Comments and Communications

Trenches in Venezuelan Archeology

DURING recent exploration in the Río Negro region in the Territorio Federal Amazonas (Venezuela) the author was advised of the existence, in a place known as *"lugares viejos de los indios,"* of man-made trenches about 300 ft long, 6 ft deep, and 6–9 ft wide. Sherds, some with anthropomorphic decorations, found in the immediate vicinity of these trenches are evidence of former human habitation. In this same zone and, probably, in the same locality or in its immediate vicinity, lanceolate-shaped canoe paddles of hard wood with anthropomorphic decorations on the handle have been found. A wooden implement, decorated with a carved human face, has been found in Caño Loro by Sr. Clemente Calderón of San Carlos de Río Negro and donated by him to Salesian Rev. Father Bombequio, who in turn presented it to the convent of this order in Puerto Ayacucho.

Trenches of this type are reported to be present in many other places, among which are the following: Caserio de Mayagua, Río Pasimoni, Caserio Solano, Caño Pasimoni, Capaco, Caserio Iguaynape, Caño Darigua, Caserio Uraña, Caño Motuiti, Laguna de la Brega, and Caño Bunte, all in Distrito San Carlos de Río Negro. Wooden paddles are said to have been recovered in all these places.

Recently the writer had an opportunity to study one of these trenches in Santa Rosa de Amenodora. Considerably damaged by modern fill from the surfacing of the adjoining village streets, it still retains its ancient morphology and principal characteristics. Starting in the swampy land near the bank of Río Negro, it forms a shallow crescent in the general direction of the village square. Many of the natives remember it when it was unencumbered by recent fill and when its entire length of 210 ft was 9 ft wide and 9 ft deep.

According to popular belief, these trenches were filled with briars or pointed sticks and loosely covered to serve as traps for an attacking enemy. For lack of any evidence that would confirm or refute this belief, the present author advances the hypothesis that these trenches served as hiding places for the Indian dugouts at night or during wartime. These dugouts would not be left in the river because, in the eventuality of an enemy attack, the group that lost its canoes would find itself in a precarious position indeed, in consequence of loss of mobility in forests that, flooded as they are during the greater part of the year, offer very little facility for foot travel.

In the public square of Santa Rosa de Amenodora, a village originally inhabited by the Baré Indians, in a small exposure where erosion has removed the humous horizon of the soil, the author found numerous sherds of archeologic value. One of the local inhabitants declared that the boys of the village had found clay figurines. Indeed, a fragment of such a figurine was presented to the author, but unfortunately it is so badly disfigured that identification is very uncertain.

These trenches deserve careful study, as does the pottery, in view of the fact that the carved canoe paddles and other anthropomorphic wood carvings are, to the knowledge of the author, the first specimens of wooden artifacts recorded in Venezuelan archeology.

J. M. CRUXENT

Museo de Ciencias Naturales
Caracas, Venezuela

Tissue Respiration and Body Size

In a recent paper (*Science,* 113, 599 [1951]) von Bertalanffy and Pirozynski express the opinion that "The experiments seem to be a blow to the hypothesis that the decrease of basal metabolic rates with increasing size is due to cellular factors. . . ."

One wonders what noncellular factors could be responsible. However, what the authors apparently have in mind is that their material does not support the idea that the level of tissue respiration changes with body size to the same extent as the metabolic rate of the whole animal.

The tissues they used originate from one species only, the rat, in sizes from 9 g to 392 g body weight. The metabolic rate in this animal changes approximately according to the usual $M = b\,W^{2/3}$ or $b\,W^{3/4}$. However, if tissues from newborn to adults in one species do not show similar variations, this finding is hardly sufficient for categorically dismissing previous findings of such variations in tissue metabolism in different species. Physiological differences between newborn and adults are too great for use of such material for conclusions concerning the much-disputed "surface law."

The authors refer to Kleiber's review on the subject (*Physiol. Rev.,* 27, 511 [1947]). A careful reader of his chapter on "Intraspecific Comparisons" will find that "only a few species show great enough differences in mature weight for the establishment of the best fitting power function. . . ." Kleiber here refers to total metabolic rate, but the condition of comparing mature animals should also be fulfilled when relating tissue respiration to body size.

The work of von Bertalanffy and Pirozynski is an interesting contribution to the knowledge of differences between mature and immature individuals, and their data on tissue respiration are valuable. In particular, their data on the diaphragm show a considerable decrease in oxygen consumption with increasing body size, which is not properly described by their words: "But it is definitely smaller than the extent postulated by the surface rule. . . ." When plotting their data for this tissue one finds a curve for total oxygen consumption of the diaphragm versus animal size with a slope of approximately 0.74 (log log scale) —for practical purposes identical with the slope for total oxygen consumption of the animal vs. body size. Field, Belding, and Martin (*J. Cellular Comp. Physiol.,* **14**, 143 [1939]) attributed half the tissue oxygen consumption for which they could give an account to muscle. The findings of von Bertalanffy and Pirozynski, therefore, to the extent that comparisons between immature and mature animals are justified, support the view expressed by Krebs (*Biochim. et Biophys. Acta,* **4**, 249 [1950]) (who did not publish data on muscle): "The characteristic differences in the basal rate of heat production in animals of different size are to be attributed mainly to variations in the Q_{O_2} of the musculature."

