerned that it may be dismissed as an artifact of poor data analysis. I would like to point out to other readers who may have gained a similar impression that Neill and Stevens do not dispute the fact of thermoregulation, but have interested themselves in the importance of thermal buffering by the heat exchange system in achieving a stable temperature. They analyzed our experiments with bluefin No. 8 and bluefin No. 14 (*1*) and show that the observed thermoregulation could be achieved through a low coefficient of temperature change. However, this does not apply to the experiment with bluefin No. 13 (*1*). Here the stomach temperature of a 270-kg bluefin tuna increased by 7°C over a 20-hour period while the fish remained in water of constant temperature. There is more involved in the ability of the fish to control its temperature than a k value similar to that of a Thermos bottle.

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Evidently Neill and Stevens agree both that leatherback turtles can maintain body temperatures well above that of the environment and that the cooling rates (k values) for the larger turtles are in the order of 0.0015°C min^{-1} $°C^{-1}$. The only point of difference is that they suggest that a k value of 0.002 would have been appropriate, whereas Frair, Ackman, and Mrosovsky (*1*) used a figure of 0.0015. There seems little point in arguing about the exact k value on the basis of the meager data available. Whether a k of 0.002 or 0.0015 was appropriate makes little difference to the overall conclusions.

We agree with Neill and Stevens that, in principle, k values are better calculated on the basis of the equilibrium rather than the ambient temperature. However, in the present case, making this point is unhelpful because one does not know what the equilibrium temperature was. Moreover, Neill and Stevens have assumed that the excess body temperature is 5°C and that it would be the same at different ambient temperatures. Both these assumptions are unlikely. In using the 5°C difference they ignore data on temperature of leatherbacks in tropical waters (*2*). In suggesting that excess body temperatures are constant at different ambient temperatures, they ignore observations of increased activity of marine turtles in cooler water (*3*).

It would be desirable to learn more about the equilibrium temperatures of leatherbacks and also about the functioning of their recently discovered countercurrent system (*4*). Unfortunately there is little chance of someone with a suitable thermometer in hand encountering a leatherback in northern waters, or of a specimen in good condition being caught near adequate experimental facilities. We hope that if this should occur, someone will communicate with us so that arrangements can be made for obtaining fuller information on this warm-bodied turtle.

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15 October 1973

Screwworm Eradication Program

Calman (*1*) and Smith (*2*) comment adversely about the status of the Southwestern Screwworm Eradication Program. This program is conducted by the U.S. Department of Agriculture (USDA) with the cooperation of livestock regulatory agencies, extension services, and grower organizations in affected U.S. states and Mexico. The program is conducted by USDA's Animal and Plant Health Inspection Service (APHIS), with research support from the Agricultural Research Service (ARS).

There could be many reasons for our recent difficulty in keeping populations of the screwworm *Cochliomyia hominivorax* from growing and spreading in this country. Ecological as well as genetic and physiological factors must be considered. Abundant rainfall for the past 2 years has favored screwworm increase in the warm months. An

unusually mild winter in 1971–72 aided the survival of this subtropical insect.

The sterile insect release program was designed with the assumption that most growers take good care of their livestock. We believed that the release of 100 million sterile flies per week would overwhelm the native flies that bred in wildlife and neglected domestic animals. After the screwworm almost completely disappeared in 1964, southwestern ranchers were able to save more than $100 million (1962 dollars) annually that had previously been spent for screwworm control. When screwworms reappeared in 1972, cattlemen could not immediately return to their earlier ranch practices. Because of a lack of skilled cowboys, trained cow horses, spray pens, dipping vats, and special holding pastures, ranchers could not prevent screwworm breeding, so their herds became a new class of "neglected" livestock.

In 1968, APHIS increased the distribution of sterile flies to 200 million per week, but these numbers, released in northern Mexico and the adjoining U.S. states, did not create an effective barrier in 1972, nor in 1973. The situation in Texas in 1973 was much better than it was in 1972. Only in an area south of Laredo and Corpus Christi was there inadequate control. Most of the cases occurred in five counties where an uncontrolled outbreak of Gulf Coast ticks, *Amblyomma maculatum*, made thousands of cattle unusually susceptible to screwworm infestation. In spite of local failures, chiefly in Texas and Arizona, the situation in the past 2 years has not been as disastrous as Calman indicates. The 1972 infestation north of Texas was minor; it occurred late in the season and was quickly terminated by cold weather. The vast majority of animals previously susceptible to screwworm infestation have been protected, to the benefit of producers, consumers, and wildlife.

