

session. Such a proactive effect would, likewise, produce fewer explorations, and thus result in a spurious amnesic effect. If this interpretation were correct, delayed presentation of ECS (group D) could be expected to elicit a similar effect. This did not occur. In contrast to group C, group D explored the hole significantly ( $p = .01$ ) more than the nonreinforced group A. Again, in contrast to group C, group D was not significantly ( $p > .20$ ) different from the reinforced pseudo-ECS group B. Moreover, observations of overall activity showed that there were no significant differences between the ECS and non-ECS (excluding group E) groups. Thus, the interpretation that the one ECS produced diminished activity and a spurious amnesic effect is not supported.

However, the greater number of hole explorations of the delayed-ECS group over the immediate-ECS group did not reach significance ( $p = .10$ ). Thus, it seems possible that ECS might exert some limited retrograde amnesic effects even 3 hours after reinforcement (4). A longer reinforcement-ECS interval might have produced a significant difference making the findings more conclusive.

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#### References and Notes

1. J. Burř and O. Burřová, *J. Comp. Physiol. Psychol.* **56**, 268 (1963); M. C. Madsen and J. L. McGaugh, *ibid.* **54**, 522 (1961); C. A. Pearlman, S. K. Sharpless, M. E. Jarvik, *ibid.*, p. 109.
2. The increase in hole explorations from the 4th to test session of group A can possibly be attributed to either of two variables or both. Three days had lapsed between the 4th and test sessions, whereas only about 4 hours had passed between the 3rd and 4th sessions. The prolonged absence from the apparatus could have resulted in a reawakening of "curiosity." The second factor is probably more important. Whereas the animals were only deprived of water for about 22 hours on the 4th session, they were deprived for about 40 hours during the test session. This increase in drive level could easily have resulted in increased activity.
3. E. E. Coons and N. E. Miller, *J. Comp. Physiol. Psychol.* **53**, 524 (1960); W. J. Hudspeth, J. L. McGaugh, C. W. Thomson, *ibid.* **57**, 61 (1964).
4. J. Burř and O. Burřová, *ibid.* **56**, 268 (1963).
5. Probably any discretely measurable, "naturally" occurring response that is of a moderately high frequency (and which will diminish with repeated trials) is reliably responsive to the effect of one reinforcement. This hole-in-the-wall procedure was suggested by some unpublished experiments of Dr. J. R. Trotter (Australian National University, Canberra). He had used the response of the rat placing its head into the food cup as an operant, and aperiodically reinforced it with food pellets.
6. The technical assistance of Miss Gale Geist is gratefully acknowledged.

26 March 1965

## Reptilian Thermoregulation

J. E. Heath [*Science* **146**, 784 (1964)] takes "many workers" to task for poor scientific practices in the study of reptilian thermoregulation. Since I [*Copeia* **1963**, 107 (1963)] am the only one of the workers mentioned, and since Heath's conclusions are controverted by a reanalysis of his data, I feel obliged to indicate some of the probable sources of confusion and error.

Heath's remarks are largely a protest against a progressive simplification of approach. He complains that "many workers have discarded the several categories of thermal responses proposed by Cowles and Bogert [*Bull. Am. Museum Nat. Hist.* **85**, 265 (1944)] in favor of determining the body temperatures of reptiles surprised in the field." (These categories are: the lethal minimum, the critical minimum or cold narcosis, the voluntary minimum, the basking range, the normal activity range, the maximum voluntary tolerance, the critical maximum, and the lethal.) The wording of Heath's complaint suggests a possible confusion between the *schema*, that is, the response categories of Cowles and Bogert, and the *methodology* practiced by field workers. If his point is that the characteristics of certain thermal responses cannot be elucidated by the collection of body temperatures in the field, I agree.

However, his main complaint seems to be that this attrition of tradition in the analysis of reptilian thermal relations has reached such proportions that those of us who take temperatures of lizards in the field can no longer be said to be studying thermoregulation. Heath says that "only two of [Cowles and Bogert's] categories, the maximum voluntary tolerance and minimum voluntary tolerance, contain behavior which alters the heat load upon the animal" and that the other categories, including the basking range, "are not directly related to active regulation." This is perplexing. It is well documented that basking and foraging lizards assume postures and choose microhabitats that depend in varying degrees on their body temperatures, the position of the sun, and the time of day. Do not these activities alter the radiational and conductive heat loads on lizards?

Heath also criticizes workers for

purposely ignoring data: "In some cases body temperatures below an arbitrary level are ignored because they lie in the so-called 'basking range' of the animal." In my report I deleted three low temperature records from a total of 297 because of the marginal thermal conditions at the times of collection. Heath's justifiable criticism is the motivation for an experiment on beer cans from which he concludes that the deletion of such lower records markedly changes the results and interpretations.

Heath exposed 11 water-filled beer cans to the sun during July 1963, and monitored the temperatures of the cans and the nearby air temperatures hourly from 1030 to 1830 P.S.T. Assuming that recording began at 1130, there should be 110 pairs of observations. However, in the legend of his Fig. 1, a histogram of the beer-can temperature data,  $N$  is given as 97 (although there appear to be 100 entries in the histogram itself). In Fig. 2, a scatter diagram of beer-can and the corresponding air-temperature records, there are 96 entries. (The two figures are irreconcilable in other ways as well.)