KNUT SCHMIDT-NIELSEN

Kettering Laboratory
University of Cincinnati

The interesting comment by K. Schmidt-Nielsen raises questions that were only touched in our preliminary communication.

The decrease in weight-specific basal metabolic rate, as expressed in the surface, or ¾-power, rule, can be explained in two ways—namely, that it is based upon intrinsic differences of cell respiration in animals of different size which will show up also *in vitro*; or that it is regulated by factors lying in the organism as a whole. Of course, there may be also a combination of both possibilities. If the first alternative is accepted, two hypotheses can further be distinguished; namely, (1) that the decline in weight-specific metabolic rate is based upon a decline of Q_{O_2} in some tissue or tissues, and (2) more specifically, that all tissues show a corresponding and parallel decline in Q_{O_2}, as proposed by Weymouth *et al.* (*Proc. Soc. Exptl. Biol. Med.,* **49**, 367 [1942]). Our results, and also those of Krebs, contradict the second hypothesis. As far as the first hypothesis is concerned, we have noticed, of course, that the diaphragm, as the only muscular tissue used in our experiments, shows a remarkable decline of Q_{O_2}, and that this corresponds to Krebs' view that variations in the Q_{O_2} of masculature are responsible for those in basal metabolic rate. Statistical evaluation of our data for diaphragm gives an exponent $\alpha = -.26$, with a high correlation coefficient $\rho = .93$. Further experiments with skeletal muscles are on our program. A quantitative estimate assuming a decrease in Q_{O_2} similar to that of the diaphragm for total musculature, and attributing, according to Field *et al.,* half of total oxygen consumption to musculature, shows, however, that variations in the Q_{O_2} of musculature cannot be considered as the main factor for the differences of basal metabolic rate within the same species.

We have no prejudice against the "cellular" interpretation. The results favoring it are mentioned in our paper, and our investigation was actually started in the hope of contributing to that view. On the other hand, "organismic" factors are acknowledged, for example, by Kleiber, who points at hormonal and neural regulators, concentrations of ions, nutrients, oxygen, etc., and considers "the analysis of these factors controlling the metabolic level *in vitro* and *in vivo* a most fruitful field for further research" (*Physiol. Rev.,* **27**, 523 [1947]).

Our experiments naturally tell nothing about interspecific relations in tissue metabolism, which were studied by Krebs. Though the physiological differences between newborn and adults are great, the same is true for anatomical, physiological, biochemical, ecological, etc., differences even between related species, not to speak of comparisons "from the mouse to the elephant." The striking fact is that in spite of this simple quantitative relations can be established. Our own work is mainly concerned with intraspecific relations in metabolism because it appears that they are basic in the connection between metabolism and growth and the mechanism of the latter (*cf. Am. Naturalist,* **85**, 111 [1951]).

LUDWIG VON BERTALANFFY
W. J. PITOZYNSKI

University of Ottawa

Time of Flowering of *Delonix regia*

THE royal poinciana, or "flame tree," *Delonix regia* (Bojer) Raf., called by the Spanish "flamboyant," is endemic in Madagascar, and possibly its poor adjustment to the seasons of the Northern Hemisphere is due to its origin in the Southern Hemisphere. The entire foliage of individual trees sometimes turns yellow, presenting a magnificent spectacle for a few days, like a sugar maple in New England. The feathery leaves, dropped at the same time, cover the ground with an even thicker and more even layer of yellow than the uneven red litter when the blossoms fall. Such trees appear in every other way similar to others near by and quickly have a new crop of leaves. They show no subsequent idiosyncrasy at time of blossoming, but idiosyncrasies are somewhat difficult to detect, for blossoming starts very slowly along the roads of coastal Puerto Rico in mid-May (too late for the tourist season), with most trees in full bloom in June and July, and some straggling on into August and September. Of some two dozen trees planted with seed from a single tree, accidentally selected after the last hurricane of San Ciprián because it was the only one to produce seed, variation in the time and amount of blooming is as great as though no selection had been made.

A trip across the island of Puerto Rico from Río Piedras to Aguirre on May 11, 1951, disclosed conspicuous differences in the amount of flowering of flamboyant. At Río Piedras, where the elevation above sea level is less than 100 ft, the only indication of fresh foliage and blossoming was on a single branch directly underneath a street light. In the hills between Río Piedras and Caguas a few trees had begun to bloom, but it was only in the much higher hills between Caguas and Cayey (elevation 1,233 ft) that