The screwworm eradication program has had obvious benefits, but ARS is not complacent about the need for supportive research. For those interested in references to investigations, I cite four articles (*3*) by scientists who have done screwworm research. In this comment, I discuss the unpublished results of research done in 1973 by E. H. Ahrens, E. C. Corristan, A. J. Graham, and H. C. Hofmann of APHIS and by R. C. Bushland, M. M. Crystal, G. W. Eddy, R. R. Grabbe, O. H. Graham, B. G. Hightower, and C. M. Jones of ARS.

The problems in screwworm genetics cited by Smith have received attention. The APHIS screwworm breeding stock at Mission, Texas, is not descended from the original Florida colony used at the beginning of the program. To prevent the loss of field adaptation because of prolonged laboratory rearing, breeding stocks have been completely changed three times during the course of the program. In 1966 a strain developed from flies collected in Mexico replaced the Florida strain. In 1971 that strain was replaced by another laboratory strain of Mexican origin, and in 1972 a new strain developed from flies collected in Texas at the beginning of the outbreak became the APHIS production strain. Field comparisons are now under way to select a new strain for 1974.

We made many collections of screwworms from infested livestock at diverse locations in Texas and Mexico during 1972 and 1973 to establish laboratory populations for mating tests in cages. In all tests, the laboratory strains have mated with the newly colonized flies. In competitiveness tests, the old laboratory-adapted strains of sterilized males mated more successfully with wild-type females than did fertile, wild-type males.

Working jointly with the Methods Development Section of APHIS, we have conducted a series of field evaluations in Texas and in Tamaulipas, Mexico. The APHIS–ARS entomological team has made release and trapback tests with a new strain of flies (designated "Tex-Mex") developed from eggs collected in 1972 at 18 locations in Texas and at 6 locations in Mexico. The new strain seemed to migrate farther, faster, and survive longer than the APHIS production strain. Both strains are now being compared for mating competitiveness with wild flies on plots of 5180 km^2 in south Texas. We have brought many additional screwworm collections into the laboratory to breed for genetic and physiological studies. We are combining some of these stocks and attempting to develop a better strain.

Although mild winters, rainy summers, and changed ranch practices have favored the wild screwworm, there may also have been genetic changes in the wild population. Field experiments indicate that our sterile males are not now mating as effectively with wild females as we might expect on the basis of tests with caged flies. The northern fringe of the wild population may have evolved mechanisms that isolate them from sterile flies in the barrier zone. Rearing the larvae on artificial media produces flies visibly different from those that grow in wounds. The sterile flies are not as large as wild flies, and their color is a different shade of blue. There are undoubtedly other deficiencies in artificially reared flies that wild flies might detect. Isolating mechanisms could also have evolved that do not involve rejection of sterile males, but rather factors in dispersal, aggregation, or mating behavior that keep wild and sterile flies from intermingling at mating time. We are planning a field release in southern Mexico, where the wild population has not been exposed to sterile flies, and a similar release of sterile flies in northern Mexico, to see if there is a difference in the proportion of sterile matings.

I regretfully agree with Calman that "no one has gotten to the bottom" of the screwworm problem, but we are digging (*4*).

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4. We are aided in our work by geneticists, physiologists, and chemists from other ARS laboratories at Fargo, North Dakota; Kerrville, Texas; Florence, South Carolina; and Gainesville, Florida, who have come to Mission, Texas, on temporary duty to work under quarantine with fertile flies before returning to their home laboratories to continue analysis of frozen specimens and to experiment with sterile flies shipped to them by APHIS. In addition, we have contracts for screwworm research with the University of Texas at Austin (isozyme studies in population genetics); with the Instituto Tecnológico y de Estudios Superiores de Monterrey, Monterrey, Mexico (studies on screwworm ecology and collection of new strains in northern Mexico); and with the Instituto Nacional de Investigaciones Agrícolas, Mexico City (trapping studies, study of the effects of weather on populations, and collection of new strains in southern Mexico).

27 December 1973