Heath reports that "can temperature is loosely correlated with air temperature ( $r = +.41$ ;  $P < .005$ ). The same statistics recalculated from his Fig. 2 ( $N = 96$ ) are  $r = +.68$ ;  $P < .001$ . Heath goes on, "Following the precedent of others, all can temperatures below an arbitrary level, in this case  $30^{\circ}\text{C}$ , were ignored . . ." (about one-third of the data). In the relevant part of Fig. 2, Heath actually omits all can temperatures below  $30.5^{\circ}\text{C}$  plus two of the highest records. He found the correlation between can and air temperatures in this amputated scatter distribution to be  $-.09$ ; a probability value of less than .05 is given for this insignificant  $r$  value. By inspection it is clear that the association is positive rather than negative. The recalculated correlation in this case, including the omitted records, is  $+.45$ ;  $P < .001$ .

The magnitude of these errors casts doubt on the validity of Heath's conclusions. First, the omission of the lower records turns out not to have an appreciable effect on the results; all the recalculated  $r$  values are positive and highly significant. Second, his conclusion that can temperature is inde-

pendent of air temperature after deletion of the lower records is not supported by the significant correlation coefficient. Third, his contention that "the range of correlation approximates that recorded among nine species of lizards" in my work is supported neither by his original coefficients nor by the recalculated ones. I found that the range of  $r$  values in question for lizards was from  $-0.27$  to  $+0.73$ ; therefore, my rather obvious conclusion that environmental thermal diversity cannot be ignored in any comprehensive analysis of thermoregulatory relations still seems valid.

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My conclusions from the beer can demonstration were that the temperature distributions obtained from beer cans cannot be distinguished from those derived from the studies of reptiles in the field, and, for this reason, that "a study of behavioral temperature regulation requires either that the regulatory behavior be witnessed and body temperature immediately measured or that a control be used so that the body temperature of the regulating animal can be compared to the temperature of a model." Soulé's criticisms do not affect either of these conclusions.

Soulé recalculated  $r$  values for beer cans from a scatter diagram which shows can temperatures grouped at  $0.5^\circ\text{C}$  intervals. Without using the ungrouped values and clarifying to one another the procedures used in the calculations, reconciliation of our figures is not possible. However, there is no need to argue over these differences. Soulé's recalculated  $r$  values for beer cans,  $+0.68$  and  $+0.45$ , approximate the range of  $r$  values he found [*Copeia* 1963, 107, (1963)] in four of seven species of lizards,  $+0.73$ ,  $+0.72$ ,  $+0.63$ , and  $+0.38$ . In short, with either set of values the temperature distribution ob-

tained from beer cans resembles those of lizards to a disturbing degree. Since the above data obtained from lizards are used as evidence of thermoregulation, are we to assume that the beer cans were also regulating their internal temperatures?

I agree with Soulé that reptiles from time to time assume body postures and select microhabitats that alter the heat load on the animals. However, to demonstrate regulation we must show in each case that the body temperatures collected from regulating animals differ significantly from those of nonregulating controls. In this connection, Cowles and Bogert's categories of basking and normal activity range were constructed in part from uncontrolled data. Consequently, I remarked that these categories could not be directly related to thermoregulation. On the other hand, Cowles and Bogert constructed the categories of minimum and maximum voluntary tolerance from body temperatures obtained from reptiles observed to undergo a regulatory movement, for example, running into the shade. These two categories represent body-temperature thresholds eliciting regulatory behavior.

Soulé builds his criticism around my unelaborated comment that the deletion of lower temperature records of beer cans alters the  $r$  value. He says that the omission of lower temperature records has no appreciable effect on his recalculated  $r$  values for beer cans. He then uses his recalculations to negate the inferred conclusion that "the deletion of such lower records markedly changes the results and interpretations." This presumably applies to reptiles as well as beer cans. Even if the correlation coefficients were not altered by such deletions, the mean temperature is changed (in beer cans by more than  $2^\circ\text{C}$ ), as are the range and variance. These parameters are the ones normally used to compare "thermoregulation" among reptiles. Certainly the arbitrary deletion of data must be regarded as a questionable practice.

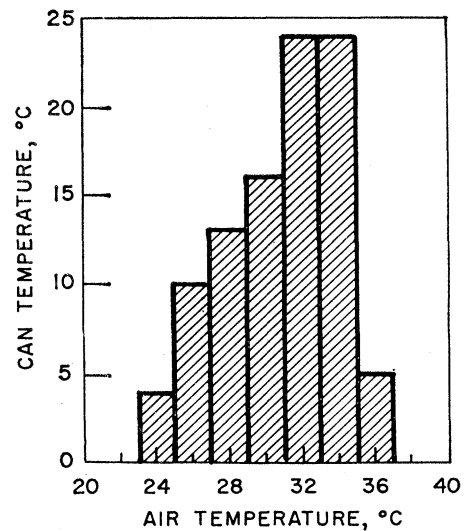


Fig. 1. Temperature distribution of cans in direct sunlight (July 1963).

I thank Soulé for bringing to my attention the erroneous histogram presented as Fig. 1 of my report. The correct histogram is shown in the accompanying figure. A can temperature of  $34^\circ\text{C}$  at an air temperature of  $29^\circ\text{C}$  was left out of the scatter diagram (Fig. 2 of my report). There were 110 observations, but in 13 instances, involving 6 cans, the cans became partially shaded during the course of the day. I moved each of these cans back into direct sunlight and included them in the succeeding hourly measurements. Soulé may object to this maneuver, but the method used by field workers assumes nothing regarding the previous history of a reptile. Accordingly, the hourly measurements of cans were made in feigned ignorance of the previous state of any individual can.

In summary, the question I posed was: Can we study a regulatory process with a method that involves no controls and no direct observation of regulation?

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